

International
Encyclopedia of the
SOCIAL
SCIENCES

Volume 18

Complete and Unabridged

International
Encyclopedia of the
SOCIAL
SCIENCES

BIOGRAPHICAL SUPPLEMENT

DAVID L. SILLS EDITOR

VOLUME 18

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To the memory of
TALCOTT PARSONS
1902-1979

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Preface: On the Uses of Biographies

The preface to a scholarly work should place the book in a context by indicating its relationship to other publications. My first task is thus to relate this biographical supplement both to the 17 volumes of the *Encyclopedia* published in 1968 and to the 15-volume *Encyclopaedia of the Social Sciences* published by The Macmillan Company between 1930 and 1935.

Alvin Johnson, the economist who served as associate editor (and *de facto* editor) of the 1930–1935 *Encyclopaedia*, was nearly ninety years old when I knew him in the early 1960s, but he had forgotten little of what he had learned as an encyclopedist thirty years earlier. One of his bits of advice to me in 1962 was not to listen to people who would say that it is “too early” to produce a new encyclopedia of the social sciences; some one will always say that, he told me, and such advice should be ignored. Searching for a way to tell us to “do it now,” he invoked a bit of country advice he had learned in his youth: “Prune when your knife is sharp!” I am delighted that The Free Press has decided that its knife is now sharp, and that this is an appropriate time to publish a supplementary volume. For reasons given below, I think it is particularly important that this supplement is devoted to new biographies.

Another bit of Alvin Johnson’s advice which we followed in 1962 was to be more selective than he had been in commissioning biographical articles. The earlier encyclopedia contains some four thousand biographies; although many are still useful today, it is easy to imagine that many more have been read by only a handful of scholars during the last five decades.

Since my colleagues and I were aiming at a conceptual rather than a descriptive encyclopedia, we at first considered having no biographies at all. Luckily, we rejected this notion, and in retrospect we are pleased with our decision to include some six hundred biographies. This supplementary volume contains an additional 215 biographies.

Barbara A. Chernow served as full-time managing editor for the duration of the project, and her “Introduction” describes the criteria for selecting subjects and contributors. In these remarks, I note some ways in which biographies can be used.

Most obviously, biographies are useful for finding answers to factual questions about people: when was so-and-so born? where edu-

cated? major publications? when was the famous book published? Information of this kind is often difficult if not impossible to find in a library, and an encyclopedia that includes biographical articles is thus an essential reference work.

A second use of biographies of scientists is to help us understand their ideas and their methods. Particularly in the social sciences, the substance of scholarly work is to some degree a reflection of the lives of those who produced it, and the more we know of their lives, the more we know of their work. This relationship of course varies a great deal from scientist to scientist, and it is certainly not implied here that reading biographies is a substitute for reading original sources.

The relationship between a writer's life and his work is the focus of a continuous argument in the humanities. Some scholars assert that the publications of a writer should alone be the basis for critical analysis (the so-called New Criticism), and others object to biographical analysis on invasion-of-privacy grounds. *Telling Lives: The Biographer's Art*, edited by Marc Pachter (1979), contains eight useful essays that describe these and other opinions about biographies as data.

Biographies, and particularly collections of biographies, can clearly contribute a great deal both to intellectual history and to the history and sociology of science. Collections of biographies provide information about the historical and social context in which new ideas emerge; they provide data useful in determining the sequences of ideas and what else we need to know about the priority of discovery; they help us to understand the influence of social and cultural origins and of generational membership upon scientific development; they tell us of particularly influential teachers and colleagues; they describe the "invisible colleges" to which scientists belonged, and thus their networks of exchange and influence; and they indicate the ways that ideas become (or fail to become) institutionalized by their creators or by succeeding generations, either in terminology or in research institutes. The present volume joins not only the 17 volumes that preceded it; it also joins such collections of biographies as Charles C. Gillispie's 15-volume *Dictionary of Scientific Biography* (1971–1978) and Gardner Lindzey's *A History of Psychology in Autobiography* (1974), which is itself an extension of the 5 volumes that preceded it.

An eighteenth century word—prosopography—has been used to describe the method of systematically studying collections of biographies, a method that historians have called "collective biography" and that other social scientists might call "multiple career-line analysis" (Stone 1971, p. 46). Sociologists and historians of science are now giving more of their attention to this technique. One example is Steven Shapin's and Arnold Thackray's (1974) analysis of the British scientific community from 1700–1900. Another is Harriet Zuckerman's *Scientific Elite* (1977), an intensive analysis of the careers of 92 Nobel laureates which informs us about the familial and social origins of creative scientists in interaction with the system of recognition and reward in science. Robert K. Merton's review (1979) of new developments in the sociology of science stresses the usefulness for research of collections of biographies. Merton also provides a fascinating account of how Gillispie's *Dictionary of Scientific Biography* was almost prepared in such a way as to make systematic prosopographic analysis possible. It was proposed at one time that all the writers for the *Dictionary* would have to provide systematic data about their subjects,

so that computerized storage, retrieval, and analysis would be possible, but in the end the idea was judged to be premature (Merton 1979, pp. 41-47).

The biographies in the *International Encyclopedia of the Social Sciences* (including those in this supplementary volume), like those in the *Dictionary of Scientific Biography*, are of enormous use individually, but they are of uneven utility for prosopographical purposes. For example, some biographers do not provide sufficient data on employment and research activities to make systematic career-line analysis possible; others make almost exclusive use of the publications of their subjects as sources, and thus omit much of the information about the setting of their subjects' work which a historian or a sociologist of science would prefer. But they all provide a glimpse of one member of an important generation of social scientists.

Elsewhere in the front matter of this volume there is a list of the disciplines represented by the biographies. All of the social sciences are of course represented. Also represented are disciplines at the margins of the social sciences: philosophy and legal theory at one margin, human biology at the other. This selection of persons from the humanities and the natural sciences reflects a current trend within the social sciences to learn from people at the margin, and thus enhances the usefulness of the volume.

The generation of scholars represented in this volume was educated between the two World Wars, when there were relatively few social scientists of any kind. It is the generation whose members were the teachers of a substantial proportion of contemporary social scientists; it is the generation that came between the nineteenth century founders of the modern social sciences and the thousands of practicing social scientists today. For this reason alone, this supplementary volume of biographies should be a welcome addition to our personal and institutional libraries.

The volume speaks for itself. I would like only to record here my appreciation for the intelligence, the diligence, and the diplomacy of Barbara A. Chernow.

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xii **PREFACE: ON THE USES OF BIOGRAPHIES**

SHAPIN, STEVEN; and THACKRAY, ARNOLD 1974 Prosopography as a Research Tool in the History of Science: The British Scientific Community 1700–1900. *History of Science* 12:1–28.

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ZUCKERMAN, HARRIET 1977 *Scientific Elite: Nobel Laureates in the United States*. New York: Free Press.

Introduction

This *Biographical Supplement* to the *International Encyclopedia of the Social Sciences* is the third link in an encyclopedic chain that began in 1930. Between 1930 and 1935, The Macmillan Company published the 15-volume *Encyclopaedia of the Social Sciences*, edited by Edwin R. A. Seligman and Alvin Johnson. Never revised, this classic encyclopedia is now available only in microfiche. In the 1950s, Alvin Johnson and other members of the social science community considered the possibility of revising the *Encyclopaedia*, but they preferred the idea of publishing an entirely new reference work that would reflect the expanding and changing nature of the social sciences. Finally, in the early 1960s, The Macmillan Company and The Free Press decided to publish a completely new encyclopedia. Published in 1968, the 17-volume *International Encyclopedia of the Social Sciences*, edited by David L. Sills, is a comprehensive summary of the state of knowledge in the social sciences. Like its predecessor, the *International Encyclopedia of the Social Sciences* has never been revised, although an unabridged eight-volume edition was published in 1978.

Because an encyclopedia reflects a generation's contributions to and perspectives on knowledge, it must be revised, updated, or supplemented if it is to maintain its intellectual credibility. In order for the 1968 *International Encyclopedia of the Social Sciences* to remain a useful reference tool, subsequent developments in the social sciences have to be incorporated. This supplement is the first step toward meeting that need.

Of the various supplements considered, the favorite, perhaps because of its manageability, was one composed entirely of biographies of eminent social scientists who had been too young in the mid-1960s to qualify for inclusion in the original volumes. The Free Press prepared an initial list of approximately three hundred biographical subjects that was circulated among some fifty social scientists. Their reactions to the proposal were largely enthusiastic, and they returned the lists with suggestions for additions, deletions, and possible biographers. With this information and this support from the academic community, work on the supplement began in earnest early in 1977.

Selection of biographical subjects

The editors determined that a one-volume supplement should contain approximately two hundred biographies. As anticipated, the

selection of the subjects was the most sensitive phase of the project. Personal and professional alliances and friendships influenced the recommendations of all of the scholars who willingly advised the editorial staff. Even after the final list of biographies was established, the same sensitivities affected the question of the relative importance of the biographies, and thus the comparative length of the articles.

The criteria for inclusion in the supplement were the same as for the original 17 volumes, which had included biographies of deceased social scientists as well as biographies of living social scientists past the age of 70, that is, who were born prior to 1891. Thus, to be considered for a biography in the supplement, a social scientist had to have died since the preparation of the original volumes or to have been born no later than December 31, 1908. By searching obituary notices in newspapers and professional journals, and by consulting more than two hundred social scientists in the United States and abroad—including all of the associate and field editors who served on the staff of the encyclopedia in the 1960s—the original list grew to almost five hundred names. In November 1977, an ad hoc committee of scholars met with the editors to amend and refine the list. The Free Press, however, made the final decisions concerning the contents of the volume, which includes 215 biographies.

The advisers divided the biographies into three categories: major figures, who made pathbreaking and sustained contributions to the social sciences; important figures, whose work was essential to the growth of their disciplines; and influential figures, whose research offered a representative cross-section of developments in the social sciences. The third category is particularly subjective, and we cannot claim that our selections are the only appropriate choices.

In addition, the supplement includes three biographies of social scientists whose biographies were omitted, for different reasons, from the earlier volumes. In four other instances, husbands and wives, whose work overlapped to such a degree that separate biographies would have been repetitive, received joint biographies. Finally, because of an error in our initial information search, one scholar who did not meet the age criterion was included, and the editors judged it inappropriate to delete the entry after commissioning the biography.

Selection of contributors

The criteria for contributors were knowledge of the biographees' work, high intellectual standards, and institutional and geographical representation (the 217 contributors to this volume come from 13 different countries). Based on our advisers' recommendations, we had ranked lists of biographers for each biography. In reviewing the directory of contributors and the alphabetical list of biographies for the supplement, the continuity from the *Encyclopaedia of the Social Sciences* to the *International Encyclopedia of the Social Sciences* to this supplement is striking. For example, Talcott Parsons, to whose memory the supplement is dedicated, contributed to all three works and is also the subject of a biography in the supplement; 18 social scientists who contributed to both encyclopedias are biographees in the supplement; 52 contributors to the *International Encyclopedia of the Social Sciences* are biographees in the supplement; and 50 contributors to the supplement were also contributors to the original 17 volumes of the encyclopedia.

As with any project involving so many authors and a tightly

followed schedule, a handful of contributors failed to deliver the manuscripts they agreed to write. The editors are grateful to those individuals who accepted these assignments at the last minute so that deserving biographees would not be omitted.

Preparation of the manuscript

All of the articles in the supplement are original and conform to the style rules that were developed in the 1960s for the publication of the encyclopedia. Contributors received specific instructions concerning the tone and organization of the biographies. The editors tried to maintain a balance between insisting on total uniformity of the contents of the biographies and on allowing the contributors so much flexibility that the volume might lose its unity.

In keeping with the encyclopedia's policy, the emphases in the biographies are the subjects' intellectual development and contributions to the social sciences. Thus, comparatively little biographical data is included in many articles. We urged contributors to discuss the subjects' intellectual surroundings and to analyze the intellectual climate during their university and professional years. Whose ideas were dominant during their formative years? Whom have they taught and influenced? With whom among their colleagues have they collaborated? To present a balanced picture, we encouraged contributors to present any major criticisms of their biographees' work as well as to analyze any schools of thought that grew up in support of or in opposition to their findings. Contributors were free to divide their biographies topically or chronologically if it helped to present the material in a more organized format. Each biography was reviewed by at least one specialist in the biographees' field before it was accepted. All of the contributors saw the edited versions of their manuscripts and had the opportunity to read their articles in galley.

Bibliographies

Perhaps the most frequently consulted feature of any reference work is its bibliographies. The bibliographies in the supplement were prepared by the contributors, but a staff of three bibliographers verified each item for accuracy and, where necessary, updated the entry or added useful information in the annotation. The bibliographies are divided into works by the biographee, which range from highly selective to almost complete listings, and supplementary entries, including readings about the biographee and his or her work.

Following the precedent established in the *International Encyclopedia of the Social Sciences*, the date of first publication is given for all works, but the publishing information refers, where possible, to the most recent, readily available, English language edition of each work. In this way, the historical development of the biographees' intellectual growth is indicated. Where later editions or reprint editions of works are available, the date of first publication is given in parentheses in the bibliography. In the text, the date of first publication is given for all works cited; if a citation specifically refers to a later edition, the date of first publication is in brackets. Information concerning translations, changes in titles of publications, and forthcoming works is available in the annotations.

Acknowledgments

Above all, this volume is a product of the community of social scientists. During the past two and one-half years, the editors have incurred special obligations to those individuals named in the list of advisers.

No such project is successful, however, without a professional support staff. Their efforts must be well-coordinated, their work accurately and quickly performed, and their patience endless. The staff, whose specialized knowledge and skills made this volume possible, includes Evelyn Geller, the copy editor, and Nancy E. Schroeder, Patricia Maughan, and Susan Copeland, the bibliographers. A more general debt of gratitude is due to the Columbia University Libraries and the New York Public Library for use of their facilities.

David L. Sills deserves a special thanks for his constant support of the idea of a supplementary volume of biographies and for his frequent advice at every stage. He participated in the process of selecting subjects and contributors; he read all of the articles in galley, searching for substantive and editorial problems; he prepared the "Preface" to the volume; and he wrote one of the longest articles—on the sociologist Paul F. Lazarsfeld.

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The classified list of biographies that follows offers readers a survey of the different fields represented by the social scientists discussed in the supplement. Every biography in the supplement has been classified at least once, despite the fact that not every biography fits easily into the categories listed below. It should be noted that the categories listed below do not correspond exactly to those listed in the "Classification of Articles" on pages 83–108 in volume 17.

Anthropology
Criminology
Demography
Economics
History and Philosophy of History
Human Biology
Linguistics

Philosophy
Political Science and Legal Theory
Psychiatry
Psychology
Religion
Sociology
Statistics

ANTHROPOLOGY

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A

ABEL, THEODORE

Theodore Abel was born in Lodz, Poland, in 1896, to a prominent industrial family. After completion of his Gymnasium studies in 1914, he spent four years as a journalist, while also publishing poetry and philosophical essays. He served in the Polish Army during the Polish-Russian War, 1918-1920, and as liaison officer to the American Young Men's Christian Association, 1920, and later director of its activities in Poland. From 1920 to 1923 he studied law and philosophy under Leon Petrazycki at the University of Warsaw, and philosophy and sociology under Florian Znaniecki at the University of Poznan. He emigrated to the United States in 1923 and, as a Gilder fellow, studied sociology under Franklin H. Giddings and William Fielding Ogburn at Columbia University, from which he received his PH.D. in 1929.

Abel began his academic career at the University of Illinois, where he taught from 1925 to 1929, and then returned to Columbia University, where he stayed from 1929 to 1951. In the latter year he became professor and chairman of the department of sociology at Hunter College of the City University of New York, a post he retained until his retirement in 1967. He spent post-retirement years teaching at the University of Notre Dame, the University of Waterloo (Canada), and the University of New Mexico. For many years he was a member of the board of directors of the Institute of Immigrant Welfare and served also during World War II as a member of the Hoover Commission for Polish Relief.

He was president of the Eastern Sociological Society during the year 1957/1958, and received its merit award in 1969.

Although Abel is thought of primarily as a sociological theorist, he made several contributions to sociological research. The first of these was a study of Polish immigrants in a small New England farming community, in which he discovered that immigrants to this rural area were assimilated more rapidly than those who settled in such urban communities as Chicago and Detroit, and that they also exhibited, in the second generation, less delinquency and crime (1929a).

The second, a study conducted under the auspices of the Institute of Social and Religious Research, was entitled *Protestant Home Missions to Catholic Immigrants* (1933). Protestant church leaders (especially Presbyterian, Baptist, and Methodist) had come to fear that the influx of immigrants from Catholic countries—Czechoslovakia, Hungary, Italy, Poland, Russia, and Mexico—would alter the Protestant character of American culture. Accordingly, they set up, especially in the cities where these immigrants settled in large numbers, more than a thousand "missions," ostensibly designed for charitable, social, and civic purposes, but actually concerned with the conversion of Catholic immigrants to Protestantism. By means of interview, schedule, questionnaire, and life history, Abel studied some 150 of these missions and discovered that their successes were insignificant. Only one in three hundred was converted and Abel was able to explore the reasons for the failure of the

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movement. The ties of the ancestral Church were too strong, especially because religious affiliation also meant loyalty to an ethnic group.

In this study, following the method developed by W. I. Thomas and Florian Znaniecki in their *The Polish Peasant* (1918–1920), Abel also experimented with the life history of mission workers and converts as a source of sociological data, and published in the book the autobiography of an immigrant minister. He expanded and refined this method in *Why Hitler Came Into Power* (1938). By offering cash prizes for the best personal life histories of those who joined the National Socialist Party before 1933, he was able to collect more than six hundred of them and used them to answer the question posed by his title. Rejecting both psychoanalytic and Marxist explanations of the rise of the Hitler movement, Abel concluded that a combination of factors was involved, including discontent with the place of Germany in the world after World War I, an ideology (including anti-Semitism) which emphasized the unity of the German *Volk*, the organizational skill of the party leaders, and finally the charisma of Hitler himself. Abel's six hundred life histories, six of which appear in the book, were later subjected to statistical analysis by P. H. Merkl, who discovered that anti-Marxism was a more important motive for early members of the party than anti-Semitism (Merkl 1975).

Abel's doctoral dissertation at Columbia University, *Systematic Sociology in Germany* (1929b), undertook to introduce to the English reader the work of Georg Simmel, Alfred Vierkandt, Leopold von Wiese, and Max Weber. More than an introduction, it was an effort to judge the success of each of these writers in establishing sociology as an autonomous discipline with a subject matter of its own. He judged their work in terms of four criteria of the validity of a scientific system: (1) an adequately delimited subject matter; (2) justifiable tasks of investigation; (3) a basis for the systematization of the subject matter; and (4) adequate methods of investigation. Incidentally, he rejected as unreal the argument as to whether sociology was a general or a special social science on the ground that it was both.

With respect to Simmel, Abel concludes that the basic distinction between form and content was untenable and that Simmel's procedure was philosophical rather than scientific. Simmel himself was unable to treat form in abstraction from content, and furthermore, adequate analysis of

a social situation requires a consideration of its historical setting. There is, in fact, an incongruity between his theory and his practice, one that diminishes the significance of his contribution to sociology.

Abel was similarly critical of Vierkandt. The phenomenological method that he proposed does not advance the establishment of sociology as an autonomous science. A method that concerns itself exclusively with mental states and "inner experiences" can result only in a descriptive psychology and can have little to do with sociological analysis. In any case, Abel was skeptical of the scientific validity of the phenomenological method. Like Simmel, Vierkandt is leading sociology in a philosophical and speculative, not a scientific, direction. Vierkandt has made no contribution to the methodology of sociology and his work indicates the way that sociology must not—need not—go if it is to be a science.

Von Wiese, on the contrary, struck a new note in German sociology when he abandoned introspective and phenomenological methods and adopted behavioristic and quantitative ones. Abel approves of von Wiese's insistence upon association and dissociation as the *specificum sociologicum*. Although von Wiese's efforts at quantification did not succeed and although he had mistaken views on the nature of measurement, Abel nevertheless gives him high marks for his contributions to a sociology that can be objective and scientific.

Weber escapes Abel's strictures and indeed receives his highest praise. Of the four writers Abel gives rather higher rank to Weber and von Wiese than to Simmel and Vierkandt, an evaluation that he was to change. His own preferences for an objective scientific sociology are clear. His control—not to say mastery—of the German literature in both philosophy and sociology, and his lucid exposition of the ideas of his four writers, made his dissertation one of the most useful texts of its time—a time when few of the German works had been translated into English.

In 1970 Abel published another book on theory, *The Foundation of Sociological Theory*. By "theory" Abel means the concepts used in the analysis and interpretation of data. These concepts are of two kinds, those that serve to classify and those that serve as variables in general propositions. Theories thus are the conceptual schemes designed to explain sociological laws. They vary greatly in generality, precision, testability, elegance, and predictive power, and they arrange themselves on a continuum with respect

to these attributes. After suggesting that the founders of the discipline in the nineteenth century had been either synthetic or clinical in their interests, he discovers a new approach, an analytical approach, in the writings of Durkheim, Simmel, and Weber, which complement one another. Accordingly, he devotes half of his book to a treatment of the analytical contributions of these three writers. He does not, however, treat the corpus of their work, but only those aspects of it that contribute to the growth of sociological theory. Abel contends that historians who consign them to different schools are wrong, and that, on the contrary, they were "collaborators" in the development of sociological theory and provided for it a single foundation. In order to reach this conclusion, Abel has to minimize the differences between them and emphasize the agreements.

Weber's nominalistic view of collective reality, for example, does not differ greatly from Durkheim's realistic view, especially when the more moderate statements of both authors are closely examined. Weber's *verstehende* sociology is not necessarily incompatible with Durkheim's positivistic sociology. Weber's *Wertfreiheit* is almost a paraphrase of Durkheim's admonition to treat social facts as things. Durkheim's forms of solidarity are Weberian ideal types, as are also his types of suicide. In the same way, similarities can be found between Durkheim and Simmel and between Weber and Simmel. All three thus shared a community of ideas which became, in turn, the foundation of modern sociological theory.

In the latter half of his book Abel returns to his distinction between the synthetic, the clinical, and the analytical approaches to sociology, and shows that writers commonly associated with the first of these—Auguste Comte, Herbert Spencer, Ferdinand Tönnies, Franklin H. Giddings, and Charles H. Cooley—anticipated, or made contributions to, the third. He goes on to offer brief discussions of the analytical contributions of such other writers as Vilfredo Pareto, Florian Znaniecki, Talcott Parsons, Pitirim A. Sorokin, George C. Homans, Robert K. Merton, and Leopold von Wiese, and in a different context, Paul F. Lazarsfeld, Stuart Dodd, and Robert M. MacIver. He is concerned, in a succeeding section, with the characteristics and types of sociological laws. In conclusion, he defends the status of sociology as a science.

Abel's contributions are not limited to his books. He published a number of separate papers

on sociological theory, the most influential of which was his paper on *Verstehen* (1948). Steeped in the tradition of German philosophical and sociological thought, and aware of the methodological significance of the latter, Abel was nevertheless attracted by the more objective and even behavioristic character of American sociology. Was there any way of reconciling the two? How are insight and "understanding" related to the "explanations" of the positive sciences? Abel addressed himself to these questions on a number of occasions.

In the paper mentioned, Abel first suggests that *Verstehen* by no means had its origin in such German writers as Wilhelm Dilthey and Weber. Indeed, traces of it are to be found in Giovanni Battista Vico and in Comte, the first of whom thought that men have a special kind of knowledge about themselves, and the second that we have a knowledge of human nature that is somehow different from our knowledge of the nature of the physical world. American writers too, such as Cooley with his "emphatic" knowledge, Znaniecki with his "humanistic coefficient," Sorokin with his "logicomeaningful method," and MacIver with his "imaginative reconstruction," all suggest that some special method or approach, denied to students of the natural universe, has been conferred upon those who inquire into the social universe. These writers, and others, have given it different names but all suggest that there is something there that attains the status of a method.

Unfortunately, these writers failed to specify in detail the operations to be performed in using the method, and this is a defect that Abel proposes to remedy. By specifying the operations involved, as he does with the help of illustrations, he shows how one is able to internalize observed behaviors and apply behavior maxims to them derived from general and personal experience. If statistical technique, for example, discloses a high correlation between crop yields and marriage rates, we say we understand the relationship because we have a behavior maxim which tells us that when people feel prosperous they are more willing to make commitments than when they do not. The "understanding" is the process of connecting the observation with the maxim. In other words, *Verstehen* brings to awareness and makes explicit the maxim that intervenes between our perceptions of stimulus and response. This is especially necessary when the observed behavior is neither routine nor commonplace and when the connections are

other than those we would expect. The steps involved in *Verstehen* are therefore three: (1) internalizing the stimulus; (2) internalizing the response; and (3) applying behavior maxims. The techniques by which we do this, however, are not themselves specified. They are matters of sympathetic imagination and of introducing our own personal experience into the situation. This is what happens generally in the imputation of motives. The source of behavior maxims is not in science but in personal experience. Thus, the operation of *Verstehen* is the application of personal experience to observed behavior.

The method, however, has certain limitations. Personal experience is variable and its quality depends upon the introspective capacity of the interpreter. The method suggests connections, but is unable to verify them. Furthermore, the connection is only possible. To determine its probability we need to utilize the more positivistic methods of experiment and comparison. *Verstehen* may give us an increment of personal satisfaction, but it adds nothing to the validity of the propositions we entertain. Abel concludes, therefore, that *Verstehen* cannot serve as a method of scientific analysis and cannot add to our store of knowledge.

Abel's conclusion was unanticipated. His title induced one to believe that by "operationalizing" *Verstehen* he would also validate it as a method. Although willing to concede that it relieves us of a sense of apprehension when confronted by the unfamiliar, he insisted nevertheless that it was merely a device for using knowledge that we have already acquired by personal experience. Responses by several critics induced Abel, in replies to them, to clarify his position, to distinguish between several different meanings of *Verstehen*, to emphasize again that the method was the chief source of hypotheses, and to underscore its role as an indispensable instrument in the study of social phenomena.

Although Abel did not himself construct a systematic sociological theory, his exposition and criticism of the theories of others—and especially his work on *Verstehen*—won him a recognized place in the history of the discipline.

ROBERT BIERSTEDT

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ADORNO, T. W.

Theodor Wiesengrund Adorno, Marxist philosopher, social theorist, and aesthetician, was born in Frankfurt am Main, Germany, in 1903. In the 1930s he joined the Institut für Sozialforschung (Institute for Social Research) which, under the direction of Max Horkheimer, developed what became known as the Frankfurt School of Critical Theory. After World War II, Adorno returned to Frankfurt from exile in the United States, and as codirector and later director of the institute, he was a leading theoretician of the German New Left until his death in 1969.

Adorno was the only child of a Jewish wine merchant and his Catholic, Corsican wife (née

Calvelli-Adorno), a professional musician. As a youth he studied piano with Bernhard Sekles (who also taught Paul Hindemith) and spent weekends reading Immanuel Kant with Siegfried Kracauer, his mentor. At 18, already the author of 2 published articles on musical criticism, he entered Frankfurt's new, liberal Johann Wolfgang Goethe University, receiving a degree in philosophy 3 years later. His dissertation (*Gesammelte Schriften*, vol. 1) criticized Edmund Husserl's phenomenology from the position of his idiosyncratic Kantian professor, Hans Cornelius, whose assistant, Max Horkheimer, became Adorno's lifelong friend.

In 1925 Adorno went to Vienna to study musical composition with Alban Berg, a disciple of Arnold Schönberg. The heyday of the Schönberg circle was over, but Berg brought Adorno into contact with Vienna's artistic avant garde. Adorno wrote (under the name Wiesengrund-Adorno) for radical aesthetic journals like *Musikblätter des Anbruch* (later *Anbruch*) and *Pult und Taktstock* in defense of Schönberg's atonal music. His early articles were not yet Marxist. They reflected Schönberg's theory of composing as a cognitive activity, a search for "truth" which proceeded by developing the potential of musical material and techniques as they evolved historically. They were also indebted to György Lukács' pre-Marxist study, *Die Theorie des Romans* (1920), which argued that aesthetic developments were closely connected to historical transformations of the structure of lived experience.

Adorno returned to Frankfurt in 1927 to prepare for an academic career in philosophy. He wrote quickly a *Habilitationsschrift* (the prerequisite) which made an academically daring attempt to legitimate Sigmund Freud's new psychoanalytic method on the basis of Cornelius' Kantianism. The conclusion contained a Marxist critique of the ideological function of non-Freudian theories of the unconscious which was philosophically incompatible with the idealism of the rest of his analysis. The study was not accepted.

Adorno needed a new philosophical base that would support both Freud and Karl Marx, and his friend Horkheimer shared his problem. They learned from the psychoanalyst Wilhelm Reich's Marxian argument that the social structure was reflected in "character structures." They were aided immensely by the dialectical interpretation of Marx provided by Lukács in *Geschichte und Klassenbewusstsein* (1923), which stressed

the relation between consciousness and society. The new "sociology of knowledge" formulated by Max Scheler, and by Karl Mannheim, who taught at Frankfurt, dealt with similar concerns, but because it maintained the bourgeois, idealist goal of absolute, unchanging truth, the social relativity of knowledge to which it pointed led them to cognitive despair. Adorno and Horkheimer rejected the idealist goal. Adopting Lukács' method of *Ideologiekritik* (critique of ideology), they argued that bourgeois idealism (including Adorno's own earlier Kantianism) bore the structural flaws of the capitalist mode of production: the concept of the "object" was fetishized and reified like a commodity; the predilection for abstract universals reflected the abstract formalism of market exchange; the separation of form from content paralleled the alienated conditions of wage labor that only the overthrow of class relations could eliminate. They considered critical awareness of this connection to be historically specific truth, which both logically and morally compelled a commitment to the revolutionary transformation of society.

Adorno and Horkheimer frequented a left-wing literary circle in Berlin, whose undogmatic reception of Marx was much like their own. The group included Walter Benjamin, Bertolt Brecht, Ernst Bloch, Laszlo Moholy-Nagy, and Gretel Karplus (whom Adorno married in 1937). Adorno was particularly impressed by Benjamin's highly original, esoteric, cognitive theories. He attempted to translate them into a "dialectical," "materialist" method of exegesis (*Gesammelte Schriften*, vol. 1) which he applied in a study of Kierkegaard (*Gesammelte Schriften*, vol. 2). As a critique of the founder of modern existentialism (the bourgeois alternative to rational idealism), this study was an oblique attack on Martin Heidegger, whose *Sein und Zeit* (1927) was hotly debated among Frankfurt academicians.

Adorno submitted the Kierkegaard study as a second *Habilitationsschrift*. It was accepted by Paul Tillich, and Adorno joined the philosophy faculty at Frankfurt. The same year (1931) Horkheimer became head of the Frankfurt Institut für Sozialforschung, a privately-funded, Marxist research institute founded in 1923, bringing to it a more dialectical, "Critical Theory." Erich Fromm, Leo Lowenthal, Herbert Marcuse, and Friedrich Pollock were then members; Adorno and Benjamin were closely connected. Scarcely two years later Hitler took power. In spring 1933, the institute was forced

into exile, eventually settling in New York. Adorno emigrated to England the following year. As a scholar at Merton College, Oxford, he again launched a major critique of Husserl, this time using the critical method he had developed in his Kierkegaard study.

Adorno's focus during the 1930s on the immanent critique of philosophy separated his efforts somewhat from the "Critical Theory" of Horkheimer's institute. The latter was primarily concerned with exposing the ideological function of bourgeois theory and replacing it with an innovative fusion of Marxian sociology and Freudian psychology. Adorno's articles for the institute's journal (*Zeitschrift für Sozialforschung*), "Zur Gesellschaftlichen Lage der Musik" (1932), "Über Jazz" (1936, under the pseudonym Hektor Rottweiler), and "Über den Fetischcharakter in der Musik und die Regression des Hörens" (1938) demonstrated that he was in full accord with the goals and assumptions of "Critical Theory." But his own program was not identical. His analysis of music as a bourgeois cultural phenomenon went beyond the critique of its ideological content. He insisted that bourgeois artists and thinkers, despite their personal politics (and against their own intent) expressed in the form of unresolved tensions in their work a veiled social criticism and latent utopian perspective which it was the task of the critical interpreter to uncover and illuminate.

In 1934 Adorno contributed to a pamphlet in honor of Schönberg's sixtieth birthday. His essay, "Der dialektische Komponist" (*Gesammelte Schriften*, vol. 16), argued that out of the immanent logic of the musical material, Schönberg had dialectically negated and transcended bourgeois tonality, and the resulting atonal revolution not only liberated music from its ideological social function, but provided a cognitive model for nondominative social structures. Adorno's goal was a parallel achievement in philosophy. His new study of Husserl aimed at transcending bourgeois idealism once and for all (written in 1934–1937, it was published in 1953 with a new introduction as *Zur Metakritik der Erkenntnistheorie* [*Gesammelte Schriften*, vol. 5]). He chose to attack Husserl's phenomenology as the most advanced stage of bourgeois idealism—in the sense of advancing decay. His tactic was to focus on the logical gaps, or "ruptures," of Husserl's phenomenology so that what first appeared as logical contradiction was seen as a reflection of social contradiction—not philosophic error, but material truth. By showing that socially con-

crete, historically specific reality permeated all Husserl's premises and categories—the autonomy of reason, the priority of thought, the ahistorical universality of truth—Adorno hoped to demonstrate as false the fundamental idealist assumption that such categories were independent of social history, and to prove that dialectical, materialist principles (the priority of matter, the necessity of a logic of contradictions) needed to be accepted in their place.

Adorno's debt to Benjamin. Adorno's interpretive method of making the social structure quite literally appear within the words of bourgeois texts was indebted to Walter Benjamin. The latter was inspired by an unlikely combination of sources: the romantic tradition of German literary criticism (Novalis, Schlegel, Jean Paul), the Jewish mystical tradition of the Kabbalah (to which he had been exposed by his close friend Gershom Scholem), a revolutionary but unorthodox Marxism, and an avant-garde aesthetics that drew on theories of cinematic montage, surrealism, and Bertolt Brecht's epic theater. In Benjamin's theory of cognition the extreme experiences of critical demystification and religious revelation converged. He focused on the seemingly insignificant details of historically transient, cognitively reified phenomena so that they provided "profane illumination" by being read as "ciphers" of social truth.

Neither Brecht nor Scholem appreciated the fusion of Marxist and mystical motifs. Only Adorno felt a strong appreciation for the philosophic power of what he called Benjamin's "negative" or "inverse theology." Yet the very affinities of their positions led to disagreement. Exiled in Paris during the period of the popular front, Benjamin adopted a political position close to the Communist party. Adorno, whose initial aversion to party control of intellectuals was greatly intensified by Stalin's purges, was dismayed by Benjamin's move, and he blamed Brecht's influence. They carried on a protracted debate by correspondence in the 1930s (Adorno 1970) which has come to be considered a major document of Marxist literary theory. Adorno argued that Benjamin was falling into philosophic as well as political error when he affirmed the actually existing consciousness of the working class and the mass culture that appealed to it, claiming it was as truncated as that of the bourgeoisie. Adorno held stubbornly to a position of "nicht mit-machen" (nonparticipation). He argued that because social antagonisms were embedded within cognitive problems, the

intellectual "labor" which unearthed them was a form of practice in itself.

Benjamin asserted that the new technologies of film production and mechanically reproduced art contained within themselves the power to transcend bourgeois cultural forms, because they robbed art of its uniqueness ("aura"), turned artists into technicians, and transformed the creation of art into a collective, directly political activity. Adorno complained that the static repetition of mechanical reproduction encouraged passivity, and stunted active, critical interpretation. He felt Benjamin's celebration of the new technological forces that liquidated art's aura was premature, given the distortions of class society. Art took the side of the oppressed, not by becoming directly political, but by positing, through its very autonomous existence, the possibility of a realm not identical to the given.

When Benjamin imitated the new technique of film montage in his own writings, juxtaposing literary fragments and historical images with the barest minimum of interpretive comment, Adorno warned that without the mediation of theory, the theological motif of "profane illumination" degenerated into magic, and the Marxist motif into positivism. He then turned his critical method on Benjamin, and claimed that these theoretical difficulties were themselves reflections of social antagonisms: Benjamin's fragmented style uncritically reproduced the general fragmentation of experience that made grasping the interconnectedness of reality so difficult.

Ultimately, Benjamin heeded Adorno's criticisms, and in fact documented Adorno's thesis of the disintegration of experience. He used historical evidence gathered for his study of nineteenth-century Paris (the *Passagenarbeit*) to argue (in ways later developed by Adorno) that the characteristics of modern urban life and factory labor (which *anticipated* the new technologies of film and arts reproduction)—fragmented, abrupt gestures, shocklike bombardment of the senses, collision of visual images, the disruption of spatial and temporal coherence—needed to be judged negatively because they weakened people's critical powers (Benjamin [1961] 1969, pp. 155–200).

Exile in the United States. In 1938 Adorno emigrated to New York, where the institute, despite limited funds, was a haven for German intellectual refugees. Adorno secured a position as director of music research for the Princeton "Office of Radio Research" headed by the Aus-

trian social scientist Paul F. Lazarsfeld. He began to analyze the "social physiognomy" of radio music, characteristically interpreting the antagonisms of the social whole from out of this particular detail of modern experience. It soon became evident, however, that the critical, theoretical nature of his approach was incompatible with the project's market-research concern for empirical data, such as the "likes and dislikes" of the listening audience, and funding for his work was terminated in 1939.

Adorno then joined Horkheimer in Los Angeles. The war years were their period of closest collaboration, and it marked a new, more pessimistic stage of "Critical Theory." They accepted the argument of institute economist, Friedrich Pollock, that "state capitalism" had developed in Soviet Russia, similar in bureaucratic structure to the interventionist state of Roosevelt's New Deal. They maintained such structures were inherently authoritarian. If, in the earlier period, they saw reification as the main ideological obstacle to critical consciousness and critical reason as the path to overcome it, now, in the dark years of Auschwitz and Hiroshima, their far greater concern was passive submission to authority, and they began to see reason itself as a form of domination. They saw their intellectual work not as an anticipation of social revolution, but as a struggle merely to keep critical consciousness alive.

Benjamin committed suicide in 1940, when he was denied exit from France at the Spanish border. His last text, written after his political disillusionment over Stalin's pact with Hitler, was a series of theses on the philosophy of history (Benjamin [1961] 1969, pp. 253–264) that argued that history did not equal progress, even on the level of culture. Instead, progress was subjective intervention in order to stop the historical course of events, an action that under current conditions was only a utopian and theological hope.

The new pessimism marked a break from the radical-liberal tradition. Adorno and Horkheimer worked closely to develop its implications for social theory. They analyzed the totalitarian tendencies common to political structures of fascism, late capitalism and state capitalism, and cognitive structures of authoritarianism, anti-Semitism, and cultural conformism, which, they argued, resulted in the "extinction of the ego," the impotency of subjects in a totally "administered world." They wrote a critical history of reason as a "dialectic of enlightenment." Its

thesis was that reason, which was meant to demythify the world and liberate human beings, had itself turned into myth. Tolerating nothing outside, or "nonidentical" to itself, it provided a new tool, not only for social oppression, but for the domination of nature, including the human body. It legitimated psychological, sexual, and physical repression.

As a method of positivist research that accepted "given" empirical data without question, but also as the ground for consensus rather than critical politics, reason had become an instrument of conformism. In this sense, its ideological function paralleled that of mass culture. *Dialektik der Aufklärung* (*Gesammelte Schriften*, vol. 3) included a theory of the "culture industry" which argued that, as a commodity, modern culture (high-brow and popular alike) was caught up in structures of domination and internalized their authoritarian forms. The new technologies of mass media were "enlightenment as mass deception": purporting to entertain, they promoted obedience among their passive audience; purporting to provide perpetual novelty, they supplied a repetition of the ever-identical. Adorno and Horkheimer argued that the phenomenon of anti-Semitism was closely linked to both dominative reason and mass culture because it manifested the same intolerance and fear of the "nonidentical," the same lack of imagination, the same inability for autonomous, critical cognitive experience.

Dialektik der Aufklärung was a departure from Marxist social theory. Nonetheless, for Adorno its very pessimism concerning the historical development of culture marked a concession to Marxist principles, as he no longer attempted to affirm revolutions in the superstructure which left the substructure of oppression unchanged. In a long essay (which provided Thomas Mann with the aesthetic theory for *Doktor Faustus*, *Philosophie der neuen Musik* [*Gesammelte Schriften*, vol. 12]), Adorno interpreted Schönberg's twelve-tone system of composing critically: the narrowness and rigidity of its rules had reenslaved music, a dialectical reversal of atonality's temporary liberation from bourgeois forms. Adorno strove in his own writings to avoid the systematizing tendency inherent in conceptual theoretical writing. In *Minima Moralia* (*Gesammelte Schriften*, vol. 4), he expressed social reflections in short, dialectical fragments.

The Authoritarian Personality. In the late 1940s, Adorno again became involved in a pro-

ject that necessitated translating his method into empirical social research, a task he compared to squaring a circle. As part of a series, *Studies in Prejudice*, edited by Max Horkheimer and funded by the American Jewish Committee, he worked in collaboration with social psychologists R. Nevitt Sanford, Daniel Levinson, and Else Frenkel-Brunswik (the "Berkeley Public Opinion Study Group") to develop an experimental method for investigating anti-Semitism. The results and their interpretation, published as *The Authoritarian Personality* (1949), became highly controversial (see Christie & Jahoda 1954).

The theoretical frame of the study drew heavily on *Dialektik der Aufklärung*, where Adorno and Horkheimer had argued that anti-Semitism, which misdirected protest against economic injustice by making Jews the scapegoat, was only one element of an "authoritarian character structure," and this in turn had its origins in objective social conditions. The characteristics of this authoritarian type included passivity, conformism, rigidity of thought, a tendency to stereotype, lack of critical reflection, sexual repression, and a fear and abhorrence of anything "nonidentical." The research problem was to design a questionnaire that would reveal the presence of these traits as an interconnected structure. The innovative solution bore great similarities to Adorno's "social physiognomics." The Berkeley group interpreted out of the details (in this case, the questionnaire items) the general character structure which was unintentionally revealed (the latent, rather than manifest meaning of the responses). The items were evaluated on an "F-scale," and clusters of high-scoring responses were read as symptomatic of "fascist tendencies." The antifascist character structure, which was critical, nonconformist, and open, had, predictably, more diverse characteristics, tending to elude the typing procedure altogether. As a counter to the quantitative results of the F-scale, some in-depth interviews were made for "content analysis." Adorno assumed the task of interpreting these interviews, using the close, textual analysis typical of his critical exegesis.

Return to Germany. In 1950, the city of Frankfurt invited Horkheimer to reestablish the institute at the university, and Adorno returned with him as codirector. For the next two decades he taught, guided institute research projects, and wrote prolifically on aspects of social theory, philosophy, and aesthetics. "Critical Theory"

played a significant role in the intellectual reconstruction of Germany, providing students with a Freudian–Marxist framework for the analysis of fascism, and with a critical rereading of the German Enlightenment tradition. The fact that the institute was anti-Soviet as well as antifascist allowed it to flourish in the cold war climate. “Critical Theory” remained committed to democratic ideals. However, it condemned as “pseudodemocracy” the superimposition of egalitarian forms onto a hierarchical, class-structured society. Moreover, due largely to Adorno’s American experience, the institute resisted “market research” methods of social science and Anglo–Saxon empiricism in philosophy, both of which were influential in Germany as the intellectual accompaniment to military occupation. Hence (and because of its critique of mass culture), “Critical Theory” resisted Allied hegemony as well.

The positivist dispute. “Critical Theory” remained revolutionary in its orientation no matter how great its distance from orthodox Marxism. A 1961 debate on method between Adorno and Karl Popper at the German Sociological Association meeting in Tübingen hinged precisely on this point. The debate, which was continued by Adorno’s younger colleague, Jürgen Habermas, and Popper’s German student, Karl Albert, in 1963, spread to academic circles generally, where it became known—Popper felt incorrectly—as the *Positivismusstreit* (“Positivist Dispute”; see Adorno et al. [1969] 1976). It was essentially a debate between a liberal and a radical critique of contemporary cognition.

On the surface, Adorno’s “Critical Theory” and Popper’s “critical rationalism” had much in common: both opposed specialization and bureaucratization of scientific inquiry; both criticized closed thought structures and total systems; both deplored the reduction of reason to an uncritical technical instrument. Their real differences were political. Popper felt that a free, competitive debate by a “community of scientists” was possible within Western society and would lead to theoretical coherence; Adorno felt that such debate was inevitably distorted by the dominant economic and social structures in which it took place, and theoretical coherence served an ideological function by masking social contradictions. Because Adorno considered social liberation the precondition for a reconciliation of theoretical contradictions, he believed his position was allied with the emancipatory interests of the exploited class. Ironically, Popper, in

attacking Adorno’s esoteric language and the abstruse Hegelian legacy in “Critical Theory,” saw himself as championing an antielitist, democratic position.

Negative dialectics. For Adorno, the key political problem was that reason was not applied critically, against the grain of given reality. Instead, thought either dominated reality (idealism), or reality dominated thought (empiricism), and when either pole was posited as a philosophic first principle, thought was led into complicity with the status quo. The key theoretical problem was to devise a method of dialectics without identity, in which the tension between thought and reality remained unresolved. In 1966, as the title of his major philosophic work, Adorno named this method *negative* dialectics, to distinguish it from Hegel’s idealism (*Gesammelte Schriften*, vol. 6). On the one hand, it was a *method of argument* based on the non-identity of concepts to their objective content (for example, of reason to reality) on several levels: (a) the object did not live up to its concept (“rationalized” society was irrational); (b) the concept did violence to the object it identified (reason grasped reality by dominating it); (c) only contradictory concepts (reason—myth) could define the object (reality) which was in fact contradictory. On the other hand, negative dialectics was a *method of exegesis* based on the nonidentity between the meaning of a text and the intent of its author: When texts were interpreted by dialectical argument they were made to bear witness against themselves, so that subjective, cultural, cognitive products could be read as ciphers of the objective, social totality.

The unresolved nature of negative dialectics prohibited “Critical Theory” from becoming a systematic social theory. Adorno did not, for example, attempt to reconcile Freud and Marx, as he believed such a synthesis mitigated the critical potential of both. Instead, Adorno used their theories contrapunctually: he interpreted psychological phenomena as reflections of the socioeconomic structure, but he analyzed the socioeconomic structure in terms of the mental phenomena (culture, cognition, character structure) that maintained it.

Adorno applied the same sophisticated method in interpreting the phenomena of mass culture. He has been criticized (Shils 1957) for failing to appreciate mass culture’s democratic characteristics. Yet he took jazz, radio listening, or newspaper horoscopes with the same philosophic seriousness as a Heideggerian text, and

believed that when they were read critically, they were no less capable of illuminating social truth.

In order to understand Adorno's "Marxism," it is necessary to realize that for him, always, dialectical materialism was an epistemological method compelled by the material, independent of any class viewpoint. Uniformity of critical insight resulted because of the uniformity of the social object, not the collective nature of the cognitive subject.

Adorno's stress on the individual rather than the class nature of experience, coupled with his contempt for existing working-class consciousness, brought him criticism from more orthodox Marxists of his generation. Lukács accused him of lamenting the decline of bourgeois culture from a Grand Hotel abyss replete with bourgeois comforts; Lucien Goldman protested that revolutionary art articulated, not domination or contradiction generally, but class oppression and class conflict specifically; Hans Eisler claimed that Adorno's aesthetic theory was metaphysical and ignored the fact that art was made by and for people.

Yet Adorno's fear of the authoritarian moment in collective mentality had historical justification, and if his language was elitist, his motive was radically democratic. Adorno's incessant negativity, which Kracauer criticized as infinite dialectics, aimed at escaping systematization for a political as well as a philosophical purpose. Each of his essays was a unique cognitive experience, a one-time-only configuration of elements that had a pedagogic goal of showing people what autonomous thinking was, and of encouraging them to think for themselves by frustrating attempts to pigeon-hole knowledge through easy categorizing and definitions. His insistence on the historical transiency of truth meant that interpretation needed constant revision, so that no texts could be treated as dogma, including his own. The open, nonauthoritarian, nonreified structure of negative dialectics was the antithesis of bourgeois cognition. It thus provided an anticipatory model for nondominative relations in a new society.

The German student movement responded to the events in Paris of May 1968 with a series of strikes and demonstrations. Adorno was skeptical of the objective possibility of revolution and maintained his stance of nonparticipation. Students disrupted his lectures, challenging him for not being radical enough. They accused "Critical Theory" of leading to a dead end, be-

cause it left no space for revolutionary praxis (Schoeller 1969). Adorno died in August 1969, at the height of the disturbances, and the institute dissolved shortly afterwards.

SUSAN BUCK-MORSS

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ALBRIGHT, WILLIAM F.

William F. Albright (1891-1971), a scholar of the twentieth century in ancient Near Eastern

and Biblical studies, was born May 24, 1891, in Coquimbo, Chile, the son of Methodist missionaries. He was handicapped by poor eyesight—a lifelong burden—and by a hand crippled in an accident, and suffered the prejudice against foreign Protestants in a regressively Catholic environment. When the family returned to the United States, he attended Upper Iowa University for formal instruction in Latin, Greek, and mathematics. He taught himself Biblical Hebrew and Akkadian, the language of the ancient Babylonians and Assyrians.

So far largely self-taught, Albright was able to undertake graduate study at the Johns Hopkins University in 1913. He remained six years, continuing Semitic languages, ancient history and Biblical studies, Egyptian, and especially Akkadian under Paul Haupt, who had come from Göttingen to teach at the newly-founded Hopkins in 1883 as the first Assyriologist in the United States. Haupt, whose later interest turned more and more to Biblical studies, also brought from Germany a particularly radical critical approach to Old Testament studies.

After this exposure to the broadest and most vigorous training on the German model available in his time, Albright entered a new phase, with concentration on field archeology, as fellow and then director of the new American School of Oriental Research in Jerusalem (1919–1929; 1933–1934). In the Near East, Albright traveled and explored tirelessly, studied Arabic and modern Hebrew, and excavated, especially at Tell Beit Mirsim, Tell el Ful, and Bethel. In 1927 he joined the faculty of the Johns Hopkins University, and headed its Oriental Seminary until his retirement in 1958. Under Albright, the department was a dominating influence, especially in American Biblical studies and Palestinian archeology. His students came to occupy posts at numerous American theological seminaries and universities, notably Harvard, which under Frank Moore Cross, Jr., in Old Testament studies, and G. Ernest Wright in Palestinian archeology and Biblical theology, together with other Albright students, colonized what Albright called the “Baltimore school” at Cambridge.

Although Albright worked throughout his life at a synthesis of the early history of human thought and belief, it is perhaps not unfair to say that his resolute efforts as a philosophical historian, whatever their ultimate value, had less to do with his contemporary influence than did a number of other factors. Albright impressed people because he knew and achieved

so much over so broad a span. His establishment of the ceramic chronology of ancient Palestine, the main means for dating archeological remains, survives, with modifications, to this day and will doubtless remain fundamental in its essentials. Frank Cross has characterized Albright as a master of typological method and has shown how the essentially scientific methods he used in classifying pottery types into groups and studying their change in regular fashion over time were successfully used by him elsewhere: in linguistics (e.g., 1934); in paleography (his study of the Nash papyrus in the *Journal of Biblical Literature* [1937] was a cornerstone in dating the Dead Sea Scrolls); in the typology of Hebrew poetic forms (1968); and so on. His memory for detail was fantastic and his self-confidence boundless. He was quick to grasp the significance of new discoveries and their impact on old syntheses. These qualities of erudition and imagination do more to account for the eagerness of Jewish and Christian students of all sorts to study with him than does his defined philosophical position. Moreover, his acquaintance with all kinds and conditions of men, his eager appreciation of Jewish scholars and their work, and the conversion of his wife, in Palestine, to Roman Catholicism, combined to give him a broad and deep sympathy with students and scholars of all religious persuasions.

Yet a portion of Albright's influence must be said to rest on a tendency to conservatism in religious matters that, in his own account, arose early in his career, and that is expressed in *From the Stone Age to Christianity* (1940) and *History, Archaeology and Christian Humanism* (1964).

Under the influence of his Assyriologist teacher, Paul Haupt, Albright was early led to a partial espousal of the “pan-Babylonian” point of view, according to which much of what is valuable in ancient and particularly Israelite civilization had its origin in Mesopotamia. Far from being led to discredit Israelite religion, however, Albright turned in a different direction. Nineteenth-century German Old Testament studies, culminating in the work of Julius Wellhausen, had led to a rewriting of Israel's religious history in which monotheism was held to have come late into Israelite thought. Albright felt strongly that Wellhausen had unjustly cut Israel off from the great civilizations of the ancient Near East, and, perhaps unfairly, he charged Wellhausen with imposing on Israel's religious development an arbitrary and impos-

sibly compressed evolutionary scheme. (Note, however, that Albright described himself, correctly, as an evolutionist, and, like Wellhausen, always held to a version of classic source-criticism of the Pentateuch.) Albright, instead, came to insist on the substantial historicity of Mosaic tradition and thus the antiquity of Israelite monotheism.

The epochal change from many gods to the one God who is over all was due, in Albright's mind, to the intuitive discovery that the incongruities of polytheism flouted the empirically recognized unity of nature. In praising the Old Testament as a masterpiece of "empirical logic," Albright was introducing a middle term between Lucien Lévy-Bruhl's "prelogical" and "logical" thought. Empirical logic, born of experience, Albright believed, enjoys some advantages over formal logical systems, and thus the achievement of ancient Israel is of permanent human significance. Albright's apparent return to conservative opinions, and his optimism that continued recovery of the ancient Near East would enhance the significance of the Bible, account for part of his fame and some of the misunderstanding and abuse of his pronouncements by fundamentalists. Through his published work and his many students, he remains an important influence, even if the synthesis he sought remains in the remote distance.

DELBERT R. HILLERS

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ALLPORT, GORDON W.

Gordon Allport is generally recognized as the chief pioneer in the development of a psychology of personality that emerged in the United States after World War I. His brother, Floyd H. Allport, was earlier acclaimed the founder of modern social psychology, establishing it as a scientific discipline distinct from social psychology as a branch of sociology. Eventually Gordon Allport also became preeminent in social psychology, devoting approximately half of his efforts to that field.

Beginning in the 1920s, the fields of personality and social psychology developed as one broad academic discipline with vague boundaries, rather than as two separate areas of study. When the American Psychological Association (APA) created special interest groups, the division of personality and social psychology soon became the largest branch. The Allports had much in common with Gardner Murphy, a third pioneer in personality and social psychology, during his early career. Although Murphy attended Harvard University, where the Allports studied, for only one year, all three were students of Hugo Münsterberg, Herbert S. Langfeld, and Edwin B. Holt. All were influenced, in one way or another, by William McDougall, the first psychologist after Wilhelm Wundt to write a major treatise on social psychology. Each worked first on personality; each was chairman of the Society for the Psychological Study of Social Issues; and each was awarded the gold medal of the American Psychological Foundation, which is presented annually to an American psychologist in recognition of an outstanding career.

Gordon Allport was a severe critic of Freudian theory, radical behaviorism, and theories of personality based largely on clinical observation of abnormal behavior. Yet in 1951, when the APA's

division of clinical and abnormal psychology asked practicing clinical psychologists to name the personality theorist who had influenced them most in their daily work, Gordon Allport was second only to Sigmund Freud (Schafer, Berg, & McCandless 1951).

Gordon W. Allport, the son of John Edward Allport and Nellie Wise Allport, was born in Montezuma, Indiana, November 11, 1897. His father was a physician, and his mother had been a schoolteacher. She provided a strong religious background, as did his maternal grandmother, who had been a "preacher" in a Free Methodist church. Gordon and Floyd Allport sometimes experienced inner conflicts as a result of revivals and religious conversions, but as adults they showed more interest than most psychologists in the role of religion in human life. After attending school in several small towns and graduating from a Cleveland, Ohio, high school, Gordon Allport entered Harvard in 1915, where Floyd Allport had received his bachelor's degree the previous spring (G. Allport 1967; F. Allport 1974).

Gordon Allport's first course in psychology was taught by Münsterberg, who also directed his brother's doctoral research. Langfeld trained him in experimental psychology, and Holt introduced him to epistemology and the history of psychology. James Ford in the department of social ethics interested him in social service and secured part-time employment for him working with foreign students, finding housing for industrial workers, acting as a probation officer, and supervising a boy's club. He served briefly in the Student Army Training Corps, but returned to his regular studies in time to graduate from Harvard in 1919.

Torn between his interests in teaching and social service, Allport taught for a year at Robert College in Constantinople [now Istanbul]. On the way home, while visiting his brother Fayette in Vienna, he called on Freud, who failed to make him a disciple.

At the age of 22, Allport began graduate work in psychology at Harvard. He received his PH.D. two years later for a thesis that he believed was "probably the first American dissertation written explicitly on the question of component traits of personality" (Allport 1967). While a graduate student, he assisted his brother, then an instructor at Harvard, in editing the *Journal of Abnormal Psychology*, and his first publication was a product of their joint efforts (Allport & Allport 1921).

A Sheldon traveling fellowship enabled Allport to spend two years in Germany and England, studying under Carl Stumpf, Max Dessoir, Max Wertheimer, Wolfgang Köhler, Eduard Spranger, Erich Jaensch, Heinz Werner, and William Stern. Thus he learned the personalistic and gestalt theories that characterized his later work, and established personal relationships that years later enabled him to assume leadership in bringing to the United States many European psychologists no longer able to pursue careers under the Nazi régime.

In the fall of 1924 he began teaching at Harvard, not in psychology proper, but in social ethics. It was in that department, encouraged by its chairman, Richard C. Cabot, that he first taught "Personality: Its Psychological and Social Aspects," which he claimed was probably the first course in this subject taught in an American college (Allport 1967). Cabot was also a professor of cardiology in the medical school and the principal pioneer in medical social work. His influence on Allport's life and career was at least as important as that of any of Allport's teachers. Since Langfeld had moved to Princeton University, and his brother, Floyd, to the University of North Carolina, Allport felt isolated from psychology. Fortunately, he overcame this handicap by volunteering to assist Edwin G. Boring, Langfeld's successor, and later McDougall, in their respective introductory courses.

In the spring of 1928, Allport married Ada Lufkin Gould, a graduate student at Radcliffe College, who became a clinical psychologist. A year later he accepted an assistant professorship at Dartmouth College, where he taught courses in personality and social psychology and made plans for a book on personality. In 1928, in collaboration with his brother, he published the *A-S Reaction Study*, an inventory designed to measure the trait of ascendance-submission. After that, the brothers seldom collaborated, though they corresponded frequently on professional matters.

In 1928, after McDougall had moved to Duke University, Boring invited Allport to return to Harvard as assistant professor of psychology, but it was not until 1930 that he returned to Cambridge. Several of his Dartmouth students came to Harvard for graduate work. Among them were Leonard Doob, Hadley Cantril, with whom he collaborated on *The Psychology of Radio* (1935), and Henry S. Odbert, with whom he published a list of nearly eighteen thousand terms used to describe personality and behavioral

characteristics (1936). Early in the 1930s, he and Philip E. Vernon designed *A Study of Values* (1931), a personality test, and conducted research that led to *Studies in Expressive Movement* (1933).

In 1935 Allport joined Cabot and others in establishing the Cambridge-Somerville youth study, of which Edwin Powers and P. S. deQ. Cabot were executive directors. Three hundred delinquent boys under various kinds of treatment for three to seven years were matched with an equal number of "control" delinquents not under treatment in an attempt to discover which treatments would be most effective in preventing delinquency. Unfortunately, World War II interfered with the project, and the results, though encouraging, were not conclusive.

In the mid-1930s, Allport devoted seminars to the use of personal documents and the question of the way life histories should be written. John Dollard had contended that the individual should be treated "as a specimen in a cultural series" (1935), but Allport countered that for psychological purposes, at least, every person should be regarded as unique (1942), and he interested many students in biographical studies.

Allport was also influenced by a study conducted in the late 1920s by Floyd Allport, Daniel Katz, and Margaret Jenness, in which they carried out the first major systematic program of research on social attitudes (Katz & Allport 1931). This led Gordon Allport to make *attitude* a fundamental concept in his theoretical system. In a chapter on attitudes in Carl Murchison's *Handbook of Social Psychology* (1935), he defined "attitude" as "a mental and neural state of readiness, organized through experience, exerting a directive or dynamic influence upon the individual's response to all objects and situations with which it is related." In discussing the genesis of attitudes, he postulated four common conditions for the formation of attitudes: (1) accretion of experience through the *integration* of numerous responses of a similar type; (2) *individuation, differentiation, or segregation* of action patterns that will supply him with adequate preparation for the direction of his adaptive conduct; (3) dramatic experience or *trauma*; (4) *imitation* of parents, teachers, or playmates (Allport 1935).

Personality: A Psychological Interpretation (1937), the book Allport had been planning since he was a graduate student, appeared in the same year as Ross Stagner's textbook, *Psychology of*

Personality. With these books available, courses in personality were introduced in colleges throughout the country. Allport defined personality as "the dynamic organization within the individual of those psychophysical systems that determine his unique adjustments to his environment." Though well aware of the general laws concerning the behavior of all hominids, he sought principles that would help in understanding the uniqueness of individuals (Allport 1968b; Hall & Lindzey 1957). "A general law," he wrote, "may be a law that tells how uniqueness comes about" (Allport 1937, pp. 194, 558). One of the best examples of such laws occurs in his treatment of motivation. Beginning with the motives of infants, he grants that any theory of motivation must take into account biological needs and innate patterns of behavior (e.g., reflexes). But, he argues, no listing of infantile behavioral tendencies is adequate to account for adult behavior: "The dynamic psychology proposed here regards adult motives as infinitely varied, and as self-sustaining contemporary systems growing out of antecedent systems, but functionally independent of them" (Allport 1937, p. 194). In essence, this is his principle of the functional autonomy of motives. A classical example concerns a sailor who in early life works hard to make a living, i.e., to buy food, shelter, clothing, etc. Thus he comes to love the sea. Later, after engaging in commerce in a large city, he returns to the seaside for recreation. The "biological" drives for food and shelter are no longer his principal motives. In discussing motivation, Allport emphasized aims, ambitions, and goals rather than postulating innate drives and their modification through learning and experience (Allport 1937; 1961; 1968b).

In his *magnum opus* (1937) Allport insisted that a concept such as *self* or *ego* was essential in an adequate theory of personality. Years later he substituted *proprium*, a term proposed by Emanuel Swedenborg two hundred years earlier. For Allport, the word refers to "the aspects of personality that seem peculiarly one's own and make for individuality and inward unity." Kinesthetic, organic, and proprioceptive sensory processes are important components. To discriminate appropriate from nonappropriate behavior and awareness, Allport suggested that one first pay attention to swallowing one's saliva (appropriate), and then imagine expectorating saliva into a tumbler and drinking it, which is nonappropriate (Allport 1955; 1961; 1968b).

Allport saw himself as a social activist. At times this involved personal risk. During the depth of the depression of the 1930s, when many assistant professors were dropped from the Harvard faculty, Allport, who was being considered for a tenured appointment, publicly criticized the president's and fellows' action in some of the dismissals. He joined the American Federation of Teachers and actively recruited members.

During the 48 years when one or both of the Allports served on its staff, the *Journal of Abnormal and Social Psychology* played an important role in the development of personality and social psychology. Founded in 1906 by Morton Prince, its title was expanded to include social psychology while Floyd Allport, who was consulting editor from 1921 to 1949, was acting editor early in the 1920s. Under Gordon Allport's editorship (1938–1949), the journal soon became the leading scientific periodical in personality and social psychology, while still devoting considerable space to abnormal psychology. During many of the remaining 17 years of his life, Gordon Allport was its most active consulting editor, aiding and encouraging his former student, M. Brewster Smith, as well as Arthur Jenness and Daniel Katz, Floyd Allport's former students, in their editorial duties.

In 1938, when Gordon Allport became chairman of the department of psychology, he carried a heavy load of administrative responsibility and social action. He was chairman of the APA's committee on displaced foreign psychologists, which helped to bring many European psychologists to North and South America, among them William Stern, Kurt Lewin, Heinz Werner, and Egon and Else Brunswik. He was also concerned about "displaced" American psychologists who opposed the entry of the United States into World War II. When George Hartmann, vice chairman of the War Resisters' League, and later head of the militant Peace Now movement, was dismissed from Columbia University Teachers College, Allport arranged to have Hartmann appointed director of research at the Cambridge–Somerville youth study and visiting lecturer at Harvard.

Toward the end of World War II, Allport edited a statement by himself, Gardner Murphy, and others, signed by more than two thousand psychologists and social scientists, suggesting steps that should be taken to insure a just and lasting peace. Several of the items in this "Psy-

chologists' Manifesto" were adopted as official policy in military government—e.g., "liberated and enemy peoples must participate in planning their own destiny" (Allport 1945).

Allport was president of the APA in 1938. He was also a member of the division of anthropology and psychology of the National Research Council, as well as of the APA's emergency committee on psychology. During World War II, he and Henry A. Murray offered a morale research seminar. With Robert H. Knapp and Leo Postman, Allport conducted a syndicated "Rumor Clinic," sponsored by the *Boston Traveler*. The attempt was not only to "scotch" harmful rumors, but also to lessen prejudice and antagonism that might hinder the war effort and hurt individuals, particularly those in minority groups. Allport sponsored a pamphlet, *The ABC's of Scapegoating* (1944), that explained some prejudices as attempts to avoid responsibility for one's own faults by projecting the blame on others, particularly in the case of anti-Semitism. Floyd Allport, who had operated a rumor clinic at Syracuse University, questioned the importance of the scapegoat hypothesis, citing experiments by his student, Nancy Morse, that failed to support it (Morse & F. Allport 1952; F. Allport 1974).

In *The Psychology of Rumor* (1947), Allport and Postman proposed that "the amount of rumor in circulation will vary with the importance of the subject to the individuals concerned *times* the ambiguity of the evidence pertaining to the topic at issue." The formula for this is $R \sim i \times a$. To demonstrate what *might* happen in a rumor, though no *actual* rumor was involved, they would ask a subject to describe the details of a picture projected on a screen. A second person, listening but not seeing the picture, would repeat the details *as accurately as he could* to a third person, and so on, until six or seven subjects had passed the information orally without having seen the picture. Conclusions from some thirty of these demonstrations were that the "rumor" tended (1) to become shorter, more concise, more easily grasped and told (*leveling and sharpening*); (2) to be *assimilated* to the emotional state of the listener; and (3) to be *elaborated* or shifted in theme.

Allport also concluded that prejudice was an important element in many rumors. Allport agreed with Thomas Aquinas that the general nature of prejudice is "thinking ill of others without sufficient warrant," but he noted that

prejudice may also be favorable; nevertheless, it should seldom be condoned. Perhaps the most important finding of his many publications on the subject was that prejudice tends to diminish whenever members of different groups meet in terms of equal status in the pursuit of common objectives. Among the strategies and devices for reducing or eliminating prejudice to which he called attention were role playing, group retraining, exhortation, catharsis, and legislation to control or limit prejudiced behavior.

Later, when Allport realized that the concepts and techniques of psychology alone were too limited to provide adequate prediction and understanding of prejudice, he proposed that the "causes" be classified as (1) historical; (2) sociocultural; (3) character, personality, and perceptual; and (4) characteristics of the *victims* of prejudice (Allport 1950*b*; 1960*b*, chapters 14, 15; Evans 1970).

During the war, Allport developed closer relationships with sociologists and cultural anthropologists that led the social and clinical psychologists to leave the department of psychology in 1946 to join the sociologists and cultural anthropologists in a new department of social relations, chaired by Talcott Parsons. Allport was chairman of the committee on higher degrees, and with George C. Homans, he offered an introductory course on social relations. In 1966, having reached mandatory retirement age, Allport was appointed the first Richard Clarke Cabot professor of social ethics. After his death, many members of the staff retired or went elsewhere. Eventually, social relations ceased to exist as a separate department.

In 1956, Allport went to Durban, South Africa, to deliver a memorial lecture sponsored by the South African Institute of Race Relations in honor of Alfred Hoernlé, who had been his teacher at Harvard. While in Africa, he and Thomas F. Pettigrew carried on experiments that seemed to indicate that social factors were most effective in perception when the stimulus was ambiguous. Using the Ames rotating trapezoidal window apparatus, which in our civilization usually creates the illusion that a window frame is swaying back and forth instead of rotating, they found that Zulu children who lived in rectangular houses with rectangular windows responded much as do normal subjects in the United States. Zulu children who lived in round houses with no windows in remote areas did not experience the illusion. Other more complicated experiments with persons of differ-

ing ethnic characteristics demonstrated subtle differences in perceiving paired passport photographs of persons of different ethnic groups exposed briefly in a tachistoscope. Allport said that these demonstrations convinced him that "he who knows only his own culture does not know his own culture" (Allport & Pettigrew 1957; Allport, Pettigrew, & Barnett 1958).

By the time they were well-established professionally, the Allport brothers had overcome their earlier distaste for religion and were insisting that religion was more important in human life and civilization than most psychologists realized. In a widely quoted article, "The Religion of a Scientist" (1930), Floyd Allport, himself an agnostic, argued that science and religion were not antithetical. The real conflict, he said, was between science and theology. Gordon Allport, though concerned about psychologists' neglect of religion, published little on the subject until twenty years later. *The Individual and His Religion* (1950*a*) was devoted largely to "a portrayal of the place of subjective religion in the structure of personality whenever and wherever religion has such a place." In the preface, he wrote: "One underlying value judgment favors my writing. It is a value that to my mind every supporter of democracy must hold: the right of each individual to work out his own philosophy of life, to find his personal niche in creation as best he can" (pp. vii, viii). In *Becoming* (1955), he urged psychologists and other readers to approach religious behavior in terms of what the individual is trying to do, rather than to depend so much on the study of his past. "The final truths of religion are unknown," he said, "but a psychology that impedes understanding of the religious potentialities of man scarcely deserves to be called a logos of the human psyche at all" (p. 98).

Numerous psychologists, realizing that the study of religious behavior had become respectable, began to act as if they remembered that the two principal founders of the APA, William James and G. Stanley Hall, were interested in religion. Allport himself was active in founding the Society for the Scientific Study of Religion by psychologists, social scientists, and religious educators. Within five years of his death, the APA established a division of humanistic psychology, followed a few years later by a division of psychologists interested in religious issues. At times, however, it seemed that the excursions of Allport and others into the psychology of religion might lead to their own undoing. For

instance, Allport and Kramer (1946) found that, in general, church attenders were more intolerant of ethnic minorities than were non-attenders and those who claimed no religious affiliation. Several significant negative correlations of "religious ideology" or religious value with "intelligence" or scholastic aptitude were reported (Dittes 1968). The more research there was in the psychology of religion, the worse was the chaos. Shortly before he died, Allport himself discovered that criteria of religiosity such as church attendance and the religious value score on the *Study of Values* were not valid measures of the *kind* of religiosity he was writing about. When he employed new scales of *intrinsic* religiosity (the extent to which the person *serves* his religion) and of *extrinsic* religiosity (the extent to which he *uses* religious behavior to further his other needs and values), the research findings proved to be more orderly and valid (Allport & Ross 1967; Kahoe 1974).

During most of his career, Allport conducted an advanced course on the history and methods of social psychology. Much of the content is preserved in the *Handbook of Social Psychology*, edited by his former student and colleague, Gardner Lindzey (Allport 1954a).

Though he contributed abundantly to psychological theory, Allport disclaimed leadership of any particular school or limited system of psychology. With respect to personality, he advocated an "open and eclectic system." To be *open*, he stipulated, a system must meet four criteria: (1) constant intake and output of matter and energy; (2) maintenance of steady (homeostatic) states; (3) progressive internal organization over time, owing to an increase in complexity and differentiation of parts; (4) creative transaction with the environment (Allport 1960a). Because he could find no "simple and sovereign" system that dealt adequately with the whole field of personality, he maintained that eclecticism was the only acceptable means of encompassing the available knowledge about personality (for Allport's own comprehensive statement of his point of view, see Allport 1968b).

Gordon W. Allport died in 1967. Eleven years later there appeared *Waiting for the Lord*, a remarkable series of 33 of Allport's "meditations" delivered in Appleton Chapel in the Harvard Yard over a period of 29 years, edited by Peter Bertocci (Allport 1978).

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ANGELL, ROBERT COOLEY

The life and work of Robert Cooley Angell mirrored, in many respects, the career of so-

ciology from its *nouveau* and ambiguous status in American universities during the first quarter of the twentieth century to its firm establishment and rapid expansion in the post-World War II era. Angell was born in 1899 in Detroit, Michigan, into an old and distinguished American family. One grandfather and an uncle had served as presidents of the University of Michigan and of Yale University, respectively. His other grandfather, his father, and his brother were lawyers, and Angell himself spent a semester at the Harvard Law School following service in the U.S. Army Air Corps and after graduation from the University of Michigan in 1921. The major influence in Angell's decision to do his graduate work in sociology, however, was his uncle, Charles Horton Cooley, then one of the leading American sociologists. It was under Cooley at the University of Michigan that Angell received his theoretical and research training, and in 1924, he received the first doctorate in sociology awarded by that institution.

Cooley's emphasis on the fundamentally mental character of social life and on society as a "moral organism," held together and expanded by modern means of communication, became abiding themes in Angell's own life and work. Cooley's methodological injunction to carve out an area of study, to examine it intensively and empathetically, and to explain it on the basis of such an endeavor was also of influence. Taking this to heart while still a graduate student, Angell obtained a part-time appointment as assistant to the dean of students at Michigan and wrote his PH.D. dissertation on "The Student Mind." This led him to focus on the sociology of education and this research resulted in the publication of *The Campus* (1928) and *A Study in Undergraduate Adjustment* (1930b). He also devoted his energies to keeping Cooley's thought alive by editing a collection of the latter's major papers in *Sociological Theory and Social Research* (1930a) and coauthoring, after Cooley's death, an introductory text which incorporated large portions of the latter's previously published work (Cooley, Angell, & Carr 1933).

Angell was also in touch with the newer empirical and statistical approaches to sociology that were being developed in the sociology department at the University of Chicago, many of whose members were visitors in the Michigan department. As a result, Angell also attempted to wed the subjective and sympathetic qualities

of the case study approach to the statistical analysis of large numbers of cases in an effort to identify causal factors operating in a social situation. His programmatic "Memorandum Concerning a Proposed Research Technique" (1931) became the model for his classic methodological contribution, *The Family Encounters the Depression* (1936). In the latter, on the basis of student-written protocols, Angell sought to predict the type of adjustment made by the family to the impact of the depression through the measurement of two previously existing family characteristics, "integration" and "adaptability." The appendix to this study is generally considered to be a model of detailed honesty regarding the actual process of research. Some years later, Angell cooperated in a critical review of the use of personal documents in the social sciences. In connection with the use of such documents in sociology, he observed "a slow and steady, but not very impressive advance in method" (Gottschalk, Kluckhohn, & Angell 1945, p. 226). He emphasized (as Cooley might have) the greater promise they held in comparison to the current trend of studying "objective" variables in order "to probe the subtleties of human relationships" (*ibid.*, p. 232).

As a member of the department at the University of Michigan for 46 years, Angell early evidenced the cosmopolitan outlook and interests that were later to characterize his discipline. Before 1940, he had visited Europe three times, meeting with many of the prominent French and German sociologists. By this time, he had become more critical of Cooley's optimism regarding the future of modern democratic societies. Especially did Angell come to question Cooley's view of primary groups as the guarantors of those moral sentiments that would effectively integrate the larger society. Impressed by Parsons' (1937) treatment of Durkheim's concept of mechanical solidarity, Angell turned his efforts to the investigation of the larger question of social integration—an interest he continued to pursue throughout the rest of his career.

In *The Integration of American Society* (1941), Angell codified the sociological literature relevant to the question of whether the decline of the neighborhood and the local community was seriously impairing the integration of American society. He viewed the proliferation of "free-standing groups," which functioned beyond the family, as failing to assure the existence

and operation of the common values and institutions that he saw as the bases of societal social integration. He noted especially the tendency of such groups to take on a class character and thus to emphasize noncommon interests and to nullify common values. In the context of the economic depression, Angell concluded that the main task for American society was to foster understanding across class lines.

This effort was essentially descriptive and theoretical. His next step was to engage in a more analytically pointed study of the problem in an area where more relevant, empirical data were available: the large American city. In 1942 appeared the first of a series of papers and monographs that extended over a thirty year period in which Angell sought to identify the major determinants of social integration and to measure and compare the degree of such integration among selected samples of cities. Heterogeneity and horizontal mobility were discovered to explain 62 per cent of the variance in his initial effort. His work was interrupted when he became an officer in the U.S. Army Air Forces for the second time, during World War II, but another effort (1947) identified four cities representing the extremes of the social integration scores, unpredicted by his previous analyses. This provided him with an opportunity, in the Cooley tradition, to engage in four detailed case studies aimed at the identification of the social psychological determinants of social integration. Moreover, in the light of his positive and negative empirical indicators of integration—welfare effort and crime—he refined his use of the concept of integration, and in his 1949 study used it to refer only to the extent that a community constituted a *moral* order, apart from the question of the degree of solidarity in interpersonal relationships. Although an extensive report he published identified the optimal characteristics of community leadership groups, it found only the organizational activities of schools and churches related to the degree of moral integration of a city (1951a). In his theoretical statement on social integration, Angell reiterated his view of the centrality of the concept to the discipline, differentiated among three subtypes (normative, functional, and communicative), yet was forced to conclude that it was a concept that "so far has borne little fruit" (1968b, p. 386). Finally, in his last study, Angell concluded that "the moral integration of large American communities . . . was seriously impaired during the period 1940

to 1970; these communities are becoming more alike in moral integration; city population size above 100,000 has become a significant independent variable . . . ; the difficulties of studying moral integration in such communities have increased" (1974, p. 628).

Undoubtedly Angell's second war experience sensitized him further to the larger problem of international peace. Although he continued his work on moral integration in American cities and on the question of the future of democracy (1958), his growing interest in international order was sparked into activity by his selection in 1949 as director of the Tensions Project for the Social Science Program of the United Nations Educational, Scientific and Cultural Organization. Stationed in Paris for a year, this experience probably helped lead to his appointment by President Harry Truman in 1951 as one of the at-large members of the U.S. National Commission to UNESCO. His six years of service included a stint as chairman of the Committee on the UNESCO Program.

In the meantime, Angell's contributions and leadership skills were increasingly recognized by his peers through his editorship of *The American Sociological Review*, his presidency of the American Sociological Society in 1951, and his presidency of the International Sociological Association in 1953. As chairman of the department of sociology at the University of Michigan from 1940 to 1952, he recruited to the faculty Theodore M. Newcomb, Horace M. Miner, Ronald Freedman, Guy E. Swanson, Morris Janowitz, and Gerhard E. Lenski. Finally, in line with his interest in the problem of international order and world peace, Angell played a major role in the founding and direction of the Center for Research on Conflict Resolution and *The Journal of Conflict Resolution*. In this latter connection he was drawn into research and consulting roles with the U.S. Navy and the Institute for Defense Analysis in the early sixties. His involvement in the latter led to a month's stay in Russia and visits to a number of Russian universities, all of which contributed to the publication of his *Peace on the March* (1969), a book that considered the impact that transnational contacts have on participants and policy makers.

Just before his retirement in 1968, Angell became the director of the American Sociological Association's project on "Sociological Resources for Secondary Schools," which was oriented to the development of teaching and learning materials in sociology for high schools. The circle

of his original interest in the sociology of education was rounded.

HAROLD W. PFAUTZ

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ARENDT, HANNAH

In the preface to *The Origins of Totalitarianism*, Hannah Arendt, a major political thinker of the twentieth century, defined her ambition: "Comprehension, in short, means the unpremeditated, attentive facing up to, and resisting of, reality—whatever it may be" ([1951] 1958, p. viii). Such comprehension is manifest not only in this book but in all her work. The phrase "facing up to" suggests clearly her judgment that the mind naturally recoils from much of political reality, which is so complex and so morally offensive. Courage lies in steady perception, the readiness to endure intellectually the almost unendurable. Yet she injected resistance to reality in the very definition of "comprehension." Holding that the act of comprehension was incomplete if it lacked an articulation of moral response—indeed, that without moral interest, in the first place, the energy required for comprehension would be deficient—she insisted on giving as adequate a moral response as possible to political reality. It is this combination of trying to know the worst while remaining unparalyzed by knowledge that is her defining characteristic. She was fortified in her comprehension by a certain understanding of political excellence. In the life of Athenian democracy, the Roman republic, the New England towns, and in the experience of modern people in revolutionary conditions or the circumstances of organized dissent, she saw a saving reality, the reality of freedom.

Arendt was born in Hanover on October 14, 1906, of German Jewish parents. She studied at the universities of Marburg, Freiburg, and Heidelberg, and received her doctorate in philosophy in 1928 at Heidelberg for her thesis *Der Liebesbegriff bei Augustin* (1929). Among her teachers was Karl Jaspers, whose philosophical interest in human freedom is reflected in

Arendt's work. More generally, Arendt's thought shows a strong resemblance in everything but doctrine to other German philosophers of the modern age, including Martin Heidegger. Her work displays the same intellectual audacity, passion to interpret and reconstruct, impatience with conventional opinion, and intimate acquaintance with classical thought and the classical sources of modern thought. She left Germany in Hitler's first year, 1933, and went to live in Paris. She moved to New York in 1941 and lived and worked there for the rest of her life. In 1967, after teaching for a few years at the University of Chicago, she joined the faculty of the New School for Social Research, where she held a university professorship until her death on December 4, 1975.

Knowing the worst was Arendt's philosophically first aim. *The Origins of Totalitarianism* contains her most extended investigation into the worst, the phenomenon of totalitarianism. She was persuaded that the phenomenon had not been understood. Though she made full use of an extensive bibliography, her book is dominated by the insistence that totalitarianism is "a novel form of government" that differs essentially "from other forms of political oppression known to us such as despotism, tyranny, and dictatorship" ([1951] 1958, p. 460). Her analysis is basically psychological in nature. She tries to make an inhuman phenomenon intelligible. Her categories are "mob," "elite," and "mass"; and though she means her analysis to apply equally to Nazism and Stalinism, it is most richly suggestive in the case of the Nazis.

The Origins of Totalitarianism is divided into three main parts. The first two, "Antisemitism" and "Imperialism," concern the "shadowy forebodings" of totalitarianism in the nineteenth century. The decomposition of the old European order, of its class system and traditional beliefs, set free social movements that were marked by an effort to make sense of an increasingly senseless world or to exploit new opportunities for action in behalf of indefinite or limitless purposes. World War I, in its horror, was the culmination of the unchained tendencies of the nineteenth century as well as the further impetus to totalitarianism. Totalitarianism was hardly the inevitable outcome of preceding events and conditions; yet without them the very raw material of totalitarianism would have been lacking. It was thus a phenomenon of the twentieth century, of the chaos produced by war, the decline of stable polities, economic disorder,

the transfer of millions of people, and an overwhelming feeling of lostness. The concentrated psychological condition of the masses was loneliness, "the experience of not belonging to the world at all, which is among the most radical and desperate experiences of man" ([1951] 1958, p. 475). This widespread loneliness, related as it was to feelings of uprootedness and hence superfluousness, prepared human beings for totalitarian domination. For the masses, the totalitarian organization of all life was a "suicidal escape" from a reality that was oppressive in itself and that became all the more oppressive for not being amenable to the understanding of those who suffered. With grimness she says that totalitarianism became "this century's curse only because it so terrifyingly took care of its problems" (1951, p. 430).

But the masses did not spontaneously devise their own suicidal escape. Their susceptibility was one thing; quite another was the mentality of those who created totalitarian movements and later exercised totalitarian domination. Arendt mentioned the "hysterical fanaticism" of Hitler and the "sensual vindictive cruelty" of Stalin, but her general analysis was not given over to the rhetoric of demonology; nor, on the other hand, to the clinical language of any technical psychology (toward which disciplines, in any case, she had an invincible antipathy). She used the Kantian concept of radical or absolute evil to designate totalitarianism and suggested that its commission defied even the most pessimistic ordinary understanding. The "evil motives of self-interest, greed, covetousness, resentment, lust for power, and cowardice" could not account for totalitarian action ([1951] 1958, p. 459). What, then, did it take for the totalitarian movements to start and then disclose themselves, ever more characteristically, ever more criminally, until the creation of the death camps, "the first crime in the long and sinful history of mankind that is a greater sin than murder itself?" (1951, p. 434). Or, to ask the question she was later to borrow from Daniel Ellsberg and ask in regard to American war makers in Vietnam, "How could they?" (1972, p. 33).

The Nazi leaders came for the most part from the mob, which she defined as the "refuse" of all classes and the "by-product of bourgeois society" ([1951] 1958, p. 155). In her description they were made up of a driven group of semi-intellectuals in passionate quest of some rectification of deep personal and spiritual disturbance, along with an anomic following eager for com-

plete and self-surrendering obedience. At the same time, the elite, the class of cultivated leaders of social and intellectual life, contained some who were fascinated by the mob because they were fascinated by the possibilities of destruction. Their aestheticism was nihilist. As Arendt's analysis unfolded, her attention focused ever more sharply on the totalitarian leadership in power, enabled at last to act out their nature without significant resistance. They did not rest until they made the world over to conform to their initial ideological "supersense" of the way it should be. Their maxim was "everything is possible," which meant both that nothing material, and that nothing moral, would impede their determination.

When Arendt said that totalitarianism was a novel form of government, she meant first that its mode of governance was novel and second that its aim—implicit in the mode and unthinkable without it—was novel. An understanding of the leaders' mentality was finally an understanding of the mode and the aim. The mode was terror; the aim was to compel the world to demonstrate the correctness of their ideology. Her main points were that the terror increased as the population grew more docile and that the ideology was not merely a cynical instrument of action but, essentially, an image of the only acceptable world. The only acceptable world was one in which helpless victims were transformed into noxious, subhuman elements by being treated as such. In the death camps the world became what it should be: "the supreme proof of his [the totalitarian leader's] omnipotence" (1951, p. 414). That the camps were useless from the point of view of the war effort, and indeed interfered with it, only showed how profoundly antiutilitarian the leadership was. In no conventional sense were the leaders impelled by the lust for power or expansion. It would seem that the motive that Arendt found at the heart of such crime was an infinite desire for revenge on life, revenge for feelings of smallness and impotence. Only omnipotence sufficed for an infinite desire of this sort. There are resemblances between Arendt's analysis and Nietzsche's thought on *ressentiment* and Heidegger's on revenge.

Arendt did distinguish various motivations in leaders. She maintained that Nazism before it had perished had not yet been taken over utterly by "the authentic mass man" acting with the "meticulous, calculated correctness" of a Heinrich Himmler ([1951] 1958, p. 327). She

also contrasted the rough animal brutality of the *Sturmabteilung* (SA) concentration camp guards—"men who really belonged in mental institutions and prisons"—with the methodical and passionless murderousness of the *Schutzstaffel* (SS)—"perfectly normal men" ([1951] 1958, p. 454). In her brief description of Himmler and the SS may be seen the incipient statement of the theme of "the banality of evil," which Arendt developed in her later, controversial book, *Eichmann in Jerusalem* (1963a). Eichmann, an important functionary though not in the inner circle, could have been, she suggested, perfectly harmless in other circumstances. But with power his deficiency became lethal: he was unable to think. He could not see the world as someone else might see it, nor could he imagine himself in the place of another. Without this capacity, a person interested simply in doing his job and in getting ahead could commit and abet crimes on an inconceivable and unforgivable scale.

There is great stimulus to reflection in Arendt's work on totalitarianism. One may reject her principal contentions or find that they need revision in the light of the conventional understanding that she strove to shock and overturn; but her writing on this subject has the amplitude of epic.

If Arendt explored the darkness of the century's worst experiences, she also tried to recapture the meaning of political greatness. The horror of absolute political evil did not diminish her eagerness to theorize about the peculiarly human excellence that she thought only political action could reach. At the same time, she did not claim that the absence of opportunity for political action produced the totalitarian pathology, or that involvement in political action would necessarily cure the psychological tendencies that were the root of totalitarianism. To be sure, totalitarian leaders and masses alike shared a sense of superfluosity—and therefore found it easier to project total loss of individuation onto their victims in death camps, while the greatest prize of political action was the enhanced individuation that actors came to have—the feeling of being irreplaceable. There was thus a strong conceptual link between the extremes, but no actual interdependence. In fact, each was, and had to be, historically rare. The burden of preventing totalitarianism rested on modern societies and their efforts to diminish the sense of superfluosity.

Arendt belonged to that small company of

thinkers ranging from classical to modern times for whom political action is intrinsically great—morally, aesthetically, or existentially—quite apart from any practical, especially economic, end it may accomplish. Varying with the philosopher, the form of involvement may be citizenship, governance, conquest, violent or nonviolent revolution. Each form has had theoretical vindication as an occasion for acquiring, perfecting, or exhibiting a humanly valuable trait or capacity. The emphasis may be on the enlistment of practical wisdom, the attainment of one's autonomy and the acknowledgment of that of others, the expression of virtue or *virtù*, the spontaneous creation of meaning or a memorably beautiful pattern of action, the growth of self-knowledge or worldly knowledge, the creation of new bonds between people, the satisfaction of leaving one's mark on the world, the exhilaration of overcoming resistance, the discovery of self-respect, or the enjoyment of a new kind of happiness that rivals private happiness or relieves private woe. Theoretical suggestions or elaborations can be found in Aristotle, Machiavelli, Rousseau, Hegel, and Nietzsche; and in such diverse twentieth-century figures as Georges Sorel, John Dewey, Mohandas K. Gandhi, André Malraux, and the leaders of the New Left in America and France. To this accumulation of thought on the intrinsic, non-instrumental value of political action, Arendt made a most complex and instructive contribution. She presented her theory in *The Human Condition* (1958), *Between Past and Future* (1961), *On Revolution* (1963b), and numerous essays.

If there is any substantial influence on Arendt's thought it comes from the Greek poets and historians Homer, Pindar, Herodotus, and Thucydides. Although the Aristotelian notion of *praxis* obviously provided some guidance, Arendt worked on the assumption that Greek philosophers, like most philosophers, were by nature hostile to the celebration of political action, or, when not hostile (as, say, Rousseau was not), praised action on unacceptable grounds. Arendt thus distanced herself from other theorists of action and claimed to have been either one of the first or one of the few to have seen what the early Greeks saw in action, and to be willing to render that vision philosophically.

Her discussion of political action is set against two other main kinds of human activity: the labor of the body to keep itself alive,

and the work of the fabricator to furnish the world with durable and sometimes beautiful objects. The greatness of political action is that through it, human beings achieve two things existentially. They do not necessarily intend either of them, but political action is the only way to achieve them. The first is the differentiation of humanity from nature, and the second is the differentiation of each human individual from every other. Arendt had a fundamental aversion to nature in its species characteristic, its samenesses and cycles, its domination by the care to survive. The shame lies in complete identification with, imitation of, or resemblance to, nature; honor lies in collective artifice and in individuation. A wholly private life does not sponsor individuation, which has nothing to do with individuality in John Stuart Mill's sense, an essentially private cultivation of private distinctiveness, a resistance to intellectual and behavioral conformity. Arendt saw indistinctness and self-delusion in liberal withdrawal from the public realm, and bondage to the perishable and biological in the apolitical consumerist society. Only in political action does the human individual acquire identity and a group of individuals their humanity. A person discloses who he is amidst the formalities and disciplines of appearing and speaking in the public realm; he can never know himself as others know him, but he can know others as they know him. At the same time, the common involvement of all citizens creates the world, the space, the nonnatural medium in which persons say great words and do great deeds that escape the futility of natural repetition by surviving in the memory of posterity.

Above all, political action is speech: "Without the accompaniment of speech . . . action would . . . lose its revelatory character. Speechless action would no longer be action because there would no longer be an actor, and the actor, the doer of deeds, is possible only if he is at the same time the speaker of words. The action . . . becomes relevant only through the spoken word in which he identifies himself as the actor, announcing what he does, has done, and intends to do" (1958, pp. 178-179). The content of speech is the public business, especially the decisions that must be taken to preserve the well-being and luster of the public realm itself or to launch great and heroic enterprises. Political action is not administration or governance. Arendt's model here is the assembly of the Athenian city.

In *On Revolution* she tried to show that there was a more recent model, that of the committees, councils, and communes spontaneously created by people from all classes during the revolutions of the last two centuries in America, France, Russia, Germany, and Hungary. These occurrences were not imitations of each other but rather the result of the sudden awareness in each case that some opportunity could be seized for involvement in the public life of decision making. Arendt acclaimed this public happiness, the splendid intoxication when previously private and silent individuals displayed capacities for concerted action that they had not known they possessed. By this display, they acquired a genuine identity. These occurrences were true illustrations of Arendt's dictum: "The *raison d'être* of politics is freedom, and its field of experience is action" ([1961] 1977, p. 146). The tragedy was that under the pressure of economic advantage (as in America) or of the attempt to abolish material misery (as in France) or of superior military strength (as in Russia and Hungary), the space of political action, the locus of participatory democracy, was destroyed. The supreme political achievement would be to devise and maintain a political system in which all who wished could have the daily experience of direct democracy. Representative democracy, though admirable for its liberties and securities, was a democracy only for the representatives. Arendt did not think that modern life, except in extraordinary circumstances (like revolution) or in episodes (like civil disobedience in the name of constitutional principles), could accommodate or could even understand freedom. Her main hope was to preserve the memory of freedom.

Arendt's writing was not confined to the extremes of totalitarianism and political freedom. She thought a great deal about modern culture, where she saw mostly darkness, though she looked for glimmers of light, finding them in exemplary modern lives, in the careers and fates of worldly writers and political actors. Her deepest philosophical concerns were expressed in her occasional pieces on actors and specific political events, but she could treat the particular on its own terms and not force it to serve her own larger theoretical purpose. *Men in Dark Times* (1968), with its essays on men and women whose work showed their love of freedom and the world, exhibits perfectly what she admired most.

In the last years of her life Arendt ventured

from "the relatively safe fields of political science and theory" (1978*b*, vol. 1, p. 3), turning to an examination of mental life. She tried to synthesize and expand the suggestions scattered throughout her earlier writings about the tensions and even the desirable connections between the life of action and the life of the mind. By the time of her death she had completed work on two mental phenomena, thinking and willing, but had not begun the third part, on judging. Although her discourse at times is unpolitical, her passion for the avoidance of political horror was the instigation of *Thinking*, as her equally intense passion for political freedom helps to unify *Willing*.

GEORGE KATEB

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ARON, RAYMOND

Raymond Claude Ferdinand Aron was born in Paris on March 14, 1905. In 1928 he came out of the École Normale Supérieure at the top of his class, and in 1970 he was named a professor at the Collège de France. Aron's early inclination was toward metaphysical speculation but his principal teacher, the neo-Kantian and historian of mathematics and physics, Léon Brunschvicg, dampened such interests in the name of the triumph of science over philosophy, and encouraged him to think about the biological sciences. After a year of work in this field, Aron decided that it was impossible to theorize about biology without becoming a biologist. He concluded that a philosopher, on the other hand, could reflect upon either the natural or the moral and historical sciences. He chose the latter.

Aron then went to Germany, where as a young Jewish scholar of philosophy, he witnessed the relentless rise of National Socialism. The experience imbued him with a degree of pessimism about human affairs that he was never to lose. The ahistorical moralism of his French university training seemed irremediably bankrupt, as did his brief allegiance to the Socialist party. In 1931, he formulated his intellectual goal: to probe the relations between theory and action. It is out of this initial query that all of his books have emerged. In Max Weber he found inspiration and a kindred spirit. During these same years, he began his lifelong study of Karl Marx, which has affected all of his works.

Aron returned to Paris before the outbreak of World War II to complete his *Introduction to the Philosophy of History* (1938*a*). Exiled in London during the war, he edited the monthly *La France Libre*, and developed a taste for journalism that was in keeping with his desire to be both historian and participant in the events of his times. For decades he was a regular columnist for the conservative French paper *Le Figaro*, from which he resigned in June 1977. He subsequently joined the more progressive *Express*.

Aron's postwar works follow logically from his prewar concerns with epistemological and formal problems in the philosophy of history, which were influenced by, among others, Wilhelm Dilthey, Heinrich Rickert, Georg Simmel, and Max Weber. After the war, however, his work has a different orientation and addresses the immediate concrete political, economic, social, and international problems of life in the twentieth century. Because Aron's work is thoroughly

interdisciplinary, both in method and substance, and because his contributions to scholarship are prolific and dispersed, his books can be categorized in a variety of ways. A rough ordering would include: analysis of the industrial order; study of international relations involved in peace and war; ideological criticism; studies in French politics; and history and critique of sociological thought.

Aron's comparative sociological studies of the industrial order focus on liberal and communist régimes. While his analysis of class structures and bureaucratic imperatives has inspired "convergence theory" between "capitalist" and "communist" modes, Aron himself only partially subscribes to such an approach. In the age of industrial society, he believes that the political régime is the crucial factor that provides the specific difference between collectivities. In the tradition of Weber he sets up ideal types—the monopolistic and the constitutional pluralist—and argues that they provide social wholes with critical determinants that cannot be modified without affecting the entire order. The existence of a genuine opposition and the protection of individual rights are intrinsic to the constitutional pluralist mode. With this value of freedom comes the risk of corruption and conflict. Intrinsic to the monopolistic mode are bureaucratic solidarity and ideological conformity. The risk of servitude comes with this ability to centrally plan social and economic life. Social wholes are unities that can be reformed only if one understands the structural realities they imply. There is only a choice for change at the margin.

Although the nature of hierarchical structures of production and the impact of new technologies on all aspects of social life highlight the similarities of industrial societies whatever the régime, past values and formal political rules remain critical to understanding the fundamental differences between the two forms of industrial society that are well on their way to dividing the entire world. Both systems loudly proclaim an egalitarian ethic, but both have to face a nonegalitarian reality as the most cursory studies of income distribution, prestige differentiation, and power concentrations amply demonstrate. The dialectic between the ethic of equality and the reality of hierarchy forces some hard choices. Reconciling human dignity and the imperatives of modern industry is a problem for whoever owns the means of production—individuals, corporations, or state planning agencies. While industrial hierarchies are built on efficiency and

lack the stability of the traditional hierarchies of the past, Aron denies the possibility of their elimination through socialism; in fact he claims socialism leads to their proliferation. As the avowed heir of Montesquieu and Alexis de Tocqueville, he stresses historical and political forces and sees them, rather than underlying social and economic structures, as ultimately shaping the long-range destinies of collectivities. Indeed, Aron's distinction between the constitutional pluralist model and the monopolistic one is a continuation of Tocqueville's insight that the democratic age could lead to despotism as well as to régimes of freedom.

Aron's focus on history and politics also characterizes his monumental studies of peace and war in the twentieth century. He notes that for a century wracked by wars, war itself seems to play a singularly small part in scholarly interpretations. Aron approaches the study of war through theoretical, sociological, and historical categories. He analyzes the consequences of the shift from the balance-of-power to the balance-of-terror. By accumulating data on the space, resources, and political structures of competing sovereign states, he demonstrates how, for the first time in history, a global system of international politics is emerging, threatened and shaped by nuclear arrangements and rent apart by countless divisions, including the new nationalisms and political, economic, religious, and ideological deadlocks. He studies the evolution of the Western and communist blocs, the divisions between the rich and the poor, and between the atomic nations and those excluded from the atomic club. He analyzes three main diplomatic strategies in the twentieth century: dissuasion, subversion, and persuasion. War, to Aron, is central to understanding human history. Peace emerges as the effort to limit war—an effort that the nuclear threat may spur on to some form of transcendence—through a legal or imperial solution.

A traditionalist in international relations, Aron is skeptical about the explanatory value of games theory and abstract models. If even economic models have a dubious fit with social reality, how can models in international relations purport to reduce the cultural and historical complexities of collectivities competing through particular political leaders? He has serious doubts about computer-assisted simulations and about sophisticated scenarios of wars involving non-Western peoples, whose history and value systems become a dangerously ignored dimen-

sion in an attempt to predict the outcome of international political struggles. Although searching for general laws in international politics is probably futile, because the weight of the past and the clash of unique personalities in the present usher in an unpredictable future, Aron has established a wide-ranging framework for the study of international relations, and his pioneering scholarship has influenced the development of such fields as "polemology" and peace studies.

In tireless and ruthless attacks on the myths of the right and the left, his favorite targets are left wing French intellectuals, particularly Jean Paul Sartre and Maurice Merleau-Ponty. Aron's analysis of politically emotive words confronts ideology with reality. He accuses Parisian "left-bank Marxists" of confusing Marxist goals with Stalinist practice, deluding themselves on what the nature of French life would be under communism. In the late 1940s and early 1950s, he stood quite alone in this respect among the French intelligentsia. Then, in the late 1970s, the atmosphere changed abruptly as the "new philosophers" (particularly Bernard Henri Levy and André Glucksman) criticized the cynical and bloodthirsty applications of radical revolutionary ideology and utopian visions in the twentieth century. If Aron accuses the left of harboring terroristic methods in the name of illusory goals, he also sees terror behind the reactionary who turns radical in order to block change. He analyzes the ominous slide from conservative to fascist positions and tactics. The reactionary wishes to preserve or return to the past at all costs and denies change, while the revolutionary attempts to abolish the past and transcend it. Neither goal is possible, but supporters of both will go to inhuman extremes in the name of one or the other. Aron's complaint that French intellectuals play with words and do not understand the realities of communal existence has ties with Simone Weil's critique in the 1930s. It also has much in common with Albert Camus' onslaught against revolution and ideology. While Camus confronts the problem of human cruelty in the present in the name of the future from a moral point of view, Aron is repeatedly demonstrating this phenomenon from the perspective of history and the social sciences. The accumulated impact of Aron's work in this field influenced the "end of ideology" debate. He does not discount the impact of ideology on history so much as he wishes to show the excesses that are perpetuated in its name.

Aron's works on French politics are prompted by circumstances and the rush of events. He called for Algerian independence long before many of the thinkers on his left did. Despite his admiration for Charles de Gaulle, he openly attacked Gaullist policy toward Israel. In general he has traced and criticized the centralizing and statist tendency in French history and politics. He has not taken a conservative role in French politics as many have claimed; indeed, he has argued that both the right and the left reinforce state power in their plans and practices. Outspoken in his preference for economic liberalism as long as it strives for socially responsible policies, he has consistently argued for strong economic growth, and claims that the postwar period has been, in this respect, despite ups and downs, a real success in France.

Aron's intellectual portraits of Montesquieu, Comte, Marx, Tocqueville, Émile Durkheim, Vilfredo Pareto, and Weber are classics that provide a wealth of balanced analysis; they also place Aron in the sociological tradition and reveal his views on sociology itself. He is critical of the French sociological tradition of Saint-Simon, Comte, and Durkheim for relying too heavily on a deterministic theory of the evolution of industrial society, and for placing too much faith in science and in the economic and social benefits directly deriving from rationality. This optimistic sociology, and its normative twin, socialism, ignore the perennial reality of politics. So does Marxism with its view of economics as fate. Even if politics is but a subsystem of society, Aron reasons that it is a crucial one. From a different perspective he reaches the same position as did Giovanni Sartori on the primacy of the political. Aron, a classical realist, often seems closer to the great traditional political philosophers, Aristotle, Niccolò Machiavelli, and Thomas Hobbes, than to modern sociologists. Still, against the deterministic and optimistic conclusions of the latter, Aron sides with the liberal, realist, relativistic, and historically oriented views of Montesquieu and Tocqueville, and with the "value-free" approach of Weber. He cannot, however, accept Weber's radical distinction between moral choice and scientific demonstration. For Aron, commitment to specific values is an inescapable human necessity. Science can and does help make alternative value choices explicit even though it cannot decide the substance of the choice itself.

What unites all these areas of Aron's work is his search for the link between theory and ac-

tion, between the general tendencies of human collectivities and the unique, the contingent, and the accidental. Aron focuses on the interaction of historical experience and political will, and shies away from historical determinisms, believing that history is open ended. At the same time, he rejects extreme historical relativism and an anarchistic approach to values. Individuals and people need a perception of themselves, their past, and their future. Aron explores with insight and flexibility the many options of human consciousness expressed in history. He argues that a probabilistic theory of history is the best one, accounting for the contingent and the necessary, shedding light on what might have happened as well as what did, and revealing a past that helps determine a more open, progressive, and humane future. Aron's probabilistic theory of history is imbued with the same spirit as his margin of choice option in politics, that is the wedding of the ideal and the real, the working for progressive change without fanaticism or illusions.

Another theme that unites Aron's work is a concern for the epistemological and methodological limits of reason and knowledge. There is an irreducible subjectivity that no social science can conquer, particularly since its subjects are endowed with a self-consciousness that modifies all abstract models set up to understand them. All sociological theories remain marked by an ideology, charged with positive and negative values that cannot be eliminated scientifically. Sociological theory can only provide concepts, regularities, and patterns, not scientific laws. Aron believes this holds true for a Parsonian theory as well as for a Marxist one.

There are very few secondary sources on Aron's thought. Criticism focuses on his more polemic works and on the seemingly endless repetition of his major themes in lengthy and overlapping volumes. A more serious challenge, which Aron is very much aware of, involves consistency between his critical stance toward ideology and his own obvious and strongly stated preference for a liberal view. If the constitutional pluralist model alone permits freedom, how can one maintain a nonideological attitude toward it? If the freedom to criticize is a primary value, superior to the search for equality, how can differing systems be judged on their own terms as Aron insists they be, with an objective appraisal of the successes of each? Furthermore, in assessing structural realities, which are to be considered immutable and which transcendable? A Marxist would focus on economic, not political,

struggles as the key to historical development, the dignity of individuals, and collective liberation. On the other hand, proponents of a functional, statistically based social science would challenge the weight Aron gives to the past. Structural realities and personal values are viewed as a complex and integrated whole, interdependent and amenable to scientific methodology. Aron's work can be viewed as "old-fashioned."

For Aron there is no alternative to coming to terms with incompatible visions in politics or academic disciplines. One must take a stand through scientific study, and ultimately make a decision based on conscience. The scientific spirit can determine the outcome of some, but not all, debates. Commitments remain precarious because all régimes are imperfect and betray their ideals. Twentieth-century régimes, turned toward the future, offer the additional ambiguity of pleading their cause in the name of what they will become, not what they are. Perfection in human affairs can be stated formally only as a criterion and aspired to only as a goal. A degree of relativism is the authentic experience of politics because of the multiplicity of competing values and inescapable economic and social servitudes.

Aron's philosophy rests on a belief in the overwhelming value of freedom, and a perception of the impact of human reason, however limited, on history. It also rests on empirical and pragmatic assumptions and faith in industrial development. There are obviously tensions and difficulties in this constellation of human objectives. Aron's problem is the problem of liberalism itself, that eclectic and at times ambiguous commitment to both common-sense compromises and absolute principles. As an observer and scholar of his times he has done more than most to give heart to liberals and pause to its critics. In his work intellectual honesty has been a form of action, forcing a confrontation between theorizing and public policy. Aron's unique contribution to the social sciences lies in the lucidity and energy with which he has undertaken to understand the twentieth century as a whole, and with it the dawn of universal history.

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ASCH, SOLOMON E.

Solomon E. Asch was born in Warsaw, Poland, in 1907. He came to the United States with his family at the age of 13 and settled in the lower east side of New York. Shortly after, he entered an elite high school, Townsend Harris, the preparatory school for the City College of New York, then did his undergraduate work at City College, obtaining a B.S. in 1928. His time at college was a period of search; his studies ranged from natural science to literature. During the next four years, he was a graduate student in the department of psychology at Columbia University; he received his Ph.D. in 1932.

Although Asch was attracted to psychology because he felt the need for a discipline that would solve human social problems, he thought of himself in his graduate school days as primarily a general experimental psychologist. He studied with Robert S. Woodworth, among others, and received friendly encouragement from the young social psychologists in the department, Gardner Murphy and Otto Klineberg, but did not work directly with them. His doctoral dissertation was an analysis of a problem in learning.

Asch began his academic career as an instructor at Brooklyn College, a newly established unit of the free system of higher education in

New York, later to become the City University of New York. During this early period two experiences turned him toward social psychology. One was a summer fellowship arranged by Gardner and Lois B. Murphy through Franz Boas and Ruth Benedict. It permitted Asch and his wife to spend the summer of 1936 in observations of child-rearing practices among the Hopi Indians, one of the Pueblo peoples of Arizona. The purpose of the study was to test hypotheses derived from Benedict's work among the Zuni, a related group. The summer did not lead to firm findings; the time was too short and the language barriers too high. But it did broaden Asch's perspectives. The second experience was a collaboration with two colleagues at Brooklyn College, Max Hertzman and Helen Block Lewis, on studies of stereotyping and halo effects in social judgments. At about the same time, Asch began a study of perceptual and space orientation based on Max Wertheimer's early observations of individual differences in *Phi* phenomenon. Later, these studies developed into the work on personality as related to field dependency, conducted with Herman Witkin.

Wertheimer's influence on Asch during his beginning years at Brooklyn College transformed his intellectual life. Fleeing the Nazis, Wertheimer had gone to New York to teach at the University in Exile, the New School for Social Research. In his ten-year association with Wertheimer, Asch was completely absorbed into the gestalt tradition, and in 1944, upon Wertheimer's death, Asch took over his position at the New School. Three years later he left for Swarthmore College, where he joined W. C. H. Prentice, Wolfgang Köhler, and others in a department with a marked gestalt orientation. He remained at Swarthmore for the next 19 years, a fruitful period that saw the publication of his widely read textbook, *Social Psychology* (1952), his studies on conformity (1951; 1955*b*; 1956), and, toward the end, the inception of his work on association (Asch & Prentice 1958).

In 1966 Asch left Swarthmore to become director of a new Institute for Cognitive Studies at the Newark campus of Rutgers University. There he joined a number of psychologists with whom he had previously worked on problems of memory, association, and cognition. They included Irwin Rock, John Ceraso, Dorothy Dinerstein, and Howard Gruber. In 1972 he moved to the University of Pennsylvania as a professor of psychology.

His many honors include two Guggenheim

fellowships, two residences at the Institute for Advanced Study in Princeton, a Nicholas Murray Butler award from Columbia University, membership in the American Academy of Arts and Sciences, and the distinguished scientific contribution award of the American Psychological Association.

Fundamental orientation. The basic theme of Asch's work, constant despite the shifting character of the particular problems, has been an examination and critique of widely accepted assumptions about the nature of man. The most important influence was Wertheimer, though Köhler's formulations of gestalt principles were also an important guide. Kurt Koffka's concept of memory traces and their evolution was a secondary influence. Unlike his mentors, however, who tended to use illustrative material whose "self-evident" character was evidence for the gestalt laws. Asch carried out a series of tight, well-designed experiments. In a way, he combined the habits of thought of the gestalt tradition with the experimental approach of American psychologists of his generation. He did not go past the techniques of the 1930s to the complex multivariate designs characteristic of the 1960s and 1970s in most areas of experimental study of personality and social psychology. His work consists of linked series of studies, each of which explores the influence of one independent variable on a clearly defined dependent variable. During his career he has made contributions to four areas: prestige suggestion, impression formation, "conformity," and association.

Prestige suggestion. A traditional view held that sources with high status would automatically affect attitudes and beliefs. Thus a statement praising revolution would be viewed positively by Americans if it were attributed to Thomas Jefferson, negatively if attributed to Lenin. Asch demonstrated that the response was not an irrational change in the value placed on the statement but a restructuring of its meaning. Incidentally, in a discussion of the positive results of an experiment by Muzafer Sherif on the prestige effects, Asch explained the findings in terms of the status of the investigator—a view based on "demand" characteristics not to become a common point of view for another two decades.

Impression formation. When two people meet, each forms an impression of the character and personality of the other. Traditional trait analyses required that these impressions be derived from the sum of perceived traits:

$P = a + b + c + d \dots n$, where a , b , c are specific traits. Asch rejected this notion, arguing that an impression is the product of a totality created by the *interaction* of perceptions of traits where each trait is affected by the context in which it is imbedded. He demonstrated this tenet with the well-known experiment in which subjects were given lists of traits and asked to describe the person. Identical lists differing only in the inclusion of "warm" or "cold" led to totally different impressions. Apparently the change from "warm" to "cold" was capable of altering an impression based on many components. However, "polite" and "blunt" had no such overriding effect. But when "warm" and "cold" were imbedded in inappropriate lists, the interpretation of those two terms was itself affected by the context. Most interestingly, subjects given triads of traits with one inconstant element (e.g., helpful, quick, skillful *vs.* helpful, quick, clumsy) were able to describe a change in the meaning of "quick" from one triad to the other.

Asch went on to demonstrate the occurrence of metaphoric uses of trait-defining adjectives, such as "sweet," in many languages from different language families and cultures, and to trace the development of such metaphoric usages in children from age three to adolescence. In the latter study, he demonstrated something like a Piagetian sequence. Children did use "sweet" or "warm" to define the personal characteristics of others at the ages of three to six, but it was not until the later years that they could verbalize the nature of the connection between the physical and psychological attributes denoted by the same word. This ability may coincide with the occurrence of formal operations, in Piaget's sense of the term. Later investigators have replicated Asch's "warm-cold" effect with judgments based on contact with real people rather than with lists of traits (Kelley 1950) and have studied some of the characteristics of potentially central traits (Hays 1958).

Conformity. It is ironic that Asch is probably best known as the inventor of the experiment in which a hapless subject is exposed to the non-veridical judgments of a group of supposed fellow judges. Asch developed the experiment because he believed that conformity is *not* an arbitrary, inevitable, mechanical reaction to social pressure. He was sure that even under extreme pressure some people would maintain independence; he was actually astonished at the extent to which many individuals yielded to group pressure. Nevertheless, he felt that even

those who "conformed" would be found to have redefined their ideas of the stimuli being judged or of the social situation.

In the best-known study, individual college students asked to judge lengths of lines were confronted with eight instructed fellow judges who made obviously incorrect judgments. Asch found that despite pressures to conform 68 per cent of the judgments were correct—that is, counter to those of the instructed majority. Subjects varied widely; about a quarter never yielded to the majority and a third always yielded. On postexperimental interviews some independent subjects were found to be confident, others were filled with doubt. Some yielders rationalized their behavior by postulating an illusion to which the majority were subject. Others went along because they feared the social consequences of nonconformity although they had no doubt of their own covert judgments. Through these reports of internal reactions Asch was able to describe the cognitive and emotional changes related to either conformity or independence. Neither the studies of conformity that preceded Asch's work nor the many parametric investigations based on his paradigm shared his concern for the subjects' perception of the situation. True, a decade later a number of social psychologists turned to such analyses, but the germs of the ideas found in the work of Herbert C. Kelman, Richard S. Crutchfield, or Morton Deutsch and Harold B. Gerard are either explicitly stated or implied in Asch's original discussions. The theoretical developments in social psychology of the 1960s and 1970s, in the work of Kelley and others (Kelley 1971) on attribution, or in Walster and her colleagues on equity (Berkowitz & Walster 1976), share with Asch's work an emphasis on cognitive processes, on the internal restructuring of ideas.

Association. The traditional view of association is the mechanical bonding of items presented contiguously. Students of verbal learning tend to regard these as stimulus and response units. During the 1950s this model was extended by the concept of mediating responses. These provide central and unobservable responses as part of the chain that links input to behavior.

Gestalt psychologists have always rejected this model. As formulated by Asch, gestalt theory approaches the problem of association as a part of the general issue of structure. An association is a central process in which units are

embedded in a structural whole; the association cannot be viewed as a simple additive consequence of the bonding of invariant individual units. Learning is restructuring, not the acquisition of responses. Asch spent a number of years conducting ingenious experiments to explore the implications of the gestalt approach. These include demonstrations of backward association as evidence that one does not learn responses to stimuli; of the changes in the character of errors as a function of the matrix of items; of the superior acquisition of association between related rather than unrelated items. This body of work was summarized in Asch's 1968 address to the American Psychological Association. One sentence from this address expresses his objective most succinctly: "To study the formation of associations is to study the coherence of experienced relations."

It is difficult for a nonspecialist to evaluate these contributions to the study of association. They address themselves to problems that were ignored or treated tangentially in the great explosion of experimentation in verbal learning and memory that occupied so many American psychologists from 1945 to 1970. In that body of work, for example, "meaningfulness" is linked not to the characteristics of items but to the number of associations they inspire in subjects (Underwood & Schultz 1960). The models derived from analogies to the computer require unchanged units that progress from box to box in metaphorically labelled storages. But they do not include the notion of a flexible restructuring of the "data" of experience or of the importance of their "intrinsic properties." It is hardly surprising that neither Endel Tulving and Stephen A. Madigan (1970) nor Leo Postman (1975) in their attempts at systematic reviews of research and theory in memory even mention the work of Asch and his colleagues. Whether their work will continue to be regarded as irrelevant, or whether psychologists will turn to the issues raised by Asch, remains to be seen.

Final comment. A final summary of Asch's work should stress its underlying unity. Each contribution strengthens his view of human beings, first fully presented in the *Social Psychology*, as rational, creative, structuring, and initiating organisms—in contrast to the tradition that views mankind as passive, responding only to environmental pressures or to variations in milieu. His search for sources of independence from social pressure was carried out, not coincidentally, during the height of the McCarthy

era. In fusing his social orientation with his psychological credo as a gestalt psychologist, he has provided a model for a generation of psychologists.

BERNARD MAUSNER

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AYRES, CLARENCE E.

Clarence Edwin Ayres, American institutional economist and social philosopher, was born on May 6, 1891, in Lowell, Massachusetts, and died on July 25, 1972, in Alamogordo, New Mexico. After receiving his A.B. degree from Brown University in 1912, he spent one year at Harvard University, returning to Brown for his A.M. degree in 1914. From Brown he moved to the University of Chicago to continue graduate work in philosophy and economics, receiving his PH.D. degree in philosophy in 1917. He stayed at Chicago as instructor of philosophy until 1920, when he was appointed associate professor of philosophy at Amherst College. Resigning this position in 1923, Ayres was appointed professor of philosophy at Reed College. In 1924/1925 he was associate editor of the *New Republic*. In 1930 Ayres was appointed professor of economics at the University of Texas, a position he held until his retirement in 1968.

Ayres's intellectual development was affected powerfully by the stimulating, enriching environment of the University of Chicago. Although John Dewey had left Chicago a decade before Ayres arrived, his influence survived, and the young Ayres came under the spell of American pragmatism in its Deweyian variant. In particular, Dewey's *Studies in Logical Theory* (1903) made a deep impression. Moreover, Chicago's economics department had by this time given birth to that distinctly American school of economics, institutionalism. Thorstein Veblen, Wesley C. Mitchell, Walton H. Hamilton, John Maurice Clark, and Robert Hoxie had all taught there in their formative years. Only Clark and Hoxie were in residence during Ayres's student days, but the heterodox ideas of Veblen and his followers that were still in the Chicago air attracted him. This interest was decisively reinforced at Amherst by his colleague, Walton H. Hamilton, one of Veblen's most brilliant disciples. It was Hamilton who coined the term "in-

stitutionalism" to describe the way of thinking of which Ayres became the leading representative in the post-World War II era.

During Ayres's foray into journalism at the *New Republic* he read voraciously in numerous fields and wrote articles on a wide range of topics: theology, politics, sociology, education, music, philosophy, psychology, and belles-lettres. To an impressive degree he was able to integrate much of what he learned into his own teaching. Although he generously and correctly gave credit to Dewey and Veblen as his intellectual progenitors, the social philosophy that emerged from this mosaic of influences was highly original.

Ayres developed a theoretical system for institutionalism in an effort to do for economics what Dewey had attempted in philosophy: to bring about a genuine reconstruction. The building blocks were fashioned over many years, but the basic elements can be seen in his first two books, *Science: The False Messiah* (1927) and *Holier Than Thou: The Way of the Righteous* (1929). In the first book he described how the rapid growth of technology undermines traditional values and beliefs, placing emphasis upon the immense importance of industrial technology in transforming the values of civilization. The connection between science and values, between machines and morality, intrigued Ayres and remained the most important single conception that he held of change and progress throughout his career. In this volume he explicitly raised the issue of the moral requirements of social order by showing how the interaction of institutions and technology affects society's ethical and moral values. In *Holier Than Thou*, Ayres was concerned less with the growth of technology and its potentially devastating effects on social order than with the structure of the belief systems that were undergoing modification. The book's approach was strongly influenced by Veblen's dissection of the folkways and mores of a leisure-class society. However, it also contained a hint of the transcultural theory of value that Ayres was to develop in later years.

The publication of *The Theory of Economic Progress* (1944) established Ayres as the leading theoretician of institutional economics. The system he adumbrated in that work was refined and elaborated in *The Industrial Economy* (1952) and in his last book, *Toward a Reasonable Society* (1961). All these books exhibit a unity of method and intention. Ayres saw all

social behavior as capable of being dichotomized into technological and ceremonial components. He used the term "technological behavior" to refer to those activities in which mankind rationally applies hand and brain to solve problems and thereby contribute to the ongoing life process of the race. This process inevitably involves the use of tools. The tool-using function is broadly conceived to include all instruments by which human skills are organized, disparate artifacts that are related to each other by the same developmental forces. In Ayres's words: "The continuity of civilization is the continuity of tools. All the arts, all the sciences . . . together owe their existence and derive their substance from the continuity which links the surrealist's pigments to the clays in which the Aurignacian caves were daubed, and in terms of which the cyclotron is but a continuation of Neanderthal experiments in chipping flint" ([1944] 1962, p. 222). It is this process that was responsible for the enormous changes in the welfare of the human race over time. Ayres thus identified technological development as being a cultural process. The progression by which the arts and sciences develop results from the fact that accessible and objective instruments are capable of combination into new forms. Earlier tools (including ideas) always make the later ones possible. The retention and preservation of implements, a function of human memory power, are what make man a cultural animal and allow for progress over time in a way impossible for any other species.

By "ceremonial behavior" Ayres meant those activities in which emotion rather than reason holds sway. Accompanying the technological process are myths and ritualistic fantasies that define the status and caste system of a social order. Although the stock of tools, instruments, skill, and knowledge is constantly growing, a human element remains constant through the ages. In this, culture plays a decisive role. The human animal is given to fantasy making, obsessions, superstitions, and taboos that themselves are transmitted by community indoctrination through emotional conditioning. All tribal legends thus derive their sanction from the past. The past-bindingness of ceremonial behavior is completely contradictory to the ongoingness of technological behavior. Against the background of these opposing forces the drama of economic progress—or its failure—is played out.

The dichotomy of technological–ceremonial interaction enabled Ayres to make an important

contribution to the understanding of economic history. By identifying the dynamic and static elements inherent in the historical situation, he explained the timing and location of the industrial revolution. His stress on the rigidity of institutions as inhibitors of development was highly original and useful in demonstrating the importance of economic institutions for economic change.

One of the distinctive features of Ayres's work was his development of a theory of value which eschewed both moral relativism and supernatural authority. Ayres located the source of value on the technological side of his dichotomy, in the way of thinking and conduct that involves the use of tools. Human progress is made possible by the continuity of the technological process. The values derived from that process are transcultural, while the values deriving from superstition and ceremonial behavior are culture bound. Ayres argued that the values of freedom, equality, security, abundance, and excellence are technological values and stand in a symbiotic relationship to one another. This approach permitted a full-bodied defense of Western culture and the industrial way of life.

Although Thorstein Veblen, John R. Commons, and Wesley C. Mitchell laid the foundations for institutional economics, Clarence Ayres went beyond them in significant ways that advanced the cause of the movement. He was a creative social scientist of extraordinary ability, remarkable for his erudition and exuberance of style. More than any economist of his generation, he recognized the importance of philosophical issues and was competent to make contributions in that arena. He was also among the first economists to propose what has come to be called a negative income tax as a method of redistributing income to prevent the problem of inadequate demand. Since he rejected the orthodox theoretical framework of economics, Ayres's influence, not surprisingly, has been greater outside that science than within it. His students have included Talcott Parsons, C. Wright Mills, and Marion J. Levy, Jr. Among economists of distinction, John Kenneth Galbraith and Joseph Dorfman have acknowledged their intellectual debt to him. But outside of the southwestern and western United States, where

a regional school of institutional economists claims Ayres as its intellectual forebear, he has had few literal disciples.

WILLIAM BREIT

WORKS BY AYRES

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- (1929) 1973 *Holier Than Thou: The Way of the Righteous*. Clifton, N.J.: Kelley. → The 1973 reprint is bound together with a reprint of *Science* (1927) and has a new preface by Ayres.
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- 1938 *The Problem of Economic Order*. New York: Farrar & Rinehart.
- (1944) 1978 *The Theory of Economic Progress*. 3d ed. Kalamazoo: New Issues Press of Western Michigan University. → Includes a new introduction by Louis Junker and the "Ayres addendum." The quotation in the text is from the 1962 edition, which contains a new foreword by Ayres.
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- 1952 *The Industrial Economy: Its Technological Basis and Institutional Destiny*. Boston: Houghton Mifflin.
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B

BARON, SALO W.

Salo (Shalom) Wittmayer Baron was born on May 26, 1895, in Tarnow, Galicia, the southern region of Poland which then belonged to the Austro-Hungarian empire. His parents were Elias and Mina (*née* Wittmayer) Baron.

Baron was a middle child and an only son. His parents belonged, by the right of both ancestry and wealth, to the Jewish aristocracy of Galicia. The Wittmayers had been brokers in commodities. Elias Baron was a banker, whose interests included oil fields, lumber yards, mills, and real estate. The father, though somewhat "modern," was punctiliously Orthodox in religion. The mother was much more Western and stylish. Her manners were Frenchified, and she was fluent, in addition to Yiddish and Hebrew, in French, Polish, and German.

As a small child, Baron was entirely in the charge of a Polish nursemaid, and, thus, in the very midst of a religiously Orthodox household, his first language was not Yiddish but Polish. When he was four years old, his Hebrew studies began in earnest with a tutor, who started to teach him the Hebrew Bible translated into Yiddish. He was thus forced to learn both these languages at once. When Baron was ten, a new tutor, Aaron Wrubel (the father of the well-known Israeli medievalist Zvi Ankori) arrived and remained for the next eight years. During this period, Baron not only acquired a broad education in Jewish religious literature, especially in the Talmud, but also completed his work in the local Polish Gymnasium as an "external student" who was examined periodically by the faculty.

Thus, in these formative years, Baron lived in two cultures. He wrote Hebrew poetry as a 12-year-old, and was a passionately convinced Polish nationalist at the age of 15. Although he wore the traditional dress of Orthodox Jews into his teens, he changed into Western dress when the occasion required. All during these years, from the age of ten on, he was also trained for his father's business. By his 15th year, he was occasionally in charge of family enterprises during his father's absences.

At age 18, Baron decided against business and for scholarship. By 1917, he had received a doctorate in philosophy from the University of Vienna. To this degree he added rabbinic ordination in 1920 from the city's Jewish Theological Seminary, and two more doctorates from that university—in political science (1922) and law (1923).

After teaching history at the Jewish Teachers College (*Juedisches Paedagogium*) in Vienna from 1919 to 1926, he was invited by Stephen S. Wise to teach at the Jewish Institute of Religion in New York. He remained there from 1927 to 1930, when he was appointed to a newly-created chair at Columbia University, the Linda R. Miller professorship in Jewish literature and institutions. This was the first appointment in Jewish history to the history faculty of any American university. The many such chairs that have since been established owe much to his example, and nearly the majority of their present occupants are Baron's former students.

Baron retired from his Columbia professorship in 1963 but until 1968 remained the director of its Center for Israel and Jewish Studies,

which he had founded in 1950. Both before and after his retirement, he lectured and taught in many universities at home and in foreign countries, including Israel. Upon his retirement from Columbia, the university took the rare steps of conferring an honorary degree on a member of its own faculty and of creating a new chair in Jewish history, named in his honor.

Baron was never a scholarly hermit. He was the leading spirit of the American Academy for Jewish Research and its president three times (from 1940 to 1953; 1958 to 1966; 1968 to date); president of the American Jewish Historical Society from 1953 to 1955; cofounder in 1936 (with Morris Raphael Cohen) of the Conference of Jewish Social Studies, called in its early years the Conference on Jewish Relations; and president of that body from 1941 to 1954 and 1963 to 1967. Right after World War II, he was the founder and president of Jewish Cultural Reconstruction (the executive director whom he chose was Hannah Arendt), which worked in identifying and reclaiming libraries and other cultural treasures despoiled by the Nazis. In 1952, he became a corresponding member of the United Nations Educational, Scientific and Cultural Organization's International Commission for a Scientific and Cultural History of Mankind.

The Nazi assault on the Jews was a very personal matter, for his roots were in Poland. His parents were killed there by the Nazis in the spring of 1942. The climactic moment of Baron's public career came in a Jerusalem courtroom in 1961, when he was called by the prosecution in the Eichmann trial to deliver a historical account of anti-Semitism. He spoke for many hours, with a uniquely individual learning and passion.

Baron's scholarly productivity has been extraordinary, in a class by itself both in breadth of knowledge and sheer quantity. The most recent bibliography of his writings appeared in the first of a three-volume jubilee publication issued in 1974 to celebrate his eightieth birthday. It contains five hundred items; most are articles, but there are dozens of books. Baron has always regarded himself as primarily a historian of the modern era, but he undertook several works of synthesis that cover the range of Jewish history. The major ones are: *The Jewish Community* (1942); *A Social and Religious History of the Jews* (1937), which has been revised through the middle of the seventeenth century.

While engaged in these major tasks, Baron also wrote such books as *Modern Nationalism*

and *Religion* (1947); *Russian Jews under Tsars and Soviets* (1964); and edited, together with Joseph L. Blau, *Jews of the United States, 1790-1940: A Documentary History* (1963). His abiding concern in all his work has remained the problem that he defined for himself in his first monograph, *Die Judenfrage auf dem Wiener Kongress* (1920), in which he discussed the relationship of Jewish needs and rights to the Gentile society, as they had been considered on this scale, for the first time in Western history, at a meeting of Europe's major powers at the Congress of Vienna.

Central to Baron's vision of Jewish history is his view that Jewish experience through the ages has not occurred in isolation, but in a larger world within which the Jews have lived. In some versions of Jewish history this world was ignored or was perceived as the unchanging enemy. Jewish history thus became an account of the inner life of the Jews and of the ways they could find to resist hostility (*Leidens- und Gelehrten-geschichte*). Such an approach required of the Jewish historian no great insight into the history of the various Gentile worlds, for, by definition, such knowledge was of little importance to understanding Jewish history. Baron has insisted that at no point in the whole of their experience have the Jews failed to come into serious encounter with other cultures. The image of a sealed community does not hold for even the most closed of ghettos. In addition, those influences have not always been of one kind. The multinational Austro-Hungarian empire and the multicultural late Roman empire are contexts for Jewish history that differ vastly from the religious and cultural monism of the generation of Mohammed. In each of the changing ages and climes within which Jews have lived, their history can be understood only as part of human history.

This view has important consequences. As a theory it reflects an outlook that refuses to regard the Jews as pariahs, always in a position of "otherness" and "over-againstness." Baron is no Pollyanna; he is keenly aware of the tragic dimensions of Jewish history. But neither is he a Cassandra. He has been preeminent in calling attention to the times of peace and even cooperation between Jews and Gentiles. Rooted as he also is in classic Jewish thought and feeling, Baron continues to believe, even in the twentieth century, that man is not irretrievably evil and that the human past is not a tale of unrelieved wickedness.

This view also has important consequences for defining the field of Jewish historical research. It increases enormously the burden upon the Jewish historian. To work in the manner of Baron, one must be a first-class general historian, who can write with Baron's originality, on such subjects as Ferdinand Lassalle's political theory and on the interrelations between modern nationalism and religion. The task is all the more onerous if the field of a historian's research is not limited to one period but extends over the whole of Jewish history.

Having asserted that Jewish history is part of general history, Baron must inevitably confront the question: What is different about the Jews? His answer stands in a line of classic Jewish thought rooted in the Bible itself. In his view, the Jews are a peculiar people because they have been the bearers, from their beginnings, of a Messianic religion of universal import. The meaning of the career of Jewry in history is to live, in the dimension of time, as a people who through a particular way of life, exemplified universal moral ideas. Other peoples are created in space, on their lands. Their sense of community is rooted in geography, and it tends to die when such a people is exiled. The encounters of the Jews with Palestine are indeed the historical apexes of Jewish creativity. Still, Baron sees more to Jewish experience, even in those periods, than the simple fact of a people living on its land. Even during the First Commonwealth, and certainly during the Second, the Jews were characterized by the presence of an influential Diaspora. When the complete Exile began, they were prepared by prior experience to exist without a base on their own land. This people yearned always to be restored to Palestine, but it could cope with the fact of landlessness.

Baron thus affirms the importance for Jewish history both of Zion and of the Jewish communities outside the homeland, and he passes no judgment on the merit of one against the other. Indeed, this international people, retaining a spiritual and communal identity through changing ages and places, is the harbinger of the human future, when all identities will be historical rather than geographical. Nor are scholars and men of the spirit alone the major bearers of Judaism. Peddlers, economic innovators, and factions struggling for the control of Jewish communal organization—all the people who make up a society are of consequence in his depiction. If Israel, the people, is the bearer of its spirit,

Baron's vision is broad enough to recognize that the spirit has expressed itself in the most mundane social facts.

ARTHUR HERTZBERG

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BASTIDE, ROGER

Roger Marius César Bastide (1898–1974) was a sociologist and anthropologist whose work reflected many personal and academic interests: mysticism, dreams, trances, madness, Afro-American cultures, and the comparative study of race relations. Between the years 1924 and 1938, Bastide taught at lycées in Cahors, Lorient, Valence, and Versailles. Between 1938 and 1951 he taught at the University of São Paulo in Brazil. On his return to France, Bastide served as a directeur d'études at the École Pratique des Hautes Études. In 1958 he became director of the Centre de Psychiatrie Sociale. Although Bastide retired from the Sorbonne in 1968, he remained an active scholar until his death.

Bastide and the Durkheimians. Bastide was not a Durkheimian in the strictest sense of the word, but his early academic work was conducted in a country in which the influence of Émile Durkheim and his nephew Marcel Mauss was still paramount. The Durkheimians regarded psychology and sociology as separate sciences, each occupying its own distinct territory. Any explanation of social phenomena in terms of individual psychology was regarded as suspect. Durkheim viewed religion as a product of social organization, and all attempts to explain it in terms of its emotional or intellectual value to its individual inventors were rejected by him as methodologically unsound. Attempts to relate variations in the suicide rate of different classes, as well as ethnic and religious groups to mental pathology and "race psychology" were rejected. One clear implication of the Durkheimian position was that certain phenomena—e.g., madness, dreams—were individual rather than social in nature and were therefore rightly excluded from the realm of sociology. Bastide's writings from 1930 until his death may be viewed as acts of loyal rebellion against some of these doctrinal positions.

In the first place Bastide, in some of his earliest writings on mysticism, was unwilling to accept the notion that religion is merely the product of other social phenomena. The mystic experience, the inexpressible and ineffable sentiment of union with the unknown, transcends

cultural boundaries and may even be attainable by the modern academic who knows no traditional faith and lives in a secular world (Bastide 1975). Second, Bastide was one of the first French sociologists seriously to consider the potential importance of Freudian theory. Although he condemned Freudian reductionism, he was not inclined to ignore Freudian and neo-Freudian contributions to the understanding of the growth of individual personality. Accordingly, he felt (1950) that the heirs of Freud and Durkheim had much to gain from a cautious interchange of ideas and perspectives. Third, a logical consequence of the latter position was Bastide's belief that sociology could successfully expand its territory and invade the psychologist's and psychoanalyst's realm. In an essay originally published in 1932 (reprinted in 1972*b*), "Materials for a Sociology of the Dream," Bastide claimed that dreams were, in a sense, social facts. Primitives used the dream experience as a guide to the cosmos and as a manual for social action. Furthermore, there was a relationship between the content of the dream and the social milieu of the dreamer. In his later work Bastide supported this argument with citations from the ethnographic writings of Peter Lawrence, Kenelm Burridge, and George Devereux, and with reports of his own interviews with subjects in São Paulo and Paris. In brief, Bastide applied Durkheimian notions to data that sociologists had excluded from the social realm. He believed that, insofar as social scientists excluded the "irrational"—e.g., dreams, trance, and madness—from their purview, they were the victims of the rationalist prejudices of contemporary Western man, who values only aspects of his culture that are of immediate technological and economic utility.

The extension of the Durkheimian program is clearly outlined in *The Sociology of Mental Disorder* (1965*b*). In this work Bastide, following Henri Ey and Michel Foucault, observes first that the definition of insanity varies geographically and historically; second, that the healing process, whether it involves shaman and sufferer or psychoanalyst and patient, reflects the expectations, beliefs, and desires of a third party—society; third, that each society has its own characteristic form of madness, insofar as the insane tend to act in the way society deems appropriate for them; and last, that much evidence has accumulated on the relationship between class, religion, ethnicity, and the incidence of mental disorder.

Brazil: The *candomblés*. Bastide's work in the sociology of religion, Afro-American studies, and race relations benefited greatly from his stay in Brazil. His earlier observations on religion in traditional and modernizing societies had been secondhand at best, and he had adopted the evolutionist view of religion that was still acceptable in some quarters. While in Brazil, Bastide conducted field research on the *candomblés* of Bahia (1958). The *candomblés* are cults in which African religious elements predominate. The gods of the Yoruba and Fon come down from Africa to take possession of their "horses" (cult initiates, who are usually female) in the course of elaborate, strictly regulated ceremonies that involve drumming, dancing, and trance states. Bastide's work on the *candomblés* reflected his growing interest in the question of African survivals in the New World. Melville Herkovits was, of course, the pioneer in this field, and Bastide was proud to follow in his footsteps. Bastide's scholarly thoroughness eventually led him to study Yoruba and Fon rituals on the other side of the Atlantic Ocean.

The most reported or misreported aspect of the *candomblés* was the trance state. Trances had traditionally been viewed as a phenomenon of psychopathology, and researchers had searched for evidence of insanity amongst members of *candomblés*. Bastide observed that, inasmuch as the guests at *candomblé* do not enter trance, and inasmuch as the possessed individual is expected to play the role of the deity who "rides" him while cult priests stand by to ensure that his actions become neither inappropriate nor overly frenetic, the trance is a social phenomenon, a rite that reenacts west African cosmogony in an alien environment. Furthermore, many of the cult initiates exhibit no sign of abnormality in their daily lives, which are conducted in an urban environment whose Luso-Hispanic culture and modern technology seem so remote from the "Africa" of the *candomblés*. Remarkably, participants in *candomblés* and other urban rituals of African origin seem able to dissociate these two aspects of their lives. Bastide elevated this behavior to the level of a scientific principle of social action, the *principe de coupure* or dissociation principle. Perhaps *candomblés* act as a compensation mechanism for the poor and oppressed. Bastide paid particular attention to the role of black women in *candomblés* and noted that this was a way in which they could still hold some power in a society that, the less African and more Brazilian

it became, increased rather than diminished the level of their subservience.

Bastide: Student of race relations. Bastide took part in two United Nations Educational, Scientific and Cultural Organization surveys in the field of race relations. The first began in 1950 in Brazil, as a result of a suggestion by L. A. da Costa Pinto, with a number of researchers working in different cities. Bastide and his student Florestan Fernandes undertook research in São Paulo. The second project was on African elites in French universities. The aim of the UNESCO project in Brazil was to attempt an explanation of the relative harmony of race relations in Catholic Brazil as compared with the Protestant United States. In his work on race relations, Bastide noted that miscegenation had been encouraged in Brazil and that a rich mulatto might well become an honorary white, whereas in the United States the slightest evidence of black ancestry relegated the individual to a low "caste" status, race in the United States being defined in social rather than biological terms. Bastide accepted Pierre L. van den Berghe's distinction between two ideal types of racially pluralistic societies, the paternalistic and the competitive. Brazilian society, being paternalistic and hierarchical, exhibited "color prejudice" rather than the virulent prejudice of "social race" that had been prevalent in the United States after the Civil War.

However, a more competitive pattern was emerging in São Paulo; as male blacks entered the proletariat, race prejudice was accordingly increasing. Noting that Calvinism and the early development of capitalism had been linked to the rise of racial separatism in the United States and elsewhere, Bastide endeavored to search for the reason why Protestant and Catholic countries had followed such distinct directions. He linked Protestant racism to the failure on the part of the Indians to live up to the unreal expectations of early settlers in North America that they should immediately pursue the work ethic and thereby demonstrate their worthiness to join the elect. The justification of racial separation by the Dutch Reformed Church in South Africa was perhaps a direct consequence of Calvin's denial of the doctrine of the mystic body of Christ and his support of the idea of a national church. There was, perhaps, much of value, but nothing that was particularly original in these assertions. Bastide's work on the role of sexual contact as well as sexual fantasies and dreams in racial conflict displays a striking origi-

nality. On the basis of interviews in São Paulo, Bastide observed that the mere fact of miscegenation was not necessarily the sign of good race relations. In fact, the reverse was more often true. In Brazil the young white exploited the black girl in order to gain his sexual initiation. Once he had slept with her, her social reputation in her own community declined. In France, many African students slept with white girls in order both to enhance their self-image by "climbing" the social ladder and to exact revenge for humiliation by whites, whereas white girls used black men as a symbol of their rebellion against their parents and conformist values. Once the African married the white girl, which he was often loathe to do, he could no longer return to the colonial environment and had to remain as a marginal individual in metropolitan France.

Bastide: Social visionary. Bastide's work, both in race relations and in the sociology of religion, reveals his staunch belief that sociology is and should be the reformer's science and should pursue the destiny that its creator Comte intended for it. He was a social democrat who believed in a socialism which would incorporate Christian ethical values. He was a defender of African traditions who proclaimed that the religions of west Africa were as beautiful as those of Greece and Rome. Last, but not least, he stated that his primary professional commitment was to engage in the most important battle of our times, the struggle against racism.

ANDREW P. LYONS

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BATESON, GREGORY

Testing the fit and stretch of a new idea is not unlike trying on a garment. Ideas, however, unlike clothing, are best appreciated when we have been privy to their development—the cutting and stitching of the cloth, and all the painful trials in the testing of the fit. Regrettably, few among us have that degree of unself-conscious commitment to ideas that allows us to expose them (and ourselves) during their growth. Gregory Bateson is among those few. As a consequence he has achieved unusual influence

upon scholars of widely varying interests and ages.

Bateson was born May 9, 1904, son of William and Beatrice (Durham) Bateson. His father was a well-known geneticist and the Bateson clan generally has always been well represented in scholarly circles.

Bateson's earliest published work reflected anthropological interests, and though much of this work was of a seminal character, it was, at the time, largely outshone by the ebullience of the contributions of his first wife (and colleague), Margaret Mead. *Naven* appeared in 1936, and represented an original application of the comparative method of anatomists to anthropology. A joint photo study with Mead, *Balinese Character*, produced a few years later, was a blend of science and aesthetics that is rarely achieved. His next major series of scientific publications were in the field of psychiatry, followed by analyses of communication in a variety of animals. This brief list by no means encompasses all of Bateson's contributions, nor does it provide more than a suggestion of his Renaissance mind capable of dealing with a variety of subjects. Actually, Renaissance men were more often than not dabblers; although engrossing, entertaining savants, their contributions were fleeting and insubstantial. There is nothing of the dilettante in Bateson, nor does the range of his subjects reflect an absence of continuity in his attention.

The focus of Bateson's attentions have been questions and ideas: How do they interact? What sort of economic order limits their multiplicity and survival? He wrote: "In late 1969 I became fully conscious . . . that in my work with primitive peoples, schizophrenia, biological symmetry, and in my discontent with the conventional theories of evolution and learning, I had identified a widely scattered set of benchmark marks or points of reference from which a new scientific territory could be defined. These benchmark marks I have called 'steps' . . ." (1972, p. xvi). Thus does Bateson himself argue for the continuity underlying his various scholarly tasks. But it is not his contributions to the various disciplines, nor yet the large net into which he drew students from anthropology to zoology, that has earned him his influence. It is, rather, his emphasis on seeking to understand how we come to understand, and the role of consciousness reflection in adaptation. Or, in his words: "It is surely true that the content of consciousness is no random sample of reports on events

occurring in the remainder of mind. Rather, the content of the screen of consciousness systematically selected from the enormously great plethora of mental events. But of the rules and preferences of this selection, very little is known" (quoted in M. C. Bateson 1972, p. 16).

Bateson's major opus of the 1970s (although another major work is in preparation) was a collection of essays entitled *Steps to an Ecology of Mind* (1972). It is in this volume that he admitted the world to his dressing room to observe how he tries the fit of his new garments, a procedure in which hundreds of his students and fellow conferees were able to participate directly. He discussed the relation of principles to hypotheses, of hypotheses to facts, and the latter to our perceptions. Implicit throughout is the view that scientific research entails two disparate kinds of authority: observations cannot be denied and fundamental principles must be fitted (or replaced). He rejects a belief in induction from experience. Such a rejection would scarcely be novel for a modern philosopher of science, but is decidedly unusual among practitioners and teachers of sciences. How then to construct "a bridge between the facts of life and behavior and what we know today of the nature of pattern and order" (1972, p. xxvi)? Bateson has generated enthusiasm and commitment to the search by allowing witnesses to his struggles with that construction.

It is unlikely that Bateson's "double-bind" theory on the cause of schizophrenia will effect a revolution in the treatment of that disease. Nor will his analysis of the relation of analogy to homology in the study of morphology radically alter views on vertebrate evolution. However, what has revised the intellectual habits of his students is Bateson's belief that "such matters as the bilateral symmetry of an animal, the patterned arrangement of leaves in a plant, the escalation of an armaments race, the processes of courtship, the nature of play, the grammar of a sentence, the mystery of biological evolution, and the contemporary crises in man's relationship to his environment, can only be understood in terms of . . . an ecology of ideas . . ." (*ibid.*, p. xv).

Some intellectuals have argued that the complexity of our universe so far exceeds the complexity of our central nervous system as to preclude any but the crudest representation of the one by the other. This despairing counsel was an altogether expectable response to the ecological disasters that resulted from reduc-

tionist simplifications (as chronicled, for example, in Commoner 1971). Bateson's greatest contribution has been not merely to urge rejection of both extremes, reductionism and "standing in awe," but to pioneer in the development of more fruitful epistemologies (quoted from Bateson 1958, p. 54).

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BECKER, ERNEST

Ernest Becker was a rare, panoramic intellectual who dreamed the Enlightenment dream of a unified, universally accepted science of man which could serve as the basis for the rational

formulation of human ideals and the means of achieving them. This intellectual quest gave him a personal sense of history, purpose, and meaning. At the same time, it caused him great disappointment.

Becker was born to Jewish immigrants in Springfield, Massachusetts, on September 27, 1924. He enlisted in the infantry during World War II and served with a second-line unit that won five campaign ribbons and helped liberate a Nazi concentration camp. After college at Syracuse University he served as an administrative officer and occasional intelligence liaison with the United States embassy in Paris. He loved France and learned French fluently, but became bored with his work. At the age of 32, he realized that he wanted to understand the meaning of his existence and so he decided to study anthropology because the term literally signified "the study of man."

In 1969 he wrote to the Reverend Harvey Bates: "I have arrived at a definition, finally, of the human personality that I think accurately reflects the basic truth: that what we call man's personality or his life style is really a series of techniques that he has developed, and that these techniques have one major end in view—the denial of the fact that he has no control over death or over the meaning of his life. If you expose this denial by undermining or exposing his techniques, you undermine his whole personality—which is the same thing" (Bates 1977). This central, fertile insight of his mature work was not the result of intellectual study alone; it was also a personal confession—the result of sober self-reflection on the meaning of his own life. Becker believed that thought must be understood in the context of its author's life and times, and his theory of human nature, designed as it was to guide our understanding of mankind, was also Becker's guide to understanding himself.

As a graduate student in anthropology at Syracuse University, Becker came under the formative influence of Douglas Haring, who was trained at Columbia under Franz Boas and Franklin H. Giddings. Becker's first book, *Zen: A Rational Critique* (1961), is an edited version of his doctoral dissertation, completed in 1960. It is dedicated to Haring with the motto: "the teacher imparts the spirit." By this Becker specifically meant the spirit of free reason as the guide of human progress. This was his basic faith and the ideal-critical basis of his science of man. This faith implies a battle with the anti-

rational forces of authority, tradition, superstition, and passion which constrict reason and freedom and foster instead uncritical obedience, compliance, and conformity. This antiauthoritarian rationalism is the underlying spirit of Becker's first book which was a critical comparison of Zen, psychotherapy, and thought reform. The mechanism by which the individual surrenders his autonomous reason to authority in each of these methods of personal transformation is the psychoanalytic concept of transference: the submission of the vulnerable individual to a powerful protective authority. Becker's first step toward an interdisciplinary theory of human nature was to claim the concept of transference from medical psychiatry for the science of man.

Haring was Becker's ideal social scientist: an interdisciplinary scholar who could construct a unified but critical perspective on particular human problems. Haring was a Japanese specialist and served as anthropological adviser to the Douglas MacArthur occupation in post-World War II Japan. This is the model that shaped Becker's personal ambition. He was never ambitious for wealth or power; he wanted the recognition and self-affirmation he believed would be rightfully his if he formulated a comprehensive and accurate account of the human condition which could contribute to human happiness. Becker hoped that if his work counted for something in human history, then he would count for something.

Becker was awarded his Ph.D. in June 1960, and then joined the department of psychiatry at the nearby Upstate Medical Center as instructor in anthropology—essentially the same position held by Haring. The next three years were idyllic. Becker developed a close relationship with Thomas Szasz, best known for his critique of psychiatric authoritarianism and its ideological correlate, the medical model of psychiatry. He saw in Szasz a broadly educated, incisively critical mind and an Enlightenment thinker, a Jeffersonian, who shared Becker's basic faith in individual liberty and the reign of free reason. Becker agreed with Szasz's critique of medical psychiatry but he was especially intrigued by his analysis of hysteria as a problem in communication, which suggested the possibility of "a re-assessment of all so called 'mental illness' as broadly cultural behavior rather than narrowly medical, phenomenon" (1964, p. 3). As a result of his association with Szasz at this historical moment in psychiatry, Becker was empowered

to translate and reinterpret the psychiatric syndromes from the language of medicine into the language of the combined science of man. Becker learned clinical psychiatry as an insider, in an open and invigorating intellectual climate. He attended clinical lectures and conferences and met frequently with Szasz and a small circle of intellectuals for informal, wide-ranging, and detailed discussions of human nature and mental illness in the context of the history of ideas. With Szasz's encouragement, Becker interviewed selected psychiatric patients and became a highly skilled clinical diagnostician.

In *The Birth and Death of Meaning* (1962b), an edited series of lectures to psychiatric residents, Becker drew from psychoanalysis and the social sciences to present his integrated theory of human behavior, which was the foundation of his work. The key idea is that self and society are derivatives of the human capacity for language. This means that the evolution of both the human species and the human individual are marked by the development of language. Becker reformulates the concept of the Oedipus complex from a theory of infantile sexual neurosis to a theory of the development of specifically human symbolic reactivity. The Oedipal transition, as Becker renamed it, is the period of socialization in which the individual learns techniques for mastering the anxieties of self-maintenance in the theater of social symbolisms. He conceived of personality formation as the narrowing (fetishization) of consciousness and performance to a range within which the individual masters anxiety. This mastery of anxiety through socially approved command of performance is self-esteem. Becker regarded self-esteem as the basic invariant motive of human action and the central integrative principle of his science of man. Broaden the theory of anxiety with the concept of the terror of death and one has the nucleus of Becker's mature work.

In his next major work *The Revolution in Psychiatry* (1964) Becker analyzed various psychiatric syndromes—schizophrenia, depression, psychopathy, anxiety, shame, guilt, sadism, and masochism—from the point of view of his unified theory of human nature. Heavily influenced by John Dewey and the American pragmatists and symbolic interactionists, yet drawing broadly from philosophy and the social sciences, Becker constructed a theory of mental illness based on the struggle of the linguistic self to maintain reflexive significance in the field of

social action. The basic problem for the human animal is anxiety because he is necessarily constricted by his transferences and fetishizations, but is constantly confronted by an incomplete sense of self and an uncertain future. Following Szasz, Becker viewed mental health and illness not as specialized medical problems, but as problems of living. The task of psychiatry therefore, as viewed by Szasz, Becker, and their circle, is the task of education and social reconstruction. Becker's revolution in psychiatry meant the absorption of psychiatry by the science of man.

Psychiatry was quick to react. In November 1962 Szasz received an order from the New York State Department of Mental Hygiene forbidding him to teach psychiatric interns and residents of the local state hospital, the main clinical teaching facility of the medical school's department of psychiatry. The reason given was that Szasz's concept of the myth of mental illness was dangerous to mental patients. Szasz, Becker, and others interpreted this as an infringement of academic freedom. They wanted the order rescinded or the department of psychiatry to sever its relationship with the state hospital. They decided as individuals to boycott the departmental meetings that were held at the hospital from which Szasz had been expelled. The members of this group were thus brought into direct conflict with the chairman of the department, who was also the director of the state hospital and was responsible to the commissioner of mental hygiene. He was supported by the ideological adversaries of Szasz and Becker. This produced an emotionally charged schism within the department that eventually engulfed the entire field of psychiatry as it came under criticism for the coercive tactic of involuntary hospitalization. At Syracuse, the result of this conflict was that Becker and several of his associates were dismissed and excluded from academic psychiatry. Becker's theories of mental illness have been ignored by medical psychiatry ever since.

Becker spent the following year in Rome reflecting on the historical origins and significance of his unified theory of human behavior. The result was *The Structure of Evil: An Essay on the Unification of the Science of Man* (1968). In this, he oriented his theory in the context of Western thought from the breakdown of the medieval world. The key period is the Enlightenment in which the problem of evil is transferred from the dominion of the medieval

church and feudal authority to the dominion of free reason. Becker believed that the proper function of the science of man is to replace traditional religious cosmology with an integrated, panoramic, and penetrating rational-empirical understanding of man and his place in the universe. This understanding could be used as a practical guide to the solution of life problems and the realization of human ideals. The urgency of contemporary human problems implied to Becker that social scientists must synthesize what is now known rather than to continually accumulate disjointed data in the hope of the future perfection of their science. This attitude, conveyed with messianic zeal, irritated and threatened many of his academic colleagues.

Becker firmly believed that he had realized both the Enlightenment and the Marxist vision of an ideal-typical science of man, which could be used as the basis of a critical analysis of both the individual and society. As Becker conceived it, mental health is a pseudomedical ideal individual type against which personal flaws (mental illness), or "failed heroics" as he called them, could be measured. Becker recognized that ideal-typical science was vulnerable to use as crypto-ideology; it could be used to promote authoritarian values, as in psychiatry in the Soviet Union, the United States, and elsewhere. But Becker was clear about his ideal-type individual: a free, self-reliant person with a flexible library of competent social performances who approaches life with an open, ideal-critical intelligence. This implies that in order to foster the free, creative energies of the individual, the ideal society must be an open democratic community, in the anthropological sense, which guides its destiny scientifically, by means of an integrated world view. Becker remained faithful to this ideal, but he struggled continuously with the obstacles to its achievement. In his early period, he adopted the Rousseauian position that man is basically good, or neutral, and becomes evil as the result of an authoritarian-repressive-alienated society. Later, he recognized that evil springs also from the breast of man—from the ego's restless striving to deny its own finitude by asserting its own perfection, power, and perpetuity regardless of the cost to humanity.

In the fall of 1964 Becker returned to Syracuse University, where some friends helped him to arrange a joint appointment for one year in sociology and education. Although he was ostracized from the medical school, he continued to refine his theory of mental illness, stimulated by

frequent, informal discussions with Szasz and his circle. He also became friendly with a small group of radical intellectuals at the university, including Stanley Diamond and Irving Louis Horowitz. The student rebellion against university paternalism was brewing, and the civil rights struggle was intensifying, led in Syracuse by George Wiley, Rudy Lombard, and Ron Corwin. Becker knew and admired these local leaders and shared their hope that these movements would catalyze a new era of reason and freedom in human affairs. In his lectures and public discussions, Becker presented his unified theory as the ideology for a radical critique of the issues raised by the student, civil rights, and antiwar movements. He focused particularly on the process of university education as a technique of socialization. In *Beyond Alienation: A Philosophy of Education for the Crisis in Democracy* (1967), Becker argued that the aim of education should be to develop the ideal type specified by his science of man: the competent, intelligent, self-reliant individual. He proposed as the ideal curriculum for achieving this ideal, the science of man: an integrated presentation of current knowledge about the human condition, the human ideal, and the method of progressing toward that ideal.

Becker was critical of the alliance of the university with patriotic and commercial interests. He believed this has the effect of constricting consciousness, fostering uncritical obedience to authority, and promoting fetishization of commodities. Becker was also outspokenly critical of empirical social science which, he believed, gave silent assent to evil by pursuing methodological purity for its own sake. Again, as a result of his views, Becker was not rehired.

In 1965, with some help from Erving Goffman, Becker was appointed visiting lecturer in sociology at the University of California at Berkeley for a one-year term. In 1966 he moved to anthropology on a similar contract. At Berkeley, during the period of student protest, Becker continued to speak boldly to the problems of the times. He became a favorite of the students who crowded his lectures. When his second appointment expired and it became apparent that the university would not rehire him, 2,000 students petitioned to retain him. In an unprecedented act, the student government voted to pay his salary of \$13,000 from their own treasury, but the administration forbade this. In the fall of 1967 he moved across the bay to San Francisco State University as professor of social psychology.

These were years of intense inner contradiction for Becker. On the one hand, he believed that the war in southeast Asia and the internal resistance to civil rights were of one piece and would lead to military-industrial totalitarianism. The martyred Protestant theologian Friedrich Bonhoefer was his model and he admired the heroics of Martin Luther King, Jr., David Miller, Daniel Berrigan, and others who took positive moral action. On the other hand, he was aware that his personal desire for self-affirmation was a basic flaw.

Becker was haunted by this contradiction and attempted to resolve it through the Christian existentialist attitude of positive action against evil combined with the acceptance of God's will. However, he discovered that the line between the heroic strivings of the ego and genuine Christian courage is ambiguous. Although he loved San Francisco and could have lived and worked there for many years, he resigned in January 1969 from San Francisco State University because he claimed that he could not teach freedom with the police on campus. This was a courageous act since he had no prospect of another job.

Becker wrote two books during his California period which reflect his inner struggle with the contradiction between piety and heroism. His spiritual task of renouncing heroic striving enabled him to accept as a social fact the failure in his time of a unified science of man. He wrote a historical account of this fact in *The Lost Science of Man* (1971). Yet, he retained his faith in an ideal-typical social science, not with the naive, utopian optimism of his youth, but with the sober hope that it could serve as a "holding action" against the forces of evil. In *Angel in Armor* (1969), a series of short essays, Becker drew from literature and film to illustrate the relevance of his science of man to universal human experiences.

In the fall of 1969, Becker moved to Simon Fraser University in Vancouver, Canada, as professor in the combined department of sociology, anthropology, and political science. Here, he completed his final trilogy. He rewrote *The Birth and Death of Meaning* ([1962b] 1971) for its second edition, integrating the theme of the denial of death into his basic theory of human nature. With the added richness of a decade of life and study, this book is the best introduction to his mature work. *The Denial of Death* (1973) is based on the concept of a basic paradox in human nature—self-body dualism—which is the

origin of human tragedy. Man is haunted by the inescapable contradiction that he knows his body is imperfect and doomed to death, yet can imagine and desire perfection and immortality. This contradiction generates anxiety. For man is plagued both by the desire to expand in the search for self-validation and by the desire to constrict in the search for safety from death and meaninglessness. The terror of death is mastered through denial, by means of the affirmation of its opposite: the "reality" of self and society. But death cannot be evaded. Hence, self and society are vital lies, or illusions. They are vital because meaningful social action depends upon them. But they are lies because they blind us to our basic creatureliness.

In his third work during these years, the posthumously published *Escape From Evil* (1975), Becker further developed the idea that everything man does is an ultimately futile attempt to transcend his creatureliness. Everything he does, therefore, is religious and heroic, but fictitious and fallible. The illusory ground of our immortality mechanisms creates in us a sense of guilt, which Becker defines as hesitation in fear that one has expanded too far to be sustained by one's limited powers. To overcome guilt, man, like other animals, must draw power from external sources. But for the symbolic animal, power is symbolic. One mechanism for drawing self-power from others is transference, which Becker believed no one could live without. Transferences are loci of safe operation. But when they are excessively narrow, as they tend to be under modern conditions of alienation, they become loci of repression and unfreedom in the web of other persons.

Another mechanism for symbolically increasing self-power is sacrifice, a primitive ritual for increasing the pool of life power in the group. This same mechanism is the basis for the ostensibly secular practices of the vendetta, the blood feud, scapegoating, and prejudice. It appears in interpersonal sexual form as sadism—the ritual increase in one life force at the expense of another. Money is also an immortality vehicle. Becker suggests that man's basic striving for immortality takes the form of a search for prosperity, the universal ambition of mankind. Money began as a fetish symbol for God—giver of prosperity. In modern secular society our gods and chiefs are still engraved on our money; and although we have forgotten its religious significance, money continues as a symbolic source of personal power which is psy-

chologically equivalent to the denial of creatureliness, the denial of death, and the illusion of immortality. Indeed, in Becker's view, patterned social behaviors are basically religious rituals in which individuals heroically play out their immortality schemes in everyday life.

With this view of the human condition, the pivotal question for Becker became: "On which level of illusion should man live?" His answer is that man should live at the grandest heights—the religious level. Not through unreflective compliance with rules and dogma, but in the primitive Christian sense, through humility—the acceptance of creatureliness; and through faith—the transference to the Ultimate Ground of Being. At this level the psychological ideal of mental health is merged with the religious ideal of enlightenment. The ideal—typical individual is the religious hero, on the model of Buddha, Moses, and Christ, men with a self-disciplined awareness of the transient, contradictory, and fictional qualities of self and society who are open to the mystery of the cosmic forces which shape ultimate human destiny.

Many of Becker's colleagues were threatened by his efforts to merge social science and religion. By rejecting Becker, they believed they were protecting the rational—empirical tradition from possible contamination by irrational and cultic influences. Ironically, Becker's merger of the scientific and religious perspectives was for him a logical imperative of his rational approach to human nature. Nevertheless, there seems little possibility of Becker's theories being integrated into the social sciences until these sciences are open to a merger with theology.

By the summer of 1972, Becker's major work was complete. In November, 1972 he developed an intestinal obstruction which, after emergency surgery, was diagnosed as cancer. He died on March 6, 1974, at the age of 49. On May 6, 1974, he was awarded the Pulitzer Prize in nonfiction for *The Denial of Death*.

RONALD LEIFER

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BERNARD, JESSIE

Both Jessie Bernard's life history and her sociological contributions emanate reasonable, but unyielding, defiance—defiance of family tradition, life styles, occupational trajectories, sociological paradigms, and popular myths, as well as age-related patterns. In the 14 solo books, 7 coauthored or edited volumes, 25 chapters for other authors' or editors' books, and the 60-odd journal articles, her humane, but unflinching, examination of the "givens" of sociological, and often psychological, historical, and political, phenomena bespeak challenge and critical analysis of the status quo.

Bernard's unpretentious style, in her written work as well as in her personal and professional life, has created a medium in which her often defiant, even radical, message ultimately is attended without arousing resistance. Her private history may be read as a contrapuntal theme of amiable, but undaunted, rebellion against the expectations that family and society pressed upon a female born shortly after the turn of the century and living through two world wars and the subsequent societal upheavals that were to mark the second half of the twentieth century.

Early background. Born in Minneapolis, June 8, 1903, to parents of Romanian Jewish heritage, Jessie Bernard was the third of Bessie Kanter and David Solomon Ravitch's four children. Her maternal grandmother, Betsy Kanter,

set an early model of feminine pioneering (a topic which later was to concern Bernard in her own research on academic women, black women, and early American frontier women) by her emigration to the United States as a young widow in the late 1880s. Leaving her two grown sons behind in Romania, Bettsy Kanter traveled from Transylvania to New York, accompanied only by her preadolescent daughter, Bessie. Bessie Kanter, later the mother of Jessie Bernard, worked as a young girl in the New York garment district, demonstrating her own pioneering mettle by marching for women's suffrage and chafing against her mother's refusal to let her join the union.

Almost six years later, when the brothers in Romania had arranged a suitable marriage for their sister, both Bessie Kanter and her mother returned to their native country, where Bessie married David Solomon Ravitch, a local candlemaker. Shortly thereafter, the young couple and Bettsy Kanter returned to the United States, this time, as the 1900 census records, settling in Minneapolis, where the bridegroom's brother lived (a pattern of immigration Bernard describes in an anonymously authored article, "An Analysis of Jewish Culture" [1942*b*]). Even after Bessie and David Ravitch had their own family of four children, Bettsy Kanter reigned as the "cheerful, industrious, pious" matriarch of the family.

Bernard's Jewish cultural heritage, dispensed—in daily prayers and rituals—by her grandmother, was counterpointed by the "Americanization" practices which her older sister, Clara, another pioneering influence, brought home from public school. The acculturation Bernard experienced through her sister, whom she regarded as second generation (compared to her own second-and-a-half generation status) (1978, p. 327), served as a firsthand model for understanding and empathizing with the American acculturation experience of black generations in the postslavery years. It was Clara Ravitch who "pioneered the way" (1978, p. xix) for Bernard, not yet 17 years old, to enter the University of Minnesota, in January 1920, where she was a student of Pitirim Sorokin, Alvin Hanson, N. S. B. Gras, and Karl Lashley, all before they left for teaching posts at Harvard University.

Adulthood, marriage, and social positivism. In her freshman year, Jessie Bernard audited a sociology class of another prominent sociology professor, Luther Lee Bernard, 21 years her senior, whom she served as a research assistant

and, in 1925, married. The marriage occurred in defiance of her family's serious objections to age and religious differences. During the 21 years of their marriage, L. L. Bernard created a personal and intellectual milieu infused with social positivism within which Bernard grew to maturity, but then ineluctably renounced. L. L. Bernard, with whom Jessie Bernard, as a graduate student at Washington University, coauthored *Sociology and the Study of International Relations* (1934) and *Origins of American Sociology* (1943), was a sociological heir to Auguste Comte's nineteenth-century social positivism. She received her Ph.D. from Washington University in 1935.

It was on this intellectual foundation of social positivism that L. L. Bernard based his belief in social science's potential—through scientific, particularly mathematical, methods—for objectively determining "the best way to achieve desired societal ends" (1978, p. 130). Thus, social positivism was to transfuse Jessie Bernard's personal and sociological perspective for almost two decades, from her undergraduate days and early graduate work at Minnesota through her doctoral work at Washington University, where she was trained "strictly in the measurement tradition" (personal communication, 1979). Only the mid-1940s, the shock of the Nazi Holocaust ultimately evoked her disbelief, denial, and defection—intellectually, from the social positivist paradigm, and emotionally, from her mentor—husband, who personified that position.

The death of 69-year-old L. L. Bernard in 1951 left Bernard responsible for their three young children. Thus, she personally became a reluctant pioneer in new sociological territory—the female-headed family, which was to become a major family pattern of the future, as well as a focus of her own research. Again, Bernard's personal life as widow and female family head provided the experiential background for empathetic and insightful understanding of a major emerging sociological problem.

The beginnings of an independent career. Since her arrival with an aging scholar—husband at Pennsylvania State University in 1947, Jessie Bernard had been assisted in academia by another sociologist, Seth Russell, the chairman of the sociology department and later, associate dean. Under Russell's protective guidance, Bernard "shot . . . up the academic ladder to the top in record time" (1978, p. 5). During L. L. Bernard's declining days and last illness, Russell developed a close relationship with the family, becoming a father surrogate, particularly to the

youngest child. During the ensuing two-and-one-half years, Bernard relied on Russell for emotional and familial help; however, by the fall of 1953, with her youngest child in tow, Bernard had left for Europe to study postwar trends in sociological research and to mend her emotional wounds.

Before coming to Pennsylvania State, Bernard published her first solo book, *American Family Behavior* (1942a), and in the early years in College Park, Pennsylvania, she wrote *American Community Behavior* (1949a). In both volumes, there is a "preview of things to come," a foreshadowing of later work in the early efforts to measure how well the American family was performing its several functions, success in marriage, and the degree of "institutionalization" of marriage and family norms, topics in which Bernard would maintain a lifelong, but expanding, interest.

Renunciation of social positivism and academia. Bernard's first major intellectual defiance—against social positivism—had left a permanent residue of skepticism about social science's potential for changing the social order according to any rational plan. The trail of Bernard's disenchantment with the "social-salvation-through-science creed" is traced in a series of articles between 1947 and 1950 (1947; 1949b; 1949c; 1949d; 1949e; 1950a; 1950b). Later, in *Self-portrait of a Family* (1978), she recalled her disillusionment in a "faith" she had not questioned for more than 25 years: "It took a catastrophe as overwhelming as the Nazi Holocaust to shake that faith. A whole network of intellectual, emotional, and moral roots were dug up and exposed in the process. The faith on which I had built a life was shaken, the nineteenth-century faith in the benign nature of science, a belief that it was intrinsically good" (p. 131). Although her faith in social science's capacity to shape the social order was extinguished, Bernard's reliance on research findings as the best, or only available, evidence remained a hallmark of her scholarly treatises on dating, marriage, mating, motherhood, divorce, remarriage, and female culture.

After Bernard's year in Europe, she returned to Pennsylvania State, where she continued to work on two new volumes, the first study of *Remarriage: A Study of Marriage* (1957a) and *Social Problems at Midcentury: Role, Status, and Stress in a Context of Abundance* (1957b). A year as a visiting professor at Princeton University (1959/1960), the first year in which

women were allowed to enter the front door of the university library, proved to be a lonely and difficult experience. She later recalled smiling in the faculty club at faces that did not smile back. The next year, Bernard moved to Washington, D.C., where she spent her sabbatical year. From 1962 to 1964, she commuted from Washington to teach her classes at Pennsylvania State; however, by 1964, she had decided to abandon the constraints of academic life, where she sensed it was necessary to "censor [her] ideas to fit the pattern of ideas surrounding [her]" (personal communication, 1979).

Academic women and a new intellectual thrust. Bernard's break from academia came almost simultaneously with the publication of her most significant work to that time, *Academic Women* (1964). This study of women in academia, which received the Pennsylvania State University Bell award and later the Kappa Gamma honorary award, heralded a new thrust in intellectual energy and direction; however, as Bernard herself perceived, the academic Establishment responded initially with "a great big yawn" ([1964] 1974, p. xxvii).

Academic Women explored the condition of women who taught and conducted research in academe, dispassionately examining the evidence for and against discrimination. In addressing the question of discrimination ahead of the tide, it presaged Bernard's later attempts to identify and deal with significant social issues—abortion, battered women, child abuse, the culture of poverty, female-headed families, and sexism—before they had become the incendiary social questions of the day. Among her most influential works, both despite and because of the feminist criticism levelled against it, *Academic Women* scrutinized the factors—both formal and informal—that accounted for women's subordinate role in academia, before concluding that sex was more salient than role as a status determinant. Bernard accounts for women's unequal condition in terms of their propensity to teach in colleges rather than universities, to teach rather than undertake research, to act as bearers or transmitters of established knowledge rather than "men of knowledge," to follow patiently rather than innovate boldly—without tracing the discriminatory practices that force women into such "choices."

Bernard recognizes that scientific productivity is a function of a researcher's position in the communication system rather than of his or her sex. And she describes the "stag effect," a

subtle process excluding women from the informal communication system along which emerging scientific knowledge is disseminated. Although Bernard described the palpable procedures and processes of sexism (a term not yet coined), she fell just shy of recognizing them as the informal underpinnings of discrimination. Despite highlighting the informal mechanisms by which women in academia were prevented from developing their natural academic potential, she concluded that no formal or structural discrimination existed in the Halls of Ivy. Bernard's analysis of the "stag effect's" preclusion of women from the informal academic communication network and her awareness of the relationship between innovation and a public of peers did not yet include an unequivocal recognition of the underbelly of discrimination. In *Academic Women*, Bernard senses the presence, but does not yet perceive, as she will in her later works, the crippling relationship between the informal practices and attitudes of sexism and the formal structural manifestations of discrimination.

Academic Women touched the beachhead of concern about women's condition in society just ahead of the swelling wave of feminism. The radical feminists railed against Bernard's conclusion that no formal discrimination existed and identified the informal and subtle processes she had sensed and described—but not recognized—as "sexism." After her own conversion to a feminist perspective, Bernard would side with her critics and applaud their deserved critique. But *Academic Women* was the beginning of a new surge of intellectual strength, which even the male-dominated academic Establishment could no longer ignore.

Major themes and patterns. Over the next 14 years, Bernard's work gained a new momentum, eventually breaking through the functionalist (although not Parsonian) paradigm that had held mainstream sociology in its thrall and entering the new terrain of the feminist perspective. (Parenthetically, it is relevant to note Bernard's defiance of the mythology about age and creativity.) Beginning in her sixth decade, long after conventional wisdom would have us believe creativity is quelled, she began to mine a new, related, richer sociological vein that would prove to be her strongest and most prodigious intellectual contribution.

A chronological review of four decades of Bernard's work reveals complex recurrent patterns. Themes touched upon in her early work

reemerge later, each time with greater maturity and enlarged understanding. The seeds of later books can be seen as they are unconsciously planted, often three or four volumes earlier. Ideas and insights first appear, as grains of sand in an oyster, then begin to expand, creating an irritating, insistent presence, finally culminating in an independent entity—a new volume.

A case in point is Bernard's treatment of biculturalism, first discussed in terms of Jewish immigrants in America (1942c). She later expands this theme in *Marriage and Family Among Negroes* (1966) by developing the notion of two black cultures—the externally adapted and the acculturated—within the larger white context.

The Sex Game (1968b), an interactionist approach to communication between the sexes, returns to the bicultural motif. Here Bernard elaborates the concept to depict the sexes as two large, relatively unstructured collectivities living in sexual apartheid, beset by the difficulties common to discourse between inhabitants of different cultures. In *Women and the Public Interest: An Essay on Policy and Protest* (1971), a variation of the bicultural theme is introduced: women's sphere. This variation is further explored the following year in *The Future of Marriage* (1972), probably her most widely read published work. Here Bernard conceptualizes each marital union as composed of two different, noncoinciding marriages or worlds, in which the wife inhabits her own sphere, distinct and isolated from her husband's marriage. Later, in *The Female World* (forthcoming), the concept depicted in the title is elaborated to include a complex analysis of women's station, sphere, social class, and culture. Now not only do women live in different marriages than their husbands, but they live in different families, educational systems, social strata, occupational structures, political and cultural realms, as well. In short, from the time they enter their first "pink world," women live in the female world—a highly structured society.

Bernard does not deny that women also live in the world of men, simply that they are not "of" that male world, in much the same way that an American living in Paris is not a *bona fide* Frenchman. The anatomy of the female world's unique class structure is probed, and Bernard suggests that the very subconcepts underlying the notion of social class (long accepted in mainstream—read "male"—sociology

and applied to the male world) fail to reflect the class structure of the complex world within which women spend much of their lives.

Other major themes weave in and out the fabric of Bernard's work: formal *vs.* informal discrimination; power; women as a subordinate, dependent group; "stroking" as a fundamental unit of social action; the conflict between women's family and work roles; sexism as power relationships; the deteriorating effect of housework and total responsibility for child care on women's mental health; homosociality; sex differences, including typical *vs.* characteristic differences between males and females; biological sex roles *vs.* cultural gender roles; the function of stereotypes as mechanisms for papering over the lack of fit between gender roles and individual differences; tipping points and turning points; social policy as an instrument for alleviating the disadvantages women face; the historical youthfulness of the nuclear family; and the "cichlid effect."

This recurrent and intensified exploration of themes, particularly since the appearance of *Academic Women*, represents a relentless search for new understanding, even when that enhanced perception would require relinquishing comfortable metaphors and previous interpretations. Thus, in *Women and the Public Interest: An Essay on Policy and Protest*, Bernard discusses the pervasive influence of the "stroking" or supportive function performed by females.

Although Bernard marshalls evidence that both males and females are capable of stroking (or expressive) as well as instrumental behavior, she notes that women's specialization in stroking in most cultures throughout the world contributes to their subordinate position. Harking back to the Bales (1950) instrumental—expressive dichotomy, she describes stroking as expressive, supportive behavior in which the stroker "shows solidarity, raises the status of others, gives help, rewards, agrees, concurs, complies, understands, passively accepts" (1971, pp. 88–89). Such stroking behavior, Bernard argues, is incompatible with high level occupational roles, in which instrumental, aggressive, and often competitive behavior is required.

Bernard then introduces the "cichlid effect," stemming from Lorenz' description (1963) of cichlid fish, a species in which the male's sexual drive is extinguished by fear of his partner, and the female's sexuality is similarly affected by lack of awe for her male partner. Women, she argues, are denuded of their aggression, held in

awe of males by their assignment to the subordinate stroking or supportive role—all in the name of sexuality, male and female alike. Bernard traces the crippling that depriving women of aggression inflicts on their achievement and creative potential and suggests that very high achieving women are the primary victims. Why, she asks, do *all* women have to ransom sexuality by subordination and dependency? Why do women have to sacrifice achievement vis-à-vis men with whom they have no sexual relationships? Stroking, the behavior that addresses the cichlid effect, is not singlehandedly responsible for women's inequality, according to Bernard; however, "taken in conjunction with other aspects of the 'sphere of women' . . . [stroking] is a recipe for subservience" (1971, p. 94).

By the very next year, Bernard had moved beyond the cichlid effect in her explanation of women's subordination and its link to the stroking function. In *The Future of Marriage* (1972), she makes an important distinction between the

physical power of men and the male mystique which identifies it with sexuality . . . If power is measured as the ability to conquer a woman and if masculinity is defined as such power, the subjugation of women is demanded for potency. And, in fact, such a definition of masculinity has characterized the male mystique for centuries. From time immemorial, therefore, sex has meant power to men . . . The very term *impotence* betrays the power component in the male mystique. (pp. 168–69)

Feminist paradigm shift. Replacing the cichlid effect concept with the equation between masculine power and subjugation of women was an insight born of her breakthrough to a feminist paradigm. As she recounts in the autobiographical note in *The Future of Marriage* (1972, afterword, pp. 329–330), during the gestation of that work, she had her consciousness painfully and wrenchingly raised to the feminist perspective. In the late 1960s, a growing feminist consciousness had begun to infuse the scholarship of radical women, the same radical scholars and activists who had taken her to task for failing to take that important next step in *Academic Women*.

Just as Bernard had moved agonizingly, but irrevocably, from the social positivism of her first two decades of scholarship, now she was able to make an analogous quantum leap from a functionalist to a feminist perspective. After this new paradigmatic shift, she was able to look

back at *Academic Women*, describing it as a study "conceived, researched, and written in the old-fashioned scientific and scholarly tradition, quite well-mannered and subdued" ([1964] 1974, p. xxvii).

Surveying sociological phenomena through a feminist prism cast a different, more vivid spectrum of colors than Bernard had seen before. The cool "objectivity" of social science could now be perceived more objectively as a distortion, a rationalization for remaining emotionally uninvolved in the fundamental inequities and moral dilemmas of social life. Value judgments implicit in the selection of research topics suddenly became obvious, and social scientists' responsibility for helping to redress the gaping social disparities could no longer be denied—a new variation on an earlier social positivism, an integration of previous positions. Social science now could be diagnosed as part of the problem, the same social science that, in its scholarly cool, dispassionate stance simply studied social problems and implicitly accused, rather than helped, the victims.

Social science, long the bastion of Establishment male dominance, had possessed only the male lens through which to study society. Male and female researchers alike had to view society, record its ills, but remain aloof, through that single distorting lens. Missing, until the feminist perspective developed, was the female lens, the second lens essential to complete the social stereoscope through which the images of the two worlds could be combined to produce the realistic depth, compassion, and involvement of reality.

Since that important paradigmatic shift, Bernard has studied the female world through the feminist prism, insisting upon showing her audience, reasonably but firmly, previously unseen structures. Deliberately casting only occasional glances toward the male world, Bernard reports the new feminist historical discoveries, speaking boldly for the emerging options that will reduce the loss of women to meaningless relationships and empty, literally maddening lives. *The Female World* is an incisive examination of the worlds in which women have lived separated from men—their own world of family, friendships, education, occupations, childbirth and illness, politics, art, and death. Bernard concludes the ambience of the female world is sadness; however, she makes no apologia for women or for the sex differences whose irreduc-

bility she has insisted upon both before and after her feminist conversion.

The Female World is a serious, documentary celebration of women's unique strengths and differences, on which Bernard rests the chances for the future salvation of both men and women in a postindustrial society. Sex role transcendence and shared roles may be the wave of the future as Bernard predicted (1972; 1974; 1975; Lipman-Blumen & Bernard 1979), but now the entire panoply of the female world with its complicated structure and ethos, past and present, is dealt with in still well-mannered, but this time unsubdued, tones.

An eclectic perspective ahead of her time. Earlier in this essay, Bernard's propensity to move ahead of her time in addressing the key issues that were to concern scholars and policy makers was noted. Her research was groundbreaking, not simply in terms of issues per se, but also with respect to the strategies by which she attacked her subject. Here, again, we see her most salient intellectual characteristic—critical defiance of conventional beliefs and myths.

A case in point is *Marriage and Family Among Negroes* in which she laid bare the fallacies of the "black matriarchy" and debunked the notion that black family structure was typically "broken." By careful, scholarly use of census data, she demonstrated that the modal black family structure consisted of a marital union between original spouses living with and caring for their own offspring. She traces the profound influence of the history of slavery and racism on the relationship between black women and men. Although she perceives the black female as an unwitting participant in the travail of black men, she more properly attributes the condition confronting blacks to urbanization, with its crowding, mobility, and discontinuities, as well as to unemployment and poverty. Bernard's empathetic analysis of black society within a white environment (echoing her earlier writing about immigrant Jews)—albeit with some interpretations she would now reject—led a number of her black readers to conclude that Bernard, herself, was black. She hails the importance and predicts the rise of black racial pride and suggests that the self-emancipation of blacks is the critical treatment for the black male's wounds.

Another major myth that Bernard explodes is the fiction that marriage and motherhood are desirable for women. In *The Future of Marriage*

and *The Future of Motherhood* (1974), Bernard, with Durkheimian precision, carefully demonstrates that marriage benefits men more than women and that the Western, postindustrial structure of marriage, as well as motherhood, is deleterious to women's mental health.

Analogous to her foresight in identifying timely substantive issues is Bernard's anticipation of methodological strategies ahead of the trends. As early as 1966, she advocated investigations of the differential experiences of groups born or socialized at different points in history, a method to be raised to recent prominence under the label of "cohort analysis." Her early call for longitudinal studies to offset the distortions of cross-cultural research was ahead of the vogue. Earlier still, in 1964, Bernard was using cross-national data to make key comparisons among academic women in the Soviet Union, France, Italy, Britain, and the United States—again, a methodological approach which has since gained an impressive following.

Bernard's scholarship is eclectic, spreading over a wide range of substantive areas and using a complicated palette of methodologies. Stemming from her training in measurement, Bernard's earliest work, such as *American Family Behavior* (1942a), through *The Future of Marriage* utilizes macrodata, including macrostatistics from census and other government reports, to compare the patterns of various sociological groups and to gauge the strength and trends of sociological phenomena. Later, she turned to microdata in the form of personal documents, the letters she and her children exchanged over more than 25 years, in a courageously self-revealing effort to chronicle the intimate infrastructure of family life. In her work on black marriage and family structure, as well as her most recent research on the female world, Bernard focuses on blacks and women, respectively, deliberately giving scant attention to comparisons with whites and men. Thus, she intentionally eschews measurements of blacks compared to whites, women compared to men. In defiance of traditional social science canons, Bernard makes a conscious attempt to see blacks as blacks (not as deviants from white patterns) and women as women (not as would-be males). In *The Sex Game*, Bernard moves easily to an interactionist (although neither a Meadian nor a Goffman disciple) perspective. And her shift from social positivism to non-Parsonian functionalism to feminism ultimately seems a hu-

manistic integration of the strengths of each paradigm.

Awards and honors. Recognition of Jessie Bernard's work as a major contribution to the discipline, as well as to the education of a general audience, has come from every quarter. Ironically, her ability to speak lucidly and incisively to a larger public, as well as to her social science colleagues, probably delayed her inevitable professional recognition. (She once facetiously warned a colleague whose work had been cited in the popular press to "shield your co-workers from this; how good can a sociologist be if she is worth quoting in a popular magazine?" [personal communication, 1978]).

Amiably defying professional customs and mores, Bernard declined nomination to the presidency of the American Sociological Association, as well as to a host of other organizational positions. Nonetheless, in recent years, she has been the recipient of the Kurt Lewin award from the Society for the Psychological Study of Social Issues (1976); the first emeriti award presented by Pennsylvania State University (1976); the outstanding achievement award from the American Association of University Women (1976); honorary doctorates from Hood College, Northwestern University, Washington University, and Radcliffe College; the Stuart A. Rice merit award from the District of Columbia Sociological Association (1974), as well as the merit award of the Eastern Sociological Society and the Burgess award from the National Council of Family Relations (1973).

Several awards have been established in Bernard's name, awards designed to honor those, who, like Jessie Bernard herself, have contributed intellectually, professionally, and humanely to the world of scholarship and feminism. Thus, the Jessie Bernard awards, annual prizes established by the American Sociological Association in 1976 and the District of Columbia Sociologists for Women in Society in 1978, reflect the esteem in which her contributions—intellectual eclecticism, personal courage, reasonable defiance, unpretentious humanity, and feminist sisterhood—are held.

JEAN LIPMAN-BLUMEN

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BERTALANFFY, LUDWIG VON

Ludwig von Bertalanffy was born in 1901 in Atzgersdorf near Vienna, and died in 1972 in Buffalo, New York. His interests developed early and were always wide ranging. They encompassed experimental as well as theoretical biology, philosophy of science and of man, psychology and psychiatry, theory of symbolism, history, a broad variety of social problems, and even a topic as arcane as the origin of the postal service in the Middle Ages. In most of these fields he was a true pioneer, with ideas running ahead of the dominant views of his times. In many respects he suffered a fate not unusual for pioneers: the full recognition of the validity and significance of his theories late in life or posthumously, and the subsequent reemergence of his original notions in different fields and forms, sometimes without reference to his earlier statements.

Bertalanffy received his PH.D. at the University of Vienna in 1926, but his autodidactic studies ranged far beyond the formal curriculum. While still in his teens he studied Jean B. Lamarck, Charles Darwin, Ernst Haeckel, Karl Marx, and Henry T. Buckle; while in his twenties he also felt the influence of such early biological theorists as Wilhelm Roux, Hans Driesch, and Julius Schaxel. The “as if” philosophy of Hans Vaihinger, whom he knew as an elderly man, also influenced his intellectual development. Bertalanffy was named *Privatdozent* at the University of Vienna in 1934 under the aegis of philosophers Robert Reininger and Moritz Schlick. The latter was a founder of the famed Vienna Circle of positivism, a mode of thought that did not attract the young biologist, who despite his experimental work, believed in bold theorizing, had profound literary interests, and leaned toward mysticism. As professor at the University of Vienna, Bertalanffy belonged to the philosophical and medical faculties and lectured at the Zoological Institute. In 1937, he was awarded a Rockefeller Foundation fellowship to study United States developments in biology with regard to their applicability to Austrian universities. He traveled extensively in the United States and held appointments at the University of Chicago and the Marine Biological Laboratory at Woods Hole. In October 1938 he returned to Austria, where the outbreak of World War II the next year cut off further contact with America. His house, library, and laboratory were destroyed in 1945, during the siege

of Vienna, and a number of almost completed manuscripts were lost. He left Austria definitively in 1948 and, after travels in western Europe, assumed a position as professor and director of research at the newly founded medical faculty of the University of Ottawa. During the year 1954/1955 he was a fellow at the Stanford Center for Advanced Study in the Behavioral Sciences. While there, at a meeting of the American Association for the Advancement of Science in San Francisco in December 1954, he established the Society for the Advancement of General System Theory (now the Society for General Systems Research), together with Anatol Rapoport, Kenneth E. Boulding, and Ralph W. Gerard. From 1955 to 1958 Bertalanffy was director of biological research at Mount Sinai Hospital in Los Angeles and visiting professor at the University of Southern California. His subsequent appointments included a term as Sloan visiting professor and member of the research department at the Menninger Foundation in Topeka, Kansas, an eight-year tenure as professor of theoretical biology at the University of Alberta in Edmonton (1961–1969), and finally an appointment as faculty professor at the Center for Theoretical Biology of the State University of New York at Buffalo (1969 until his death in 1972).

Throughout an adventurous lifetime, Bertalanffy combined experimental work in biology with the broadest range of theoretical interests. His biological work centered initially on a quantitative determination of metabolism; the model derived from his laboratory experiments in Vienna and Ottawa provided the foundation for the widely used Bertalanffy growth equations. He later focused on cytochemistry, developing in this field the acridine-orange technique of determining DNA (deoxyribonucleic acid) and RNA (ribonucleic acid) by fluorescence microscopy, important in the early diagnosis of cancer.

Bertalanffy's enduring interests, however, were mainly theoretical. He pioneered the "organismic conception" of biology on the Continent, publishing papers on this topic as early as 1925–1926, and a major book, *Kritische Theorie der Formbildung* (1928a), summarized his ideas. This conception was to transcend the dichotomies of a mechanistic *vs.* a vitalistic explanation of life through consideration of the organism as an open system endowed with specific properties capable of scientific investigation. Together with the related concepts of levels

of organization and of the "active" as opposed to the "passive" (or reactive) organism, it constituted an early statement of a "holistic" theory of life and nature. Bertalanffy's organismic conception appeared a few years after Alfred North Whitehead published his first essays on the philosophy of organism (Whitehead 1920). However, Bertalanffy was not aware of Whitehead's writings, which then were little known on the Continent. Moreover, unlike Whitehead, he attempted to provide usable hypotheses for working scientists rather than a general philosophy. He elaborated the original organismic conception in *Theoretische Biologie* (1932–1942), *Das Gefüge des Lebens* (1937), and *Das biologische Weltbild* (1949a).

The organismic conception found a number of distinguished adherents, including Joseph H. Woodger, whose *Biological Principles* appeared in 1929, and Joseph Needham, whose conversion to the organismic concept is documented in *Chemical Embryology* (1931), *Order and Life* (1936), and *Integrative Levels* (1937). However, the conception encountered general resistance among experimental biologists, who sought to explain the processes of life by investigating physical and chemical laws on sub-cellular levels. (For example, the then authoritative textbook by Max Hartmann, *Allgemeine Biologie* [1927], contained hardly any reference to processes above the level of the cell.) The issue reemerged in the 1960s in the debates over whether life was ultimately explainable in terms of the properties of DNA and the laws of biophysics and biochemistry. Although Bertalanffy took an active part in these debates on "reductionism" (cf. Koestler and Smythies 1969), his original organismic conception was largely neglected in the literature.

The organismic conception of life, elaborated by Bertalanffy into a general theory of biology, later became the foundation for the general theory of systems. The development was logical. The organismic conception referred to the organism as an "organized system" and defined the fundamental task of biology as discovery of the laws of biological systems at all levels of organization. The task itself was taken up by Bertalanffy who, interested in the broadest implications of his conception, went beyond biology to consider psychological, social, and historical levels of organization. He conceived of a general theory capable of elaborating principles and models that were applicable to all systems, whatever the nature of their parts and the level

of their organization. He outlined the framework for such a theory in a seminar of Charles Morris at the University of Chicago in 1937 and later in lectures in Vienna. Unfortunately, the publication of the manuscript in which this "general system theory" was first described was prevented by the general upheaval at the end of World War II. Bertalanffy first published a paper on it, entitled "Zu einer allgemeinen Systemlehre," in 1949. It was followed in the next year by "The Theory of Open Systems in Physics and Biology" and "An Outline of General System Theory." The oft reprinted classical formulation of the principles, scope, and aims of the theory was given in 1955 in "General System Theory" and developed in greater detail in 1969 in the book with that title. Bertalanffy used these principles to explore a host of scientific and philosophical issues, including a humanistic conception of human nature that opposed the mechanistic "robot" conception of behaviorists and cyberneticists as failing to do justice to the spontaneous self-motivated activity of mind and body; a philosophy of mind and culture that distinguished human from animal nature in terms of the ability to use symbols; and the seeds of a new philosophy of history that revived valid components of Oswald Spengler's and Arnold J. Toynbee's theories in a perspective that saw civilizations themselves as systems.

The concept of system and the possibility of constructing system theories to explain human behavior and social progress became a major interest in the social and policy sciences almost four decades after Bertalanffy first outlined his organismic conception, and years after he began to explore the applicability of a general system theory to diverse psychological and social phenomena. Interest in systems theory was spurred by several factors, including recognition of the need for, and the legitimacy of, general theories in natural and social science, and the coming of age of modern automated technology with its concepts of cybernetic control and multichannel quasi-instantaneous communication. Norbert Wiener's classic work on cybernetics (1948), aptly subtitled *Control and Communication in the Animal and the Machine*, drew attention to the similarity between self-guided control processes in biology and technology and simultaneously encouraged expectation that through modern technology some living processes might be simulated and better understood. The study of systems became acceptable, even fashionable, among social and policy scientists. A crop of new

specialities emerged—systems analysis; cybernetics; information, decision, and game theory; communication and organization theory; functionalism; and "systems approaches" to a variety of problems ranging from urban planning, health delivery systems, and environmental pollution control, to international and world affairs. However, in the 1960s most systems theorizing drew its inspiration from relatively simple mechanistic cybernetic conceptions derived more from engineering than from biology (for a critique, see Hoos 1972). Only a handful of "general system theorists" rallied around the ideas originally proposed by Bertalanffy and elaborated and paralleled by Kenneth Boulding in economics, Paul Weiss and Ralph Gerald in biology, Anatol Rapoport in mathematics, and Karl Deutsch and David Easton in political science. These few adherents emphasized the basic concepts of irreducibility or "wholeness" of systems; their openness to energy, matter, and information; their "activeness" or spontaneity in coping with environmental changes and disturbances; and their evolutionary transformability and hierarchic organization.

In the 1970s several further developments lent support to the organismic conception of systems and credence to the eventual formulation of a fully worked out general theory. They included the Nobel prize-winning mathematical definition of Ilya Prigogine and his coworkers of nonequilibrium systems that offset their entropy production with entropy transport as they emerge through processes of alternating fluctuations and stability; the evolutionary perspective provided by Jacob Bronowski in describing the progressive buildup of complex systems (1970); Ervin Laszlo's attempts to provide a philosophical foundation for a general system theory of nature, man, and society (1972a); and a host of occasionally brilliant works in mathematics, humanistic psychology, psychiatry, management science, and policy and "futures" study.

Bertalanffy's original vision of an organismic conception in biological theory became a fountainhead for a nonreductionist and nonmechanistic systems approach, not only to phenomena of life but to problems of mind, evolution, society, and history. If general system theory is reproached for being vague and qualitative, and for failing to live up to expectations, it must be remembered that a fully quantified and experimentally confirmed general theory of systems would constitute nothing short of a master theory of evolution and of all that evolution pro-

duced in the way of life, mind, and society. The general system theory proposed by Bertalanffy is at present a "program" or "philosophy" (as emphasized by Rapoport, Boulding, and Laszlo, among others) of great vision and scope, with ample promise of bearing fruit in the biological as well as in the social sciences. This promise is demonstrated in the wide-ranging essays in *The Relevance of General Systems Theory*, papers presented to von Bertalanffy on his 70th birthday (Laszlo 1972b) and in *Unity Through Diversity* (Gray and Rizzo 1973), the two-volume *Festschrift* prepared in his honor by some 63 scholars and scientists from 12 countries.

ERVIN LASZLO

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BETTELHEIM, BRUNO

On the basis of a lifetime of clinical practice and academic research, Bruno Bettelheim emerged as one of the central figures in the reformulation and application of psychoanalytic thought both in the area of psychotherapy and in the analysis of mass society. The boldness of his formulations and the immense range of his interests make him an intellectual of immense influence. At times, he has been subject to intense debate, but with the passage of time his contributions have come to be accepted ingredients of clinical practice and of social science analysis. He was a university professor who believed in the unity of theory and practice, and who was deeply concerned with the impact of ideas on contemporary society and with the social responsibilities of the social scientist. One of his outstanding accomplishments was his

extraordinary ability to fuse together complex analytic discourse with clear and literate prose, which made his writings widely accessible.

Bruno Bettelheim in one lifetime followed three full careers, in each of which he achieved an international reputation of the first rank. As a psychoanalytic psychologist, he prepared a body of theoretical and empirical research literature which fundamentally fashioned group and milieu psychotherapy. As an educator-administrator, he was director of the famed Orthogenic School of the University of Chicago for emotionally disturbed children and teacher of generations of teachers and psychotherapists. In addition, he had widespread impact on lay audiences through his lectures and mass media appearances. As a social scientist and humanistic critic, he authored classic works on collective behavior in modern society, especially his analysis of the psychopathology of the Nazi concentration camp system. These were roles that he pursued concurrently through an extraordinarily active and productive career.

He was born in Vienna, Austria, on August 28, 1903. His parents were upper middle-class Jews who came from Eastern Europe and who were rapidly assimilated into the urban culture of Vienna. He was educated at the Reform Realgymnasium which was relatively "modern" in its curriculum and atmosphere, but which offered Bettelheim a classical continental education. After matriculating at the University of Vienna, he received his PH.D. in psychology and philosophy in 1938. In the course of his studies he spent considerable time in the humanities, especially in art history, but he became increasingly concerned with the study of psychoanalysis and the practice of psychotherapy. His work led him to an interest in emotionally disturbed youngsters, and foreshadowed his lifetime commitment to the development of residential treatment. He was influenced by the accomplishments and writings of Anna Freud, August Aichhorn, and others who were transforming the procedures of classical psychoanalysis.

Bettelheim's education included not only his formal academic training, but his exposure to the intellectual ferment of Vienna during the interwar years. Although there was a profound respect for the classics, a self-critical, inquiring, and innovative attitude was dominant. The remarkable intellectual figures who were present in Vienna during that era fashioned Bettelheim's interdisciplinary outlook and generated his con-

cern with linking dynamic psychology to social processes and social institutions.

For the time being, the increasing political tensions hardly demoralized the scholarly community. But the rise to power of the Nazis in Austria ended with one blow the Viennese scene, with deep trauma for all those involved. In 1938, after the Germans annexed Austria, Bettelheim was transported to Germany and spent one year in concentration camps—both in Dachau and Buchenwald. After his release in 1939, he was able to make his way to the United States and to start to rebuild a professional career. While the bulk of this wave of European intellectual refugees settled in New York and on the eastern seaboard, he found himself relocated in Chicago and the Midwest, a set of circumstances which deepened his integration into United States society and culture.

From 1940 to 1942 he was employed as a research associate at the University of Chicago on a project dealing with the psychology of art and with art education. He was an associate professor of psychology at Rockford College, Illinois from 1942 to 1944. During this period his health, which had deteriorated during his internment, gradually improved. In 1944, he was appointed to the faculty of the University of Chicago and soon became director of the Orthogenic School. He remained uninterruptedly at the University of Chicago throughout his academic career until he retired in 1973. Although he traveled widely throughout the United States and abroad, and lectured at numerous universities and therapeutic centers, the management of the Orthogenic School was a continuous and consuming task that dominated his work schedule. The diversity of his academic interests is indicated by the fact that he rose to the position of distinguished service professor of education, psychology, and psychiatry, in addition to his directorship of the Orthogenic School.

The core of his scholarly writings derived from his work with emotionally disturbed children and resulted in the publication of a massive series of theoretical and technical papers which were accompanied by a series of widely read books (1950; 1955; 1960; 1967). These books presented not only his general ideas and strategies, but also were rich in clinical detail. These studies were particularly noteworthy because they dealt not only with the young children and adolescents at the Orthogenic School, but encompassed the parents, staff, and social environ-

ment—that is the total lifespace of the school. Bettelheim opened his school to researchers who examined its institutional character in depth. This body of writing was an integral contribution to the shift in emphasis from classical two-person treatment systems to the development of supportive group and milieu therapy. This approach came to have a profound impact on the management of mental institutions and in turn on educational and social work practice.

Bettelheim sought to reach out and treat individuals who were deeply disturbed and who were excluded from conventional psychoanalytic treatment. He believed that such treatment was a worthy objective per se, and that it would also supply the basis for modifying family and educational practice for a free and democratic society. While he believed in the significance of psychoanalytic therapy, his long-term goal was to make dynamic psychology relevant for the educational process of modern mass society.

The focus of his therapeutic procedures centered on residential treatment. He believed that the class of patients with whom he and his staff were working came from such disorganized families and had suffered such traumas that they required a new lifespace if they were to be returned to society. In addition, he believed that treatment could not be limited to brief and periodic intervention. Instead he relied on ongoing contact between students and staff. His students had to be exposed to a staff that could engage extensively in therapeutic intervention as required by the human circumstance. In its outward manifestations, the Orthogenic School made use of a minimum amount of coercive restraint and a maximum reliance on interpersonal support. The logic of the treatment rested on the pervasive commitment of the staff to meeting the needs of the students as human beings, needs that were expressed in distorted form because of their previous traumas.

While many European refugees who specialized in psychoanalysis remained wedded to the analytical perspectives which they imported into the United States, Bettelheim was one of those few who displayed a strong sense of self-criticism and a desire to enrich their frame of reference. In the American scene, psychoanalysis was placing a greater importance on the centrality of ego functions and on ego psychology rather than continuing its traditional emphasis on the analysis and reconstruction of "instinctual" drives.

Bettelheim's theoretical writings and his strategy of residential treatment was an essential ingredient in this trend. In addition, Bettelheim himself acknowledged the importance of his exposure to American pragmatic philosophy and psychology for his work. Although pragmatism did not admit of the "unconscious," and thus of unconscious motivation, the parallel formulations of pragmatism and psychoanalysis were striking. The incorporation of pragmatic psychology into psychoanalysis by writers such as Bettelheim served to enrich and explicate dynamic theories of personality. Of critical importance was the formulation of human personality in interactional terms, rather than in conformity with stimulus response theory, and the emphasis placed on the continuity of the emotive and the rational.

The impact of the Nazi seizure of power, the events of World War II, and his enforced refugee status were traumatic events which penetrated his daily consciousness and existence. But Bettelheim responded to the opportunity for creative experimentation that the United States setting afforded his psychotherapeutic institution after 1945. Moreover, in his new found intellectual colleagues at the University of Chicago and throughout the United States, he also found powerful sources of intellectual stimulation and support for his core interests.

Bettelheim's application of psychoanalytical theory to the study of collective behavior was motivated by the same set of concerns that were at work in his approach to psychotherapy: namely, he was preoccupied with the defense of the individual against the destructive aspects of contemporary institutions; emotive energy and personal commitment were required.

After his arrival in the United States, he achieved widespread recognition with the publication of his famous paper, "Individual and Mass Behavior in Extreme Situations" (1943). The originality of his effort to examine the concentration camp in psychological and personality terms was accepted as a virtuoso performance. He traced the impact of the German concentration camp on personality and as a means for producing changes in prisoners which make them "more useful subjects" of the Nazi state. The ability of the Nazi officials to manage the internal administration of the concentration camp attracted his attention. The day-to-day administration of the concentration camp rested not merely on force and coercion. Bettelheim

focused on the process by which inmates under the brutal treatment of their captors came to identify with the camp authorities and to serve in time actively in support of their captors, the so-called identification with the aggressor syndrome. He underscored that realistic perception of the camp guards and camp officials assisted him to survive. As an inmate, his efforts to understand intellectually how the camp functioned as a social system contributed to his ability to endure its repressive impact. In his view, survival in the concentration camp, to the extent that it was possible, required the person to mobilize all of the skills and resources he had before entering the camp and to strain to approach the immediate reality in a realistic and unstereotyped fashion.

After this publication about the concentration camp, there ensued a series of major studies dealing with the relation between personality and social institutions in concrete settings. Bettelheim never presented his theories of human behavior without clinical and empirical illustration. In 1950, with Morris Janowitz, he published *Dynamics of Prejudice*, which dealt with the impact of World War II experiences on young veterans. In contradistinction to the then existing theories of authoritarian personality, the role of personal and social controls in containing hostility and prejudice was formulated. It was most difficult to identify a personality structure linked to prejudice; the ability of social scientists to describe personality structure on a group basis or in statistical terms was indeed limited. Instead the level of expressed prejudice was a function of both social and psychological factors; that is of a person's education, social position, and pattern of social mobility in interaction with his personal controls, in particular his ego strength. Out of these studies came a recognition of the markedly low level of anti-Semitism in the United States after 1945 and bold explanations of its decline.

Bettelheim was one of the earliest critics of the male orientation of psychological analysis as it was practiced during that period. Male viewpoints and the male outlook dominated psychological theory, including the central elements of psychoanalysis. Bettelheim's objective was not to develop a female psychology, but rather to make personality theories more accurate and more objective. In *Symbolic Wounds: Puberty Rites and the Envious Male* (1954), he used anthropological materials to reformulate the psychology of the "rite de passage" in order to pre-

sent a balanced analysis of male and female personality development.

The study of the concentration camp system supplied the basis for his subsequent book *The Informed Heart* (1960), in which he elaborated his argument about the limits of rationality in the defense of individuality. Bettelheim's philosophic position and his argument, based on empirical investigation, in no sense represents a rejection of rationality or rationalistic objectives. He fully avoided those intellectual trends which he thought were "scapegoating" science and the scientific method. Instead he was concerned with self-defeating limitations of a purely rationalistic psychology. He did not believe that the modern world suffered because it rejected and undermined the irrational and superstitious, but because the social institutions in mass society failed to take into account and satisfy unconscious motivation in a constructive fashion. The weakening of strong personal commitment, based on emotional energy, has had disruptive consequences. *The Informed Heart* continued the classic argument of Freud's "Civilization and Its Discontents" (1930). But because of his liberal social philosophy, Bettelheim left open the possibility of, and in fact argued for, a personal and individual solution or at least management of the dilemmas of mass society.

His continuing interest in the construction of new and alternative social institutions on behalf of the individual led him to study communal childrearing in Israeli kibbutzim, *Children of the Dream* (1969). He presented an interpretation that was highly controversial, because he sought to assess both the "costs" and "benefits"—the advantages and disadvantages—of collective child raising and personality development. He emphasized that the kibbutzim produced a new generation committed to group responsibility—values of high importance in a pioneer nation, especially in fulfilling the needs of military defense. But the result was to inhibit individuality, creativity, and individual adaptability to changing social circumstances.

His search for the psychologically significant led him to undertake an elaborate analysis of the changing character of fairy tales (1976). It represented the epitome of his elegant literary style, which in part accounted for its acclaim in humanistic circles. Bettelheim concluded that fairy tales and nursery stories had a positive function in assisting the child to master the realities of the outer world. The child was exposed to a threatening fantasy but the fact that

the parent or familiar household figure would read the story served to reassure the child. Moreover, the very fact that the fantasy was repeatedly read to the child also worked to communicate that the dangers of the external world could be mastered.

Throughout his university career he never abandoned his interest in making his theories and himself available to classroom teachers and to parents of normal children. He conducted extensive group discussions with mothers and parents which led to the publication of *Dialogues With Mothers* (1962). He wrote for and appeared in the mass media as part of his commitment to effective modern education. He was a popular teacher who had a reputation as one who made effective use of the Socratic method in order to stimulate independent thinking among his students.

He emphasized the fundamental debt to Freud's dynamic psychology to which he nevertheless had a critical stance. At the same time he acknowledged the impact on his work in the United States of the thinking of John Dewey, and remained proud that he was a member of the department of education that Dewey had helped to establish. Bettelheim's early youth gave him the opportunity to observe the ideological movements of the left in Vienna; he never accepted their political format, but thought of himself after his transplantation to the United States as committed to the liberal tradition. His reputation as a controversial figure rested not only on the boldness and originality of his writings and formulations, but on his personal opposition to totalitarianism—political and intellectual—of the left and of the right.

MORRIS JANOWITZ

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BICKEL, ALEXANDER M.

Alexander Mordecai Bickel (1924-1974), Sterling professor of law at Yale University, was at the time of his death one of the nation's leading constitutional authorities. In addition, he was active as a teacher, scholar, lawyer, journalist, and adviser to government officials. The range of his interests, as well as the penetration of his mind, prompted the columnist George F. Will to write of him posthumously as the "keenest public philosopher of our time."

Born in Bucharest, Romania, Bickel immigrated with his family at the age of 14 to the United States, where his father became a prominent Yiddish journalist in New York City. Bickel, though he spoke no English upon his arrival, rapidly acquired an ease and fluency in the language that distinguished his speaking and writing all of his life. He graduated from the City College of New York in 1947 and from Harvard Law School in 1949. He served as a clerk to Calvert Magruder, chief judge of the United States Court of Appeals for the First Circuit, was a law officer in the Office of the U.S. High Commissioner in Germany, and returned to serve as law clerk to Felix Frankfurter, then associate justice of the United States Supreme Court. Bickel's relationship with Frankfurter was particularly close, and the older man became a major intellectual influence upon him. In 1956 Bickel was appointed to the Yale law faculty, where he remained until his death 18 years later. During the brief span of his academic career, he published seven books, and two more appeared posthumously. Few academic lawyers publish as much in an entire career.

As a scholar, Bickel set for himself, as serious students of the American Constitution must, the central question of whether and how the

practice of judicial review—the power of the federal judiciary to set aside as incongruent with the Constitution the laws and actions of popularly elected officials—can be reconciled with the theory and practice of political democracy. An effort to summarize his intellectual legacy involves two kinds of distortion. In the first place, his thought was so rich, his insights in the course of argument so prolific and often profound, that a statement of the major features of his philosophy misses much of his genius. Secondly, his thought was in continual evolution so that positions stated, for example, in his first major work, *The Least Dangerous Branch: The Supreme Court at the Bar of Politics* (1962), were often expanded, modified, or qualified, explicitly or implicitly, in later works, including *The Supreme Court and the Idea of Progress* (1970) and *The Morality of Consent* (1975), as well as in his journalism, teaching, and conversation. These caveats, however, do not mean that his general approach was not consistent from first to last. Perhaps he became, over time, less hopeful of the capacity of judges to manage large political and social affairs, as well as more dubious of the legitimacy of their efforts to do so. He displayed at all times a complex understanding of the ways in which institutions of government and of the private order interact and limit each other's power. His thought owes much of its complexity to his Burkean insistence that most principles, in law as well as politics, be limited, nonabsolute, and depend heavily upon circumstance and context—that we be governed not by ultimates but by what he called “principles in the middle distance,” themselves subject to continual evolution.

Since the basic premise of American government is majoritarian, the power of the federal judiciary, and particularly of the Supreme Court, to thwart the expressed will of the peoples' representatives requires justification. Bickel located that justification in the Court's capacity to perform a function that the political branches of government could be expected to perform only inadequately: the explication of principle, as distinguished from expediency. “Judges have, or should have, the leisure, the training, and the insulation to follow the ways of the scholar in pursuing the ends of government. This is crucial in sorting out the enduring values of a society . . .” (1962, p. 25). Yet the Court was not to impose principle everywhere:

No society, certainly not a large and heterogeneous one, can fail in time to explode if it is deprived of

the arts of compromise, if it knows no ways of muddling through. No good society can be unprincipled; and no viable society can be principle-ridden. . . . The role of principle, when it cannot be the immutable governing rule, is to affect the tendency of the politics of expediency. And it is a potent role. (1962, p. 64)

It was in that continual tension between principle and expediency that he thought the Court and the function of judicial review must live, and the key to its ability to maintain itself was the Court's capacity, through a variety of technical legal doctrines, usually thought of as technicalities, to avoid decisions when the evolving principle had not yet matured sufficiently to be politically acceptable. The Court could, in a variety of ways, avoid legitimating what it thought would one day be unconstitutional, while at the same time avoid stating its immanent principle in such a way as to produce a head-on collision with the body politic. In this process the Court would certainly attempt to lead society and would create constitutional law that could not be discovered by traditional modes of interpretation. Judges were to extract “fundamental presuppositions” not from the document, and not from their own political sympathies, but “from the evolving morality of our tradition” (1962, p. 236). They could anticipate the evolution, and exert their moral support in one possible direction rather than another; but the Court could not go too far ahead of present law. According to Bickel, it “should declare as law only such principles as will—in time, but in the rather immediate foreseeable future—gain general assent” (1962, p. 239).

This role of anticipating future law can be reconciled to the theory of political democracy because the Court is not, in fact, final. As the subtitle to *The Least Dangerous Branch* puts it, *The Supreme Court [must stand] at the Bar of Politics*. Bickel wrote:

The Supreme Court's law could not in our system prevail—not merely in the very long run, but within the decade—if it ran counter to deeply felt popular needs or convictions, or even if it was opposed by a determined and substantial minority and received with indifference by the rest of the country. This, in the end, is how and why judicial review is consistent with the theory and practice of political democracy. This is why the Supreme Court is a court of last resort presumptively only. (1962, p. 258)

Bickel's justification for a Court that departs from the historic meaning of the Constitution

set the terms of a debate that has increased, rather than diminished, between those who defend an "activist" Court and those who would confine the Court to traditional modes of interpreting the document. That debate is still unresolved. Post-Bickelians have not yet established the legitimacy of the introduction of judicial "principle" as opposed to legislative "expediency" into our political processes; nor is it clear that the Court is not, in fact, final in more instances than Bickel thought it is or ought to be. Finally, there remains the problem whether the Court is or can be the philosophical and self-conscious institution that Bickel's theory requires.

Bickel himself was one of the activist Warren Court's severest critics. He found its opinions often intellectually incoherent, a failing that he thought inexcusable even on that Court's own premise that it sought to do social good, largely as perceived from an egalitarian viewpoint. In *The Supreme Court and the Idea of Progress*, he argued that the Court understood the public good very inadequately. Some of its most assured pronouncements seemed irrelevant to the ways in which the society seemed likely to develop. Moreover, though he thought a Court might, perhaps must, free itself to a large degree from the written and historical Constitution, it was properly limited to a slow, evolutionary pace in the modification of doctrine. Moreover, its choice of new values had to be explained and justified. This task the Court had shirked, perhaps because it had reached results that defied reasoned justification. Arguing that the Warren Court was one of the major examples of an attitude toward law and procedures that too closely resembled those that produced Watergate, Bickel wrote:

The assault upon the legal order by moral imperatives wasn't only or perhaps even the most effective assault from the outside. It came as well from within, in the Supreme Court headed for fifteen years by Earl Warren. . . . More than once, and in some of its most important actions, the Warren Court got over doctrinal difficulties or issues of the allocation of competences among various institutions by asking what it viewed as a decisive practical question: If the Court did not take a certain action which was *right* and *good*, would other institutions do so, given political realities? (1975, p. 120)

That, for Bickel, was deeply, profoundly wrong. "It is," he said, "the premise of our legal order that its own complicated arrangements, although subject to evolutionary change, are more im-

portant than any momentary objective" (*ibid.*). Perhaps in the long run Bickel's insistence upon the centrality of process, close reasoning, and intellectual coherence will come to seem more important than his justification of a Court that evolves new constitutional principles. Perhaps, indeed, those two major strands of his thought may come to seem in opposition to one another.

Outside of his central work on the theory and practice of judicial review, Alexander Bickel's interests and work ranged widely. He was given a joint appointment in Yale's history department. At the time of his death he had nearly completed a volume of the Holmes Devise History of the Supreme Court of the United States. For 18 years before his death he wrote continually for the *New Republic*, contributing articles, reviews, and unsigned notes. He wrote also for other magazines, some of his best work appearing in *Commentary*, and for scholarly journals. He testified frequently before congressional committees, drafted legislation, and advised politicians of both parties. He was lead counsel for the *New York Times* in the Pentagon papers case, a landmark first amendment controversy. At the time of his death, shortly before his fiftieth birthday, he had turned from concern with the Court and the Constitution to plan more wide-ranging work in political philosophy.

ROBERT BORK

WORKS BY BICKEL

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- 1975 *The Morality of Consent*. New Haven: Yale Univ. Press.
- 1979 *The Judiciary and Responsible Government, 1910-1921*. Part 1. New York: Macmillan. → Volume 9 of the Oliver Wendell Holmes Devise History of the Supreme Court of the United States.

BOGARDUS, EMORY S.

Emory Stephen Bogardus (1882-1973) could have served as the prototype of the American

sociologist for his era. He was a scientist, a teacher, a philosopher, and an activist. He was committed to his profession, to his students, to his family, and to his university and world communities. Born in Belvedere, Illinois, descended from the earliest Dutch and English settlers in the New World, he completed his M.A. in philosophy and psychology at Northwestern University in 1909.

Bogardus received a Northwestern University Settlement fellowship in 1908, and during his year as director of Boys' Clubs at the settlement, he met Edward A. Ross of the University of Wisconsin, who, through his informal remarks at a dinner, introduced Bogardus to the science of sociology.

Ross had both direct and "sleeper" effects on Bogardus' career in sociology. Bogardus had, and continued to have, a person-centered theory of the causation of human social conditions. During his work at the settlement, he asked: "Can these tireless, restless denizens of the street become interested in practical and useful activities . . . ?" (quoted in 1962, p. 42).

When Ross came to visit the settlement, he explained to Bogardus "that sociology was engaged in studying the underlying causes of social problems, social inequalities, social conflicts, and how these situations were the result of social processes" (1970*b*, p. 120). Bogardus had found the bridge between his humanitarian philosophy and his intellectual quest for valid and reliable information about human behavior.

Inspired by his meeting with Ross and encouraged by him, Bogardus called on Albion W. Small, head of the department of sociology at the University of Chicago. He was offered and accepted a scholarship which, when renewed, provided funds for him to complete his doctorate at the University of Chicago in 1911.

Whether Bogardus foresaw the impact that the University of Chicago would have on him is perhaps irrelevant. The fact is that Small, James Rowland Angell, Robert E. Park, George Herbert Mead, Charles R. Henderson, and W. I. Thomas produced such ferment through their interactions as well as their individual achievements that he could hardly have avoided being tremendously influenced. From Small, he learned sociological theory; from Thomas, the technique and application of the method of gathering life histories by personal interview in order to learn more about the development of attitudes; and from Mead, the sociological aspects of role play-

ing (1962). Park, although never one of Bogardus' teachers, urged him to develop an objective instrument for measuring human reactions in the field of race relations, and this proved to be one of Bogardus' most important contributions to social science (1967, p. 1). Along with Bogardus, this powerful core of scholars at Chicago attracted such students and faculty as Ellsworth Faris, L. L. Bernard, William Fielding Ogburn, Harold D. Lasswell, Charles E. Merriam, Louis Wirth, Herbert Blumer, Samuel A. Stouffer, and Robert Redfield.

Bogardus continued his interest in psychology at Chicago, and his work under the direction of Harvey Carr resulted in the publication of "Experiments on Tactual Sensations of the White Rat" (Henke & Bogardus 1911). Angell and Henderson were members of his dissertation committee and his thesis, *The Relation of Fatigue to Industrial Accidents* (1912), could have been in psychology quite as logically as in sociology. Theory and method were as important in his studies as findings had been to most of his predecessors, and the label of the discipline in which he worked did not seem to present any kind of barrier to him, although after his Chicago days he consistently identified himself as a sociologist. Upon completing his dissertation, Bogardus accepted a position as assistant professor at the University of Southern California, where he remained until his death in 1973.

As a social scientist, Bogardus influenced both the discipline and the profession of sociology. His philosophy that all information is useful if it is placed in a meaningful context paralleled his philosophy that all persons can contribute to the common good in some way if leaders know how to elicit appropriate contributions from them.

In 1923, Bogardus began working on *The New Social Research* (1926*b*), in which he carefully organized a sociological methodology that would procure and process large quantities of personal data. This approach reflected a combination of the methods of Thomas and Carr, the motivations of Ross and Park, and the emphasis on meaning of Mead. In 1924, he began work on the operational definition of social and sociological concepts through the use of unidimensional scales.

In 1926 Bogardus administered a social distance scale to a nationwide sample of respondents. The original purpose of the scale was to

assign a value to the degree of sympathetic understanding between members of different races, but Bogardus and many other behavioral scientists quickly found it to be useful for measuring the social distance between persons and any stereotyped category of human beings—ethnic, religious, occupational, to name a few. The Social Distance Scale became so widely used and attracted so much interest that Bogardus repeated his surveys in 1946, 1956, and 1966. It then constituted one of the largest series of sociological studies ever made using the same operational definition of the phenomenon being observed. The same instrument has been used in virtually all quarters of the globe, so that comparisons can be made through both time and space.

Bogardus observed that the mean measured reaction to racial categories by United States respondents had changed slowly over the forty years of his study, and that the range of mean distance scores had decreased more remarkably. From these findings, he concluded that Americans in general had become measurably more accepting of persons of different races. He remarked, however, that his findings did not indicate that racial distances would disappear in the foreseeable future.

Bogardus was the first president of the American Sociological Society to be chosen from a school west of the Rocky Mountains. He founded one of the largest departments of sociology in the world at the time (it became the fifth most productive of doctorates under his leadership); he founded the sociological honor society, Alpha Kappa Delta, and the sociological journal, *Sociology and Social Research*; he was the first president of the Pacific Sociological Society; and he wrote more than three hundred books and articles during his career.

As president of the American Sociological Society, Bogardus actively encouraged liaisons with sociologists throughout the world, greatly increasing the amount of interchange with non-European sociologists in particular. Although he had visited personally many European scholars and was a close friend to several of them, including Leopold von Wiese, Henri Bergson, and Florian Znaniecki, he also visited sociologists in the rest of the world, and his students have taught in many countries in Asia, Africa, Oceania, and South America as well as in Europe and North America. He regularly published "Foreign Sociological Notes" in *Sociology*

and *Social Research*, which he had subtitled *An International Journal*, with the hope that it would encourage a world-wide exchange of sociological information.

Among Bogardus' students, who were greatly influenced by him, were G. Bromley Oxnam, Meyer F. Nimkoff, Pauline V. Young, J. Max Bond, and Edward C. McDonagh.

THOMAS E. LASSWELL

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BOSE, N. K.

Nirmal Kumar Bose (1901–1972) belonged to a generation of Indian intellectuals who combined the pursuit of ideas with active involvement in the process of national transformation. Despite his wide-ranging intellectual interests, he was out of step with the professional social sciences developing in the metropolitan centers of Europe and America, and his published work was not generally written in that Western idiom.

Bose saw himself as a cultural anthropologist and presented his theoretical framework in his *Cultural Anthropology* (1929). He excavated paleolithic sites, studied the material culture of India's large tribal population, and undertook a survey of Calcutta, India's largest city. His writings covered other academic subjects, too, including temple architecture on which he completed a definitive work in 1932. For many years he edited almost singlehandedly India's oldest anthropological journal, *Man in India*, contributed papers to it regularly, and sometimes wrote half its book reviews.

Bose had a lifelong interest in the ideas of Mohandas K. Gandhi. He had already started to edit and interpret Gandhi's writings when he met him in 1934. Later he became more intimate with Gandhi and for a brief time (1946–1947), at a crucial point in Gandhi's life and the life of the nation, was his secretary. It is impossible to understand Bose's views on society and culture without a proper appreciation of Gandhi's influence. Although not a religious man, Bose admired Gandhi for his tolerance, freedom from dogma, and radically experimental attitude toward life.

Bose had a varied career. He studied geology (B.Sc., 1921), then anthropology (M.Sc., 1925), at the University of Calcutta, taught for some time in the department of anthropology, and until 1959, was reader in anthropogeography in the department of geography. He served as director of the anthropological survey of India from 1959 to 1964, and between 1967 and 1970, he held the office of Commissioner for Scheduled Castes and Scheduled Tribes. Concurrent with his academic and professional career was his involvement in the nationalist movement, which resulted in his imprisonment, and his association with the Gandhian movement before and after independence.

Bose saw anthropology as a field science and was himself an indefatigable field worker, partly because of his training, partly because of his long association with a department of geography, and partly because of the example of Gandhi, to whom ideas meant nothing if they were not tested through experience. However, Bose's field work did not follow the tradition of either Franz Boas or Bronislaw Malinowski. Adapted to the requirements of a man concerned with the study of his own society, it was extensive rather than intensive. Bose traveled widely and continually, visiting remote tribal settlements, multicasite villages, pilgrim centers, fairs, and India's many towns and cities. He had something of the restless spirit of the medieval wanderers whom he so greatly admired.

Bose wrote extensively in English and Bengali. His English prose lacked the finish of the professional social scientist, but his Bengali was always clear, direct, and vigorous. Although he recognized that the pursuit of science had its own logic, he believed that the fruits of science should be made widely available, not stored within narrow professional confines. In his youth it was a matter of honor to work to develop indigenous institutions; thus he hardly published

abroad, and his work received little attention in the international community of anthropologists. Even in India, he contributed not only to such professional journals as *Man in India*, *Geographical Review of India*, and *Science and Culture*, but to many popular and semipopular periodicals in English and Bengali.

Bose's views on Indian society and culture are best expressed in his Bengali book *The Structure of Hindu Society* (1949), first published in serial form in the literary magazine *Desh*. In it, he combined the perspectives and methodologies of field ethnography, classical indology, and social history. In the first part of the book, he described the tribal communities of India and their relationship to the wider civilization on the basis of his own field work and that of other ethnographers, notably S. C. Roy. In the second part, he gave an account of the theory and practice of Hindu social life as they were set forth in several major classical texts. The design of Indian civilization as reflected in the classical texts closely matched the one discovered through systematic field work.

According to Bose, the two distinctive features of Indian civilization were pluralism and hierarchy—the equal tolerance of diverse modes of life and their unequal ranking on a widely accepted scale. Bose noted the tension between hierarchy (as represented by the order of *varnas*) and equality (as represented in the succession of *bhakti* movements) in Hindu civilization, as well as the tension between the collectivity (as represented by the *sannyasi*). The last part of *The Structure of Hindu Society*, an exercise in social history, analyzed the way in which the structure of Hindu society, hardly altered by several centuries of Moslem rule, was toppled by the onslaught of British imperialism and capitalism.

Bose had a special interest in India's forty million tribal people, and he wrote extensively on their material culture, religious practices, and social organization. In a seminal paper titled "Hindu Method of Tribal Absorption" (1941), he argued that tribe and civilization had coexisted in India for several millennia, and he showed how tribal communities had transformed themselves into castes by carving out niches for themselves in the larger society. Hindu civilization recognized both *jati* (caste) and *jana* (tribe). The tribes lived in relatively isolated areas; their technology was simple and their division of labor rudimentary. Sometimes, due to pressure of population, they might aban-

don their isolation and establish regular economic relations, say as basketmakers, ropemakers, or agricultural laborers, with Hindus already organized on the basis of caste. They might then abandon their tribal dialect, adopt the language of the Hindus among whom they lived, and in due course emerge as a caste or subcaste of basketmakers, ropemakers, or agricultural laborers. He also published an important paper on Calcutta, showing it to be neither an "industrial" nor a "preindustrial" city, but the product of a unique historical process (1965).

Bose had planned an analytical study of the Gandhian technique of *satyagraha* (which may be very broadly described as "nonviolent persuasion through the force of truth") based on a detailed and systematic examination of a series of cases on which he had collected materials over the years. He died of cancer on October 15, 1972, before he could put this ambitious project into effect.

ANDRÉ BÉTEILLE

WORKS BY BOSE

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BRAUDEL, FERNAND

Fernand Braudel, born in 1902 in a small village of eastern France, wrote of himself in 1972: "I was in the beginning and I remain now a historian of peasant stock" (pp. 448-449). In the period following World War II he

came to incarnate, and represented the culmination of, the so-called *Annales* tradition of historical interpretation.

He did not found this tradition. The tradition is conventionally traced to Henri Berr, who launched the *Revue de synthèse historique* in 1900. Berr argued that it was not fruitful for history to be compartmentalized into a multiplicity of separate fields; rather, historical synthesis was the essential task of historians. This view of the unicity or totality of knowledge of the social world was the first pillar of the *Annales* viewpoint.

In 1903 the economist François Simiand published a celebrated article in Berr's journal in which he attacked the then leading figures in French historical writing for giving no place in their work to repeatable, quantifiable phenomena. He argued that history was about underlying "structures" and not ephemeral, inevitably unique, "events." This view was the second pillar of the *Annales* viewpoint.

When in 1929 Lucien Febvre and Marc Bloch, young professors at the University of Strasbourg, founded the *Annales d'histoire économique et sociale*, they proclaimed by its very title the third pillar of the *Annales* viewpoint, the centrality of economic and social history to the analysis of underlying structures. Politics was virtually to be relegated to the sphere of the event and hence to that which was "dust."

Braudel came to this viewpoint slowly in the interwar years as a history teacher in a lycée in Algiers. His decision to write on the Mediterranean "ripened of its own accord" between 1927 and 1933, and even then he still thought of his study primarily as a history of Philip II of Spain (1972, p. 452). It was Lucien Febvre who suggested to him that while Philip II and the Mediterranean was a fine subject, the Mediterranean and Philip II might be a greater one (Febvre 1950, p. 217). Happily, Braudel took what Febvre himself called this "imprudent advice."

After more than a decade in Algiers and several years in São Paulo, Braudel returned to Paris just before World War II. He spent most of the war as a prisoner in the confines of Lübeck prison, where he wrote, more or less from memory, his work on the Mediterranean, ferreting out chapter after chapter to Febvre. The locus of writing reinforced his attachment to the *Annales* viewpoint, as he explained: "All those occurrences which poured in upon us from the radio and the newspapers of our ene-

mies, or even the news from London which our clandestine receivers gave us—I had to outdistance, reject, deny them. Down with occurrences, especially vexing ones! I had to believe that history, destiny, was written at a much more profound level" (1972, p. 454).

The period following World War II was the moment of opportunity for the *Annales* viewpoint, and Braudel, more than anyone, was responsible for seizing fortuna. In 1945, as in 1929, as in 1900, the *Annales* viewpoint stood in opposition to, in "combat" with, "*l'histoire historicisante*," "*l'histoire événementielle*"—the viewpoint of the French university as expressed by the historians of the Sorbonne and their journal, the *Revue historique*. French Establishment history had emerged out of the assimilation after 1871 of the historical viewpoint of Leopold von Ranke, of the belief in the existence, primacy, and retrievability of the historical fact. Febvre called it "history as written by those defeated in 1870 . . ." ([1953] 1965, p. vii).

Against the narrative, ideographic history that was practiced by the vast majority of French (indeed of world) historians, *Annales* championed "structural" history, inspired by another German tradition, that derived from Gustav Schmoller and the *Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte*, a tradition "much admired" by Febvre and Bloch, and one that rejected the universalizing and segmentalizing premises of British empiricism (Braudel 1972, p. 463). Braudel would himself point out: "Is it by chance that Henri Berr, Lucien Febvre, Marc Bloch, and myself all four came from eastern France? That the *Annales* began at Strasbourg, next door to Germany and to German historical thought?" (*ibid.*, p. 467).

If in 1945 *Annales* still found the university "hostile" to it—Braudel like Febvre found the doors of the Sorbonne closed to him—*Annales* was suddenly taken up by the "youth of the university" (1972, p. 462). All it needed was an organizational channel. It found this channel in the construction, beginning in 1948, of the VI^e Section of the École Pratique des Hautes Études, presided over by Febvre until his death in 1956, but organized from the beginning by Braudel. The field of inquiry of the VI^e Section was defined as "social and economic sciences," but its presidents have always been historians. Though it was "outside" the university, it became, nonetheless, in the following two decades, the center of most of what was creative and innovative in French history and social sci-

ence. When Braudel disclaimed, overmodestly, that he had contributed any new idea to those of Febvre and Bloch, and said that what his generation had accomplished between 1945 and 1968 was simply that "the program became reality," it was to the VI^e Section—its hundreds of professors and visiting scholars, its multiplicity of published volumes, its international role—that he was referring (1976, p. 14). Yet, despite these achievements, "in those years, we remained, in effect, marginal men and heretics, still very far from the Establishment" (*ibid.*).

It is not true, however, that Braudel contributed no new ideas. In fact, he crystallized three underlying themes latent in the *Annales* viewpoint and hence introduced them as explicit guiding concerns of research: the concept of multiple temporalities, and most particularly of the *longue durée*; the view that early modern times (1450–1750) were intellectually a crucial nexus of analysis because they were the crucible of modern capitalism; the concern with the world-economy as a locus of social action and hence as a focus of research per se.

All three themes are exemplified in Braudel's great book *The Mediterranean and the Mediterranean World in the Age of Philip II* (1949). Braudel organized *The Mediterranean* by treating the subject thrice, according to each of three social times: structure (or *longue durée*), *conjuncture*, and event. In his essay "History and the Social Sciences" (1958)—the essential complement of *The Mediterranean*—Braudel spelled out the differences in these temporalities and in the importance, for the analyst, of the consciousness of *multiple* temporalities.

While defending the importance of the analysis of long time against short time, he simultaneously defended long time against very long (or universal) time. Braudel thus brought to the fore what had been implicit in the *Annales* viewpoint from the beginning. The *Annales* viewpoint was conducting a war on two fronts: against the "historicists," to be sure; but equally against the transhistorical generalizers (Braudel cited Claude Lévi-Strauss as an example). If the long organizational battle had meant that *Annales* supporters had emphasized the struggle against the French historical Establishment, and preached the necessity of history's learning from the social sciences, the *Annales* discovered in the years of its maturity under Braudel that, on its other flank, France (and the world) was witnessing a flourishing of an ahistorical variety of "structuralism," which had nothing to do, in-

deed was radically different from, the "structural" history practiced by Braudel.

The combination of the founding of the VI^e Section in 1948 and the publication of *The Mediterranean* in 1949 had a profound impact on both French and world scholarship. Within France, in the two decades thereafter, a large number of "regional" monographs were published, each emphasizing the total perspective, the rural majority, and the collection of serial data, over the *longue durée* of early modern times. Similar monographs were written under *Annales* influence about other countries, especially Spain and Italy. In addition, a second set of monographs and *opii magni* were written about trade, banking, and other manifestations of the operations of the world-economy, whose locus was sometimes the Mediterranean, sometimes the Atlantic, and even the Pacific. The ripples outward of this scholarship transformed in very basic ways the writing both on the Ottoman Empire and on Iberian America.

Annales had an international vocation from the outset, both in terms of its mode of analysis and of the two-way flow of influence. Braudel, however, institutionalized this vocation through the role of the VI^e Section and the Maison des Sciences de l'Homme, which he founded in 1963. The network of their impact spread along traditional lines of French cultural influence: to "Latin" Europe (Italy, Spain, Portugal), Turkey (because of *The Mediterranean*), eastern Europe (particularly Poland and Hungary), Quebec, and, with greater difficulty, Germany, England, and the Low Countries.

The effect was cumulative. In 1969 the creation of the annual *Settimana di Studi* in Prato, Italy, of whose scientific committee Braudel became the president, served as a pan-European intellectual event stamped by the *Annales* spirit. In 1972 and 1973, *The Mediterranean* was translated into English, and after years of neglect in the United States, Braudel began to have the same impact there on the "youth of the university" that the *Annales* had achieved in France in 1945.

Yet at the moment of its greatest success, the *Annales* viewpoint entered an intellectual and organizational crisis. The year 1968 was the turning point. Braudel, in his eulogy of Febvre, had written: "I do not believe in ivory towers, nor in intelligence without passion. Quite the contrary, I believe . . . in the intellectual value of passions" (1953, p. 16). Thus for Braudel it was, as he said, an "irony of fate" that in May

1968 he, the heretic, became the Establishment (1976, p. 16). Perhaps he had been that already, but it had not been official.

Soon thereafter, Braudel renounced the editorship of *Annales*. Within a few years, he stepped down as president of the VI^e Section, which would transform itself into a university, the *École des Hautes Études en Sciences Sociales*.

New winds affected the "new" *Annales*, and observers began to speak of *émiettement* (splintering), some as though it were a good thing, others to deplore it. Perhaps the correct way to appreciate it is to perceive that *Annales* had finally transcended its French national origins. It was no longer merely a French school with an international vocation, but rather a "paradigm" whose fate was being "played out not only in France but on a worldwide scale" (1976, p. 17). If so, this may be counted as the most significant accomplishment of the "historian of peasant stock" of eastern France, who "loved the Mediterranean with passion, no doubt because I am a northerner" (1949, p. 17).

IMMANUEL WALLERSTEIN

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BRINTON, C. CRANE

In a rare moment of self-revelation, Clarence Crane Brinton submitted a short sketch of himself for inclusion in the Twenty-fifth Class Report of his Harvard College class of 1919. Written in the slightly ironic style Brinton often found congenial to his temperament, he said:

. . . at present there are still a few ragged edges on my *Weltanschauung*. The more malicious among you will no doubt understand if I say that, whereas in 1919 I thought of myself as a liberal with at least an initial capital, I now think of myself as a liberal in inverted commas. As a Harvard freshman I was an innocent rationalist and Wilsonian democrat. Even while I was an undergraduate, and with the generous enthusiasm of my tutor, Harold Laski, to fortify me, the influence of the late Irving Babbitt began to undermine the foundations of that belief. In a sense, I have been ever since trying to reconcile the contrary influences of Laski and Babbitt. Towards that reconciliation—which would no doubt be unsatisfactory to both men—I have been greatly helped by my friendship with the late Lawrence Henderson. Briefly, my earlier rationalism has been tempered by an awareness of the

place of prejudices, sentiments, the unconscious and the subconscious, in human life. . . . You may write me down as born in the eighteenth century and yet not too uncomfortable—not at any rate schizophrenic—in the mid-twentieth.

Although this is a remarkably candid statement for Brinton to make, it raises as many questions as it answers. What was the young student and the lifelong teacher of history at Harvard like and what were his own special interests? What kind of influence did Laski, so different in his passionate concern with politics, have on the young Brinton who was only five years his junior? What was the fascination that Irving Babbitt, Harvard's maverick professor of French literature who was an intransigent conservative and rationalist and violently anti-Rousseauist, held for Brinton? What unlikely bridge did Lawrence J. Henderson, the biological chemist who studied industrial fatigue at the Harvard Business School, provide between two such disparate influences?

Clarence Crane Brinton was born in Winsted, Connecticut, on February 2, 1898, to Clarence H. and Eva Josephine Crane. He was soon called by his mother's maiden name, a choice that was not without significance, for she was a strong-willed woman who, after divorcing two husbands, concentrated her emotional attention on her only surviving son. His father was a department store manager. His ancestors on both sides were, however, New England farmers, and Brinton retained an attachment to the land and the temperament of an earthy, skeptical Yankee in spite of his urbane and scholarly life.

After receiving a classical high school education, Brinton entered Harvard. He received a scholarship each year, won the Bowdoin prize in his junior year for an essay on "Lord Acton's Philosophy of history" which he wrote for Irving Babbitt, and graduated magna cum laude in 1919. Fortified with a Shaw traveling fellowship, he then sailed for France to study art for a year before going to Oxford University to take up his Rhodes scholarship. At Oxford he wrote a dissertation on the "Political Ideas of the English Romanticists" and (as one of his students suggests) acquired the Oxonian style of nimbly and wittily avoiding serious subjects. It was a style which fitted Brinton, who has been described by colleagues, students, and friends as shy and exceptionally reticent, often appearing to be "detached, casual, even flippant, some-

times cryptic and baffling" (Ford et al. 1970). At Oxford, Brinton also acquired his interest in intellectual history, a new type of history that he interpreted as the study of the "relations between the ideas of the philosophers, the intellectuals, the thinkers, and the actual way of living of the millions who carry the tasks of civilization" (1950, p. 7).

Of the nature of the influence of Laski and Babbitt, Brinton has left tantalizingly little evidence. That he preferred the younger Laski, who was "rather under the influence of Maitland and Acton than Marx and Lenin," is suggested by Brinton's distrust of "unprincipled idealists" (a phrase he borrowed from A. Lawrence Lowell) and their abstractionist schemes and his referring to Laski as "a very bold spirit at least in his younger days" (Brinton to Carlton J. H. Hayes, March 13, 1936, Harvard University Archives). Brinton dedicated what he called his "brick to the temple of scholarship," *The Jacobins: An Essay in the New History* (1930) to Laski. Often considered his best scholarly writing, the book is a study of the membership records of the Jacobin clubs that uses statistical methods to achieve a dispassionate analysis of the motivations and actions of the Jacobins. The degree of Laski's influence on the book is not discernible, but it shows clear evidence of the impact of his exchanges with Henderson. In his introduction he compared the historian's aspiration to discover uniformities, or laws, of society to that of the biologist; in his conclusion he drew on Vilfredo Pareto (a favorite of Henderson) and his understanding of the influence of men's desires and passions on their ideas and actions.

Although Brinton did not often mention Babbitt in his writings, he shared Babbitt's belief in "New Humanism," an impassioned plea for moderation, balance, and rationalism and an attack on sentimentalism. This is most evident in Brinton's book *Nietzsche* (1941), which William Langer described as a book about Brinton rather than Nietzsche. Brinton himself later recognized its lack of objectivity and came to dislike it as much as he disliked the man. It was Nietzsche's contribution to the cult of irrationality (or "romanticism" as Brinton called it) that aroused his ire. In much the same way he must have disliked Babbitt's cult of rationality that attracted so many followers. Brinton was too much of a social scientist and historian simply to excoriate the passions. He wanted to study, as objectively as possible and without

doctrinal prejudice, the role they played in the lives of men and he wanted no blinders to obscure the rich "diversity of life, the nature of purposes and cross purposes," as he said in his Bowdoin prize essay.

Such were the interests he shared with Henderson, who was impressed by Brinton's Bowdoin prize essay. The older man first discovered Pareto in 1926 and then shared his enthusiasm with Brinton. Henderson also aroused Brinton's interest in Talleyrand and advised him on the details and the conclusion of his book, *The Lives of Talleyrand* (1936b). Brinton, with great wit and irony, portrayed this seemingly wily and opportunistic man as a diplomat who, like Machiavelli, saw the world as it really was and was thus able to achieve remarkable diplomatic successes. He contrasted Talleyrand's realism with Woodrow Wilson's idealistic and bungling diplomacy. Brinton wanted to deliver the much maligned Talleyrand from the distorting pens of those historians who announce "rather loudly that the historian is a scientist" while in fact they are making moral judgments and writing "violently partisan history" (1936b, pp. 15, 18). Brinton's pleasure in his subject and his zeal for the mission led him, as he admitted later, to overshoot his mark and be overly flippant.

Although *The Lives of Talleyrand* is Brinton's most Hendersonian book, the shadow of Henderson's influence is also evident in his two books on revolution, *A Decade of Revolution* (1934) and *The Anatomy of Revolution* (1938). The first is a study of the French Revolution which, lucidly and engagingly, attempts to portray the forces of change that made the revolution and the effects of that event on the lives of ordinary people. The second is an effort to delineate the stages through which every major revolution must move. It initiated a scholarly debate on theories of revolution. In both books he used an analogy drawn from pathology. He compares revolution to a fever. First, symptoms of the coming disturbance appear. Then the disease fully discloses itself, "and we can say the fever of revolution has begun." A crisis follows, often accompanied by delirium (the Reign of Terror), then convalescence and a return to equilibrium ([1934] 1963, p. 1; [1938] 1952, pp. 16-18). It is the kind of physiological metaphor that appealed to Henderson. Although the books use a conceptual framework drawn from the natural sciences, they are primarily historical and are an interchange with

such men as R. M. Johnston, with whom Brinton began his study of the French Revolution, Albert Mathiez, Georges Lefebvre, Brinton's lifelong friend Leo Gershoy, and other historians.

Having seen in the depression, the rise of fascism, World War II, and the postwar world a threat to the "good democratic" life that he held so dear, Brinton's postwar books carry a heightened sense of the need for understanding and knowledge. Many of them are surveys which draw on Brinton's great wealth of knowledge; others are addressed to specific political and cultural problems in the postwar world. Aimed at a larger public than his scholarly studies were, Brinton called them an attempt at "haute vulgarization." They indicate that Brinton always remained a "child of the Enlightenment," as he frequently called himself, in spite of his skepticism. The books were widely read in the United States and Europe.

Brinton shared his interests with a wide variety of scholars and students through his teaching, books, articles, reviews, his voluminous correspondence, speeches, and his membership in many clubs and societies with such engaging names as the Chance, Love, and Logic Society, the Star Chamber, and the venerable Saturday Club. The most important society for scholarly exchange of which he was a member was unquestionably the Society of Fellows at Harvard. Founded largely through the efforts of President A. Lawrence Lowell (who anonymously gave the necessary capital when the Harvard Corporation turned down his request for funds), Alfred North Whitehead, and Henderson, the society was designed to avoid what Lowell called the "mass production of mediocrity," that he felt was characteristic of existing PH.D. programs, by providing support and encouragement for the "rare and independent genius." Brinton, a senior fellow, was the society's chairman from 1942 to 1964. Its members often discussed problems of the methodology of the social sciences and revolutionary theory, and there was a remarkable interplay of ideas among men of very different backgrounds and disciplines (Brinton, disliking controversy, refused to raise the question of admitting women while he was chairman). Thomas Kuhn's *The Structure of Scientific Revolutions* (1962) was one of the many fruits of this intellectual exchange.

Brinton died in 1968 a few months after he retired as professor emeritus at Harvard. He was still planning to write the one book he had

never found time to write—a description of the little known roads and rambles of New England.

ELIZABETH C. ALTMAN

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BROGAN, DENIS

Denis William Brogan (1900–1971) was born in Glasgow, Scotland. His father, who was born in Ireland, had emigrated briefly to the United States, but had returned to Britain to settle in Glasgow as a merchant tailor, and Brogan grew up in a home where interest in Ireland, America, and Liberal politics was strong.

After early education at a parochial Roman Catholic primary school, Brogan proceeded, via Rutherglen Academy, to Glasgow University, intending to study medicine, but soon switching to history when he found that he lacked the practical skills necessary for clinical work. After taking his M.A. in 1923 he proceeded for further historical study to Balliol College, Oxford. His principal tutor was Humphrey Sumner, the Russian historian, subsequently warden of All Souls College, but Brogan's American interests were also powerfully stimulated by contact with Samuel Eliot Morison, then in Oxford as the first holder of the Harmsworth chair of American history. At Balliol Brogan formed two lifelong and influential friendships, with Darsie Gillie, later the *Manchester Guardian's* Paris correspondent, and Norman Robertson, later Canadian high commissioner in London. In 1925 he went to Harvard University as a Laura Spelman Rockefeller fellow for two years of graduate study. But by now Brogan was essentially an autodidact, and although Harvard counted a good deal in his development, firsthand experience of America counted for more. For the rest of his life Brogan was a kind of intellectual oceanographer, indefatigably recording every shifting current of American life, public and private.

Returning to Britain in 1922, Brogan worked briefly on the *Times* (in him the gifts of the journalist were as innate as those of the scholar), before taking up successive appointments in history and politics at University College London, and at the London School of Economics. It was at the L.S.E. that in 1933 he wrote his first book, *The American Political System*. The book represented a total breakaway from the pre-

scriptive and theoretical approach to political science then dominant in Britain, reverting to the earlier tradition of firsthand observation and historical elucidation of James Bryce (also a Glasgow and Balliol man). Brogan's book, in its vividness, objectivity, and relish for the diversity of the American political experience, revealed a new America to British readers, academic and otherwise, who had been accustomed to view the country, when they were conscious of it at all, through the spectacles of the "hands across the seas" tradition or through the lenses of the sensational press. The book stressed the distinctiveness of American politics, its roots in the American past, and the social and economic environment that shaped it.

Hardly had *The American Political System* appeared than the coming of the New Deal transformed much of the political scene that it had described, and in 1954 Brogan revised his exposition in a work of comparable scale, *An Introduction to American Politics*. In one sense the *Introduction* was a reworking of the *System*; it utilized essentially the same categories of treatment, placed a comparable emphasis on the political process, reflected an equivalent belief in the dominance of historical forces and the same lack of interest in the administrative side of government. But in its range of material, its fecundity of illustration, its responsiveness to all the new factors shaping postwar America, the *Introduction* is a new book.

Alongside these major interpretations of American politics there flowed from Brogan's pen, during these years and after, a stream of occasional writing ranging from articles for the learned journals to what were virtually newspaper dispatches. To Brogan, the occasion, the auspices, and the audience made relatively little difference; neither by diminishing his notorious allusiveness nor by ballasting his sprightly prose with pompous periphrases did he seek to demarcate his scholarship from his journalism. It was a rare *pièce d'occasion* of his which did not contain some arresting idea or novel detail, an even rarer learned article which contained nothing to entertain or amuse. A selection of these appeared in *American Themes* (1947) and *American Aspects* (1964); the latter contains the justly celebrated essay, "The Illusion of American Omnipotence."

Brogan's devotion to American history is reflected in no major work. *The American Problem* (1944b) is a wide-ranging series of essays on the historical factors which have shaped

America's place and America's dilemmas in the postwar world. *The Era of Franklin D. Roosevelt* (1950) has the freshness that comes from the author's intimate acquaintance with the period and personalities described, but in scale and treatment it is a minor work. The accidents of Brogan's professional career may provide in part an explanation of this, in that he never held an academic appointment specifically in the American field (there were, in any case, very few such in the Britain of these years). From the L.S.E. he returned in 1934 to Oxford, as a fellow and tutor at Corpus Christi College. There the nature of his teaching duties invited him to venture into another area where, in any case, private circumstances, in the form of his wife's archeological activities, provided a supplementary stimulus. This was the history and politics of France. In fact, *The Development of Modern France, 1870-1939* (1940) is easily Brogan's longest work, more than twice the length of *The American Political System*. As a guide and stimulus to British understanding of her nearest neighbor, the book merits comparison with its American forerunner. For the first time the tangled and seemingly inconsequential events of the Third Republic were reduced to a narrative which, if sometimes too tightly packed, made sense of the story in terms, once again, of the legacy of the national past and the constraints, physical, social, and economic, of the present. The same qualities, on a smaller canvas, mark *The French Nation* (1957) and a volume of essays, *French Personalities and Problems* (1946a), which largely deals with the subsequent France of World War II.

In pre-World War II Britain there was a conspicuous lack of academic vacancies suitable for a man of Brogan's protean capabilities. Consequently, when the chair of political science at Cambridge fell vacant in 1939 on the retirement of Ernest Barker, Brogan accepted the offer of the post even though its subject matter lay at a tangent to his main interests. In fact, owing to the outbreak of war, Brogan did not move to Cambridge until he had completed a characteristically intensive bout of war service (he served in various capacities, but most notably as intelligence officer in the British Broadcasting Corporation's European Service and as adviser to its North American Service, providing each with a sustained and apposite flow of guidance and information derived from his encyclopedic knowledge of France and the United States).

Assuming his professorial duties at Cambridge after the war, Brogan was, for so notoriously allusive an author, unusually explicit and frank in his inaugural lecture: "The study of politics, as seen by me, is first of all and, perhaps, last of all, the study of the means whereby liberty and authority may be best combined" (1946*b*). He went on to express his skepticism about the possibility of a science of politics and to warn against imposing on the teaching of politics "a degree of abstractness or bogus neutrality which it cannot stand." Instead, he avowed his own conviction that the teacher could and should relate politics to history—"present politics is always at least half history"—and bring to bear on contemporary problems "the great minds of the past." He was frank about his own approach: "I am by equipment, by temperament and by limitations, a student of political institutions. I am incapable of changing, of becoming a political philosopher or a philosophical historian."

Indeed, in the conventional sense, Brogan was neither a system builder nor even primarily a political theorist. Yet his disclaimer went a little far. Not only were Brogan's lectures (regrettably unpublished) cogent and often bravura expositions of the history of political theory; he also had contributions to make of his own at the point where theory and institutions meet. *The Price of Revolution* (1951) is, at the very least, a searching examination of the applicability of political theory to political practice, an analysis of the effects on political and social life of the ideas of violent and revolutionary change. Moreover his *The American Problem* (1944*b*) is essentially a study of the formative roles in American history of tenaciously held American ideas and ideals and their implications for America's functioning in the postwar world. Both books are informed by Brogan's deep and intuitively liberal cast of thought. Though no doctrinaire laissez-faire-ist, he stood to one side of the fashionable socialism of postwar Britain and, although without bigotry, was more readily aware of the menace of a totalitarianism of the left than many of his scholarly contemporaries on either side of the Atlantic.

Despite the conscientious discharge of his professorial duties, it was the United States that continued to dominate Brogan's interests during his Cambridge years (a fact by no means to the university's disadvantage at a time when it lacked any denominated posts in American his-

tory or politics). The scholarly products of these years, mostly listed above, are marked by certain common characteristics. Brogan's interest in things American was rooted, in the first place, in a deep affection and intense relish for the variety and vitality of the United States. No one has better captured the sparkle and color of the American experience. Yet he never mistook surface appearances for underlying reality. He wrote as a pluralist and a pragmatist, devoted to the liberal, suspicious of the conservative, values of American society, and swift to pounce on the pompous, the pretentious, and the sham. Similarly, beneath the gaiety and vivacity of Brogan's almost uniquely wide-ranging depiction of America's past and present, there always lurks the stern moralist and the equally severe cultural mentor. To Brogan the observer, nothing is alien or insignificant; to Brogan the critic, values do not change when they cross the Atlantic or don modern dress. If his writings continue to have significance for the student of American history and politics, it lies in their distinctive combination of avidity for all the details of the American experience with an unsleeping awareness of the relativity of that experience. Not for nothing was Brogan a scholar of America *and* France. Though the points of intersection of the two cultures are limited and perhaps not crucially significant in themselves, they provide each other, perhaps for that very reason, with a set of contrasts and comparisons. Brogan had a remarkable gift for holding in his extraordinarily capacious memory something very like the total experience of these two societies and using their resemblances and differences to sharpen awareness of the distinctive character of each. As such, he is a constant prophylactic against parochial historiography, against illusions of American (or French) exceptionalism, against the latent menace of "un-American" as a value judgment. Other scholars may reasonably be judged to be greater authorities on the experience of either society; none has so effectively informed his understanding (and ours) of the one by his engagement with the other.

Brogan retired from his chair in 1968 but remained in Cambridge active as a writer and lecturer until his death in 1974.

H. G. NICHOLAS

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BURKE, KENNETH

Kenneth Burke, born in 1897, has been influential in many fields: literary criticism, literary theory, semiotics, and rhetoric, as well as ap-

plied psychology, sociology, anthropology, philosophy, and religion. His wide influence has always been somewhat informal and indirect. Although he has taught and lectured in many colleges and universities in the United States, he has always remained somewhat on the margin of the academic world. He has not produced a "school" of explicit followers in any of the disciplines he has worked in, although he has been a decisive influence on many important writers and teachers. He has come to be recognized as one of America's most innovative twentieth-century literary and social theorists.

Although Burke's work may seem characteristically "American" in being homespun, eclectic, and miscellaneous, in the tradition of Emerson, Charles S. Peirce, or William James, it has in fact a constant focus, a consistent curve of development from the beginning on, and a constant set of decisive "sources." The sources include Aristotle, Coleridge, Bentham, Marx, and Freud; equally important, however, are the native American traditions of pragmatism and behaviorism. All of Burke's writing is characterized by a kind of pugnacious flexibility or playfulness, a desire to see things differently and upset the apperception of received opinion or institutionalized assumptions. His focus has always been on "man as the specifically symbol-using animal." This focus has meant a primary interest in language or signs generally, in relation to nonsymbolic bodily motion, in one direction, and to the realm of manifold social motivations, in the other. The center of Burke's investigations has always been language as a form of "symbolic action." Language for him is always designed to do something or to work as "a strategy for encompassing a situation." A text, he would say, whether it is a poem, a philosophical treatise, or a corporation executive's letter to stockholders, is the "dance of an attitude." The curve of Burke's development has remained within this single channel, moving back and forth between the analysis of a great variety of philosophical, social, and literary texts, on the one hand, and wide-ranging theoretical formulations, on the other.

Kenneth Burke was born in Pittsburgh. He spent his first years in a lower-class suburb there. The contrast in Pittsburgh between the vast smoky concentration of industrial power and the threatened remnants of an original natural beauty gave him an abiding (and prophetic) insight into the problems of "ecology." His early life also gave him a permanent sus-

picion of technology. He attended Ohio State University for a semester in 1916, then, after a brief time at Columbia University, abandoned a promising academic career to devote himself to writing. In 1918 he moved to Greenwich Village in New York, where he began a long-term association with such *avant-garde* writers as Malcolm Cowley, Matthew Josephson, Hart Crane, and William Carlos Williams. In 1922 he bought a farm near Andover, New Jersey, which he has made his permanent home. He worked as editor and writer for some of the important "little magazines" of the time: *The Dial*, *Secession*, and *Broom*. During this period he published much short fiction and a novel, as well as his earliest important criticism.

Burke's first collection of critical essays, *Counter-statement*, was published in 1931. He has worked out the implications of that first book in the multitude of books and essays that have followed. Service as a lecturer in criticism at the New School for Social Research in 1937 was the beginning of a career as university lecturer. From 1943 to 1961, with intermissions, he was professor of criticism at Bennington College in Vermont. He has taught or been appointed a research fellow at many universities and colleges, among them the universities of Chicago, Harvard, Wesleyan, Princeton, California (at Santa Barbara and at Irvine), and Pittsburgh, the Center for Advanced Study in the Behavioral Sciences, and the Princeton Institute for Advanced Study. He has received many grants, honors, and awards for eminence in humanistic and social studies.

A useful survey by Burke of his own development is the "Curriculum Criticum" he prepared for the 1953 reprinting of *Counter-statement*, and then extended further in a 1968 paperback edition.

Although *Counter-statement* may seem to differ from Burke's later books in its "aestheticism" and in its focus on such "art for art's sake" writers as Flaubert, Pater, Gide, and Remy de Gourmont, this first book is already characteristically Burkean in springing, as he says, out of the notion that dramatic or symbolic action forms the basis of all literature; in its concern with the revealing of motives that may be hidden under the aesthetic and representational surface of a work; and in its movement from the interpretation of particular works toward formal, rhetorical, or methodological generalizations. An example is the "Lexicon Rhetoricae" near the end of *Counter-statement*.

Burke's next book, *Permanence and Change: An Anatomy of Purpose* (1935), shifts from "the individualist emphasis of his earlier aestheticist period" to "interdependent, social, or collective aspects of meaning": "communication, interpretation, orientation, integration, cooperation, translation, simplification." Burke introduces in this book one of his most productive strategies, what he calls "perspective by incongruity." This is a Nietzschean technique of viewing a process or relationship through the lens of an unfamiliar terminology or metaphor. "Perspective by incongruity" is a codification of the linguistic nimbleness, the ability to shift from one code to another, and to translate vocabularies into one another, that has always characterized Burke's way with words. The emphasis in *Permanence and Change* is on the universals of social organization and communication in man the symbol-using animal, whereas the next book, *Attitudes Toward History* (1937), tries to "characterize tactics and patterns of conflict typical of actual human associations." This book analyzes the way even the most idealistic aims lead to less than ideal results when they are embodied in institutions or practical human activities. Burke calls this the "bureaucratization of the imaginative."

These two more or less sociological books were followed by one of Burke's most useful books for literary study: *Philosophy of Literary Form: Studies in Symbolic Action* (1941). This collection of essays and reviews exemplifies his notion that the surface patterns of a text will manifest, with proper translation of its terms, a cryptic symbolic action taking place behind the scenes. The theoretical groundwork behind such practical essays in interpretation was developed in the two books that followed: *A Grammar of Motives* (1945) and *A Rhetoric of Motives* (1950). They were to have been completed by "A Symbolic of Motives," which has never been published as such, though its outlines are clear enough from Burke's later books and essays, such as the large collection of periodical essays, *Language as Symbolic Action: Essays on Life, Literature, and Method* (1966). The *Grammar* works out the five key "dramatist" terms: act, scene, agent, agency, purpose. This pentad is based on the assumption that "any complete statement about motives will offer *some kind of* answers to these five questions: what was done (act), when or where was it done (scene), who did it (agent), how he did it (agency), and why (purpose)." *A Rhetoric of Motives* moves, with

many examples, back into the social realm of persuasion, but now with a special concern for pyramidal or hierarchical relations of higher and lower, up and down, in any bureaucracy, social order, or institution.

One of Burke's basic insights has always been into the way any system of language or other signs, even the most secular, takes on a structure like that of a metaphysical or religious system of terms and has its own "god terms," its own equivalents of heaven and hell, and the like. *The Rhetoric of Religion: Studies in Logology* (1961) reverses this perspective. It attempts to show, by way of interpretations of "Verbal Action in St. Augustine's *Confessions*" and of the first three chapters of Genesis, that "words about God" (theology) can be applied to a wholly empirical end, for the light they throw upon 'words about words' (logology)."

Burke's work will remain seminal for the wide range of disciplines he has influenced. He is a powerful model as much for the examples he provides of how to do things with texts, for his wonderfully exuberant, daring, and challenging interpretations, as for the bold but always somewhat incomplete system-building. In this he is true to his own principles, which are based on a sense of the constant mobility in any symbol system and on the notion that any piece of language—for example an essay on a particular poem or philosophical essay—must have a local purpose. Burke's own work has always itself been a strategy for encompassing a situation. His work of the 1930s was surprisingly prophetic of developments in the 1960s and 1970s in the humanities and social sciences, Parisian "structuralism," for example, or the "paradigm shift" initiated by linguistics, psychology, and Wittgensteinian trends in philosophy. This shift is from a constative or descriptive to a "performative" view of language. Burke's view of language has always been "performative." Nevertheless, he remains true to his behavioristic bent, to his sense that nonsymbolic bodily motion underlies even the most elaborate symbolic system and can never be happily coordinated with it. This means the belief that there is always a trustworthy referential dimension to words, along with their participation in elaborate processes of symbolic action that transcend the body and can never be reconciled with its sheer motion. As Burke puts this in an essay exemplifying the development of his thought in the 1970s: "In every tribal idiom, however rudimentary, there is a wholly reliable basic correspondence be-

tween a thing and its name; never the twain shall meet" (1978). In that "never the twain shall meet," in Burke's sense of "the unresolvable polar relationship between symbolicity and the nonsymbolic realm of motion" (*ibid.*), Burke's thought has remained from the beginning; but the ever-dissatisfied movement from one pole to the other has generated his remarkably fecund series of books and essays. In spite of his exuberance, Burke's sense of the incompatibility of these two poles has made his work to some degree somber, even "tragic," appropriately enough for someone whose orientation has always been "dramatistic."

J. HILLIS MILLER

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BURNS, ARTHUR F.

Many of America's great economists have come to the United States from other lands. Arthur F. Burns, born in Austria in 1904, emigrated to the United States with his parents in 1914, and became one of America's most prominent economists. He found many opportunities in his chosen profession: a university professor for many years; an economic researcher; director of research and later president of a famous economic research institution; president of the American Economic Association; adviser to several presidents of the United States; and chairman of the Board of Governors of the Federal Reserve System. At the same time, through his teaching, guidance of the economic investigations of others, thoughtful and expert advice on economic policy, and role in managing a vital segment of that policy, he has enhanced the capacity of others to find their own opportunities.

Burns's professional career has been devoted largely to the study, and in recent years to the amelioration, of economic fluctuations. Secular trends, seasonal variations, cyclical movements—all have occupied his scholarly attention at one time or another. During his eight years as chairman of the Board of Governors of the Federal Reserve System, he became best known as America's inflation fighter. He was concerned about this problem because of the hardship he saw it create for so many people who were not in a position to adjust to a general, rapid, and sustained rise in prices. He was concerned also because inflation jeopardized economic growth, produced uncertainty about the health of financial institutions, encouraged speculation, and rendered ineffective many of the instruments of economic policy that had been developed to deal with recession and unemployment. He was in a position to see at firsthand how the various types of economic fluctuation interact with one another and how they affect and are affected by international trade and financial transactions. He brought to the current problems that he had to face a broad knowledge: of economic his-

tory; of economic growth and the factors that have hastened or retarded it; of construction activity and other forms of capital investment and the processes that create it; of the causes and consequences of financial crises and economic depressions as well as the milder recessions; of the role of government, its intervention in economic life, and the practical political process; of statistical data and methods of analysis and their value and limitations. His scientific books and papers, his testimony before Congress, his addresses to public audiences and his interviews with the press all reflect this store of knowledge and its relevance to matters of present-day concern.

Burns is meticulous about words and can convey complex ideas in simple language. He rarely resorts to mathematical symbols, though his work shows a capacity to use them—in footnotes or appendixes. Part of his early training in effective writing was developed when he wrote for the financial pages of the *New York Herald Tribune* in the early 1930s. He benefited also from the literary skill of his mentor and collaborator, Wesley Mitchell. In his work at the National Bureau of Economic Research, his professional home for more than thirty years, he often evaluated manuscripts produced by the staff and would invariably make editorial as well as substantive suggestions for their improvement. He likes to write, and even during his busy life as administrator of the Federal Reserve System he composed more than a hundred essays in the form of addresses before university, banking, business, and congressional audiences that are marked by his style of expression and knowledge of the subject matter.

Burns's first major research project was his doctoral dissertation at Columbia University, *Production Trends in the United States Since 1870* (1934). The book displays many of the qualities of his later work: careful attention to the factors affecting economic decisions by business enterprises; a blend of theoretical reasoning and empirical investigation; thorough acquaintance with the statistics being analyzed and their relationship to the phenomena they are taken to represent; and a sophisticated statistical treatment of the problem of measuring the growth and decline of industries. He found retardation of growth—that is, slower growth as an industry matures—to be nearly universal, and he attributed it largely to the rapid growth of newer industries that displaced the products of the older industries. Thus, the phenomenon of retar-

dation in growth was an inherent consequence of the competitive process, broadly defined, and of economic progress. The more vigorous the competition of new industries, the more evidence of retardation there would be.

Nevertheless, Burns observed, the retardation of growth in individual industries did not imply a slowdown in the growth of the total output of the nation. On the contrary, the rapid development of new industries, which produced the retardation, was essential to the continued rapid growth of total output. Retardation might, therefore, be found everywhere, but not in the aggregate output of the economy as a whole. What was characteristic of the parts was not characteristic of, or even visible in, the whole. This, too, was a finding that affected Burns's later thinking about the conduct of economic research and of economic policy. It was essential to look beneath the surface of the aggregates, such as gross national product, total employment, the general price level, in order to discover how the economy really worked. His continued distrust of models constructed largely from such aggregates stems in part from his view that the aggregates do not adequately reflect what goes on in the economy. His later work on business cycles stressed the "unseen cycle" in the components of aggregates, the fact that "corporations do not have a common pocketbook," and the "cross-currents" that develop as a turning point in the cycle approaches. Hence, the factors that are crucial to growth are often not apparent in such aggregates as the gross national product (GNP). Burns's first report as chairman of the Council of Economic Advisers under President Dwight D. Eisenhower started with the proposition that one of the powerful forces for economic growth was the development of mass markets for new consumer goods, and the first table in the report was devoted to figures on the output of new products such as television sets, frozen foods, and room air conditioners.

Burns's early work on production trends led Frank W. Taussig, then the editor of the *Quarterly Journal of Economics*, to ask him for a review article on the Brookings Institution's four-volume study of America's capacity to produce and consume. The review was published in May 1936 and faulted the work on both conceptual and empirical grounds. The study's estimates of productive capacity were not developed with sufficient attention to the economic, as opposed to the technological, possibilities of increasing production, which led Burns to note

that "an estimate of potential increase in output, whether high or low, has little meaning unless it is accompanied by a theoretical analysis which indicates how the increase may be realized." He did not attach much significance, therefore, to the Brookings Institution estimate that in the boom year 1929 the potential national output exceeded the actual output by 19 per cent. His skepticism regarding such estimates came to the fore again many years later, when estimates of the GNP gap became an extremely important guide to economic policy. In 1967, in diagnosing the failure of economic policy makers in 1964–1965 to recognize the signs of inflation that had begun to appear and to shift to measures of restraint rather than continued stimulation, he pointed to the estimates of the gap and the theoretical apparatus behind them, which attributed the gap entirely to a shortage of aggregate demand rather than to obstacles on the side of supply or to an incomplete adjustment of demand and supply to one another.

Burns's association with the National Bureau of Economic Research began in 1930, when he was appointed a research associate. In the same year he married Helen Bernstein. The bureau, then only ten years old, was directed by Wesley C. Mitchell, one of Burns's professors at Columbia. They became the best of friends and collaborators. Following the publication of *Production Trends* in 1934, Burns turned his full attention to business cycles and to the method of business cycle analysis that Mitchell, Simon Kuznets, and others had been developing. Together with Mitchell, he set about preparing a comprehensive description of the method and an exhaustive test of its properties. A short paper on the subject, the first of several authored jointly by Mitchell and Burns, appeared in 1935. Publication of the full report was delayed during World War II, but the massive book, entitled *Measuring Business Cycles*, finally appeared in 1946. It became the "bible" of the many researchers who have participated in the bureau's business cycle studies over the years.

In 1937 Mitchell and Burns collaborated in a study, requested by Secretary of the Treasury Henry Morgenthau, that utilized the measures of cyclical behavior as a guide to the selection of the most useful indicators of cyclical revivals. They laid down certain principles that should guide the choice and applied them to the 487 American monthly or quarterly series that had been analyzed in the research program, selecting 71 that had been "tolerably consistent in

their timing in relation to business cycle revivals and that at the same time are of sufficiently general interest to warrant some attention by students of current economic conditions" (Mitchell & Burns 1938, p. 4). They further refined this list to obtain 21 series that seemed to be the most trustworthy indicators of revivals. Although this report was considered by the authors to be a "minor by-product" of the bureau's studies of business cycles, it has had a lasting impact. The sets of "leading, roughly coincident, and lagging indicators" that are in wide use in the United States, Japan, the United Kingdom, Canada, and several other countries are the direct descendants of this work, and many of the indicators originally selected by Mitchell and Burns have continued to display the same cyclical properties that they discerned in them.

Measuring Business Cycles provoked a professional controversy. Its critics claimed it was "measurement without theory," the title of a review by Tjalling Koopmans. Burns and Mitchell had devised a system of measurement without first setting forth the theory of business cycles that they expected the measurements to test—an ineffective and inefficient way to proceed, according to the critics. The more promising approach was to adopt a provisional theory (and the one favored by most of the critics was the one set forth by John Maynard Keynes some ten years before), create a mathematical model that expressed the theory, obtain the data on the variables called for by the model, and see how well the model fit these facts. During the next thirty years this became the more popular approach in academic circles, but the controversy remained alive, partly because no single theory or model took the field.

This indeed was the condition that led Burns and Mitchell to adopt a comprehensive plan of measurement based upon a descriptive definition rather than a causal theory of business cycles. Their plan was to establish what was typical about these phenomena in terms relevant to the large number of theories that had already been excogitated. Knowledge of what was or was not typical would show what a general theory had to account for. Too many theories, they believed, had been based upon an incomplete knowledge of the facts, and hence either "explained" phenomena that were not typical or failed to explain phenomena that were typical. In the event, certain parts of this measurement system came to be widely used and kept up to date, notably the chronology of business cycle

peaks and troughs, patterns of expansion and contraction based upon these dates, and characteristic lead-lag sequences. In establishing a comprehensive measurement system with a minimal theoretical structure, capable of investigating the current and past behavior of virtually any economic variable, avoiding arbitrary and therefore controversial decisions about "causes" and "consequences," and emphasizing easily comprehended results, the research strategy that Burns and Mitchell advocated bore fruit in a wider public awareness of many of the facts about business cycles that they thought should be better known "before making a fresh attempt to explain them" (Burns & Mitchell 1946, p. 4).

Burns continued to hold the view that theorizing on the basis of an incomplete set of facts posed a serious danger to the development of economic science and to its role in policymaking. Between 1946 and 1952 he wrote a series of works on this theme: *Economic Research and the Keynesian Thinking of Our Times* (1946), "Keynesian Economics Once Again" (1947), *Current Research in Business Cycles* (1949), *The Instability of Consumer Spending* (1952*b*), and "Hicks and the Real Cycle" (1952*a*). He characterized the kind of economic analysis stimulated by Keynes as "bold and vigorous theoretical speculation" that especially needed to be checked against the facts because the results of such speculation were being used to guide economic policy. "Keynesian Economics Once Again" contains a strong statement on the responsibility of an economic adviser:

An economic theorist is justified on many occasions in oversimplifying facts to clarify in his own mind what he believes to be significant relationships. He is likewise justified in bringing the results of his speculative inquiries before his colleagues, whether to seek their critical appraisal before going further or to stimulate them by his work. As long as the economist moves within these boundaries he may be excused even for not making a strenuous effort to discover how seriously he has distorted the facts by his simplifying assumptions. But when he attempts to give practical advice, he loses his license to suppose anything he likes and to consider merely the logical implications of untested assumptions. It then becomes his duty to examine with scrupulous care the degree to which his assumptions are factually valid. If he finds reason to question the close correspondence between the assumptions and actual conditions, he should either not undertake to give any practical advice, or should frankly and fully disclose the penumbra that surrounds his analysis

and the conclusions drawn from it. Better still, he should rework his assumptions in the light of the facts and see whether he is justified on this new basis in telling men in positions of power how they should act. Economics is a very serious subject when the economist assumes the role of counselor to nations. (1947, pp. 264–265)

Some five years after enunciating these principles Burns had the opportunity to put them into practice, when he became chairman of President Dwight D. Eisenhower's Council of Economic Advisers. His path to this position was, however, not a smooth one, because Congress had become disaffected with the functioning of the previous council and was not eager to appropriate funds for this purpose under the newly elected president. After several months of uncertainty early in 1953 about the status of the council, a reorganization plan devised by Burns was adopted, the main feature of which was to concentrate authority in the hands of the chairman and make him directly responsible to the president. Burns was appointed to the post. He was immediately faced with the prospect of a business cycle recession. Indeed, his nomination and confirmation as chairman of the reconstituted council came in July 1953, a date he has reason to remember because that month was later designated a peak in the National Bureau's business cycle chronology, the same chronology that Burns had done so much to establish.

Undaunted by the challenge, he stepped into the task with authority and boldness. His account of the years 1953–1954, which is contained in the January 1954 and January 1955 *Economic Report of the President*, reveals a wide range of actions taken to offset recession, encourage growth, and prevent inflation. It also reveals much of what Burns had learned in his studies of business cycles, as well as his scholarly interest in stepping back and setting forth the principles governing policy actions as well as the lessons to be learned from the experience.

The January 1954 *Report*, which expressed the view—happily correct—that the relatively “minor readjustment underway since mid-1953 is likely soon to come to a close,” states the principles that would guide the administration in dealing with the possible threat of a depression:

The first and foremost principle is to take preventive action. . . . The second principle is to avoid a doctrinaire position, work simultaneously on several fronts, and make sure that the actions being taken harmonize and reinforce one another. The

third basic principle is to pursue measures that will foster the expansion of private activity, by stimulating consumers to spend more money and businessmen to create more jobs. . . . The fourth principle is to act promptly and vigorously if economic conditions require it. The government will not hesitate to make greater use of monetary, debt management, and credit policy, including liberalized use of federal insurance of private obligations, or to modify the tax structure, or to reduce taxes, or to expand on a large scale the construction of useful public works, or to take any other steps that may be necessary. . . .

The need for constant vigilance and preparedness by Government [the *Report* went on to say] does not, however, justify constant stirring or meddling. Minor variations in activity are bound to occur in a free economy, or for that matter in any type of economy. The arsenal of stabilizing weapons will be drawn upon by the government boldly, but not more frequently than is required to help maintain reasonable stability. Nor will flexible policies aiming to minimize economic fluctuations be permitted to interfere any more than is necessary with the fiscal objective of bringing down the scale of Federal expenditures, reducing taxes, and arriving at a budgetary balance. (*Report*, January 1954, pp. 113–114)

The January 1955 *Report*, which could point to clear evidence that economic recovery was under way, undertook to draw some lessons from “this latest encounter with the business cycle”:

First, that wise and early action by government can stave off serious difficulties later. Second, that contraction may be stopped in its tracks even when governmental expenditures and budget deficits are declining, provided effective means are taken for building confidence. Third, that monetary policy can be a powerful instrument of economic recovery, so long as the confidence of consumers and businessmen in the future remains high. Fourth, that automatic stabilizers, such as unemployment insurance and a tax system that is elastic with respect to the national income, can be of material aid in moderating cyclical fluctuations. Fifth, that a minor contraction in this country need not produce a severe depression abroad. Sixth, that an expanding world economy can facilitate our own readjustments. (*Report*, January 1955, p. 22)

Although Burns could take some pride in the essential validity of his diagnosis and prognosis in 1953–1954, and President Eisenhower could take pride in his lieutenant's crisp command of the situation, Burns was ever mindful of the limitations on economic foresight. The analyses in the *Reports* for which he was responsible frequently referred to the types of economic

indicators that his earlier studies had found useful. They are also filled with warnings like "it is well to keep in mind the sobering fact that there is no way of lifting more than a corner of the veil that separates the present from the future. . . . The uncertainty of economic predictions requires that the federal government be prepared to adjust its policies promptly if economic events should not bear out current expectations" (*Report*, January 1955, pp. 24–25). This caution led him to refuse to incorporate an explicit forecast of GNP in the *Report*, despite considerable pressure from Congress to do so.

In 1957 Burns returned to his professorship at Columbia University and became president of the National Bureau of Economic Research, where he remained until called to Washington by President Richard M. Nixon in 1969. One of the first essays he wrote during these years was "Progress Towards Economic Stability," his presidential address before the American Economic Association in December 1959. Here he traced some of the changes that had occurred in the severity and other features of business cycles over the years. He noted that the cyclical swings in production had become smaller, and that the linkages between production, employment, personal income, and consumption had become looser. "One of the triumphs of this generation," he concluded, "is the progress our nation has made in reducing economic instability."

Although the progress that had been made was clear, with the reasons for it lying partly with changes in the structure of the economy and partly with defenses erected deliberately to deal with recession, Burns did not believe the business cycle had been conquered. Many economists disagreed, especially during the long upswing of the 1960s, which was interrupted only by a mild recession in 1960–1961 and by slowdowns in 1962–1964 and 1966–1967. In Burns's view, however, the business cycle was not dead, and in 1968 he wrote a notable article to explain its self-generating features and what had been learned about them. The structure of his analysis is revealed in the headings of four sections of the essay: "The Cumulative Process of Expansion," "Gathering Forces of Recession," "The Process of Contraction," and "Forces of Progress and Recovery." To a historian, what is perhaps most striking about this explanation of the business cycle is its similarity, in main outline, with the account set forth by Mitchell in 1913. Burns's familiarity with the theoretical and empirical researches since then, with the significant

changes in economic institutions, and with the practical problems of policymaking at the highest level of government had simply reinforced his view that the Mitchellian approach to diagnosis of the swings from prosperity to recession was essentially valid and eminently useful. His chief amendment to this approach was to stress the role of factors making for long-term economic growth in the very process of a cyclical revival.

One of the reasons why, despite the progress that had been made in countering recession, Burns believed that business cycles were still to be reckoned with, was that he did not think similar progress had been made in dealing with inflation. Increasingly during the 1950s and 1960s he devoted attention to this problem. In 1957, in an illuminating essay on "The New Environment of Monetary Policy"—illuminating especially because of Burns's later role as head of the Federal Reserve—he noted that the great expansion of government expenditures since the 1920s had "reduced the area over which a restrictive credit policy can nowadays be effective," because governments are not likely to allow such policies to thwart their other objectives. He noted, too, that the Employment Act of 1946 pledges the government to "utilize all its plans, functions and resources" to foster economic expansion and to help prevent depressions, that this embraced the Federal Reserve authorities, and that they are therefore "fairly bound to pursue a policy of credit restraint with considerable caution, lest the application of restraints bring on the very decline of aggregate economic activity which it is the responsibility of the government to try to prevent" (1969, p. 164). In view of these and other limitations that attached to restrictive Federal Reserve policies, he concluded that "it would be unwise to depend on the Federal Reserve System as our sole or principal guardian of the stability of the dollar" (p. 174). It was an apt forecast of the situation he would face later.

Burns's term as chairman of the Federal Reserve Board, from 1970 to 1978, began in the midst of a mild recession and ended in a period of vigorous expansion. It encompassed the severe recession of 1973–1975, and it also encompassed a period of sharp inflation. He held office for 98 months, of which 26 were months of recession, 72 were months of economic expansion. The general level of prices and wages rose throughout the period, but the rate of inflation in both was not very different when he left office from

when he entered it. He presided during a turbulent period, the history of which is still to be written. Burns himself will surely participate in writing this history, and he has already provided some of the documentation in the form of a 475-page volume, *Reflections of an Economic Policy Maker* (1978), containing a selection of his speeches and testimony during his eight-year tenure as Federal Reserve chief. But historians' future appraisal of his role is unlikely to diminish his reputation as a wise counselor, skillful administrator, thorough scholar, responsible official, and patriotic citizen, a reputation that was enhanced by his chairmanship, both in his own country and around the world.

Despite his professional responsibilities and penchant for hard work, Burns has had a rewarding family life. One son, David, is a lawyer; the other, Joseph, is an economist. For many years the family spent their summers on a farm in Ely, Vermont, where Burns built a separate cabin to use as a study. Each summer he takes some time to paint abstract oils, always in vivid colors. Occasionally he takes a turn at house painting, one of the jobs he worked at as a youth under the guidance of his father.

A few weeks before he left the Federal Reserve, Burns recalled that he began his life in America as a Jewish immigrant. He summed up his feelings in the following words:

It is clear to me, therefore, as I reflect on my own life, that whatever success I have been able to achieve is due fundamentally to the fact that it has been my good fortune to be a citizen of a country whose people have had the opportunity to acquire a decent education, whose people have had the freedom to seek truth and to express it as they see fit, whose people have had the opportunity to put to work such special talents as they may have and to utilize those talents for themselves, their families, and their country.

In short, it has been my great privilege to be an American. (1978, p. 68)

GEOFFREY H. MOORE

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BURT, CYRIL

Cyril Burt (1883-1971) was one of the leading pioneers in the development of psychology as an applied scientific discipline. While still a schoolboy he came into contact with, and under the influence of, Francis Galton, and his work can be regarded as essentially an attempt to uphold the Galtonian tradition of individual psychology. This tradition stressed the importance of individual differences in ability and character, the role of heredity in determining these differences, and the need to develop quantitative methods in order to study them scientifically. From the publication of his first article on intelligence in 1909 to his death in 1971 Burt worked assiduously and single-mindedly within this Galtonian framework.

Burt's early boyhood was spent in humble circumstances in London, where his father was a struggling medical practitioner. Nine years after his son's birth, Dr. Burt left the capital, and moved to a rural practice near Stratford-on-Avon,

and it was here that his contacts with Galton were first established. The Galton family resided in a nearby village, and Burt senior became their family physician. Young Cyril would accompany his father on his rounds during school holidays, and in this way first met the great Francis Galton. The meeting was to have a permanent and decisive influence on his development.

Burt's induction into psychology, however, was slow and uphill. At the beginning of the century it had hardly become a recognized subject of instruction, or a recognized career, in Great Britain. Since his father was never financially prosperous Burt's education depended almost entirely on his own efforts, and his success in winning scholarships. He was academically gifted, intellectually curious, hardworking, and quick on the uptake. So he made his way first to the ancient school of Christ's Hospital in London, and then to Oxford University. His education at school and university was primarily in the ancient classical languages, and in the literature of Greece and Rome. His mathematical and scientific education was acquired almost wholly by his own efforts after he had left school. At Oxford, however, the classical curriculum was a broad one, and included a training in ancient philosophy, logic, and, as an optional extra, psychology. Fortunately for Burt, William McDougall had just been appointed to take charge of psychology at Oxford, and Burt became his first student. McDougall, after Galton, was the other major influence shaping Burt's psychological development. McDougall confirmed him in his determination to take up psychology as a career, and provided him with his first opportunity. In 1902 the British Association for the Advancement of Science, largely as a result of Galton's advocacy, had decided to initiate an anthropometric survey of the British people, involving the measurement of both physical and psychological characteristics. In 1907 a psychological subcommittee was set up under McDougall's direction to plan the psychological side of the survey. In connection with this project, McDougall enlisted Burt to carry out an investigation into intelligence with the idea of checking some of the ideas on the existence and nature of general intelligence propounded a few years previously by C. E. Spearman. Burt carried out the practical work for this investigation during the 1907/1908 session immediately after graduating, and published the results in 1909. It was to set the pattern for the whole of his subsequent life's work. Meanwhile he had advanced his psychological

education by spending the summer of 1908 in Oswald Külpe's laboratory at the University of Würzburg, and had secured his first post as a lecturer in psychology attached to C. S. Sherrington's department of physiology at the University of Liverpool. He spent five years (1908-1913) with Sherrington, and although he had not had the training to take up physiological research as such, he was undoubtedly much influenced by Sherrington's general viewpoint on the integrative function of the nervous system and on mind-brain relations.

In 1913 Burt returned to his birthplace, London, on his appointment as psychologist to the London County Council. This was a new appointment, and the first of its kind in Great Britain. Burt's job was to assist the Council's education committee, which was responsible for London's schools, in two main tasks: to identify and assess pupils who required special treatment, either by reason of mental subnormality or of behavioral maladjustment, and to assist in the selection of gifted pupils for entry to secondary education, which was then, for those who could not afford to pay for it, highly competitive. In furtherance of these aims Burt conducted a systematic survey of the distribution of educational abilities among London school children, and compiled and standardized a set of mental and scholastic tests that remained in use for more than forty years. Among his tests were one of the first British versions of the Binet scale, and a verbal reasoning test, which was important in providing the stimulus for Jean Piaget's early studies on the development of intelligence. In addition to his psychometric work, Burt ran what was in effect the first child guidance clinic in Great Britain. The material he collected in this clinical work formed the core of his classical treatises, *The Young Delinquent* (1925) and *The Backward Child* (1937).

Burt's routine duties with the London County Council occupied only part of his time. The remainder he spent in various ways. During World War I he was engaged for some time at the Ministry of Munitions in statistical work. After the war he assisted the National Institute of Industrial Psychology, which had been founded in 1921, to establish a vocational guidance service, and directed a research project into vocational guidance. In 1924 he became part-time professor of educational psychology at the London Day Training College, a teachers' training establishment affiliated with the University of London. In addition he was encouraged by the London

County Council to undertake his own research, and it was in this connection that he began to collect the material on twins, which was published many years later, and which became a major focus of controversy.

Burt held the post of psychologist to the London County Council from 1913 to 1932. During this period he not only carried out his routine duties with great energy and competence, but he collected, with the aid of teachers, social workers, and research assistants, a large body of data, both clinical and psychometric—in fact most of the data he was to make use of in his subsequent publications. In quantity Burt's data were impressive; qualitatively, however, much of it lacked the rigor and precision required of research material. It was for the most part collected incidentally to Burt's practical duties as an educational psychologist; it was collected for a variety of different purposes; and much of it was gathered by assistants lacking in adequate psychological training. Accordingly Burt felt entitled to "adjust" test scores in the light of other evidence, often of a subjective kind. He possessed considerable skill in deriving conclusions by statistical reasoning from imperfect data, but it is questionable whether the elaborate treatment he later accorded it was ever really justifiable.

Burt's work for the London County Council soon brought him into public prominence, and his advice was increasingly in demand. He was called on to assist the Board of Education in its plans for the reconstruction of the English education system, and his evidence played a considerable part in shaping the scheme of selective secondary schooling that was incorporated in the Education Act of 1944. He was, too, largely instrumental in the setting up of a Child Guidance Council in 1926, which, with the help of the Commonwealth Fund of America, led to the establishment of the first fully staffed child guidance clinics in Great Britain.

In 1932 Burt's attachment to the London County Council terminated with his appointment to the chair of psychology at University College London, in succession to Spearman. This appointment marked the climax of, and at the same time a turning point in, his career. Until 1932, Burt's energies had been directed primarily towards applied problems; after 1932, he turned mainly to theoretical and methodological questions, chief among which was factor analysis. Burt's first acquaintance with factorial techniques arose through his contacts with Spearman

while still at Oxford. He employed factor analysis on a small scale in his early investigation into intelligence, and a few years later undertook a factorial study of emotional traits, one of the first excursions into the statistical analysis of personality. In London he applied factorial techniques to the analysis of educational abilities, and developed a method of analysis which anticipated L. L. Thurstone's centroid method. On his appointment to University College he began to concentrate on factorial studies. He acquired a mastery of the mathematics and methodology of factor analysis, and his conclusions began to diverge from those of Spearman, though he still publicly acknowledged Spearman's priority and preeminence in the field. His views were set out definitively in *The Factors of the Mind* (1940), and developed from 1947 onwards in the pages of *The British Journal of Statistical Psychology*, which he founded with Godfrey H. Thomson, and edited until 1963. In the statistical journal and elsewhere, Burt elaborated and amplified his factorial techniques, and summarized the conclusions of his factorial investigations, presenting arguments in favor of a hierarchical scheme of general, group, and specific factors, both of ability and personality. The most important factor was the factor at the head of the ability hierarchy, which, following Spearman, Burt designated "g" and defined as "innate, general, cognitive ability." This supreme ability he believed could be accurately assessed by means of intelligence tests. Burt was also much concerned, in the final phases of his factorial work subsequent to Spearman's death in 1945, in dethroning Spearman as the founder of factor analysis, and asserting his own claims to priority as the first user of factorial method in psychology. These claims were wholly unfounded, and led Burt to falsify the historical record in a way that suggests a morbidly egotistical streak in his personality.

Burt retired from his chair at University College in September 1950, and lived quietly at his flat near Primrose Hill for the remaining 21 years of his life. He kept extremely busy, writing articles, reviewing books and manuscripts, examining, editing his journal, and, until 1960, lecturing. Apart from an experimental study of typography, based on work carried out before his retirement, most of his publications during this period were of a theoretical nature—on consciousness, values, psychical research, factor analysis, and, in particular, the multifactorial theory of inheritance. After 1940 Burt did not

produce any major, large-scale work. The articles which flowed unceasingly from his pen were in the main either restatements of his earlier views, or rejoinders to attacks, to which he was increasingly subjected from the early 1950s onwards. After his retirement he carried out no research either in the laboratory or in the field. He became more and more a lonely and isolated figure, who, though convinced of his own rightness, felt the tides of opinion turning against him.

Things had, in fact, begun to go wrong for Burt in the early 1940s. At the outbreak of war, parts of University College, including the psychology department, were evacuated to Aberystwyth in Wales. Burt spent five years (1939–1944) in this remote spot on the Welsh coast, largely isolated from the center of affairs. This period of isolation affected him profoundly in several ways. It precipitated the breakdown of an unwise marriage he had contracted in 1932 with a student 26 years younger than himself, while the worries of the war situation led to a breakdown in his health. In particular, University College suffered serious bomb damage, and a great many of Burt's papers (his collection of children's drawings, case reports, and research data) were destroyed. In 1941, a few months after this devastating loss, he developed his first bout of Ménière's disease, a disorder which is now widely regarded as being precipitated by stress (Stephens 1975). He was to be handicapped by Ménière symptoms for the rest of his life, suffering severe attacks again in 1966. Though Burt preserved an outward equanimity he became more sensitive to criticism and more on the defensive. Particularly when under attack he began to display mildly paranoid traits, suspicious, devious, and self-aggrandizing. He fell out with many of his old students and former colleagues. After his retirement he was ordered, by the college authorities, to keep out of his old department, following his attempts to interfere with its running. In 1963, after long and bitter arguments with the British Psychological Society, he was deprived of the editorship of the statistical journal he had helped to found. These rebuffs aggravated his psychological condition. Beneath an apparently unruffled exterior Burt suffered from a whole range of psychosomatic symptoms, which he described fully in letters to his sister. His paranoid tendency became more marked. He was constantly engaged in controversy, going out of his way to pounce on those who disputed his views, and he became more

devious in his counterattacks. It is against this background that some of his later work must be judged.

Burt was 88 years old when he died on October 10, 1971, from carcinoma of the liver. He remained active till a few weeks before his death, and although his powers of concentration had begun to fall off and he was apt to make careless mistakes, his mind remained lucid and powerful up to the end. He was a person of remarkable erudition, with an extensive knowledge of psychology and its history, philosophy, theology, the physical and biological sciences, literature, and the arts. He acquired great technical skill in statistics and related branches of mathematics. He could expound his views with dazzling displays of scholarship and expertise, and as a critic and controversialist he could be devastating. Though distrusted by many, he managed, during his lifetime, to hold the fort against his attackers, and his reputation was not openly questioned. He remained until his death an eminent and still widely admired public figure. He had been knighted in 1946 for his services to education, and he acquired many honors both in Great Britain and abroad during the last 25 years of his life.

His downfall occurred not long after his decease. In May 1973, Leon J. Kamin of Princeton University, who had during the previous year spoken at various colloquia, delivered a paper in Washington on heredity and intelligence, which exposed numerous inconsistencies and contradictions in Burt's twin studies (Kamin 1973; 1974). Then, a year later, Arthur R. Jensen, a devoted admirer of Burt, examined Burt's kinship correlations in detail and was forced to admit that in spite of his theoretical mastery, there were serious shortcomings in his reported results (Jensen 1974). Two years later the medical correspondent of the British newspaper *The Sunday Times* maintained that in fact Burt "published false data and invented crucial facts to support his controversial theory that intelligence is largely inherited" (Gillie 1976). The matter rapidly became a *cause célèbre* and a center of controversy; one party upholding the fraud charge and maintaining that it seriously undermined the whole hereditarian position; others admitting that Burt may have been careless, but certainly not fraudulent, and arguing that in any case the hereditarian position was firmly founded on a mass of other evidence.

The twin studies, around which the contro-

versy centered, were commenced by Burt soon after he took up his post as London County Council psychologist. In particular, he was interested in collecting data on monozygotic (MZ) twins reared apart, as such twins provide highly important evidence for assessing the influence of the genetic component in intelligence. In his 1955 and 1956 articles (Burt 1955; Burt & Howard 1956) Burt claimed to have found 21 pairs of separated MZ twins. By 1966 (Burt 1966) the number had increased to 53 such pairs. Burt subjected this data, together with other kinship correlations, to an elaborate statistical analysis, using the methods of quantitative genetics developed originally by R. A. Fisher, and concluded that genetic factors contributed, in the case of intelligence, about eighty per cent of the variance, compared with only twenty per cent for environmental influences.

It was Kamin, who after a minute analysis of Burt's figures, revealed in them many strange anomalies. For example, the group intelligence test correlation of 0.771 for the separated twins was identical for the smaller population of 21 and the larger one of 53. And this was only one of a good many identical correlations with changed sample sizes. Kamin concluded that the absence of procedural description in Burt's reports, the frequent arithmetical inconsistencies and mutually contradictory statements, together with the improbable consistency of some of his results, rendered his work scientifically worthless. On the other hand, according to Jensen (1974) Burt had simply been careless in not reporting his findings more fully and accurately, but his conclusions were still valid. Even after the fraud charge had been leveled Jensen (1978) held that the peculiarities of Burt's kinship correlations were most reasonably attributable to carelessness rather than to deception.

An examination of Burt's diaries and correspondence, in the archives of the University of Liverpool, shows conclusively that Jensen's interpretation is untenable. Burt collected no twin data between 1955 and 1966; he was not in touch personally or by correspondence with the assistants, Miss Howard and Miss Conway, who were supposed to have helped him during these years; and he was incapable of collecting any data himself by reason of deafness and other infirmities. There was no ongoing research. Such data as Burt possessed were collected prior to 1950, and most of the original test sheets were lost during the war. The 1955 report was based on results abstracted into notebooks together

with a few cases recruited postwar; the 1966 report on data reconstituted from fragmentary remains, probably supplemented from memory and guesswork. From a scientific point of view the 1966 figures were a fabrication, since Burt no longer had the raw data. Indeed it is not certain that he had ever collected as many as 53 pairs of separated MZ twins. It was not until 1969 that in response to several requests Burt produced the table of intelligence quotient scores and social class ratings for his 53 MZ pairs reared apart. His diary suggests that he spent a whole week in concocting these figures, and he never was able to produce figures for the larger groups of MZ twins reared together, dizygotic (DZ) twins, and ordinary siblings. He put off inquirers by falsely alleging that the data were kept at college, and he could not provide them immediately.

Burt's motivation for this deception must be diagnosed in relation to the pressures to which he was subjected at the time. His work during the period of his retirement was mainly of a defensive kind, designed to uphold the Galtonian standpoint against mounting environmentalist and other attacks. These attacks came in two main waves, in the years 1953 to 1956, and again in 1963 to 1965. His two principal twin articles in 1955 and 1966, respectively, were his rejoinders. He wrote them in haste and anger, and with a determination to get the better of his adversaries by fair means or foul. There was a paranoid weakness in his personality which led him into these subterfuges.

These posthumous exposures, though they do not *ipso facto* undermine the whole hereditarian case, do largely destroy Burt's own scientific credibility. Burt, in fact, was never at heart a scientist. He was an able and ambitious man, who early came to regard the Galtonian tradition almost as gospel truth and himself as Galton's heir apparent. He was immensely erudite, industrious, and accomplished; he was, to those who were prepared to look up to him, kind and generous; and in his early years, up to the 1940s, he did valuable pioneering work in the field of applied psychology. But he could not take criticism or rebuffs; he could not brook opposition; and in the last resort he chose to cheat rather than see his opponents triumph.

L. S. HEARNshaw

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BUTTERFIELD, HERBERT

Herbert Butterfield was born into a devout Methodist family on October 7, 1900, in Oxenhope, an industrial revolution village with two thousand inhabitants at the edge of the moors and a few miles from the Lancashire–Yorkshire border. His father left school at ten to work as a wool sorter in the mill, but through self-education trained himself to become a bookkeeper.

He carried with him through life his frustrated ambition to have been a Methodist minister, and without pressing his son unduly, made it clear that he would like him to move in the same direction. It was partly in the knowledge of the pleasure it would give his father that Butterfield, from the age of 16, became a lay preacher, a step that provoked the impatience of the man who was his most stimulating teacher at the local grammar school, a man very outspoken in his hostility to Christianity. Later, when Butterfield was at Cambridge University working under the famous historian, Harold Temperley, his growing interest in history, and his consciousness of being too shy to succeed in pastoral work, helped to draw him away from the idea of going into the ministry. But whereas religious and political thinkers have characteristically rebelled against the tenets of their childhood, Butterfield has never wavered from a fundamental Augustinian theology that included belief in the infinite worth of human personality, the reality of sin, the sovereignty of God, the limitations of human nature, and the fragmentary character of human existence. He never espoused Marxism, as did Reinhold Niebuhr and Paul Tillich, two philosophers and theologians with whom he had some affinity. He studied Marxist thought, however, and extracted from it certain insights on conflict in history and the importance of economic and social factors in history. A threefold interest in European and British history, in the relations of Christianity to history, and in the theory of international relations determined the focus of his thought.

But for the formative influence of his childhood and family life, Butterfield might have remained content with the traditional historical scholarship expected from a professor of modern history at Cambridge and the editor of the *Cambridge Historical Journal*. He was educated at Cambridge where he earned the degrees of master of arts and doctor of literature. In 1923, he became a fellow of Peterhouse at that university, and then, in 1955, its master. He served as president of the Historical Association of England in the late 1960s and, as a historian, was invited to take up residence at the Institute for Advanced Study at Princeton, New Jersey. His first publication, *The Historical Novel* (1924), reflected his early concern with the relation between literature and history. He demonstrated the role of literary imagination in furthering the actual rediscovery of the past. It was an anach-

ronism, he believed, to assume that the events from another era could be understood in the context of the present or recent past. Shorter and more concentrated books in which the historian drew on literary if not poetical skills might help recreate the past, and this became the standard for almost all Butterfield's writing. He followed his first book with a detailed study, *The Peace Tactics of Napoleon, 1806–1808* (1929), an analysis of the relation between military and diplomatic tactics of the period, including the Treaty of Tilsit in 1807. Reviewers praised his historical portraiture and observed that he had disproved the claims of German historians that Czar Alexander of Russia had conspired to desert Britain; it was the Prussians who persuaded the Czar to meet with Napoleon on a raft in the River Nieman. He edited *Select Documents of European History, 1715–1920* (1931a), and began research on what was to be a lifelong interest in George III, eventuating in his *George III, Lord North, and the People, 1779–1780* (1949b) and *George III and the Historians* (1957). His intention to write the definitive work on Charles James Fox and George III was sacrificed to a new set of interests in analytical historical studies, which dominated his research for the next four decades.

The first clear evidence that Butterfield was to go beyond traditional historical research came in 1931 with the publication of *The Whig Interpretation of History*, a critical analysis of the Whig and liberal view of inevitable progress in history. His concern for historiography was clearly demonstrated in this study, which earned for him a reputation as an authentic historical genius. Although the book criticized liberal politics and historical Protestantism, Butterfield questioned in a more fundamental way the failure of historians to do justice to the unique conditions and mentality of bygone ages. It was evident in this work that Butterfield was to follow in the steps of J. E. E. D. Acton (even though he was a chief object of Butterfield's criticism) and Leopold von Ranke, and not that of traditional British historians who concentrated on what was primarily descriptive history. His subsequent works, *Napoleon* (1939), *The Englishman and His History* (1944), and *The Statecraft of Machiavelli* (1940), carried him even further along the path of the philosopher of history and the political theorist.

In the late 1930s and 1940s, Butterfield, whose eminence as a leading British historian was by then unquestioned, entered a second

phase of historical writing that was inspired by the world crisis. In 1939 he lectured at four German universities on the "History of Historiography," emphasizing the developments in the sixteenth and seventeenth centuries. He argued that it was the Whig historians whom he criticized and not the Whig politicians who had fostered freedom and moderation from the later years of Charles II. The lectures enabled him to say that although he still regarded the Whig interpretation as fallacious, he believed it had helped in the development of liberty in England. It was the Whig historians who had perverted the story and not the politicians who used political compromise and political persuasion rather than coercion and force.

In 1948 he turned to religion and history and delivered a series of seven lectures at the request of the Divinity Faculty of Cambridge University. He expanded these lectures into six broadcasts presented in April and May 1949 by the British Broadcasting Corporation. He amplified the major themes of these lectures into a book entitled *Christianity and History* (1949a). Butterfield had hesitated to undertake these lectures because he doubted that a layman was qualified to prepare them, and he knew the suspicion that such an enterprise could generate among his fellow historians. But it became difficult for him to avoid the invitation when the representatives of the Divinity Faculty made it clear that they did not feel that it would be most useful to have a clergyman—the undergraduates might be more ready to listen to a man who had made his name as a historian. His turn to philosophical questions was doubtless inspired by the dual crisis confronting the West: the formidable challenge of the Soviet Union with its Marxist creed and the position of the Western countries as "frightened defenders of the status quo, upholding the values of an ancient civilisation against the encroachments of something new . . ." (1949a, p. 5). Butterfield warned that the idolatrous worship of some superperson, "society," "state," or other large-scale organization could so transform man's perspective that he would see the world as comparable to the world of the ants. It was dangerous to bypass history or to imagine that the natural sciences could safely be left to shape human destiny. It was not enough to look for God or man in nature or to conclude with Hitler in *Mein Kampf* that since nature is concerned not with individual human lives but with the development of the species, history in-

evitably imposes its cruelties, idolatries, and human sacrifices. This attitude Butterfield described as "the facile heresy of the self-educated in a scientific age" and counseled: "Too easily we may think of man as merely the last of the animals and in this way arrive at verdicts which we are tempted to transpose into the world of human relations" (1949a, p. 6). *Christianity and History* reasserted the author's profound belief in the central place of human personality in the historical process. He protested against the opposing view of certain behavioral social scientists and naturalists that history is the story of great collectivities to be studied through science and mathematics as no more than another chapter in "the great book of biology."

Butterfield elaborated and extended this criticism and restated the Christian perspective in a succession of writings in which he claimed to speak not as a theologian but, as had Arnold J. Toynbee, as a historian. He wrote as an individual scholar, not interacting, as had Arnold J. Toynbee, with other historians and theologians who propounded an elaborate historical scheme. His aim, as he conceived it, was to challenge Christians and non-Christians alike to renew awareness of the place in history of the Christian view. He added books on *Christianity in European History* (1951a); *History and Human Relations* (1951b); and *Christianity, Diplomacy, and War* (1953). Whatever questions critics raised about his Christian perspective, Butterfield resolutely maintained that history seen in its broadest dimensions is consonant with a Christian view of history. He has inveighed against mere "technical history" which falls short of a subtle comprehension of the past. For him history at its core is a drama, oftentimes tragic in dimension, of human personalities. The tenets of historical scholarship require the historian to practice intellectual humility and flexibility of mind. He must walk alongside the actors in history, placing himself in their position, seeking to recapture their perceptions of events and to understand the problems with which they had to cope.

Another facet of Butterfield's contribution stems from his study of science. Beginning in 1931, he had lectured to Cambridge undergraduates on modern history from 1492, with attention to such larger developments and long-term movements as the Renaissance, the Reformation, the French Revolution, and their interconnections. His approach, which followed the example of Acton, was intended as a reac-

tion against the overspecialization that had come to dominate the university study of modern history with its use of "outlines" and dry textbook accounts of the external relations of states. Instead, he focused on the emergence and major developments of the modern world and modern thought, devoting, for example, approximately six lectures to the rise of modern science. These lectures brought him into contact with prominent Cambridge scientists such as Joseph Needham and the group at the Cavendish Laboratory, which was in search of an ally among historians. Under the influence of these people, Cambridge appointed a history of science committee in 1947, and it was this body which, after a considerable conflict, induced Butterfield to deliver in the Michaelmas term 1948 a course of lectures on the subject. The lectures led to the publication of his most successful book, *The Origins of Modern Science* (1949c), and to a reawakening of interest in the subject in England and America. It brought trained scientists into touch with serious historical study and helped produce a new generation of historians of science.

In 1953, Butterfield was asked to deliver the Wiles Trust lectures at Queens University, Belfast, and he chose to return to the topic of "History of Historiography" in the eighteenth and nineteenth centuries. The subject had been a preoccupation of German historians and a few American historians, but despite the efforts of Acton and his disciple, G. P. Gooch, who wrote *History and Historians in the Nineteenth Century* (1913), it had not taken root in England. The Wiles lectureship brought together Butterfield, who lectured on *Man on His Past* (1955), and ten European and British historians who, as authorities on different aspects of his subject, criticized and commented on his presentation. When published in 1955, these lectures led to a considerable reawakening of interest in historiography among university undergraduates, research students, and professors. Subsequently, he wondered if enthusiasm for historiography had gone too far and urged that universities limit its study to historians who were equipped by temperament and experience for the task. Although he was pleased he had been able to advance the work of Acton and Gooch, he believed that for the ordinary research student, training in the processing of evidence and the main techniques of research must come first.

Ten years later in 1965 and 1966, he was called on to deliver the famed Gifford lectures

at the University of Glasgow, a series in which Americans such as William James, John Dewey, and Reinhold Niebuhr had participated. Butterfield chose to address the broad question of how the human race had come to possess a concept of "the Past," and how and why mankind had been interested in its past before historical writing had begun. He speculated that early religion might have had some influence, but wondered if it might also have been the enemy of genuine history. The historical mentality, he concluded, had emerged gradually from the conflicting desire to reconcile both religious and secular history. He believed that conclusions based on recent research in Egyptian, Mesopotamian, and Jewish languages were adaptable to the kind of questions that Western historians characteristically studied. The lectures constituted his boldest historical adventure, for he undertook to survey the entire "History of Historiography." His effort inspired interest, particularly in the United States, but left him uncertain as to whether he had or could ever acquire the universal knowledge on which a published treatise on the subject should be based. Then in the mid-1960s the focus of Butterfield's interest shifted and he never returned in a systematic way to the theme of his Gifford lectures.

In all his historical writings, he has argued that at one level, historical interpretation calls for painstaking research and the ability to relive "the lost life of yesterday" using the accumulated traces of history "to recapture a bygone age and turn it into something that is at once a picture and a story" (1924, p. 8). In the pursuit of this goal, Butterfield was following the tradition he had absorbed from his teacher, the master historian of diplomacy, Harold Temperley, seeking always to write history of a high and complicated texture. At another level, the historian has the obligation to identify with his subject in order neither to praise nor blame him but, rather, to understand the circumstances confronting him. In studying the diplomatic interchanges that accompanied Napoleon's campaigns against the Fourth Coalition, Butterfield felt compelled to consult major archives in London, Paris, and Vienna, the correspondence drawn from the Prussian and Russian archives, and the reports of ambassadors, ministers, and spies to the leading statesmen of that day. His purpose was to mirror the thoughts of the major personages of the time and to uncover "the strange tangle, the hidden undercurrents and the clash of personalities that lay behind a Na-

poleonic war" (1929, p. vii). Even in the two studies of Napoleon written early in his career, the structure of Butterfield's maturing view of history was discernible. The unfolding story was for him unpredictable and wayward, based on the interaction of diverse personalities proceeding, not in accordance with predetermined doctrines of a superior people or of inevitable progress, or with consequences clearly linked to their intentions, but with goals and actions whose efforts are deflected by the mysterious workings of Providence.

Butterfield's essential ideas about historical studies matured and were well-defined before he was 31 years old. He had won the LeBas prize at Cambridge for his first book, *The Historical Novel*, but his third, *The Whig Interpretation of History* (1931*b*), established the foundations of his later works, with their underlying theme of "the complexity of human change and the unpredictable character of the ultimate consequences of any given act or decision of men" (1931*b*, p. 21). Historical writing must communicate the texture of such complexity. Historical change for Butterfield necessitated reform more than revolution, which inevitably left a legacy of hatred, human suffering, and destruction. In his early thirties, Butterfield was persuaded that the Christian interpreter, more than other political historians, was safeguarded against the worst illusions and idolatries, being prepared by his faith for accidents and surprises in history. By his worship of God, he was rescued from such distorting influences as a fanatical worship of the state, the idea of progress, or an abstract political ideology. By devotion to the ultimate ends of religion, the historian was enabled to understand "the web spun out of the play of time and circumstances . . ." (1931*b*, pp. 65-66). Providence held mankind under the judgment of God; yet God was not a tyrannical ruler but the source of grace in history. Even the clash of human wills reflecting man's pride and indestructible egotism could serve to further God's will and bring good out of evil. For example, the American Revolution had led the British to invent a new and more civilized concept of empire. Man's most creative achievements were usually born out of human distress, and inner pressures and political systems founded on brigandage, such as those of ancient Rome and the British Empire, might evolve in time in the direction of some tolerable measure of justice and order.

For Butterfield, therefore, historical studies

and theology cohere and reinforce one another, for both have their center in a concern for human personalities. His dual emphasis on history and religion, however, has opened him to criticism from which he might have been spared had he followed the course of traditional history. Some critics see in his religious writings a diversion from the historian's primary task of producing a large corpus of solid historical writing, such as might have resulted had he continued his research on Charles James Fox and George III. His position as scholar-statesman in the Cambridge University community, in which he lived and worked for more than a half-century, culminating in his appointment as vice chancellor from 1959 to 1961, was certainly dependent to some degree upon his recognized preoccupation with moral values. His positions at Cambridge and his leadership roles in national and international educational bodies drew him away from full-time scholarship, as did the series of endowed lectures, characteristically on religious themes, in Germany, the United States, and the United Kingdom. Peter Geyl and other historians have questioned his emphasis on the persistence of evil in man's behavior and of human cupidity in society and his low estimate of the capacity of honorable men to effect social change through actions with certain and predictable consequences. This criticism of his questioning of the consequences of the moral and rational intentions of statesmen overlooks his debate with such twentieth-century British historians as Lewis Namier and his disciples, who portrayed politics largely as a struggle for gain and selfish interests. For Butterfield, ideas, attitudes, and rational intentions interact with self-interest in politics, and historians err when they reduce politics for all individuals and every century to the same dull level. The Namier school, in so often assuming that politicians are no more than the repository of self-interest, misinterprets political movements and political parties, which do not merely advance group interests but also articulate values and ideals.

A more serious and partly legitimate criticism of Butterfield's use of the Christian perspective has been leveled at some of his historical judgments. In *Christianity and History*, written after World War II, he appeared to explain the defeat and destruction of Germany as divine punishment for the sins of the German people. Yet critics ask: What of the many Germans who at grave personal risk opposed Hitler's régime? And what of the Baltic peoples who did not com-

mit the brutalities of the Nazis, yet suffered as grievously as the Germans? Butterfield has written of the need for thinking historically at two different levels, the level of technical history, which deals with the limited and the mundane and takes into account hard and tangible historical evidence, and the level of Providence, which is beyond the reach of the technical historian. Critics have maintained that Butterfield, especially when he enters the realm of general history, smuggles into his interpretations the Christian points of emphasis and doctrine that he excludes from his more technical writings in narrative history, where he is more cautious in his assessments of individual leaders. Worst of all, he sometimes appears to speak for the Providence whose divine words he merely notes and records. In fairness, those critics who extract from his writings moral valuations such as his indictment of the entire German people contained in a wider discussion of militarism will find him going on to say that "if Germany is under judgment so are all of us—the whole of our existing order and the very fabric of our civilisation" (1949a, p. 52).

Finally, Butterfield's influence on Western thought is not exhausted in his two major contributions to traditional and Christian history. In 1958 he founded the British Committee on the Theory of International Politics and has served for approximately two decades as its chairman and honorary chairman. This group took as its mandate the making of an inquiry "into the nature of the international states system, the assumptions and ideas of diplomacy, the principles of foreign policy, the ethics of international relations and war" (Butterfield & Wight 1966, p. 11). From the start the committee's approach reflected Butterfield's historical and religious orientation. The interests of this group of British thinkers contrasted with those of American colleagues who formed a similar committee, and its work expressed greater concern "with the historical than the contemporary, with the normative than the scientific, with the philosophical than the methodological, with principles than with policy" (*ibid.*, p. 12). The themes of the group's first major publication mirrored Butterfield's own writings and included "Natural Law," "The New Diplomacy and Historical Diplomacy," "Society and Anarchy in International Relations," "Western Values in International Relations," and "The Balance of Power." Butterfield's closest associate in the venture, Martin Wight, introduced the

volume with a paper entitled: "Why Is There No International Theory?"

The British approach was the antithesis of that of flourishing schools in America and Australia that dealt with international relations theory and systems analysis. Its frame of reference was the conduct of diplomacy, international society, and the nation-state system. Its point of view was historical, empirical, and deductive. Its underlying presuppositions assumed that historical continuities were more important than innovations in the international system; that statecraft provided a historical deposit of accumulated practical wisdom; that the classical writers in politics, diplomacy, and law had not been superseded by recent findings in such disciplines as psychology and sociology, and that the corpus of earlier diplomatic and military experience was worthy of study and reformulation to meet contemporary needs. Undergirding the committee's work was a pervasive moral concern about which Butterfield wrote: "The underlying aim . . . is to clarify the principles of prudence and moral obligation which have held together the international society of states throughout its history, and still hold it together" (*ibid.*, p. 13).

The influence of Butterfield and the British Committee has been greater in the United States than in Britain. American interest in the group's approach coincided with a mounting awareness in the 1950s and 1960s, first by a handful of political realists and later by decision makers and journalists, that the struggle between the Soviet Union and the United States was too complex to define as a clash between absolute right and wrong. The conflict involved a profound moral predicament, for even if the two powers were approximately equal in strength and virtue, each could justifiably fear the other. Each could be sure of its own good intentions without being able to trust the other. Each could feel that its rival was withholding the one thing that could make it feel secure. Each side in a conflict could pursue its own security through displays and threats of power, yet overlook the fact that it could ensure its own security only by destroying the security of the other. For Butterfield the security-power dilemma was the most urgent problem of war and peace, and for him the only solution was for one or the other great power to risk something in the name of peace. The way out of the worst of deadlocks, he prophesied, was through some kind of marginal experiment; but for such an experiment, Amer-

ica would need the hard-level judgment of a hard-boiled Bismarck, not sentimentalists for whom giving way was always too easy. Butterfield made his proposal more than a decade before the Nixon-Kissinger policy of détente was proclaimed. Butterfield approved of this détente and added that what was needed for accommodation was a leader experienced in power politics and capable of bold and subtle, yet hard-headed, acts of political and military judgment. He doubted that either a conventional moralist or an ordinary intellectual or idealist in the White House or in Whitehall could succeed in formulating and defending a policy of coexistence.

In the United States, the response to Butterfield's ideas came from political realists. Although he can be viewed as a spokesman for realism and practical morality, Butterfield's realism has been tempered by his profession as a historian and by Britain's ancient tradition in foreign policy. Like Reinhold Niebuhr, he has had considerable influence on more thoughtful leaders in the religious community, and has quoted the Bible as often as he has historical texts, applying its wisdom to the realm of foreign policy. For Butterfield, the enduring contribution of Christianity to the requirements of international relations lies not so much in the choosing of actual policies but in providing a background of ideas or a more civilized spirit with which men can work. Christianity can help the citizen to formulate his ideas on human sin, to recognize that although extraordinarily evil men do exist, the most difficult problem in international politics is the moderate cupidity of large numbers of men who hope to realize through their nations what society denies them as individuals. Such men exercise vast pressures on governments and make normalized relations among states more difficult. It is the main responsibility of religion to check the sovereign will of the people who want to achieve their objectives by too great an exercise of power, instead of by consciously cooperating with Providence.

In his appeal to American leaders in statecraft and religion to recognize their limits and to exercise prudence, Butterfield, writing at the height of the cold war to a citizenry overanxious about the Soviet threat and imperiled by the risk of a thermonuclear holocaust, may have made his most lasting and valuable contribution.

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C

CANTRIL, HADLEY

Hadley Cantril (1906–1969), American social psychologist, received his B.S. in 1928 from Dartmouth College. After two years of study at universities in Munich and Berlin (1929/1930), he received his PH.D. from Harvard in 1931 and his LL.D. from Washington and Lee University in 1949. At the time of his death, Cantril was devoting his full energies to the application of the social sciences to the highest levels of government policy making. However, his personal commitment to the betterment of society, his belief in the social role of science, and his faith in the democratic process made that effort the culmination of a lifelong commitment. Of his 19 published books, 10, written during a span of 25 years, directly addressed this issue. His earlier work was restricted to the use of public opinion polling data, but his later books, remaining grounded in empirical research, drew on psychological concepts as a result of his development of the transactional approach to human behavior. The latter endeavor accounts for the remaining half of his scientific output.

Cantril's place in psychology and the social sciences grew out of the interplay of these two related themes. His belief that social science had value only if it could lead to the betterment of people's lives was linked to the proposition that this end could be achieved only by a social science that was theoretically and empirically sound.

Cantril held it "much more important to analyze crucial questions with whatever methods are available . . . than . . . to study trivial prob-

lems with precise methods" (1967*b*, p. 93). Throughout his career, he moved continually between applied and theoretical social science. He saw these not as two separate arenas but as aspects of a unitary process, and argued against the conventional separation of basic from applied research, of theory from practice, or of method from content.

At no point did Cantril even briefly abandon one interest for the other, although periods of different emphases divide his career into well-defined phases. From 1932 to 1946, his major efforts, including his dissertation (1932), concerned the development of the potential of polling techniques, with particular reference to public policy. In the years 1946 to 1955, he focused on the development of the transactional perspective in psychology, and the remaining 14 years of his life were devoted to synthesizing polling practice and transactional thinking into a comprehensive approach to global social and political realities.

In 1935, Cantril was invited to Princeton University, where he was to spend the next 34 years. He soon became a leading figure in the emergence of public opinion polling as a scientific endeavor of tremendous power. He pioneered in teaching opinion research as an academic discipline, participating in the establishment of the Office of Public Opinion Research in 1940, and four years later published *Gauging Public Opinion* (1944). In the prewar and war years, he advised the government on propaganda and policy matters through the interpretation of polling data. "Throughout all my Washington activities, I tried to avoid any publicity" (1967*a*,

p. vii), and most of his direct involvement at the government policy level at this time and later remained virtually unknown until the publication of the semiautobiographical *The Human Dimension* (1967a).

During this period Cantril also published a number of social psychological studies, most notably *The Invasion From Mars* (1940), *The Psychology of Social Movements* (1941), and *The Psychology of Ego-involvements* (Sherif & Cantril 1947). Although his interest in psychological theory is evident throughout these books, it was not until he coupled his long interest in the work of George Herbert Mead with the work of Adelbert Ames, Jr., whom he met in 1946, that he found the key to a way of thinking about psychological processes—the transactional point of view, which Ames had developed by combining his research in perception with the theories of John Dewey. Cantril adapted this perspective to meet the needs of social science, maintaining that “we do not ‘react to’ our environment . . . but ‘transact with’ an environment in which we ourselves play the role of active agent” (1967a, p. 17). Over the years he painstakingly elaborated this position into a comprehensive approach to psychology that received its initial expression in *The “Why” of Man’s Experience* (1950b) and was still being elaborated in terms of neurology and physiology at the time of his death.

Cantril’s interest in international social psychology remained active during this period, and was expressed in *Tensions That Cause Wars* (1950a) and *How Nations See Each Other* (Cantril & Buchanan 1953). In 1955 he established the Institute for International Social Research, to which he devoted his full energies thereafter. By then he believed he had both the methodological tools and the theoretical framework to permit a significant contribution to international understanding, a venture he advanced in a series of published and unpublished reports. Although always optimistic, he was toward the end dismayed at the slow acceptance of social science in government and politics.

Cantril’s lifelong interest in large-scale psychological theory and in global social relations was rooted in his concern for individual well-being and his belief in the unique potential of each human being. For him the human condition was expressed ultimately at the level of individual human feeling. His first published paper was a study of emotion (Cantril & Hunt 1932), and his last major paper, entitled “Sentio, Ergo

Sum,” asserted his conviction that “being is more reliably indicated by an awareness of feeling than by any rational thought” (1967b, p. 94). The 35 years that elapsed between these works can be seen as an attempt to systematize that position and apply it for man’s benefit.

It may be too soon to evaluate the success of that effort. During his life Cantril was occasionally attacked and often ignored by much of established social science. His appeal and ultimate influence were primarily with students and younger professionals who characteristically sought new ways of thinking, and for whom Cantril always had time and words of encouragement. He was fond of quoting Einstein to the effect that it is futile to try to convince one’s colleagues, and, true to that belief, he functioned largely outside the mainstream of American psychology. Yet psychology increasingly bears closer resemblance to Cantril’s thinking than to that of his critics. The concept of transaction has become commonplace in social science writing, although few think to acknowledge its originators or Cantril’s anticipation—prior to 1950—of the intellectual currents of the late twentieth century.

WILLIAM H. ITTELSON

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CARMICHAEL, LEONARD

Leonard Carmichael was born November 9, 1898, in the Germantown section of Philadelphia, Pennsylvania, and died September 16, 1973, in Washington, at the age of 74. At the time of his death, he was vice president for research and exploration of the National Geographic Society. Elected to the National Academy of Sciences in 1943, he served as chairman of its section of psychology from 1950 to 1953. Earlier honors included election to the American Academy of Arts and Sciences in 1932, and to the American Philosophical Society in 1942.

A physiological psychologist, Carmichael also had a flair for administration, serving as chairman of the Brown University psychology department from 1927 to 1936; as chairman of psychology and dean of faculty at the University of Rochester, 1936-1938; as president of Tufts University, 1938-1952; as secretary of the Smithsonian Institution, 1953-1964; and as vice president of the National Geographic Society, 1964-1973.

Carmichael was the only child of Thomas Harrison Carmichael, a successful physician, and Emily Henrietta Leonard Carmichael, who was active on many volunteer charitable boards. At the time of her death, she was chief of the Philadelphia Bureau of Recreation. His maternal grandfather, Charles Hall Leonard, D.D., LL.D., was for many years dean of the Crane Theological School of Tufts University.

Although his parents were not Quakers, Carmichael attended the Germantown Friends School. He entered Tufts University in 1917, and graduated in 1921 with a B.S. As a senior

he carried out a small research problem on the embryology of the eye muscles of the shark; it aroused his interest in the significance and evolutionary history of sense organs. His interest in sensory psychology and physiology as determinants of an organism's behavior was to become the dominant scientific theme of his career. At Brown, Rochester, and Tufts, he was director of the laboratory of psychology and sensory physiology. At Rochester, as professor and chairman of the department of psychology and dean of the Faculty of Arts and Sciences, and at Tufts, as president, he maintained an active and direct involvement in the research of these laboratories.

The books that influenced him most as an undergraduate were those of Jacques Loeb, the biological ultramechanist, and C. Lloyd Morgan, the proponent of emergent evolution. After reading Howard C. Warren's *Human Psychology* (1919), however, he decided that psychology, rather than anatomy or physiology, was the discipline in which he could best study the senses with a view to their functional role in behavior as well as in the biological setting.

He entered Harvard University as a graduate student on a fellowship provided by the educational psychologist Walter F. Dearborn, with whom he had an especially close association. Early in his graduate work, he participated in rebuilding an improved model of the famous Dodge-Deardorn eye movement recording camera. Carmichael was encouraged to satisfy his interest in biology as well as psychology. He took a number of courses in zoology, and his first piece of graduate laboratory research was a quantitative study of the reaction of the meal worm (*Tenebrio molitor*) to light. G. H. Parker, a professor of zoology, guided this work. Carmichael regarded Parker's lectures on the nervous system and the sense organs as models of clarity and scholarship. In psychology his teachers included E. G. Boring, L. T. Troland, and William McDougall.

Carmichael's PH.D. thesis, a theoretical and historical dissertation on the psychology and biology of human and animal instincts, was published under the title "Heredity and Environment: Are They Antithetical?" (1925a). In reviewing the literature on this subject, he reported accidentally discovering Wilhelm T. Preyer's work on signs of life in the fetus. He saw this approach as the way to study the morphological growth of receptors and the nervous system in relation to behavior at various stages

of early ontogenetic development in mammals before learning begins or is important.

In 1924, after receiving his PH.D. and completing a Sheldon Traveling fellowship, he joined the faculty of Princeton University to teach physiological psychology and history and systems of psychology. At Princeton he began his research on behavior development with larval amblystoma and frog tadpoles. He found that their physical development proceeded normally even after they had been immobilized with a mild concentration of the anesthetic chloretone. Here was an ideal situation, in which, presumably, all sensory input was reduced and all motor movement, clearly, inhibited, so that no practice (i.e., learning) was possible. He observed that when the anesthetic was removed, the experimentally treated organisms swam vigorously in as coordinated a manner as did the undrugged controls, who had been allowed to move throughout development. As he states in his autobiography: "These studies supported a hereditary rather than an environmentalistic theory of the determination of the growth of organized behavior. At the time, the results of these experiments surprised and almost shocked me. They did not support my then strongly held belief in the determining influence of the environment at every stage in the growth of behavior" (1967, p. 37). As a consequence, his reports of these experiments in *Psychological Review* (1926; 1927; 1928) seemed to dodge the obvious conclusion with his reiteration of the intimate interrelation of heredity and environment and the difficulties of disentangling their interaction.

In 1927 Carmichael moved to Brown University. Still in his twenties at the time of his appointment, he was one of the youngest full professors on the Brown faculty. Although he became busily involved in organizing the new laboratory and department, equipping it for graduate training and research in experimental and physiological psychology, at the same time that he continued his own research, he personally taught undergraduate and graduate courses and guided the research of undergraduate and graduate students. Carmichael was a clear and dynamic lecturer; his elementary course was extremely popular and students voted him the most popular teacher a number of times.

At Brown University, Carmichael achieved his long-cherished goal of studying the development of behavior in fetal mammals. His own preparatory work and that of others had shown

that fetal neural function rapidly deteriorated if circulation was occluded. He began with the study of the fetal cat and developed an especially designed cradle in which the pregnant mother cat could be supported while a heated trough filled with saline solution received the fetus, its fetal circulation intact, after a Caesarean section. In order to prevent the complicating effects of the anesthesia under which the preliminary surgery was performed, he devised a way of sectioning the mother's spinal cord in the cervical region. A kind of spinal anesthesia without drugs, it rendered her motionless and kept pain and other sensory input from reaching her brain. The fetus could then be studied in a nearly normal physiological state. James Coronius and Harold Schlosberg participated with Carmichael in the first study of the fetal cat. Motion pictures were taken and verbal records of behavior descriptions were dictated. After Carmichael's marriage to Pearl L. Kidston in 1932, she assisted in many early experiments, especially in transcribing and keeping records.

Carmichael's most extensive studies of development were performed on fetal guinea pigs, focusing on responses in more than 100 cutaneous pressure reflexogenous zones throughout the entire active prenatal period of 68 days. Carmichael remarked that it was not the nature of the physical stimulus so much as the fact that it was above the threshold of some skin receptors in a specific locus that determined the response. Many typical patterns of behavior remained amazingly constant in the rapidly growing organism. Yet growth could suddenly alter responses in such a way that they could easily be confused with learned responses, especially if such changes had been seen in postnatal life. He noted in "The Experimental Embryology of Mind" (1941):

I have never seen any responses in the late fetus which, in their elements, have not appeared as a typical patterned reaction to isolated stimuli many times before. In the late guinea pig fetus the hair coat is well grown, the teeth are erupted, eyes and ears are functional, and adaptive integrated behavior is well established. At this time such an animal will, to use the language of teleology, attempt in a most effective and even ingenious way to deal with a tactual stimulus applied to its lip. First, it may be, it will attempt to remove the stimulus by curling the lip; then, if the stimulus remains, it is brushed by the forepaw on the stimulated side. If the stimulus still persists, the head is turned sharply. Finally, a general struggle is

resorted to which involved movements of all four limbs and all trunk muscles. In a late fetus this final maneuver is sometimes so quick and effective that the experimenter is often thwarted and the offending stimulus is removed—by a guinea pig fetus that is having its own willful and annoying way in spite of anything the experimenter can do. . . .

Complex patterns of behavior emerge as a result of maturation. Such behavior is possibly as truly end-seeking and purposeful as is any behavior in the world which does not involve the use of language. I see no reason to believe that this emergent purposeful behavior is not as natural a result of the processes of growth as is the length of the fetal whiskers, and quite as independent of learning. (pp. 16, 17)

His findings on the development of sensorimotor responses in mammalian fetuses brought home dramatically the natural sequence of maturation and the role of genetic factors in behavioral development. But he was careful to avoid the broad generalizations about the course and nature of developmental sequences to which some workers were prone. The classic work of W. T. Preyer and G. E. Coghill on the sequence of motility in the developing amphibian larvae had shown the first movement to be a C-shaped or reversed C curvature, followed by an S or sigmoid form of reaction. Coghill (1929) had characterized the S reaction as a reversal of flexure before the earlier C flexure was completed, thus producing the sinuous behavior of the total organism. The entire sequence, when speeded up, was in fact the larval swimming movement. These movements appeared before the limbs developed. Although in this organism the forelimbs developed before the hindlimbs, both sets of limbs moved only as part of the larger trunk movement. Gradually, however, independent limb action began to individuate out of the dominant trunk movements. First the forelimbs, then the hindlimbs, showed a certain autonomy. Trunk movement in walking was regarded as nothing more or less than swimming movement at a reduced speed. Development was a progressive expansion of a perfectly integrated total pattern from which the parts—i.e., partial patterns—individuated with various degrees of discreteness.

Carmichael saw something different in fetal mammals. He gave more importance to the early individuation of certain quite specific responses that later became parts of integrated behaviors. Rather than debate the issue of holistic *vs.* particulate development, Carmichael cautioned that

the researcher would do better to record as unambiguously as possible the responses made by a fetus at any stage, rather than fit all developmental changes into one formula.

Carmichael began his experimental studies at a time when the American biological and psychological community was not familiar with the advances in ethology that documented the releaser function of patterned stimuli in eliciting species-specific responses. The regularity of these species-specific behaviors, and their occurrence in animals reared in environmental isolation with restricted postnatal experience, had led Konrad Lorenz and Nikolaas Tinbergen to argue for their instinctual basis. In much animal behavior occurring under natural circumstances, such “releaser stimuli” were often perceptually complex, a characteristic coloring and size of an egg, particular location and size of a red bill spot, even a sequence of movements by another animal.

Psychologists as a group are cautious in attributing behavior patterns to genetically determined processes or propensities. In the past, however, some have used this facile explanation to account for behavior whose full ontogenetic history and experience had not been adequately documented. With time, increasing interaction between students of animal behavior and psychologists has produced a sounder appreciation of the role of genetic determinants in behavior, both in their own right and as setting the stage upon which experience and learning can interact. Carmichael's work on the development of behavior and its sensory control was in a sense premonitory of changing views on the heredity–environment issue. His first two editions of the *Manual of Child Psychology* ([1946] 1954), and a third edition (1970) under Paul H. Mussen's editorship, are witness to Carmichael's unflagging interest in this subject. He was less able to devote his energies to such research during the Tufts years, which overlapped with the World War II effort. The Laboratory of Sensory Physiology and Psychology at Tufts turned to war-related projects that included the improvement and application of new techniques to the study of eye movements and visual fatigue. Electronic recording, rather than ocular photography, proved more suitable for long-time studies of reading fatigue, an old interest from his days with Dearborn. To this method of registration could be added the simultaneous registration of brain waves, the electrical signs of oscillatory neural activity in different brain

regions throughout the reading, and other visual tasks. Earlier, at Brown University and the Bradley House, he had pioneered with H. H. Jasper some of the first electroencephalographic (EEG) registrations of brain waves in humans and animals in the United States (Carmichael & Jasper 1935).

With his call to the Smithsonian secretaryship, Carmichael not only turned his administrative talents to modernizing that institution, which included the new Museum of Sciences and Technology, but found some opportunity to pursue his interest in behavioral development. He informed the superintendent of the Washington Zoological Park that he wished to be present whenever a birth was imminent among any of its exotic animal species. He reveled in observing the new-born yak or giraffe struggle to its feet and in relatively short order display coordinated, though awkward, motor patterns. He had become extremely interested in developmental studies of primates, and indeed served as first president of the Primatological Society. Later, at National Geographic, he was proud of its support of Jane Goodall's original and pathbreaking field studies of chimpanzees in their natural habitat.

Although Leonard Carmichael was active also in national affairs and in scientific and educational domains, this memoir has focused on his contributions in the study of behavioral development. His memberships, officerships, awards, and distinctions, too numerous to recount, include 23 honorary degrees, the presidential citation of merit, the public service medal of the National Academy of Sciences, orders of merit from four foreign countries, fellowships, trusteeships, and a legion of responsibilities and duties of distinction. His honorary degree citation from Harvard called him "a psychologist who combines distinction in his science and success in administration."

CARL PFAFFMANN

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CARNAP, RUDOLF

Rudolf Carnap, the noted philosopher of science, logic, and language, was born in 1891 at Ronsdorf, near Barmen, Germany. He studied mathematics, physics, and philosophy at the universities of Jena and Freiburg-im-Breisgau before and after service in World War I, and received his doctorate in philosophy at Jena in 1921. Appointed a lecturer at the University of Vienna, he soon became, after Moritz Schlick, the leading exponent of logical positivism (later, logical empiricism)—the philosophy of the Vienna Circle. With Hitlerism threatening to overwhelm all Europe, Carnap, a humanist socialist in his sympathies, emigrated to the United States in 1936. There he taught philosophy for many years, first at the University of Chicago

and then at the University of California at Los Angeles. He died in California on September 14, 1970, at the age of 79.

Education. After having been taught at home for several years by his mother, a former teacher, Carnap entered the Gymnasium at Barmen. Here he was early drawn to mathematics because of its precise concepts and rigorous proofs. This concern for concept and proof continued throughout his life. His university studies at Jena and Freiburg-im-Breisgau centered first on mathematics and philosophy, shifting later to philosophy and physics.

At Jena, Carnap read Kant with Bruno Bauch and some Hegel with Hermann Nohl. But the event that most shaped his future career was his chance enrollment in the fall of 1910 in a seminar conducted by Gottlob Frege. In 1879, Frege had published a little book, *Begriffsschrift*, which founded the modern science of symbolic or mathematical logic. Now, a generation later, his great work still virtually unknown, he remained an obscure associate professor of mathematics at Jena, lecturing to a handful of students. The 1910 seminar, on the *Begriffsschrift*, gave Carnap his first view of "real logic"; he returned for another course with Frege in the spring of 1913, and again in the spring of 1914.

Meanwhile, in 1913, Carnap had begun doctoral work in experimental physics, but progress was slow ("I was certainly not a good experimenter"). When World War I broke out ("for me an incomprehensible catastrophe"), Carnap, nonpolitical but vaguely antimilitarist like most of his associates, nonetheless felt it his duty to go to the front. He served there until transferred to Berlin in the summer of 1917 for military research in physics. At this time, Carnap began to study the ideas and theory of the socialist workers' movement. Thus he, with his friends, was able to welcome the German revolution, and the Russian revolution before it, as a "liberation from the old powers." (But "in both cases, after a few years, we saw that the promised high ideals were not realized.")

In 1919, Carnap resumed his studies at Jena and Freiburg, this time in the philosophy of physics. His doctoral dissertation was a highly technical essay in conceptual analysis in which he sought to show that major controversies about the nature of "space" arise from a failure to distinguish among three meanings of the term: the formal or logicomathematical, the intuitive (here described in semi-Kantian terms),

and the physical. The dissertation was approved by Bruno Bauch in 1921 and was published a year later as a supplement to the influential journal *Kantstudien*, under the title *Der Raum: Ein Beitrag zur Wissenschaftslehre*.

During this period, Carnap continued to work in logic. He made a thorough study both of the massive *Principia Mathematica* (1910–1913) of Alfred North Whitehead and Bertrand Russell, to which Frege had referred in his lectures, and of Frege's own writings. Late in 1921, he turned to Russell's *Our Knowledge of the External World as a Field for Scientific Method in Philosophy* (1914), in which Russell had urged philosophers to follow Frege's example and use the "logico-mathematical method of philosophy." Carnap later wrote: "I felt as if this appeal had been directed to me personally." He at once set about an intensive study of all of Russell's writings on epistemology and scientific methodology. Soon thereafter he began a major work on the logical analysis of empirical concepts in the spirit of Russell's program.

In the early 1920s Carnap, who had been working in comparative isolation, started to develop relations with other scholars in his field. Among them was another young physicist turned philosopher, Hans Reichenbach, with whom Carnap remained in very close touch until Reichenbach's death in 1953.

The Vienna Circle years. In 1922, Moritz Schlick, a widely respected German epistemologist and philosopher of science, had accepted the prestigious chair in the philosophy of the inductive sciences at the University of Vienna. He quickly gathered around himself a group of scientists and philosophers, including Hans Hahn, the mathematician, and Otto Neurath, the sociologist-economist. In 1924, two of his students, Herbert Feigl and Friedrich Waismann, suggested that he form a regular discussion group in the philosophy of science. From these Thursday evenings was born the famed Vienna Circle.

Carnap had met Schlick through Reichenbach in 1924 and a year later was asked to lecture to the Vienna Circle. In 1926, he and Reichenbach became the leading candidates for appointment as instructor in philosophy at the University of Vienna. Schlick and Hahn, facing a difficult choice, finally recommended Carnap.

Life in the Vienna Circle was ideal for Carnap. Here was a spirit of free and undogmatic inquiry; here philosophy was practiced not in

the traditional mode of speculative metaphysics but as the painstaking, informed logical analysis of the concepts of the sciences; here too was an atmosphere of cultural and political enlightenment. Years later he recalled the five year instructorship as "one of the most stimulating, enjoyable, and fruitful periods of my life."

And fruitful it was. The manuscript of his Russell-inspired work, which he brought with him to Vienna, was discussed avidly and in detail in the circle. In revised form, it appeared in 1928 as *Der logische Aufbau der Welt*. Exploiting the logical apparatus forged by Frege and Russell, Carnap offered in the *Aufbau* a logical reconstruction of the concepts used in empirical science in terms of one or two concepts drawn from immediate experience. But he warned against construing this as a solipsistic "ontological" reduction of all "reality" to immediate personal experience. Here, and in a small monograph published the same year, and indeed throughout his career, he insisted that science is metaphysically neutral—and philosophy should be. Moreover, the only genuine issue that lurks behind such metaphysical pseudoissues as, say, realism versus idealism, is the problem of choosing a suitable language or conceptual framework for science. And this is a matter to be decided on practical grounds, not theoretical ones.

By 1929, Carnap had become a leader in the circle second only to Schlick. That year he published a text on symbolic logic; coauthored with Hahn and Neurath the circle's programmatic declaration *Wissenschaftliche Weltauffassung: Der Wiener Kreis*; joined Reichenbach in founding the new journal *Erkenntnis*; and helped organize the Prague Conference on the Epistemology of the Exact Sciences.

Two years later, Carnap accepted a chair in natural philosophy at the German University in Prague. Keeping in touch with the circle through frequent visits, he now focused his own research on language. His initial motivation is reflected in the title of a 1932 paper, "The Elimination of Metaphysics Through the Logical Analysis of Language." This research led to his second major work, *Die logische Syntax der Sprache* (1934), which laid the foundations for a new discipline, the study of the logical syntax of formal or constructed languages. Stimulated by the ideas of Alfred Tarski, the Polish mathematician, Carnap then extended his studies from the logical syntax to the semantics of formal languages.

Meanwhile, the Vienna Circle had begun to take on the aspect of a movement, "a new movement in European philosophy." Visitors had come to Vienna from Germany, Poland, Finland, the United States, and England. In Prague, Carnap was sought out by such younger American philosophers as W. V. Quine of Harvard University and Charles Morris of the University of Chicago. In 1934, he gave a series of lectures at London University, which became his first book published in English, *Philosophy and Logical Syntax* (1935). It was on this trip too that he first met his mentor, Bertrand Russell.

But another movement was also spreading. Nazism, having come to power in Germany, was preparing to absorb Austria and Czechoslovakia. The German University at Prague had become a center of Nazi agitation. There was no place on the continent for a German of Carnap's outlook to live and work. Thus he, like most of his fellow members of the Vienna Circle, found it necessary to emigrate. With his wife Ina, he left Prague at the end of 1935. In September 1936, through Quine, he was invited to the Harvard Tercentenary Celebration as a participating scholar, and was awarded an honorary sc.d. degree. With the help of Morris, he obtained a teaching post that fall at the University of Chicago.

Teaching in the United States. Carnap taught at Chicago from 1936 to 1952, with several leaves for visiting professorships and for research. He soon had many colleagues, old and new, with whom to discuss logical and philosophical problems. Feigl had come to the United States in 1930 and was teaching at Iowa State University. Reichenbach, after a stay in Turkey, was appointed to a professorship in 1938 at the University of California at Los Angeles. Carnap himself arranged in 1938/1939 for Carl G. Hempel and Olaf Helmer to join him as assistants.

There were many memorable experiences. Russell came to Chicago in the fall of 1939 as a visiting professor. During Carnap's own visiting professorship at Harvard in 1940/1941, Russell delivered the William James lectures the first semester and Tarski spent the full year there. Soon a regular discussion group was formed, with Russell, Tarski, Quine, and Carnap as the most active participants.

Carnap's books, monographs, papers, and reviews were now appearing in rapid sequence, especially from 1942 on. That year he published

his *Introduction to Semantics*, and a year later his *Formalization of Logic*. In 1947, he published *Meaning and Necessity: A Study in Semantics and Modal Logic*.

In the early 1940s, Carnap, prompted by Feigl, had begun to shift his attention from deductive logic and semantics to the problems of probability theory and inductive logic. The year 1950 saw the publication of his treatise *Logical Foundations of Probability* (as well as his influential paper, "Empiricism, Semantics, and Ontology," in which he developed and refined his 1928 thesis of metaphysical neutrality). From 1952 to 1954, he continued his researches on probability at the Institute for Advanced Study in Princeton, New Jersey. There he had a number of lively, enjoyable discussions with Albert Einstein.

In 1954, Carnap accepted an appointment at the University of California at Los Angeles, succeeding his friend Reichenbach. Carnap taught at the university until 1962, when he retired, as he wrote Russell, "to devote myself to the further development of my theory of inductive probability."

Originally, Carnap had intended his 1950 treatise on probability to be the first of a two-volume work. But his ideas on the subject had changed materially during the decade that followed. Instead of a second volume, he finally worked out a plan for a large group of papers to be written by himself and such colleagues and former students as John Kemeny, Richard C. Jeffrey, and Chaim Gaifman, and to be published from time to time as *Studies in Inductive Logic and Probability*. Delays set in, and volume 1 of *Studies*, edited by Carnap and Jeffrey, did not appear until 1971, some months after Carnap's death. It contained, among other contributions, his 130-page monograph, *A Basic System of Inductive Logic: Part I*. He was working on the final revisions for *Basic System: Part II* and had almost completed them, when, following a short illness, he died in September 1970.

"A towering figure." Carnap was a brilliant, dedicated scholar, who wrote—and spoke—with extraordinary lucidity and rigor. So much so, indeed, that, as Hempel put it:

To observers who did not know him closely, Carnap may well have appeared as intellect and rationality incarnate, as a powerful thinker moving in a theoretical realm far distant from the domain of human passions and hopes and fears; but this picture does not do him justice. Carnap formed very strong human

bonds; he was a warm-hearted and loyal friend, a most interested and sympathetic listener, and a broad-minded observer of the human scene. . . . (1971, p. xviii)

Although not a political activist in the usual sense of the term, Carnap took a keen, informed interest in the great issues of the day. In a letter (1962) congratulating Russell on his ninetieth birthday and signed "with deep affection and gratitude," Carnap wrote: "I am in complete agreement with the aims for which you are fighting: serious negotiations instead of the cold war, no bomb-testing, no fallout shelters. But, not having your wonderful power of words, I limit myself to participation in public appeals and petitions initiated by others and to some private letters to President Kennedy" (Russell 1969, p. 133).

Characteristically, he closed his "Intellectual Autobiography" (1963) with these lines:

Thus one of the main problems, perhaps the most important and most difficult one, after the terribly urgent problem of the avoidance of atomic war, is the task of finding ways of organizing society which will reconcile the personal and cultural freedom of the individual with the development of an efficient organization of state and economy.

It was also in keeping with his character that in January 1970, just a few months before his death, he made it a point to visit two Mexican philosophers, De Gortari and Molina, held as political prisoners in Mexico City. On his return, he sent a report to friends and colleagues, describing his visit and urging support for the release of the two men—one more expression of his profound commitment to justice and freedom. As W. V. Quine wrote: "Carnap is a towering figure. I see him as the dominant figure in philosophy from the 1930s onward, as Russell had been in the decades before" (1971, p. xxii).

ALBERT E. BLUMBERG

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CARR, E. H.

Edward Hallett Carr was born June 28, 1892, in north London, the son of a small manufacturer whom he has described as belonging to “the middle middle class.” He was educated at the Merchants Taylors’ School and in 1911 went on scholarship to Trinity College, Cambridge. His

contacts with Trinity’s most celebrated dons, such as Alfred North Whitehead, A. E. Housman, James G. Frazer, and Bertrand Russell were limited, but he earned a first in the Classics Tripos and university prizes in Latin epigrams, Latin poems, and Greek translation.

Had World War I not intervened, Carr might have become a classics don at Cambridge; instead he spent twenty years as a diplomat. Such accidents were to shape his intellectual journey for more than sixty years. One year after joining the Foreign Office, he was, in his words, “shifted onto the Russian Revolution.” By accident he was present at the Paris Peace Conference and later served as assistant adviser on League of Nations affairs in the Foreign Office. These experiences led him to try, in *The Twenty Years’ Crisis* (1939), to unravel the tangled web of international politics. During World War II he was assistant editor of *The (London) Times* and its chief leader writer; he exercised quiet influence in preparing the way for a Labour government in 1945. *The Times* in this period was referred to as the “Thrupenny” *Daily Worker*, and conservative critics identified Carr as a member of a band of *étatist* radicals. The two world wars, perhaps World War I especially, led to the erosion of his faith in the established values of liberal England and to his search for a world different from the conventional one in which he had been raised. He followed, not the route common to intellectuals in the 1930s—of flirtation with Leninism—but one that led to a discovery of the intellectual world of nineteenth-century Russia and the writings of F. M. Dostoevsky and M. A. Bakunin. Twice he returned to university life: in 1936 when he became Wilson professor of international politics at the University College of Wales at Aberystwyth, leaving the Foreign Office (acknowledging that he had stayed too long) to have greater freedom to write about the Russian revolution; and again in 1953 when he returned to Cambridge to take up what would be a quarter century of research on the history of Soviet Russia, an effort culminating in fourteen books arranged in ten volumes (1950–1977). His return to Cambridge came not because he was asked to leave *The Times*, but because, at age 52, he was determined to return to writing his history of Soviet Russia and its dual fascination with “otherworldliness” and the capacity for planned social change. As a journalist, he had been denounced in the House of Lords as “an active danger,” and Randolph Churchill in 1946 had charged that

Carr "had been mainly responsible for *The Times* becoming, apart from the Communist *Daily Worker*, the main British apologist for policies of the Kremlin." Carr had influenced *The Times* to support the Beveridge Report and postwar planning efforts that bore a resemblance to experiments in Soviet Russia. Having been identified in the United States with "hard line" policies toward the Soviet Union, he was second secretary from 1925 to 1929. His critics charged that he led *The Times* to "soft line" policies, although Carr has said that this criticism became significant only in his last year with *The Times* as the cold war set in.

Carr's intellectual contribution will ultimately be judged less by his journalism or his diplomacy than by his theoretical writings on international relations and by his *History* (1950-1977). The latter, though representing prodigious scholarship, is the more difficult to assess. As a historian of Russia, Carr represents no clearly identifiable school of thought. He has written contemporary history on a highly volatile subject not yet sufficiently removed in time to permit dispassionate scholarship. He has emphasized the broad economic, social, and political aspects of the Russian revolution without attempting much historical judgment of its more brutal and controversial aspects, which recede into his broader perspective. His volumes, whose publication spread over 28 years, reflect the shifts of British attitudes toward the Soviet Union. (Carr acknowledged that he would approach the first volume differently if he were writing in the late 1970s, stressing constitutional arrangements less and emphasizing the social and economic environment in which the new Soviet state operated.) Thus his *History*, written from three to six decades after the revolution, stands as an ambiguous historical classic, marred by its lack of passion and historical imagination, too cautious in its moral judgments for some critics and too defensive of collectivist social change for others, but accepted by all for its persistent scholarship on the first ten years of Soviet rule. The genius of the work is Carr's success in placing himself in the roles of leading Soviet statesmen, confronting the reader with the choices these men faced and enabling future generations thereby to walk alongside Russian policy makers despite the wide gulf of ideology and culture separating them. No other historian of the Soviet Union has achieved quite the same empathy with Soviet leaders and their tasks. None has made Carr's physical and intellectual investment in a painstaking

survey of a broad sweep of history utterly critical for understanding the contemporary Soviet Union.

In his career Carr combined the professions of political scientist and historian with those of journalist and man of action. He joined the Foreign Office as temporary clerk in 1916 and more than two decades later, while serving as assistant editor of *The Times*, wrote anonymous editorials on the need for a better understanding of Russia and its role in the postwar world. Although frequently at odds with the prevailing approach of the British foreign policy establishment, he served on various study commissions. He held diplomatic posts in Paris and in Riga, Latvia, was assistant adviser on League of Nations affairs, and first secretary to the Foreign Office. He also served as director of foreign publicity in the British Ministry of Information. He wrote extensively on intellectual and diplomatic history of the nineteenth and twentieth centuries and on the philosophy of history. He contributed biographical studies of Bakunin, Dostoevsky, A. I. Herzen and Albert Sorel, and theoretical works on international politics, as well as the monumental ten-volume history of Soviet Russia, which is comparable to Theodor Mommsen's history of Rome.

Carr's major theoretical contribution to international studies has been to lay the foundations for what is called "political realism." *The Twenty Years' Crisis, 1919-1939* was published in 1939, as the rise of Hitler and the outbreak of World War II shattered the illusions of the interwar period. Expressing the euphoria of their era, statesmen like Woodrow Wilson and William Howard Taft in the United States and David Cecil in Great Britain had heralded the rise of new forms of international relationships that were modifying the ancient and destructive patterns of national rivalries and power politics. Intellectuals and scholars within and outside leading universities had dedicated themselves as much to the reform as to the study of the existing international system, a trend prompting Carr to assert that international studies were becoming encumbered by utopianism, half truth, and ignorance. Foreign policies were judged in terms of categories of good and evil—good nations and bad nations, good internationalism and bad nationalism. "Heretic and renegade" to the prevailing establishment view, Carr as the editor of an *Ambassadors at Large* series wrote in an introduction to the volume on France: "In international politics, few of us have got be-

yond the stage of the small child who says, 'You *are* naughty' to anyone who does something it doesn't like; for the temptation to impute moral turpitude to policies which do not suit our interests is almost irresistible" (Ormesson 1939, p. iv).

Carr explained that *Crisis* was written "with the deliberate aim of counteracting the glaring and dangerous defect of nearly all thinking, both academic and popular, about international politics in English-speaking countries from 1919 to 1939—the almost total neglect of the factor of power" ([1939] 1946, p. vii). In explaining this neglect he concluded that the science of international politics was still in its infancy. Every science, Carr said, passes first through a utopian phase in which purpose gives to analysis its initial impulse and direction. In the biological sciences, the desire for improved health led to the emergence of medical science, while the necessity of roads and bridges ushered in the science of engineering. In this first phase, the element of wish or purpose is overwhelmingly strong and that of analysis of facts weak or even nonexistent. For political science, the impulse in studying war comes from a desire to cure a profound illness of the body politic, and whereas the basic scientist in his laboratory is also often several, or many, steps removed from the aim of eradicating a disease—e.g., cancer—and his emotions are irrelevant to his research, the political scientist in his research is never far from the urgent social need his inquiry is intended to serve. Indeed the ends of social research themselves become determining factors: the student orders and rearranges the data on which his conclusions are based—as do economists who seek to preserve or defend capitalism, or Marxists, however "scientific," whose analysis of capitalism becomes inseparably joined with the goal of supplanting or overthrowing that system. The social position and moral and political purpose of the observer inevitably shape and affect his research and give both meaning and direction to analysis and interpretation.

In the second phase, realism in the social and political sciences succeeds utopianism, and what is distinguished from what should be. Realism demands both the acceptance of facts and a search for their causes and consequences. By 1932 Winston S. Churchill had observed that never had "the gap between the kind of words which statesmen used and what was actually happening" been so great. Carr undertook to

diagnose and criticize utopianism and found that its weakness lay in its proponents' attempts to carry over the principles of nineteenth-century liberal rationalist thought from homogeneous national societies to the currently existing heterogeneous and half-anarchic international order. The League of Nations and other experiments in collective security were efforts to translate principles and institutions that had achieved relative success within particular nation-states into universal principles for the world of international affairs. Underlying these enterprises was the belief in a natural harmony of interest among nations, derived from the doctrines of laissez-faire economics in the nineteenth century. The passionate desire to eliminate war determined the first phase of international studies, and criticism of the means proposed to achieve it was branded mischievous or destructive. "The advocate of 'collective security,'" wrote Carr, "replied to the critic . . . by a statement that it must be made to work . . . or by a demand for some alternative nostrum" ([1939] 1946, p. 8). The concept of a natural harmony of interests, fundamental to the idea of a community among nations, conflicted with the division of the world into satisfied and unsatisfied, or have and have not, nations and the need for hard bargaining on conflicting interests. Both power and interest remained centered in independent sovereign nation-states, and conflicts resulted not only from a failure of understanding of national leaders but from the clash of incompatible goals and aspirations that could be accommodated, not by a priori rational principles, but only through compromise and diplomacy. International politics lacked objective and disinterested moral or legal standards for resolving conflicts.

Thoroughgoing realist that he seemed to be, Carr nevertheless recognized that realism was not enough. He observed: "Consistent realism excludes four things which appear to be essential ingredients of all effective political thinking: a finite goal, an emotional appeal, a right of moral judgment, and a ground for action" ([1939] 1946, p. 89). Utopian and realist thinking were both needed throughout history, and they existed in dialectical relationship with one another, the need for one or the other being greater in a particular era. Having demolished the cornerstone of the utopianism of the interwar period, Carr acknowledged the necessity of a new international vision for the future. Such a vision had to be free of illusions, and he warned a young

assistant lecturer: "I hope you will . . . steer clear of SUNFED [Special United Nations Fund for Economic Development], ECOSOC [Economic and Social Council] and all those other horrors which have no substance in them" (Abramsky 1974, p. 179). Carr saw the period between the wars as having called for greater emphasis, among defenders and foes of the status quo, on the need for peaceful change, based on what was just and reasonable, and reflecting shifts in international power relationships. Carr felt that those who were the major beneficiaries of a particular status quo (France and England between the wars) had to prepare themselves for sacrifice to the less satisfied powers. A successful foreign policy, therefore, had to oscillate between the poles of force and appeasement. The Munich settlement of 1938 had represented changes in both the distribution of power within the European balance of power and in the view of justice expressed in the principle of national self-determination. Critics noted that Carr's general principle calling for relating power and morality was unexceptionable, but that its application to the Munich settlement showed him surrendering to "the immanence power." He had himself fallen victim to a relativistic conception of morality to which he had been led by his concept of values being conditioned by the power and status of the nations espousing them.

Carr followed his major theoretical work, *The Twenty Years' Crisis*, with a more topical analysis of the international crisis, *Conditions of Peace* (1942). In it he repeated the warning that the democracies were trying to meet the world crisis with ideas and institutions carried over from the nineteenth century. The book was already in press when the attack on Pearl Harbor brought the United States into World War II. Germany and Japan were at the height of their power as Carr prepared the manuscript; thus he added an explanatory note upon its publication, apologizing for the tentative nature of his policy conclusions. He had argued that Soviet Russia and Nazi Germany held the key to the future because they had virtually eliminated unemployment by instituting planned economies. The privileged status quo powers, with the possible exception of the United States, idealized the past while minimizing the advantages of a collectivist approach to economic problems. They suffered from a preoccupation with security and privilege and clung to laissez-faire capitalism and national self-determination while the Germans and the Russians tried to build a world of

larger units under centralized planning and control. Hitler, whom Carr depicted as a twentieth-century Napoleon, "consummated the work, which Marx and Lenin had begun, of overthrowing the nineteenth-century capitalist system" (1942, p. 9). Carr prophesied that even if Hitler were overthrown, nineteenth-century German capitalism could not be restored any more than the downfall of Napoleon had restored feudalism. The idealists of the English-speaking world courted disaster by placing themselves "in opposition to the new world revolution which first broke through the crust of the existing order in the Bolshevik Revolution of 1917" (1942, p. 7).

When the peace settlement of 1919 had increased the number of weak and struggling states in the name of national self-determination and left the economic order of central and eastern Europe in an enfeebled state, the victorious Allies by this action had in effect lost the peace. It was beside the point to argue that the Versailles treaty had been too vindictive or not vindictive enough. Soviet Russia and Nazi Germany had won the peace because their leaders had understood the contemporary revolution and made giant strides toward recovery through economic planning, while the victors had remained helpless spectators and prisoners to nineteenth-century thinking. Political parties in the democratic countries represented powerful established interests, and the will of the unorganized majority remained impotent against the might of organized economic power. Democracy's survival depended on the redefinition and reinterpretation of the majority's rights and the discovery of a common moral purpose powerful enough to generate self-sacrifice by the strong in the interests of the weak. Communism, much like Christianity, had discovered its ground for action and its finite goal in a higher purpose. Carr prophesied: "The cooperation between the Western peoples and Soviet Russia in the war should help to resolve the antithesis, incidental rather than fundamental, between the secular ideals of Christianity and those of communism" (1942, p. 121). He hoped that the democracies might profit from the lessons of communism's moral and economic successes.

In *Nationalism and After* (1945), which appeared at the end of World War II, Carr examined the far-reaching changes between nineteenth- and twentieth-century nationalism, principally the extension of political participation within nations to include new social groups, the visible reunion of economic and political

power, and the beginnings of a vast proliferation ultimately quadrupling the number of nation-states following the war. He warned of the bankruptcy of nationalism resulting from the carnage of two world wars and the coming obsolescence of the nation-state, which could no longer provide military security or economic well-being to its citizens on an autonomous basis. A new international order was necessary to transcend the destructive consequences of rampant national self-determination, but it could not be established by constitutional design. It must rest rather on some new enterprise or common effort, such as the world-wide quest for social justice, which comprised for Carr equality of opportunity, freedom from want, and full employment. While acknowledging the unlikelihood of world-wide cooperation, he found that the best means of achieving these goals lay in large multinational and regional groupings and joint planning on programs of full employment or assistance to backward areas. In the aftermath of the age of nationalism, Carr suggested, European cooperation for social justice, with Britain participating, might offer an alternative to the Soviet ideology of state monopoly and the American ideology of unrestricted cooperation.

In *The Soviet Impact on the Western World* (1946), and in his monumental *The Bolshevik Revolution* (1950–1977, vols. 1–3) Carr turned explicitly to a study of the Soviet challenge to the West. In his biography of Marx he had described his subject as “the protagonist and the forerunner of the whole twentieth-century revolution of thought” (1934, p. 302). Reflecting certain continuing Marxist presuppositions, he declared in *The Soviet Impact*: “The missionary role which had been filled in the first world war by American democracy and Woodrow Wilson had passed in the second world war to Soviet democracy and Marshal Stalin” ([1946] 1947, p. 3). Soviet democracy was an outgrowth of Western democracy; just as the Cromwellian and Jacobin dictatorships had given birth to political democracy, the Soviet dictatorship of Lenin and Stalin and the Russian proletariat had created social democracy. The first impact of Soviet democracy on the West was to stimulate concern for “the common man”—a challenge “western democrats will be well advised to ponder” ([1946] 1947, p. 19). The achievements of the Soviet planned economy helped prepare the ground for Keynesian economics in the West. In foreign policy, the Soviets openly acknowledged the power factor as the determinant of policy freed of the ideo-

logical disguises of League of Nations statesmen. They also perfected the use of propaganda as a normal instrument of foreign policy, but Carr looked elsewhere in explaining the impact of the Soviet challenge. He observed, “the gravamen of the Marxist revolution is . . . that it has called in question the moral authority of the ideals and principles of western democracy by declaring them to be a reflexion of the interests of a privileged class” ([1946] 1947, p. 94). The age of individualism that had prevailed from 1500 to 1900 was drawing to a close and represented little more than an oasis between the totalitarianism of the medieval church and empire and modern collectivist societies. Two world wars, revolutions around the globe, and far-reaching social and economic upheavals had altered the moral climate and convinced “all but the blind and the incurable that the forces of individualism have somehow lost their potency and their relevance in the contemporary world” ([1946] 1947, p. 111). The nature of the Soviet threat, therefore, was predominantly moral, not military, and “security is and will remain” the primary factor in the Soviet policy toward Europe. “Nothing in the Russian tradition supports a policy of military action in Europe beyond the eastern zone, . . . [which is] . . . for the Soviet Union today what the Monroe Doctrine is for the United States, the Low Countries for Great Britain, or the Rhine frontier for France” ([1946] 1947, pp. 107, 108). A far more important and likely threat is the penetration of the West by ideas from the East, and he warned that “the danger for the English-speaking world lies . . . in its relative lack of flexibility and in its tendency to rest on the laurels of past achievements,” rather than in its searching “for new forms of social and economic action in which what is valid in individualist and democratic tradition can be applied to the problems of mass civilization” ([1946] 1947, p. 113).

Any assessment of the enduring value of Carr’s contribution to international political theory must be qualified and tentative at this stage. By demolishing utopian beliefs in the natural harmony of interests of states, Carr was among the first to restructure the study of international politics in the name of political realism. Making use of the intellectual tools of the sociology of knowledge, he demonstrated the relationship between national values and ideals and the interests and power of states. His claim that international disputes were more likely to be resolved through hard bargaining that recognized con-

flicting national interests than through international processes analogous to national judicial or legislative actions has survived the test of time. (Carr's analogy between great power negotiations and collective bargaining in industry is more debatable, given the lack of moral consensus in international relations.) The defects of his contribution to these issues stem more from polemical overstatement than from untruth. His critical analysis of the limitations of Western political thought continues to have value. The legacy of his history of the Bolshevik revolution helps explain the actions and motives of Soviet leaders and the wellsprings of Soviet policy.

The question remains, however, how so major a thinker could have gone astray so grievously in his practical judgments in the interwar and postwar period. He found in Neville Chamberlain the exemplar of the moral realist and in the Munich settlement the paradigm of a political settlement based on principle and power. He linked Hitler and Mussolini uncritically with Mustapha Kemal, Józef Pilsudski, and Antonio de Oliveira Salazar as leaders in the revolt against liberal democracy. He felt no need to make moral distinctions between Wilson and Stalin or between the crusade for American and what he called "Soviet democracy." He could write: "There is . . . no essential incompatibility between democracy and dictatorship" ([1946] 1947, p. 11). These errors in judgment appear not accidental but rooted in a more fundamental weakness in his political philosophy.

Four explanations are possible for the shortcomings of Carr's philosophy, and all four have roots in the apparent philosophical weakness of his approach. First, his Marxist orientation (he once remarked that he was more truly Marxian than uncritical and dogmatic Marxists who asked no questions of their ideology) drove him, in the spirit of Hegel and Marx, to search for values within and not outside the historical process. As a result, he lacks an objective basis for practical moral judgments. Second, and related to the first explanation, he appears repeatedly to have equated superior morality with superior power. Moral reasoning is for him not a weighing of good and evil or of competing *goods*, but exclusively the means for "an escape from the logical consequences of realism" (1939, p. 118). Critics, with this weakness in mind, have asked whether he lacks an understanding of the essential nature of morality. Third, Carr, who has relied on the inspiration of Reinhold

Niebuhr in other respects, says little about proximate morality and provides few examples of prudential judgments of contending political movements. (Niebuhr, by contrast, wrote in the early 1940s that if men could not make moral distinctions between democracy and Nazism, then moral evaluation of any type was in practice impossible.) Fourth, Carr, having affirmed the need for a standard of judgment of politics, has had no transcendent point of view from which to appraise the phenomenon of power. In *What Is History?* (1961) he seeks a philosophy of history to supply such a viewpoint but concludes there is no possibility of discovering an objective standard outside history. The closest approximation for him is to state:

The absolute in history is not something in the past from which we start; it is not something in the present, since all present thinking is necessarily relative. It is something still incomplete and in process of becoming—something in the future toward which we move, which begins to take shape only as we move towards it, and in the light of which, as we move forward, we gradually shape our interpretation of the past. (1961, pp. 160–161)

If this definition is Carr's clearest formulation of an objective standard, he must clarify whether he means that the only basis for judging history is history itself or that what survives in history is *ipso facto* good. It is a question by which earlier historians have been judged and found wanting. One critic of Carr concludes: "It is a dangerous thing to be a Machiavelli. It is a disastrous thing to be a Machiavelli without *virtù*" (Morgenthau 1962, p. 43).

To this criticism Carr has responded that certain moral absolutes do exist: liberty and equality, justice and democracy, but as broad categories of thought almost devoid of meaning or application unless given specific content. They are blank checks that are valueless unless and until they specify how much liberty is to be allocated to whom, and whom we recognize as equals. How a given people fills in such checks is a matter of history. The content given these values in the nineteenth century was undermined in World War I. After that war, which revealed the bankruptcy of liberalism, Carr has argued, only one of two responses was possible: the establishment of socialism or the establishment of conservatism. He has opted for socialism in part because he is a man of the left and in part because socialism represented the optimism of change; and he has staunchly arrayed him-

self against those who find change menacing and the source of doubt, gloom, and fear. At the age of 86 he declared:

When Sir Lewis Namier warns me to eschew programs and ideals, and Professor [Michael] Oakeshott tells me that we are going nowhere in particular, and Professor [Karl] Popper wants to keep the dear old Model T on the road by dint of a little piecemeal engineering, and Professor [H. R.] Trevor-Roper knocks screaming radicals on the nose, I shall look out on a world in tumult and a world in travail, and shall answer in the well-worn words of a great scientist: "And yet—it moves." (Scott 1978, p. 8)

KENNETH W. THOMPSON

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CATTELL, RAYMOND B.

Under Sail Through Red Devon (1937b) may seem a curious title for a book by a young psychologist dedicated to the multivariate study of human personality. Yet it reflects the depth of the roots of Raymond B. Cattell, born in 1905, in his British homeland and the Devonshire countryside. The relative social and economic homogeneity that he knew as a child was to stand in marked contrast to the broad range of conditions that he later found when he entered the University of London at age 16. The city's extremes of wealth and poverty reawakened a social concern that had been incubating since his days as a young hospital helper during World War I.

Shortly before receiving his degree in physics and chemistry three years later, Cattell decided that psychology would be his life's work. Although he was still registered at King's College, Cambridge, it was C. E. Spearman, in whose laboratory at University College Cattell spent most of the next five years, who had the most profound influence on his professional development. He saw in Spearman's factor analysis a tool that could be applied to the study of behavior and that might yield results with an objectivity and replicability rivaling those of the physical sciences.

Cattell received his doctorate in 1929 from King's College, but the economic climate of Great Britain offered bleak prospects for a research career in the emerging science of psychology. To support himself, Cattell accepted a teaching position at Exeter University and there began formulating his research program. With Spearman's retirement and emigration to the United States, he turned to Cyril Burt for critical discussion of his substantive and methodological ideas. It was at this time that Cattell decided to leave academic life in order to gain clinical experience as the director of school psychological services for the city of Leicester. This experience helped to crystallize his views on issues of personality, and the population of Leicester was the subject of his first major research effort. Noting a tendency for children of lower

intelligence quotients (IQ's) to come from larger families, Cattell researched and published *The Fight for Our National Intelligence* (1937a). Although his prediction of a slow decline in the average intelligence of the population was not realized in a subsequent investigation, Cattell's concern with possible dysgenic effects on complex behavioral traits was to remain a primary social concern reflected in his later work.

Despite his prolific writing, the early years in England were frustrating because support for basic research in the behavioral sciences was practically nonexistent. Thus, he accepted Edward L. Thorndike's invitation to be a research associate in his laboratory at Columbia University for a year, although it meant uprooting himself from his beloved England. At Columbia, Cattell found an innovative and pioneering style of attack that he incorporated into his own research strategy. He remained in the United States, first as G. Stanley Hall professor at Clark University, later moving to a lectureship at Harvard University. With the advent of World War II, he joined the U.S. Adjutant General's Office, where he was offered the opportunity to continue his development of "objective" measures of personality that might prove useful in officer selection. Through these experiences Cattell recognized the efficiency to be gained when a small group of researchers gathered to attack a problem of common interest from a variety of perspectives. This he envisioned as the optimal research setting.

With the war's end, Cattell accepted a research professorship at the University of Illinois, soon to become preeminent in the area of multivariate analysis under the leadership of Herbert Woodrow. There he established his Laboratory of Personality and Group Analysis, which was to attract 77 research associates and numerous other social and behavioral scientists in its 37-year history.

Cattell considered Spearman's *The Abilities of Man* (1927) a definitive work in that area and had set as his own goal and that of his laboratory the systematic study of the realm of human personality. Two considerations defined the general framework within which the work of the laboratory would proceed. From his background in the British school of psychology, with its greater emphasis on biological influences on behavior, Cattell viewed behavior as essentially organic in nature, with an as yet undefined structure. Further, the complexity of human behavior required that the methodology employed

allow for the investigation of multiple causation. Spearman's factor analysis, with its further development by Godfrey H. Thomson, Cyril Burt, and L. L. Thurstone, appeared to offer the only reasonable approach to elucidating this structure. Thus, first priority was given to the specification of the relevant underlying dimensions of personality. The development of specific measures and the establishment of their reliability and validity, of necessity, assumed a secondary position.

A limited number of principles, derived from Cattell's philosophy of the manner in which the objective investigation of personality should proceed, may be identified. Each has had a major influence on the substantive productivity of the scientist.

One of Cattell's major contentions was that theory should derive from research, which in turn would suggest the appropriate direction for further empirical investigations. The search for structure and a theoretical model that would accommodate the data thus became an iterative procedure. He recognized that factor analysis did not lead to a unique solution except under the most simplistic of conditions, and he saw the pitfalls of overemphasizing the results obtained from a single sample. Thus, his empirical efforts required careful cross validation as both instruments and theories were refined. Researches in personality, motivation, social psychology, and abilities each became programmatic, with later work building on and extending what had gone before. As a result, his more complete and refined statements of theory do not appear in the literature until the mid-1950s.

A second major principle guiding Cattell's research was that of catholicity. This requirement expressed itself in two ways. First, if the dimensions that were identified as the primary source traits of personality were indeed the salient variables, then it should be possible to demonstrate parallel structure across measuring instruments. These included peer ratings, already in wide usage, personality questionnaires, which were undergoing rapid development, and objective tests or miniature behavior samples. Cattell pioneered the development of this latter approach, with work beginning in England and culminating in the publication of descriptions of some four hundred measures (Cattell & Warburton 1967). The second dimension on which he expected to be able to demonstrate the universality of the source traits was across cultures. His organic conception of personality specified these

traits as ingrained characteristics of mankind. Thus, local populations might differ with regard to mean values on the dimensions, but the interrelationship of the elements defining the traits should remain unchanged. Cross-cultural studies of the structure of personality and of abilities became an essential feature of Cattell's work and were undertaken as opportunities arose.

Cattell's commitment to factor analysis grew from both his desire to identify multiple causation and the necessity to remove the subjective element from the determination of structure as much as possible. This commitment was further stimulated and enhanced by his association with Thurstone whose *Multiple-factor Analysis* (1947) laid out the principles and procedures with which Cattell had become so familiar. As with any research program, theoretical development or empirical verification often reached an impasse because appropriate statistical methods to answer the most pressing questions were lacking. At these junctures, Cattell would turn his attention to the further development of the factor analytic model or the innovative application of existing procedures. These efforts led to numerous developments, including his "scree" test for number of factors; the development of a similarity coefficient for comparing factor analytic solutions; with his wife, Karen Cattell, a method for the unique rotation of two-factor analytic solutions derived from proportional correlation matrices; the factoring of repeated measures on single individuals to study fluctuating personality states; and others. His efforts in the development of a theory of personality fathered many methodological innovations and could not have proceeded without them.

Finally, Cattell's interest in ultimate causation led him into occasional forays into human behavioral genetics. The result was his development of the method of multiple abstract variance analysis for genetic analysis of sib data, obtained from all combinations of identical and fraternal twins and full and half sibs, raised together and apart.

Cattell's pursuit of a comprehensive theory of behavior through factor analytic methods has produced a variety of theoretical models and psychometric instruments. His theoretical developments in the measurement of personality by questionnaire are embodied in the 16 PF (personality factor) (Cattell, Eber, & Tatsuoka 1970), the High School Personality Questionnaire (Cattell & Cattell 1975), and other tests

for different age ranges. The study of personality structure in objective test devices has produced the Objective-Analytic (O-A) Battery (Cattell, Hundleby, & Pawlik 1965) and the High School O-A (Cattell & Schuerger 1978). Closely related has been the study of state patterns with the subsequent development of modulation theory to account for short term perturbations in measured traits, reflected in his work with Scheier, Curran, and Nesselrode. Excursions into the study of motivation led to the dynamic calculus, developed with Child, the Motivation Analysis Test (Cattell, Horn, Sweney, & Radcliffe 1964) and a school version of the same. Cattell's Culture Fair Intelligence Test (Cattell 1950a) is a direct descendent of his theory of fluid and crystallized intelligence, designed to measure the former. His work on the dimensions of cultures has been picked up and pursued further by political scientists and most notably by Rudolf J. Rummell. Finally, his efforts to integrate these areas of behavior into a comprehensive framework has produced *Comprehensive Personality and Learning Theory* (1978a).

It is not surprising that such dedication to a methodological approach, coupled to the development of theory, should have generated some criticism from behavioral and social scientists. The most persistent have dealt with the psychometric properties of the measuring instruments and the incomplete mathematical development of Cattell's methodological innovations. Both might be more understandable in the context of the disparate aims of the theoretician and the psychometrician. For the former, cross validation through the repeated demonstration of similar structural patterns in the developing instruments is the *sine qua non* of his endeavor. Although some satisfactory level of reliability and validity must be present for replicable patterns to be demonstrated, these levels need not necessarily be those most desired by the applied behavioral scientist. Given limitations on time and energy, some degree of compromise is inevitable. Whether the course that has been chosen will affect the utility of the measures produced remains an empirical question.

It may be decades before Cattell's impact on the social and behavioral sciences can be adequately determined. Certainly the methods and directions that he pioneered will continue to be explored by his colleagues and students, and by the members of the Society for Multivariate Experimental Psychology, which he founded. In

the meanwhile, with the increasing methodological sophistication of social and behavioral scientists, the familiar refrain of many introductory texts in personality, that "Cattell's methods are too complex for presentation here," may no longer be necessary.

THOMAS W. KLEIN

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CHAMBERLIN, EDWARD H.

Edward Hastings Chamberlin (1899-1967) was born in La Conner, Washington, May 18, 1899, the second son of Fred Hastings and Irene (Dagan) Chamberlin. His father, a minister of the Methodist Episcopal Church, died while he

was still in grammar school. Soon afterwards his mother moved to Iowa City, Iowa, where she worked to put the two Chamberlin boys through high school and the University of Iowa. While at the university, young Chamberlin contributed to his and his family's support by writing for the *Iowa City Citizen* as well as for the university's student newspaper.

On graduating from the University of Iowa, Chamberlin began graduate study in economics at the University of Michigan. His interest in economics had no doubt been sparked at the University of Iowa by the distinguished American economist Frank H. Knight, who was later to move to the University of Chicago and become recognized as the precursor of what is popularly identified as the "Chicago School" of economic thought, known for its contributions to the theory of the firm and the operative mechanics of market economies. It is likely that Knight's forceful writings and teachings, to which members of the school are said to owe their parentage, also shaped Chamberlin's interests. At Michigan, Chamberlin also studied under Leo Sharfman and Fred Taylor. Sharfman was an authority on railroad rate structures and Taylor a distinguished analytical economist concerned with the functioning of firms in a socialistic economy. After taking his M.A. degree at Michigan in 1922, Chamberlin moved on to Harvard University, where he completed his doctoral thesis in 1927. Except for a year as distinguished visiting professor at the University of Paris (1950/1951) and wartime service with the Office of Strategic Services (1943-1945), he was to spend the rest of his life at Harvard. In 1924 he married Lucienne Foubert; they had one daughter.

While at Harvard he wrote his thesis under the well-known economist Allyn Young, who at Cornell University had had Frank Knight among his students. Both Young and Knight had written and lectured widely on the theory of the firm and the relationship among rival firms in the market place. Chamberlin's intimate colleagues have observed: "It is now possible with hindsight to see that Chamberlin's thought may have emerged from reflections on these ideas and problems—but only if hindsight is very acute" (*Harvard University Gazette*, March 23, 1968).

Chamberlin wrote many articles and several books in the broad area of inquiry known as microeconomics, but the single work that clearly defines his distinction among the economists of

the world is his seminal *The Theory of Monopolistic Competition* (1933), an outgrowth of his doctoral thesis. Although the writings of a few economists published shortly before the publication of *Monopolistic Competition* had, at least in some sense, anticipated fragments of Chamberlin's essential theory, the work clearly represented an innovation of impressive magnitude. Classical and neoclassical microeconomic theories had been erected on the proposition that markets were *either* monopolistic *or* competitive. Accordingly, the received economic wisdom consisted of a theory of perfect monopoly and a theory of perfect competition. The essential thrust of Chamberlin's work was that since neither of these theoretical conditions was often found in the real world, prevailing theories of the firm and markets were largely irrelevant. The mission of the theory of monopolistic competition was to bring microeconomic theory into conformity with the conditions of the business world to which it pertained.

As a general proposition, Chamberlin contended, each individual business firm had some control over its price. In fact, the attainment of this control was the obvious objective of such familiar firm activities as advertising, product differentiation, the search for superior business locations, and so on. In most cases, however, these business strategies fell far short of establishing the degree of control over price that conventional microeconomic theory ascribed to monopoly. Rival firms, it must be borne in mind, were engaging in similar strategies. Hence, while each firm's *limited* control over its market bestowed on it some finite element of monopoly, the strategies of its many rivals limiting this control bore a strong resemblance to the normative models of competition. The resulting Chamberlinian model of *monopolistic competition*, though built upon elements of the existing models of monopoly and competition, emerged as a new explanatory hypothesis of the firm that was more in keeping with the observable conditions of the real business world.

In his systematic and meticulous analysis of the shortcomings of existing models of the firm leading up to the full-blown development of his own model (chapters 1-3), Chamberlin presented a searching review of the theories of markets in which firms were few in number and found them equally wanting. Antoine Augustin Cournot, the early nineteenth-century French economist and mathematician, had developed a model of a two-firm industry (duopoly)

in which a determinate equilibrium solution rested on the condition that each firm assumed its rival's output to be fixed (1838). The model was readily adaptable to the few-firm case (oligopoly). While other scholars, notably Bertrand (1883) and Edgeworth (1881), had pointed out the lack of realism in the Cournot model, only Chamberlin sought to develop a new model overcoming its logical flaws.

The essence of Chamberlin's oligopoly model (chapter 3) springs from his observation that, when a firm makes a move that affects the fortunes of its rivals, that firm is obviously refusing to look beyond its nose if it assumes its rivals will not react. For example, if firm A reduces its price to obtain larger sales, and the market consists of only a small number of firms, this move will obviously reduce the sales of its rival B, forcing it to react—to take the countermeasure of reducing its price. Since A should reason beforehand that B will retaliate, it will make the initial move *only* if both A and B would be better off after B reacted. In time, a determinate equilibrium solution to oligopoly must postulate that all firms will take into account the *indirect* as well as the *direct* effect of their actions. In the special case where the number of firms is quite small, the final equilibrium solution would be very close to that of monopoly.

Chamberlin's new theories had immediate impact on both the formal structure of microeconomic science and its implications for public policy. College textbooks, at least those of the more discerning authors, were quickly enlarged to accommodate chapters on monopolistic competition. This impact would have been explicitly attributed exclusively to Chamberlin had it not been for the appearance of Joan Robinson's *The Economics of Imperfect Competition* (1933) shortly after the publication of *Monopolistic Competition*. For some years, in spite of the vast differences in substantive content of the two volumes, members of the economics profession tended to lump them together as the joint foundations of the new microeconomics. Although their substantive difference came gradually to be recognized, the profession's tendency at first to treat them as quite similar theories was an obvious source of injury to Chamberlin's professional pride, and prompted him to devote much scholarly energy to distinguishing between them. In the preface to the fifth edition he lamented that "it has been unfortunate that two theories as divergent in their interpretation

of economic phenomena as Mrs. Robinson's and my own should have been identified in the minds of so many." To clarify the differences he added a new chapter in that edition, "The Difference Between Monopolistic and 'Imperfect' Competition." In all, the book went through eight editions, each of which improved the orderliness of the presentation and refined the theoretical construction.

There are eminent scholars whose fame rests on a major contribution and whose professional lives are devoted to elaborating and perfecting it. There are also those who explore most salient aspects of their discipline. Chamberlin belongs in the first group (see *Harvard University Gazette*, March 23, 1968). The eminence of his contribution is attested to by the many empirical studies of firm behavior and markets that it inspired, most designed in one way or another to test some tenet of Chamberlin's theory, and by the honors bestowed upon him. He received honorary degrees from the Università Cattolica del Sacro Cuore in Milan (1949), from the Sorbonne, University of Paris (1951), and from Boston College (1954). He was vice president of the American Economic Association and a member of the American Academy of Arts and Sciences and the Real Academia de Ciencias Economicas y Financieras of Barcelona. When the International Economic Association devoted a conference to monopoly and competition in 1952, it called upon Chamberlin to give one of the principal papers and to edit the resulting volume, *Monopoly and Competition and Their Regulation* (1954). At Harvard he occupied the prestigious David A. Wells chair in political economy, was chairman of the department of economics from 1939 to 1943, and was editor of the *Quarterly Journal of Economics* from 1948 to 1958. The capstone to this stream of honors came with his election in 1965 as distinguished fellow of the American Economic Association and the publication in 1967 of *Monopolistic Competition Theory: Studies in Impact; Essays in Honor of Edward H. Chamberlin*, under the editorship of Robert E. Kuenne. The central theme of that volume of essays was anticipated in the citation that accompanied his election as AEA fellow—which singled out the immense impact that *Monopolistic Competition* had on economic thought in the author's lifetime: "It is not given to many scientists to reach into the minds of all their fellows and to influence the work of a whole generation, but the author of *The Theory of Monopolistic Com-*

petition did so. The nature of products, the character of interproduct competition, the accommodation of selling costs in price theory, and the theory of oligopoly have all undergone major change because of Chamberlin's great work."

The volume of essays appeared, unfortunately, almost exactly at the time of Chamberlin's death, and the manuscripts made available to him reached him while he was in the final stages of a complete paralysis that had gradually set in after a cruel stroke in 1961. The authors, perhaps as distinguished a group of economists as were ever represented in a single volume, weighed Chamberlin's impact on microeconomic theory in the United States and abroad. Kuenne summarized the results of their assessments as follows: "[The book] bears witness to the revolutionary impact of the complex of concepts and analytical frameworks introduced by Edward H. Chamberlin thirty-three years ago, and to the correctness of his decision to devote his career to the perfection and development of that theory."

Although Chamberlin was preoccupied almost exclusively with the elegance of his theory and paid scant attention to its public policy implications, its relevance to the practical issues of policy soon became evident. National anti-monopoly policies, especially those expressed in the antitrust laws of the United States, are based on assumptions about the interrelationship between market structure, business behavior, and business performance. The more ardent proponents of an aggressive antitrust policy found in Chamberlin's determinate oligopoly model a highly respectable *raison d'être* for breaking up large corporations with substantial shares of relevant markets and for the prevention of mergers that substantially increased firms' market shares. Since in markets where a few firms controlled the entire output each firm would, by taking into account the indirect as well as the direct effect of a price move, eventually charge the monopoly price, the obvious antitrust remedy was to make all firms so small in relation to the market that they would ignore the indirect effect, thereby bringing about the structural conditions conducive to competitive pricing. In this sense it can be said that Chamberlin is a silent expert witness in virtually every important monopoly case prosecuted under various nations' antitrust and cartel policies. Beyond this, those who propose that these policies be strengthened by new legislation often

appeal to the Chamberlinian solution as their authority.

Chamberlin's great impact on economic thought during his lifetime, an impact that obviously endures, is all the more impressive when assessed in the light of the time frame in which *Monopolistic Competition* was born and matured. The great depression of 1929–1933 had diverted the attention of economists from the market allocative problem with which Chamberlin was concerned and toward the macroeconomic problems of unemployment, general price levels, and economic growth. The appearance in 1936 of John Maynard Keynes's *General Theory of Employment, Interest and Money* provided a theoretical framework for analyzing and prescribing remedies for these new and pressing problems. The Keynesian model quickly became the "new" economics, absorbing the intellectual energies of the vast majority of the economics profession. It is a signal tribute to Chamberlin's dedication to refining his own theories that he did not succumb to the immensely popular trend. It is an equally high tribute to the results of his tenacity that *Monopolistic Competition* and the *General Theory* are often identified as the two most influential economic treatises of the twentieth century.

In contrast with his professional concerns, Chamberlin's personal life was enriched by a variety of interests, including music, painting, the theater, and literature. He spoke French with the fluency of a Parisian and lectured widely in French when visiting universities in France, a country he came to love dearly as the birthplace of his wife, Lucienne. Throughout his uninterrupted career at Harvard, often characterized as the "Golden Age" of Harvard's economics department, consisting as it did of such internationally known economists as Frank W. Taussig, Joseph A. Schumpeter, Sumner H. Schlichter, Edward S. Mason, Alvin Hansen, Wassily Leontief, and, of course, Chamberlin himself, his reputation was of a cultivated man with a circle of friends drawn from all parts of the university. He was in every sense a cultured gentleman and a courageous and tenacious scholar.

JESSE W. MARKHAM

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CLARK, COLIN

Colin Clark, one of the most fertile minds in twentieth-century applied economics, was born in London in 1905. After graduating in chemistry at Oxford University in 1924, he worked as assistant to William H. Beveridge, Allyn Young, and A. M. Carr-Saunders, stood unsuccessfully as a Labour candidate in the May 1929

general elections, then joined the staff of the Economic Advisory Council which had recently been formed by the Ramsay MacDonald government and which included among its members John Maynard Keynes, G. D. H. Cole, Josiah C. Stamp, and R. H. Tawney. Having declined an invitation by MacDonald to help him prepare a protectionist manifesto, he was glad to receive an appointment in 1931 as lecturer in statistics at Cambridge University (Clark 1977). In 1937, he accepted an invitation to spend two terms as visiting lecturer at the universities of Melbourne and Sydney, but remained in Australia for fourteen years, from 1938 as director of the Bureau of Industry and economic adviser to the government of Queensland. When, in 1952, after increasing divergence of opinion, there remained no policies of the government with which he could agree, he resigned and spent a year, first as a freelance writer and business consultant, then as visiting professor at the University of Chicago. From 1953 until 1968 he was director of the Institute of Agricultural Economics at Oxford University, then he returned to Australia, as research fellow at Monash University (1969–1977) and since 1977 as research consultant in economics at the University of Queensland.

In the first decade of an astonishingly prolific half century of research and writing, Colin Clark established himself as one of the pioneers of national income estimates. His first two books (1932; 1937) greatly improved on existing estimates for the United Kingdom and extended them for additional years; the next two (Clark & Crawford 1938; Clark 1939) did the same for Australia and the Soviet Union. In these books and several major papers he made methodological contributions so fundamental that he has justly been described as coauthor, with Simon Kuznets, of the “statistical revolution” that accompanied the revolution in macroeconomic theory of the 1930s (Patinkin 1976). He was the first to use the gross national product (GNP) rather than national income—he may reasonably be regarded as the inventor of GNP—and among other innovations made the first quarterly estimates of national product and devised time series of labor productivity and fixed capital formation. An early Keynesian, he made some of the earliest attempts to estimate the size of the multiplier from national income data (Clark & Crawford 1938; Patinkin 1976, pp. 1100–1104) and was among the first to present estimates in the framework of the main compo-

nents of aggregate demand, $C + I + G$ (Patinkin 1976), and to develop a multisector multiplier analysis (Samuelson 1966). By 1940, he had gone a long way towards justifying in his published work Keynes’s discovery in 1931: “Indeed, Clark is, I think, a bit of a genius: almost the only economic statistician I have ever met who seems to me quite first-class” (letter to Daniel Macmillan, December 1931, quoted in Patinkin 1976). He had an international reputation for posing important questions and answering them by ingenious use of inadequate statistics. In the words of another fellow of King’s College: “Mr. Clark is a famous maker of bricks without straw” (Giblin 1941).

In 1940 appeared Clark’s most important work, *The Conditions of Economic Progress*. The idea of drawing on all available national income and other data for an international comparative study of the problem which Adam Smith had called that of the origin of the wealth of nations came to Clark in 1935 and he worked on it intermittently and single-handedly but with incomparable energy, verve, and imagination for four years. The book is a seminal work in at least three respects. First, it was one of the first systematic attempts to use national accounts data for a wide range of countries to test macroeconomic hypotheses and in this sense a pioneering contribution to modern macroeconomic econometrics. Second, it was the first major work, after decades of almost exclusive preoccupation of the Western economic profession with static problems of resource allocation and latterly with economic fluctuations, that turned attention back to the classical problem of economic growth and was thus a starting point of modern development economics. Third, by supplying the first substantial statistical evidence of the gulf in living standards between rich and poor countries, it helped awaken Western opinion to the problems of underdevelopment. Clark may be regarded as the discoverer of the “Gap.” Well into the post-war years, until United Nations data became available, almost every writer on development economics quoted his estimates.

Among many specific novel ideas in the book, three deserve special mention because each gave rise to a later literature of its own. One was the technique he devised for international comparisons of the purchasing power of national currencies, and thus of real national product, his so-called IU (international unit) (Stu-

denski 1958). The second was his thesis that, in the course of economic growth, a country's occupational structure shifts, in the terminology he took over from A. G. B. Fisher, from primary to secondary and tertiary industries (Hoselitz 1960). The third was the use of a Cobb–Douglas production function for macroeconomic estimates of the marginal productivity of capital.

In the following years, Clark revised and developed all parts of the book; indeed for some years he published a monthly journal, *Review of Economic Progress* (1949–1952), which consisted entirely of revisions and new studies subsequently incorporated in the third edition of the book. But the most notable development was the publication of *The Economics of 1960* (1942), a daring (to use no stronger word) attempt at an econometric forecasting model for the world economy. The most surprising forecast was that the terms of trade would move by ninety per cent in favor of primary products, mainly because of massive industrialization of China and India. "The prediction was not fulfilled," as he has since conceded in a personal communication, "but, as [Bertrand] de Jouvenel pointed out, the book performed a service, in showing on how many points the estimates can go wrong" (1978). It was useful also as a counterweight to arguments soon to become fashionable that the terms of trade must inevitably move against primary products (Higgins 1959).

So important and original was Clark's work of the 1930s that one wonders why his name is not universally recognized as among the half dozen great economists of the twentieth century. Part of the answer is suggested in two brilliant reviews of *The Conditions of Economic Progress* (Rothbarth 1941; Giblin 1941). One reads in part:

It is difficult to know what to admire most in this book: the boldness of the underlying scheme or the energy and labour that went into its execution, the avoidance of unnecessary subtleties or the width of observation and experience that made this avoidance easy. And yet it is in many ways a very annoying book. . . . Mr. Clark has such a wealth of material to work on that it seems to matter very little what he does with it; he does not grade it sufficiently according to quality; he frequently throws it before the reader in the raw state and equally frequently he works on it with primitive and inappropriate tools. . . . He never reaches that highest level of achievement—the product of close theoretical penetration of complex facts—where one perceives

suddenly with a feeling of its inevitability a new unity and a new simplicity amongst the mass of accidental material. (Rothbarth 1941)

Since the end of World War II, while continuing to make contributions to applied economics on all manner of subjects—from the declining importance of capital (compared with knowledge) as a determinant of economic growth, trade cycles in the United States, the causes of inflation, the economics of beef cattle, irrigation, and housework, to estimation of net capital stock and the marginal utility of income—Clark's work has been dominated by an event that preceded the war, his conversion to Roman Catholicism. Whether, as his critics claim, he has subordinated his own thinking to the tenets of the Church or whether, as he would probably see it, he has been attracted to Catholic social thought because it has corresponded most closely to his own values, most of his writings in the last thirty years explicitly or implicitly support the social teachings of the Catholic Church, and he has been one of its most effective lay protagonists. Two themes stand out.

One is opposition to neo-Malthusianism. As the most influential lay member of the Pope's Commission on Population (1964–1966), Clark is believed to have provided much of the economic rationale for the hard line against birth control by contraceptive devices of the papal encyclical *Humanae Vitae*. In essence, his case, expounded in numerous books, pamphlets, and articles, has been that the world's resources, rationally used, are sufficient to provide adequate food for any foreseeable population growth and that, generally speaking, growth of population and per capita real income are positively, not negatively, correlated (1951; 1967; 1970). The former part of the case stimulated nearly two decades of specialization on agricultural economics and drew on the empirical evidence yielded by this work; the latter part, with its belief in increasing returns, went back for its inspiration in part to his earliest professional work as assistant to Allyn Young and editor of G. T. Jones's study (1933).

The second theme has been opposition to totalitarianism and increasingly to big government of any kind. In 1945, Colin Clark amazed the economic profession by arguing in an article in the *Economic Journal* the thesis (for which he gave credit to a "brilliant insight" by Keynes 20 years earlier) that there is a

limit of about 25 per cent to the ratio of taxation to national income, the limit being set by resistances to higher taxation which cause inflation and thus bring the ratio back to about 25 per cent. In 1954, he created an even greater stir by arguing, in a pamphlet on *Welfare and Taxation* that, if the welfare state were dismantled, so that people had to obtain their social welfare requirements through voluntary organizations, but having at the same time the major part of what they now pay in taxation refunded, they might in the end be better off. Both theses were distinctly unfashionable when they were first expounded, and the coat-trailing assertiveness with which they were put forward did not help win them adherents. Now, almost a generation later, they may be recognized as anticipating the reaction to welfare statism associated especially with the University of Chicago, Virginia Polytechnic, and proposition 13, but by no means confined to the United States.

Clark has always relished being provokingly unfashionable:

He supported Keynes long before he became fashionable, and questioned Keynes when he was fashionable. He opposed the protection of agriculture in Britain and of manufactures in Australia. Long before others he questioned the high rate of economic growth in Soviet Russia. In *Growthmanship* . . . he argued, against the fashion, that economic growth could not be accelerated artificially by capital investment . . . and now [he argues] that the way to reduce [taxation] from 40 to 25 per cent is to dismantle many state welfare services and return to the average family a large part of the income taken from it so that it can make better provision for itself. (Editor's preface 1964)

This, too, may be a view less unfashionable in the 1980s than it was in the 1950s.

Like George Bernard Shaw and Bertrand Russell, Colin Clark has often written and spoken *pour épater le bourgeois*, not just for the fun of it but to startle his audience out of the errors of its ways. Even more than a first-rate quantitative economist, he has been a prophet. How far his has been a voice crying in the wilderness is for a later generation to judge.

H. W. ARNDT

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COASE, R. H.

Ronald Harry Coase was born in Willesden, England, on December 29, 1910, and was educated at the London School of Economics (B.COM., 1932; D.SC., 1951). There he enrolled in the industry and trade curriculum, which exposed him to both economic theory and the detailed study of business and legal institutions. Apart from wartime service with the British government, Coase has spent his career in the world of books and ideas. In Britain he served on the faculties of the Dundee School of Economics and Commerce, the University of Liverpool, and his alma mater. In 1951 he moved to the United States, where he has held appointments at the University of Buffalo, the University of Virginia, and the University of Chicago. In 1964 Coase assumed the editorship of the *Journal of Law and Economics*, the outlet for some of his most important contributions.

The theory of the firm. In Coase's first major work, "The Nature of the Firm" (1937a), he resolved several difficulties in the economic theory of the firm. Coase began by contrasting economic activity between firms, where resources are allocated in response to market-determined relative prices and economic activity within firms, where inputs are assigned to various jobs according to managerial directives. He sought to explain why production was sometimes coordinated centrally within firms, but determined at other times by decentralized market forces. In short, why did firms exist?

The answer pivoted on Coase's recognition of the costs of making market transactions. Real resources are required to learn prices and make contracts governing every productive activity of each input. Whenever the coordination of resources can be accomplished more economically by central organization, firms will be established. But the costliness of coordinating many inputs limits the firm's size. Beyond that size, resources will be allocated more economically by market forces.

Coase's theory of the firm is an important breakthrough. It eliminates relying upon legal requirements, residual income claimants, or the division of labor as reasons for the existence of firms. The theory explains a firm's size: If the cost of organizing an additional transaction is less within a firm than accomplishing the same activity through market exchange, a firm will expand (an explanation that neatly fits the Marshallian framework of substitution at the

margin). Coase's focus on the cost of using the market became an integral component of the economic theory of property rights.

Cost and pricing behavior. In economic analysis, the cost of something is the highest valued alternative given up in order to get it. Early in his career Coase clarified the kind of cost figures businessmen would need to operate economically. He demonstrated the logic and relevance of data based on opportunity costs, contrasting these with the statistics traditionally gathered by accountants. By this time Coase was intimately acquainted with the decision-making methods of the business world; yet he defended the adequacy of the marginal analysis of firm behavior used by economists. When managers purported to use a nonmarginal rule in decision-making, such as full cost pricing, he viewed this as reconcilable with orthodox economic theory because the implementation of such rules implicitly takes some account of demand forces or incremental costs of output variations.

Since the 1930s, many economists have advocated that public utilities price their output at marginal cost. Coase has been a critic of this proposal, ascribing its appeal to "blackboard economics"—analysis with diagrammatic allure that nevertheless overlooks a policy's operational requirements. While not denying the significance of marginal cost, Coase argued that the total economic effect of a policy must be assessed, including the behavior of the social institutions that will implement it. He proposed a multipart pricing structure, giving appropriate weight to marginal cost while avoiding difficulties perceived in proposals based strictly on marginal cost pricing.

The economics of communication. Coase's interest in public utilities focused primarily on institutions of communication, notably radio, television, and the post office. On one level his work was historical. The account of the monopoly origins of radio and television in Great Britain, the operation of the Federal Communications Commission in the United States, and the response of the British post office to rival enterprise required exhaustive examination of newspaper clippings, corporate archives, and government documents. Yet Coase viewed this material through economist's spectacles, and from it also came important analytical contributions.

Coase explained how radio broadcasting could be organized by the market system, meet-

ing head-on the conventional argument that without government control or monopoly there would be interference as stations transmitted simultaneously on the same frequency. He demonstrated that such congestion was not an example of market failure, but rather of a situation where one of the preconditions for the proper functioning of a market had not been met: the existence of private property in the electromagnetic spectrum. If individuals could, by virtue of ownership rights, exclude others from particular frequencies, there was nothing to prevent a free market in broadcast frequencies; and in testimony before the Federal Communications Commission in 1959, Coase recommended that the agency auction broadcast frequencies to the highest bidder, the proceeds going to the Treasury, and the media then being subject to the discipline of competition. Coase's scholarly interest in broadcasting led to his seminal article, "The Problem of Social Cost" (1960), the rudiments of which appear in his article on the Federal Communications Commission (1959).

In addition, Coase took the arguments in support of freedom of speech, religion, and the press and showed how, conceptually, these arguments favored a free market in radio and television as well. Expanding upon the work of Aaron Director, he later advanced the broader proposition that, from both an economic and a libertarian perspective, there is no distinction between the market for ideas and the market for goods; economic freedom serves the same ends in each sphere.

The concept of social cost. Before Coase's social cost paper was published in 1960, external economies and diseconomies were catalogued by economists as market failure. For example, it was argued that polluting firms would choose outputs based on private costs of operation, ignoring spillover costs imposed upon others; inefficiency in resource allocation would be the result; a tax on the pollutor was usually recommended to remedy the market's defect. Coase challenged this view in tripartite fashion.

First, he showed that if property rights are established for all inputs, and transaction costs are negligible, external effects will be mitigated efficiently through voluntary agreement, *sans* government action. Then he proved that under these conditions, the locus of liability (or fault) for any external diseconomy does not alter the composition or value of total output. In the case

of a polluting firm, resources will be allocated efficiently by private negotiation, no matter who is technically liable for the pollution. This proposition is now called the Coase theorem. Coase then focused on a world of nonnegligible transaction costs, demonstrating that here rules of liability did affect resource allocation but that no a priori assignment was efficient; instead, the total costs and benefits of alternative rules had to be estimated.

The article has proved rich in theoretical implication and institutional detail, receiving great attention and generating a literature all its own. It has provided a framework for the discussion of externality policy, such as pollution and accident control. So germinative has Coase's analysis proved that it has served for the tackling of such diverse topics as antitrust, share tenancy, blood delivery systems, and the baseball reserve clause as well.

Other work. Coase's scholarly interests have been diversified. In addition to the contributions reviewed, Coase has authored papers on duopoly and monopoly pricing, the pig-cycle, consumer's surplus, the pricing of North Sea gas, advertising, the financing of public goods, antitrust enforcement, and the proper scope of economic inquiry. His paragons are Adam Smith and Alfred Marshall, about whom he has astonishing knowledge, and he also has written about the lives and work of these economists.

Assessment. It is no easy task to trace out the direct influence of other scholars upon Coase. A case could be made for his teacher, Arnold Plant, and for Edwin Cannan (Plant's mentor). Frank H. Knight, Philip Henry Wicksteed, Friedrich A. von Hayek, Aaron Director, and Leo Herzel made contributions from which Coase has drawn. But basically he must be seen as his own man, an archetype of the self-contained scholar. Not unlike Adam Smith, Coase has worked productively alone, drawing ideas from his reading and reflection, classifying facts through his grasp of economic theory. Indeed, his work is a testimony to the power of inductive reasoning, since Coase uses his extraordinary interest in the commonplace facts of economic behavior to generate ideas with far-flung consequences.

Coase's scholarly influence has been selective, for the methodology and interests of his research have commonly run against the grain. His enthusiasm for abstract theory without practical application has always been restrained;

he eschews advanced mathematical and statistical techniques; and he considers the history of ideas to be consequential. The theme of his work has been to preserve and expand the scope of market processes. His influence, accordingly, has been most notable in the United States where there is greater interest in the market system than elsewhere.

It is too early for an unqualified evaluation of Coase's work, in part because he continues to produce. But to rank him as one of the principal founders of the modern discipline of law and economics is not premature. He has altered fundamentally the way many legal scholars view torts, contracts, and property law. His editorship of the *Journal of Law and Economics* has contributed significantly to the intellectual growth of this area of study. In economics Coase's work has greatly advanced the property rights paradigm and appreciably altered the way economists view business firms, externalities, public goods, and the case for an economy organized by market exchange.

KENNETH G. ELZINGA

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COCHRAN, THOMAS C.

Thomas Childs Cochran, born in 1902, was a pioneer in the introduction of the behavioral sciences into history and the concomitant effort to shift scholarly focus away from the study of national political and military events toward social history. Believing that business and economic changes were the most distinctive and dynamic elements in the American past, he concentrated his research and publications on business history. He was the best-known and most widely respected analyst of our "business civilization" in the middle decades of the twentieth century.

Educated at Adelphi Academy, New York University (B.S., 1923; M.A., 1925), and the University of Pennsylvania (PH.D., 1930), Cochran was a "late bloomer" as a scholar. He began his undergraduate studies at N.Y.U. with the intention of becoming a chemical engineer, but decided late in his junior year that his real interests lay in history. Through his mid-thirties, relative financial security and a wide range of interests made his academic job at N.Y.U. and his scholarship virtually avocations. As he later wrote in a typescript autobiography, he initially pursued these avocations "chiefly because I had a Calvinistic urge to be purposefully occupied" (1977a). His doctoral dissertation, written under the direction of St. George L. Sioussat, was published in 1932 as *New York in the Confederation*. Cochran later worked sporadically on a set of ideas concerning what he saw as the central role of business in the American past. In 1938

he married his third wife, Rosamond Beebe Cochran, and, he wrote many years after, "became, for the first time, a real scholar." By the close of the depression decade, both his personal and professional lives had become more settled and purposive; his marriage to his helpmate and his intellectual interest in the role of business in shaping culture would occupy his attention and energies for the following four decades.

In his developing career Cochran sought to disseminate some of his emerging ideas about the need for a "cultural approach" to history, employing concepts from such disciplines as anthropology, sociology, and psychology. Many of these notions were apparent in the book he published in 1942 with William Miller, *The Age of Enterprise: A Social History of Industrial America*. That study sought to reinterpret the broad contours of nineteenth- and twentieth-century America in terms of the impact of business enterprise; the quest for "economic opportunities and economic freedom," they asserted, "has been most powerful in determining the nature of our culture." Cochran expanded and drove home these arguments in a highly influential essay entitled "The 'Presidential Synthesis' in American History" (1948*b*). There he denounced as a failure "the old skeleton of wars, presidential administrations, and the westward movement" which still held the edifice of American history together despite "fifty years of rapid growth in the social sciences." History as practiced in academe was virtually irrelevant; it lacked "social realism." Cochran called for a new "social science synthesis" explicating the "material or psychological" changes that most affected "such human conditioning factors as family life, physical living conditions, choice of occupations, sources of prestige, and social beliefs." In the next three decades, many historians in the United States came to accept this emphasis on underlying social structures and processes rather than on superficial political history. Relatively few, however, would follow Cochran in stressing business as the central element influencing the evolution of our society.

Cochran raised the analysis of American business history far above the dominant categories of case studies of individuals or firms and the shallow, moralistic interpretations of business leaders, whether as "robber barons" or as "industrial statesmen." In *Railroad Leaders, 1845-1890* (1953), written during his stay at Arthur H. Cole's Research Center in Entrepreneurial History at Harvard University, he sought

to explicate the "business mind." Influenced by historian-sociologist Leland Jenks, he employed hypotheses from sociology and social psychology in that study. Throughout his work, and especially in *Business in American Life* (1972*a*), he explored the interrelationships between business and other social institutions such as the family, religion, education, the law, and politics. More than any other historian, he succeeded in analyzing business in its wider social context.

From the late 1940s on, Cochran enjoyed a varied and highly successful professional career, teaching and writing at N.Y.U. and then at the University of Pennsylvania, sitting on influential committees of historical associations, serving briefly as a director of the National Bureau of Economic Research (1950-1952) and later of the Social Science Research Council (1960-1964), editing professional journals, and receiving many honors, including the presidency of both the Organization of American Historians and the American Historical Association, as well as honorary degrees and prestigious lectureships. He wrote prodigiously, primarily but not exclusively, on the history of business in the United States.

Ironically, although Cochran was widely identified with the use of social science in history, he always remained the inductive historian, refusing to allow any body of theory to determine his view of the past. And, despite his emphasis on business and economic history, he made little use of perhaps the most powerful of social science theories, economics. He preferred the "softer" behavioral sciences, and he took no part in the move toward highly mathematical and theoretical "cliometrics" that became so popular in economic history in the 1960s. Moreover, he persisted in defining himself as a business and social historian, not an economic historian. In his 1977 memoir he declared, "I have always held abstract ideas tentatively, trying to weigh them against evidence, and ready to discard generalizations that seemed no longer tenable." His role was to broaden historians' acquaintance with other disciplines, to encourage the use of other methodologies when they seemed helpful, and to push historians toward a focus on new topics. The social sciences were for Cochran never more than another assortment of tools for the eclectic historian's toolbox. Hugh Aitken in *Explorations in Enterprise* (1965) offered a wise assessment of Cochran's use of sociological and psychosocial notions in his days at Cole's center: "Cochran

. . . took much of the heat out of the controversies and misunderstandings that were . . . developing, demonstrating by his work—the only way that could carry conviction—that there was nothing in the new concepts and hypotheses that need divert an honest craftsman from doing an honest job” (p. 13). That assessment could well have been written of his work as a whole.

GLENN PORTER

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CORWIN, EDWARD S.

Edward Samuel Corwin was born, as he used to emphasize, “of Old English stock” on a farm near Plymouth, Michigan, in 1878. As an undergraduate at the University of Michigan (B.A., 1900), he concentrated on the study of American history under the tutelage of Andrew C. McLaughlin. He went on for graduate work to the University of Pennsylvania (Ph.D., 1905) where, under John Bach Masters, he specialized in the history of the American colonial and revolutionary periods. In 1905 Woodrow Wilson, then president of Princeton University, appointed him one of the original fifty-five “preceptors” at the university with which Corwin’s name would be associated throughout his scholarly career.

Princeton’s new preceptorial system, regarded at the time as a radical innovation in undergraduate teaching methods, combined lectures with intensive small group discussion of assigned readings. The faculty preceptor in each such group, which typically covered several hundreds of pages each week from “standard” works (e.g., Holland’s *Jurisprudence*, Maine’s *Ancient Law*), was expected to work closely and on a personal, even a social, basis with his students. Hence, Wilson required that preceptors be “clubbable,” a term referring to the personality traits especially valued by the members of Princeton’s somewhat snobbish eating clubs. In an instructional system that put heavy demands on the faculty member, Corwin rapidly established a reputation as a tough teacher. A bit of a martinet in the classroom, he retained among students and colleagues alike the nickname “the General” throughout his career at Princeton.

Corwin’s vocation as a commentator on American public law spanned an eventful half century that saw the U.S. Supreme Court reach the apogee of “constitutionalized laissez-faire,” then retreat to a position of relative neutrality on economic issues during the “constitutional revolution” of the New Deal years. Throughout the first three decades of the twentieth century, Corwin was widely considered to be one of the two or three most influential students and critics of the Court. He served as president of the American Political Science Association in 1931. In the 1940s Corwin shifted his interest somewhat, from the Supreme Court to a focus on the office and powers of the American presidency (1940b). During the 1950s Corwin authored, coauthored, or edited five books, one of them

the magisterial *Constitution of the United States of America: Analysis and Interpretation* (1953), the result of a major research effort sponsored by the Library of Congress. He continued to publish major articles, reviews, and letters to the editor, many of them severely critical of the post-1940s trend of constitutional decision (1947; 1949). Active almost to the end, Corwin died in Princeton in 1963.

Corwin began to specialize in constitutional history and interpretation soon after he assumed his preceptorial duties at Princeton, in what was then the department of history (which in 1937 would see the "secession," under Corwin's leadership, of a new department of politics). Corwin once told a student that the mentor of his undergraduate days, "Andy Mac" McLaughlin, had been the leading influence on his scholarly and pedagogical approach. McLaughlin, however, had kept his courses in American constitutional history and constitutional law in more or less separate compartments—a not unusual approach in those days. On the one hand, McLaughlin dealt in his lectures and books with personal, political, and social forces that had influenced the authors of the constitutional document. But he treated the interpretation of this document as the product of pure judicial reasoning from agreed premises that were largely beyond the give and take of politics.

Along with Charles A. Beard of Columbia University and Thomas Reed Powell of Harvard University, Corwin attained leadership in the development of a broader conception of public law. Rejecting McLaughlin's dualistic approach, Corwin in his classes and his scholarship stressed the connection between legal doctrines and larger philosophical, political, and socio-economic issues. Corwin's emphasis on the historical context of constitutional development, rather than merely the internal logic of exfoliating legal doctrines and rules, probably represents his most significant contribution to scholarship.

The fruits of the first phase of Corwin's scholarship were probably the most important and enduring of his career. This phase stretched from the earliest years of his preceptorship through the late 1920s. During this period, Corwin produced what he himself regarded as his most creative work: his studies of such foundational doctrines as "due process of law," "vested rights," and "higher law" (see, for example, 1909; 1911; 1914*a*; 1914*b*; 1914*c*; 1925*b*; 1928–1929). Most particularly, Corwin directed his attention to the doctrinal underpinnings of

the keystone institution of the American constitutional system, judicial review itself.

Corwin's concern with the philosophical basis of judicial review was altogether natural, in view of the fact that the first two decades of the twentieth century saw the reopening—and eventually, the settling—of a great debate over the role of the federal appellate courts in American politics. Some progressive politicians (such as Wisconsin Senator Robert M. LaFollette), some labor leaders (such as Eugene V. Debs), and some scholars, who objected to the probusiness orientation of an increasingly active federal judiciary, either denied the validity of judicial review or proposed its repudiation by constitutional amendment. Other observers of the American political scene (such as Beard, whose *The Supreme Court and the Constitution* [1912] and *An Economic Interpretation of the Constitution* [1913] touched off a flood of controversy) conceded the legitimacy of judicial review. But the supporters of the institution offered a variety of theories on which to base the distinctive judicial authority. All contributors to the debate had to contend with the framers' failure explicitly to provide for any power of review.

In contrast to Beard (whose most famous book, the *Economic Interpretation*, Corwin seems not to have particularly admired; see 1914*c*), Corwin regarded judicial review not as a device to secure certain economic privileges for the propertied class, but as a product of prevailing political–legal philosophy at the time of the framing. A proper understanding of the basis for judicial review, Corwin argued, must begin with an appreciation of the framers' assumptions. The founding fathers, most of them lawyers, were heirs to the British tradition of a fundamental law. Since the time of Edward Coke, this tradition had implied the existence of a vaguely defined "higher law" capable of controlling acts of the legislature. From this seventeenth-century conception, it was but a short step to the concept of higher law in the sense of a body of principles that possess intrinsic validity. In relation to such principles, positive enactments of legislatures are not exertions of human will, but rather acts of discovery or declaration (1928–1929). The framers, Corwin argued, regarded the written Constitution as the embodiment of such higher law principles. This view would automatically have yielded a justification of some power, somewhere in government, to review positive legislation.

But how did this power become the peculiar

province of the courts? In the higher law tradition, judges, who were the defenders of rule of law against royal prerogative, had occasionally been willing to invoke natural law—always defined in light of common law precedents (1914*a*, p. 255). The common law tradition carried the further implication that judges possessed a special competence. As the adepts in what Coke had called the “artificial reason of the law,” judges were the only persons competent to discharge the task of comparing legislative enactments with superior law. In this way, Corwin argued, “law had become a *professional*, nay an *official mystery*” (1934, p. 109) rather than man’s universal inheritance, attesting his participation in the reason and order of the universe and, indeed, in the Godhead itself. According to Corwin, then, judicial review rested on general principles implicit in the jurisprudential assumptions of an age, and hence needed no express indication of the intent of the framers, registered in a written line of the Constitution.

But Corwin also argued that judicial review must keep pace with change over time if it were to survive as an institution in a democracy. Just as its initial validity (in the sense of the historicity of the framers’ intent) depended on the “public opinion” of the earlier generation, the continuing vitality of judicial review (in the sense of widespread popular acceptance and hence legitimacy) required decisions capable of eliciting broad public support. Laissez-faire decisions by the Supreme Court had begun to erode public confidence in the judiciary. Beginning in the late 1920s and running to the eve of World War II, Corwin turned his attention from historical and jurisprudential questions to the economic and political issues of the times (1925*a*; 1932; 1934).

Underpinning the attacks that Corwin directed with increasing frequency (and vehemence) against the Supreme Court under Chief Justice William Howard Taft and later, under Charles Evans Hughes, was a distinctive conception of the judicial process. In a paper for the *American Political Science Review* (1925*a*), Corwin asserted that constitutional interpretation depends not only on “constitutional law” in the strict sense of a set of rules whose logical application to the facts of a case will yield a decision. Interpenetrating constitutional law as thus defined is “constitutional theory,” a less coherent corpus of doctrine rooted in contending ideas of some historical standing about the meaning of the

Constitution. Constitutional theory inevitably presented itself in the form of mutually contradictory ideas: strict *vs.* loose construction; dual federalism *vs.* national supremacy; Chief Justice John Marshall’s conception of an adaptive, expansible constitution *vs.* Roger B. Taney’s (professed) strictness of adherence to the views of the framers.

Because constitutional law consists of a set of rules for decision, it should logically compel a judge to reach a unique decision in any given case at the bar. The hallmark of law, in other words, must be mutual consistency of each decision with decisions on the same point that went before it.

Corwin’s argument against judicial supremacy turned on his charge that the Supreme Court had gradually ceased to function as a court in the strict sense of a panel applying “law.” The justices had acquired freedom to choose from competing lines of precedents so as to endow themselves with the liberty to reach politically predetermined results under the guise of drawing conclusions compelled by law. In short, they had substituted their own preferred version of constitutional theory for constitutional law. For example, as against state laws regulating wages and hours or the quality of merchandise, the Court could apply the due process clause of the fourteenth amendment on the ground that such laws interfered with employees’ “liberty” or with the entrepreneurs’ “property.” Or again, the Court could strike down such laws—provided that the enterprise involved was not strictly local—on the ground that states lacked constitutional authority to regulate interstate commerce. On the other hand, the Court could overturn congressional laws regulating industry, citing the “reserved powers” clause of the tenth amendment as a restriction on national power. “And the total result of this kind of reasoning,” Corwin wrote at the height of the New Deal court battle, “was the appearance of a no-man’s land in which interests organized on a national scale at all times escape all regulation” (1936, p. 153).

The Supreme Court retreat during the years 1937–1941 to positions consistent with the needs of a unitary national economy subject to active legislative regulation (and, when Congress deemed it desirable, active stimulation too) represented a victory for those identified with Franklin D. Roosevelt (1941). Corwin, in the forefront of scholars who championed the New Deal, had been selected by Roosevelt’s

attorney general, Homer S. Cummings, as the first academic figure—indeed, as the first witness who was not a member of the administration—to testify before the Senate on behalf of the eventually unsuccessful “court-packing bill” (1937). There is evidence that Corwin had been led to expect a reward of some sort—perhaps even a seat on the high bench itself (Garvey 1969)—if the “court-packing bill” passed. However, the judicial “switch in time that saved nine” helped to defeat Roosevelt’s disingenuous plan for judicial reorganization. Corwin, his reputation for independence now damaged by the example of his serviceability to political figures, never heard the call that might have taken him from Princeton to power and place in Washington. For whatever reason, following the “constitutional revolution,” Corwin’s enthusiasm for Roosevelt’s expansive view of executive power waned, and several important titles during the years of World War II challenged the basis of broad powers being exercised by the president (1940a; 1940b; 1947).

GERALD GARVEY

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CURTI, MERLE

Merle Eugene Curti, dean of American intellectual historians working in the Progressive tradition, has himself summed up the major theme and approach of a lifetime of scholarship and service: he “emphasized the functional role of democratic ideas and forces in the history of American thought and put that history in a social setting which includes tussles between com-

peting interests and values" (1952, quoted in 1955, p. 23).

For well over two decades, Curti played a special and even unparalleled role. From at least 1935 to 1955 he was not only a major contributor to the literature of historical scholarship, but he was moreover a recognized and honored leader of his profession in its educational and organizational work. In 1952 a survey of more than one hundred American historians revealed that his Pulitzer Prize winning book, *The Growth of American Thought* (1943), was, judged on the basis of accuracy and contributions to knowledge, the "most preferred" of all works in American history or biography published between 1936 and 1950, "best for reference or for reading or for both purposes" (Caughey 1952, pp. 289-302). He had served as president of both the Mississippi Valley Historical Association (1951/1952) and the American Historical Association (1953/1954). In 1955 he published a collection of his essays that originally appeared between 1926 and 1953, demonstrating not only the wide range of his interests, the depth of his research, and the breadth of source materials with which he worked, but also a rich interpretive ability sufficient to produce significant synthesis as well. By the close of that period he had already trained a remarkable number of graduate students now teaching in leading departments of history in every section of the country. While he never became especially well known to a wider public or a figure of general political importance as other historians have, few in the history of American history have had such a career of professional and scholarly achievement.

Born in Nebraska in 1897, he remained proud of his Swiss heritage that he always associated with the love of liberty. He received his undergraduate and graduate training at Harvard University where he found himself especially drawn to the teaching of another middle-westerer, Frederick Jackson Turner. In Turner's seminar he developed a series of essays that were to be among his first publications. Often studies of the role of ideals in history in mid-nineteenth-century America, these early works show a particular fascination with the role of reformers and reform movements, idealists and social critics in the optimistic America of that era. They also indicate an interest in the question of national self-consciousness and patriotism. Perhaps the best known of these pieces is the study of the "Young America" movement, his first

publication in the *American Historical Review* in 1926. When Turner retired from Harvard in 1924 he was replaced by another middle-westerer, Arthur M. Schlesinger, under whom Curti completed his PH.D. work in 1927. For his dissertation he elected to expand one of his earlier essays on the American peace movement into a full-length and more definitive study. This work was published in 1929.

Curti had taught at several smaller colleges before moving in 1925 to Smith College, where he remained until 1937, advancing rapidly in rank. The same year he came to Smith he married Margaret Wooster, an active child psychologist with a publishing career of her own. About this same time, Curti accepted a commission to prepare a paper that reflected his own developing interests. From the start of his career he showed a special concern for the social sciences in general, and more specifically, an interest in the relationship between these areas of inquiry and historical study. When the Committee on Scientific Method in the Social Sciences of the Social Science Research Council asked Curti to contribute to a large and important project, he accepted. It began for him a lifelong association with the SSRC. The committee was organizing a major collection of interpretations of scientific methods employed by authors of significant contributions to social science. Curti provided an analysis of the methodological concepts of Frederick Jackson Turner (1928, in 1931*b*, pp. 353-367). He showed that Turner, far from being simply a historian of the American frontier, also made important contributions to social, intellectual, and diplomatic history—indeed to all aspects of American development—as well as to explaining the importance of the role of economic group conflicts in United States history. The essay clearly demonstrated Curti's deep commitment to applying research from all of the social sciences to history, and the volume itself is a landmark effort to deal with the methods and nature of the social sciences.

Before he left Smith, Curti accepted another assignment of importance, this time for the Commission on the Social Studies of the American Historical Association. Established to recommend improvements in the teaching of history and the social sciences, it commissioned a series of studies on which its proposals might be more solidly based. Curti prepared part ten of that report, *The Social Ideas of American Educators* (1935). The result was a book of major impact in the history of education and in the

history of American social thought as well. Arguing that ideas about education always reflected more fundamental social ideas and attitudes, existing social problems and conflicts, Curti produced a work of historical scholarship based on careful and detailed research. It was sound but not disinterested. As Curti made clear in his preface, he believed that his historical analysis might prove "useful in the work of clarifying the purposes of social studies in this time of rapid social change."

Thus, by the time Curti accepted a post at Columbia's Teachers College, in 1937, he was not only a scholar with several major publications on his record, but also a professional, seriously involved with the work of national professional associations in history and the social sciences, and most especially concerned with the quality of teaching in these fields. He had already committed himself to the Progressive vision of the New History that had officially announced itself in 1912 (he insisted correctly that Turner was a charter member of the new history group that included James Harvey Robinson, Charles A. Beard, and Carl Becker). He might well have associated Columbia University with this school of historians as well as with one of the philosophers who helped inspire it. For John Dewey, a hero of his book on American educators, was perhaps the living philosopher with whom Curti felt most comfortable. He had come to accept as his own Dewey's belief that "ideas are plans for action," and had in his own work increasingly stressed not only the functional nature of ideas in history but also the view that historical understanding was itself the function of time and place, of social forces and social needs.

Curti soon had occasion to develop further his own strong interest in the interrelationship between history and the other social sciences. He served on the program committee planning the American Historical Association national convention for 1939. That committee decided to focus on three areas: techniques of cultural analysis and synthesis; the cultural role of ideas; and cultural conflict and nationality groups. These are precisely the areas to which Curti had devoted himself from the start of his career. As a member of the committee, he was responsible for sessions on psychology and history. He also presented a paper at a session discussing the so-called "flowering of New England" in the middle of the nineteenth century. Curti's comments, in the form of a statistical analysis of the background of the participants in the "flower-

ing," suggested that the roots of the movement might be found in the intellectualistic tradition of Puritanism and in the nature of the middle class.

The 1939 program was an event of importance professionally; for Curti it was a prologue to his greatest work, already in progress. In 1942 he accepted a position at Turner's old school, the University of Wisconsin, where he remained until his retirement in 1968. (In 1947 he was fittingly given a chair bearing the name of his own great teacher, Frederick Jackson Turner.) But before this event, Curti published a work of such remarkable scholarship, organization, and synthesis that he was awarded the Pulitzer Prize in 1943. It is a difficult work to discuss. An enormous contribution to knowledge just in terms of its research base and the new data unearthed, it is often so thorough that some readers are overwhelmed by its encyclopedic nature and by Curti's persistence in grounding the entire discussion in some meaningful social or socioeconomic context. Curti sought such contexts to prove the broader significance of the thoughts he presented, but the book offers no general theory or interpretation, no particular view of social change. In this sense, its viewpoint is eclectic. "Social" and even "socioeconomic" are terms Curti used, but refused to define with precision.

Yet this is a work of greatness. Curti insisted on the widest possible definition of "thought." Thus the work deals with formal ideas and systematic philosophical positions: ideas about politics, the state, society, human nature. But Curti also considered attitudes and beliefs important aspects of thought; thus he dealt with attitudes toward children and child rearing, the family, women, Indians, blacks, workers, the poor, classes, patriotism, and national self-consciousness—revealed not only by philosophers or self-conscious thinkers, but rather in ordinary social behavior and action, in the popular culture and literature, in the songs and folklore. There is still another kind of thought important in the work: knowledge. For *The Growth of American Thought* is also in many ways a historical sociology of knowledge, dealing with how knowledge is developed and used; how knowledge is diffused more widely and democratically over time; how it is organized and institutionalized. Thus, he devoted considerable attention to a wide range of educational and intellectual institutions and to the social role of scholars, scientists, and intellectuals.

The importance of this book is not in its

general view of social relations or the functional nature of ideas. It is rather the total vision of the nature of thought in America—its broad definition—and the remarkable revelation of the availability of sources previously largely untapped. The book was thus a basic document of, and a significant influence in the shaping of, the rapidly growing American Studies movement of the period. (Curti was, with others, a founder of the most important journal of that movement, the *American Quarterly*, which began publication in 1949. He served as the historian on a board of editors that included distinguished scholars in American art, literature, philosophy, economics, and sociology.)

During the 1940s, Curti undertook one more major professional responsibility with which he will always be associated. He assumed the chairmanship of another important SSRC committee—the Committee on Historiography. Its final report, *Theory and Practice in Historical Study*, the famous Bulletin No. 54 (1946b), was a series of essays and bibliographies for the clarification of key historiographic issues. Especially addressed to graduate students, it was one of the finest statements in defense of historical relativism, and often a brilliant extension of the vision of Beard, who as a member of the committee wrote parts of the report.

The 1940s was a fruitful decade for Curti. In addition to his professional work, he further developed his earlier interests. His *Roots of American Loyalty* (1946a) probed some of the ideas that had fascinated him from the time of his early essays. In this slim but impressive work Curti perhaps pushed closer to the kind of internal analysis of ideas his critics generally faulted him for failing to do. This charge was never quite just—Curti had to understand ideas before he could relate them to the social world in which he was especially interested—but there are passages in this work that show how effectively Curti can present such analysis. The second work was the huge history of the University of Wisconsin that he coauthored with his colleague and friend, Vernon Carstensen (1949). Curti was always interested in education, particularly in the sociology, organization, and diffusion of knowledge. Thus, the university was a fitting subject and the work itself one of a handful of good university or college histories.

While the 1950s represented a period of personal triumph, it also marked a time of significant rebuff to many ideas that Curti valued. In the historical profession itself, there was a major

assault on the bastions of historical relativism and the theories of Dewey, Beard, Becker, Turner, and therefore Curti. He found himself increasingly on the defensive in these debates in historical journals and at professional meetings. He spoke out vigorously against what he regarded as the voices of unreason that denied the possibility of rational action and planning. The newer vision of irrational and existential man troubled his old Progressive orientation, and it was during this period that he began his detailed study of various ideas of human nature in American thought, a study completed in the late 1970s. It was difficult to be a Progressive in the face of the challenge of a new pessimism. As early as the concluding words of his Pulitzer Prize volume, Curti could announce, in the midst of war and totalitarian challenge, that “hope, if not optimism, still lived.” Yet in later revisions in 1951 and 1964, Curti’s tone seemed increasingly uncertain about the future. During the remainder of his teaching career at Wisconsin in the 1950s and 1960s, he devoted more of his own efforts to the study of American influence abroad (again a subject that had interested him since his first essays) and the history of American philanthropy. Both of these efforts produced a series of creditable books and articles marking an honorable contribution to knowledge, but offering little in the way of new interpretation of our national past that could capture the professional interest accorded his previous writings.

There was, however, one exception. In 1959 Curti, with the aid of several of his graduate students, published a work of singular importance—a book, in a sense that Curti had spent a lifetime preparing to write. *The Making of an American Community* is a landmark volume. In it, Curti brought to bear all his knowledge and interest in social science. Using all available data—quantifiable data as well as the more traditional sources—he projected a two-fold strategy. In the wake of the historiographic debates of three decades could there really be an objective or more objective history? Was quantification a way out of total relativism? And, second, could Curti’s own interpretation of Turner’s frontier thesis actually be tested scientifically with the data from one frontier county in Wisconsin? The aims are within the tradition Curti had established for himself from the start of his career and were a fulfillment of Turner’s own dreams. The results are fascinating and important. The full significance of this achievement in reconstructing the social structure and

operation of an American community has yet to be fully appreciated. Too many dismissed it in the recent impatience with the old debate about Turner and his frontier, missing its larger implications.

Merle Curti is no historian of the frontier but he is most profoundly one of Frederick Jackson Turner's major students. Like his own teacher, he has opened many new directions in historical study. Like Turner, he had encouraged his vast number of graduate students to undertake work developing their own interests. He urged no particular methodology but was rather open to all ways of understanding. He provided no singular interpretation of history, but instead proposed a theme and an approach most significant for his own times. He struggled to retain a Progressive vision in an era when precisely that vision was under assault. Always one to doubt the possibility of one truth or of an objective history, he nonetheless pressed the use of various social science approaches in an effort to move closer to the impossible goal. In all of this lies his strength, rather than his weakness, his greatness as a man, a teacher, and a historian.

WARREN I. SUSMAN

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D

DART, RAYMOND A.

Raymond Arthur Dart was born to Samuel Dart and Eliza Anne Brimblecombe in Brisbane, Australia, on February 4, 1893. He was educated at schools in Queensland and at the universities of Queensland and Sydney. After receiving a B.Sc. (Hons.) in 1913, he became a medical student at the University of Sydney. In 1915 he was awarded the M.Sc. and in 1917 the M.B., Ch.M. (Hons.) from the same university. The latter year saw him a demonstrator of anatomy under James T. Wilson, who was, as Dart himself would be, a "maker of men": he left a long record of distinguished protégés rather than an outstanding research record. Dart was to prove a "maker of men" as well as a brilliant, intuitive researcher. Enlisted in the Australian Army Medical Corps, he served in England and France (1918–1919). On demobilization he became senior demonstrator in anatomy under Grafton Elliot Smith at University College London. Elliot Smith, with his strong emphasis on the nervous system and its role in primate evolution, was another major influence in Dart's life. When the Rockefeller Foundation established its fellowships program, Dart and his fellow Australian anatomist, Joseph Shellshear, were the first two Rockefeller fellows (1920–1921). Dart spent most of his American visit under Robert J. Terry in the anatomy department of Washington University in St. Louis, Missouri. Within a year of returning to University College, he accepted the chair of anatomy at the University of the Witwatersrand, Johannesburg, succeeding the first incumbent, Edward P.

Stibbe. Dart held the chair from 1923 to 1958, a period of 36 years. He piloted the growth of the infant medical school (founded in 1919), serving as dean of the medical faculty for 18 years. During his stewardship, a dental school was established and courses in physiotherapy, occupational therapy, and nursing came into being. He gave much time to the needs of the school and to public service.

Some years after he retired from the chair, he was appointed a visiting professor in the Institutes for the Achievement of Human Potential at Philadelphia and spent 6 months a year there between the ages of 70 and 86.

Dart's initial research contributions were in comparative neuroanatomy and neuroembryology, and a generation of protégés followed his work. He was interested in the peoples of Africa and originated the concept that the Khoisan peoples of southern Africa were descended from an earlier population he called the "Boskop race" (after the fossil human cranium found at Boskop near Potchefstroom, Transvaal, in 1913). This concept was supplanted by newer, nontypological approaches focused on the history of southern African peoples.

Dart's name will always be associated with the discovery of the Taung skull and his prescient recognition of its significance. At a time when discoveries in Java and China had led to the view that Asia had cradled mankind, there fell into Dart's hands in November 1924 a cache of fossil bones encased in hard calcified breccia. They had been recovered from a dolomitic limestone cave between Kimberley and Mafeking, on the edge of the great Kalahari desert. At that

stage the only fossilized apes found in Africa had come from much further north. Dart spent six weeks working on one of the specimens, whose partly-exposed, natural endocranial cast had immediately struck him as being too large to be a baboon's (it was a fossilized baboon skull that had originally drawn Dart's attention to Taung). He laid bare the superbly preserved skull and "brain-cast" of a child of perhaps five years old. Although the endocranial cast was no bigger than that of an ape of comparable age, it showed a number of man-like features, as did the teeth, especially the small canines. Dart (1925a) published an account of the skull and proposed to make the creature the type-specimen of a new genus and species, *Australopithecus africanus* ("southern ape of Africa"). He recognized it as a higher primate that was not a pongid (or member of the zoological family of the apes): in its departures from the ape's structure, it had moved decidedly in a human direction, despite its ape-sized brain. He even demonstrated that the head must have been held on a virtually upright spine.

The 32-year-old Dart's claims met with almost universal hostility. Nor did it help for him to remind his colleagues that Charles Darwin in 1871 had predicted that such ancestral forms were more likely to be found in Africa than anywhere else. For a quarter of a century the place of *Australopithecus* was in dispute, but Dart maintained his position. Later adult specimens were found by Robert Broom at Sterkfontein and Wilfrid E. Le Gros Clark of Oxford University showed not merely that the South African higher primates were not apes, but that they were members of the hominids or family of men, a view later to become widely accepted.

Dart and his students and staff were responsible for finding another cave site containing australopithecine fossils—Makapansgat in the northern Transvaal. Long before such finds of very early hominids had started to emerge from east Africa, Dart had been responsible for a remarkable series of fresh advances:

(1) He had corroborated Darwin's old prediction that Africa would prove to have cradled mankind;

(2) He had forced upon a reluctant world the realization that a creature with a brain no larger than that of a modern ape (recent man's brain size is three times that of an ape) could nonetheless show signs of moving in the human direction;

(3) He had shown that the principle of *mo-*

saic evolution had applied to these early claimants to human ancestry—that is, that some parts of the body (e.g., the teeth and the postural mechanism) had hominized in advance of other parts;

(4) He had shown that the particular pattern of mosaicism evinced by *Australopithecus* was totally at variance with that prognosticated by his old mentor, Elliot Smith, who had held that brain enlargement must have been in the vanguard of hominization. Instead, brain enlargement was not evident in *Australopithecus*, whereas dental and postural hominization were!

Dart's appreciation and interpretation of the hominoid traits of *Australopithecus* were the most important breakthroughs in paleoanthropology of the twentieth century; next to it, all the other African discoveries represented a mere filling-in of details. Dart lived to see his early claims vindicated. His claim that the australopithecines had used as tools the bones, horns, and teeth of the animals they are presumed to have eaten was, however, strenuously resisted by most paleoanthropologists. Some believe that in this concept Dart made another seminal contribution to the understanding of early stages in the hominization process.

Considerable recognition was accorded Dart in many parts of the world; in his honor the Institute for the Study of Man in Africa was founded at the Witwatersrand University to foster the study of man in Africa, past and present, in health and disease. A Raymond Dart lectureship was also established by that institute.

PHILLIP V. TOBIAS

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DAVIS, KINGSLEY

Born in 1908, Kingsley Davis, demographer, sociologist, educator, analyst of urban phenomena, and Latin American specialist, can be best described as a social demographer. Once the relatively narrow field of formal demography was thoroughly explored, it was inevitable that its range would be expanded to include the causes and consequences of population phenomena in their full economic, social, and cultural contexts. The credit for setting this broader perspective cannot be assigned to any one person or nation, but in a list of pioneers Davis would be close to the top. *The Population of India and Pakistan* (1951b) contains all the analysis of fertility, mortality, and migration that its title would lead one to expect, but also well-rounded chapters on caste, religion, education and literacy, the characteristics of cities, and population policy. In his words, the book attempted an “analysis of social organization and social change . . . a contribution to the sociology and economics as well as to the demography” of the two countries. The nondemographic elements, far from being diversions from the main theme, are fully integrated parts of a book conceived in a spirit of what might be deemed a new discipline—social demography.

Others who have tried to combine social with demographic analysis typically started with population and found the murky field of sociology difficult to master. Davis moved in the opposite direction. His undergraduate major at the University of Texas was English; he took avidly to the study of literature, joined the Scribblers Club, and edited two of the campus literary magazines. He mastered a clear style uncluttered with jargon, the means of communication on a wide range of subjects to both professional peers and the general public. It was he who coined such terms as “the population explosion,” “the demographic transition,” and “zero population growth”—so graphic and felicitous that they have become commonplace in analyses at all levels.

After receiving an A.B. (1930), Davis continued graduate work at the University of Texas in philosophy, economics (under Everett Hale and Clarence E. Ayres), and sociology (under Carl Rosenquist and Warner Gettys). With a

master's degree in philosophy (1932), he entered Harvard University and initially concentrated on sociological theory under Talcott Parsons and the study of the family under W. Lloyd Warner. Both as a graduate student and subsequently as a junior member of the faculties at Smith College and Clark University, he participated in Talcott Parsons' famous discussion group, joining there such later prominent sociologists as Émile Benoit-Smullyan, Robert Bierstedt, Edward Devereaux, Robert K. Merton, and Logan Wilson. When Parsons' *The Structure of Social Action* appeared in 1937, Davis had already received his doctoral degree (1936), but he used the book in graduate seminars and thus improved his grasp of social theory. After joining, as chairman, the sociology department at Pennsylvania State University (1937–1944), Davis received a postdoctoral fellowship from the Social Science Research Council to study statistics, mathematics, and demography, partly under Samuel A. Stouffer at the University of Chicago, partly at the Bureau of the Census. He also did field work in the region surrounding the mountain town of Lares in Puerto Rico.

With this rich and diverse preparation, Davis went to Princeton University, first with the Office of Population Research, then half-time in the Wilson School of Public Affairs, ultimately in the economics department as associate professor of anthropology and sociology, charged with starting a program in those two fields. He brought to Princeton Wilbert E. Moore, Paul K. Hatt, Melvin Tumin, Edward Devereaux, Marion J. Levy, Jr., and Harry Bredemeier—men who soon made the department one of America's best. There, and subsequently at Columbia University (1948–1955) and the University of California at Berkeley (1955–1977), the wide perspective of social demography characterized his classroom teaching. For instance, nearly two decades before ecology became a fad, Davis was devoting a tenth of the assigned reading in his basic course to a highly sophisticated treatment of ecological relations as these affect the quality of life. In his teaching no less than in his writing, Davis' demographic inquiry was a means to a fuller understanding of human behavior and social structure, as well as a criterion with which to assess policies designed to improve human welfare.

The subjects of Davis' first published papers were sexual behavior, the family, parent-child relations, and thus (but almost incidentally) fertility—all analyzed in a neo-Parsonian manner.

That is, he took topics ordinarily discussed emotionally and moralistically and tried to show why certain all but universally condemned practices persist. One "function" of prostitutes, for example, is to provide a sexual outlet for men lacking a legitimate one. Illegitimacy, similarly, is not merely a "social problem" but the absence of legitimacy, the principle that (following Bronislaw Malinowski) he defined as the functionally necessary bond linking father and child into a socializing agency, the complete family. Colleagues found these pieces stimulating and often amusing, but some of them reacted differently to a piece that Davis and Wilbert Moore wrote on stratification, for in this case it was the conventional beliefs of academic liberals that were subjected to the same astringent analysis. In spite of repeated invocations over the centuries to social equality, why is it that a class structure persists? It survives all efforts to erase it, Davis and Moore wrote, because the persons carrying out tasks most important to "societal survival," especially if these tasks demand high intelligence and long training, must be rewarded with "great prestige, high salary, ample leisure, and the like" (Davis & Moore 1945). In the seemingly endless debate, opponents challenged whether in real societies values were in fact distributed according to this principle of justice, but the more acerbic reaction was to the authors' irreverent dismissal of the dogma of equality.

The high point of this early phase was a first-rate text, *Human Society* (1949), which drew heavily on ethnographic data to illustrate general principles of societal analysis. Davis' skill in exposition was—as it should be—almost invisible, and the functional theory that set the volume's content was also beneath the surface. As he developed skills in statistical and demographic analysis, Davis became less enamored of purely verbal theory, less confident that cultural patterns determine behavior, and more interested in social-economic and demographic theories that can be exemplified, documented, or tested with numerical data.

One of Davis' enduring interests was cities. In 1946, he wrote, with Ana Casis, "Urbanization in Latin America." At Berkeley he organized the International Population and Urban Research Unit, which sponsored international conferences—for example, the set of papers later published as *India's Urban Future* (Seminar . . . 1962)—and published a series of 17 monographs. Most importantly, Davis and his associates issued a remarkable set of statistics on the world's cities,

a considerable improvement over those published by the somewhat larger and wealthier United Nations, and in a second volume used the figures to analyze *World Urbanization, 1950-1970* (1969-1972). In 1971, he became Ford professor of sociology and comparative studies at the University of California at Berkeley, and in 1977, the distinguished professor of sociology at the University of Southern California.

While producing a long list of notable publications and teaching many students indebted to his meticulous pedagogy, Davis found time to advance his profession in time-consuming functions. He was a member and then chairman of the Division of Behavioral Sciences of the National Research Council when the NRC was beginning to extend its range from physical to social sciences. As the first sociologist elected to the National Academy of Sciences, he took pains to see that he was not the last. He was president of both the American Sociological Association and the Population Association of America, and he worked in comfortable tandem with anthropologists and economists. His frequent efforts to explain social trends to the lay public culminated in a very fruitful relation with the publishers of *Scientific American*.

Kingsley Davis has had several overlapping careers. His works on India, even years after their publication, are still cited in India as authoritative. From the first field work in Puerto Rico, he developed an increasing knowledge of Latin America. Several of his books and articles were translated into Spanish, and two of his better papers were in that language—"Apreciación crítica de Malthus" (1951a) and "Las causas y efectos del fenómeno de primacía urbana" (1962). He had competence in every social discipline except psychology. Yet his life's work was not dispersed; it shows a remarkable unity with a clear line of development, linked by persistent interests and a clear-sighted search for consistent excellence.

WILLIAM PETERSEN

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DOBB, MAURICE H.

Maurice Dobb, one of the foremost Marxist economists of the twentieth century, was born in 1900, at a time when the main flow of innovative Marxist thought was moving away from economic analysis toward cultural theory, philosophy, and aesthetics. At the time of his death in 1976 the tide had turned, and once again political economy was attracting widespread attention, not only in Marxian studies *per se*, but also in economics in general, where the obvious failure of neoclassical orthodoxy to explain major economic problems, and the exposure of logical fallacies in its analytical framework, led many to turn to classical and Marxian ideas. In this process Dobb had played a central role as both scholar and publicist. For many years he was, with Paul M. Sweezy, the only significant Marxist economist writing in the West. Their work in difficult periods, such as the late 1940s and the 1950s, was a guide and inspiration to later generations.

Dobb was attracted to the political left, to Marxism, and to the study of economics as a reaction against the mindless chauvinism of the World War I era, and by the contrast between Western economic failure in the aftermath of the war and the hopes inspired by the 1917 Soviet revolution. He studied economics at Cambridge University and at the London School of Economics, where he received a PH.D. in 1924 for a thesis on the history and theory of capitalist enterprise. He returned to Cambridge at the end of 1924 as a lecturer in the faculty of economics and politics, from which he retired, as a reader, in 1967. While in London Dobb had joined, in 1922, the recently formed Communist party of Great Britain. The dominant elements in his life's work were to be a profound intellectual grasp of orthodox economic theory, a commitment to the over-all development of Marxian thought in the social sciences in general and in economics in particular, and a belief in the necessity of social revolution in the interests of economic welfare and human dignity.

A major part of Dobb's work was devoted to the construction of a satisfactory theoretical framework, built on Marxian foundations, for the analysis of capitalism. Since a theory of value must stand at the core of any theory of capitalism, Dobb devoted much of his career to the study of contemporary theories and their historical development. The unusual combina-

tion of a deep understanding both of Marxian analysis and of orthodox ideas was to lead to a curious ambivalence in Dobb's early writings, for while he regarded Marxian theory as a superior system of social thought, he apparently still believed the neoclassical orthodoxy to be *logically* sound. Thus his rejection of the neoclassical theory of value was based on its failure to comprehend the anarchy of an individualistic system (1925), its inadequate representation of the institutional structure of capitalism (1928*b*), and the essentially vacuous character of a subjectivist theory of the sphere of circulation—in contrast to an objective theory grounded in the reality of the process of production (1937). Some uncertainty may also be found in his discussion of the logical foundations of Marxian analysis (see Bharadwaj 1978). However, following his collaboration with Piero Sraffa on the *Works and Correspondence of David Ricardo* (Sraffa 1951–1973), and the publication of Sraffa's *Production of Commodities by Means of Commodities* (1960), Dobb's position changed. For not only was the "Ricardo–Marx tradition in economics" rehabilitated on a sound logical footing; but also the neoclassical orthodoxy was shown to be incapable of providing a logically sound theory of the determination of the rate of profit, and hence incapable of analyzing capitalist markets. Although a methodological critique remains an important part of Dobb's *Theories of Value and Distribution* (1973), the emphasis is on the logical structure of the surplus approach to the theory of value and distribution, exemplified by the works of Ricardo, Marx, and Sraffa.

Dobb's attitude to the other major theoretical innovation in twentieth-century economics, John Maynard Keynes's theory of effective demand, was somewhat equivocal. Although not unaware of the significance of Keynes's ideas Dobb regarded them to be of fundamentally less importance than the rehabilitation of the Marxian approach; indeed, they were incomplete unless allied with a satisfactory theory of value (Garegnani 1978–1979). Dobb was not involved in the Cambridge debates that surrounded the construction of Keynes's *General Theory of Employment, Interest and Money* (1936) in the 1930s, being heavily committed to the antifascist movement and similar political work.

While the theory of value was a major theoretical concern, Dobb also developed other aspects of Marxian theory, notably the theory of

crises, and made important contributions to the history of economic thought in which he stressed the historically relativist character of economic thought, both in the choice of problems analyzed and in the social "vision" embodied in the formal framework of theory.

Early in his career, Dobb's political interest dictated an attempt to understand and interpret the new Soviet society. In 1927 he completed *Russian Economic Development Since the Revolution* (1928a), working with a translator, since he did not then read Russian. In this and successive revised editions, Dobb charted the path of Soviet development and analyzed the theoretical approach adopted by Soviet planners. The book has subsequently been criticized for ignoring the harsher realities of the Soviet experience, but at the time it was well received. Keynes wrote to Dobb in 1927 that it was "a most valuable and original work. . . . It gives a picture of what has really been happening in Russia such as has not been available before to English readers." Dobb emphasized the advantages of centralized planning over the anarchy of the market, a basic theoretical position he was to maintain against the seductive charms of so-called "market socialism." But he became increasingly aware of the institutional problems inherent in a centralized state: "The centralised system will itself have bred attitudes and habits of work of its own, together with a structure of relationships between administrative levels that may exercise a strongly conservative resistance to change and to the adoption and cultivation of new attitudes, relationships, and methods" (1970, p. 62). Nonetheless, he was confident that "even if there be signs of a freezing of bureaucratic structure . . . it is hardly likely that the new technological age and higher living standards can be contained within the old administrative mould inherited from Stalin's day" (*ibid.*, pp. 68-69).

Dobb's interpretation of the Soviet experience was to be of major importance in his analysis of development in the Third World. In his famous Delhi lectures (1951), in which he identified economic development with industrialization, he advanced the then novel propositions that in a surplus labor economy investment can be increased without depressing *average* consumption, and that decisions on investment must involve both the structure of investment as between sectors and the time pattern of production. In the more complete statement of his views (1960),

he concentrated on what he felt to be the central issue—the relationship between the generation of surplus, the structure of the labor force, and the choice of technique. The problem was analyzed in an essentially static framework, with a focus on the choice of the optimum capital-labor ratio in a model with a (paradoxically) neoclassical flavor. While today emphasis would be placed more on the dynamics of development and the relationship between technological and social change, Dobb's emphasis on industrialization points to the key to the attainment of economic independence in the Third World.

Dobb's interest in development was not confined to analysis of socialism and of the Third World. Perhaps his most innovative and influential application of Marxian analysis was in an area in which he was a self-confessed amateur. In his *Studies in the Development of Capitalism* (1946), he not only analyzed the history of capitalist development but also posed the vital question of the nature of the transition from feudalism to capitalism. He utilized Marx's notion of a mode of production as a combination of social relations (in particular property relations) and technical forces to provide an interpretation of the "organic" collapse of feudalism. The interaction between material and social forces in feudal society had led to crises in East and West, producing in the East, greater repression, and in the West, the irreversible development of capitalism. This analysis emphasized the role of class struggle in the development and overthrow of the feudal mode of production, in opposition to Marxist and other arguments dependent on technological determinism or the role of "external forces." Dobb's position has been the basis for a continuing debate (see Hilton 1976).

Dobb's career was devoted to the *development* of Marxian analysis as a political tool. A confirmed opponent of dogma in both intellectual and political affairs, he gained world-wide influence through his care and consideration as a teacher, the wide range of his writings, and his unique position as a Communist prominent in the Western economics profession. He was, however, a quiet and modest man. His intellectual commitment and personal modesty are illustrated by his work with Sraffa on Ricardo's *Works and Correspondence*. Given Sraffa's well-known reluctance to set pen to paper, the necessary editorial introduction was accommodated by a procedure in which Dobb and Sraffa discussed

each paragraph, Dobb wrote it up, Sraffa revised it, Dobb rewrote it, and so on. The editorial introduction to Ricardo's *Principles* proved a vital step in the development of understanding of the role of the labor theory of value in classical analysis, and in the provision of logically sound foundations for the Marxian theory of value and distribution.

The breadth and penetration of Dobb's contributions could, perhaps, have been made only by an innovative thinker using the Marxian tradition of social thought, a tradition of which Dobb's work now forms an important part.

JOHN EATWELL

WORKS BY DOBB

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DOBZHANSKY, THEODOSIUS

The modern understanding of the evolutionary significance of genetic variation within and between human groups rests almost entirely on the work of Theodosius Grigorievich Dobzhansky. Although his research was devoted chiefly to other species, particularly natural populations of *Drosophila*, the views he developed on genetic variability in natural populations, and on the evolution of geographical races, had a profound effect on physical anthropology.

Theodosius Dobzhansky was born in Nemirov, Russia, on January 25, 1900. He was the son of a high school teacher, the grandson of a Russian Orthodox priest. After his university training at Kiev (his only doctorates were honorary degrees), Dobzhansky began an extensive study of variation in natural populations of *Coccinellid* beetles distributed across Europe and Asia. His first contribution to the study of genetic variation, published in 1924, described the polymorphism of color patterns in these insects. This work was important for Dobzhansky's own development and generally for the modern view of genetic variation. In the study, some species showed the same frequencies of genetic variants over their entire range, while others showed marked geographical variation in frequencies of different phenotypes. Instead of taking the conventional view—naming morphological races and subspecies and describing the geographical range of each—Dobzhansky maintained that there was in each instance a single polymorphic species, and that the problem was to understand the pattern of geographic variation in terms of various evolution-

ary forces. The change from viewing each phenotype as a "race," to be named and mapped, to viewing the species as a genetically segregating population, or series of populations, with no single "wild-type," was fundamental. It led to Dobzhansky's concept of the Mendelian population, a geographically coherent group of individuals exchanging genes, which is now the fundamental concept of evolutionary genetics. This was a major departure from the typological view that a species could be broken into a few well-defined races on a purely phenotypic basis, with no reference to the breeding structure or genetics of the species.

Like most middle-class intellectuals of the time, Dobzhansky supported the February revolution but opposed the Bolsheviks, and in 1927 he left the Soviet Union never to return. He joined T. H. Morgan's group at Columbia University, which had for some years been the center of development of classical genetics, and began his studies of the cytogenetics of *Drosophila* and of the causes of sterility in species hybrids. When Dobzhansky began his work on the genetic differences between species, the species concept was primarily a typological and taxonomic one. Using chiefly morphological characters, taxonomists tried to define the stage of differentiation at which races should be regarded as species. At the same time it was understood that there were a variety of barriers to crossing between species. They included failure to mate; inviability of hybrid embryos if mating did occur; or sterility of hybrid adults if development of hybrids succeeded. Yet there was no understanding of the relationship between these observations and the problem of differentiation of races and species, because the problem of species definition was seen from a static, rather than a dynamic, viewpoint. It was Dobzhansky who saw, from the geneticist's standpoint, that the critical issue was the passage of genes from one population to another. In 1935 he provided the biological definition of species that remains the underlying concept of species formulation at present. Dobzhansky defined the species as "that stage of evolutionary divergence at which the once actually or potentially interbreeding array of forms becomes segregated into two or more separate arrays which are physiologically incapable of interbreeding" (1935, p. 354).

The essence of this concept is that a continuous process of genetic differentiation between geographically separated populations finally results in a stage of total genetic isolation. This

stage is a critical one, however, and is qualitatively different from mere race formation, because after the point of speciation the units are genetically and evolutionarily independent.

Dobzhansky's work on cytogenetics, species hybrid sterility, and his studies of genetic polymorphism in natural populations converged in the production of a synthetic theory of evolutionary genetics published in 1937 as *Genetics and the Origin of Species*. His most influential work, it is the point of departure for all subsequent synthetic treatments of the evolutionary process. *Genetics and the Origin of Species* begins with the observations of classical genetics: the production of genetic variation by mutation and chromosomal rearrangements. These types of variation are then related to the variation observed within and between natural populations. The dynamics of the variation is explained in terms of theoretical population genetics, especially the stochastic theory of Sewall Wright. For the first time evidence is brought together from a variety of sources that natural selection actually operates to mold the genetic composition of populations. Dobzhansky's successful melding of the stochastic theory of gene frequency change with the observed facts of genetic variation was the first real synthesis in biology of a complex theoretical structure with a large body of observation and experiment.

The second half of *Genetics and the Origin of Species* concerns the development of species differences by the acquisition of genetic barriers to gene exchange. As a basis for the speciation process, there is a large amount of genetic and cytological variation within populations. The variable populations then become spatially differentiated either through natural selection, which favors different genes in different environments, or through the interaction of selective forces with random processes of accidental differentiation. Genetic differentiation in space results in geographical races which are incipient species. Whether or not these geographical races continue to diverge to the stage of speciation when they can no longer exchange genes depends upon the balance between selection for divergence and the homogenizing effect on all populations of migration and of common selective pressures. Human populations, for example, have diverged from each other in the past to form a large number of genetically differentiated local populations which can, more or less arbitrarily, be grouped into major "races." But this differentiation has not and cannot lead

to speciation because of greatly increased migration, because selective forces have become increasingly homogenized spatially, and because populations have become so large that random differentiation is very weak.

Beginning in 1935, and until his death in 1975, Dobzhansky devoted his scientific work to the study of genetic variation within natural populations, chiefly of *Drosophila*, but also of humans. His studies of chromosomal polymorphism in *Drosophila* populations convinced him that sexually reproducing species were highly polymorphic genetically and that the typical individual in such a species was heterozygous for most of its genes. He regarded the main causes of this genetic variation to be the balancing forces of natural selection, in particular the superior fitness of heterozygous individuals. The central core of this theory was developed in a series of 43 papers on "The Genetics of Natural Populations," which attempted to document the genetic polymorphism of *Drosophila*, to measure the force of natural selection maintaining the polymorphism, and to apply the full apparatus of population genetic theory to the study of natural populations. Dobzhansky stood at the center of an international school of evolutionary geneticists devoted to his view of the genetic structure of species. This "balance school," as Dobzhansky called it, was opposed by the "classical school" of H. J. Muller who believed that species were chiefly monomorphic genetically, except for a "load of mutations" that were deleterious and that were constantly being swept out of the population by natural selection. Muller regarded the load of mutations as limiting the reproductive potential of a species; he feared, for example, that the human species might be extinguished if radiation-induced mutations increased the genetic load. Dobzhansky, to support his position, attempted to show metabolic polymorphism in humans by studying patterns of excretion of amino acids, but this technique proved too crude for the purpose. In fact, no technique available during most of Dobzhansky's lifetime was capable of deciding the issue, and an acrimonious debate on the meaning of a series of ambiguous experiments marked population genetics for many years. Recent work on the polymorphism of enzymes has shown that Dobzhansky was correct in his assertion that most organisms, including the human species, are highly polymorphic genetically. Whether or not this polymorphism is maintained by hetero-

sis and other forms of balancing selection is yet to be determined.

Dobzhansky and Muller both agreed that different human abilities were largely genetically determined. For Muller these abilities were on a graded scale from bad to good, and he promoted a program of selective breeding through artificial insemination that would eliminate the inferior genotypes and increase the superior ones. Dobzhansky's general view on polymorphism and heterosis led him to the conclusion that genetic variation in ability was advantageous to the species as a whole, that polymorphism was adaptive. Moreover, since he believed that heterozygotes were the superior genotypes, no program of eugenics could concentrate these types. These views, together with the argument that differences in ability should not be differentially rewarded, are summed up in Dobzhansky's major statement on human polymorphism, *Genetic Diversity and Human Equality* (1973). It is ironic that the struggle between Dobzhansky and Muller was based upon a common assumption for which neither had any evidence. It is still not known whether differences in human skills, abilities, and temperaments have any genetic basis at all, or whether they are the consequences of developmental accidents and cultural conditioning.

Dobzhansky's theory of race and species formation and his study of polymorphism led to a complete reorientation of physical anthropology. Like the rest of biology, physical anthropology had long been typological and taxonomic in its approach. Using morphology, anthropologists described and classified human racial types, the number of "races" varying from five to more than one hundred, depending upon the prejudices of the observer. Catalogs of photographs of racial types were produced, and even Dobzhansky, in *Mankind Evolving* (1962), gave serious consideration to these classifications. Although it was recognized that migration and interbreeding had produced mixed groups, even these hybrid entities were implicitly regarded as stable types to which typical descriptions could be applied. If any evolutionary dynamic was assumed, it was a branching process by which one racial type gave rise to another in a phylogenetic tree like the phylogenetic trees of species.

With the destruction of typological systematics in biology, a reorientation in anthropology began. Morphological traits were progressively deemphasized because their genetic basis was

complex and uncertain. Instead, anthropologists began to measure the frequencies of simple Mendelian genetic characters—e.g., blood groups and other biochemical traits. But the characterization of a local human population in terms of gene frequencies rather than morphological traits changes the entire emphasis of description. Frequency is a characteristic of an ensemble rather than an individual, so that it becomes impossible to pick a “typical” individual to represent the group. Local populations are observed to be mixtures of different genotypes, and what characterizes the differences between populations is differences in relative frequencies of the different genotypes. The concept of pure race also becomes untenable. Since, for any of the genes studied, two human populations do not differ absolutely, but only in relative frequency, no “pure” types exist. It also becomes impossible to delineate major races, since it is purely arbitrary which level of frequency difference is to be regarded as defining race. Indeed the concept of race has virtually disappeared from modern anthropology.

A second effect of Dobzhansky's view of the polymorphic species was a change in the problematic of anthropology. From a purely descriptive and taxonomic science, physical anthropology became a branch of population genetics. The problem of physical anthropology came to be an explanation of the causes of human polymorphism. The entire apparatus of theoretical population genetics has been brought to bear on the explanation of genetic differentiation between groups. It has become important to characterize the actual breeding structure of a village or tribe; thus, data from cultural anthropology on kinship systems and marriage patterns are now important to physical anthropology. The study of genetic differentiation at the level of neighboring villages, as for example among the Yanomamo and Makritare, is now the mode for anthropology. As a consequence, physical anthropology has passed from being a simple consumer of evolutionary genetic theory to a primary producer of it. A major justification of the study of primitive peoples and cultural isolates at the present time is that they are expected to provide tests of general evolutionary and population genetic theories.

What is so remarkable about the revolutionizing influence of Dobzhansky on the study of human variation is that it is based entirely upon the extension of ideas developed outside of an-

thropology. Except for the ill-fated attempt to study excretory patterns of amino acids in the early 1950s, Dobzhansky never made any attempt to study human genetic variation directly; and the Institute for the Study of Human Variation, which he founded together with L. C. Dunn in 1951, was short-lived and unproductive. Dobzhansky recognized, however, the significance of population genetics for the study of the human species and carried out an active campaign of propaganda among anthropologists and students. Anthropologists, in their turn, seized on population genetics as a way to break out of the sterile taxonomy of classical studies, and to regenerate the study of human evolution.

RICHARD C. LEWONTIN

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DODD, STUART C.

From the time Stuart Carter Dodd began collaborative efforts with George Lundberg until the time of Dodd's death at the end of 1975, he symbolized for many sociologists the pursuit of an elusive Holy Grail: social science modeled on physical science and approaching the latter's precision and predictive power.

The son of a medical missionary, Dodd was born in 1900 in Talas, Turkey. After serving as a psychologist at the State Home for Boys in Jamesburg, New Jersey, and earning a B.S. magna cum laude in 1922, an M.A. in 1924, and a Ph.D. in psychology in 1926, all at Princeton University, he received a postdoctoral fellowship from the National Research Council and studied in London. From 1927 to 1947 he was

affiliated as a sociologist with the American University of Beirut, where he developed and directed that university's Social Science Research Section. His teaching there was interrupted when he and his family were evacuated during World War II, and he served for a time as director of surveys with the U.S. Army in Sicily. He was decorated by the Republic of Lebanon "for Humanitarian Services," and his eldest son subsequently taught sociology at Beirut.

From 1947 on, as research professor of sociology at the University of Washington, Dodd directed the Washington Public Opinion Laboratory (1947–1961), and was a dedicated teacher of methodologically oriented seminars until he became professor emeritus in 1971. During his four years of retirement, he continued to express in writing his vision of science. Over his lifetime he published some 150 papers in a wide array of journals.

The major theme of Dodd's academic career was a lifelong effort to promote a system of symbols and to contribute to a pattern of language use that he believed would make the social sciences more scientific. In addition, he also pursued a range of other causes, all, in his view, related to his major objective. In the final year of World War II, he proposed the establishment of an international polling agency to provide a "barometer of international security" that could facilitate efforts by the United Nations to preserve peace and foster human progress. In pursuit of this goal he helped organize the World Association of Public Opinion Research. As president of the Pacific Sociological Association in 1953, he advocated applied social research in a presidential address entitled "Can the Social Scientist Serve Two Masters?" In 1960/1961 he served as vice president of the American Humanist Association, after proclaiming in a mimeographed circular to its membership the importance of an "emerging science of Axiology" and affirming his belief that "measuring man's pursuit of goals helps make evolution more purposive." In 1966 he took time to give enthusiastic assistance to a group of students who were devising measurement techniques for a campuswide course critique.

Dodd came to the attention of Lundberg through publication of *A Controlled Experiment on Rural Hygiene in Syria* (1934), which Lundberg cited in the second edition of his textbook on research methods ([1929] 1942), calling the

first 38 pages of Dodd's monograph a "model of clear orderly statement of the methods of a research project." The two men met when Lundberg visited the Middle East, and, as a result of their discussions, Lundberg's *Foundations of Sociology* (1939) and Dodd's *Dimensions of Society* (1942) were published as "companion volumes," although publication of the latter was delayed by the war.

Two papers by Dodd, effectively previewing *Dimensions of Society*, appeared in 1939. The first, "A Tension Theory of Societal Action," remains one of the best examples of Dodd's contribution to useful reconceptualization in sociology. It provided a unique clarification of the nature and interrelations of such processes as conflict, competition, and accommodation, an unusually lucid analysis of Malthusian theory, and a perceptive comparison of Nietzschean, Buddhist, and Western philosophies. Except for the use of presubscripts (in addition to the more conventional postsubscripts) in Dodd's equations, nothing in the first three-fifths of the article was particularly unconventional or difficult to follow, and even the more difficult section on "second order processes" was illuminating. The second paper, "A System of Operationally Defined Concepts for Sociology," moved farther from customary sociological thoughtways. It succinctly outlined the "S-theory" that he elaborated subsequently in *Dimensions*, and it remains the most palatable first-hand overview of Dodd's ambitious attempt to rebuild the sociologist's vocabulary.

Dodd sought to synthesize the description and analysis of all social situations by an equation, $S = T; L; P; I$, where S stood for situation, T for time, L for linear distance, P for population, and I for indicators of the innumerable characteristics of people and their environments. The semicolon could stand for any of the mathematical operators—addition, multiplication, and so on. Further, each "dimension" in the formula could be modified by an exponent. In the case of L , for example, if the exponent were 1, then the base letter designated a length; if the exponent were 2, it designated an area; if it were 3, a volume. Thus, a population density—a ratio of people per unit area—was P^1/L^2 , which could also be written P^1L^{-2} . Each base letter or dimension could also be modified by three other corner scripts, the postsubscript designating a class, the presubscript designating class intervals, and the presuperscript designating a case.

The exponents, however, were the most im-

portant of the corner scripts. A process—involving a characteristic changing in time—could be represented as P^1T^{-1} . Different processes would involve different amounts of different characteristics, but all would share the -1 exponent on T , showing they all were a kind of “velocity.” Similarly, a rate of natural increase in a population would be recognized as a velocity when written P^1T^{-1} . A *change* in a rate of natural increase would be P^1T^{-2} —the time period *increment in the rate* of population increase per time period. Thus, any formula containing T^{-2} represented an acceleration.

Dodd believed that sociologists would find their efforts to classify social phenomena became more scientific if they categorized situations according to whether or not the formulas for those situations shared the same sequence of exponents. He even took qualitative variables into account by assigning them a zero exponent (so that their quantity—taken as 1—did not modify any product in which they entered as a factor).

Reviews of Dodd's work were mixed but generally negative. Not all criticisms were cogent; of those that were, the one that most affected his subsequent writings was the charge that what matters most to sociologists was left to his residual category, *I*. Several reviewers warned that it would be inadvisable for sociologists generally to emulate Dodd or adopt his notation. One critic noted that Lundberg's valuable natural science approach for sociology might be neglected if it were identified with Dodd's S-theory (Calhoun 1942, p. 504). Others denied that the S-system accomplished what Dodd claimed for it (Parsons 1942; Shanas 1942). One reviewer, however, wrote that it was more likely than any alternative to make sociology scientific (Robinson 1942, p. 454), and another held that it would be profitable for any sociologist to read and evaluate *Dimensions* (Calhoun 1942, p. 504). A mathematician who reviewed the book called S-theory “a feeble mathematical pun” (Bell 1942, p. 709), and one author of a textbook on sociological theory later called Dodd's work “almost unreadable” but credited him with “a tour de force” in his use of the zero exponent (Timasheff 1955, pp. 308, 309). Another theory text cited *Dimensions* as “an example of the danger always facing pure positivism of degenerating into an empty formula” (Martindale 1960, p. 119). Howard B. Woolston, reviewing the book in the *Annals* of the American Academy, suggested that “perhaps a brief

handbook presenting typical problems and solutions might be more useful for students, and offer a definite basis for trial by investigators” (1942). Dodd's approximation to such a handbook was entitled *Systematic Social Science* (1947), a typescript volume reproduced by offset in a “temporary edition” as a publication of the Faculty of Arts and Science of the American University of Beirut.

Convinced that his scientific language would become more widely adopted if offered in more piecemeal form, Dodd intended, when he went to the University of Washington, to spend the next twenty years publishing segments of his system in journal articles. At the University of Washington, he also directed a series of team research projects, the largest and most notable being Project Revere, a three-year study for the U.S. Air Force during the Korean War that explored various aspects of communication through airborne leaflets. A number of able graduate students were trained under Dodd's direction, and many research papers by him and by them were published, but his hope that a multivolume “definitive treatise” would eventually be published setting forth sociologically fundamental findings from Project Revere (comparable in his mind to the World War II series by Samuel A. Stouffer and others on *The American Soldier* [1950]) remained unfulfilled.

In 1961 the Washington Public Opinion Laboratory became the Institute for Sociological Research, and Dodd relinquished the directorship. His efforts turned increasingly to the development of a set of ideas that had fascinated him earlier (1944; 1950*b*). By now he had broadened his interests beyond the boundaries of the behavioral sciences, and began to apply his symbolic analyses to cosmological questions in an endeavor he called “Project Epicosm.” In an unpublished memorandum written in 1967, Dodd spoke of all research as “the search for . . . optimal symbolizing,” and alluded to “formulas which so describe as to explain and predict” all parts of the cosmos.

Dodd's style of thought and his approach to science derived in part from his bilingual childhood in Turkey. A fascination with classification, with categorical distinctions, and especially with logical products, arose from his early acquaintance with contrasts between a highly agglutinative and a highly analytic language. This fascination was nurtured further by his pleasure in studying French and Latin in school, and by modest encounters with Greek,

German, Italian, Arabic, and Esperanto. Later, as a psychology student, he readily embraced Karl Pearson's conception of statistics as the "grammar of science" and easily viewed all of mathematics as a language.

His worldview emphasized sets of elements interacting in random combinations. It was doubtless influenced by his efforts as a young man to combine C. E. Spearman's and Godfrey H. Thomson's different approaches to the factor analysis of human intelligence. Dodd's essentially religious approach to science, rooted in his missionary heritage, drew also from the ideas of Henry Nelson Wieman and was reinforced by his exposure to the cosmological writing of Harlow Shapley. The work of Percy W. Bridgman provided a basis for Dodd's devotion to operational definitions. His conviction that physical science offered promising models for social science drew support from admired works by J. Q. Stewart and George K. Zipf. The writing and counsel of George Lundberg was the chief sociological influence upon Dodd's thought, and Lundberg remained until the end of his life Dodd's staunch defender, close friend, and sternly constructive critic.

WILLIAM R. CATTON, JR.

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DOLLARD, JOHN

John Dollard's training in sociology, anthropology, psychoanalysis, and behavior theory has enabled him to integrate psychoanalytic and learning principles with concepts relating to

society and its culture. Displaying an exceptional concern for the wholeness of knowledge, he has derided the "ologies" that describe man from many different and segmented perspectives, thus creating a "sociological" or an "economic" man. In Dollard's view, there is just one man and there should be one science that deals with all of him, flesh, blood, and culture. Thus he has been a pioneer in the interdisciplinary integration in behavioral science.

Born in Wisconsin in 1900, John Dollard was the eldest son of the seven children of James E. and Ellen Brady Dollard, of third generation Irish stock. After graduation from the University of Wisconsin in 1922, Dollard became a fund raiser for the Wisconsin Memorial Union. Work for the union brought Dollard into contact with Max Mason, the physicist and inventor, who became his model.

When Mason was appointed president of the University of Chicago, Dollard joined his staff as an assistant. The University of Chicago was an exciting place, and Dollard enjoyed the sense of being at the forefront of intellectual change. Mason introduced Dollard to William Fielding Ogburn, who gave him his first scientific training and became his thesis sponsor in sociology. The subject of Dollard's dissertation was "The Form and Functions of the Early American Family" (1931). Both at Wisconsin and Chicago, Dollard took mathematics courses that served him well in learning statistics and the use of experimental methods.

After receiving a PH.D from Chicago, Dollard was awarded a Social Science Research Council fellowship to study psychoanalysis in Germany. His analyst was Hans Sachs, one of the "seven rings" of Freud's circle.

When Dollard returned from Berlin, his friend Edward Sapir brought him to Yale University to assist in the direction of a one-year international seminar on the impact of culture on personality. After the seminar, Dollard was hired by Mark A. May, director of the Institute of Human Relations at Yale. This was a teaching as well as a research institution, and thus appropriate for a person with Dollard's broad training. Nevertheless, he paid a price for crossing traditional disciplinary boundaries, being a peripheral member of two other departments before he was belatedly accepted into full membership in the department of psychology.

At the institute, Dollard had a stimulating relationship with the psychoanalyst Earl F. Zinn, and with social scientists such as George

P. Murdock and Allen Gregg, who worked for the Rockefeller Foundation. He also met Neal E. Miller, a young psychologist who had studied psychoanalysis. Miller taught Dollard the principles of reinforcement learning theory based on Clark L. Hull's synthesis of the thinking of Edward L. Thorndike and Ivan P. Pavlov. Combining research and theory on learning with social science observation, Dollard and Miller developed the conditions-principles thesis—that human learning occurs according to certain biopsychological principles under conditions arranged by society. This theory disregards disciplines and centers on how man's habits are established and changed by social conditions.

Dollard's first book, *Criteria for the Life History* (1935), was a call for unity in the life history field. The goal was to show how the biological substratum of an individual, with all its innate urges and potentialities, was transformed into the observed social and cultural being. It established seven criteria for the effective life history and judged a number of documents according to them. None met the test of the seven criteria because they neglected either biological or cultural factors. Margaret Mead described this book as "a landmark in the study of personality and culture" (Mead 1936).

Of all of Dollard's books, *Caste and Class in a Southern Town* (1937) is the most widely read. It was a six-month study of the color-based caste system that barred children of a black-white union from legitimate descent and locked blacks into a lower social and economic class. In his research in "Southerntown," Dollard noted prejudice and hostility along caste lines, overt in whites and latent in blacks. He concluded that the system was designed for the benefit of the white population and that it conferred certain prestige, as well as economic and social gains, on the white caste. When it was first published, this book aroused considerable hostility among many whites, including scholars, and at one time it was banned in Georgia and in South Africa. The changes in the legal and social status of blacks are due in part to the long-term effects of the early insights provided by this courageous, pioneering study.

Dollard's second book relating to the problem of race relations, *Children of Bondage* (1940), was coauthored by the social anthropologist Allison Davis. This work analyzed seven life histories of black youths in New Orleans and Natchez in an effort to discern the psychological and social variables in the lives of black chil-

dren. Although the authors discovered internal class and color lines within the black caste, all types of personality variables seemed to be represented and no evidence appeared of genetic differences between the castes. The research for this book gave Dollard the opportunity to use social class analysis as developed by W. Lloyd Warner.

During the 1930s, Dollard and his colleagues at the Institute of Human Relations began interdisciplinary research into the problems of frustration and aggression (Dollard et al. 1939). Dollard had already formulated the hypothesis that "the usual human response to frustration is aggression against the frustrating object" (1937, p. 267). The institute encouraged further work on this theme because it helped to explain a wide range of psychological, sociological, and cultural observations.

Dollard's special interest, however, was in learning theory and analysis. He and Miller stressed the importance of the principles and social conditions of human learning and investigated the orderly way in which human culture is learned in the more than five thousand different cultures on record (Miller & Dollard 1941). To understand human behavior, one must know both the principles of learning (including the nature and demands of the organism) and the conditions of culture and society. This view is a point of departure for a unified social science. Miller and Dollard supported their thesis with experiments on children and animals, and they discussed the economy of imitation as a way of hitting on the "right response" in an early trial. This work has encouraged others to study social learning and to study aspects of imitation, such as the importance of the prestige of the model.

With the outbreak of World War II, Dollard pursued a project that he and Miller had developed for a book on fear in battle. Dollard conducted a questionnaire study of veterans of the Abraham Lincoln Brigade in the Spanish Civil War to gather information that might be of use in training servicemen in World War II. The salient point of the book (1943) was that everyone is afraid, and that differences in behavior result from more or less successful ways of learning to cope with fear in combat.

Dollard considers his best book to be *Personality and Psychotherapy* (Dollard & Miller 1950). It attempted to synthesize knowledge about the principles of learning derived from experiments in the laboratory with knowledge about the social conditions of learning derived from studies

of culture, and to use this knowledge to explain natural-history observations of human behavior in psychotherapy. The book also analyzed the ways in which different aspects of neuroses can be learned, repression can interfere with reasoning and thought, and psychotherapy can teach the patient to discriminate between real and imaginary dangers, recover the use of higher mental processes, and learn more adaptive social behavior. By demonstrating in concrete detail that psychotherapy is a process of social learning and emotional reeducation, this book helped to lay a scientific foundation for the participation of psychologists in psychotherapy. Although it did not establish any school of psychotherapy, it strongly influenced generations of psychotherapists.

In *Scoring Human Motives* (Dollard & Auld 1959), the authors constructed a system to correlate and code the content of psychotherapeutic interviews with a score for each sentence based on symbols for components in the patient's speech and the therapist's comments in transcripts of recorded interviews. Such emotions as anxiety, hostility, and resistance are scored, as are interpretations by the therapist. Conscious and unconscious events are identified separately; and reliability of scoring is reported. Dollard strongly believes that this type of study is the only way to bring recorded therapy interviews into the domain of science.

In the period from 1960 until his retirement in 1969, Dollard and his research associate, Alice M. White, waged a long and stubborn battle with the problem of coding psychotherapy content. They tested both the reliability and validity of their coding system and published a number of papers related to the measurement of psychotherapy content.

Throughout his career, Dollard's research has creatively integrated contributions from four separate disciplines. If this no longer seems to be a daring and novel enterprise, it is a measure of his long-term influence on the field of behavioral science.

NEAL E. MILLER

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DOUGLAS, PAUL H.

Paul H. Douglas (1892–1976) made lasting contributions to labor economics during his academic career, 1916–1948 and 1966–1969, and he achieved greater distinction in his political career, most notably as a United States Senator, 1948–1966, than any academic economist in American history. Douglas began his career as a labor economist at a time when economics was sharply divided between the deductive methods of neoclassical theory and the inductive methods of the historical and institutional schools. He became a leading figure in the transformation of economics into an empirical science based on econometric testing of micro- and macroeconomic theories.

The transformation of economics occurred between 1920 and 1950, and Douglas' contributions to measurement and econometric estimation in labor economics were unmatched. His research on the theory and measurement of income distribution, the labor demand function, the labor supply function, and on the measurement of wages and unemployment inspired several generations of labor economists and is still cited today.

In addition, Douglas contributed significantly to the scholarship on many institutional aspects of labor markets. This work was enriched by the

knowledge he gained from his activities in social reform causes, campaigns for economic and welfare legislation, and arbitration in collective bargaining.

The academic, public, and personal sides of Douglas were closely interwoven. He merged his economic research with his interests in public policies, and imbued both with his personal feelings. His life was remarkably diversified.

Biographical sketch

Douglas was born in Salem, Massachusetts, on March 26, 1892, but his childhood home was in Maine. He grew up in frontier conditions, and less than a year and a half of his elementary education was spent in a classroom.

In 1913 Douglas graduated from Bowdoin College with honors in economics. About his college studies he wrote: "John Stuart Mill became an inspiration, and has remained so throughout my life" (1972, p. 24). Indeed, their lives had interesting similarities. Both were famous economists who engaged in social reform movements and who, late in their careers, were elected to their nation's highest legislative office. Douglas, like Mill, attempted a difficult task for a superior neoclassical economist: namely, that of advocating and rationalizing a liberal and quasisocialistic role for governmental intervention in the economy.

Douglas took his graduate work at Columbia University. There, John Bates Clark, one of the founders of the marginal productivity theory, provided Douglas with the training in neoclassical economic theory that became the basis for Douglas' later work in production theory. Douglas was, however, repelled by the strain of social Darwinism that was then so much a part of neoclassical economics. He was sympathetic to trade unionism and protective labor legislation. His mentor at Columbia in these interests was Henry R. Seager, whose work in labor legislation in New York paralleled the work by John R. Commons and his followers in Wisconsin. In 1915/1916 Douglas spent a year in graduate studies at Harvard University, where Frank W. Taussig strengthened his training in neoclassical economics. Douglas received his doctorate in 1921. His dissertation was published with the title, *American Apprenticeship and Industrial Education* (1921).

Douglas was an instructor in economics at the University of Illinois, Reed College, and the University of Washington before joining the University of Chicago in 1920, where he re-

mained until 1948. During a three-year leave of absence at Amherst College, 1924–1927, he collaborated with a mathematician, Charles W. Cobb, on work on production functions. After his third term in the U.S. Senate ended in 1966, Douglas taught at the New School for Social Research in New York (1966–1969). He was 77 years old when poor health forced his retirement.

Douglas married Dorothy Wolff in 1915. She was the daughter of a prominent New York banker, and she became an academic sociologist and an activist in social reform movements. After their divorce in 1930, he married Emily Taft, the daughter of the famous sculptor, Lorado Taft. Her active life included careers as an actress, a social activist—particularly in civil rights causes, a congresswoman representing Chicago (1944–1946), and an author of several books on the feminist movement.

Douglas' life as a social reformer began in his student days. At Bowdoin he opposed the fraternity caste system, and while at Columbia he was arrested once for participating in a union organizing campaign among retail clerks. After his conversion to the Quaker religion and his move to Chicago in the 1920s, Douglas was so frequently and deeply involved in reform causes that his scholarly achievements during these same years became all the more impressive. He worked with Jane Addams, the American Friends Service Committee, and, in 1928, with a national citizens committee for the socialist candidate for president of the United States. From the late 1920s to 1940, Douglas participated in the advocacy, and often the subsequent drafting, of legislative proposals for social welfare: a family allowance plan (which he preferred as an antipoverty measure to minimum wage laws), the Illinois State Utilities Act of 1933 (following his work in exposing the corrupt practices of Samuel Insull), the Illinois Old Age Pension Act of 1935, and the Illinois Unemployment Insurance Act of 1937. At the federal level he served on commissions on consumer protection and the social security system.

Douglas won his first political election in 1939 when he became alderman from Chicago's famous maverick fifth ward. Although a Democrat, Douglas fought against the notorious corruption in the Democratic city administration as vigorously as he had opposed corruption in the Republican city administrations during the 1920s. However, on rare occasions, he could work with the Democratic political machine, and in a strategy that was prophetic of his

future political career, he was actually supported by the Democratic machine in his 1939 victory. In 1942, on the other hand, the machine candidate defeated him in the primary for the U.S. Senate.

Although he was fifty years old, Douglas joined the marines as a private and served in combat in the South Pacific in World War II. He received the Bronze Star for heroism and was twice wounded.

In 1946, he returned to the University of Chicago, where he remained until his election to the U.S. Senate in 1948. He was dissatisfied with the department of economics during his last two years, partly because the laissez-faire orientation of the department clashed with his political liberalism. Douglas was considered the underdog in the Senate general election in 1948, but that was the year of the political upset. He won second and third terms easily in 1954 and 1960, but in 1966 he was defeated in his last political campaign by Charles H. Percy.

As a senator, Douglas was often characterized as a champion of lost causes, who was frequently at odds with Senate leaders in his own party. In an article in *The New York Times*, Charles Kaiser referred to the paradox of his combination of the "moral rectitude of New England . . . with crusading liberalism," and the article quotes Newton Minnow, who said: "Douglas was a liberal who was also simultaneously concerned about what things cost—that was a rare combination" (Kaiser 1976). But it should not be rare for a first-rate economist, even a liberal, to apply the cost-benefit calculus to government policies. In fact, Douglas as a senator was an enemy of waste in government and of pork-barrel legislation generally (see Douglas 1952*a*; 1952*b*). His professional skills as an economist were also evident in his battles, mostly futile, for tax reform, but also when he was on the winning side of free trade, conservation, and truth-in-lending legislation. On the other hand, most economists would question his legislative efforts on behalf of two per cent interest rates for the Rural Electrification Agency, large federal subsidies to housing, restrictions on branch banking, and price regulation of natural gas.

Douglas as an economist

Two principal themes stand out in Douglas' economic research. One was his desire to use economics for understanding and dealing with social problems. The second was his goal of

bridging the large chasm between textbook theory and real world data. In his generation he ranks with Wesley C. Mitchell and Henry Schultz in this important accomplishment.

Production theory and the demand for labor. Douglas is most renowned in the literature of economics for his work on production theory. His outstanding contribution was not so much the "discovery" of the famous Cobb–Douglas production function, but his pioneering estimation and application of it to major issues. Douglas reconciled two important real world phenomena: the *estimated* shares of labor and capital in national *production* and the *observed* shares of labor and capital in national *income*. His methodological technique is still widely used, and even his numerical estimates of these shares, 30 to 35 per cent for capital and 65 to 70 per cent for labor, have held up over time in innumerable applications (see Douglas 1948; Tobin 1973, p. 439).

It is difficult to determine what standards are appropriate for examining the robustness (or stability) of Douglas' parameter estimates, however. The estimation consisted simply of a linear function (in logarithms) in which indexes of output were regressed on indexes of labor and capital inputs. The functions for separate industries within countries often had widely varying parameter estimates, and these did not always correspond closely to earnings shares. With national aggregates as the units of observation and with variables measured in physical quantities, the aggregation and measurement problems are overwhelming. Crude data are often fit as well by linear functions as by more complicated functions. When the variables are in monetary units, the variation in prices on both sides of the equation introduces simultaneity in the relation, and estimation by least squares is probably inappropriate. The regression function loses the simple interpretation of one-way causality it had when the variables were physical quantities.

Douglas' first paper on the Cobb–Douglas production function was presented at the American Economic Association meetings in 1927. In his presidential address to the AEA in 1947, he surveyed the work done in the field during the preceding twenty years (Douglas 1948). Approximately 25 years later, when Douglas was an emeritus professor at the New School, he returned to empirical research on production functions. His work on production functions therefore spanned 45 years.

The main body of Douglas' early work is in his classic book, *The Theory of Wages* (1934). James Tobin (1973) described it as a "major vindication of the marginal productivity theory," which carried the following ironical implication. The correspondence between the production and income shares of labor seemed to imply that not much could be done by minimum wage laws, unions, or other interventions in production to change the distribution of earnings. Yet, Douglas became an ardent champion of minimum wage laws, unions, and various governmental interventionist strategies. The tensions between a neoclassical analysis of the workings of the economy—Douglas the economist—and an advocacy of an interventionist strategy—Douglas the reformer and politician—were never thoroughly discussed or analyzed in Douglas' writings. Albert Rees (1979) suggests that Douglas saw business monopoly and monopsony, along with prevailing unemployment, as impediments to a reliance on laissez-faire policies.

Two additional ironies concerning Douglas and the Cobb–Douglas production function are worth noting. One stems from the development of the function in the hands of mathematical economists and econometricians, who discovered all sorts of convenient and illuminating mathematical properties in it and extended it to applications in utility theory. However, there is not the slightest indication that Douglas understood or even cared about the substance of any of this work. Until the late 1960s Douglas was unaware of the popularity of the Cobb–Douglas production function in economics literature, and then he expressed surprise and gratification, and remarked: "A seedling that I had planted long ago, but left untended, had been cared for by younger men and in the fullness of time became a sturdy tree" (Douglas 1972, pp. 614–615). The other irony is that although the function carries Douglas' name, it was probably discovered or invented much earlier by Knut Wicksell of Sweden and by Phillip H. Wicksteed of England.

Labor supply. Douglas' work on production functions was his contribution to the theory of the demand for labor. His work on the supply of labor was also influential and laid the foundation for the important modern work by Clarence D. Long, H. Gregg Lewis, and Jacob Mincer. Douglas' supply research first appeared in *The Theory of Wages*. In the best-known part of this research on labor supply, Douglas and his co-workers regressed labor force participation

rates for a large number of age-sex groups on real wages for a number of large cities, using data from the 1920 and 1930 censuses.

There are several notable points about this work: (1) Relatively stable negatively sloped supply curves of labor were estimated. The cross-section relationships were reconciled, therefore, with the observed time series negative relationship between the labor supply and real wages. (2) For men, the elasticities of labor supply with respect to real wages were low— -0.1 to -0.2 —numerical estimates that have held up for the last 40 years. (3) For women, two lasting insights that emerged were the importance of the industrial structure in the labor market and the effect that the dual role of women in both home and market production had in increasing the elasticity of the female labor supply function. However, the most serious flaw in Douglas' labor supply research concerned women. The primary fact about the female labor supply function emerging from modern research is its large positive wage elasticity, whereas Douglas found a large negative elasticity. Douglas' mistake was his failure to separate income effects (as measured by male earnings) from wage effects (as measured by female wage rates) in the function. The positive wage effect has been found to be dominant, and, in fact, this effect is a major explanation for the increase in market work by women over time (see Mincer 1962). Douglas' female labor supply function was both theoretically misspecified and inherently an unstable empirical relation. (4) Perhaps the most methodologically profound aspect of Douglas' work was his grasp of the distinction between the abstract labor supply model of price theory and the various labor supply models that apply to varying circumstances in the real world (Douglas & Schoenberg 1937, pp. 78–79).

The measurement of wages and unemployment. The empirical testing by Douglas of economic theories of the labor market had to await the construction of an appropriate data base, and the chief architect of this was Douglas himself. Wage series were not compiled by the Bureau of Labor Statistics in the first part of the century, and cost-of-living indexes were nearly nonexistent. Douglas examined real wage trends and real wage differentials from 1890 to 1926, a period of rapid economic growth, huge waves of immigration, and sporadic spurts of union growth. His book, *Real Wages in the United States: 1890–1926* (1930), was a major work

in its own right, and it established the foundation for *The Theory of Wages*, published four years later.

Douglas' contributions to the study of unemployment are several. In his scholarly works in support of unemployment insurance programs, the role of unemployment benefits as an automatic stabilizer over the business cycle is developed. While a senator and member of the Joint Economic Committee he developed an unemployment measure that was an alternative to the conventional unemployment rate; specifically, a measure that included part-time workers who want full-time jobs. The measure has appeared regularly in the "economic indicator" series of the Joint Economic Committee, and it is a forerunner of the current emphasis on multiple measures of unemployment (see Shiskin 1976). Douglas wrote several books on unemployment and depressions, but his macroeconomic theories are not well developed. Despite his interventionist position regarding the role of government and his advocacy of public employment programs to combat unemployment, Douglas was more a monetarist than a Keynesian. He was one of the first senators to invite testimony from Milton Friedman, and there is an ironical note to Douglas' indirect role in the resurgence of the monetarist position in the federal government.

Paul H. Douglas had many careers—economist, reformer, soldier, and politician. Each was marked by superlative achievements. Tobin's tribute (1973 p. 438) is fitting: "Any one [career] alone would fill with distinction the lifetime of a man of unusually prodigious talent and energy. Men of Paul Douglas' range, intensity, and dedication are not just unusual; they stride across the national scene only once or twice a generation."

GLEN G. CAIN

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DUBOS, RENÉ

René Dubos, biological scientist and social philosopher, was born in France in 1901. He emigrated to the United States in 1924 and became a citizen in 1938. He has been associated with the Rockefeller University for more than fifty years, and since 1971 has been emeritus professor of environmental biomedicine.

Raised in small farming villages of the Ile de France, Dubos moved with his family in 1913 to Paris. In 1919 he won a scholarship to the Collège Chaptal where he became interested in human geography and social history. Despite a professor's encouragement to pursue a career in social history, Dubos decided upon agronomy as a more secure livelihood. He received a degree in agronomical sciences at the Institut National Agronomique in 1921, and soon obtained a post as a research reviewer for the International Institute of Agriculture in Rome.

Two years later on board a ship bound for the United States, he chanced to meet for the second time Selman A. Waksman, an agricultural bacteriologist at Rutgers University. As a result of the re-encounter Dubos enrolled at Rutgers, and earned a doctorate in agricultural chemistry and bacteriology in 1927. Almost immediately thereafter he began his long affiliation with the Rockefeller Institute for Medical Research, later the Rockefeller University. For 14

years he worked in the laboratory of Oswald T. Avery, best known for his chemical identification of DNA (deoxyribonucleic acid). In *The Professor, the Institute, and DNA* (1976), Dubos writes not only of Avery but of the intellectual and scientific stimulation of the institute as environment—its genius loci. His own professional development was influenced by Avery, Alexis Carrel, Alfred E. Cohn, Thomas M. Rivers, and others through the ease of collaboration with a variety of biomedical disciplines. Early in his affiliation, Dubos won international acclaim for his discoveries related to the successful use of soil microbes in the treatment of disease.

After a two-year leave as George Fabyan professor of comparative pathology and professor of tropical medicine at the Harvard University School of Medicine, Dubos returned to the institute in 1944 to direct his own laboratory. He has written more than 200 scientific papers, numerous articles for specialists and lay persons, and 21 books. He holds 39 honorary degrees, including 3 honorary medical degrees. In addition to many scientific honors, Dubos won the Pulitzer Prize in 1969 for *So Human an Animal* (1968*b*). Since his retirement, Dubos has continued an active career as writer, lecturer, and educator. As author, he is best known for his books on the relations between the environment and health/disease, science in society, and an evolutionary, adaptive perspective on the social problems and opportunities of contemporary life. Dubos' contributions to social thought are characterized by three major intellectual themes: an interest in the relations between things rather than in things as independent entities, an emphasis on human choice and free will, and a concern for the humanizing of science.

Relations between human beings and environments. Dubos' earliest work in soil microbiology was already ecological. His doctoral work was shaped by his interest not in the microbe alone, but in the soil as milieu for the microbe and in how its nutriment and acidity affect the nature of its microbial population. Later, his medical research was rooted in the relations among microbe, individual host, and the population in which the host is embedded. A personal tragedy led to further development of Dubos' systemic ideas. After his first wife died of tuberculosis in 1942, Dubos' interests turned to studying the relations between social conditions and susceptibility to tuberculosis. His subsequent work

reflects Dubos' growing environmentalism as first he explored the relations between environment and infectious disease, then between environment and disease in general, and finally, the environmental influences at work in all aspects of human life.

For Dubos, the environment comprises the natural, the built, and the social worlds. It is anything that evokes an active response in the human being (passive reactions to environmental conditions occur, of course, at the physico-chemical level). People's active, and often creative, responses to the environment change it to conform to human needs and goals, sometimes enhancing and sometimes degrading the environment in the process. In either event, humans must then adapt to the very changes they have created, as well as to the constant changes occurring in the environment as the result of nonhuman forces. The extreme adaptability of humans poses a danger in that some environmental changes induced by human activity have negative consequences for the quality of human life and for human biological and psychological functioning, based as it is on a high degree of genetic stability since the Stone Age.

Because human beings have remarkable biological plasticity and a low degree of biological specialization, Dubos gives more weight to the role of the environment in the nature-nurture interplay. For him, it is not so much that environment determines behavior as it is that past responses condition subsequent responses to the environment. More importantly, the environment offers options to humankind: men and women are shaped by the selections they make among many possible physical and social environments. In general, Dubos' position is that over evolutionary time, certain features have been built into the human genome. These programs of potentialities, however, must be activated by appropriate environmental stimuli at the appropriate time if they are to become functional attributes. Hence, Dubos' major humanistic thesis calls for the creation and sustainment of environmental diversity so that the multiple potentialities in human children and adults may be released.

Free will. Dubos' convictions concerning human choice and purpose distinguishes his position from the environmental emphasis of B. F. Skinner and the genetic emphasis of the sociobiologist Edward O. Wilson. He abjures behaviorism on the basis that willful actions influence

environmental conditioning, and sociobiology on the grounds that social factors are more important than genetic factors in history and in individual lives. Human beings are not merely reactive to environmental conditions or merely impelled by genetically programmed behaviors. Human development and functioning is the outcome of the dynamic interplay between both, *plus* the exercise of free will and deliberate choice based on values, knowledge, and experience.

Dubos views the acceptance of two seemingly incompatible notions—determinism (genetic-environmental interplay) and free will—within Niels Bohr's conception of complementarity. Both determinism and free will are needed to understand the richness and complexity of human life. While obeying the laws of genetic-environmental determinism, humans at the same time transcend them. By virtue of the great complexity of the information programs in their DNA, humans are the most teleonomic of all organisms. Within the constraints of a given culture, they make choices and take action guided by awareness of past experience, aspirations for the future, and value judgments. Thus human beings have influenced the course of their evolution as well as their individual development. Human freedom arises in good measure from social evolution (culture) which, though different in kind, is itself the result of organic evolution. Like Paul Tillich, Dubos sees humans as most human at the moment of choice.

Although Dubos, like other biologists, does not address the traditional philosophical issues in free will, he does recognize the presence of the unconscious and irrationality in human affairs. And, more explicitly, he is deeply concerned about the constraints on human freedom imposed by the social forces of poverty, racism, malnutrition, poor housing, and all forms of social injustice that stifle the expression of individual potentiality. Yet, for Dubos, the very existence of free will "makes it possible to envisage a future world in which human society, despite the genetic stability of the species, can be improved by changing environmental forces and ways of life" (1974a, p. 32). Thus the Promethean capacity for deliberate choice carries responsibility for creative stewardship of the earth, including the design of social environments that will foster the realization of human and environmental potential. Admittedly, this is an anthropocentric position, but ecological

values are necessarily anthropocentric: what is "really" good for human beings is good for the rest of nature.

Science in society. Dubos' concerns about science arise inevitably from his holistic perspective on human-environment relations and his emphasis on free will. In his judgment, the biological sciences have continued for too long their analytic focus on elementary structures and processes, and have failed to study human life in its wholeness. Without discounting the remarkable achievements of biology, Dubos believes that to continue solely to study the human being as a fragmented and isolated object will limit science's contributions to human welfare. He advocates, instead, studies of the nonlinear, closed-loop feedback systems that maintain and those that undermine the goodness-of-fit between human beings and environments. Such studies are likely to uncover the multiple and so far unexpressed potentialities of both, and will therefore complement the more mechanistic studies that are limited to the predictive aspects of the human organism. They can lead toward the anticipation and then prevention of short- and long-term consequences of technological innovations and to an understanding of free will. Science will then have extended the rational basis for the exercise of human purpose, choice, and action.

The social philosophy of Dubos is best summed up in the title of his regular column in *The American Scholar*, "The Despairing Optimist." Once pessimistic about humanity's ability to stave off ecological disaster, Dubos has taken comfort from the growing public concern, a spreading commitment to social values among scientists, world-wide efforts to develop alternative sources of energy, new social designs and life styles, and plans to conserve the wilderness and improve rural and urban land use. His own manifold contributions to each of these developments have helped point to new directions in the life sciences and the social sciences, and have helped shape new objectives and methods in such diverse professions as architecture, engineering, medicine, social work, and urban planning.

CAREL B. GERMAIN

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DUMÉZIL, GEORGES

In the early decades of the present century, as a result of the eclipse of Max Müller's "solar mythology" (see Dorson 1955), the science of comparative mythology—especially comparative Indo-European mythology—reached a low ebb. Narrowly focused research into the specific Indo-European traditions—Greek, Italic, Celtic, Indic, and the like—became the order of the day. Yet the basic questions to which Müller and his adherents had addressed themselves remained unresolved, and in 1920 a young Indo-Europeanist, Georges Dumézil, set out to find a new and viable framework in terms of which these problems might once again be approached;

problems posed by the obvious thematic, if not etymological, parallels among a great many ancient Indo-European gods and heroes.

Born in Paris in 1898, Dumézil had attended the Lycée Louis-le-Grand and later the prestigious École Normale Supérieure. After serving as an artillery officer from 1917 to 1918, he returned to his studies at the University of Paris where, in 1924, under the direction of the eminent Indo-Europeanist Antoine Meillet, he completed his doctoral thesis. Entitled *Le festin d'immortalité: Étude de mythologie comparée indo-européenne* (1924), it marked the beginning of one of the most distinguished scholarly careers the twentieth century has yet witnessed.

Dumézil's initial attempts (e.g., 1924; 1929) to develop a "new comparative mythology," grounded as they were in James G. Frazer's (e.g., 1890) now largely discredited theory that religion everywhere reflects an attempt to renew the world by periodically killing and replacing kings and other persons symbolic of divine beings, ultimately proved unsuccessful (see Littleton [1966] 1973, p. 56). By 1938, however, he had made a major discovery and had begun to draw upon a wholly different theoretical base. The discovery was that several ancient Indo-European speaking communities were characterized, at least in their earliest periods, by a tripartite social class system that broadly resembled the three Aryan or "twice-born" castes of classical and later Indian society (Dumézil 1930). The new theoretical base was the sociology of Émile Durkheim and his followers, to which Dumézil was introduced by Marcel Granet. Although it is unfair to characterize Dumézil as a full-fledged Durkheimian (his fundamental training was in philology and the history of religions), he nevertheless came to adopt one of Durkheim's most important axioms: that important social and cultural realities are "collectively represented" by supernatural beings and concepts (see Durkheim 1912).

In a remarkable series of books and essays written during the course of the next decade (1938–1948), Dumézil successfully combined the newly discovered evidence for social tripartition, Durkheimian sociology, and the traditional techniques of comparative philology, and arrived at a comprehensive model of the common Indo-European ideology—that is, the cognitive structure in terms of which the ancient Indo-European speakers ordered their social and supernatural universes. As presently understood, the salient features of this ideology are

threefold: (1) the maintenance of juridical and sacerdotal sovereignty; (2) the exercise of physical prowess; and (3) the promotion of physical well-being, fertility, wealth, etc. Each of these three principles forms the basis of what Dumézil calls a *fonction*, or "function"—a complex whole that includes both the ideological principle itself and its numerous social and supernatural manifestations.

The clearest example, perhaps, can be seen in Vedic India, where the "first function" (sovereignty) was manifested in the Brahman caste and its two primary collective representations, Mitra (the juridical aspect) and Varuna (the sacerdotal aspect). The "second function" (physical prowess) was manifested in the Kshatriya caste (the military/ruling elite) and its primary representation, the great war god Indra. The "third function" was reflected at the social level by the Vaishyas (the herder-cultivator caste) and at the divine level by the twin Ashvins, or "Divine Horsemen," as well as by the goddess Sarasvatī, all of whom were principally concerned with physical well-being, fertility, etc.

At least some evidence for the presence of similar social and supernatural triads can be found in almost every other ancient Indo-European speaking tradition from Italy to Iceland. Moreover, it eventually became clear that the three functions are reflected in an extremely wide variety of cultural phenomena, including triads of epic heroes (see Wikander 1947; Littleton 1970), threefold categories of diseases (see Puhvel 1970), and even tripartite conceptions of physical space (see Molé 1952; Dumézil 1968–1973, vol. 1).

At first glance, Dumézil's approach might appear similar to that espoused by his fellow countryman Claude Lévi-Strauss (see, for example, Lévi-Strauss 1964). However, there are some important—indeed fundamental—differences between them. Unlike Lévi-Strauss, Dumézil does not suggest that the tripartite cognitive structure he has discovered is a universal feature of the human psyche. On the contrary, the ideology in question is, in the Old World at least, uniquely Indo-European, and is therefore necessarily bounded in time and space. Thus, the two French scholars are working at wholly different levels of abstraction (see Littleton 1974).

However, if the specific structural phenomena that Dumézil has brought to light are by definition limited to the Indo-European domain, the method he has developed does have some

general implications for social research. It may be that most if not all of the world's major language families, such as the Uto-Aztecan, the Malayo-Polynesian, the Bantu, the Hamito-Semitic, etc., are, or were at some point in their histories, characterized by analogous genetically related ideologies, each with its own unique set of "functions" (see Littleton [1966] 1973, pp. 232–233).

To be sure, Dumézil's theories and methods have not met with universal approval, and there are those who suggest that on occasion he has imposed the tripartite model on data that are perhaps amenable to other interpretations (see Littleton [1966] 1973, pp. 186–202). But despite these criticisms, Dumézil, who, until his retirement in 1968, was professeur de civilisation indo-européenne at the Collège de France (the position was created for him in 1948), has been eminently successful in his lifelong effort to develop a "new comparative mythology," and the implications of what he has achieved are profound indeed for the social sciences as a whole.

In November 1978, in the autumn of his eightieth year, Dumézil was elected to the Académie française.

C. SCOTT LITTLETON

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E

EGGAN, FRED

Frederick Russell Eggan has been a centripetal force in mid-twentieth century anthropology. His scholarly work fuses the structural-functional approach developed in British social anthropology with the concern for cultural historical processes characteristic of mainstream American anthropology. His synthesis avoids both the narrowness and ahistoricity inherent in the British position and the fragmentary and diffuse problem focus of much American work. He has played a strategic role as a teacher, administrator, and spokesman, consistently advocating an integrated, holistic approach to anthropological understanding.

Born in Seattle in 1906, Eggan grew up in the Chicago area, attended public schools, where he displayed an aptitude for science and mathematics, and enrolled in the College of the University of Chicago. He majored in psychology, but exposure to courses in geography rekindled his childhood interests in distant places and peoples. He took his first anthropology course with Fay-Cooper Cole. Cole, a Boas-trained ethnologist who later devoted himself to Midwestern archeology, was in the process of building a major anthropology department at Chicago. As an undergraduate and beginning graduate student, Eggan also took courses with the brilliant Edward Sapir. Although his aptitude for linguistic transcription was limited, Eggan retained from Sapir's teachings certain conceptual models of long-range cultural process and an appreciation of the psychological dimensions of culture.

Eggan completed his undergraduate work in 1927 and continued as a graduate student in psychology but took many of his credits in anthropology. Under the supervision of L. L. Thurstone, the eminent psychometrician, he produced a master's thesis, "An Experimental Study of Attitudes Toward Race and Nationality" (1928), that combined his psychological and anthropological interests. However, survey data, numbers, and calculators proved less exciting than archeology and ethnology, so Eggan decided to pursue a career in anthropology. Lacking funds, he taught psychology, sociology, and history for two years at Wentworth Junior College and Military Academy in Missouri. During the summers he worked on Cole's archeological field projects.

Eggan returned to Chicago in 1930 as a full-time graduate student in anthropology. Courses with Leslie Spier aroused his interest in kinship studies and southwestern ethnology. He planned to write a dissertation on the relationships between ethnological and archeological cultures. The topic was premature, but Eggan later did produce a perceptive paper (1952) on this subject when more reliable archeological data became available. In 1931 Cole hired A. R. Radcliffe-Brown to replace Sapir, who had departed for Yale University. Eggan attended Radcliffe-Brown's course on family, kin, and clan, and was impressed by the Englishman's erudition and fresh theoretical orientation. Radcliffe-Brown was highly critical of most work done by American anthropologists and advocated the synchronic study of social structures as systems of action and meaning. Social structures, he

maintained, could be studied as functioning wholes, and nomothetic principles of organization could be deduced. Radcliffe-Brown came to America with a program for reanalyzing the social structures of North American Indians along the lines that he had recently developed in his Australian work (1931). Eggan became Radcliffe-Brown's research assistant and pored through the literature on North American kinship to produce extensive summaries of what was known and what remained to be done. In the summer of 1932 Eggan participated in a summer field school directed by Leslie A. White and began his lifelong association with the Hopi. Material from that field trip provided the basis of his 1933 doctoral dissertation, a structural-functional analysis of Pueblo social organization.

Eggan remained affiliated with the University of Chicago as a postdoctoral research associate. He returned to the southwest to collect additional material and also did brief field work with the Mississippi Choctaw and with the Cheyenne and Arapaho in Oklahoma. These latter experiences led to the publication of "Historical Changes in the Choctaw Kinship System" (1937*b*), which utilized ethnohistorical data to demonstrate the systematic influence of acculturation in modifying kinship behavior and terminology. Hypotheses about similar systematic changes among other southwestern tribes were substantially confirmed in later work by Eggan's student Alexander Spoehr (1947). Results of the Cheyenne and Arapaho research appeared in a sweeping essay prepared for the *Social Anthropology of North American Tribes* (1955), a *Festschrift* for Radcliffe-Brown that Eggan edited. Here the kinship systems were related to other features of social organization, and comparative analysis suggested broader interpretations about the development of the Plains' social systems. This essay clearly anticipated the relevance of what Eggan was later to label "the method of controlled comparison."

In 1934-1935 Eggan was sent by Cole and Robert Redfield to investigate problems of culture change in the northern Philippines. The Tinguian area was chosen, since Cole had done extensive ethnographic work there twenty years earlier; Redfield wanted to see if some of the general processes of change that he had recently discerned in Yucatan could be replicated in another area of Spanish contact. Aided by Cole's practical ethnographic knowledge of the area, Eggan made a rapid adjustment to field work

and collected voluminous material on social organization, ceremonialism, and dreams. However, he had difficulty applying Redfield's model of the folk-urban continuum to the northern Philippine situation. Although European contact had made significant inroads into the area, Eggan emphasized the effect of internal structural dynamics in prefiguring the degree and direction of social change (1941; 1963). He argued that only after the functional integration of the native cultures and social structures had been analyzed, compared, and placed in historical perspective could European influences be properly assessed. Eggan's rich Philippine data have only partially been published.

On his return from the Philippines, Eggan received an academic appointment at Chicago. Much of his energy during this period was devoted to teaching. Earlier interests in historical problems resurfaced, since, as he notes, "with both Radcliffe-Brown and W. Lloyd Warner [and one might add Redfield] on the departmental staff, most of my teaching in the 1930s centered on ethnology and culture history, and I gave ethnographic courses on all the major regions of the world, except for Europe" ([1934-1972] 1975, p. ix). When not in residence, he continued his southwestern field research.

Eggan held a variety of research and administrative posts during World War II. He was a consultant to the Board of Economic Warfare in 1942, after which he became chief of research for the Philippine government-in-exile. Next he was assigned to the School for Military Government. In 1943 he was sent back to Chicago to establish the Civil Affairs Training School for the Far East. Near the end of the war he served briefly as a cultural relations officer for the State Department. These wartime experiences broadened his administrative skills and his range of national and international contacts.

Eggan's academic career flourished in the postwar period. He was promoted to full professor in 1948 and served as departmental chairman from 1948 to 1952 and again from 1961 to 1963. He continued to publish the results of his southwestern and Philippine research. In 1950 a much-revised and expanded version of his doctoral dissertation appeared as *Social Organization of the Western Pueblos*. In this, his most important work, he elegantly summarized the social organization of the Hopi and compared it with four other western Pueblo groups. From this structural-functional analysis he showed a basic uniformity among the western

Pueblos in which matrilineal clans, matrilineal households, and Crow-type kinship systems predominated. This pattern contrasted with that of the eastern Pueblos, whose social organizations featured dual divisions, bilateral kinship systems, and an emphasis on relative age. Forming a bridge between these two types, both structurally and geographically, were the Keresan Pueblos. Thus Eggan demonstrated that underlying the apparent homogeneity of Pueblo culture were distinct types of social structure. He offered historical hypotheses about the way these variations could have evolved from a common type. His reconstruction has been challenged by Robin Fox (1967), who did field work with Keresan groups, but the issues remain unresolved. Eggan's description and analysis of western Pueblo social systems remain a landmark in Americanist studies. In his postretirement years Eggan has sought the ancestral forms of Hopi social organization among linguistically related groups in the Great Basin.

Eggan's 1953 presidential address to the American Anthropological Association, "Social Anthropology and the Method of Controlled Comparison" (1954), articulated the particular synthesis that he effected between British social anthropology and the American historical tradition. In it he called for comparative synchronic studies of geographically proximal, historically related, or typologically similar societies. Such focused comparisons could then be utilized for cultural reconstruction and the discovery of regularities in diachronic process. In the expanded edition of *Social Anthropology of North American Tribes* ([1937c] 1955), Eggan contributed a state-of-the-art essay in which the methods and results of controlled comparison of American Indian societies were masterfully demonstrated.

By the 1960s Eggan had become a senior statesman in anthropology. He continued to publish important articles on the southwest and Philippines, but he was increasingly called upon to write forewords to the work of others, to contribute articles to *Festschriften* of distinguished colleagues, and to serve as a convener, contributor, and commentator on important symposia. Eggan's interests also turned more toward the history of anthropology. He established intellectual kinship with that pioneering student of social organization and the American Indian, Lewis Henry Morgan. A series of articles culminated in his published University of Rochester lectures, *The American Indian: Perspectives for*

the Study of Social Change (1966). This book not only surveyed Morgan's contributions but evaluated his legacy through a selective review of subsequent work on North American social organization.

Eggan epitomized the excellence of anthropology at the University of Chicago during the middle decades of the twentieth century. High standards set by his teaching for two generations of graduate students; quiet diplomacy and administrative talents that help provide an environment for creative anthropological research; service on many scholarly committees; and active membership in honorary societies brought national and international distinction to himself and to Chicago.

Eggan's greatest impact, however, derives from his writings. His clarity of vision, ability to reduce complex phenomena to their essentials with minimal distortion, and capacity to demonstrate productive connections between hitherto disparate approaches and theories have earned him an enduring reputation in the social sciences.

RAYMOND D. FOGELSON

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ELIADE, MIRCEA

Mircea Eliade, historian of religions, orientalist, and man of letters, is noted for his interpretation of archaic and primitive modes of religious experience and expression. His scholarly work manifests an unusual combination of philological–historical research with an intuitive–imaginative approach, perhaps reflecting his double career as scholar and *littérateur*. In the post-World War II era, he attained a predominant position among historians of religion and won a wide audience for his works.

Life and works. Eliade was born in Bucharest, Romania, March 9, 1907. He began his literary career at 14 with a piece on the silkworm in a popular science review, and followed with articles on entomology, alchemy, history of religions, travel, and literature in various Romanian journals. During his student days at the University of Bucharest he founded a short-lived literary journal and regularly contributed profiles, book reviews, and travel pieces to a local newspaper. At the same time, he pursued his

academic career in philosophy and oriental studies, earning his licentiate in philosophy (1928) with a thesis on the Italian Renaissance thinkers, Marsilio Ficino and Giordano Bruno. Inspired by reading the first volume of Surendranath Dasgupta's *History of Indian Philosophy* (1922–1955), he journeyed to India to study under the eminent Indian scholar, aided by a scholarship from the Maharajah of Kassimbar. Arduous textual and linguistic studies at the University of Calcutta and the Asiatic Society of Bengal were interspersed with travels throughout India and a six-month period of intensive Yoga practice and meditation at a Himalayan *ashram* (retreat). Eliade had decided to devote his doctoral dissertation to a comparative history of Yoga, a decision that was to mark the beginning of his illustrious career as a historian of religions. The Indian experience was to have a lasting effect on his work and thought, but, as he noted later, for a complete view of man he would also require what he had learned from the Italian Renaissance, plus an understanding of “primitive” man and popular European folklore. Conversely, he found that what he had learned in India provided insight into peasant cultures generally and into Romanian folk culture in particular.

Back in Bucharest in late 1931, his double career as scholar and writer flourished. After translating his dissertation on Yoga from the original English into Romanian, he received his Ph.D. in 1933, and became an assistant to his master Naë Ionescu, the eminent Romanian philosopher, at the University of Bucharest, where he taught philosophy and history of religions until 1940. His novel *Maitreyi*, an intense erotic–spiritual tale, set in Calcutta and based on his own experience, won first prize in a contest for unpublished novels (1933) and gained him instant literary fame in Romania. He also published other novels, a book on his travels in India, and numerous scholarly articles and books on folklore, alchemy, and the history of religions. His work on Yoga was translated into French (1936) and many of his articles were written in French, English, and Italian, gaining him a wider, non-Romanian scholarly audience. He founded and edited (1938–1942) *Zalmoxis*, a distinguished international journal of religious studies. (Eliade was also later cofounder and coeditor of the journals *Antaios* [1960–1972], and *History of Religions* [1961–].) During World War II, he served as cultural attaché at the Romanian legations in London (1940–1941)

and Lisbon (1941–1945). After the war, when the new Communist régime was established in Romania, he emigrated to Paris rather than return to his own country.

The Paris years (1945–1956) were a period of both great difficulties and great triumphs for Eliade. His personal journal reveals the depression, frustration, and sometimes despair of the exile compelled to write and lecture in a foreign tongue, unable to fulfill his vocation as a Romanian writer, and seeking a place in a strange new environment. However, Eliade's work on Yoga and his scholarly articles were well known and respected by eminent French orientalist and historians of religions. Under the sponsorship of Georges Dumézil, he lectured at the prestigious *École des Hautes Études* on subjects that were the themes of two key works that would later establish his scholarly reputation: the *Traité d'histoire des religions* (1949b) and *Le mythe de l'éternel retour* (1949a). In the early 1950s, he published a series of works that further solidified his position as a leading historian of religions: *Le chamanisme* (1951), *Images et symboles* (1952), an improved, expanded version of *Yoga* (1954), and *Forgerons et alchimistes* (1956a). In a creative rebirth of his fictional powers he also brought forth *Forêt interdite* (1955), regarded as his literary masterpiece.

In 1956, Eliade came to the United States as visiting professor of the history of religions at the University of Chicago, where he delivered the Haskell lectures, later published as *Birth and Rebirth* (1958). In 1957, he became professor and chairman of the history of religions and professor of the committee on social thought at Chicago, where in 1964 he became the Sewell L. Avery distinguished service professor. During the late 1950s and subsequently, his scholarly work, most of which appeared in English translation, became the subject of intense interest in the United States, not only among university scholars, but among circles far removed from academic disciplines. The vigorous critiques of his approach that began to appear in the 1960s apparently did not diminish his self-confidence and certainly not his productivity. In 1976 he published the first volume of *Histoire des croyances et des idées religieuses*, a historical work covering all the major developments in man's religions from the Stone Age to modern times, purposely written after decades of work on the basic cross-cultural, transhistorical patterns of religious

experience and expression, thus perhaps refuting the accusation that he was "an anti-historian of religions" (Dudley 1976; 1977).

Basic concepts and approaches. Eliade saw the task of the historian of religions as the understanding of *homo religiosus*, man as a religious being, intended toward a transcendent, infinite, absolute realm. Although this intentionality is a universal human structure, it is most evident in archaic and primitive cultures and has tended to be repressed in modern, urbanized societies. Hence the attention devoted by the historian of religions to archaic and primitive phenomena is not aimed merely at providing interesting exotica, but rather at arriving at an understanding of how premodern man has seen his situation in the cosmos and the means of coping with it—at basic matters of being, meaning, and truth. In doing this the historian of religions accomplishes not only a cognitive mission, but also a transformative one: the recuperation of lost or forgotten areas of the essentially human for the modern inquirer and his audience. Thus at the end of his quest self-understanding, as well as other-understanding, and the makings of a complete philosophical anthropology are promised.

While not claiming that the mind and life of archaic and primitive man are confined to the spiritual aspect, Eliade concentrated on the latter as the one that most needed exploration and illumination, an aspect that had been relatively neglected, misunderstood, and traduced (there is also no doubt that he considered it the essentially human aspect). He insisted emphatically that it was not to be understood in sociological, psychological, economic, or any other nonreligious terms, but nonreductively, as something absolutely *sui generis*. Religion, or the religious man, was to be understood on its own terms—i.e., in terms of the sacred and its manifestations.

Here Eliade had recourse to a concept that had been highlighted by two generations of anthropologists and historians of religions: Robert R. Marett, Émile Durkheim, Arnold van Gennep, Nathan Söderblom, Rudolf Otto, Gerardus van der Leeuw, and Roger Caillois. Like the latter he simply defined the sacred as the opposite of the profane: The sacred is utterly other than the ordinary, the usual, the mundane. Archaic and primitive man has experienced the world in these two contrasting aspects in what Eliade called "the dialectic of the sacred and profane." Anything may be selected as bearer of the

sacred—a place, a man, a stone, a tree, nutrition, excretion, coition—and the bearer both remains itself, a “natural” object, person, or process, and at the same time becomes (is revealed as) something absolutely other, acquiring a transcendent character. The locus of the sacred may change, desacralization, as well as sacralization, occurring in this paradoxical dialectic. Eliade coined the term “hierophany” to indicate the character of the particular objects or actions as manifestations of the sacred. And it is to the systematic elucidation of the hierophanies that he devoted his work.

In this quest for the “morphology of the sacred,” Eliade sought the general structures, the typical patterns, underlying the specific phenomena. For instance, an examination of the multitudinous varieties of plant hierophanies among the ages and peoples of the world, from the archetypal symbol of the Cosmic Tree to the popular custom of “bringing home the May,” indicates a coherent system of meanings, centered on the recurrent rhythm of birth and rebirth, the eternal cycle of vegetative life, and, beyond that, the eternal renewal of the cosmos. The meaning of each particular plant hierophany, therefore, is fully understandable only in terms of the whole system, a “whole” that is constructed intuitively. Hence, when discussing such topics as sky gods, water symbolism, or moon mystique, Eliade brought together materials from widely separated places and ages, and sought their basic, underlying, common structure, apart from the specific, local contexts and meanings. His essential concern was for the typical, the general, the universal.

Although, in this view, the inquirer must begin with concrete historical data, acquired through his own research or that of others, philological, historical, or ethnological description and analysis are not enough. The historian of religions must also possess *understanding*. He must seek to attain the depth meanings underlying the data, through a discernment of the basic patterns in a much larger context and through an interpretation that deciphers their transhistorical intentionality. Thus in his noted work *Shamanism*, Eliade not only extended the term to a far wider geographical area than had been customary, but also saw it as a primitive mode of the ecstatic type of mysticism, usually regarded as a speciality of the “higher religions.” He placed shamanism solidly in the general history of religions. In his conception, the historian of religions is not primarily a historian or

philologist (e.g., a Sinologist or Indologist), but a comparativist, phenomenologist, and “hermeneut” (practitioner of the art of interpretation). He proceeds by an intuitive act resembling the eidetic vision of the philosophical phenomenologist, but exercises it on empirical historical phenomena.

For Eliade, all religious facts have a symbolic character, which must be deciphered by the historian of religions. The fullness of religious meanings (particularly the paradoxical coincidence of opposites) is not conveyable in rational concepts. Hence, the interpretation of myth and symbol plays a central role in his work. This involves not only sympathetic participation in the thought world of the (usually archaic or primitive) *homo religiosus*, but also a significant emphasis on the objectivity of the interpreter’s perception. Interpretation is not merely a matter of the replication of what the subjects may have in their consciousness. The historian of religions must go down beneath all the elaborations, accretions, distortions, and amnesias to the essential meaning of a particular religious phenomenon. He does this on the basis of his systematic construction of the general structures of hierophanies. For example, people who bring home tree branches at the beginning of spring may have no idea of the sacral significance of their act. Here the historian of religions resembles the literary critic who claims to understand the writer better than he understands himself or the psychoanalyst who claims to discern unconscious motivations in the patient’s mind.

Another important element in Eliade’s synoptic vision of man’s religiousness is the central role of *archetypes* and *repetition*. There are universal archetypes of human intentionality, which, when discerned by the inquirer, signal the meaning of all kinds of phenomena, a meaning which is not always or usually immediately apparent. Through the repetition of these archetypes in mythical narratives and ritual performances, premodern man finds the ground of value and meaning for his daily activities and the pathway to a transcendent realm. Thus rites of initiation are not fully understandable merely in biological or societal terms; essentially they involve a transformation to a higher mode of being in a repetition of the basic archetype of death and rebirth (conversely, death is an initiation into a higher realm). Archaic and primitive man finds the meaning and value of his existence in the archetypes that serve as models

and thus provide ontological rootage for his works and days. The central model, myth, and rite is that representing the Creation, the emergence of cosmos out of chaos, periodically repeated in a renewal of the world and time. Traditional man seeks to attain the original time of beginnings by reactualizing it in ritual repetition. This is the basic model for all actions, beginnings, productions, and constructions in everyday life. Its ultimate expression is "the myth of the eternal return," which is acted out in everything from New Year's festivals to housewarming celebrations. Man seeks release from the serial progression of historical time in the cyclical rhythms of the cosmos.

There are some obvious similarities between Eliade's approach and that of depth psychology, particularly Jungianism, with its notion of archetypes of the collective unconscious. Although Eliade saw a certain corroboration and encouragement in this pursuit, his concern and emphasis were rather to discern and decipher what is expressed in overt, concrete, historical phenomena—in the lore, ritual, and myths of peoples—not in the individual psyche and its processes. He stressed the transhistoric and "trans-conscious" orientation of man's religiousness, the *nisus* toward being, the avid aspiration for ontological reality, within a cosmic and meta-cosmic framework. He projected his *Myth of the Eternal Return* as a study of "archaic ontology" and once expressed the desire to construct a metaphysics and ethics based solely on archaic and primitive phenomena. Indeed, he claimed Plato as "the outstanding philosopher of 'primitive mentality,'" because Plato, like primitive man, views essence as preceding existence. For Eliade, myth, ritual, and symbol are concerned with being and its attainment by man, who has fallen into a profane condition and become prey to "the terror of history." It may be said, that paraphrasing Freud's dictum on the id and the ego, Eliade would have his *homo religiosus* proclaim, "Where there profane is, there shall sacred be." In a sense, Eliade was pursuing philosophy of the metaphysical (and salvific) variety, not through abstract speculation, but by considering in depth the concrete materials of the history of religions. In so doing, he may have made a noteworthy contribution to philosophical anthropology.

Critiques. As the main lines of Eliade's work and thought emerged, negative criticisms of his methods and conclusions increased. Historically minded historians of religions (e.g., Raffaele

Pettazoni and the European comparative-historical school generally) objected to what they considered sweeping generalizations and ingenious reconstructions based on fragmentary data, and the juxtaposition of instances widely separated in space and time under some general structural category arrived at in an a priori manner. They preferred more cautious conclusions derived methodically from documentary sources. American critics, such as Hans H. Penner and Robert D. Baird, complained that his was an intuitive method, never clearly explicated and not subject to ordinary, consensual verification or falsification; moreover, that definitely ontological assertions were being hidden under a phenomenological manner. They insisted that Eliade was not only describing archaic man's religious stance, but vigorously espousing it, and that he was asserting the supreme value and meaning of the sacred (never established in any logical or empirical way), not merely presenting it as a phenomenological referent. Although he signed the 1960 Marburg manifesto of the International Congress for the History of Religions, which proclaimed the field an empirical, nonnormative discipline, he was trenchantly criticized as having normative, *parti pris* stances: proreligious, proarchaic, and even pro-Christian (e.g., his position that the incarnation of God in Christ may be viewed as the supreme hierophany). His refusal to define religion beforehand, except to say that it has to do with man's experience of the sacred (also undefined), was assailed as irrational and unscientific; his antireductionist stance, his insistence that religion is something *sui generis*, was attacked as a theoretically inadmissible position that would make a "science of religions" impossible (Penner & Yonan 1972; Baird 1970; 1971). It was also charged that his approach to and appreciation of religious phenomena was skewed by his intimate study and experience of Indian Yoga. (Excellent presentations of historical critiques are in Dudley 1977; Bianchi 1975).

Anthropological critiques followed along much the same lines as those by historians of religions: inadequate or sloppily handled data, precariously supported grand generalizations, one-factor universals, glaring errors of fact, the ascription of a specific cast of mind to primitive-archaic man, proceeding deductively from a priori metaphysical postulates, providing no explicit method of verification and correction, abstracting religion from its sociocultural con-

text, deemphasizing actual primitive–archaic views to bring out the scholar's pet interpretations, and armchair scholarship with outdated sources and theories. Adverse judgments on Eliade's works from an anthropological viewpoint were expressed in reviews and books by Lord Raglan, Peter Lienhardt, William Lessa, Anthony F. C. Wallace, Annemaire de Waal Malefijt, Weston La Barre, Edmund Leach, John A. Saliba, and many others. Dudley (1977) and Mac Linscott Ricketts (1973) summarize the significant negative views. A few approving views were expressed (e.g., Hudson 1968).

Despite his alleged pro-Christian bias, Eliade was also forcefully criticized by some Christian scholars as depreciative of Christianity and the biblical tradition (Hamilton 1965), or at least as illegitimately trying to subsume them under his archaic, cosmocentric, ahistoric paradigm (Altizer 1962; 1963). Where Eliade saw modernity as a Fall into meaningless, historical time, these critics saw it rather as an ascent to a new and fuller mode of existence. Altizer, a devoted but critical disciple of Eliade, viewed him as untrue to his dialectic in not heralding the emptying out of the sacred into the profane, signaled by the Death of God in Christ; in not welcoming, instead of rejecting, the godless, profane, modern age.

Whatever the cogency of some of these criticisms—historical, anthropological, or theological—Eliade's work as a whole was not necessarily vitiated by them. A closer consideration of the careful qualifications expressed in his works—sometimes obscured by Eliade's sonorous, poetic style—may have modified some of the critiques. Besides, many of the latter came from theoretical and methodological positions that were themselves being vigorously questioned as naive or historically false—e.g., the empirical data-hypothesis–verification (or falsification) pattern, both in the natural and human sciences (see, for example, the attacks by Thomas Kuhn and Paul Feyerabend on the conventional views of scientific method). Meanwhile, the creative role of intuition and a priori elements in human knowledge was being more positively evaluated, and scholars like Ian C. Jarvie (1964) were calling for broad generalizations and syntheses to rejuvenate the social sciences. The safer, more obvious, more manageable methods advocated by some of his critics contrasted with Eliade's grand vision and adventurous risks, but perhaps promised to provide more security than insight. To see Eliade as a shaman in scholar's garb, whether disapprovingly (Leach 1966) or

approvingly (Altizer 1963), is, of course, flagrant hyperbole, but still may indicate a special kind of scholarship and science, more in the spirit of Vico than of Comte.

SEYMOUR CAIN

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ERIKSON, ERIK H.

Erik H. Erikson was born on June 15, 1902, in Frankfort am Main, Germany. Winner of both the Pulitzer Prize and the National Book Award, Erikson is one of the most eminent and influential psychoanalysts of the twentieth century. His theories concerning the biologically-determined stages of human development as shaped by environment have been widely accepted by, and have profoundly changed, such seemingly diverse theoretical and practical fields as clinical psychoanalysis, ethics, history, literature, politics, the social sciences, child care, and family relations. The multifaceted application of Eriksonian theory has made his contributions difficult to define and categorize. The difficulty in evaluating Erikson's work is a consequence of its scope as an all-embracing and rational explanation of the psychosocial development of humans in any given environment. Erikson views every person as a biological entity produced, and ineluctably guided by, both genetic and environmental factors, who also changes his own culture and, therefore, history as a whole. Thus, everyone becomes a strand in the tapestry of humanity, acted upon while acting, observing and altering his environment and himself, motivated and motivating. Erikson's son Kai, a sociologist, has observed of his father: "He's got his own discipline. Like most original people

he places greater faith in the furniture in his own head than in any particular discipline."

Erikson's contributions to an understanding of mankind in society began introspectively, with the insight provided by his analysis with Anna Freud. Erik Erikson is, in a sense, the son of two fathers. His natural father, a Dane, abandoned his mother, an artist, before their child was born. For some time, in keeping with the practices of his day, the child, Erik, was kept unaware of the existence of his natural father. The father he knew, in whose house he grew up, was a German-Jewish pediatrician, Theodor Homburger, whom Erikson's mother married. Homburger not only fully accepted the stepson but expected him to follow his own profession. After some occupational detours, Erikson did so, in a sense, becoming a healer of children's emotional and mental disturbances and not infrequently, thereby, also of their physical malfunctions.

Erikson has acknowledged both his fathers, first publishing under the name of Homburger and later under the name of Erik H. (Homburger) Erikson. His ambiguous personal background was formed in the also ambiguous historical and geographical setting in which he lived. As a young man Erikson lived in Schleswig-Holstein, an area claimed by both Germany and Denmark. This territorial dispute was very much in focus while Erikson attended the Gymnasium in Karlsruhe, and Erikson, a partisan for the German claim, soon found himself rebuffed by his classmates. The implication was that, as a Jew, he was unsuited to embrace a German cause. Yet he found himself equally a stranger in his father's synagogue, where he was dubbed "the goy" because of his fair Scandinavian appearance and perhaps, as well, because of his mixed parentage. Erikson's exploration of the effects on the individual of ambiguous background and the resulting uncertainty about one's identity, which originated in his personal conflict—and which is an acute problem in contemporary Western civilization—is one of his main contributions to the understanding of individual psychology. Among his many other theoretical and clinical contributions to an understanding of humans in society are his complete "scaffolding" for human development in the eight stages of the human life cycle, and the coalescence or clash of individual history and the historical setting (the time and place in which a person happens to live).

On finishing high school, Erikson, following

his mother's profession, sketched his way as a budding artist through Europe, thus experiencing (according to his later postulations about the stages of the life cycle) a kind of "moratorium" between childhood and adulthood. This interim period gives a young person an opportunity to explore his potentials and limits, to shed, in however a painful way, that part of his childhood which seems useless or obsolete, to shape his conscience and, if not his destiny, at least his destination.

In 1927, on the suggestion of his friend Peter Blos, Erikson went to Vienna, where both men taught in the Burlingham School, a small private school with a psychoanalytic orientation. The logical sequel of this affiliation was Erikson's contact with the Vienna Psychoanalytic Institute, where, lacking medical training, he found himself a kind of odd man. Erikson, himself, describes his role there and elsewhere as having a "favored stepson identity, that made me take for granted that I should be accepted where I didn't belong." This identity of favored stepson has been Erikson's fate. An eminent scholar without academic credentials; a psychoanalyst who pioneered in exploring the ego rather than, as has usually been the case, the drives or the id; a wide-ranging theorist who explored the lifelong interplay and influence of a person's genetic endowment and biological ancestry, his environment, his chance fate, Erikson reaches across borders into other experts' territories.

In keeping with his role as favored stepson, Erikson first found far greater response and appreciation among such outstanding anthropologists as Margaret Mead, Ruth Benedict, A. L. Kroeber, and S. Mekeel than among his colleagues at Harvard Medical School. He, in turn, benefited from anthropological methods and arenas of exploration, drawing on the experiences of other cultures (Sioux, Yurok) in addition to his own, in order to test his life-cycle theory. The study of other "simple" cultures also helped him to gain understanding of the ever present organic system, culture-family-child.

According to Erikson's life-cycle theory, the sequence of developmental stages has always been the same for the entire human race. In brief, the life cycle comprises an inescapable succession of eight developmental stages. Each stage has its specific conflict, the resolution of which, with a positive feeling dominating, leads to the next stage. The developmental conflicts characteristic of each stage are: trust versus

mistrust in the first stage of infancy; as trust is reinforced and becomes dominant, the second stage, autonomy versus shame and doubt, is attained during the toddler years; with the dominance of autonomy over shame and doubt, the next, third stage, initiative versus guilt, occurs during the preschool years; this leads to the fourth stage, industriousness versus inferiority during the school years, which, in turn, leads to the fifth stage, identity versus role confusion during adolescence, to be followed by intimacy versus isolation, the sixth stage, during young adulthood, then the seventh stage, generativity versus stagnation during mature adulthood, and finally, in the eighth stage, ego integrity versus despair during old age. The life style of, say, the Sioux, the French Canadian, the Zulu, or any definable subgroup depends on "what a culture makes" of a given developmental phase.

Thus, for instance, the second stage, which roughly coincides with the so-called toddler years, is characterized by the conflict between wanting to be autonomous and the doubt that one will ever be able to. This conflict is accompanied by shame whenever the child feels that he is not succeeding in mastering such bodily functions as urination, defecation, walking at will. He is equally perturbed about having the adult's standards forcefully imposed upon him. The battle between the parent who must put limits on the child's behavior, and the youngster's determination to have his own way, leads to any number of possible conflict resolutions, or compromises. For further healthy development it is necessary that the sense of autonomy prevail over the shame and doubt which, though mastered, is nonetheless potentially ever-present.

The main characteristic of the next, third, stage is initiative, which must, however, be tempered by a consideration for fellow men, especially parents and siblings. Thus, the child feels torn between his urge to action, curiosity, intrusiveness, and a sense of guilt when he "goes too far." Going too far may be differently defined in different civilizations. Indeed, each stage of the life cycle takes on a different hue, depending on "what a culture makes" of it, and specifically, how flexible the relevant adults in a child's life are about their hoped-for-gains in his or her socialization. The life-cycle theory thus takes cognizance of the variable aspects, as well as those which are biologically fixed and invariant, in each person's progressive adaptation from birth to old age.

Men's and women's development to adulthood (generativity) and old age safeguards the survival of our species. The life cycle's phylogenetic implications are in the function of older people as the concerned guardians of the young.

The resolution of each conflict, in the eight stages of the life cycle, also supplies the developing person with a measure of mastery over his own environment, as well as a measure of insight into and mastery of his inner self. Or, to relate this theory to a much discussed issue, conflict changes a person's view of himself and, thereby, changes his identity.

In 1942, Erikson joined the University of California at Berkeley as professor of psychology, an appointment which afforded him a great deal of clinical observation. It was during his Berkeley years that he was able to consolidate the mutual connection, the biological web, as it were, of culture-family-child as laid down in his first major work, *Childhood and Society* (1950). From this first work, and continuing to the present, Erikson's wife, Joan, has been an able editor, collaborator, and supporter of his efforts. Herself, a lucid author as well as a professional dancer and artist, Joan Erikson has confirmed and enhanced Erikson's clinical understanding and clarity of style.

The life-cycle theory developed during Erikson's Berkeley period, was completed in 1950 during the political "witch-hunting" period in the United States known as the McCarthy era. Refusing to sign the loyalty oath then demanded by the Board of Regents of the University of California, Erikson became part of an exodus of faculty members, moving to the Austin Riggs Center in Stockbridge, Massachusetts. There, the Eriksons' work with young, so-called borderline patients led them to an appreciation of a phenomenon which Erikson termed "identity crisis." This conflict or crisis is perceived by him as an exacerbation of the normal developmental problems characteristic of adolescence and young adulthood. The concept of identity crisis (the notion for which Erikson is most widely known in the general population), is now embedded in psychoanalytic theory and practice. The concept was first explicated in Erikson's book, *Young Man Luther: A Study in Psychoanalysis and History* (1958). Erikson traces the coming into being of this kind of crisis as a widespread phenomenon to the sixteenth-century upheaval of European culture and the birth of the passionate and powerful reformer, Luther. The mutual influence of an exceptional man

(the greater the man the more powerful his own identity crisis) and the times in which he lives is quite possibly nowhere else as well described as in this book. Erikson followed this book with another historical-biographical study: *Gandhi's Truth* (1969), in which Gandhi is also revealed as a great man who altered the course of history by changing himself. To put it simply, just as Erikson termed the policy of nonviolence (civil disobedience) to effect social change, *Gandhi's Truth*, so, the internalized conscience might be termed Luther's contribution to Western civilization, or Luther's truth.

By the time he worked on the Gandhi book in 1960, Erikson had returned to Harvard, "in order to learn how to teach my whole conception of the life cycle—including the identity crisis—to people normatively very much in it." Not that the identity crisis is limited to adolescence or young adulthood; like all developmental crises, it is often re-experienced from time to time throughout life, though usually in a less dramatic form.

In 1975 the Eriksons left Harvard and "retired" to Tiburon, California, where, as generative people, they are dealing with a number of theoretical problems and their applications. One of their main interests is play: its biological function, its role in inventiveness, its healing and integrative functions, and its practical and artistic uses in the framework of community centers. The Eriksons' joint community ventures are designed to break down the segregation of people according to social class, age, and sex, as well as between the "mentally disturbed" and "normal people."

In sum, the three Eriksonian concepts generally regarded as most influential and important are the *life-cycle theory*, the *identity theory*, and the *relationship between individual history and the historical period* in which one lives.

As a young clinician, Erikson found that the psychological problems his patients complained of were frequently the very ones he felt himself. The "I don't know who I am" feeling reported by his patients was sometimes experienced by them as a persistent, vague malaise, at other times as an outright confusion, sometimes as a frightening discrepancy between self-perception and the way others perceived one.

However, identity, as Erikson sees it, is by no means the exclusive estate of humans or higher animals, but, of all living things. Every creature, be it centipede or oak tree, is a member of a species with which he shares a number

of characteristics; yet every creature is also different from all others, identifiably unique, and, therefore, has an identity.

The uniqueness of each living being is, in a seemingly paradoxical way, what all have in common. What they also share—people, plants, and animals, alike—is a life cycle, evolving in predictable stages. Even very primitive creatures live through a stage of childhood (however brief or long), a mature and in some way reproductive or generative period, and a time of decline ultimately leading to death—of the individual, but not of the species as a whole. Not for a long time. Thus each individual identity constitutes a link in the phylogenetic chain. How well this living link supports the viability of the links to come depends on (1) its unique genetic endowment; (2) the care and nourishment it has received from the environment; and (3) chance. It might happen that an apple tree descending from a long and healthy line is hit by lightning, while all neighboring trees are spared. Identity thus comprises genetic endowment, shaped by environment throughout life, and mitigated by chance events. What distinguishes human identity from all others is our superior ability to other living beings to cope with our environment and ourselves in ingenious and flexible ways, and to observe ourselves, reflecting on our observations. By means of these abilities we have become, probably, the only creatures who can assume a measure of responsibility for our own and our fellow men's fate. Other living things have an identity, but only we have a *sense* of identity, and an awareness that we can, up to a point, change this identity at will. As already indicated, the gains in individual mastery (or the failure to achieve such gains) accomplished through the resolution of developmental conflicts, add to a person's growing sense of identity. Since these gains are largely due to identification with meaningful adults, and occur within the framework of a cultural setting, they differ according to the groups to which the parents—caretakers—educators belong. Adult models may be Japanese, Norwegian, or Maori, members of a nation, tribe, church, labor union, multinational corporation, or, indeed, a Christmas Club. The fact that they “belong” shapes their own and their children's identities and constitutes the element referred to as a person's group identity.

Erikson also distinguishes between this learned and culturally differentiated *ego* identity, on the one hand, and the universal or species-related

capacity to look at oneself as a creature who changes unceasingly in time (and often in space as well) from infancy through old age, yet experiences self-sameness, in spite of all changes.

The most frequently discussed, and frequently misunderstood, element of Erikson's identity theory is the *identity crisis*, occurring during the fifth stage of the life cycle, typically in late adolescence or young adulthood. Because our culture changes so rapidly, parental values frequently are or seem to be outdated or inconsistent to young persons. Because our culture lacks definite, universally accepted rites of passage to delineate developmental stages, young persons are frightened by the thought that it is up to them to decide when and how to make the leap into adulthood. Also, because everyone in contemporary Western culture is exposed to so many different faiths, life styles, ways of making a living, there are many directions in which one may leap. Added to this is geographic mobility, which can widen horizons and our acceptance of cultural variation, but also can add confusion and uncertainty about where one belongs.

It is for these reasons that the identity crisis has proliferated in the twentieth century. Every developmental stage is accompanied by conflict, but the one we call identity crisis can be so especially painful, that it has caused us to redefine our concept of mental illness; the inability to align oneself with any life style can be so painful and disruptive that the condition becomes indistinguishable from severe psychological disturbance. Yet the prognosis for adolescents and young adults suffering an identity crisis as part of their developmental stage of life is far more auspicious than a deep depression or schizophrenia (which the crisis may resemble) would be at a much earlier or later time in life.

For some, an identity crisis is evidenced by no more than somewhat erratic, highly charged behavior, say, exaggerated antagonism against parental standards or customs, or by a prolonged period of seeming inactivity. Erikson calls this kind of inactivity the “moratorium,” a time during which a person's self-image, his more or less realistic appraisal of his potentialities, limits, and allegiances, and, therefore, the way he should live as an adult, all slowly consolidate.

Erikson's holistic approach to the question of individual identity has contributed immeasurably not only to the understanding of this complex time of life, but also to the understanding

of humankind in the context of whatever time and space they inhabit. Philosophically, his approach to the unfolding of the future of mankind runs parallel to the model of his developmental conflicts in individuals. Erik H. Erikson is fully aware of the global dangers should mistrust prevail over trust, as in wars and other inhumane actions, but even so he hopes that the prevalence of trust over mistrust will help mankind, as it does the individual, toward maturity and survival.

MARIA W. PIERS AND
GENEVIEVE MILLET LANDAU

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EVANS-PRITCHARD, E. E.

Edward Evan Evans-Pritchard (1902–1973) is a major figure in British social anthropology who, along with Raymond Firth, Edmund Leach, and Max Gluckman, dominated the immensely productive and influential period of British field work and theorization following World War II.

Evans-Pritchard was born in Crowborough, Sussex, the son of John Evan Evans-Pritchard, a clergyman of the Church of England, and his wife, the former Dorothea Edwards. He attended Winchester College (1916–1921) and Exeter College (1921–1924), Oxford University, where he took a B.A. in modern history. He did graduate studies in anthropology at the London School of Economics, where he received his PH.D. in 1927 and where he taught from 1928 to 1936.

Evans-Pritchard's thesis was based on his field work among the Azande of the southern Sudan, and he continued his work in this region intermittently through 1938. In 1930 he made his first visit to the Nuer, also of the southern Sudan, and worked off and on with that people until 1936. In 1935 he also prepared an ethnographic survey of the Anuak of the southern Sudan, and in 1937 a survey of the Luo of Kenya. In 1932, while studying the Nuer, Evans-Pritchard accepted the professorship of sociology at Fuad I University of Cairo. Two years later he resigned this post to become research lecturer in African sociology at Oxford University, a position he held from 1935 to 1940, during the time when Oxford anthropology was dominated by A. R. Radcliffe-Brown. In 1940 Evans-Pritchard enlisted in the army serving first in Ethiopia and the Sudan, and later in the Near East and north Africa, mainly in Cyrenaica, Libya, leaving military service a major. During his military duties he collected ethnographic data, the most important being a study of the Sanusi religious order among the bedouin of Cyrenaica. While in Libya, Evans-Pritchard was converted to Roman Catholicism, a decision which some contend influenced his later work. In 1945 Evans-Pritchard returned to England to become reader in anthropology at Cambridge University. In 1946 he became professor of social anthropology at Oxford University upon Radcliffe-Brown's retirement. In 1970 he retired from the chair at Oxford and was knighted the following year.

Evans-Pritchard's own acknowledged teachers were C. G. and B. Z. Seligman, to whom he owed his initial interest in the peoples of the

Sudan. As a graduate student, he also took courses with Bronislaw Malinowski and knew and liked A. M. Hocart. He rejected Radcliffe-Brown's quest for a science of society and his somewhat simplistic functionalism, although he may have owed to him some of his interest in the French sociological tradition. While rarely citing Malinowski in later life, Evans-Pritchard may have in part derived from him his initial interest in the nature of magic and his profound commitment to an empiricism demanding prolonged field work. Evans-Pritchard's study of the Azande was the first intensive study of an African people by a trained social anthropologist.

Evans-Pritchard's intellectual roots lay in the classic tradition: the interdependent influences of the Scottish rationalists and their descendant, William Robertson Smith, and those of the French sociologists, such as Émile Durkheim and his school. Evans-Pritchard's immensely influential writings on magic, witchcraft, and religion derived from an enduring concern with questions about rationality and belief raised by James George Frazer, Edward Burnett Tylor, Lucien Lévy-Bruhl, Durkheim, Marcel Mauss, and Vilfredo Pareto. Aside from the French school and Pareto, Evans-Pritchard made few references to other sociological classics or to American or continental anthropologists, and he rarely cited his contemporaries. Much to his loss analytically, Evans-Pritchard virtually ignored Max Weber throughout his career, despite their common concern with religious and political institutions or their interest in history.

For Evans-Pritchard, ethnography was the true measure of one's worth as an anthropologist. He produced rich ethnographic accounts of two African societies, the Azande and Nuer; an innovative historical study of an Islamic brotherhood (Sanusi); a study of the Anuak; and numerous briefer surveys and essays on other peoples. Unlike many other anthropologists, Evans-Pritchard never approached theory through the parochial path of only one society.

Evans-Pritchard believed that the key problem in cultural anthropology is translation (1965*b*). This involves two complex, interrelated issues: the difficulty of entering into the mental world of an alien, exotic culture, and the problem of making that world understandable and thereby comparable to that of other societies, especially in terms of modern Western thought. This problem occupied him throughout his career.

His first book on the Azande (1937) is one of the most profoundly original and influential

studies in all social anthropology. Its value lies in its uncommonly acute employment of rich ethnographic data towards a usefully narrow, yet profoundly important, set of sociological ends. The questions had been raised before, but no one had previously gone to the field to confirm or refute the points involved. These issues revolved around the question of whether preliterate peoples think as literate people do. Evans-Pritchard's predecessors provided various answers, but none did field work or utilized sufficient data to sustain the arguments. Evans-Pritchard's research provides a clear and extended exposition of the ideas behind Zande beliefs in witchcraft, magic, and sorcery. While supplementary concepts such as those involving gestation, kinship, and causation are also brought in, Evans-Pritchard centers his attention upon beliefs in the supernatural, precisely because such notions had elicited the greatest disdain and misunderstanding from his predecessors. He indicates the systematic, enclosed, and self-perpetuating qualities of these ideas and demonstrates how such views are consistent with rational, workable, everyday experience. He further indicates that these are no more open to the onslaughts of alien criticism than are other philosophical beliefs and religious faiths easily vulnerable to the attacks of opposing believers and atheists. In the course of his exposition, Evans-Pritchard indicates how such notions sustain and confirm the forms of authority and power within a society. Not only does he maintain that allegedly irrational, exotic beliefs make sense within the experience of a particular society, but, conversely, that all societies hold existential beliefs nearly impervious to the demands of logic and consistency, as these are more rigorously defined by the supposedly scientifically minded. His study begins by presenting the thought of one society; it proceeds to use these data to re-evaluate and correct earlier anthropological conceptions about the nature of thought in all preliterate societies; it ends by implying that common features of rationality, nonrationality, existential assumptions, and conventional compartmentalization characterize all thought systems everywhere. Never before had the beliefs of a preliterate society been considered so carefully or with such empathy, yet Winch (1964) has convincingly shown that even this study reveals traces of ethnocentrism. For example, Evans-Pritchard denies that witches exist, even though there is no way within Zande thought that such existence can be dis-

proved, while his own disbelief requires a commitment to our own Western assumptions, which are different, but by no means necessarily more rational than those of the Azande.

In his next important work, Evans-Pritchard approached the problem of the sociology of knowledge from a different perspective. His study of Nuer time-reckoning (1939) provides an account of the subtle and complex relations between perceptions of time and space and their connection with various aspects of social experience and needs. In part this is shown to be the result of aspects of lineal and cyclical time connected to seasonal events and personal careers. Such notions are demonstrated to extend no further than their social utility requires for providing a coherent mode for thinking about both social solidarity and difference as required by features of demography, economy, and political affairs. For Evans-Pritchard, cosmologies are thus bounded by the needs of society and extend no further than are socially necessary in time and space. Evans-Pritchard's data sustain a Durkheimian view of knowledge, even though he elsewhere wrote somewhat disparagingly of both Durkheim's and Mauss's propositions on such issues (1965b).

The Nuer (1940a) is undoubtedly Evans-Pritchard's best-known book. It appeared in the same year as his analysis of Anuak political institutions and *African Political Systems* (Fortes & Evans-Pritchard 1940). These three works revolutionized social anthropological research in Africa and had a deep, although not entirely beneficial, impact on the discipline. *The Nuer* remains the classic exposition of a segmentary system of acephalous lineages. Evans-Pritchard opens the book by asking how a society can have law and order without the conventional institutions of government, such as chiefs, courts, and police. The key again lies in an overarching system of beliefs and values. The work subtly emphasizes the integrative aspects of conflict (peace in the feud) and the changing relativity of meaningful social alliances (fission and fusion) expressed in terms of lineal affiliations. Evans-Pritchard conceded (though never in print) that the model derived largely from W. Robertson Smith's *Kinship and Marriage in Early Arabia* (1885). Although the work enabled many subsequent fieldworkers to make sense out of the social life of lineage based societies, it is unfortunate that the very brilliance of the model seduced many into an oversimplification of material which was sometimes forced into seg-

mentary schemes. While such models may have proved useful, at least initially, for digesting much of the African material, where kin and political institutions are often closely connected, these models obstructed insights into most societies in the Pacific and New World, where other organizing principles are often at work. Such a view even obscured some aspects of the actual workings of societies in Africa, even among pastoralists. Peters has produced the most telling critique of such segmentary models (1967). The best features of Evans-Pritchard's analysis of the interconnections between political, kin, and religious beliefs are incisively combined in his Frazer Lecture of 1948 on the Shilluk (1948, in 1962).

In his second Nuer volume (1951a) Evans-Pritchard examines Nuer kinship and marriage. Although the weakest of the Nuer trilogy, it contains extremely interesting expositions of Nuer practices which were initially disarming to many analysts: ghost marriage, woman-woman marriage, and the levirate. The manner of exposition derives much from Henry Sumner Maine, but is considerably weakened by a restriction to normative, ideological explanations and a failure to fit social actions within the varying details of property, demography, and personal manipulation. Women's roles in social life also receive remarkably poor attention (Beidelman 1971), nor are the complex relations between Nuer and the neighboring Dinka adequately drawn (Southall 1976).

Evans-Pritchard's final work on the Nuer (1956) was the first scholarly study to present the religious beliefs of a preliterate people in a manner suggesting that these bear theological significance and profundity comparable to the religious thought of more complex societies. Various aspects of Nuer religious beliefs—totemism, classification, sacrifice, body symbolism—are examined in many of their dimensions. Evans-Pritchard's mastery of the French sociological classics and his appreciation of Semitic and biblical studies provide a useful framework for his exposition of a system of normative ideas and values so that sophisticated theologians and philosophers can cite this with relevance (MacIntyre 1964). Despite its implicitly Durkheimian position of presenting religious and cosmological awareness as a refraction of social experience, the monograph ends on a note of deferring to theologians, an inconsistent position which some have ascribed to Evans-Pritchard's own religious sentiments. Nowhere in such later work is there

any substantial discussion of how religious or other beliefs provide avenues of power and coercion for certain groups or individuals, even though Evans-Pritchard's own data and that of other writers on the Nuer, including Paul Howell, B. A. Lewis, and Father Crazzolaro, suggest otherwise (Beidelman 1971). Nor is there much attempt to examine ritual or symbolism beyond their manifest content (Lévi-Strauss 1962; Beidelman 1966; Beidelman 1968). In his later survey of various sociological interpretations of religion (1965*b*), Evans-Pritchard appears to reject even those moderate sociological methods he himself had put to such brilliant use in his previous work.

Probably as an outcome of his earlier historical training, Evans-Pritchard promoted the historical perspective in British social anthropology. Radcliffe-Brown and Malinowski had left British social anthropology preoccupied with functional analysis and unable to appreciate fully the advantages that historical material may provide both for understanding contemporary societies and for analyzing social change. Evans-Pritchard's contribution to historical anthropology has three aspects: (a) he revived interest in oral history, his publications on Zande history extending over his lifetime and culminating in his penultimate book (1971); (b) his study of the Sanusi (1949) combines the social perceptions of a social anthropologist trained in the analysis of religion and politics in a pastoral society with the study of events temporally using oral and archival sources; (c) Evans-Pritchard appreciated, as did increasingly some historians, that epistemological problems posed by historical studies closely resemble those confronting a comparative sociologist or anthropologist. Here, again, his indifference to Weber as well as to Marx is perplexing.

Few British anthropologists since Malinowski have done more than Evans-Pritchard to elevate the study of oral literature from neglect by social anthropologists, who are preoccupied with social structure. Evans-Pritchard stressed that texts be studied as literature, his view being epitomized in his analysis of ambiguity in Zande proverbs (1956, c.f., 1962). Evans-Pritchard published a large collection of Zande texts (e.g., 1967) and in 1964 helped found the Oxford Library of African Literature which has published many monographs by an international group of scholars.

Evans-Pritchard excelled in drawing attention to topics for the most part ignored by his colleagues. He was the first to provide a truly

sociological analysis of the dance among primitive societies (1928, in 1965*a*) and to write thoughtfully about the social functions of ritual obscenity (1929, in 1965*a*). He pioneered the reconception of the functions of marriage payments and stressed the need for replacing the term *brideprice* with *bridewealth*. He presented the most searching and complex exposition of concepts of marriage and paternity since Maine and wrote the first sensible and detailed analysis of the practice of blood brotherhood (1933, in 1962). Few anthropologists have displayed such a wide range of interests combined with such a wealth of rich and dependable ethnography. Consider, for example, such diverse and unlikely topics as how children are socialized into using kin terms, rituals for twins, terms of address, naming, deviant sexual behavior, iron working, string games, onomatopoeia, and cannibalism and its relation to nutrition.

It has often been remarked that it was Radcliffe-Brown who resuscitated the French sociological classics for social anthropologists; Evans-Pritchard continued and extended this mission not only by lecturing, but also by encouraging the translation and publication in English of the works of Durkheim, Mauss, Henri Hubert, Célestine Bouglé, Lévy-Bruhl, Arnold van Gennep, and others. It is to him initially that we owe a renewed appreciation of Robert Hertz, whose seminal essays on death and body imagery exerted a deep influence on Evans-Pritchard's own work.

Much can be learned by considering the manner in which Evans-Pritchard presented his work, both in terms of his style of writing and in the strategy behind the forms, places, and schedules of his publications. He published in an immensely varied number of journals, from major periodicals to obscure serials. This reflects his own deep commitment to the catholicity of anthropological scholarship. His schedule of publications suggests a carefully considered plan whereby each of his books was preceded by an extended series of essays, sometimes over a decade, which presented either theoretical considerations of a fairly narrow set of problems or exposition of valuable yet cumbersome supplementary ethnography. When the book ultimately appeared, the arguments initially advanced acquired a polished, condensed form, and much ethnographic detail, essential yet distracting from the main thrust of the arguments, would have been recorded elsewhere. This procedure produced proportioned monographs, but ones whose full significance requires considerable lit-

erary and intellectual backtracking by scholarly readers. Evans-Pritchard's style also merits consideration, for while the prose is forceful, it is also deceptively lucid, terms often being pregnant with ambiguities. His works rarely contain summaries or conclusions indicating his points; instead, his theories and reasoning flow modestly within the narrative, and the analyses and conclusions must be searched out from the exposition. In this, his works conceal values which are revealed only through intensive and repeated consideration.

T. O. BEIDELMAN

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F

FAIRBANK, JOHN K.

John K. Fairbank was born in South Dakota in 1907 and educated at Phillips Exeter Academy, the University of Wisconsin, and Harvard University. He received his Harvard A.B. in 1929, *summa cum laude*, in history. By this time, he had already decided to pursue a scholarly career and study the Chinese language and Chinese-Western relations. The turn to scholarship was based soundly on a brilliant academic career, but the turn to China was at that time an almost unilateral aberration for a professional historian. Certainly most academics in 1929 would have considered an investment in Chinese language and history a hazardous undertaking for anyone interested in a university professorship. That Fairbank used his Rhodes scholarship first at Balliol College, Oxford, and then in Peking, China, to study nineteenth-century Chinese relations with the West was at least in part a response to intellectual trends of that time.

Fairbank's professional interest in China was aroused by C. K. Webster, who suggested to a Harvard audience in 1929 that new documentation on China's foreign relations would presumably throw light on the "ominous problem of east Asia." The idea fell on fertile soil. Fairbank was one of the first to take advantage of China's decision to publish the 130 volumes of documents on its foreign relations from 1836 to 1874—the *Ch'ou Pan I Wu Shih Mo*. At the same time Webster was clearly reacting to stirrings in American intellectual life.

American neglect of east Asian studies had been pointed out by Paul S. Reinsch when he

was president of the American Political Science Association just after World War I. However, little was done until 1928, when the American Oriental Society came out in favor of promoting scientific Sinological research, and the Library of Congress initiated a division of Chinese literature, of which Arthur Hummel became director. About the same time Harvard announced the establishment of the Harvard-Yenching Institute. Very active in helping to create a favorable climate for American scholarship on Asia were Edward C. Carter of the Institute of Pacific Relations and Mortimer Graves and Waldo G. Leland of the American Council of Learned Societies (ACLS).

The foundations for an independent American approach to east Asian studies were being laid, therefore, during Fairbank's Harvard years. Several universities had for some time offered serious courses on China. American missionary Sinologues and officials such as Elijah Coleman Bridgman, Samuel Wells Williams, W. A. P. Martin, Arthur H. Smith, Justus Doolittle, William W. Rickhill, and Edward T. Williams had made substantial contributions to scholarship; some had held chairs in leading universities. But there was no professional academic standing for studies of China or Japan in any of the disciplines. Surveys made in 1928 showed that east Asia was far from being neglected as far as curriculum was concerned, but there were no American professionals in the field. In 1929 the ACLS set up standing committees for the promotion of Chinese and Japanese studies. The committees took on the task of stimulating the production of research tools and the provision

of fellowships to train scholars in these fields. At the same time the nationalist revolution in China, which brought Chiang Kai-shek to power, increased interest in that land. The age of American scholarship on east Asia had begun.

Fairbank's interest in China at this time was well within the framework of conventional diplomatic history. It was not political. At Oxford University he met Hosea B. Morse, who had been with the Chinese Maritime Customs Service and was author of *The International Relations of the Chinese Empire* (1910–1918). Fairbank found a thesis subject that combined Chinese with Western sources—a study of British policy in relation to the origin of the Chinese Imperial Maritime Customs Service, later to become his *Trade and Diplomacy on the China Coast* (1953). The influence of Morse was that of a scholar, as Fairbank put it, devoted to the ideal of scrupulous, impartial, and accurate presentation of the facts. T. F. Tsiang, the Chinese scholar who in a sense had launched all this activity by telling Webster about the new Chinese documentation, was a man of the same qualities as Morse in the field of scholarship. He was generous in his help to any scholar of whatever nationality who wished to work with Chinese materials. For Fairbank's needs while he lived in Peking, Tsiang had no equal. By the time Fairbank was appointed to the Harvard faculty in 1936, he had produced a study that was a model of Anglo-American historical scholarship. The pattern had been set.

Fairbank was one of the first to be aware that China must be analyzed as a society in its own right and that it was a society different in kind, rather than in degree, from our own. He was aware of the insights of German scholarship, of Max Weber and Karl A. Wittfogel in particular, and of the large theoretical concepts that these scholars produced. He acknowledged the debt, but he was cautious of Germanic world views. Bodies of theory, he said, are not magic formulas, but are broad avenues of approach that afford new insights into Chinese social behavior. In his view it was fundamental that all such theories assume that the record of events is basic to our understanding; conceptual schemes can inspire and guide research but are not meant to substitute for it. The moral? Our understanding of China must await painstaking monographic research. Exactly, but research without concepts?

Such a view of historical method and theory

was common among American historians of the period. It was not an area in which Fairbank came up with innovative ideas, in spite of his effort to identify new categories, such as "synarchy"—joint rule by two or more parties. His main intellectual contribution to the discipline of history was to overcome its ethnocentricity, to push it in the direction of absorbing the great independent self-regenerating civilization of China and eventually the whole non-Western world. He helped change the course of American scholarship on China with his illumination, through the use of Chinese documentation, of the role of the imperial court in the institutional development of the maritime customs.

In drawing attention to the urgency of understanding Chinese society, Fairbank pressed for the participation of the social sciences in that task. He was hospitable to theories of Chinese society, whatever their origin, but was not always quite sure where to place them. His insistence that the historian must proceed with his monographic work, steadily inscribing the record of events, was sound enough as far as it went. But the conventional training of the historian could not fully cope with the new pressures arising from the study of non-Western societies and the urgency of enlisting the aid of other disciplines, some of which were reluctant to give up their ethnocentrism. Some, in fact, held the view that ethnocentrism was the necessary basis of their theoretical position. Fairbank was aware of the theoretical stirrings in the social sciences as well as in the discipline of history, but this was not an area in which he felt obligated to contribute. He stayed with the muse of history as he knew her, took her to China, brought her home, and settled himself down with that grand old mother of the disciplines. That is sufficient for one lifetime.

The growth of serious academic interest in the non-Western world other than the Near East took a great deal of time, money, and power. Considerable forces in the American academic world were moving in this direction by 1928, and Fairbank benefited from their support during the 1930s. But important traditional academic forces were also indifferent or antagonistic to the new intellectual trends. The development of east Asian studies in particular and non-Western studies in general was neither easy nor inevitable. It is a matter of record that Fairbank, on the local scene at Harvard, on the national and eventually on the international

scene, made a forceful and distinguished contribution to this development.

In evaluating the contribution that Fairbank and others made to American scholarship, one must remember that interest in east Asia had for long been limited to the colonial and commercial interests of the major powers. It was the object, not the subject of academic interest. These attitudes were certainly changing, even in the 1920s, but there was not yet an independent American tradition; European influence was still strong. The French school, represented by Serge Elissieff, emphasized the textual approach to Sinology and Japanology and commanded the prestige and resources of the Harvard-Yenching Institute.

Many thought the institute was dead weight as far as the growth of modern Asian studies was concerned. The school of history to which Fairbank belonged, still strongly Europe-centered and Anglophile (e.g., Webster and William L. Langer), stressed the "scientific" use of documentation and was as suspicious of recent history as it was of other disciplines. But it provided the bridge, as it were, over which Fairbank made a professionally acceptable transition first to the inclusion of societies as independent entities, and finally to academic respectability for the contemporary scene. While the battle had to be won at Harvard if Fairbank were to have credibility elsewhere, it is to his credit that he put so much energy into the development of the field nationally, realizing presumably that the one strengthened the other. It is amazing that so much was achieved before 1941, when Fairbank moved to Washington, where he worked with the Office of Strategic Services and later with the Office of War Information during World War II.

The war took most of the country's Asian experts away from their universities, but the ACLS, with Fairbank always in the wings, plugged away with plans for the future. By the time the war was over it was obvious, of course, that the United States had to be serious about the study of the non-Western world. It was now possible to take advantage of the advances that had been made in the teaching of foreign languages, especially the contemporary colloquial. The climate for language and area studies was so propitious as a result of wartime training that it was possible to establish a Far Eastern Association (later to become the Association of Asian Studies) to promote the study of non-Western

societies and to keep together the many disciplines that had become involved in that enterprise. Fairbank played his part in establishing and fostering this mass organization.

Right after the war, the Social Science Research Council took the leadership in the crucial task of defining the academic role of area studies. An SSRC committee on world area studies, chaired by Robert Hall, the Michigan geographer, recommended that language and area studies be included in M.A. degree requirements, that there be at least five disciplines represented (with one of them to be stressed), and that all Ph.D. degrees were to be given in the discipline departments. Area studies were not to be separate disciplines. Fairbank played no particular role in this activity, but accepted, as did the rest of the field and the foundations, the SSRC guidelines. The American academic world, with the help first of the Rockefeller and Carnegie and later the Ford foundations was now on its way; today the sun does not set on American scholarship.

It is not surprising that when the challenge to become better acquainted with mainland China arose in 1959 Fairbank was in the front ranks of those who wanted to face it head on. He turned first to the Association for Asian Studies, a body that had been an appropriate instrument for many purposes, but when it became clear that not everyone thought it best for this particular goal, he turned to the SSRC, which set up, with the ACLS, a joint committee on contemporary China on which Fairbank served for several years.

Fairbank's contribution to American scholarship has been substantial. Intellectually he stood out among historians in his grasp of the significance of east Asian societies and among social scientists for his understanding of the need for interdisciplinary cooperation. He was active, with others, in helping to formulate the ways and means to shift academic attention on a national rather than a single institutional basis; thus he helped to prepare the way for the major foundations to participate in the development of area studies after World War II. He was active on all fronts, including the political, the most controversial of all and the most difficult to interpret. There was a debt perhaps to the La Follette Progressive movement in his championship of civil liberties and his strong anti-imperialism and more than a dash of populism in his interpretation of the Communist takeover of

China as a case of the Kuomintang betraying the "revolution." He tended to believe that the Communist party had inherited the leadership of the "revolution" because it was closer to the "people," a view that never commanded unanimous support. It explains both why the government of the People's Republic of China thought of him as a friend and why its leaders did not quite understand the roots of his intellectual and political composition. Their alleged interest in the "people" is instrumental, whereas Fairbank's was as genuine as it was inappropriate to the analysis of international affairs.

GEORGE E. TAYLOR

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FELLMAN, DAVID

David Fellman, born in Omaha, Nebraska, in 1907, retired in 1978 from 45 years of teaching public law at the universities of Nebraska and Wisconsin. A political scientist, Fellman carried forward established traditions of public law by emphasizing authoritative rulings of appellate courts to elucidate his concerns for civil rights and liberties. He advanced these substantive interests through graduate and undergraduate teaching, as an adviser to public officials, particularly in Wisconsin, activity in the Wisconsin Civil Liberties Union, and dedication to the work of the American Association of University Professors. He was Vilas professor of political sci-

ence at the University of Wisconsin at Madison when he retired from active teaching.

Fellman's teaching and writing, his professional and public careers, grew out of a thoughtful commitment to principles of constitutional democracy. Treasuring a humane and liberal polity, he believed democracy "a method for arriving at decisions in the area of governmental activity" rather than a set of solutions. In sum, "democracy is thus mainly a method of avoiding violence by arriving at essentially tentative solutions to social conflicts through argument, persuasion, compromise and votes" (1966*b*). The fact that his writings were concerned chiefly with case law should not obscure his dedication to and interest in political parties, majority rule, and elected leaders like Senator Norris (1946). More than his younger contemporaries in the field of public law in political science after 1950, Fellman relied upon the liberal political theory of the utilitarians and the judicial opinions of Oliver Wendell Holmes, Louis D. Brandeis, Harlan F. Stone, Robert H. Jackson, and Felix Frankfurter in taking stands. He assumed constitutional law

must take chances because the best it can do is to discover points of equilibrium or balance between competing ideas, desires, needs or interests. This has been a central theme of American constitutional law. Our political system is a democracy, and our constitutional law is therefore an instrument of our democracy. Our democracy is basically a collective bargaining society and the judicial branch of government is one of the great instruments through which agreement is reached. It follows that our constitutional law rejects fanaticism or extremism and tends to move to the middle of the road. (1966*b*)

Many of Fellman's detailed studies of case law were aimed at understanding and urging the protection of the disadvantaged. In one of his earliest published studies, he deplored the tendency in American law to narrow the alien's right to work, believing it contradicted the aspiration of a truly modern civilization (1938). This impulse led him to make several of the most searching studies of the rights of defendants in his time ([1958] 1976; 1966*a*). Yet Fellman saw these rights in terms of the broad aims of a civilized society, and thus he wrote that "due process of law is not, primarily, the right of the accused. It is basically the community's assurance that prosecutors, judges, and juries will behave properly within rules distilled from long centuries of concrete experience. For procedural safeguards are abso-

lutely essential for justice. In fact, in large measure justice is fair procedure" ([1958] 1976, p. 6).

Fellman's innovations and receptivity to novel approaches by others are important attributes assessing his career in political science. He was among the first to see how the business of the Supreme Court after World War II would be dominated by issues of civil liberties and civil rights. At the University of Wisconsin, he was a pioneer in introducing these subjects in the late 1940s, both in the undergraduate curriculum and in graduate seminars designed to explore narrower topics among these issues. In guiding graduate students, Fellman set high standards of style, rigor, and craftsmanship while leaving open subjects to be addressed and methods to be employed. Beginning in 1950 several of his students made in-depth studies of interest groups in particular litigation, contributing to the burgeoning interest in the judicial process (Cortner 1964; Manwaring 1962; Vose 1959). His support for eclecticism in the discipline was also exemplified by his acceptance for publication of many articles with a behaviorist orientation while he was the founding editor of the *Midwest Journal of Political Science*.

Although Fellman's methods of analysis in public law have been consistent, the subjects he has addressed include all aspects of free expression, religious and academic freedom, racial equality, and freedom of association, as well as the rights of defendants. His many publications on decided cases in state supreme courts, as well as the Supreme Court of the United States, extended the style, quality, and approach of such scholars as Ernst Freund, Thomas Reed Powell, and Edward S. Corwin. Fellman also brought to his craft a deep knowledge of American political thought derived in part from his teacher at Yale University, Francis W. Coker.

Fellman's expressed concern for academic freedom resulted in his appointments as chairman of committee A of the American Association of University Professors for five years (1960-1964), national president (1964-1966), and chairman of the AAUP Legal Defense Fund (1975-1976). Applying his view of public law to this subject, Fellman believed the independence of scholars from college trustees and administrators was parallel to that of judges from those who appoint them. Concerned with the humblest, most provincial campuses in American higher education, Fellman exerted himself

and the AAUP to protect individual scholars through written rules of tenure and procedure, grievance procedures, and public censorship of wayward institutions.

Fellman's scholarship on a range of civil liberties conflicts continues since his retirement from the classroom.

CLEMENT E. VOSE

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FIRTH, RAYMOND

Raymond William Firth was born in 1901 near Auckland, New Zealand; he was educated at Auckland Grammar School and Auckland University College. Since 1932 he has resided in England, but he preserves his links with New Zealand relatives. The unpretentious style of living and the no-nonsense rationalism which have been characteristic of Firth throughout his academic life have their roots in the modest simplicity of his colonial background.

At Auckland University College Firth specialized in economics, and his first publication was a short monograph on the economics of the kauri-gum industry (1924). In 1924, he came to England to work for a higher degree in economics at the London School of Economics (L.S.E.); his planned field of research was the frozen meat industry of New Zealand. At that time, Bronislaw Malinowski had just been appointed to a readership at the University of London, tenable at the L.S.E. Malinowski's various writings on the *kula* exchange system prevailing in the archipelagoes of eastern New Guinea had been published between 1920 and 1922, and had aroused great interest among economic historians, notably R. H. Tawney, who was at that time an influential member of the L.S.E. academic staff. It was probably a combination of Tawney's breadth of vision concerning the scope of economics, Malinowski's personal magnetism, and Firth's long standing interest in the ethnography and archeology of the New Zealand Maori which led Firth to change the topic of his PH.D. dissertation to *The Primitive Economics of the New Zealand Maori*. It was completed in 1927 and published as a book in 1929.

This thesis, written under Malinowski's supervision, is based on bookwork rather than field

work—that is to say, the facts of Maori ethnography derive from published records concerning the “traditional” society rather than from first-hand inquiry among present-day Maori. Malinowski’s influence on Firth’s general reading in anthropology can be seen from the bibliography, where more than three-quarters of the entries in the general non-Maori section are in German and refer to authors of whom Malinowski approved. The extension to this bibliography which appears in the substantially revised edition published in 1959 reflects not only the expansion of the field of economic anthropology, largely as a response to Firth’s own activities, but also the extent to which Firth’s anthropological vision had broadened out once he was free of the restricting influence of Malinowski.

During the decade 1925 to 1935, Malinowski’s celebrated graduate seminar became a magnet for young scholars of the most diverse backgrounds. Embryo anthropologists who were either regular or occasional participants included E. E. Evans-Pritchard, Raymond W. Firth, Isaac Schapera, Audrey I. Richards, Meyer Fortes, Reo F. Fortune, Ian Hogbin, and Gregory Bateson. From the start, Firth stood closest to Malinowski, and his subsequent achievements reflect most clearly the goals that Malinowski presented to his students. The memorial volume to Malinowski which Firth edited is an enduring expression of Firth’s indebtedness to his mentor (1957).

After obtaining his PH.D. Firth returned to New Zealand, where he prepared his thesis for publication and planned what proved to be one of the most important anthropological field trips ever undertaken. This was his 1928 visit to the tiny island of Tikopia, which is geographically located within the Solomon Islands, though its population, then numbering approximately thirteen hundred, are of Polynesian race and culture. Firth made further, briefer visits in 1952 and 1966.

Firth’s first three publications concerning Tikopia were a general field report and a paper on “Totemism in Polynesia” which appeared in the first volume of *Oceania* (1930–1931, reprinted in [1930–1931] 1967, chapters 1, 11), and a paper entitled “Marriage and the Classificatory System of Relationship” which first appeared in the *Journal of the Royal Anthropological Institute* (1930, in 1964, chapter 4). The first major monograph, *We, the Tikopia* (1936b), was concerned with kinship organization. Since then Tikopia has been the subject of five further monographs (1939; 1940; 1959b; 1961; 1970),

an essay collection (1967), and a score of uncollected articles.

Although these writings add up to a truly formidable achievement, Firth’s Tikopia studies have had less impact on the development of anthropological thinking than their bulk might suggest. Firth’s written style is sometimes florid, but seldom elegant. His closely packed paragraphs make no concessions to the novice reader. He hardly ever resorts to diagrammatic simplification of the argument; indeed he is opposed in principle to model building or reductionism in any form. Firth’s general view of the nature of culture remains close to that of Malinowski, but whereas the latter, while stressing the involuted complexity of the “system as a whole,” implied that the functional implications of cultural conventions are mutually consistent, Firth’s emphasis is on the inconsistencies. He presents his individual Tikopian as faced with a dense mesh of alternative avenues of cultural expression. Faced with such choices the individual makes decisions and it is this process of decision making within a cultural matrix that Firth claims to be able to analyze in his numerous writings on the theme of social organization. But Firth’s reader has to work hard to understand what is being said.

On returning from the field, Firth joined the staff of the department of anthropology at the University of Sydney, which was then headed by A. R. Radcliffe-Brown. Firth was active as editorial promoter of the newly established journal *Oceania*, which was based in the Sydney department. He was acting head of the department in 1931/1932.

Firth then returned to London to take up a post under Malinowski at the L.S.E. He had the status of lecturer from 1932 to 1935, and reader from 1935 to 1944. Malinowski died in the United States in 1942 and Firth was appointed to a full professorship in his stead in 1944.

In the immediate prewar period, Firth was a particularly active member of the Royal Anthropological Institute of which he was honorary secretary from 1936 to 1939. He was president from 1953 to 1955 and has played a prominent role in the institute’s affairs ever since.

In 1938, Firth completed the text of *Primitive Polynesian Economy* (1939), which is wholly devoted to Tikopia. Firth describes it as “in a sense a supplement to my *Primitive Economics of the New Zealand Maori* (1929),” and his next enterprise was designed as a further supplement. The idea was to study the economics of a

community still relatively "primitive" in terms of technological organization, but which was caught up in the world-wide monetary system and international trade in a way that the "traditional" Maori and the Tikopia were not.

In late 1939, Firth and his wife Rosemary Upcott, daughter of Sir Gilbert Upcott, embarked on field research in a fishing community in Trengganu on the northeast coast of the Malay Peninsula. The research was partially frustrated by the outbreak of World War II, but it resulted in a major monograph by Firth, *Malay Fishermen: Their Peasant Economy* (1946), and an additional volume, *Housekeeping Among Malay Peasants* (1943), written by his wife. Firth made brief return visits to Malaya in 1947 and 1963, and the 1966 revision of his book is substantially enlarged.

During the war, the L.S.E. was evacuated to Cambridge, where Firth was also posted in a subcenter of the Naval Intelligence Division of the British Admiralty, which was run by the geographer H. C. Darby and which was responsible for the production of a series of geographical handbooks designed as tools for the contingency planning of naval operations throughout the world. Firth was the principal compiler and editor of the four volumes of this series which relate to the Pacific islands. Originally issued as internal Admiralty documents in 1943, 1944, and 1945, they became more generally available ten years later. Much of the information which they contain would still be hard to obtain from any other source (Great Britain . . . 1943-1945).

As the war progressed, the British authorities began to consider the effects that the war would have on the far-flung British colonial empire. While few people foresaw the rapidity of postwar colonial dissolution, there was a widespread appreciation that radical changes were impending. Yet it was apparent that in many of the colonial dependencies sensible planning for the future was hopelessly prejudiced by a basic lack of information concerning fundamental social and economic facts. Through his work on the Admiralty handbooks, Firth understood the seriousness of these gaps in information and he actively promoted a consciousness of this situation in government circles. The outcome was the formation in 1944 of the Colonial Social Science Research Council, of which Firth was the first secretary. In the immediate postwar period an immense amount of really first-class anthropological research was carried out under the aus-

pices of the British Colonial Office on the recommendations of this body. Its successor, the centrally financed Social Science Research Council, of which Firth was again a founding member, continues to be the principal agency for funding the field research of British social anthropologists.

By the end of 1945, the L.S.E. had been re-established in London, and Firth took on the full responsibility for running Malinowski's former department, which he chaired until his retirement in 1968. During much of this period he was the guide and sponsor of an ambitious long-term project to study, by the methods of social anthropology, the operations of kinship in urban settings in London. The publications which have resulted from this enterprise were disappointing (1956; Firth, Hubert, & Forge 1969).

Firth has subsequently been a visiting professor at universities in the United States, Canada, and New Zealand. Earlier in his career, he was a visitor at the Australian National University at Canberra, where he played a leading part in the discussions that led to the establishment of the important chair of anthropology and sociology in the Research School of Pacific Studies. Since 1965, Firth has accumulated a notable string of honorary degrees, has been a fellow of the British Academy since 1949, and was awarded a knighthood in 1973.

In postwar Britain, the number of graduate students working for research degrees in social anthropology grew rapidly, and before long a vigorous rivalry developed between the two leading centers, Firth's department at the L.S.E., which became the stronghold of a slightly modified Malinowskian orthodoxy, and the Institute of Social Anthropology at Oxford, which was first directed by Radcliffe-Brown, Malinowski's long standing rival, and later by Evans-Pritchard. At a later date, the preeminence of the L.S.E. was challenged by the creation of a vigorous new department at the University of Manchester, under the leadership of Max Gluckman, and by the success of Meyer Fortes in reviving the fortunes of the long established, but languishing, department at Cambridge University. There was also a veritable mushroom growth of lesser institutions. By the early 1960s, social anthropology, which had been virtually an L.S.E. monopoly as late as 1939, was being taught in at least 18 different universities in the British Isles.

In intellectual terms, Firth responded negatively to the new styles of anthropological presentation represented in the work of Evans-

Pritchard, Fortes, Gluckman, and their students, but he recognized, more clearly than some of his senior colleagues, that despite their different approaches to the Durkheimian notion of the functional interdependence of institutions, the various emergent cliques of social anthropologists all had a great deal in common. Moreover he saw very clearly that, in the new postwar circumstances, the miniscule private trust funds through which most British anthropological research had been funded prior to 1939 would prove wholly inadequate. He also recognized that if social anthropologists were to gain access to the much more substantial resources at the disposal of the government research councils, they would have to demonstrate some degree of corporate identity. The point was not lost on his Oxford opponents and in 1946 all of the professional social anthropologists in the country—still numbering fewer than a score—met together at the L.S.E. to form the Association of Social Anthropologists of the Commonwealth, with Radcliffe-Brown as its first chairman. In 1962, Firth, then chairman of the association, took the initiative that led to the important international conference of social anthropologists held in Cambridge in 1963. As a result of this conference, a distinctive series of symposia publications known as the A.S.A. Monographs (subsequently A.S.A. Series, A.S.A. Essays) first appeared. This collection, in its variety and homogeneity, provides a specification of what British social anthropology is all about. Appropriately Firth is the life president of the association.

All of this might give the impression that Firth's principal historical significance for the history of the social sciences is that he showed the entrepreneurial and organizational flair that converted British social anthropology from being the brand name of a rather quirkish style of anthropological practice peculiar to a handful of individuals—which was the state of affairs in the 1930s—into an internationally accepted, widely practiced, and highly respected branch of study, which ranks high in the generally accepted hierarchy of the various social sciences.

Certainly this side of Firth's work deserves emphasis and is of sociological interest in itself. In the dialectical development of British social anthropology, the ancestral doctrine of Malinowski generated the rival creeds of Radcliffe-Brown, Evans-Pritchard, Fortes, and Gluckman. In this mêlée, Firth was from the start a peacemaker among the factionalists, a leader who was more concerned with the future of social anthropology

as an academic discipline than with his own academic reputation. But precisely because Firth chose to be a leader without disciples, it is difficult to spell out with any clarity just where he stands in the spectrum of anthropological opinion. He has not been the founder of a "school"; he has no immediate imitators.

The extraordinary detail of his description of Tikopia culture and society has been mentioned already and is a lasting monument. But though the Tikopia can be cited as examples (or perhaps counterexamples) of all kinds of generalizing anthropological theories, Firth's Tikopia studies did not in themselves serve to break new theoretical ground.

The succession of monographs, essays, and editorial initiatives relating to economic anthropology (1929; 1939; 1946; Conference. . . 1967; Firth & Yamey 1964), and their revisions, are another matter. In the ongoing debate between the "formal theorists" and the "substantivists," Firth has consistently adopted a "formal theorist" stance—that is to say he has maintained that economics is a unified science containing basic principles that have world-wide application. Any persisting mode of human organization, whether primitive or sophisticated, monetary or nonmonetary, market-oriented or subsistence-oriented, capitalist or socialist, has an economic aspect, and general principles relating to such factors as production, distribution, and exchange, the ownership of goods, and command over the means of production are comparable right across the board. In particular, Firth holds that economics is the study of the allocation of scarce resources among alternative ends and that the concept of scarcity is essential to any meaningful study of economics. Firth has viewed with persistent skepticism the various contrary opinions, many of which have their roots in Marxist ideology, which argue that scarcity is simply a product of the particular social formation with which we are most familiar—that of the world-wide, capitalist, market economy—and that there are different types of economy which differ as do species.

The formidable corpus of Firth's writings includes contributions to almost every aspect of social anthropology. Apart from economics, the most prominent themes are social change and religion. A crucial element in his treatment of these topics is the special meaning that he attributes to the concept of social organization.

Firth began to use this expression as early as 1930, but in the period from 1949 to 1960, he

employed it as a polemical weapon of attack against "the rigidity and limitations of a simple structuralism" alleged to be propounded by the Oxford followers of Radcliffe-Brown (Firth 1964, chapters 4, 5, 6). If viewed in its historical context, this phase of Firth's writings was an attempt to rescue from oblivion certain undervalued individualist aspects of Malinowski's functionalism, and in this regard it must be rated a failure. Admittedly structuralism in social anthropology has now become the brand name of the anthropology of Claude Lévi-Strauss rather than that of Radcliffe-Brown, but this transformation was not brought about by the empiricist criticisms that Firth leveled against the latter. But the importance of the concept of social organization for an understanding of the distinctive characteristics of Firth's personal brand of anthropological argument may be inferred from the fact that the expression appears not only in the title of his textbook (1951), but also in that of the *Festschrift* offered to him by his former pupils shortly before his retirement (Freedman 1967).

Two quotations may serve to introduce our discussion of this key concept.

(1) A theoretical framework for the analysis of social change must be concerned largely with what happens to social structures. But to be truly dynamic it must allow for individual action. . . . (1951, p. 83)

(2) . . . so far from the religion of the Tikopia merely reflecting or maintaining the social structure, in some of its aspects it offers avenues of *escape from* society, into personal fantasy, which is then allowed social recognition and credited with social functions. (1964, p. 254)

These are both typical examples of what Firth means by social organization. It is a concept which has a binary relationship with Radcliffe-Brown's social structure. *Structure* is here conceived of as a stable, inflexible system of jural rules; *organization* is the set of cultural conventions which allows individuals to interpret the rules with sufficient flexibility to make the system of structural rules viable at the level of empirical behavior. Changes in these conventions over time provide Firth with his principal tool for the analysis of social change both in the secular and religious spheres of practical activity.

Firth, like Malinowski, has consistently assumed that the core of every serious piece of anthropological research must consist of the detailed field study of a particular culture considered as a total system. The task of the an-

thropologist is to show not only how the system fits together but how it works. Most of Firth's younger colleagues now interpret the "how it works" component of this doctrine very loosely. The mechanistic notion that particular social institutions have particular identifiable functions which can be shown to serve the egocentric needs of the individual (Malinowski) or the sociocentric needs of society (Radcliffe-Brown) have been abandoned. Indeed, the word function seldom appears in any of their writings. Firth's position, on the other hand, is a compromise between the two earlier standpoints, with "social organization" serving as the mediating term.

Closely linked with this special brand of functionalism, which puts so much emphasis on the individual as decision maker, is Firth's consistent empiricist bias, which is reflected in an equally consistent antipathy towards psychoanalytic and structuralist (i.e., Lévi-Straussian) explanations of symbolic action, which emphasize unconscious rather than conscious motivations on the part of the individual. The weakness of such arguments from Firth's point of view is that they evade the guiding principle that speculation must always be tested against the fieldworker's direct and detailed observations.

This empiricist commitment has proved inhibiting. Thus, Firth's restudies of Tikopia and Trengganu culture, after an interval of years, are contributions to the particular histories of these two societies, but they have not led to conclusions about the general processes of cultural change which are other than commonplace. It should be noted, however, that Firth has entered into direct and well-informed debate with the Marxists over their treatment of anthropological data (1972).

Much the same applies to Firth's writings on primitive religion, which are based on Tikopia data. As an atheist, Firth assumes that any honest anthropological fieldworker must start out from the premise that in "real" terms, all religious belief is illusion. The task of the functional anthropologist is therefore to discover utility (or disutility) in the social consequences of religious performance.

Firth is very far from being insensitive to the aesthetic nuances of religion, but he has repeatedly attempted to separate aesthetic activity from religious activity, apparently because, while he approves of the former he views all forms of the latter as a kind of self-deception (Firth 1964, pp. 138-139).

Here is a paradox. Although Firth is clearly

fascinated by the relationship between art and religion, he remains deeply skeptical about all discussions of intangible reality. The overall point of his most recent major monograph, *Symbols* (1973), is that because there have been a plethora of general anthropological theories about symbolism (both religious and other), and because it is easy to find ethnographic counter-examples to any scheme which postulates a general systemic ordering of human symbols, the anthropologist's proper role should be to demonstrate the nonlogical inconsistency rather than the logical consistency of this major aspect of human self-expression, which "not only blurs relationships in the orders of reality; [but] it also has historically been a potent trigger of social conflict" (1973, pp. 427-428). Sometimes one has the impression that Firth looks upon current structuralist fashions of the Lévi-Straussian sort as manifestations of religious superstition, and evil at that!

Finally, there remains for consideration Firth's role as a teacher. Here his influence has been very great indeed. Firth is not at his best in the setting of a formal lecture. He has too many facts at his disposal and his empiricist honesty drives him to put too many of them on show at the same time. But, as the chairman of a seminar discussion, his performance is superb. During his years at the L.S.E., it was above all through the medium of the academic seminar that Firth made his influence felt. In this setting, he treats the contributors to a debate as if they were informants in an unstructured session of Tikopia field work. He manages to extract sense from the ramblings of even the most uninteresting informants. Over and over again, as scores of participants can bear witness, Firth's summing up of a long discursive argument has had the magical effect of converting what had seemed to be a tediously pointless discussion into an innovating discovery procedure of real significance. If he had done nothing else, and he has of course done a very great deal, Firth would by this competence alone have demonstrated that he was a thoroughly worthy successor to the formidable Malinowski.

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FORDE, DARYLL

Daryll Forde, who died in 1973, exerted a profound influence on social anthropology in

England. Throughout its period of expansion, he counteracted the tendency to insularity and narrowness with a broader, more international vision of the subject, made possible partly because of his close relations with American anthropology. He always tried to maintain the initial ties between physical anthropology, archeology, and the cultural and sociological studies of man; to preserve the tradition of wide-ranging inquiry; and to keep abreast of new work in many related disciplines. His early training in geography gave him the bias towards ecological analysis that became the hallmark of his thought, making him a pioneer in a field whose importance is only now being recognized.

Daryll Forde was born in 1902 in England. His first degree was in geography at University College London, where he later wrote his doctoral thesis on prehistoric archeology and, from 1923 to 1928, held the post of lecturer in geography. He received his doctorate in 1928. For the next two years he was a Commonwealth fellow at the University of California, and then, in 1930, he was appointed to the Gregynog chair of geography and anthropology at the University of Wales, the youngest full professor in the United Kingdom. His particular theoretical interests and his association with American studies were evident in his early publications, which stress the balance between human geography and archeology. *Ancient Mariners* (1927) was followed by articles on megaliths and prehistoric metallurgy, and reports on excavations in England, Scotland, Wales, and Brittany.

In the career of any anthropologist, the important benchmarks are the field expeditions. Forde worked in Arizona and New Mexico between 1928 and 1929, and returned to New Mexico later in 1929. In 1934 he published *Habitat, Economy and Society*, an exercise in comparative human geography that instantly became a classic. As the title suggests, the book pays special attention to the interaction of technology with environment. If at this point he had decided to stay in America, his work would probably have continued to combine the antiquarian and contemporary perspectives in American field work. However, his 1935 field trip to southeastern Nigeria was a watershed. There he started the intensive field work among the Yakö of the Cross River that he followed up with another expedition in 1939. The rich data he gathered resulted in many publications on Yakö studies. This field work also led him away from a sustained archeological approach. He

welcomed the full impact of modern Africa. Although always interested in basic theoretical issues, he enjoyed the opportunity to broaden the scope of his academic work that this program of African research provided. Just at that point World War II imposed a moratorium on anthropological field work. But opportunities reappeared as soon as the end of the war was in sight.

In 1944 Forde was appointed administrative director of the International African Institute. A year later he returned to University College London to a newly founded chair of anthropology. The first of these appointments led to the most comprehensive program of publishing and organizing research that any one British anthropologist has ever achieved. The other involved the teaching and reflection by which he worked out his own intellectual position.

Some impression should be recorded of Forde's personal style. Roguish wit, iconoclasm, and abounding joy in life are not surprising in a clever young professor in a provincial university. Normally they would be modified through the years to the austere presence expected of the head of internationally renowned institutions. But in Daryll Forde those very qualities of youth were joined with vision, intellectual power, and warm human concern. Little of his personal magnetism was reflected in his written work. There his style was always correct and impersonal. He became increasingly concerned with covering all aspects of whatever subject he was treating, leaving no careless loopholes in his argument and making no concessions to the poorly educated reader.

His publications on the Yakö came out in specialized articles, starting with "Land and Labour on the Cross River" (1936). Gradually, over the next thirty years, he covered kinship, descent, marriage, local politics, and religion in essays that were republished in one volume, *Yakö Studies* (1964). By then he had published and edited so much of other people's work that his own writing had evidently taken second place. Yet his field studies represent a distinct contribution. He described the complex system of double unilineal inheritance, which has subsequently been reported in other parts of west Africa with interesting variations. He described an oligarchical system of government in which various ties of cult and descent affiliation cut across each other. This work antedated the many analyses of African descent systems and cross-cutting political influences that appeared in the

two decades after the war. The features that distinguished his more comprehensive studies of west Africa also marked his Yakö studies: careful measurement, an eye for internal adaptations, and a fervent belief that anthropological analysis could not be made in a vacuum, but must include the effects of environmental or external political pressures.

One of the most impressive of these large-scale studies was his lecture to the New York Academy of Sciences in March 1953, "The Cultural Map of West Africa," a superb example of his wide synoptic view, mastery of detail, and controlled argument.

Interestingly, his later writings on west Africa showed a slight relaxing of his restrained impersonal style. The Yakö came to life as competitive, wealth-seeking individualists in his 1960 lecture "Death and Succession" (1962). Whereas his first study of Yakö ritual (1951) emphasized its socially integrative aspects, his lectures on *The Context of Belief* (1958a) and "Spirits, Witches and Sorcerers in the Supernatural Economy of the Yakö" (1958b) were concerned with bringing out the elements of individual choice and calculation which he felt were neglected by contemporary accounts of religion. Again, in his introduction to *West African Kingdoms in the Nineteenth Century* (Forde & Kaberry 1967), he emphasized the scope for entrepreneurial talents and leadership as well as the incentives to accumulate riches. In a way, his work can be seen as contrapuntal to the structural functionalism of his day. At many points ahead of his contemporaries and at others correcting their bias, the more they left reality for the sake of elegant abstract models, the more he doubted their fundamental assumptions and questioned their data base.

In each decade he summed up his position in a major statement reiterating his principles. In 1947 "The Anthropological Approach in the Social Sciences" was delivered as his presidential address to the Association for the Advancement of Science. In 1957 a magisterial review article in the *Encyclopaedia Britannica* gave a summary of the whole history of the subject. In 1967 his lecture on "Anthropology and the Development of African Studies" repeated, in the specialized context of Africa, his concern that anthropology keep its broad perspective, remain concerned with biological and historical evolution, and focus always on adaptive processes. He cited approvingly the studies of John Barnes on the Ngoni, Michael

G. Smith on Hausa–Fulani, Jan Vansina on the Kuba, G. I. Jones on the Oil Rivers States, and Igor de Garine on the Massa. This impressive list of younger Africanists, each with his train of students, testifies to the growth of the subject as well as to Daryll Forde's influence. For years no scholar would think of going to Africa for the first time without calling in London to pay respects to Forde and receive his advice. By the 1950s African anthropology had grown enormously in volume, but it grew as an unusually coherent body of communications. It flourished so well largely because of Forde's tireless leadership as director of the African Institute.

When Forde was appointed the institute's director in 1944 and succeeded Edwin Smith as editor of the quarterly journal *Africa*, both the organization and the journal had suffered eclipse and suspension during the war. His task was to build up an international fellowship of Africanist scholars versed in linguistic and ethnographic skills. Frans Olbrechts from Brussels, Vinigi L. Grottanelli from Rome, Germaine Dieterlen from France, together with Portuguese, Spanish, and German scholars, acted together with Max Gluckman as council members in an amity and purposefulness that achieved much. Publishing in French and English, the circulation of *Africa* grew to 2,300 by 1970. At the same time other vehicles of African scholarship also grew. *African Abstracts*, founded to keep track of new information, contained more than one thousand abstracts in its 1969 volume. It was closed then because the sheer scale of operations had grown beyond the scope of the institute. In the same period the Ethnographic Survey of Africa published 82 monographs, 8 handbooks of African languages, and 10 monographs on African languages. Overcoming the tendency for colonial boundaries to circumscribe the range of scholarship, the International African Institute developed international research: The North Bantu language survey was a partnership of British, French, and Belgian linguists, the Africa marriage survey was conducted by a team of anthropologists from the same countries, the Fulani research project was the product of Franco–British collaboration. With the growth of African studies in the world, the International African Institute saw new tasks for itself. One was to help the new African universities by keeping research and teaching staff in touch with each other over the conti-

nent. For ten years beginning in 1959, with the support of the Ford Foundation, it organized international seminars in Africa and published an important series of books on issues of common African interest.

These bare figures and dates suggest the productive energy that Daryll Forde poured into his work. Accordingly, African anthropology held a dominant place in British and world anthropology through its volume and high theoretical initiatives, both nourished by Forde's entrepreneurial talents and scholarship.

MARY DOUGLAS

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FORTES, MEYER

Meyer Fortes, who was William Wyse professor of social anthropology at Cambridge University from 1950 to 1973, is best known for his work on kinship and religion in nonindustrial societies. He has also written on anthropological aspects of psychoanalysis. His field research has been conducted largely in Ghana, among the Tallensi and Ashanti peoples, and ethnographic data from his research formed the starting point for most of his discussions. His theorizing, however, has extended beyond these research sites to include all societies.

Born in 1906 in South Africa to immigrant Jewish parents, Fortes was exposed to considerable cultural diversity. As a university student in Cape Town, he became interested in the education of Cape Coloured adolescents, and that concern led to his choice of a doctoral dissertation theme in psychology at University College London: the cross-cultural testing of intelligence. He also did research relating juvenile delinquency in the east end of London to sibling rank, under the sponsorship of psychoanalyst Emanuel Miller (1933).

Bronislaw Malinowski then invited Fortes to join the anthropological seminar at the London School of Economics as a psychologist, and it was in this capacity that C. G. Seligman arranged for him to undertake field work in Africa. One of his earliest ethnographic publications was in developmental and educational psychology (1938). However, stimulated by E. E. Evans-Pritchard, Raymond Firth, and other members of the seminar, Fortes expanded his professional interests to social and cultural institutions. His commitment to anthropology was strengthened by his friendship with Isaac Schapera and by his meeting in 1931 with A. R. Radcliffe-Brown, whose exponent he became after Radcliffe-Brown died in 1955.

Fortes' interest in the connections between political and domestic relations, first exhibited in his two works on the Tallensi, *The Dynamics of Clanship* (1945) and *The Web of Kinship* (1949), persisted through many, often controversial, articles to culminate in the book based on his Lewis Henry Morgan lectures, *Kinship and the Social Order* (1969). Although one of his earliest articles was on Tallensi ritual (1936) it was not until 1959 that he published *Oedipus and Job in West African Religion*, setting Tallensi beliefs and practices in a broad regional

and comparative context. His various analyses of social institutions, cultural norms, and religious beliefs were all based on principles that are basically psychological, and in his later writings Fortes began to focus more directly on psychological processes. His early studies of sibling rank in the east end of London, his personal experience as a first-born son, and his lifelong association with the theory and practice of psychoanalysis were synthesized in his papers on "The First Born" (1974) and "Custom and Conscience in Anthropological Perspective" (1977).

Fortes said that his instinct was "to shy away from methodological discussion" (1961, p. 211), but this preference displayed itself in a distinctive style of exposition rather than in an effort to avoid using or generating anthropological theory. His achievements as a descriptive ethnographer are manifold; he has made a substantial contribution to the ethnographic mapping of west Africa. But the categories used for description, though derived largely from the cultures themselves, are also partly of Fortes' own devising, and his analysis is innovative rather than conventional, reflecting his own theoretical interests and convictions. His theoretical pronouncements are presented as if they had been arrived at inductively from his field data and are given no explicit abstract formulation; hence his general theoretical position must be reconstituted piecemeal. Indeed, Fortes has stressed the continuous interplay between observation and analysis, between field work and theory, as basic to the scientific status of social anthropology.

Fortes' studies of kinship and political institutions are firmly rooted in the main structural-functional tradition of British social anthropology. Both the societies he studied in detail, the Tallensi and the Ashanti, had political systems described by their members largely in the language of kinship. Appropriately, therefore, Fortes paid close attention to the similarities and differences between what he called the domain of the domestic group and the politico-jural domain. The domain of kinship, extending in these societies well beyond the domestic family, is characterized by a rule of amity or prescriptive altruism, but this rule operates with greatest moral force in domestic relations, while in the politico-jural domain more emphasis is placed on rights and duties. This contrast applies even in societies, like that of aboriginal Australia, in which the widest political order is a kinship polity.

In all domains, a key concept is that of complementary opposition. It is realized in the opposition between patrilineal and matrilineal kin relations or in complementary filiation, whereby a patrilineage is differentiated internally and linked externally by matrilineal connections. Domestically, there is complementarity in the socialized roles of men and women. Fortes also distinguished between descent, a jural concept whereby a person may be linked to others through specific types of genealogical ties, and filiation, the familial linking of a person to a parent by virtue of birth under socially accepted conditions. The two contrasting domains of the family, with its moral imperatives, and of the polity, with its jural imperatives, work together to produce some kind of balance, and in many nonindustrial societies, are united by a polysegmented lineage structure.

Fortes' analysis was typically neither strictly static nor strictly diachronic. He paid relatively little attention to long-term trends but pioneered the analysis of what came to be called the developmental cycle of the domestic group. The various forms of household found in a community at any point of time are categorized as different phases in a repetitive cycle of formation, expansion, and dispersion. Filiation, which unites individuals in this cycle, also divides them into opposed generations.

The opposition and unity generated by filiation, as well as by sex and sibling rank, are regulated in customary practice and belief. Here Fortes stressed particularly the relation between a man and his eldest son, crucial because the birth of his first child makes a man irrevocably a father and hence, in many societies, a potential ancestor. The ultimate source of the rivalry between a man and his son is, however, the Oedipus conflict postulated by Freud, for the foundation of every kinship system is the culturally recognized physical bond between a woman and her child. This constitutes the value for which men in successive generations compete, father against son, brother against brother, and husband against wife's brother.

Fortes has maintained that he has only modest theoretical aims, that he has sought merely "to seek out directly observable mechanisms that play a formative part in social structure" (1969, p. 73). But he has often written as if he were enunciating general propositions, and he has been criticized by Rodney Needham, not so much for trying prematurely to construct grand theory as for giving the impression that he had already

done so while asserting merely vague, commonplace generalizations (Needham 1971). Louis Dumont has accused him of reifying descent groups and of treating them as logically prior to the interpersonal kin relations that sustain them (1971). Edmund Leach, less convincingly, has bracketed him with other structuralists who have overemphasized institutional forms and neglected the importance of the decisions of social actors (1961).

Fortes' influence on the development of social anthropology has been direct, through the work of his students at Cambridge, and indirect, through his publications. The introduction that he wrote with Evans-Pritchard for *African Political Systems* (1940) established the analytic categories used by British social anthropologists for the next two decades. His cyclical model of domestic development has been adopted in many branches of social science, and his emphasis on the distinctive features of various interdependent domains has been incorporated into many subsequent analyses of nonindustrial societies. His readiness to use explanatory mechanisms taken from psychoanalysis, and to look at clinical evidence on mental illness, has encouraged many younger social anthropologists to look beyond narrowly circumscribed social limits of data and analysis. *Oedipus and Job* has appeared in several translations and, along with Fortes' other writings on religion, has had considerable influence on the study of religion, particularly with reference to ancestor cults.

J. A. BARNES

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FREUD, ANNA

Anna Freud was born in Vienna, Austria, on December 3, 1895. The youngest daughter of Sigmund Freud, she is the only one of his six children to take up psychoanalysis as a profession. She is trained as an elementary school teacher and psychoanalyst and became a member of the Vienna Psychoanalytic Society in 1922. She has practiced and taught psychoanalysis since that time and has also actively participated in the affairs of the International Psychoanalytical Association, of which she is an honorary president.

Anna Freud's education as a teacher was inspired by the methods advocated by Maria Montessori. Her psychoanalytic training during those early years in Vienna, however, was unorthodox. In fact, she herself has described her "apprenticeship" as "a haphazard affair" (*Writings of Anna Freud*, vol. 5, p. 511; hereafter cited as *Writings*). She functioned as Sigmund Freud's companion, nurse, and secretary during his prolonged illnesses, and as his confidante and even collaborator in all matters of psychoanalysis. He was not only her father but also her scientific mentor, who watched over her progress with

obvious delight (see *Letters . . .* 1968; *Sigmund Freud . . .* 1966). As an active participant in the early psychoanalytic movement, Anna Freud was exposed to its struggles as well as its pioneering exponents, many of whom, especially Lou Andreas-Salomé, also influenced her work, as did Dorothy Burlingham, her lifelong friend and co-worker.

Having acquired much of her knowledge in the clinical practice of psychoanalysis, Anna Freud helped to initiate the more formal training of others. In 1926 and 1927 she presented her lectures on the technique of child analysis (*Writings*, vol. 1, pp. 3–69) at the newly founded Vienna Institute of Psychoanalysis and soon thereafter established and participated in the first seminar on child analysis, a regular meeting in which cases were presented, technical innovations described, and theoretical conclusions put up for discussion. In this group, such well-known analysts as Erik H. Erikson and Margaret S. Mahler had their first analytic learning experiences.

Even in these formative years she showed her lifelong interest in applying the findings of psychoanalysis to other fields in the hope of preventing unnecessary suffering and future mental illness in growing children. In 1929, at the invitation of the Board of Education of the City of Vienna, she presented a series of lectures for educators and parents in which she "translated" the complex psychoanalytic hypotheses concerning the role of the unconscious, the child's sexuality, the stages of instinctual development, the Oedipus complex, infantile amnesia, and the beginnings of the superego into ordinary language (*ibid.*, vol. 1, pp. 73–133). In 1937 she organized what is probably the first day care nursery for underprivileged children, run on analytic principles. Here, she and Dorothy Burlingham cared for one-year-old to two-year-old infants who came from families living in the worst slums of Vienna.

In 1938 Anna Freud left Austria for London, where she still resides. During World War II, she established and directed, together with Burlingham, the Hampstead War Nurseries, a residential center for homeless and evacuated children. The daily care of more than two hundred children throughout the war years posed a series of practical and administrative problems—such as the procurement of food, housing, and medical care, and the special training of the staff—all of which Anna Freud undertook. But it also provided her with a unique opportunity to make

new observations, which she recorded (on nights when the German bombers were over London and she could not get much sleep in any event) in monthly reports (*ibid.*, vol. 3). In this setting, she and Burlingham grasped the overriding importance of the role of the mother in the mental life of the developing child. They observed that children can withstand the most gruesome experiences so long as they are with their mothers and the mothers do not show fear. Separated from their mothers, however, children receiving even the best physical care and benign treatment by substitute caretakers will show profound developmental lags.

After the war, Anna Freud founded the Hampstead Child-Therapy Course and Clinic, of which she has remained the director. The organization of the clinic reflects her humanitarian and scientific concerns. It began as a training program for child therapists, and in 1952, the central clinical facilities that provide diagnostic services and analytic therapy to children and adolescents were added. There is, furthermore, a well-baby clinic where at any one time sixty children receive pediatric care and their mothers practical and psychological guidance during the infants' first two years of life. The children then progress to a nursery school for normal children, but the educational department also includes classes for the underprivileged and handicapped children and a special group for the blind.

In this setting both trainees and staff are exposed to a great variety of children at different stages of their development, for Anna Freud believes that "no analyst can consider himself *fully trained* so long as his clinical experience and technical skill are confined to only one age or stage of development" (*ibid.*, vol. 7, p. 217). All of the staff members, in addition to providing services and training, are encouraged to record their findings, pool the knowledge they have gained in such systematic endeavors as the Concept Groups or the Psychoanalytic Index, referred to as "a collective analytic memory" (*ibid.*, vol. 5, p. 484), and participate in the extensive research projects carried out at the clinic.

In addition to her activities at the Hampstead Clinic, Anna Freud has lectured to professional and lay audiences throughout Europe and the United States. For several extended periods of time during 1963, 1964, 1969, and 1970, she was visiting professor at the Yale Law School and Child Study Center. She has received honorary degrees from many universities, including

Clark, Chicago, Columbia, Jefferson, Yale, Sheffield, and Vienna. She was also honored by the White House with the first Dolly Madison award, and in England, she received the award of the Commander of the British Empire.

Anna Freud is the author of several books and more than one hundred papers, which are collected in the seven volumes of her *Writings* (see also Bergman & Freud 1965; Goldstein et al. 1973). Apart from her own writings, she collaborated with Strachey in the preparation of *The Standard Edition of the Complete Psychological Works of Sigmund Freud* (1886-1939) and has been an editor of *The Psychoanalytic Study of the Child* since its inception in 1945.

It is characteristic of Anna Freud's entire scientific career that she not only provided needed services for children and training for child care personnel and child analysts, but also used every opportunity to make new observations, reflect on them, raise new questions, and propose new conceptualizations. Firmly rooted in the traditions of classical analysis, she has always sought and accepted data derived from the direct observation of children in a great variety of settings. She applied what she learned in the treatment room to child rearing and education, the care of the physically ill, handicapped, and orphans, and the child who is subject to legal disputes. In turn, she brought what she learned from these settings back to the psychoanalytic situation. In her own words, she has used every "opportunity to maintain a close connection between theory and practice, to check constantly on theoretical ideas by practical application, and to widen practical handling and practical measures with the growth of theoretical knowledge" (*Writings*, vol. 7, p. 227).

As a consequence of this basic attitude, her work has had a wide influence both inside and outside of the field of psychoanalysis. Its impact was no doubt enhanced by her style of writing—her clarity and lucidity, the felicitous phrase, and the ease with which she can express complex thoughts in simple and generally intelligible language. She always cuts through to the central issue, and she invariably throws new light even on familiar problems. Moreover, Anna Freud writes like the great educator she is, leading the readers from what they can personally observe and know into the previously unknown.

Contributions to psychoanalysis. Anna Freud's work has influenced psychoanalytic theory and technique as well as psychoanalytic research. Her slim book on *The Ego and the Mechanisms of*

Defense (*ibid.*, vol. 2) has had the most profound and far-reaching impact of all of her writings. It is required reading in a great variety of undergraduate and graduate programs in medical and law schools and courses on child development, psychology, and social studies. Translated into many languages, it is read all over the world and is one of those rare books in the history of science that for more than forty years has remained of fundamental importance to the basic understanding of the specific area with which it deals.

Building primarily on Sigmund Freud's revisions of psychoanalytic theory, made between 1920 and 1926, Anna Freud scrutinizes the ego's ways of dealing with the conflicting demands of the instinctual drives, the superego, and reality. She concludes that the defense mechanisms chosen by a person depend on the source of danger and on the developmental level he or she has reached. Thus, the little boy who is afraid of being scolded by his parents begins to berate his puppets and soldiers for their misdeeds (identification with the aggressor). As the child develops, the source and the nature of the danger change, and so do the ego's capacities for dealing with it. For example, the immature ego of the child will simply deny what it fears in action, fantasy, or word. The older child's ego will ward off dangers by restricting its own activities and avoiding anxiety-producing situations—tendencies that are most frequently seen in learning difficulties and the common childhood phobias and fears. The adolescent, who is confronted with an intensification of instinctual impulses, will resort to asceticism and intellectualization.

With this book Anna Freud introduced a new developmental perspective on one of man's basic mental operations that are in part observable in his behavior. She thus opened the way to the following novel findings and approaches:

(1) In studying the general tendencies of the ego at different developmental periods, well-marked mental mechanisms and their exaggerations come into view. While some may produce morbid distortions of character, inhibitions, or neurotic symptoms, others are essentially normal means of dealing with phase-appropriate conflicts. She demonstrated that what is appropriate and normal behavior at a particular stage of development no longer can be so considered a few years later. Conversely, some of the most pathological behavior of the adult not only originates, but has a normal counterpart, in the life of the child. Her book thus provided a bridge from

pathology to the study of normality, a major contribution toward establishing psychoanalysis as a general theory of psychology.

(2) Viewing observable behavior against the background of normal developmental processes, Anna Freud provided a new classification of the mechanisms of defense and new insights into their varieties and combinations. In altruism, for example, she described the positive and valuable aspects of displacement and projection which enable people to form attachments. Having repressed and repudiated their own wishes, such persons can nevertheless gratify them by employing projection and identification and by actively seeking to fulfill such wishes in others.

(3) By postulating that "the proper field for our observation [in the psychoanalytic situation] is always the ego" (*ibid.*, vol. 2, p. 6), Anna Freud opened the way to a whole series of innovations in psychoanalytic technique.

These advances in the technique of adult analysis were preceded by the particular technique of child analysis that Anna Freud introduced in 1927 and which she and her followers have continued to practice and to refine. More than most analysts, she has remained concerned with defining the proper field in which analytic therapy is applicable. She has some doubts about "the widening scope of psychoanalysis" (Symposium . . . 1954, p. 608), and still insists that analytic therapy is most usefully applied to what it originally was designed for—the treatment of neuroses in children and adults. In her repeated discussions of the indications and contraindications of child analysis, she has questioned many of the generally accepted criteria and instead offered one essential consideration: children need treatment when their development in one or several areas has come to a halt, and they need analytic treatment when the lack of progressive development is due to an "internalized" conflict, which by definition is inaccessible to external manipulations.

The determination of what an individual child is suffering from requires diagnostic considerations, to which Anna Freud devoted a large part of her second major book *Normality and Pathology in Childhood* (*Writings*, vol. 6). She found that the usual classification of disorders derived from adult psychopathology is not applicable to the child, and a mere listing of symptoms does not give the diagnostician helpful clues since the same symptom—for example, a sleep disturbance—has very different meanings at different developmental levels, while the same

basic disturbance may be expressed by diverse symptoms. She has therefore introduced a new method of developmental assessment, the diagnostic profile. Based on all aspects of psychoanalytic theory, and giving attention to all constituent parts of the child's personality, the profile is far more than a mere diagnostic aid. It is a practical "instrument which imposes balance, completeness, and comparability—not only comparability in assessment of factors within an individual case and between individual cases, but comparability of *reliable* assessment between analysts, with each other and in time. This is an instrument with many potential uses. These include assessment of change over time, compilation of similar cases, comparison of differing conditions, and as a training aid" (Lustman 1967, pp. 821–822).

The comprehensive assessments that the profile permits led to a new classification of childhood phenomena—from the normal developmental processes and the typical developmental difficulties to the various manifestations of childhood pathology. These in turn opened up a new view of the relationship between childhood and adult disturbances, as well as an emphasis on the "variations of normality."

These methodological innovations reflect Anna Freud's continuing commitment to empirical research. She always advocated the use of "experiments provided by nature and fate" (e.g., *Writings*, vol. 4, pp. 141, 224–229, 510–523, 623–624). Thus she highlighted the role of the mother in the young child's development by conceptualizing the observations made in the Hampstead War Nurseries, where children were separated from their parents. She and Dorothy Burlingham studied the role of vision in normal development by investigating blind children (see *ibid.*, vol. 5, part 1, for the research projects inspired by her).

Her own major theoretical contribution, deriving in part from empirical research, is the concept of developmental lines, a central organizing concept that attempts to integrate the various constituent elements of the personality that previously had been studied in isolation. While she herself has worked out only some of the prototypical developmental lines, the full impact of her concept has not yet made itself felt. Its importance was grasped by Seymour L. Lustman who, stressing the scientific leadership of Anna Freud, stated that the concept gave a new direction to theory building and research (1967). Yet this concept, too, has direct prac-

tical applications. It can provide answers to such questions as when is a child ready for school, and point to specific areas in which teachers can be helpful. In order to arrive at an answer one will look at the lines leading from full psychological dependence on others to emotional self-reliance; from full physical dependence in matters of eating and sphincter control toward responsible management of the body; from egocentricity to companionship; and from play with the mother's body to play with toys and from constructive play to work. Each of these lines is influenced by the instinctual drives, the ego, and the superego, as well as by the external social reality as represented primarily by the important persons in a child's life. Viewing regression as a principle in normal development, Anna Freud would also examine the child's regression rate, the specific circumstances, such as fatigue, frustration, in which a child's behavior tends to fall apart, and how rapidly he or she will do so and regain the previously achieved level of functioning. This overall view of children highlights whether or not they have reached the status required for a particular activity, such as entry into school, and discloses harmonies and disharmonies in their development that would often be less visible without an integrative perspective.

Contributions to other fields. Anna Freud's efforts to apply psychoanalytic thought to the practical problems of child rearing and child care go back to her early days in the experimental day care center for underprivileged children. Subsequently she seized the opportunity provided by World War II to extend psychoanalytic principles to the group care of children. When Anna Freud found that the children's development lagged, that they were slow in overcoming reverses, she "attributed these difficulties to the lack of a stable mother relationship" (*Writings*, vol. 3, p. 220) and accordingly introduced small "family groups," in which about four children were assigned to specific workers who, as substitute mothers, were solely responsible for the physical care of these children. According to Anna Freud:

The result of this arrangement was astonishing in its force and immediacy. The need for individual attachment with the feelings which had been lying dormant came out in a rush and in the course of one week all six families were completely and firmly established. But the reactions in the beginning were far from being exclusively happy ones.

Since all these children have already undergone a

painful separation from their own mothers, their mother relationship is naturally burdened with all the effects of this experience. To have a mother means to them equally the possibility of losing a mother; the love for the mother being thus closely accompanied by the hate and resentment produced by her supposed desertion. (*ibid.*, vol. 3, p. 220)

The children showed all the signs of possessiveness, anxiety, and jealousy that arise when attention has to be shared, and the "formerly peaceful nursery reverberated with the weeping of children whose 'mother' has left the room, for instance, to get something from the next room, and whose absence was mourned as if she would never return. Fights among the children multiplied in frequency and intensity" (*ibid.*, vol. 3, p. 221).

Under these conditions few workers would have had the courage to persist in their conviction, but Anna Freud did. Gradually the children realized that their new "mothers" really belonged to them and the frenzy subsided. "At the same time the children began to develop in leaps and bounds" (*ibid.*).

This example has been given in detail because it demonstrates her capacity to make observations, conceptualize what she observed in terms of her previous knowledge, and not only to find a remedy applicable to the specific situation but at the same time to test her conclusions. In addition, this example illustrates an essential, but frequently overlooked, step in the application of psychoanalysis. Psychoanalytic theory is a complex and large body of propositions. In order to apply it to a particular concrete situation, the overridingly important proposition that fits this situation must be singled out. Anna Freud's simple statement, "we attributed the children's lagging development to the lack of a stable mother relationship," represents the selection of the essential element from a vast body of psychoanalytic knowledge, which would readily lend itself to suggest a variety of courses. Her ability to choose that which is most relevant and appropriate has been characteristic of all her endeavors to translate psychoanalytic theory into actual practice outside of the psychoanalytic situation.

She has lectured to, and written for, medical students, pediatricians, teachers, child care workers, nurses, lawyers, and judges. In each instance, her choice of what psychoanalysis can contribute is deceptively simple. For example, in working out guidelines for legally contested child placements, she put the emphasis on "the

child's need for continuity of relationships," and on "the child's sense of time," which differs radically from that of adults. Her collaboration with Joseph Goldstein, a professor of law at Yale University, and Albert J. Solnit, professor of pediatrics and psychiatry and an analyst, who is the director of the Child Study Center at Yale University, resulted in the small book, *Beyond the Best Interests of the Child* (1973), which in a very brief time has had an enormous impact. The concepts introduced in this book, such as "the psychological parent" or "the least detrimental alternative," have only a few years after its publication become part of the legal language used by courts and legislators in widely scattered countries, many of which are in the process of changing their laws governing adoption and custody assignments.

Her lifelong efforts with pediatricians and nurses, however, have been less successful. Repeatedly she has stressed the physician's readiness to accept the idea that emotional factors influence physical processes and their reluctance to realize that bodily pain and discomfort and medical and surgical interventions have a profound impact on the emotional development of children.

All of her teachings to allied professions have been guided by one basic assumption: the unity of the growing child's personality. Repeatedly she has stressed that physicians and nurses see only the sick child, teachers deal only with the well child, nursery school teachers have contact only with the young child, while high school teachers know little about toddlers. Repeatedly she has questioned the existing divisions along age lines, between education and upbringing, theory and practice—in brief, the specialization of training in the children's field. To remedy this situation she has proposed the establishment of a new profession of child experts, who would be knowledgeable in all aspects of child development and care.

Summary. In her long productive life as an analyst, Anna Freud has made vital contributions to psychoanalysis and allied fields. She pioneered in the development of child analysis and in ushering in psychoanalytic ego psychology. She contributed to the development of psychoanalytic theory and to putting that theory into practice. She has trained several generations of analysts and exerted a direct influence on the far larger number of people to whom she has lectured. For she combines in her own person a unique blend of secure knowledge, a relentless

quest to learn from whatever life has to offer, boundless energy, an independent mind, a willingness to look at facts, and an unyielding commitment to put these into practical efforts to help all children. Her personal impact as an inspiring model has been so great because "she is a benevolent, humanitarian scientist and a scientific humanitarian" (Lustman 1967, p. 814).

LOTTIE M. NEWMAN

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FREUND, PAUL A.

Paul Abraham Freund was born in St. Louis, Missouri, on February 16, 1908. He received his

A.B. from Washington University in that city in 1928. In 1938 he graduated from Harvard Law School, where he was president of the *Harvard Law Review*, the zenith of student achievement. After graduation, he stayed on for a year as a graduate assistant to Felix Frankfurter, while he earned his s.J.D. degree.

When he left Harvard, Freund went to Washington as a law clerk to Justice Louis D. Brandeis. As Owen J. Roberts, a member of the Court when Freund was there, said: "In his intimate relation to one of the Justices and to the entire personnel of the tribunal, he had the opportunity to appraise the effect of judicial judgment on varying policies and values, political and economic." That was the beginning of Freund's unbroken commitment to study and analysis of the role of the Court and the constitution in American life.

Following his clerkship at the Court, Freund joined the cadre of bright young men invited to participate in the first Franklin D. Roosevelt administration; young men responding to the needs for creativity and ingenuity that this novel American exercise in national power demanded. There, he served as counselor and advocate, first on the legal staffs of the Treasury Department and the Reconstruction Finance Corporation, from 1933 to 1935, and then as special assistant to the Attorney General, in the Office of the Solicitor General, from 1935 to 1939. He returned to the Solicitor General's office for another tour of duty during World War II, from 1942 to 1946, on leave from the Harvard Law School, where he had been appointed a lecturer in 1939 and a professor in 1940.

The primary business of the Solicitor General's office is to assert and protect the interests of the United States in litigation before the Supreme Court. In the Solicitor General's office, Freund had a command post among those engaged in the making of a constitutional revolution, bringing to fruition the conception of national hegemony first judicially conceived by Chief Justice John Marshall, but thereafter thwarted by decades of states' rights and laissez-faire jurisprudence. The great depression and World War II required flexibility in constitutional meaning and a constitutional theory to match the needs for power to meet and resolve successfully both cataclysms. The Solicitor General's office, with Freund as an intellectual leader, proffered the necessary arguments and convinced what was at first a reluctant, but later a willing, Supreme Court to bring about

the necessary constitutional changes. As advocate before the Court, Freund had the opportunity to argue to jurists who had had experience in taking his counsel. Brandeis was still a justice when Freund first appeared before the Court. And during Freund's career at the Department of Justice, Felix Frankfurter, his teacher, and Stanley Reed and Robert H. Jackson, under whom Freund served in the Solicitor General's office, became Supreme Court justices.

Freund participated in oral argument or in writing briefs in a multitude of cases, some of which still find their way into the law school casebooks and which provided a background of experience that surely enhanced his teaching of them: *United States v. Carolene Products* (304 U.S. 144, 1938), where Justice Harlan F. Stone first adumbrated the distinction between economic legislation, which the Court should treat with great deference to the legislative will, and alleged infringements on civil liberties, which calls for a good deal less obeisance to legislative or executive discretion; *Electric Bond & Share Co. v. S.E.C.* (303 U.S. 419, 1938), in which the government outmaneuvered the public utilities in securing Supreme Court sanction for the Public Utility Holding Company Act; *Coleman v. Miller* (307 U.S. 433, 1939), defined, at least temporarily, a need for judicial abstention from "political questions"; *Tennessee Power Co. v. T.V.A.* (306 U.S. 118, 1939), established the validity of the national government's Tennessee Valley Authority experiment. The cases establishing the constitutionality of the wartime regulation of prices and wages and the validity of the procedures for their effectuation were *Bowles v. Willingham* (321 U.S. 498, 1944); *Davies Warehouse Co. v. Bowles* (321 U.S. 144, 1944); *Yakus v. United States* (321 U.S. 414, 1944). These are but a few examples of a very long list of important Supreme Court decisions in the shaping of which Freund played a role as advocate.

The range of cases revealed a catholicity that has always typified Freund's legal interests. Not all the cases in which he participated, of course, were so fundamental. What may have been his first argument before the Court concerned the question whether a jigsaw puzzle was a game within the meaning of a provision of the tax law. Freund contended that it was and lost (*White v. Aronson* [302 U.S. 16, 1937]).

After the war, Freund abandoned the career of barrister and returned to his role as teacher at the Harvard Law School, where he remained,

except for occasional leaves, until his retirement in 1976. He was appointed Fairchild professor in 1950 and Royall professor in 1957. In 1958, he received the accolade of a university professorship, the Carl M. Loeb chair. This gave him the opportunity to expand his student audience beyond the law school to the undergraduates of Harvard College. This was a recognition by the university that while the law was Freund's profession, he infused its content with insights from wide learning in literature and poetry, philosophy and art, and even from the sciences. His mission, however, was more to bring an understanding of the role of law in a democratic society to undergraduates and graduates alike, than to bring these other subjects to the attention of students of law. From 1946 through 1976, Freund was, in the phrase so frequently invoked by his faculty brethren, the "quintessential teacher." Even during his teaching days he was described by *Time* as a "legend" among law teachers.

Twice he left the classrooms of Harvard during his tenure there: Once to serve as Pitt professor of American history and institutions at Cambridge University (1957/1958); once to be a fellow at the Center for Advanced Study in the Behavioral Sciences, in Palo Alto, California (1969/1970). He also became the director of the Salzburg Seminar in American Studies, where American jurists and scholars taught seminars in American law to European students, during summer sessions. He was a particularly appropriate director for such studies since, at the Harvard Law School, he served for many years as chairman of its program of graduate studies.

In the fall of 1971, Chief Justice Warren Burger, as chairman of the Federal Judicial Center, appointed a committee to study the case load of the Supreme Court and to make recommendations as to what steps should be taken in light of its discoveries. Not unnaturally, as the nation's leading scholar on the Supreme Court, Paul Freund was appointed chairman of the study group, which reported in 1972 that the Court's process of selecting cases for adjudication was swamping it and seriously detracting from its capacities to render judgments and opinions in the cases that it undertook to decide. *The Report of the Study Group on the Case Load of the Supreme Court* (1972) recommended the creation of an intermediate court to pass on petitions for review and to undertake resolution of some of the cases appropriate for additional

review but not calling for Supreme Court judgment. Instead of calming the surprising furor over this fairly technical subject, the *Report* exacerbated it. The report was never effectuated, but the issue it addressed and the resolution of it remained a matter of controversy, with justices of the Court frequently taking to the hustings to defend or attack the study group's proposals.

Like two of his predecessors and mentors at the Harvard Law School, Thomas Reed Powell and Frankfurter, Freund's commitments to legal scholarship largely invoked the oral tradition, outside the classroom as well as within it. He has written no treatises and but a single monograph. His subjects, the Supreme Court and the Constitution, do not lend themselves to encapsulation in one or two volumes. For one who had, what he once described in another as, "a dialectical mind—recognizing principles in collision," it was not possible to write a pseudodefinitive elucidation of the ever-changing constitution and the everchanging causes of the changes. Even his myriad articles have been largely derived from the spoken word. Three of his books—a fourth is a constitutional law casebook which he coedited with colleagues—*The Supreme Court of the United States* (1961), *On Understanding the Supreme Court* (1948), and *Religion and the Public Schools* (1965)—all derive from lectures. He was also the editor of *Experimentation With Human Subjects* (1970).

Freund was in demand everywhere that there was a gathering to discuss and analyze the intellectual issues of the law, and particularly where the subject was the Constitution, the Supreme Court, or its justices. And he responded to these demands with a generosity that has marked no other constitutionalist. His talks ranged from the Jefferson lecture, delivered at the instance of the National Endowment for the Humanities to a prestigious audience of Washington dignitaries, to conversations with small groups of students at the almost infinite number of law schools where he was making his substantial contributions to their extracurricular programs. Freund was a professor at Harvard, but he in fact served as teacher to law students everywhere.

Nor was his service to the life of the mind limited to his classroom teaching or his discourses on important subjects in lecture halls all over the nation. Thus, he was, *inter alia*, president of the American Academy of Arts and Sciences; vice president of the Massachusetts

Historical Society; a member of the Council of the American Law Institute; a member and chairman of the Harvard Society of Fellows.

Like the other truly great legal essayist of our times, Judge Learned Hand, his name came to the forefront as a nominee for the Supreme Court every time a vacancy on that Court occurred. It has been said that President John F. Kennedy, among whose campaign advisers he was numbered, failed to appoint him as successor to Felix Frankfurter because Freund had declined Kennedy's tendered appointment as Solicitor General of the United States. That Freund was never nominated to the Court should come as no surprise. Justices of the Supreme Court get there largely because of their political contributions or as reward for services on lower courts. Scholarship has never been a qualification for that high office. The only two justices to be appointed directly from an academic chair to the Supreme Court have been Felix Frankfurter and William Howard Taft, the first a close political adviser to Franklin Roosevelt and the latter a former president of the United States. The failure to appoint Freund has proved a great loss to the nation, for the Supreme Court has suffered during the Warren and Burger years primarily from the absence of just such scholarly credentials as Freund would have brought to it.

The great bulk of Freund's work has been all but buried in law reviews and in many volumes of collected papers and speeches where it tends to escape the study of many scholars and jurists because his name is not to be found on the spines of the printed volumes or in the catalogues of the libraries that contain them. The Freund canon will someday be collected and published so that future generations are not denied the wisdom and counsel he offered his contemporaries.

On retirement from the Harvard Law School faculty in 1976, Freund turned to completion of a volume of the history of the Supreme Court, the volume devoted to the period of the Hughes Court. Freund, at the time of the creation of the Oliver Wendell Holmes Devise project—the writing of a history of the Supreme Court—was appointed editor in chief of the multivolume series. As with all his work, Freund has been conscientious about the task, a real editor not merely a titular one. Here, as elsewhere, he has raised the quality of the writings of others, to which he has contributed without stint. When brought to fruition, the history will be a re-

markable tribute not only to Oliver Wendell Holmes, but to Paul Freund as well.

Law professors, even the greatest of them, are not the stuff of history, unless like Joseph Story they are both jurist and professor at the same time, or like Justices Holmes, Stone, Frankfurter, and William O. Douglas, they move directly or indirectly from the chair to the bench. It is not that law professors do not affect the course of legal history as much as their legal colleagues on courts, in legislatures, and as executive officials. It is rather that their effects are more subtle and less palpable. As Frankfurter once put it: "The ultimate accomplishment of a thinker is found not in his books nor in his opinions but in the minds of men." Certainly the accomplishments of Paul Freund are to be found in the minds of many men. His is a life of teaching: at Harvard University, where thousands of the brightest and best students were exposed to his wisdom; to Supreme Court justices, governors, cabinet officers, senators, and congressmen; and, not least, to his professorial colleagues, who came to Freund for advice and counseling. And his writings afford light to thousands whom he has never met. As a teacher, however, he left no cult, he taught no dogma. His faith was compassion; his formula was intelligence; his profession was honesty; his weapon was wit; his goal was civility.

PHILIP B. KURLAND

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FRIEDMANN, GEORGES

At the time he entered the École Normale Supérieure in 1923, Georges Freidmann (1902–1977) was 21 years old. Not long before, in 1921, the French Socialist party had undergone its split at the Congress of Tours. The two par-

ties, one socialist, the other communist, did not attempt an alliance until fifty years later, and at the time Friedmann died there was every indication that the alliance of these parties would form the next government of France. During those fifty years, communism ran its course in the Soviet Union. Its destiny would first enrapture and later disenchant the European intelligentsia of which Friedmann was an eminent member.

At the École Normale, he played the older brother to Jean-Paul Sartre and Claude Lévi-Strauss, and in her book *The Mandarins*, Simone de Beauvoir evokes this wary, uneasy figure. The first temptation for this scion of solid middle-class Jewish parents was literature, and in three years (1930–1932) he published three novels (which he never allowed to be reprinted) which traced the portrait of an adolescent in search of his own way. He had learned how to write, and all of his sociological works were marked by a quality and clarity of expression rare among sociologists. Having passed his advanced examination in philosophy, he taught at the lycée of Bourges during 1929/1930. Then he became a research assistant at the Center for Social Documentation of the École Normale Supérieure, an early version of the social science sections of the Centre National de la Recherche Scientifique (CNRS).

This was the epoch when three currents marked French thought: surrealism, existentialism, and Marxism. Friedmann's allegiances were with the last. He collaborated on the reviews *Clareté*, *Europe*, and *L'Esprit* (1926/1927) with Pierre Morhange, Norbert Guterman, Henri Lefebvre, and Georges Politzer. This tiny group initiated the intellectual debate on Marxism in France—a debate that had not yet been widely disseminated. Two highly polemical books came out of this period: *Problèmes du machinisme en U.R.S.S. et dans les pays capitalistes* (1934) and *La crise du progrès* (1936). These two books laid the foundation for all of Friedmann's thinking.

In the first, he showed the consequences of the division and mechanization of labor for the life of workers. The Soviet Union, by proposing five-year plans that were debated in factories and collective farms, permitted laborers to overcome the disastrous effects of the parceling out of specific tasks. The standard of living increased. For the intelligentsia of the West, there appeared to be few shadows on the far side of Europe, while all seemed gloomy in the capital-

ist countries, which were immersed in great crises. In 1930 a translation of Henry Ford's *Progress* was published in France; it justified Frederick W. Taylor's system of time and motion efficiency studies, promoted "scientific management," and offered a vision of a brilliant future for free enterprise societies. To this book and to others of a similar philosophy, Friedmann responded with his virulent *La crise du progrès*, a severe, well-documented depiction of the seven plagues besetting the kingdom of silver. Here the author took on not only labor as such, but the entire economic and financial structure of capitalism.

In the meantime, Friedmann was offered the position of instructor at a vocational school where trades such as carpentry and cabinet-making were taught, and for a start, he apprenticed himself to learn machine tools. At the same time, he conducted investigations on the evolution of labor, learned Russian, and traveled several times to the Soviet Union. Based on these travels, he wrote a book that quickly acquired notoriety, *De la Sainte Russie à l'U.R.S.S.* (1938). Here he set himself at a distance from the Soviet Union, while etching a picture of rare precision and discernment. His criticisms of the Soviet Union, intermingled with high praise, were not accepted by his French Marxist friends; Politzer and Lefebvre would not spare him, as he himself recalled in a footnote to his final book, *La puissance et la sagesse* (1970).

Then came the war. Mobilized as an officer in 1939/1940, he returned after the Armistice to the region of Toulouse, where he joined an underground network of the resistance. Of this period, when he saw many friends (notably Marc Bloch and Maurice Halbwachs) tragically die, he wrote:

The War and the Occupation, shattering the orderly confines of my bourgeois existence as an academician, stirring a new life in insecurity and sometimes in danger, at the low point of a frightful period, were marked by the worst degradation and depravity; but they also caused me to discover in people I did not know, through unforgettable experiences, courage, modesty, benevolence, the active love of my neighbor. During halts for retreat and meditation, the flame was suddenly rekindled. The four "dark years" were, I daresay, great years in my life, in the absence of which what was least mediocre in me might perhaps have been lost for good. . . . (1970, p. 363)

After the liberation of France, he was named inspector general of technical instruction. In

1946, he published his major work, *Problèmes humaines du machinisme industriel*. In this compendium, published close to twenty years before the pathbreaking *Work in America* (Special Task Force . . . 1973), Friedmann raised what would constitute, for two decades, the problematics of the sociology of work. He resumed his discussion of Taylorism and of Fordism, a discussion enriched and deepened by twenty years of reflection and observation, and by intimate acquaintance with what was being done and published in all the industrial countries. In this book, he also introduced Europeans to the most recent works in American social psychology, adding to them a critique that is always subtle in its nuances. To some, he recalled that "human relations" and "industrial relations" always played their part inside global social structures; to others, for whom all that mattered were changes in the macrostructure, he emphasized that these would be swallowed up if the microorganisms of change were not brought into play.

From 1946 to 1960, he occupied the chair of history of labor at the Conservatoire des Arts et Métiers in Paris, a celebrated engineering school, where he taught evening courses, thus permitting workers and technicians to become familiar not only with history, but with contemporary sociology. From 1949 to 1951, he was also director of the Center for Sociological Studies, a section of the CNRS. He appealed to young scholars, to whom he never stopped offering advice and support, and in this position he was one of the great sponsors of sociology in France. Under the common rubric of investigating the problems of industrial society, he enlisted among his collaborators V. and F. Isambert, J. R. Tréanton, J. D. Reynaud, R. Pagès, A. Touraine, M. Crozier, E. and V. Morin, J. Dumazedier, S. Moscovici, B. Mottez, J. Frisch-Gauthier, L. Brams, H. Mendras, R. Barthes, Marie Thérèse Basse, and Jacques Dofny. They went with Friedmann first to the Center for Sociological Studies, and later to the École Pratique des Hautes Études, a graduate research center, of which he became a director in 1948. He also formed, with Fernand Braudel and Charles Moraze, the editorial board of the celebrated journal *Les Annales*.

During his career, he published two more books on the problems of industry, *Où va le travail humain?* (1950) and *Le travail en miettes* (1956), in which he continued to explore the same themes. From 1950 on, he placed

his emphasis on the rotation of workers to different places on the assembly line, on the multivalence of machine workers, and on the enlargement of their tasks. Friedmann regarded all of his experiences—whether in a factory, a restaurant, or an office—with an inquisitive, enthusiastic eye. His books were translated into English, Italian, German, Spanish, Polish, Serbo-Croatian, Czech, Slovakian, Dutch, Portuguese, and Japanese. It therefore was no surprise when the International Sociological Association, of which he had been a charter member, elected him its third president at the time of the Amsterdam Congress (1956). Shortly afterward, the United Nations Educational, Scientific and Cultural Organization named him president of the Latin American faculty of social sciences in Santiago. In 1959 and 1961, he published two volumes of his *Problèmes d'Amérique latine*.

Then, in 1961 and 1962, he published *Traité de sociologie du travail* with the assistance of Pierre Naville and Jean René Tréanton. It involved the collective efforts of students who had sat in on his graduate seminars for ten years and had become his friends. The book was a compendium of research and it traced the outlines of French industrial society in particular, but with reference to other industrial societies. In the same period, he presided over the founding of the journal *Sociologie du travail* (1959).

After 1960, he abandoned the field of work and helped establish a Center of Studies of Mass Communications, with E. and V. Morin and R. Barthes as his principal collaborators. The center undertook to publish a journal, *Communications*, which rapidly became authoritative in this field.

Georges Friedmann was Jewish in his origins and an agnostic in practice, but he wrote a penetrating book on Israel, *Fin du peuple Juif?* (1965). In the preface, he wrote:

In 1940, I received a first shock on discovering the unsettling importance which the fact of being labeled "Jewish" could have for me. I have kept the paper with the letterhead of the Secretariat for Public Education of the Vichy government, informing me that from now on I would be relieved under particular laws. Unless, they suggested to me, I were to beg to be granted special treatment, I could not go on exercising my trade as a professor. I had up to that time been one of those whom the Jewish faithful call "marginal" or "peripheral" Jews. Born in Paris into a family detached from traditional observances, in which "mixed" marriages created no problem, profoundly assimilated to France, to its

styles of life, its culture, I had been surrounded by friends and colleagues, not one of whom raised questions about my "ethnic" origins or my religious beliefs. I had never, though designated as Jewish by my name, suffered from antisemitism. I had never felt, even in school, discriminated against in French society. I had never participated in a rite at a synagogue and, I honestly believe, never met a rabbi.

The theme of the book is a questioning of the history of the Jewish people and the creation of a modern state. Did the history of that state, which was just beginning, put an end to the history of the people of the Diaspora? Those who knew Friedmann knew that he felt solidarity with the destiny of these people. He concluded his preface by underlining: "I have felt all that I owe and wished to pay a part of my debt."

In 1970, he finished his last book, a kind of testament, entitled *La puissance et la sagesse*. It is a curiously composed book, with biographical passages and restatements of such essential themes in his work as the concept of technological civilization, the destruction of the natural milieu, science, and the spiritual revolution. This is followed by a long reflection on Christianity, Judaism, Hindu spiritualism, Marx, and subsequent varieties of Marxism and Communism. He repeated what he never stopped saying, that no revolution in structures would succeed unless it was accompanied by what he called true education: "man taking himself in hand."

Friedmann died suddenly. He was in his home, rereading the maxims of Chamfort, a French moralist of the eighteenth century, that century of Enlightenment to which Friedmann was greatly indebted (he was similarly indebted to Leibniz and Spinoza, to whom he had devoted his doctoral thesis [1946b]).

In the foreword to a collection of essays entitled *Une nouvelle civilisation* (1973), which constitutes an homage to Friedmann, these words can be read: "The school of Durkheim had withered during the period between the wars and, after World War II, there was a kind of sociological 'no man's land.' . . . After the Center for Sociological Studies was created, it achieved its high point with G. Friedmann."

Friedmann had been a forerunner. Well in advance of a large part of the sociologists and economists, he had rejected every doctrinal orthodoxy that would exclusively connect the human problems of industrial work to one or other of the great economic and political systems of his time. He had spoken since before

the war of "technological civilization." Long before the phrase came into use, he discovered the ecological problem that was posed by the "technological milieu" and the degradation of the "natural milieu."

His works have also been defined as the association of the observations of a historian of the present with the queries of the moralist of all times. Rarely have we seen a mind so ample in its breadth, never ceasing until the last day to question, to place in question all over again, his own values, the results of his labors, his ideals. He probed the boundaries of old and new societies, discarded the vague and the conventional, and was always happy to see those who followed him undertake new research with the spirit that always animated his own.

JACQUES DOFNY

(Translated by Richard Koffler)

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FRIEDRICH, CARL J.

The story is told of a colloquium at Harvard University in the early 1940s, at which one scholar after another told his colleagues what the "Unity of Science" meant to him. Finally, Carl Joachim Friedrich, who was to succeed Charles H. McIlwain as Eaton professor of the science of government at Harvard, answered by reading the entries appearing in the library card catalogue under his name. Even then, Friedrich's works comprised efforts in the disciplines of history, philosophy, jurisprudence, economics, and communications, in addition to political science proper, which he conceived in Aristotle's terms as "the master science." During the next three decades of his very active life, he deepened his scholarship in all these areas, extended it to aesthetics, and applied it successfully to the practical concerns of constitution building at all levels—e.g., winning, together with Talcott Parsons and others, the greater Boston contest, and helping to design constitutions for the Federal Republic of Germany, the Commonwealth of Puerto Rico, and the European Community. Carl Friedrich was a man of enormous, almost overwhelming erudition, whose keen awareness of the problems of communication (he always assigned Aristotle's *Rhetoric* in his course on the history of political theory) kept him from being obscure or arcane.

Friedrich was born in 1901 in Leipzig. His father, a distinguished professor of surgery, had pioneered a technique for the excision of the lung, and during the Balkan Wars, he developed a new method for cutting out wounds. His mother was the daughter of a president of the highest judicial court of the second German Empire. Friedrich considered this background an important factor in his intellectual development. He attended the universities of Marburg, Frankfurt, and Vienna, where he studied medicine and heard a course of lectures by Sigmund Freud. The experience of studying medicine led him later to denigrate the alleged methodological advantages of the natural over the social sciences. At the university in Heidelberg he turned to the social sciences and studied under Edgar Salin. He also worked with Alfred Weber; his first published book was a translation of and introduction to Weber's *Standortlehre der Industrie* (1929). It was characteristic of his loyalty to persons and institutions that, in Weber's lifetime, he returned to Heidelberg and participated in the Alfred Weber Institute of

Economics, and also organized and financed the Institute of Political Science, over which he presided for several years.

Friedrich's most important contributions to political science were made in the fields of comparative politics and political theory. He was among the first to write comprehensively about both constitutional democracy and totalitarianism, and his elaborations, refinements, and applications of both concepts were systematic without being procrustean. He thought of these two forms of government as opposed to one another, as suggested by the parallelism of the titles of the two influential books, *Constitutional Government and Democracy* (Friedrich 1937) and *Totalitarian Dictatorship and Autocracy* (Friedrich & Brzezinski 1956) suggest. Never doctrinaire, he adjusted his teachings to changes in the real world and his understanding of it, as reflected in the successive titles of his basic book in this area, from *Constitutional Government and Politics: Nature and Development* (1937) to *Constitutional Government and Democracy: Theory and Practice in Europe and America* (1950; 1968). Similarly, he revised his earlier, pioneering views of totalitarianism, in *Totalitarianism in Perspective* (Barber, Friedrich, & Curtis [1963] 1969).

Friedrich's catholicity of spirit, breadth and depth of knowledge, and ability to communicate with his students both actively and passively—in the sense that they understood him and he understood them—prevented the growth around him of the kind of discipleship that emerged with later European immigrant scholars in the United States, several of whom Friedrich, who had arrived in 1922, helped with their immigration. His influence was widespread, not because he taught a single method or a particular set of substantive views, but because his scholarship and teachings, oral and written, were respected by his colleagues and students. (He liked to address his student audiences as “my fellow students.”) His profound yet comprehensible scholarship combined with considerable organizational and political talents resulted in his elections to the presidencies of the American and international political science associations, the Institut de Philosophie Politique, and the American Society for Political and Legal Philosophy, which he founded to bring together scholars of politics, philosophy, and the law.

Friedrich understood theory to mean “valid generalizations upon observed matter of fact,” and he taught political theory in this sense.

Plato, Aristotle, Cicero, Thomas Aquinas, Althusius, Rousseau, Kant, and Hegel all had arrived at certain generalizations based upon the political “facts” that they had observed, and it was up to the student of political theory and the political practitioner to test or to apply these theories anew. It must have been this understanding of theory that made Friedrich so useful to as diverse a group of practitioners as the creators of the West German constitution and their American military government sponsors after World War II; Governor Muñoz Marín and his associates, who created Puerto Rico's commonwealth status; and the founding fathers of the European communities. He always thought that the plan for greater Boston, which he originated and worked out with the help of Parsons, Walter Bogner, Charles Cherington, and George Walker, was too original to be implemented, although some of its components, like an underground garage, were constructed many years later.

Friedrich, as constitutional consultant, self-consciously placed himself in a tradition that goes back at least to Aristotle. He told his students that as an adviser to the United States military governor of Germany (1946–1949) he was doing exactly what Aristotle and his disciples in the academy had done for the Greek rulers who consulted them. He treated his government clients much as he treated his “fellow” students. He refused to tell them what to do, preferring instead to lay out a number of options and suggesting the probable consequences of each. The choice—whether it was a particular constitution device, a policy, or a course of study—was the responsibility of the client, whether he was a chief of government or a graduate student.

Because he had the gifts of listening and speaking persuasively, he was able to generate innovation in the deliberative groups to which he belonged. Friedrich has been given credit for inventing article 67 of the Basic Law of Bonn, the constitution of the Federal Republic of Germany, which provides for the “constructive vote of no confidence,” according to which parliament cannot dismiss a chancellor unless it first agrees by a majority of its votes upon his successor. This original idea could not have been enacted into constitutional law had he not been as authoritative and persuasive as he was, in German, English, French, and Italian, both in small groups and to large audiences, as well as with the readers of his publications. Similarly,

his many contributions to the constitutional construction of the European community, starting with *Studies in Federalism* (Bowie & Friedrich 1954), in which a number of scholars extrapolated from the range of possible federal functions exhibited in five historic federal systems (Switzerland, Germany, Canada, Australia, United States), and concluded with *Europe: An Emergent Nation?* (1969). In this as in his other works on federalism, he was able to draw on his profound historical knowledge, going back at least to his second published book, which included a one hundred page introduction on the "consociational" theory of Johannes Althusius of the early seventeenth century (1932). Like his predecessor as Eaton professor, Charles H. McIlwain, Friedrich helped to reintroduce historical considerations into American political science, which had developed an ahistorical or even anti-historical bent after World War I. At the same time, he took full intellectual advantage of contemporaneous concerns, for example, in his works on bureaucracy, which helped provide a foundation for later scholarship on bureaucratic politics and organization theory.

To his work as a practical consultant—an adviser to practical men of affairs—Friedrich brought not only his profound historical knowledge, but also philosophical depth and conviction. He was firmly grounded in the Greek and Roman classics, and in his courses he always included lectures on the political theory of the Old and New Testaments. He approached every practical problem of government with a careful, indeed a cautious, weighing of normative concerns. Thus, his writings on foreign policy and on problems of war and peace were informed by his Kantian preferences, as expressed in *Inevitable Peace* (1948). But in this as in all things scholarly, he was scrupulously fair to other philosophical persuasions, e.g., producing one of the clearest English editions not only of readings in *The Philosophy of Kant* (1949), but also of its counterpiece, *The Philosophy of Hegel* (1953), in the introduction to which he declared his own lack of philosophical sympathy with Hegel.

Because of the enormous scope of his work, Friedrich's influence is hard to measure—a circumstance that might have pleased him, because he remained skeptical, in the Aristotelian sense, about measurable precision in politics, and was therefore a critic of quantitativism in the social sciences. He wrote more than forty books, edited more than thirty, and was the

author or coauthor of more than two hundred articles, not counting book reviews. During 45 years at Harvard and 15 at Heidelberg, he taught thousands of students, including future prime ministers, secretaries of state, diplomats, leaders of major political parties in Europe and North America, and other leading public servants, in addition to scores of prominent scholars of successive generations. He was too learned and too wise to be either monocausal or monothematic in his teachings. His influence was exerted more generally through his thorough, impeccable scholarship, the critical brilliance with which he could apply his knowledge to novel problems in original ways, and his lasting commitment to what he called the "humanist core of constitutionalism." Its "detailed formulation," he wrote,

will vary from community to community . . . [but will] contain . . . a common core: the recognition of man's self as a person possessing intrinsic dignity and hence entitled to an opportunity for fulfilling the potentiality of his being. The emphasis may shift from self-preserving to self-asserting and again to self-developing manifestations of the individual human being. Independence, participation, and creativity are universally valued, but their rank order is not fixed nor based on settled, absolute knowledge. Political communities will, therefore, differ in the degree to which they will leave each man alone in the pursuit of his happiness, allow him to participate in the government or undertake in free association with fellow men what is needed for man's self-development. . . . The battle for human rights is a never-ending struggle; we need not hope in order to act, nor need we to succeed in order to persevere. It can be carried forward, if the belief in man which a transcendent justice calls for is maintained as part of the Western heritage. (1964, pp. 115–116)

HERBERT J. SPIRO

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FRISCH, RAGNAR

Ragnar Frisch was born in Oslo in 1895. His father was the owner of a gold- and silverwork firm in Oslo, and it was assumed that Frisch would follow the family tradition. He started as an apprentice in a gold- and silverwork shop in Oslo, completing his probation work and receiving a certificate as a goldsmith in 1920. At the same time, he also attended the University of Oslo, where, according to his own description, he studied economics because it seemed the shortest and easiest subject. He graduated in economics in 1919. In the 1920s, he went abroad to study economics and mathematics more earnestly, spending nearly three years in France. In 1926 he received his PH.D. from the University of Oslo on a mathematical–statistical subject, semi-invariants and moments used in the study of statistical distributions. He lectured at several foreign universities, including Yale, having established himself as a pioneer in the application of mathematical and statistical methods to economic research—i.e., the trend that Frisch named “econometrics.” In an effort by members of the Norwegian Parliament and others to bring him back to Norway, a new professorship was established at the University of Oslo to which Frisch was appointed in 1931. He held the chair until his retirement in 1965, and continued his research and writing up to his death in 1973. His position as a leading economist of the world was symbolized in 1969 when, jointly with Jan Tinbergen, he was the first to receive the Alfred Nobel Memorial Prize in economic science.

Frisch's publications are not fully representa-

tive of his activities or of his influence on the development of economics. He has published only a few books and a number of scientific papers. Some represented significant advances in the application of mathematical and statistical methods to economic research and have become "classics" in the literature. However, he probably influenced research trends just as much through his lectures at various universities, through the circulation of research memoranda, lecture notes, and various drafts that he more often than not did not trouble to polish and publish in conventional form. (When he had clarified a problem for himself, he was more interested in taking up new problems than in preparing final versions of preliminary manuscripts.) Through his organizational activities, too, he greatly stimulated the development of econometrics. A founder and driving force in the organization of the Econometric Society, which has become one of the most important scientific organizations in economics, he was also, for a long period, the editor of the journal *Econometrica*.

When Frisch became a student at the University of Oslo, economics was not a well-developed subject in Norwegian academic teaching. However, he studied some of the old masters very carefully, especially David Ricardo, Alfred Marshall, and Knut Wicksell, and continued to hold them in high esteem. In his teaching he prepared notes on Marshall's *Principles* (1890) that were based on a very close reading of Marshall's work and that were used for decades in teaching in Norway. He also wrote one of the most penetrating analyses of Wicksell's theories. Nevertheless, he soon felt that a new start was necessary in economics to free it from vagueness and obscure reasoning and to establish it as a science more similar to the natural sciences. The main instruments would be the use of mathematical methods and statistics. After his appointment at the University of Oslo, economics became quite different; the introduction of methods, created largely by Frisch himself, soon destroyed the notion that economics was the shortest and easiest course of study.

Through his teaching and research, Frisch strongly influenced all economists educated at the university. Several of his students have held central positions in government and public administration. At times, Frisch also took an active part in the public discussions of economic policy and—particularly in the later years—from

a radical position. After World War II he also served as an adviser to governments in developing countries, especially India and Egypt.

When Frisch started to study economics, the theory of consumer behavior was characterized by obscure reasoning and concepts devoid of operational content. At the same time, quantification based on various kinds of empirical data seemed feasible and attractive. This is probably why Frisch turned to this field in his first important work, "Sur une problème d'économie pure" (1926). Attempting to establish a new and firm basis for the theory and empirical work, he constructed a system of basic axioms from which the other parts of the theoretical structure could be derived by means of strict logical and mathematical deductions. For instance, the existence of a utility function was derived as a consequence of the axioms rather than being directly postulated, and its properties, including its degree of determinateness, were also derived from the axioms. In this way one could rid the theory of much of its earlier obscurity. Frisch was the first to use the axiomatic approach in this field, and though his article was long ignored in the literature, today it must be seen as an early model of a "modern" approach to economic theory. Consistent with his aim of turning economics into a science based on measurement and quantification, Frisch did not rest with an abstract formulation of the theory. Even in this early work, he linked the theory to empirical studies aimed at measuring the main characteristics of the utility structure of consumers. There is a logical thread from this early work through his *New Methods of Measuring Marginal Utility* (1932), to his well-known article "A Complete Scheme for Computing All Direct and Cross-demand Elasticities in a Model With Many Sectors" (1959). This article proposed a method for estimating all the elasticities in a complete system of demand functions—a very large practical task because of the many cross-influences of some commodity prices on the demand for other commodities. Frisch's ideas in this field have formed the basis for numerous practical applications in connection with large-scale planning and forecasting models.

Frisch also contributed to the other main branch of microeconomics, production theory. In his lecture notes from the 1920s on, he improved and expanded production theory by carefully distinguishing between technological

and economic concepts. The main part of his text was published in English in 1965 and is now a standard reference work. Had it been published in an international language when it was first presented in Norwegian, it would have had a much greater impact upon the development of production theory. As early as the 1930s, Frisch included in his Norwegian lecture notes several aspects of dynamic production theory and of the theory of production with several outputs. His approach in the latter field pointed in the direction of what has later come to be known as mathematical programming—i.e., a mathematical approach to optimization for problems in which there are side conditions not only in the form of equations, but also in the form of inequalities.

Influenced by the economic crisis of the 1930s, Frisch turned toward macroeconomic problems. The first prerequisite to the construction of consistent and quantifiable theories in this field is a well-developed national accounting system, and Frisch contributed greatly to this field in the 1930s and 1940s, with regard to both basic principles and to detail. On the basis of macroeconomic conceptions, he soon developed fully dynamic, determinate trade cycle models, the most famous of which was presented in "Propagation Problems and Impulse Problems in Dynamic Economics" (1933). He also took an active part in discussions of economic policy in the 1930s. In several ways his ideas resembled the "Keynesian" ideas developed in England and other countries. Directly, and through some of his students and associates, he influenced Labour party policy, first its policy on the depression after the Labour party government was formed in 1936, then its reconstruction policy in the first years after World War II. Throughout there was a close interaction between his theoretical ideas and his contributions to economic policy.

From a methodological point of view Frisch's analysis of trade cycles led to two innovations as well as to a general approach to studying development in terms of a determinate dynamic system. The first was a clarification of the concepts of equilibrium and disequilibrium, statics and dynamics, and other related concepts. Frisch had worked on these problems for several years, but his ideas became internationally known through his brief article "On the Notion of Equilibrium and Disequilibrium" (1936*b*), which has contributed much to methodology

and terminology in dynamic economics. The other innovation was to combine dynamic structural equations with random shocks in explaining the trade cycles (inspired by, and parallel with, works by Eugen Slutsky and G. Udny Yule). Frisch showed mathematically and by simulations how the structural equations of the dynamic system would act as a system of weights, transforming random shocks into wavelike movements very similar to business cycles. Both the theoretical idea and the analytical methods used by Frisch in this regard have become standard elements of modern trade cycle analysis.

Frisch's Nobel Prize citation made specific reference to his "having developed and applied dynamic models for the analysis of economic processes." After World War II much of Frisch's work was related, directly or indirectly, to problems of economic planning. His interest in the field had started, however, in the 1930s, and in 1934 he published an article on "Circulation Planning" that contained many elements of modern planning theory, pointing toward input-output analysis and mathematical programming. The article, however, was concerned more directly with methods of organizing a sort of exchange system outside the ordinary market system, so as to revive economic activity and use the potential production capacities left idle by the depression. The article did not draw interest at the time, partly because of its unfamiliar mathematics, probably also because its theoretical ideas were presented in the context of proposals for practical and institutional arrangements that were artificial, and that could hardly have seemed attractive either to theorists or politicians at that time.

In the 1940s and early 1950s, Frisch developed the methodology of what he called "decision models" for economic policy—fairly large, empirically specified, mathematical models relating economic variables that are the main concern of economic policy, and for which government may have certain targets, with variables that are used directly as instruments to influence economic development (see especially his paper 1950*b*). Later his planning models took a new form, based more on input-output analysis and mathematical programming, with explicit objective functions or preference functions for optimization. His models started a development that has led to practical applications of large-scale models based on input-output systems,

and supplemented with demand functions, equations representing the structure of taxation, and so on, to national budgeting in Norway. He also developed models especially intended for developing countries, and he took a keen interest in the development of planning methodology in the socialist countries in eastern Europe. (In the latter context it is interesting to observe that a long paper by Frisch on an economic planning model was included, in Russian translation, in the second volume of an important series of books on mathematics in economic research and planning which was published in the Soviet Union in 1961.) Much of Frisch's writing on planning models has never been published in final versions, but there exist a couple of volumes with selected papers (1960; 1976). An important aspect of Frisch's writing on economic planning was his emphasis on optimization. As a consequence, he had to tackle the problem of establishing preference functions. For this purpose, he devised various interview techniques. Used for experimental purposes, they have not yet been put to serious practical use. However, similar methods are now used increasingly in various branches of management science (for a survey and for references to Frisch's work in this field, see Johansen 1974).

Quantification, and, in the end, practical applications were constant goals of Frisch's research program. This emphasis had several consequences for the direction of his research. In the first place, he was concerned with statistical and econometric methodology. He was not satisfied with taking over classical statistical methods, which were designed more or less for experimental situations and for measurements of natural phenomena. He soon perceived the complications stemming from the fact that most economic processes were determined by the simultaneous operation of several equations, so that it was difficult to trace the cause-effect direction in an unequivocal manner. In this connection he emphasized the concept of autonomy of economic relationships—i.e., whether a specific relationship in an economic system continues to hold even when other components of the system change. He also pointed out the identification problem in economic relationships—i.e., whether an economic relationship can be detected and estimated on the basis of observations generated by means of sets of more than one relationship. In his *Statistical Confluence Analysis* (1934*b*), Frisch proposed methods of solving some of the statistical prob-

lems raised by the simultaneous operation of several relationships (and at the same time the existence of measurement errors or "errors in variables"). Although his methods have been superseded by others, his perception of the special statistical problems raised by the interactions in an economic system and his approaches to solving the problems, have had fundamental importance for the development of modern econometric methodology.

Frisch's concern for quantification and applications also led him into the field of computational methods. Methods of computations were always part of his statistical and econometric studies, but he became particularly absorbed in computational problems in connection with his mathematical planning models. Since he emphasized optimization, mathematical programming was his main concern. At the time he started constructing large-scale mathematical planning models, mathematical programming methods and computations equipment were not well developed, and he felt that economists themselves would have to tackle many of the technical problems in the field. He developed methods of optimization for quite general situations, involving many sorts of awkward curvatures of functions and forms of feasible sets. His works in this field contain many interesting ideas and insights, but the methods he developed tended to be complicated and unsuitable to full mechanization, and they have seen only limited application.

This survey of Frisch's work has stressed aspects that absorbed his energies over longer periods of time. The resultant picture is necessarily incomplete, for Frisch's important work ranged over many other fields, including, for example, economic welfare theory, index number theory, oligopoly theory, international trade, and population theory. His scientific productivity was great, although only a fraction was ever published. (His best collection of papers was published in Hungary in 1974.) Some of his publications are hard to read and digest, partly because of his preoccupation with computational aspects, a certain tendency to pursue analytically challenging, but economically less interesting, sidetracks, but also because his development of these ideas was outside the main currents or fashions in economics, and moreover, employed his own terminology. The latter fact was, however, partly the result of his great originality. His enthusiasm and ambition in research were almost always far beyond what

could be humanly accomplished in limited time; thus his research career was full of uncompleted projects. Nevertheless, his ideas profoundly influenced the direction of modern economic science and contributed greatly to its great leap forward in the last several decades.

LEIF JOHANSEN

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FROMM, ERICH

Erich Fromm, psychoanalyst, has developed his widely read ideas on man and society by integrating psychoanalysis with the humanistic tradition in philosophy, social science, and religion. Throughout his career, he has also been concerned with translating theory into practice as a psychoanalyst, social scientist, and political activist in the prophetic mode. Born in Frankfurt, Germany, on March 23, 1900, he studied psychology, sociology, and philosophy at the universities of Frankfurt and Heidelberg, receiving his PH.D. from Frankfurt in 1922, and was trained in psychoanalysis at the Berlin Institute. In 1932 he published his first articles on social character, proposing the integration of Freud's theory of character with Marx's theory of social forces, thus explaining how people develop the motivation required by a particular economic system and why they are attracted to particular ideas, ideals, and ideologies.

From 1928 to 1938 Fromm was associated with the Institute for Social Research, first at the University of Frankfurt, then at its residence in exile, Columbia University. In 1934 Fromm emigrated to the United States to escape the Nazis and lectured at the New School for Social Research, Yale University, Columbia University, and Bennington College. He later served as chairman of the faculty of the William Alanson White Institute of Psychiatry, Psychoanalysis, and Psychology, which trains psychologists as well as psychiatrists in psychoanalysis.

Fromm first gained a large general readership with *Escape From Freedom* (1941), which explained man's unconscious fear of freedom

and the appeal of authoritarian political systems. This book influenced generations of college students and helped to shape intellectual consciousness in America. He became identified as a leading neo-Freudian, together with Karen Horney and Harry Stack Sullivan. Fromm has written that although the three differ considerably among themselves, they share an emphasis on social and cultural factors and a critical attitude to Freud's theory of the primacy of the sexual instinct.

In 1951 Fromm became a professor at the National University of Mexico, where he founded the Mexican Institute of Psychoanalysis. His main reason for moving to Mexico was his response to a group of socially committed psychiatrists who sought psychoanalytic training from him. For the next twenty years, he spent most of his time in Mexico, traveling regularly to the United States to lecture and give seminars. In 1971 he moved to Switzerland where he has continued research and writing.

Over the years Fromm has developed several key themes, progressively elaborating and refining his ideas. His work may be grouped into four general areas. The first is social character, probably his most original concept, which relates the psychoanalytic theory of dynamic motivation to socioeconomic factors. The second is his revision of Freud's theory of psychoanalysis and psychopathology, with particular attention to the issues of aggression and destructiveness. The third includes his critique of industrial society. The fourth is his analysis of religion and its relation to human development.

Social character. Fromm builds his theory on Freud's concept of the dynamic nature of character traits. Character structure explains actions, thoughts, and ideas; it determines what motivates the individual, what he finds most satisfying or frustrating. Although Fromm accepts Freud's clinical description of character orientations—e.g., the anal or the oral-receptive character, he rejects libido theory as explaining character development.

Fromm holds that, although physiological needs must be satisfied, they are not the basic inner forces that determine man's actions, feelings, and thoughts. He wrote:

Only by considering the specific conditions of human existence, and its inherent contradictions, can we understand the basic human forces and passions. Man is a "freak of nature"; lacking the instinctive equipment which regulates the life of all animals, but gifted instead with reason, imagina-

tion and self-awareness, life becomes for him a problem which must be solved. He has to relate himself to others, to find a new rootedness to replace those roots in nature which other animals have; he must acquire a sense of identity (self) and a system of orientation and an object of devotion. (1955a, p. 379)

Character, for Fromm, is the equivalent of the animal's instinctive determinism, which humans have lost. It is the (relatively permanent) way in which human energy is channeled and structured in the process of assimilation and socialization, to satisfy needs for physical survival—the need to acquire and assimilate things, and for emotional survival—the need to be emotionally related to others for defense, work, material possessions, sexual satisfaction, play, the upbringing of the young, and the transmission of knowledge.

The social character is the nucleus of the character structure and is shared by most members of a culture or a social group. It is not a statistical measure or the traits shared by a majority; rather, it is a functional concept to be understood in relationship to the socioeconomic system, particularly the dominant methods of production. According to Fromm, it is the function of the social character to shape the energies of the members of the society in such a way that their behavior is not a matter of conscious decision whether or not to follow the social pattern, but one of wanting to act as they have to act and at the same time finding gratification in acting according to the requirements of the culture. In other words, the concept of social character describes the molding and channeling of human energy within a society for the purpose of the continued functioning of this society.

For Fromm the family is the "psychic agency of society," the institution with the function of transmitting the requirements of society to the growing child.

As an example of social character, Fromm (1951) has revised Freud's concept of the anal character, which he calls the "hoarding orientation." This syndrome of traits includes inner drives for orderliness, saving, punctuality, and respect for authority that meet the economic needs of the middle class in the nineteenth century. In the twentieth century in advanced capitalistic societies, this social character is being replaced by another type oriented to consumption and to fitting into bureaucratic structures through easy adaptation to their rules and regulations. Such a "marketing personality," accord-

ing to Fromm, reduces the capacity for genuine and profound feeling and thought.

The concept of social character helps explain social conflict as well as adaptation. As conditions change, a social character may no longer fit, and the resulting resentment and frustration transform social cement into an explosive force. Moreover, Fromm holds, social needs can, and often do, conflict with needs stemming from the nature of man and its inherent need for love, human solidarity, and the development of reason and creative talents. Fromm has maintained that insofar as a given society does not satisfy these human needs it will cause a "socially patterned defect."

Fromm first experimented with a new method of studying social character in 1931 at the Institute for Social Research at the University of Frankfurt. His immediate interest was knowing how many German workers and employees were reliable fighters against Nazism. In their political opinions respondents were all anti-Nazis. However, a different picture emerged when Fromm and his co-workers distinguished opinions from convictions rooted in the character structure. Their main interest was not social character in its broad sense, but that aspect most relevant to the Nazi challenge: the authoritarian as against the democratic character. To explore character, Fromm developed an interpretive questionnaire using open-ended questions. These were analyzed and coded as a psychoanalyst examines a patient's communications, seeking the unconscious or unintended meanings that reveal character structure.

That study was never published, but the concept of the authoritarian personality was later made the basis of a study carried out by Theodor W. Adorno and his colleagues (1950) that was influential in social research. In Fromm's point of view, that study did not sufficiently distinguish between convictions rooted in the sadomasochistic authoritarian character and conventional conservative or ethnocentric ideology rooted in a different character structure.

In Mexico, with the collaboration of his students, Fromm initiated a study of social character using a more developed version of the interpretive questionnaire (Fromm & Maccoby 1970). All the adults and half the children in a rural village were interviewed. The researchers explored relationships between character structure and such socioeconomic factors as work, methods of farming, and family relationships. Fromm and Maccoby concluded the

analysis of the data by describing a process of "social selection" by which certain character types, in this case the more entrepreneurial, "productive-exploitive" villagers, were better adapted to modern agriculture and the developing capitalistic society; their ascendancy to power, however, was at the expense of other character types less able to adapt. The result was the emergence of negative character traits and behavior (including alcoholism and violence) in those whose character did not fit. The village study also included experiments in change that demonstrated the importance of taking account of character. This methodology was later used in a study of corporate managers in the United States (Maccoby 1976).

Psychoanalysis and psychopathology. Much of Fromm's professional life has been devoted to the clinical practice of psychoanalysis and the supervision of psychoanalysts. A gifted clinician and teacher, he has inspired generations of students with his combination of penetrating understanding of the unconscious, demystification of concepts, and tough-minded compassion. Fromm stresses the importance of understanding the patient's experience and of using language that expresses experience. He demonstrates this approach to dream interpretation in *The Forgotten Language* (1951), where he describes analysis as liberating creativity and critical knowledge about self and others as well as uncovering psychopathology.

Fromm's relationship to Freud and his work is complex. For ten years he practiced as an orthodox Freudian and was a member of the International Psychoanalytic Association. His criticism of Freud includes both Freud's theory and his tendency to turn psychoanalysis into a semipolitical movement demanding ideological purity (1959). However, Fromm's aim in both his teaching and his critique of psychoanalytic theory has been to preserve and develop what he considers to be the essential elements in Freud's discoveries: the art of making the unconscious conscious and the science of the irrational. Despite his critique of Freud, he believes that many of Freud's orthodox followers have diluted the more critical elements of psychoanalysis, transforming the radical exploration of the unconscious into an overly cognitive "ego psychology."

Fromm (1970) criticizes Freud's model of man as overly determined by his social views, including his belief in patriarchy and his bourgeois view of man as basically egoistic. Because

he limited his criticism of social norms to the area of sexuality, Freud was handicapped in developing the full implication of his discoveries. In reexamining the case history of "Little Hans," Fromm argues that Freud's model of man caused him to misinterpret clinical evidence, and especially to underestimate the importance of the child's pre-Oedipal relationship to the mother. Fromm's view of personality was influenced by Johann J. Bachofen's nineteenth-century work on mother right and the war between the sexes (1849). Unlike Freud, Fromm sees mature love as requiring equality between the sexes.

In rejecting Freud's instinct theories, Fromm conceives of psychopathology as rooted in character. In *Escape from Freedom* he describes sadism and masochism as outcomes of a basic need for relatedness, "springing from the inability to bear the isolation and weakness of one's own self. I suggest calling the aim which is at the basis of both sadism and masochism: *symbiosis*. . . . People are not sadistic or masochistic, but there is a constant oscillation between the active and the passive side of the symbiotic complex. . . . In both cases individuality and freedom are lost" (1941, pp. 158-159).

For Fromm, man's inability to endure feelings of powerlessness, uncertainty, and separateness lead to psychopathology. In *The Heart of Man* (1964), he describes the three main pathological forces of the psyche. Two of them, incestuous fixation and narcissism, are elaborations of Freudian concepts, though presented in a new context of relatedness. The third, *necrophilia*, is Fromm's own discovery, elaborated in *The Anatomy of Human Destructiveness* (1973).

In contrast to Freud's view, presented in his later years, that man's destructiveness is innate and rooted in a death instinct necessarily directed either against the self or against others, Fromm sees destructiveness as a result of unfavorable conditions in the child's development leading to sadism or necrophilia. Sadism is the passion for total control over a living being, including the desire to hurt and torture. Necrophilia is the malignant form of the character structure of which Freud's anal character is the more benign form. It is the ultimate perversion, just as its opposite, *biophilia* or love of life, represents the orientation to unification and integrated growth: "Necrophilia in the characterological sense can be described as *the passionate attraction to all that is dead, decayed, putrid, sickly; it is the passion to transform that*

which is alive into something unalive; to destroy for the sake of destruction; the exclusive interest in all that is purely mechanical. It is the passion to tear apart living structures" (1973, p. 332; italics in original).

For Fromm, mental health is rooted in biophilous development and a productive orientation. Few people are pure necrophiles or biophiles: "The pure necrophile is insane; the pure biophile is saintly. Most people are a particular blend of the necrophilous and biophilous orientations, and what matters is which of the two trends is dominant" (1964, p. 48).

Critique of industrial society. Fromm evaluates society according to whether or not its institutions stimulate healthy character development. From this point of view the normal is not the ideal, since normality may describe a social character with pathological elements. In a number of his books, especially *The Sane Society* (1955*b*), Fromm analyzes modern industrial society—both its capitalistic and socialistic versions, and concludes that bureaucratic-mechanistic institutions cause dehumanization and alienation from self.

For Fromm the total practice of life—at work, in the family, as a citizen, and in cultural activity—influences character development. Human development requires above all a society that satisfies needs for security, justice, and freedom—freedom not only from exploitation and tyranny, but freedom also to participate actively and responsibly, to create and construct.

In 1959 Fromm wrote a manifesto for the Socialist party of the United States, and in 1965 he edited a collection of papers by sociologists and philosophers on socialist humanism. Here and in his essay on Marx (1961*a*), Fromm sees "authentic Marxism" as "perhaps the strongest spiritual movement of a broad, nontheistic nature in nineteenth-century Europe." He views Marx as a humanist whose ideal was "a man productively related to other men and to nature, who would respond to the world in an alive manner, and who would be rich not because he *had* much but because he *was* much" (1965, p. ix). He believes that Marx's concept of man and society has been misinterpreted both by those who felt threatened by his program, and by many socialists, led by those in the Soviet Union, who believed that his goal was exclusively material affluence for all and that Marxism differed from capitalism only in its methods, which were economically more efficient and could be initiated by the working class. Fromm's view of

Marx has been endorsed by a group of eastern European philosophers, especially in Yugoslavia, with whom he has been in correspondence.

Along with criticizing the industrial-bureaucratic systems in both the West and the Communist world, Fromm has been active as a leader of the peace movement. In the early 1960s he spoke out for arms control and disarmament and opposed the war in Vietnam. An advocate of détente, he disagrees with the cold war view of the Soviets as a revolutionary society, arguing that since Stalin the Russians have been supporters of the status quo. In *May Man Prevail?* (1961*b*), he debated the leading American theorists of military and foreign policy, challenging their view of human motivation and their understanding of social change:

Our present thinking is a symptom of a deep-seated, though unconscious defeatism, of a lack of faith in the very values which we proclaim. We only cover up this defeatism by concentrating on the evil of communism and by promoting hate. If we continue with our policy of the deterrent and with our unholy alliances with dictatorial states in the name of freedom, we shall defeat the very values we hope to defend. We shall lose our freedom and probably also our lives. (1961*b*, p. 252)

Fromm has been criticized (Schaar 1961) as a utopian whose view of human nature is overly benign and who does not take sufficient account of the realities of power. In fact, while optimistic about human nature and its capacity for creative growth, Fromm, as much or more than any other psychologist of his time, focuses on destructiveness and the potential for ending human life on this planet. Although his concept of health is demanding and his view of the good society seems sketchy at times, Fromm supports positive moves in the direction of health and sanity, particularly reforms in work and education that stimulate active participation. However, he believes that today, in an age of nuclear weapons and dehumanizing bureaucratic organizations, the danger of destruction from detached and overintellectualized leaders is great. Preserving the world will ultimately require radical changes in the social system to increase economic democracy and affirm a human ideal based on "being" rather than "having."

Religion. As a youth Fromm was a devout Orthodox Jew. When he gave up religious ritual and practice in his late twenties, he did not lose his faith in the truth of religious teachings about the nature of man. Unlike Freud, who saw

religion only as an illusion, a wish for an all-powerful protecting father, Fromm (1950) distinguishes between the authoritarian, infantilizing and the humanistic, liberating aspects of religion. Fromm's view of human development is essentially that of the great humanistic religions: to overcome greed, hate, arrogance, and egocentrism; to develop a strong, courageous, and compassionate heart; to experience the fullest truth, including one's deepest despair, in order to love life more fully.

He wrote:

Mental health, in the humanistic sense, is characterized by the ability to love and to create, by the emergence from the incestuous ties to family and nature, by a sense of identity based on one's experience of self as the subject and agent of one's powers, by the grasp of reality inside and outside of ourselves—that is, by the development of objectivity and reason. The aim of life is to live it intensely, to be fully born, to be fully awake. To emerge from the ideas of infantile grandiosity into the conviction of one's real though limited strength; to be able to accept the paradox that everyone of us is the most important thing there is in the universe—and at the same time no more important than a fly or a blade of grass. (1955*b*, p. 203)

In *You Shall Be as Gods* (1966), he interprets the development of Biblical concepts as a struggle toward humanistic awakening and the overcoming of idolatry. In an open letter to humanistic Christians (1975), he wrote:

Idolatry is not the worship of certain gods instead of others, or of one God instead of many. It is a human attitude, that of the reification of all that is alive. It is a man's submission to things, his self-negation as a living, open, ego-transcending being. . . . The modern concept of alienation expresses the same idea as the traditional concept of idolatry. The alienated man bows down to the work of his own hands and the circumstances of his own doing. . . . Today's idols are the objects of a systematically cultivated greed: for money, power, lust, glory, food and drink. Man worships the means and ends of this greed: production, consumption, military might, business, the state. The stronger he makes his idol, the poorer he becomes, the emptier he feels. Instead of joy he seeks thrills, instead of life, he loves a mechanized world of gadgets, instead of growth he seeks wealth, instead of being he is interested in having and using.

Fromm believes that these religious statements are truths that would be experienced by anyone who challenged illusions and was willing

to face the roots of his anxiety, depression, and hopelessness. For him the ideal of liberation, of being rather than having as expressed in the Old Testament prophets, in Master Eckhardt, in Zen Buddhism, and in the humanist philosophers, points to a development of character and practice of life that is not only more humanly rewarding but is also the only direction toward human survival in a world of limited resources and unlimited possibilities of destruction.

MICHAEL MACCOBY

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FURNIVALL, JOHN S.

John Sydenham Furnivall (1875–1960) completed the natural science tripos at Cambridge University in 1899; passed the relevant examinations to enter upon probationer's training for the vaunted British Indian Civil Service (ICS); and in December 1902 arrived in British Burma still under training. For the next twenty years, he went up the ladder of that extraordinary bureaucracy. From subdivisional officer to district commissioner to settlement officer and finally to district commissioner (i.e., chief magistrate of a "state" or "province"), Furnivall, completely bilingual, traveled throughout Burma and studied the country's history from the beginning of the British connection in the 1820s. Upon his formal retirement from the ICS in 1923, he briefly returned to England, but almost immediately, in 1924, he decided to return to Burma as a lecturer at several colleges, later amalgamated as the University of Rangoon, and as the founder and organizer of book clubs, magazines, and adult educational enterprises. In 1931, he again returned to England to embark on a series of studies in Holland and Dutch Indonesia; to serve as ICS lecturer on Burmese language, history and law at Cambridge; and to undertake research for the British Burma Government during World War II. After Burma's

independence was formally declared on January 4, 1948, the newly independent régime invited him to return to serve the Prime Minister's office, chiefly as advisor on planning. There he remained until the spring of 1960 when once again he retired to Cambridge. Although he planned to return to Burma as a special professor of economics, he died later that year.

Throughout his official career, Furnivall wrote a large, and to some extent unknown, number of official and classified papers. His published writings, begun in 1909, in the *Economic Journal* (London), continued indefatigably until 1959. At his death, he left a considerable body of unpublished manuscripts, chief of which is "A Study of the Social and Economic History of Burma" (1959).

The corpus of Furnivall's writing has two main foci: the history, culture, and political and economic administration of British and Independent Burma, and an intensive and extensive examination of the roles and missions of the colonial systems in southeast Asia. He started with the British colonial system in Burma, and to a lesser extent with that in Malaya, worked on the Dutch system in Indonesia, and had begun to examine the United States colonial experience in the Philippines as well as the French system in Indochina. This effort became part of a "grand design" of which he completed and published two major components (1939; 1948).

Although his social philosophy appears to have been an amalgam of nineteenth-century British liberalism and Fabian socialism, Furnivall was never concerned with sociopolitical labels. Social science concepts of efficiency, progress, plural society, welfare, social justice, and cultural integration dominate his writings and served him as tools of inquiry and norms for judgment. He found that colonial powers frequently brought some progress and efficiency to their colonial empires, but at the expense of cultural disintegration and alienation. Alien domination—political, economic, and cultural—gave rise to nationalist movements seeking to oust the colonial power. Frequently, in colonial empires, different groups of individuals live together geographically, but maintain separate cultures and organizations. Thus, its members have difficulty developing common values and institutions. Anticolonial success, he indicated, is therefore followed by the difficult search for both the individual and collective autonomy that is necessary to reintegrate the formerly alienated or plural colonial society. How to rid the inde-

pendent society of the ill-effects of the forces set in train by the colonial past, and how to bring about freedom and cultural integration were, he insisted, the essential tasks for the emergent independent régimes.

Furnivall was profoundly aware of the fact that independence alone could not bridge the gap between the precolonial and colonial experiences. Tirelessly he reiterated his liberal, non-Marxist, social democratic prescription:

The task of the new government was to weld the component peoples into a united nation; to re-integrate social life from the village upwards in an organic national society; to instill into Burmans [and the nationals of other emergent states] the discipline of social and economic life in the modern world; and to equip Burma [and other states] with the political and economic institutions of a modern [democratic] state. The process was certain to impose irksome restrictions, arouse resentment and engender resistance; yet . . . [the regime] could not enforce the restrictions without the consent of the people, even when these did not appreciate the need for restrictions. ([1958] 1960, pp. 130–131)

Thirty years after independence took root in the states of southeast Asia, after incessant years of insurrection and civil and international wars, after years of analysis and prescription by others, the basic Furnivall analysis and prescription are still valid. What more could one ask of any single scholar-activist?

FRANK N. TRAGER

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GALBRAITH, JOHN KENNETH

He is among the preeminent gadflies of the Western world and can claim more readers than most other economists put together. He is the leading antitheoretical theorist of our time, who has attacked conventional economic abstractions as empty, but who has not hesitated to propose bold generalizations to describe the workings of technological society. He is, consequently, the only contemporary economist whose words are followed by squads of critics prepared on a moment's notice to write lengthy refutations.

John Kenneth Galbraith was born in 1908 near Iona Station, Ontario, in a Scotch farming community. He survived an indifferent rural education, went to the Ontario College of Agriculture at Guelph, then to the University of California at Berkeley for a PH.D in agricultural economics (1934). He arrived at Harvard University as an instructor the following year, in time for the intellectual fireworks ignited by the publication of John Maynard Keynes's *The General Theory of Employment, Interest and Money* (1936). Between 1941 and 1943 Galbraith presided in Washington as the country's chief price fixer (deputy administrator) at the Office of Price Administration. In 1945 he was a director of the U.S. Strategic Bombing Survey, set up to determine the effectiveness of Allied air raids on Germany and Japan. He returned to Harvard as Paul M. Warburg professor of economics until his retirement in 1975.

Intellectual influences. At Berkeley, Galbraith found the free-wheeling discussions conducted by Leo Rogen, Robert A. Brady, and Ewald

Grether a revelation. Berkeley in the 1930s abounded with radical ideas, Marxist in particular, but it was the new heresies then being propounded by authors with legitimate pedigrees that Galbraith found persuasive. A. A. Berle and Gardiner C. Means came out with their celebrated book, *The Modern Corporation and Private Property* (1933), which documented the control of more than half the industrial economy by two hundred nonfinancial corporations. Joan Robinson and Edward H. Chamberlin began to build the theoretical apparatus to study the much neglected phenomenon of market power. At Harvard Galbraith encountered Alvin Hansen and Seymour Harris, evangelists of the new Keynesian activism. In reading he found kindred spirits in such disparate personalities as Thorstein Veblen and Evelyn Waugh, both detached and acid.

Principal writings. The evolution of Galbraith's leading ideas can be traced in four books conceived over a period of twenty-odd years. *American Capitalism: The Concept of Countervailing Power* (1952a) was written in the flush of postwar prosperity and, not implausibly, the author set himself the task of explaining why the system worked. Characteristically, he began by attacking the conventional explanation. The system did *not* work, as adherents of the competitive model proclaimed, through the rough and tumble of many small buyers and sellers trading in the marketplace. The days of laissez-faire had ended with the great depression. Decisive parts of industry, trade, and banking were now concentrated in the hands of a small number of large corporations. Prevailing theory had

simply become irrelevant. In the spirit of Joseph A. Schumpeter, Galbraith recognized concentration as inevitable. The development of modern technology required huge masses of capital. That capital had to be protected from the vagaries of the market. Size protects. The antitrust view—and this was a theme to which Galbraith would return again and again—was hopelessly archaic. No serious person could contemplate the disestablishment of half the economy.

Since reference to the invisible hand did not explain the operation of a modern economy, another explanation had to be sought. Here Galbraith introduced the theory of countervailing power. Large organizations, such as the giant firm, begat large counterorganizations, such as the industry-wide union. Private economic power was held in check by the countervailing power that arose on the other side of the market. Where countervailing power did not arise on its own, it was the duty of government to nurture it. Government had, in fact, done this in the case of farmers. The same could be done for others.

Countervailing power belongs to the genus of the self-regulating mechanism. It was—and was explicitly offered as—a new rationale for non-intervention in corporate life. Galbraith noted one flaw, of no small consequence in light of subsequent events. The mechanism does not work in times of inflation. Then, businesses and unions form a coalition and mutually exploit the public through price and wage increases.

The next title, *The Affluent Society* (1958a), was an ironic comment on a nation that produced an increasing abundance of goods, but in which cities declined; transportation deteriorated; education, housing, and medical services sank into crisis; and the poor remained poor despite annual increases in the gross national product. By 1958, the so-called beneficial effects of the Korean War had worn off. By that time Galbraith had set aside his preoccupation with countervailing power and had taken a stance that was to remain typical for the rest of his career: that of mocking critic of a system that is out of balance. The criticism extended liberally to defenders of that system, particularly teachers of economics.

Orthodox Keynesian policy bore the brunt of Galbraith's attack. It was wrong to assume that government merely had to ensure full employment and that the allocation of resources would then take place automatically in optimal response to consumer demand. It was also wrong to assume that economic growth would solve all

problems. The composition of increasing output was distorted by imbalances, impossible for very long in the classical world of the sovereign consumer, inevitable in the Galbraithian world of the large corporation which engaged in want creation through the devious arts of advertising and salesmanship. The result of this shift Galbraith emphasized as the dependence effect: consumer demand was not independent; it was dependent on the blandishments of the producer. The quest for social balance, he concluded, lay in increasing public expenditures for deficient social services. A large part of the funds could come from cuts in excessive military budgets. The New Class of the educated and affluent, as Galbraith saw it, was the prime candidate to lead the fight. It was simply in their own interest to do so.

After serving as ambassador to India (1961–1963) during the Kennedy administration, Galbraith completed a detailed examination of the mature corporation, the archetype for several hundred firms which make up the concentrated, and dominant, part of the economy. The results appeared in *The New Industrial State* (1967b). The mature corporation has no inherent upper limit in size, since size is an advantage in the deployment of technology, the amassing of capital, and the mastery of the market. The entrepreneurial corporation was run by a single individual, often the founder or one of his heirs, but the mature corporation is too complex for such management and requires direction by an interdependent group of upper and middle level administrators and technicians who collectively command an enormous range of knowledge. This group Galbraith called the technostructure. The technostructure plans the firm's sources of supply, internal operations, the sale of products, and, in fact, seeks by all means within its power to replace the market with planning. Above all, the technostructure works for its own survival and decision-making autonomy. This manifestly includes freedom from stockholder intrusions. Survival and autonomy are served by growth, and growth replaces profit maximization as the chief goal in the industrial system.

In this system, the producer manages the consumer, albeit imperfectly. But the point is crucial, for it reverses a basic sequence in economic reasoning: the origination of instructions with the consumer; their transmission through the market; compliance by the producer. The reverse, or revised sequence (compare the dependence effect), engineered through advertising

and salesmanship, plainly means that no optimum consumer satisfaction is possible. The larger implication is that the system of mature corporations sells not only goods but a way of life—one that increases the revenues of sellers, but not necessarily the well-being of consumers.

The industrial system is closely dependent on the state for: regulation of aggregate demand; support of research and development; and control of unemployment and inflation. The logic of Galbraith's model leads to policies he has frequently advocated, namely, wage-price controls and state action to fill planning lacunae, as in housing and health. It also leads to a profoundly skeptical attitude toward the military establishment which, as part of the industrial system, also seeks growth and finds all the necessary rationalizations to justify it. In commenting, Galbraith offers the hope that the monopoly of the industrial system on social purpose can be broken; that the industrial system will become a diminishing part of life; that aesthetic goals will command increasing attention. "The danger to liberty lies in the subordination of belief to the needs of the industrial system," not in the association of corporations and government ([1967*b*] 1971, p. 401). That association is a *fait accompli* and must be accepted as the inevitable outcome of sophisticated technology and large-scale organization.

Galbraith's political activities led him to the chairmanship of Americans for Democratic Action from 1967 to 1969. Despite widespread discomfort among economists, they extended due recognition in electing him president of the American Economic Association in 1972. Galbraith's next major work, *Economics and the Public Purpose* (1973*b*), is the definitive statement of his mature views. He now uses the term planning system instead of industrial system to call attention to the problems of coordination characteristic of a modern economy. The planning system has about 1000 firms; the market system 12 million. The 1000 produce more than the 12 million combined. Power resides in the planning system: the power to plan prices and outputs; to dispose of modern technology and to grow; to sway consumers and the state; and to extract favorable terms of trade from the market system. The two systems develop unevenly. The strong go to the state and get the services they need. The weak do without. Hence the highly uneven development of public services. Power and uneven development have no place in neoclassical, or textbook, economics but

are central features of present-day reality. "Left to themselves"—and this statement epitomizes the difference between Galbraithian and neoclassical views—"economic forces do not work out for the best except perhaps for the powerful" (1973*b*, p. xiii).

Galbraith's theory of reform turns on the emancipation of belief from the thralldom of the planning system. That system can deliver more and more commodities but cannot deliver us from the pain caused by uneven development, grossly unequal incomes, environmental disruption, inflation, inadequate public services—the well-known social ailments. Pain, aided by persuasion, can produce public cognizance, which is awareness that the needs of the public are not the same as those of the planning system. The job is to pry the state loose from the grip of the planning system and place it at the disposal of the public. This is the central issue facing the electorate.

An adequate program would have to include elements of what Galbraith calls democratic socialism. We must, he argues: (1) run the weak parts of the market economy—housing, urban transportation, health care—by public authority; (2) encourage small and weak firms to merge and equalize their terms of trade with the planning system; (3) nationalize the military contractors; (4) replace the stock of the largest corporations with public bonds; and (5) plan the planning system—supply the coordination needed in the absence of a self-regulating mechanism.

Galbraith's influence. By the time *Economics and the Public Purpose* was written, a marked split had taken place in the economics profession. The neoclassical majority continued to believe, with suitable qualifications, that the concept of a self-regulating market was still a sufficient description of reality. The post-Keynesians, as they came to be called, still a minority, held that the task of economists was to chart the decline of the market and the passage of power to large organizations—in short, to devise theories and policies in conformity with the current reality. Galbraith must be regarded as one of the chief formulators of the post-Keynesian point of view, and one of the most telling critics of orthodoxy.

Galbraith's position can be contrasted with that of Keynes. Keynes believed in managing the level of effective demand but otherwise in letting the market alone. Galbraith believes in managing the market itself and replacing parts of it with

planning. Galbraith's wit and derisive style, together with a deep abhorrence of technical jargon, have won him a vast public—not to mention professional—audience, to whom he has brought home the fact that textbook economics is beyond doubt in a sorry state and that, as he would have it, a greatly improved alternative exists.

M. E. SHARPE

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GALLUP, GEORGE

The man whose name is most closely identified with public opinion research once described his life as a series of happy accidents. George Horace Gallup has always considered himself a pragmatist and technician rather than a scientist or theoretician. He built his career by asking such questions as: What data are needed to solve the problem? How can one best obtain these data? The Gallup Poll came about through the coupling of his lifelong interest in politics and democracy with his working experience in journalism and marketing.

Born in 1901 in the small town of Jefferson, Iowa, Gallup attended the local high school and the University of Iowa, where he pursued his interests in psychology and journalism. He was introduced to the world of surveys as an under-

graduate, when he was hired to interview readers of a St. Louis newspaper. In the custom of the time, the survey asked: "Do you read the editorials? . . . the international news? . . . the classified ads?" To Gallup this technique seemed a faulty means of measuring readership. Reflecting on the matter, he decided that a better measure of actual readership would be to show the reader yesterday's paper and go through it with him column by column. As a graduate student he persuaded the Des Moines *Register & Tribune* to sponsor such a survey, and he used this survey for his doctoral dissertation. His findings were sharply different from those obtained by the earlier method. Editorials, for instance, had low readership; advice to the love-lorn and obituary columns were surprisingly high. Gallup's method of measuring newspaper and magazine readership became widely adopted and was still in use more than fifty years later.

After earning his doctorate at Iowa in 1928, Gallup spent two years on the faculty of Drake University and a year at the Northwestern University School of Journalism. During these years he continued his experiments in measuring newspaper, magazine, and radio audiences by means of sample surveys, and his work earned him the attention of national advertising agencies. In 1932 he was persuaded to abandon his academic career and go to New York as director of research and marketing for Young & Rubicam.

His interest in politics had been whetted as a result of his marriage. His mother-in-law's narrow election as secretary of state in Iowa prompted Gallup to think of applying his survey methods to politics. Although many newspapers conducted local "straw polls" at the time, the only generally accepted forecast of national elections was that in *The Literary Digest*. This magazine had successfully predicted every presidential election winner from 1916 through 1932 by means of mass mailings of ballots to millions of telephone subscribers and automobile owners throughout the country.

Gallup was well aware of the potential bias that could result from the relatively affluent sample provided by telephone and automobile listings in the 1930s and from the differential response rate among various categories of respondents that the *Digest* would receive. He was convinced that accurate results could be achieved by systematic selection of much smaller samples and by expanded questioning through a longer questionnaire. He found support at this time from a newspaper syndicate

that agreed to test his ideas in the 1934 congressional elections. On the basis of past voting records, a few "barometer areas" were selected and mail questionnaires addressed to a quota sample of households. These were supplemented by personal interviews where response rates from particular income groups were disproportionately low. The experiment was successful, and on the basis of a contract to provide the syndicate with a weekly column, Gallup established the American Institute of Public Opinion. Its first release appeared October 20, 1935.

Though other market researchers had also begun to take political surveys at about this time, Gallup's regular exposure in newspapers around the country made his name better known. When the November vote was in and it was clear that Gallup had correctly forecast the Roosevelt landslide, while the *Digest* had predicted a Landon victory, the new sample survey methods were vindicated and the American Institute of Public Opinion, now referred to increasingly as the Gallup Poll, became a national institution.

In 1939 Gallup sent a colleague, Harry H. Field, to Europe to establish institutes of public opinion in Great Britain and France. Though interrupted by World War II, both resumed operation after the war and continued to thrive. In the postwar years, similar institutes were established in other countries and the Gallup organization became world-wide in scope. In the United States, Gallup's reputation brought him many contracts for marketing and audience research surveys.

The 1948 presidential election, in which Gallup, in common with other major polls, incorrectly forecast victory for Thomas E. Dewey, provided a sharp setback to the success story. Gallup steadfastly upheld the validity of his methods and systematically began to search for what had gone wrong. The major reasons soon became clear. First, quota sampling methods had resulted in a slight Republican bias. Then, polling had stopped too soon to catch a last-minute tide toward Truman. The polls had overconfidently predicted an outcome that was, in hindsight, far from certain. Applying these lessons to his future work, Gallup improved and refined his procedures, and the 1948 election mistake, far from signaling the end of polling, became a mere incident in its continued progress.

Gallup has been a strong publicist for his methods and his views of their importance. He

sees polls as "The Pulse of Democracy" (his title for a 1940 book)—a source of enlightenment that presents an alternative to the clamor of contending pressure groups claiming to represent public opinion. Although he accepts his role of election prognosticator as an opportunity to demonstrate the reliability of his techniques, he feels that the real value of polls lies in their ability to provide a vehicle for popular expression: a means of measuring and publicizing public satisfactions, concerns, fears, knowledge, beliefs, wants, and intentions. He was among the first to discern the value of asking the same question repeatedly over time to gain information about trends in opinions. Opinions on "the biggest problem facing the country" and approval of presidential performance, for example, have been regularly monitored since 1940 or earlier and provide a rich source of historical data.

Both the man and the poll have been subjects of criticism ever since 1935. Those who oppose majority opinion as revealed by Gallup are quick to attack the wording of his questions and other aspects of his methodology. Some have suspected him of latent Republican or Democratic bias. Academic critics consider his approach to social research to be simplistic and his poll mere journalism. Others argue that public opinion is more (or less) than the mere summation of individual responses to a question. But the frequency and fervor of these attacks has diminished over the years, as Gallup has steadfastly continued to do just what he set out to do.

The enormous growth of sample surveys since 1935 would of course have occurred without Gallup. Market researchers, behavioral scientists, and others were all developing sampling and questionnaire techniques at the same time, and the pressing need for fast, economical data collection in a rapidly expanding society would have guaranteed the same result. But Gallup, as the earliest and most articulate spokesman for public opinion polls, and the Gallup Poll, as the first and most widely circulated continuing survey, became identified with the industry, and the industry with Gallup. This role he has played with eloquence and honor.

PAUL B. SHEATSLEY

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GERSCHENKRON, ALEXANDER

Alexander Gerschenkron (1904-1978), Harvard University economic historian and social scientist of encyclopedic range, was born in Odessa, Russia. Shortly after the Bolshevik revolution of 1917 his family moved to Vienna, where Gerschenkron completed his secondary and academic education. The University of Vienna, at which he received his doctor's degree in 1928, was one of the major strongholds of neoclassical economics on the European continent, with the spirit of the famous "Austrian School" very much alive. Yet, Vienna was also the cradle of "Austro-Marxism," the most vibrant and sparkling variety of all European socialist thought. The sensitive young academic could not remain untouched by these intellectual crosscurrents and by the unique cultural ambience of the central European "City of Light."

At the time of Hitler's invasion of Austria in March 1938, Gerschenkron was an associate of the Austrian Institute of Business Cycle Research. He managed to escape and to make his way to the United States. He spent six years at the University of California at Berkeley, at first as research assistant and subsequently as lecturer in economics. In 1944 he accepted a position on the research staff of the Board of Governors of the Federal Reserve System in Washington; he became chief of the foreign area section two years later. In 1948 Gerschenkron joined the faculty of Harvard University. He taught European economic history and Soviet economics, and was director of the economics division of Harvard's Russian Research Center until 1956 when he gave up this position and withdrew from teaching in the Soviet area. At the time of his retirement in 1974, Gerschenkron held the title of Walter S. Barker professor of economics, and was director of the Economic History Workshop. In the same year he was

awarded an honorary D.LITT. degree at Oxford University.

Early work. Gerschenkron's first two monographs, which dealt with major issues of post-war policies, gave some premonition of his enduring scholastic concerns. *Bread and Democracy in Germany* (1943) was an eloquent plea for breaking the economic power of the Prussian Junkers as an indispensable precondition for the democratic development of post-war Germany and for a peaceful Europe. In his work on *Economic Relations With the U.S.S.R.* (1945), Gerschenkron was guardedly optimistic about the prospects of cooperation between the wartime allies after their victory over Nazi Germany. In both cases events did not develop as expected, yet this did not affect in the slightest the soundness of Gerschenkron's basic diagnosis. *Bread and Democracy in Germany* remains to this day a most reliable and lucid account of a crucially important episode in modern European history. The slim booklet on problems of Soviet-American economic relations stated with hitherto unprecedented clarity the basic institutional and economic facts defining the foreign trade potential of the Soviet Union, and laid a groundwork for all later work in the field.

The "Gerschenkron effect." The rise of the Soviet Union to the position of one of the two superpowers brought an upsurge of interest in its economic growth. The problem of measurement was among the first on the agenda. In view of the wartime performance of the Soviet Union, it was hard to maintain the view that the economic expansion of the late 1920s and the 1930s had been a mirage. Yet the rates of over-all growth as depicted by official Soviet statistics seemed to be much too high and inconsistent with some data on physical outputs to be accepted at face value, although there were good reasons to assume that, in Abram Bergson's words, no "double book-keeping" was involved.

Gerschenkron, along with Bergson, led the efforts to resolve this difficult problem. His contribution to the 1947 symposium in the *Review of Economic Statistics* constituted an important step in this direction; yet the major breakthrough came with *A Dollar Index of Soviet Machinery Output, 1927-28 to 1937* (1951). Gerschenkron took as his point of departure two propositions, which had been adumbrated in some earlier writings on the subject but had never been fully explored. He pointed out that the Soviet index

of industrial production, which was based on the preindustrialization prices of 1926/1927 as its weights, was bound to yield a higher rate of growth than an index based on prices of a later year marking a more advanced stage of industrialization: the fastest growing items, whose costs would show a relatively stronger decline over the period in question, would be affecting the total index more powerfully when measured in prices of the early year. (It would be the other way round, one might add, if the period under consideration had been short enough to make the increase in output come from a more intensive utilization of existing capacity rather than from new plant.) This difference, known to statisticians as a Laspeyre-Paasche discrepancy, is compounded whenever goods that had not been produced during the base year are allowed to enter the index at prices which are set at the (relatively high) cost of the first year of their mass production *and* which reflect a powerful inflation that took place during the period under consideration. Gerschenkron next tested these propositions. He focused on performance of Soviet machine building, which was the fastest growing branch of the rapidly expanding Soviet industry and which underwent the most drastic change in its output composition during the first decade of Stalinist industrialization; more particularly, it showed the highest incidence of new goods introduced after the base year. In setting up his alternative "Paasche"-type index, Gerschenkron chose 1939 dollar prices as weights; this was, he explained, analytically equivalent to using Soviet late period prices. The results were striking; while the official Soviet index showed a fourteenfold increase in machinery output during the years 1927/1928-1937, Gerschenkron's "dollar index" showed a slightly more than five-fold increase. The point was reinforced when Gerschenkron used American machine building in the years 1899-1939 as a control group and found the same high sensitivity of the index to the change of weights from early year prices to the late year prices for the whole period as well as for particular subperiods (in one particular instance such a change of weights resulted in a difference between pronounced growth and marked decline). His subsequent application of the same approach to the Soviet iron and steel industry and to the oil industry brought confirmation of the "Gerschenkron effect" from the opposite end: indices of industries, which were well-established on the eve of the industrializa-

tion drive, and showed a less spectacular growth as well as a smaller incidence of entirely new products, proved much less sensitive to change in price weights. The same was true, to a lesser extent, of the index of electrical power output which grew very rapidly but was relatively homogeneous. No doubt, the rate of Soviet industrial growth during the period under consideration would remain impressively high also under the Paasche regimen. But its edge over Western growth rates which were likewise derived from indices based on late year prices would be markedly reduced.

"Advantages of backwardness." The *Dollar Index* whose findings quickly gained acceptance among students of the Soviet economy was followed within a year by a pathbreaking effort in a larger problem area. In his "Economic Backwardness in Historical Perspective" (1952, in 1962), Gerschenkron argued that backward countries of late nineteenth-century Europe had the ability as well as the incentive to grow faster than older industrial countries have done in the past and in the present because the late-comers could make a shortcut to levels of technology that early arrivals had attained through a sequence of intermediate stages. This implied that new members of the club had yet another advantage over their seniors: the capital stock of their rapidly expanding modern sector would tend to be, on average, younger and more up-to-date. Besides, while the new and largely capital intensive technology was superior to the old over a range of scarcity relationships, its attractiveness was enhanced by the fact that, contrary to conventional assumptions, labor in the sense of a seasoned industrial work force was scarce rather than abundant in underdeveloped areas. Gerschenkron credited Thorstein Veblen for having recognized the role of borrowed technology. He pushed the argument a step further by setting up a "gradation of backwardness" and by arguing that the ability to grow rapidly was positively correlated to the size of the gap between the average level of technology of the country in question and the best-practice technology of advanced countries. Yet he pointed to a grave dilemma when he noted the presence of significant indivisibilities and complementarities which made the great spurt both crucially important and exceedingly difficult to carry out. The situation called for a catalyst that would spark the industrialization process and—in Gerschenkron's terms—act as a substitute for such "normal" prerequisites as adequate private

funds and ample domestic markets. At medium levels of backwardness this role was performed by investment banks as in France, Germany, and Austria; in Russia and Hungary, which were further behind the English front runner, active intervention of the state had to do the job. (In the case of Russia, the state intervention showed an added twist: retrogressive institutional arrangements which existed in agriculture were utilized, and even tightened, in order to promote the accelerated modernization in the urban sector.) Another characteristic trait of the catching-up processes was a stronger stress on producers' goods than on consumers' goods as compared with the English prototype; this was due, in Gerschenkron's view, to the fact that technological progress in the second part of the last century had been more rapid in the first group than in the second. Eventually, and true to the best form of Marxian—Hegelian dialectics, the drastic "substitute" strategies would eventually outlive their usefulness as a result of their successes, and give way to more moderate ones.

The ingredients of this ingenious construct had multiple origins. There had been increasing stirrings within the established theory, with strong attempts to upgrade the importance of the concepts of economies of scale and of externalities and to use them in the analysis of economic development. Another major influence stemmed from Gerschenkron's early exposure to Marxism in its most refined Austrian version. He had high praise for Marx's sense of discontinuities in technological progress and in capital accumulation as well as for the emphasis on the active role played by the state in breaking the logjams in development, although he felt that the insights of the last mentioned category did not fit well into the standard "basis-superstructure" scheme. Naturally enough, such ideas were resisted. The concept of "advantages of backwardness" and of "substitute" strategies went strongly against the grain of the *natura non facit saltum* approach based on assumptions of smooth interaction between steady extension of scientific knowledge and gradually shifting factor proportions. Hence, while Gerschenkron's breadth of vision, originality, and elegance of presentation were widely acclaimed, his argument was often misunderstood. Although he emphasized the significance of differences in natural endowments, and noted the role of foreign trade in promoting transfer of technology, he was reproached for ignoring both.

Nor did Gerschenkron ever deny, Stephen L. Barsby's (1969) and Richard L. Rudolph's (1973) allegations to the contrary notwithstanding, that "a leisurely growth" was possible, but he did insist that a failure to exploit advantages of backwardness to the full was bound to let the sluggish country fall more and more behind others which had risen to the challenge. Moreover, Gerschenkron never implied that the "tension between potentialities and actualities" was bound to trigger rapid growth in a quasiautomatic way. He expected it to provide opportunities which could be used or mishandled; and in essays on Austria, Bulgaria, and Italy, he convincingly portrayed several spurts which never got off the ground because of halfhearted and misguided governmental policies.

Comments by economists who were in sympathy with the major thrust of Gerschenkron's reasoning had more substance. Ronald Findlay (1978) pointed out that the notion of advantages of backwardness held true only within a certain range of development: it could not possibly apply to primitive societies. Gerschenkron would undoubtedly agree; he was careful not to claim validity for his construct beyond the limits of Europe at the turn of the century. Indeed, he also made it clear that in a country like Russia the possibility of developing a modern machine tool industry was severely restricted by a shortage of requisite skills. Henry Rosovsky (1961), who tested the Gerschenkron model against the realities of Japanese development and found the concept of the state-promoted industrialization spurt fully applicable, argued that the shortage of industrial labor did not exist in Japan or in western and central Europe, for that matter; moreover, Japan's high comparative advantages in textile production, coupled with a deliberate policy of fostering small-scale technology whenever feasible, postponed the shift toward producers' goods until another round of big spurts at a later stage of industrialization. (Interestingly, a strong support for the concept of "gradation of backwardness" with all its implications came from a writer who was evidently unaware of Gerschenkron's work when he first stated his own position [Gomulka 1971].) A few additional remarks may be in order. It is arguable that the leading role of producers' goods in the Russian industrialization of the 1880s and 1890s was not so much a result of particularly great advances of foreign technology in that area as a natural response to a steep increase of demand for these goods that was generated by the

massive state-directed program of railroad construction; it was this upsurge in investment activity that provided a vehicle for the large-scale infusion of modern technology into the Russian iron and steel industry. (No one described this more illuminatingly than did Gerschenkron.) Moreover, a faster rate of industrial growth is bound to go with a greater share of producers' goods in total output unless there is a decline in the capital-output ratio—a point that Gerschenkron readily recognized with regard to the world as a whole, but which would also have validity for individual countries with a modicum of appropriate natural resources and with a nonnegligible capacity in the producers' goods sector at the beginning of their great spurts. Lastly, some of Gerschenkron's initial formulations could be somewhat relaxed. "Development blocks" might come in smaller and larger sizes; complementarities could be loosened in spots by less than complete synchronization in investment programs, with foreign trade and varying capacity utilization playing their part; the cutting edge of the economies of scale is not equally sharp everywhere—a circumstance that allows for a measure of economic dualism. Actually, Gerschenkron took note of most of these qualifications whenever he discussed specific cases; and none of them could invalidate the thesis about the importance of discontinuities in industrialization, particularly when the indivisibility-*cum*-complementarity argument is linked with the notion of wide technological gaps and of the crucial role of investment in filling them.

The stability of dictatorship. Gerschenkron's treatment of Soviet economic development after 1928 was another variation on the general theme of backwardness. He viewed the Stalinist industrialization drive as the most striking example of the great spurt, both in its unprecedented rapidity and in the extent of coercion used to effect it. Yet he expected the post-Stalin relaxation to be much more limited than that which had followed earlier great spurts in Russian history. The reasons for it were political. The new revolutionary régime, as distinguished from its age-old Tsarist predecessor, was in dire need of legitimation; the call for discipline and sacrifices in order to catch up with the advanced and fundamentally hostile capitalist West would serve the purpose. Under such conditions, to slow down would be asking for trouble. Here was another paradox that seemed to fly in the face of common sense. The notion that greater

deprivations could mean more stability seemed odd; yet it was profoundly true. One might argue that countervailing tendencies should have received more emphasis. As was repeatedly pointed out, the costs of the brutal and crude methods of Stalin's "initial socialist accumulation" were bound to become intolerably high as the reserve of the unutilized peasant manpower was approaching exhaustion, pressures for higher living standards coming from the increasingly urbanized population were gaining strength, and economic burdens of the "competition of two systems" in the nuclear age were mounting. The vested interest in tension and in ordering people around as much as possible still persisted. Yet neither considerations of static and dynamic efficiency, nor consumers' demands could be relegated to the back seat much longer without pushing stresses and strains beyond safety limits and without inhibiting the attempts to narrow the gap in technology. Gerschenkron did not deny that concessions to consumers had been made and that there was a modicum of over-all decompression. Yet he viewed it as a harbinger of growing instability—although in the introduction to his *Continuity in History* (1968) he noted that unsettling effects have not yet materialized.

A brief sketch cannot conceivably provide more than an inkling of the richness of Gerschenkron's work. Only a few salient points have been discussed. The finely chiseled details had to be left out, although, in this particular case, great spurts of innovative thought must certainly not be allowed to overshadow the extensions, refinements, and explorations in depth; suffice to mention the learned essay on philosophical foundations of the continuity concept, the fascinating debate with Rosario Romeo on problems of Italian industrial development, or the seminal paper on the agrarian policies of prerevolutionary Russia. Gerschenkron's scholarly interests ranged across the whole spectrum of social sciences and beyond: the beautiful essay on *Doctor Zhivago* and the devastatingly erudite review of Nabokov's translation of *Evgenii Onegin* have become classics. He will be remembered as one of the great poly-histors of our time.

ALEXANDER ERLICH

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GEYL, PIETER

The career of Pieter Catharinus Arie Geyl (1887-1966), one of the most outstanding Dutch historians of the twentieth century, embodied a deep paradox. When in 1940 he was interned as a hostage by the Germans, he had already transformed the understanding of the history of the Low Countries, yet he was virtually unknown outside the Netherlands except to a handful of specialists. Only his activity as a Dutch supporter of the Flemish movement in Belgium during the 1920s and 1930s had brought him local notoriety, especially when he was expelled by the Belgian government. After World War II, he quickly gained international recognition as a historical critic and essayist, while at home his innovative ideas, which a few decades before had been attacked by traditional

historians, became part of the generally accepted historical picture.

Geyl was of mixed German and Dutch origin. His father was a medical doctor in Dordrecht, and his early retirement, due to mental illness, created emotional and financial hardship during Geyl's youth. The family moved to the Hague, where he attended a Gymnasium. In 1906 he entered the University of Leiden as a student of Dutch language and literature, but he was drawn to history by C. H. T. Bussemaker, and turned to this field after a novel he wrote was devastatingly faulted by the distinguished critic Albert Verwey. He completed his doctorate in 1913 under the guidance of P. J. Blok, a sound and somewhat dreary scholar, but probably the best Dutch historian of his generation. His dissertation on Christofforo Suriano, the Venetian resident at the Hague from 1616 to 1623, was a traditional work, a study in political history drawn directly from the sources, but it was already notable for the swiftness of its research and writing.

Geyl then gave up his embryonic career as a Gymnasium teacher and in 1914 moved to London as correspondent for the *Nieuwe Rotterdamsche Courant*, the preeminent daily newspaper of the Netherlands. This post gave him direct contact with outstanding political leaders in Britain and a reputation as a quick, lively writer. As a result, he was soon well known among intellectual and political circles in England. A year after the end of World War I, a chair in Dutch studies was created for him at the University of London, with the support of the Dutch government. His salary was not munificent and he supplemented it with work as an unofficial press attaché for the Netherlands embassy. Insecurity, however, did not keep him from taking up the controversial Flemish question in Belgium, to the annoyance and sometimes dismay of the Dutch government.

The Flemish issue provided Geyl with the theme of his early writings. Viewing the Flemings as brothers of the Dutch, he soon challenged the interpretation of Low Countries history that had been most clearly expressed in the *Histoire de Belgique* (1900–1932) by that country's most eminent historian, Henri Pirenne. According to this view, Belgium since its earliest history had been essentially a separate country from Holland, and the implication followed that the Netherlands had no fundamental ties to Belgium and therefore to the Dutch-speaking Flemings in Belgium. In this interpretation, the

separation of the northern and southern Netherlands into distinct countries and peoples during the late sixteenth-century revolt of the Low Countries is seen as the culmination of their historical development over many centuries; by contrast, their unity under the Burgundian dukes in the fifteenth and early sixteenth centuries had been a historical accident. Pirenne's view, which most historians in Holland accepted and adapted to their own national history, was to Geyl essentially inaccurate. He had publicly rejected it in lectures at University College London in 1920, and he published various articles and lectures expanding his argument in *De Groot-Nederlandsche Gedachte* ("The Great Netherlands Idea"; 1925). Geyl then began a monumental project based upon this idea that resulted in the publication of the three-volume *Geschiedenis van de Nederlandsche Stam* ("History of the Dutch Nation"; 1930–1937).

The "Great Netherlands" of these works was a nation in a special sense of the term: a linguistic community possessing or seeking political form and expression in its own state. Geyl therefore excluded from the "Great Netherlands" the Walloon provinces that had been part of the Burgundian Low Countries and that had become the culturally and politically dominant part of independent Belgium in the nineteenth century. He dismissed or disregarded other theories of the nation, both the view of the nation as the creation by states of a historic community of shared institutions and sentiments, and the anthropological view, which saw it as a community sharing an entire range of customs and attitudes, not merely language. What he did see, vividly and clearly, was that the speakers of Flemish in nineteenth- and early twentieth-century Belgium, where French was the language of culture and the chief language of government, suffered many deprivations and hardships. He failed to keep in mind, however, his own repeated warnings about anachronism—specifically, that before the modern age of mass politics and culture, those who spoke Dutch–Flemish in the southern Netherlands had suffered little or no disadvantage. His strong political commitment to at least the federal reorganization of the Belgian state—if not its actual breakup, with the Flemish provinces joining the Netherlands—is strongly reflected in his essays on Dutch–Belgian relations.

In his political activity, Geyl favored the Flemish activists, but the flirtation of extremists, some of whom were his close friends, with

theories of violence and with German racism led him to emphasize democracy as well as nationalism. The rise of the Nazi movement in Germany moved him to a passionate advocacy of political freedom and thus to an estrangement from those Flemish separatists who did not share this commitment.

Geyl's Great Netherlands idea was extensively developed in his strictly historical works. He demonstrated, with a wealth of evidence and argument, that the formation of separate states in the northern and southern Netherlands during the sixteenth-century revolt was not the result of profound cultural, religious, or political differences. Rather, it stemmed directly from the outcome of military events. Neither Catholicism nor Calvinism had been characteristically indigenous to south or north; each section had been consolidated by political and military victory—by the Spanish Habsburgs in the former case, and by the forces of independence in the latter. Geyl recognized that the two regions had grown apart over the next two centuries, and he deeply regretted that the experiment in unity under King William I had failed.

In all these studies, Geyl neglected the question of the status of the French-speaking provinces in a polity committed to predominance of Dutch-Flemish speakers. Although his views on the Flemish question in the nineteenth and twentieth centuries won anything but universal acclaim, his reinterpretation of the sixteenth-century revolt was so persuasive that within two or three decades it replaced the older picture.

While Geyl was working on his Great Netherlands theme in London, he began studies that became fundamental for a new understanding of the place in Dutch history of the House of Orange. Where it had been taken for granted, except by the minority of Dutch Catholics, that the country was essentially Protestant in character and that the Orange stadholders under the Republic and the kings after 1813 were the pure embodiment of Dutch nationhood, Geyl called attention to the dynastic interests pursued by the stadholders, from Frederick Henry to William V, in their relations with England and, to a lesser degree, Prussia. The republican ("States") party, far from being narrowly provincial and unnational, had performed a necessary national task in opposing Orange dynasticism, which fostered dependence upon the stadholders' foreign relatives (see 1924; 1939). The impact of Geyl's works on this theme upon Dutch historical writing was not as dramatic as

that of the Great Netherlands idea, but narrow Orangism was displaced by more balanced understanding of all the parties in the history of the Republic.

In 1936 Geyl was appointed professor of history at the University of Utrecht, despite the difficulties that stemmed from his reputation as a stormy petrel lacking the sedate dignity characteristic of the Dutch academic world and from his connections with the Flemish movement in Belgium. He did not become the founder of a school of disciples, partly because he favored in his students (of whom the best known are J. C. Boogman, A. J. Veenendaal, J. W. Smit, and D. J. Roorda) the same kind of independence of spirit that he himself possessed. He continued his warnings of the Nazi menace right up to the German invasion on May 10, 1940, and did not keep silent even afterwards. On October 7, he and approximately one hundred other eminent Dutchmen were arrested and sent to Buchenwald, where they were held as hostages in retaliation for the internment of Germans in the Netherlands West Indies. Thirteen months later, along with most of his fellow hostages, he was sent back to the Netherlands for continued internment. After several months' hospitalization during 1943 and 1944, he was released and permitted to return home in February 1944. He provided hiding places for resistance fighters in his home, barely escaping detection and arrest. At the same time he worked at his writing, although he had been dismissed from his professorship by the German authorities in 1942. After the liberation of Holland in 1945, he was restored to his chair.

Geyl's writing now largely changed character. His energy for primary research flagged, but he turned his attention to historical criticism, the philosophy of history (although he insisted that he was no philosopher), and comment on public events. At the same time he broadened the area of his writing from his native Netherlands to all of Europe as well as America. He wrote frequently for the weekly newspaper *Vrij Nederland*, using new books for the themes of essays on a great variety of subjects. Always concerned with the influence of contemporary events upon a historian's choice of subject and his interpretations, he began to look into the past for greater understanding of the turbulent era through which he was living. At the same time he drew upon his own experiences for deeper insight into the past. During the last months of the occupation, he had written a book on the

changing picture of Napoleon—an obvious parallel to Hitler—in French historical writing (1946*b*). To show, as he did, that French historians' views of Napoleon had changed over the decades, depending on their political and ideological commitments, was hardly novel; but to draw the conclusion that the understanding of Napoleon had been enriched by these different interpretations was to take up the cudgel against historical relativism, veering over into outright skepticism, which had characterized historical thought in the 1930s. History, he proclaimed, was "an argument without end" and was the better for it. This book was followed a year later by a short study (1947), which rehabilitated the democratic movement in the final decades of the republic of the United Provinces against the contention of H. T. Colenbrander that the Patriots had been no more than puppets in the hands of the French. He also carefully distinguished the leaders of the Batavian Republic, who put the Patriot principles into practice with the support of a French army of occupation, from the NSB'ers, the Dutch Nazis, noting that the Batavian leaders had sought to rebuild their country upon democratic principles and sought its welfare under complex and trying conditions, none of which could be said of the NSB'ers, traitors to their country and its historical traditions.

Geyl first began to receive wide attention in other countries, however, when he attacked the historical views of Arnold J. Toynbee as presented in *A Study of History* (1934–1961). He accused the English historian of twisting facts to fit his grandiose system, defended the legitimacy of nations and nationalism against Toynbee's universalism, and rejected the judgment that Christianity was the only true religion and the only salvation of mankind. Although accused of himself indulging in system making in his Great Netherlands historical writing, he asserted that he had been trying to take account of facts and to make his historical picture correspond to them, rather than twisting them to fit his preconceptions.

Geyl defined himself by argument with other historians, especially of the Netherlands. He had few strictly historical arguments with his greatest contemporary Johan Huizinga, who died in 1945, although his method of work and style of writing were very different from that of the subtle esthete Huizinga. Where Huizinga's view of the contemporary world was deeply pessimistic, Geyl defended against him what he

called in his own final lecture on the occasion of his retirement "the vitality of Western civilization." Although himself as much a man of high culture as Huizinga, he was far readier to accept the coarse vigor of the common man, and he felt that the problems of Western civilization did not arise from democracy in government or society. He saw totalitarian barbarity not as an exaggeration of faults within democracy itself, but as an attack upon the central spirit of democracy. He was even more critical, therefore, of Jan Romein, a highly influential and respected Marxist at the University of Amsterdam, because his historical vision rested not upon evidence and argument but upon his strongly felt subjective convictions. Geyl had no sympathy for Marxism, accusing it of combining utopian dreaming with Machiavellian practice of power politics in the present. He only joined the postwar Labor party (the former Social-Democratic party) after it abandoned its prior Marxist doctrines, feeling that it had become the best defender of liberal democracy. Although he has sometimes been described as a socialist because of this membership, there is no sign that he believed in socialism as a pattern for the future organization of society; indeed, he continued to proclaim himself a liberal, but not a defender of the capitalist status quo.

In the polemical and journalistic essays of the last two decades of his life, he reaffirmed his own vision of history and life. Even as death neared, he did not fall back on either traditional religion or the mysticism with which many intellectuals replaced it; he accepted the rational structure of the universe and the ability of man's rational mind to comprehend it, and he saw rationality as the basis for a healthy emotional life. His vision of the world was this-worldly. Quite unreligious himself, he defended the rights of Catholics and other non-Calvinists to full membership in the Dutch community. His belief in liberal democracy, separated from the tie established in classical liberalism between political democracy and free-enterprise capitalism, was only intensified by the attacks upon it from right and left. He was not as sensitive as many others to the social and economic problems faced by democracy, but he thought all totalitarian alternatives were false solutions. These ideas were never brought together into a full-scale exposition and therefore hardly present a neatly coherent picture. His significance lies not in the originality of his beliefs, which cannot be asserted, but in the extraordinary

vigor with which he defended them at a time when advocates of the middle way seemed to have lost their certitudes and their self-confidence.

His primary impact, however, remained his revisions in Dutch history and his historiographical and critical writings. As these became known in the Western world, he was invited to visiting professorships and lectureships in the United States, England, and other countries, and he became the best-known Dutch scholar in the postwar world. He died at his home in Utrecht on the last day of December 1966.

HERBERT H. ROWEN

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GHURYE, G. S.

Govind Sadashiv Ghurye was born into a Saraswat Brahmin family in Malvan, India, in 1893. He completed his high school and college education in Bombay, where he studied English and

Sanskrit. Ghurye was throughout a brilliant student and he won the coveted Chancellor's gold medal as the best student at the M.A. examination in 1918. After reading Ghurye's unpublished essay on "Bombay as an Urban Centre," Patrick Geddes, who had started the sociology department at the University of Bombay, recommended him for a foreign scholarship, and Ghurye went to London. Dissatisfied with the atmosphere at the London School of Economics, and uninspired by L. T. Hobhouse, Ghurye registered for the Ph.D. program at Cambridge University, where he planned to study with W. H. R. Rivers. Unfortunately, Rivers, for whom Ghurye developed an enormous regard, died in 1922, and he completed his work under Alfred Cort Haddon. He returned home in 1923. Appointed reader and head of the sociology department at the University of Bombay in 1924, he became a full professor in 1934, and at his retirement at the age of 65, he was named the first professor emeritus of the university.

Ghurye's first book, *Caste and Race in India* (1932), in which he skillfully combined historical (Indological), anthropological, and sociological approaches, established his reputation. He was as much concerned with the historical origin and the geographical spread of the caste system as with its contemporary features, including the impact of British rule. He recorded insightfully the persisting, the changing, and the emerging features of this complex system. Though firmly opposed to the caste system and hopeful that it will weaken in the urban environment, he also regretfully noted the growth of caste consciousness and the transformation of the caste into a community. In subsequent editions, Ghurye fully developed such themes as the role of the caste in politics. This book will rank as Ghurye's most lasting contribution to Indian sociology.

Though at the height of his intellectual powers, Ghurye did not publish another major work for ten years. Then, he published *The Aborigines—"So-called"—and Their Future* (1943) to refute Verrier Elwin's thesis that the separate identity of tribal groups should be maintained by enforcing their continued isolation. Ghurye argued in favor of their assimilation into the larger community. Essentially, Ghurye was directing his attention to the critical problem of nation building in a heterogeneous society, a theme to which he has returned repeatedly in his later writings. In *Social Tensions in India* (1968b), he examined, in a historical context,

the deeply divisive role of culture, language, and religion in India. Concerned about the bleak prospect of national solidarity, he expressed his dissatisfaction with the gradual amoral, manipulative drift in Indian politics in *Whither India?* (1974). In *India Recreates Democracy* (1978), however, he celebrated the restoration of democracy to India in 1977. These works show Ghurye's abiding concern with the problems of his society.

During these years, more general themes and a cosmopolitan approach to sociology were exhibited in Ghurye's broader works on culture. In *Culture and Society* (1947), he dealt with a universal theme—what makes a culture great?—and assigned universities a major role in preserving, enhancing, and disseminating culture in society. In a subsequent work, *Occidental Civilization* (1948), he focused on the reasons for peaks and clusters of creative energy. Ghurye's lifelong intellectual fascination with cities found detailed expression in *Cities and Civilization* (1962a). These works show his intimate knowledge of Western literature.

In his remaining works, Indian themes have predominated. Ghurye tried, in *Indian Costume* (1951), through a historical survey, to arrive at a position on the question of national dress; a hopeless undertaking for a country as vast and varied as India. Again, the effort was aimed at finding homogeneity in extreme heterogeneity. His deep interest in urbanization was expressed in many writings. For example, he discussed the impact of the urbanization process on a given rural hinterland (1963a). Taking an 1819 monograph on an Indian village as his base, Ghurye restudied the area and analyzed its development (1960a). He also examined literary sources to produce evidence of social change in his home state (1952b; 1954a).

The sociology of religion has been another of his major intellectual concerns. His *Indian Sadhus* (1953b) traced the role of ascetic organization. He wrote two studies of Indian gods (1962b; 1977), and prepared an analysis of the role of religion in the historical civilizations of Mesopotamia, Egypt, and India (1965a). He wrote an authoritative study of kinship (1955). His papers are many and varied, covering such subjects as dual organization in India, cross-cousin marriage, age at the time of marriage, marriage and widowhood, Egyptian affinities of Indian funerary practices, disposal of the human placenta, salary and working conditions of clerks, friendship, megalith remains in India,

population growth, sex habits of the Bombay middle class, birth control in Bombay, untouchability, explorations in pre- and protohistoric Sindh culture, and so on. A minor work deserving consideration is *Vidyas* (1957*b*), in which Ghurye paid homage to Comte on the centenary of his death. That an Indian sociologist was mindful of this event is itself remarkable; that Ghurye recognized Comte as a sociologist of knowledge is more so. Ghurye, doubtless imitating Comte, suggested a new name for sociology—humanics, a term borrowed from Roger Williams, on his own admission.

In addition to his research, Ghurye has found time to be a successful teacher. Approximately 80 dissertations (25 for the M.A.; 55 for the Ph.D.) have been completed under his guidance (1973) and his former students still teach in various Indian universities. Although he taught at a time when doctoral fellowships and funding organizations were scarce and professional employment prospects dim (Srinivas 1973), Ghurye attracted many students and inspired them, by his scholarly stature, to pursue the painstaking process of sociological research. His students often tackled new topics, including village studies, urban studies, kinship studies, community studies, regional studies, and nationalism. He allowed each student to pursue his or her own interest and showed great tolerance. One could do almost any kind of research, subjectwise and methodwise, provided standards set by Ghurye were met. Ghurye was an exacting adviser but did not impose his views. Few have equalled him and none has surpassed him in India in this respect.

In 1952, Ghurye founded the Indian Sociological Society and launched its journal, *Sociological Bulletin*. He served as the society's first president until 1966. Though there were other pioneers, no single person has done as much for sociology in India as Ghurye.

As a student of Rivers, Ghurye was influenced by diffusionist ideas that are reflected in many of his early works. K. M. Kapadia (1954) and M. N. Srinivas (1973), both Ph.D. students of Ghurye, refer to it. In later life, he either outgrew or rejected these ideas. Did he use the ten "fallow" years to secure his intellectual independence? If so, Ghurye has never surrendered it again and has been his own mentor. Deeply rooted in Indian culture, he has not allowed anything to deflect him. Ghurye has chosen to be a recluse in order to preserve his freedom to do what he thinks best. The one abiding les-

son he learned at Cambridge, and sought to transmit to his students, was respect for facts. When he began his career, very little work had been done in sociology in India, and it was salutary to insist on facts. Ghurye's commitment to facts has both a positive and a negative side. Positively, it has given him and his students a firm empirical base, curbing the speculative tendencies that are especially tempting in the absence of concrete information. Srinivas (1973) rightly compliments Ghurye for conducting a one-man ethnographic survey of India from his chair in Bombay. Negatively, Ghurye's prolonged neglect of all theoretical positions, however tentatively, has deprived his writings of a powerful thrust. It is puzzling that a mind so vital and aware should have remained indifferent to the fierce controversies in the West. Was this indifference (it was not ignorance) a mechanism to protect himself from an interminable and seemingly profitless exercise? If so, his accomplishments may attest to the value of his strategy.

DHIRENDRA NARAIN

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GLASS, D. V.

David V. Glass (1911-1978) was a pioneer in the study of population, population policy, demography, social mobility, demographic history, and historical demography. For fifty years he was associated with the London School of Economics (L.S.E.) as a student and an influential teacher.

David Victor Glass, a tailor's son, was born in London's East End in 1911 and was thus by birthplace a true cockney, though few would

recognize this later. He attended a state elementary school and Raine's Foundation School before enrolling at the L.S.E. in 1928.

The L.S.E. was founded as, and has remained, a place of heightened political awareness; in the 1930s, politics was an obsession. The Houghton Street that Glass walked daily was a marketplace of socialist ideas and ideals. Among those who contributed to the intellectual climate of the time were Beatrice and Sidney Webb, the founders of L.S.E. and early advocates of pragmatic socialism; the sociologist and philosopher, L. T. Hobhouse; the social and economic historian, Richard H. Tawney; the social anthropologist, Bronislaw Malinowski; the sociologist, Karl Mannheim; and the legendary political activist, Harold Laski. Although politics did not figure in Glass's later professional work, he did continue to hold strong convictions on social justice and inequality and on independence and self-sufficiency for the Third World. Throughout his life he remained a socialist, refusing a knighthood on this principle.

While a student, Glass was active in leftist student politics and, after graduation, he nearly became a regional organizer for the Workers' Educational Association. William Beveridge wrote in 1932: "As a student Glass was regarded as one of the most promising men of his year"; and if he only received an upper second it was "because he occupied himself too much with the activities of the Student Union" (*The [London] Times*, Sept. 27, 1978, p. 16).

In 1931 Glass graduated with a B.Sc. in economics and then worked as a research assistant to Beveridge, who years later would be the architect of Britain's national insurance system—the basis for Britain's welfare state providing coverage "from the womb to the tomb." During this time Glass also worked with the statistician Arthur Bowley.

In 1935 Glass wrote *The Town and a Changing Civilization*, an analysis of the historical development of towns as social institutions from Sumer to modern times. The work traced the origins of urban problems in an attempt to establish a precursor for a system of town planning. The book concluded that there was little value to "using economics to achieve more efficient production if the bulk of the production is to go to a minority among those individuals. In the face of such problems and such aims, there is only one way of obtaining the society we need—by building a Socialist State" (p. 141). The work was published in a series edited by

Krishna Menon, later India's Minister of Defense and Glass's lifelong friend.

In the same year, Glass joined the department of social biology to work with Lancelot Hogben. Hogben's work, and especially his edited book *Political Arithmetic* (1938) to which Glass contributed, combined empirical social research with a wide-ranging social investigation. "My own approach to social research," wrote Glass, "was very considerably affected by discussions with Professor Hogben" (1973*b*, p. 175). Glass retained this broad but empirically based intellectual style throughout his life, at a time when many others in his field adopted a more narrow, specialized approach. His work showed superb skill in the analysis of new facts within a comprehensive framework while avoiding trends toward either blind empiricism or grand theorism. Years later, in his landmark study on *Social Mobility in Britain* (1954), he acknowledged that the "approach to the study of social selection and differentiation has clearly been influenced by the investigations which [Hogben] promoted before World War II" (p. vi).

Glass's development in demography and statistics was also influenced by Robert R. Kuczynski, then also working in the department, who had earlier helped to develop and apply indices of population replacement (net and gross reproduction rates). These indices were important in that they showed the potential implications of a sustained fertility decline upon the size and age structure of the population. Because Kuczynski's indices were widely publicized and adopted by the League of Nations' *Statistical Yearbook*, considerable attention was drawn to the falling birth rates. The technical appendices of some of Glass's later work made extensive use of Kuczynski's methods.

In 1936 Glass was appointed research secretary of the newly formed Population Investigation Committee, which included Alexander M. Carr-Saunders as chairman, as well as Carlos P. Blacker, Juliet E. Rhys-Williams, and Grace Leybourne. As more attention was focused on Europe's falling birth rates, the committee asked Glass to study the family allowance system in France and Belgium as well as other pro-natalist policies in fascist Italy and Nazi Germany. This work was summarized in his second book, *The Struggle for Population*. It was published in 1936 along with Carr-Saunders' *World Population: Past Growth and Present Trends*, and Kuczynski's *Population Movements*. The three books, reviewed together in *The* (London)

Times, drew considerable attention in the press and in Parliament to the declining birth rates. The research findings and the resulting public discussion contributed directly to the passage of the Population (Statistics) Act of 1938, which greatly expanded the scope of vital registration in Britain by including background questions on age and other characteristics of the mother. This information enabled the precise calculation of reproduction rates and allowed an analysis of current changes of fertility.

Concern over the possibility of a declining population was heightened by the spreading war in Europe and by Nazi Germany's purported success in raising marriage and birth rates. Glass undertook a major study of the formulation and implementation of government policies affecting marriage and fertility in England and Wales, France, Belgium, Italy, Germany, and Scandinavia. The family allowance systems were studied in great detail because they were increasingly being used as a pro-natalist population policy. "The record of governmental attempts to stimulate fertility shows one significant and constant fact," wrote Glass, that "however urgently governments may have declared their desire to increase the supply of births, they have nevertheless persistently tried to buy babies at bargain prices" (1940, p. 371). The study, which was also his PH.D. thesis, was published in 1940 in a single volume, *Population Policies and Movements in Europe*, and was regarded as the definitive work on population policy. Nearly thirty years later, when there was concern that population was growing too rapidly, the book was republished; it remains a major classic in the study of population.

Glass went to the United States in 1940 as a Rockefeller fellow; but because of the war he was recruited as deputy director of the British Petroleum Mission in Washington. At the end of the war, Glass was appointed director of the Family Sample Census of the Royal Commission on Population, established at a time when there was still concern about low fertility. Glass directed the largest population survey conducted in Britain. It included a ten per cent sample of ever-married women who were asked complete fertility and marriage history questions (Glass & Grebenik 1954). This was one of the first large-scale studies of fertility which employed the methods of cohort analysis.

The Royal Commission found that family size had remained relatively stable at around 2.2 children per married couple and, though this

was close to the level of replacement, population was likely to increase for some time. It recommended a number of governmental social policies to reduce the cost burden on large families while also suggesting that the National Health Service distribute contraceptives. Most of the recommendations were ignored by later governments, but the reports did influence the public discussions and subsequent investigations of population changes. By the mid-1950s, fertility dramatically increased in Britain and population projections had to be revised upward.

In 1945 Glass replaced R. R. Kuczynski as a reader in demography at the L.S.E., and by 1948 he became professor of sociology, a position he retained for the rest of his life. He also served in the late 1940s as the United Kingdom representative to the United Nations Population Commission and to the United Nations Educational, Scientific and Cultural Organization. In 1947 Glass founded and coedited with E. Grebenik what became the leading journal in the population field, *Population Studies*. These two personally reviewed all the submitted manuscripts until Glass's death in 1978.

During these years, Glass worked increasingly in sociology and history because, as he said in the *Introduction to Malthus* (1953), "population is not itself an independent variable." In the 1950s a number of Malthusian arguments were proposed by different writers. In response, Glass wrote and edited some of the most incisive work ever done on Malthus, relating that theorist to his times and analyzing the relevance of Malthusian arguments to the contemporary world.

In the 1950s and 1960s many historians became increasingly interested in the role of population growth in economic and social change, and at the same time many demographers became interested in history. In an attempt to integrate work in the two disciplines, and employing their combined approaches, Glass contributed to and edited two landmark books on demographic history. The first, *Population in History* (1965), was coedited with D. E. C. Eversley, and the second, coedited with Roger Revelle, was *Population and Social Change* (1972). These books contributed greatly to our knowledge about the demographic transition in preindustrial and industrializing Europe, and perhaps shed some light on the relevance of these experiences to other countries.

Glass was interested not only in demographic history but also in the history of demography, particularly in Britain. He wrote papers on

Gregory King's estimate of the population of England and Wales in 1695 and on John Graunt's development of the life table. Published with two companion volumes of historical reprints, *The Population Controversy* (1973c) and *The Development of Population Statistics* (1973a), *Numbering the People* (1973b) was one of his major works in this area. It analyzed in great detail the eighteenth-century population controversies and the development of the census and vital statistics in Britain.

Perhaps his most important work, inspired partly by Hogben, Marshall, and Ginsberg, was his study on *Social Mobility in Britain* (1954), conducted with J. R. Hall and others. This was a pioneering investigation of intergenerational mobility; it was the first national study undertaken anywhere to examine systematically social mobility over the entire community, and its methodology remains a model on which studies are still based. After first establishing a scale of occupations by social status on the basis of empirical research, the investigators then obtained life histories of a random sample of ten thousand adults representing Great Britain. An Index of Association was employed to measure the extent to which mobility between two occupational groups went beyond that of "chance." A value greater than unity indicated that more subjects were found in their fathers' status category than would be expected assuming statistical independence. A wide range of analysis was then undertaken by a number of colleagues, while Glass himself examined the relations between social origins and education; changes in social status between fathers and sons; and the way in which education affects the social mobility of individuals of different social origins. Much greater social mobility was found in Britain than many, including Glass, had expected, in what was thought to be a more rigid class structure. Glass took a leading role within the International Sociological Association to promote similar studies in other countries, all designed for international comparability, and he strongly influenced Gösta Carlsson's study of Sweden and Kaare Svalastoga's study of Denmark. A remarkable similarity was eventually found in the general pattern of social mobility of Western industrialized countries.

Glass participated in many other special study groups and helped found the British Society of Population Studies. He was elected president of the International Union for the Scientific Study of Population (1963–1965); fellow of the British

Academy (1964); fellow of the Royal Society (1971); foreign honorary member of the American Academy of Arts and Sciences (1971); and foreign associate of the National Academy of Sciences (1973). He also received honorary doctoral degrees from the universities of Michigan and Edinburgh, and from Queen's University Belfast.

Despite his public activities, Glass was, in many ways, a lone scholar, trying not to be distracted by the popularity of his field. Yet in 1965 he created one of the world's leading population training centers which combined an orientation to the Third World, a study of population within a broad social context, and the most sophisticated of demographic techniques. He demanded the highest standards of scholarship from himself and his students. His superbly substantive and witty lectures were always crowded, with every continent well represented. Glass encouraged his students to return to their own countries, where many have moved into key governmental and university positions; thus his influence as a teacher and a scholar have extended far beyond the shores of the British Isles.

B. MAXWELL STAMPER

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GLUCKMAN, MAX

Max Gluckman, a major figure in social anthropology, was born in Johannesburg, South Africa, on January 26, 1911. He died in Jerusalem, Israel, on April 13, 1975, while serving as the Lady Davis distinguished visiting scholar at the Hebrew University.

Career. Gluckman's South African youth was decisive in determining his view of anthropology, although most of his research and his entire teaching career postdated his departure from South Africa in 1938. He was to visit that country only for brief periods afterwards, and he became increasingly critical of its political development. He opted for British rather than South African citizenship and spoke out publicly against the deepening shadow of apartheid. It was the South African experience that molded him into a firm champion of black Africans throughout the continent, a championship that led to his exclusion for many years from field

work in south or central Africa. He returned to the latter area only after northern Rhodesia became independent Zambia in 1964.

Gluckman also felt a deep emotional commitment to Israel. This devotion, too, went back to his childhood. Although his parents did not practice the Jewish religion, they were prominent members of the South African Jewish community and strong Zionists. They and most of their children ultimately emigrated to Israel. Gluckman never carried out intensive field research in Israel as he did in south and central Africa, but in his later years the majority of his students were sent to work in Israel, as earlier the majority of them had gone to work in central Africa. Much of this later work was carried out as part of the Bernstein Israel Research Project, which was initiated through the University of Manchester in 1963 under Gluckman's direction. That project has been called "the first concerted effort by a group of social anthropologists to explore an industrialized state since Warner's Yankee City studies" (Marx 1975, p. 131).

Although Gluckman spent much of his working life in England and encouraged his students to do research on its factories, shops, and housing estates, he himself did little research in that country except on the subject of sport. A fine athlete from boyhood, Gluckman turned naturally to the examination of the role of football and other sports within the context of contemporary life.

Gluckman was educated at Witwatersrand University (B.A., 1934) and at Oxford University, where he went in 1934 as a Transvaal Rhodes scholar. He received his D.PHIL. in anthropology at Oxford in 1938. From 1936 to 1938 he carried out field work in Zululand, and then, in 1939, he went to northern Rhodesia (now Zambia) as a research officer of the Rhodes-Livingstone Institute. He immediately began field work among the Lozi of Barotseland, that work being interrupted when he became director of the institute in 1941. Thereafter he was able to return to Barotseland twice for short periods during which he concentrated on the working of Lozi courts. He also spent a few weeks in research among the Tonga and Lamba peoples before he gave up the directorship in 1947. In that year he became university lecturer in social anthropology at Oxford, a post he left in 1949 when appointed the first professor of social anthropology at the University of Manchester.

Leaving the institute did not end Gluckman's

close involvement with its development. For many years he continued to train the majority of those appointed as research officers, and he gave them an academic home at Manchester when they returned from central Africa. The first reports of their research were usually given in his famous Manchester seminar, which was devoted to the analysis of field data.

At Manchester, Gluckman founded a school of social anthropology stamped with his own standards of excellence and his characteristic research interests. These emphasized the necessity of relating what happened in particular situations to the context of the total system within which the situations developed. They focused upon action guided by a concern for reputation and social values rather than upon such subjects as cognitive systems. Situational analysis, the extended case method, and the social drama were all terms used to express the essence of his method with its focus upon human interaction. F. G. Bailey, Victor Turner, Ronald Frankenberg, Arnold L. Epstein, Scarlet Epstein, William Watson, and Jaap Van Velsen were among the first of the stream of distinguished anthropologists whom he trained.

When Gluckman became a special fellow of the Nuffield Foundation in 1971, he handed over the chairmanship of the Manchester department, but continued to teach. Part of his strength as a teacher depended on his deep interest in his students and in their work. He gave time and intellectual energy to helping them develop the theoretical implications of their data, and delighted in their discoveries, which he saw as an extension of his own work. Although primarily associated with the University of Manchester in his later years, Gluckman also traveled widely and frequently served as visiting professor at universities throughout the world. He returned most consistently to the Yale University Law School, where he was honored for his work in comparative law and found congenial companions who shared with him his interest in jurisprudence. His colleagues recognized him by awarding him most of the honors that can come to an anthropologist.

Intellectual development. Although Gluckman's youthful experiences influenced his later career profoundly, his theoretical development was determined by experiences at Witwatersrand and Oxford. When Gluckman entered Witwatersrand in 1928, he expected to study law and become a lawyer like his father. A lecture course in anthropology taught by Winifred

Hoernle, however, turned him away to social anthropology. Hoernle's theoretical approach was strongly influenced by Émile Durkheim and A. R. Radcliffe-Brown, but she was also one of the founders of the South African Institute for Race Relations, created to provide factual information on the multiethnic population of South Africa. She and Isaac Schapera, who also taught at Witwatersrand for a year, saw anthropology as involved with living people. They believed that anthropologists in South Africa had an obligation to study the impact of the social conditions under which Africans were being forced to live in the reserves, on farms, and in towns and cities. Reconstructing cultural practices of the African past was a legitimate anthropological enterprise, but it was of less crucial concern than documenting and analyzing the contemporary multiethnic scene. Hoernle and Schapera, and the young men and women drawn by them into anthropology, translated the functional doctrines then dominating social anthropology under the influence of Bronislaw Malinowski and Radcliffe-Brown into a statement about the interrelationships between such phenomena as the high standard of living of South African whites, pass laws, low wages for Africans, malnutrition in the reserves, dilemmas of chieftainship, eroding agriculture in the reserves, and so on. Taking a stand by no means acceptable to most white South Africans, who thought in terms of separate cultural universes, they described Africans and Europeans as belonging to a single social system.

Gluckman was strongly drawn to this view of anthropology. Beginning with his first professional writing, he examined interethnic situations within which blacks and whites encountered each other (1940), and he encouraged colleagues and later students to study contemporary industrial society, which he saw as including the rural areas of Africa. When in 1943 he drew up a seven-year research plan for the Rhodes-Livingstone Institute, he took industrialization and labor migration as the focus for the work of all research officers to be appointed by the institute to work in northern Rhodesia (Zambia), southern Rhodesia (Zimbabwe), and Nyasaland (Malawi). He saw these two forces as both disruptive of old patterns and providing the basis for regrouping, and he wrote: "as part of this study we shall analyse the formation of new groups and relationships, in both urban and rural areas . . ." (1945, p. 9). For him, anthropology thrust forward from the

present into the future. In much the same terms some twenty years later, in 1963, he planned the Bernstein Israel Research Project, but this time the focus was to be upon industrialization and immigration.

At Oxford Gluckman studied under Robert R. Marett, E. E. Evans-Pritchard, and later Radcliffe-Brown. In the 1930s the latter two were closely associated intellectually and it was to them that Gluckman gave his allegiance. Their ideas about social structure, functional relationships, social cohesion, and political order were congenial to him, since they referred back to Durkheimian formulations already familiar to him from Hoernle's teaching. Gluckman agreed with Radcliffe-Brown and Durkheim that society is a moral order that manages to maintain itself despite conflict among its members who follow their competing interests and sometimes rebel against whatever symbolizes social constraints. However, he departed from Radcliffe-Brown increasingly as he came to emphasize the pervasiveness and bitterness of the conflicts with which society has to contend. He looked to law and ritual as the great upholders of the social order, seeing in them the mediating mechanisms that permit harmony to be restored after breaches of the peace have occurred or when people have shown themselves to be fundamentally at odds. He was to make major contributions to the study of both law and ritual.

Gluckman published two influential books based upon his field study of Lozi courts, *The Judicial Process Among the Barotse of Northern Rhodesia* (1955b) and *The Ideas in Barotse Jurisprudence* (1965a). Critics in his own discipline were later to accuse Gluckman of having imposed English and Roman Dutch legal categories upon the Lozi judges. He defended himself against his accusers, maintaining that he had the obligation to explicate Lozi ideas in terms comprehensible to the legal scholar as well as to the anthropologist, but that he was translating concepts familiar enough to Lozi litigants and judges. It was the Lozi who used the standard of "the reasonable man" in weighing the evidence; he had only the pleasure of recognizing a familiar legal concept in an unfamiliar setting. In these books, and in various articles dealing with Lozi courts and legal procedures, he dealt with technicalities of law in a fashion that suggests his early legal training. But his over-all concern with the courts was with their role as moral agents.

Gluckman's doctoral dissertation for Oxford

was entitled "The Realm of the Supernatural among the Southeastern Bantu." It was based on library research and at first glance seems an uncongenial subject for a man who as a professed agnostic admitted that he found religious belief difficult to understand. Gluckman never published the dissertation, but he mined from it a number of articles on ritual that relate to his primary interests. They examine what for him were to be perennial questions, how ritual reflects the social order and in symbolizing it reaffirms its rightness. He was to write of ritual again and again in these terms, describing the ceremony of the opening of a bridge in Zululand as symbolizing both the divergent and the common interests of those who attended (1940); of the war between the sexes during Wiko circumcision ceremonies as leading to a reaffirmation of social continuities (1949); and of rituals of rebellion in Swaziland and elsewhere as maintaining the state (1954). His final statement on the subject was made in the last article he published: "In ritual . . . the ultimate emphasis is that harmony among people can be achieved despite the conflicts, and that social institutions and values are in fact harmonious—ultimate statements that are belied to some extent by the ritualization itself. Ritual can do this since each ritual selects to some extent from the gamut of moods, of cooperative links, and of conflicts" (Gluckman & Gluckman 1977, p. 236).

In the late 1930s, Gluckman was closely associated with E. E. Evans-Pritchard and Meyer Fortes. At that time they were laying the groundwork for the study of political anthropology in the discussions that gave rise to the volume *African Political Systems* (Fortes & Evans-Pritchard 1940), which was to initiate a spate of studies of segmentation and balanced opposition. Gluckman was strongly influenced by this volume, to which he was a contributor, and also by Evans-Pritchard's *The Nuer* which appeared in the same year. He thought of himself as developing the implications of these volumes in such books as *Custom and Conflict in Africa* (1955a) and *Politics, Law and Ritual in Tribal Society* (1965b). He may have been unaware of how far he had moved in other directions in his own analysis of conflict and order. Where Evans-Pritchard and Fortes emphasized the existence of stable cognitive structures and balanced opposition of social units, Gluckman looked to the individual and recognized that the rules by which people are expected to live are

themselves often contradictory or ambiguous: people therefore are in conflict with themselves as well as with their fellows and society. The different roles they fill make conflicting and equally valid demands upon them and right behavior in one role leads to bad behavior in another. In his thought the moral dilemmas were likely to be more complex in less complex societies, for in such societies each individual must simultaneously enact a variety of roles and face the various expectations of the other members of society. He described such societies as multiplex and believed that it was within them that ritual functioned most effectively, both to mark the roles and to persuade people that despite all conflicts, they shared overriding values of fertility and harmony.

In more complex societies, he looked to legal rather than to ritual solutions, for he saw them as mobilized in terms of class or sectional interests that did not bow to some greater value placed upon the larger society. The playing out of conflict in ritual was no longer effective and could not be permitted, because there were no ritual symbols that could override the conflict. At one time Gluckman had read much of Karl Marx, and he saw the sweep of history in Marxian terms. The influence of Marx upon him later waned, and he never showed much interest in the dialectical method, but his view of the role of ritual in contemporary industrial society derives from his earlier thinking.

Other interests were spin-offs from his central concerns. He lectured and wrote on the functions of gossip (1963a), where again he faced the criticism of younger anthropologists who looked at gossip as a system of communication. Gluckman's own interest was more restricted, for he was concerned with the way in which social standards were enunciated through gossip and the way in which gossip served as a means of social control. His controversial article on the sources of marital stability (1950) led to a great deal of research on divorce rates and the various social factors that might correlate with them. It was more peripheral to his central interests, but derived from his concern to discover the conditions under which frequent marital break-ups can occur without endangering the stability of the encompassing social system.

The thread that runs through Gluckman's work is the study of conflict contained within some higher order, though threatened by the unwillingness of men and women to accept compromises that injure their immediate de-

sires. Because of this emphasis on conflict and his focus on situations, Gluckman has been seen as following in the footsteps of Georg Simmel, but he developed his ideas independently before reading Simmel. Although he was adamant that as a social anthropologist he did not deal in psychological explanations, his debt to Sigmund Freud appears to have been significant in that he placed conflict within as well as between individuals. He himself, after the early interest in Marxism, held that he belonged in the camp of Durkheim and those who believe that societies are moral systems, rather than simply collectivities of competing, calculating individuals.

ELIZABETH COLSON

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GLUECK, SHELDON
AND ELEANOR T.

Sheldon Glueck was born in Warsaw, Poland, on August 15, 1896. He came to the United States in 1903 and became a naturalized citizen in 1920. He attended Georgetown Law School, George Washington University, the National Law School, and Harvard University, and earned the degrees of A.B., A.M., LL.B., LL.M., S.S.D., PH.D., and sc.D. In 1948 the University of Thessalonika (Greece) made him an honorary doctor of laws. During his long and distinguished career Glueck taught at Harvard University. In 1950 he was appointed Roscoe Pound professor of law, an honor that he prized greatly because of his close association with Pound, whose influence is seen in much of Glueck's writings. In 1963 Glueck retired to become professor emeritus.

Glueck's interests ranged from the development of modern research in delinquency prediction to the practical world of delinquency prevention. He was invited to advise governments on a wide variety of matters relating to the prevention and eradication of these important social problems. So respected was his scholarship and judgment that on the occasion of the Nuremberg trials he was appointed adviser to Justice Robert H. Jackson on law governing war crime criminals. In recognition of his many outstanding contributions to criminal justice he received numerous honors including the Isaac Ray award of the American Psychiatric Association in 1961; the August Volmer award of the American Society of Criminology in 1961 (with Eleanor Glueck); the gold medal of the Institute of Anthropology, University of Rome in 1964; and the Beccaria gold medal of the German Society of Criminology in 1964. The Sellin-Glueck award of the American Society of Criminology was established in 1972 to honor, *inter alia*, Glueck's contributions to criminal justice.

Eleanor T. Glueck (1898-1972) was born on April 12, 1898, in New York. She studied at Barnard College, the New York School of Social Work, and Harvard University, where in 1925 she received a doctor of education degree. She met Sheldon Glueck when both were graduate students at Harvard and they were married on April 16, 1922. From 1925 to 1928 they both taught at the Harvard department of social ethics and then moved to Harvard Law School. There they staked a claim and established a sound base from which to conduct a lifelong study of crime and delinquency. Eleanor Glueck was research associate in criminology for 36 years until her retirement in 1964. Her work did not stop here and she worked along with her husband until her untimely death in 1972.

The Gluecks of Harvard, as they grew to be known by their friends, operated on the growing edge of knowledge. They were pioneers and they examined the social indicators hoping to bring sense and solution to the problems of criminology. Each brought a special type of knowledge and unique skills to the joint effort. They studied "the fog-enshrouded borderland of psychiatry and law" and it was inevitable that through them the social sciences should help to redirect the law and in its turn the law should issue its caution when intrusions into the sensitive and private affairs of men and women seemed necessary. The Gluecks looked to all of

the disciplines in their search for answers in the highly controversial areas of etiology, prediction, prevention, punishment, correction, and the individualization of justice. Their studies reflect a pragmatic determination to relate theory to practice. Some critics have claimed that on occasion they have rushed too early to conclusions, particularly with respect to causation and in their creation of a prognostic device to foretell probable delinquency and crime.

In an attempt to trace the roots of delinquency and crime the Gluecks examined the home and school in a number of studies and wrote widely about their findings. The titles of some of their publications indicate the direction taken in this early research: "Working Mothers and Delinquency" (Glueck & Glueck 1957), "Role of the Family in the Etiology of Delinquency" (E. Glueck 1960), and "Family Environment and Delinquency in the Perspective of Etiologic Research" (Glueck & Glueck 1963). However, as they studied the puzzle of causation their data led them to the conclusion that the specific influences in home and school could only be understood in the context of broader social factors.

The Glueck studies coincided with the focus upon the uniqueness of the individual during the first part of this century. In the United States William Healy had written *The Individual Delinquent* (1915), while in England Cyril Burt had brought forth his definitive psychological analysis of delinquency in *The Young Delinquent* (1925). The juvenile court was attempting to apply its *parens patriae* philosophy to troubled children one at a time. The Gluecks were eager to know how well this approach was faring. In Boston the court was regularly referring cases to the prestigious Judge Baker Guidance Center for diagnosis and advice. The Gluecks made a careful examination of a large number of delinquent boys who had been treated in this way. The results were published in *One Thousand Juvenile Delinquents* (1934b). They showed that 88 per cent committed further delinquencies during a 5 year follow up period. Notwithstanding the debate that ensued the Gluecks took a realistic position and argued in favor of the court pointing out that it was a clearing house for many of the unresolved problems of society. In their own inimitable way they described the situation in these words:

The juvenile court stands at the converging point of many of the unwholesome trends in society. The

delinquent is a microcosm in whom is reflected a tangle of vicious circles of poverty, alcoholism, inadequate education, mental deficiency, and other unhealthy conditions. However efficient a juvenile court and clinic may be in their day to day activities, they are fighting . . . conditions which may and do very easily counteract their efforts. . . . A judge cannot complaisantly regard his court as an ivory tower of aloofness, nor his particular job as an end in itself. Whether he recognizes it or not, he and his work are but elements in the intricate texture of socio-economic, biologic, and psychologic conditions. Many of these conditions he can of course not affect in any fundamental sense; a few are capable of some control, and hence it is the judge's duty to galvanize the community into action looking to their amelioration. ([1934b] 1939, pp. 273-274)

The Gluecks saw the experimentation of the juvenile justice system as providing answers that might ultimately be emulated in the adult courts. The individualization of justice was the goal. They felt that professional disciplines such as psychiatry, psychology, social work, and education should become integrated components of the criminal justice system. The Gluecks envisioned a board of qualified practitioners supplementing the judiciary in writing prescriptions for crime and delinquency. The principle of flexibility would prevail and the rights of the individual offender would be safeguarded.

The Gluecks were early pioneers in the field of prediction. They saw teachers equipped with prognostic information as being in a unique position to recognize the early signs of delinquency and thus able to establish preventive action while time was still on the side of the young person. The Gluecks also saw prediction tables as valuable to judges. Dispositions could be more wisely made based upon open knowledge of prevailing negative factors. In 1950 the Glueck social prediction tables were presented in *Unraveling Juvenile Delinquency*. Five predictive factors of social background all focused within the home were presented. These were as follows: discipline of the boy by the father; supervision of the boy by the mother; affection of the father for the boy; affection of the boy by the mother; and the cohesiveness of the boy's family. A number of scholars criticized and challenged the tables. The Gluecks proceeded to replicate their research and verify their results in such places as Australia, India, Europe, and Japan. It cannot be denied that the Gluecks with steadfast purpose, sincere scholarship, and sense of social purpose pursued a course of ac-

tion that put them in the forefront of criminological pioneers, who recognizing a need to extend our intellectual foresight did not capitulate under the pressure of intellectual contest.

It was appropriate that Harvard University created the Sheldon and Eleanor Glueck classroom in the Law School's Pound building. In it are housed many of the Gluecks' books, papers, photographs, and memorabilia. The school's archives contain the full creative product of these two prolific careers. There are approximately three hundred books, articles, and miscellanea available for study and research across the full range of the Gluecks' interests. They cover the continuum that leads from the child in distress to the adult confined.

In addition to his research, Sheldon Glueck wrote several plays. While at George Washington University he was director of the University Players Dramatic Club and was offered a lead role with a professional company that was about to produce *Disraeli*. The Harvard Law School Bulletin published a condensation of his musical play *Mister Littlefellow* in 1972.

The Gluecks of Harvard, sometimes controversial, often provocative, always ahead of the times, will be remembered as leading criminologists, great humanitarians, and inspired teachers.

V. LORNE STEWART

WORKS BY THE GLUECKS

WORKS BY SHELDON GLUECK

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GOODMAN, NELSON

Henry Nelson Goodman is an American philosopher whose work has fundamental importance for social and other human sciences and for the philosophy of social science. The reference to other human sciences is intended to mark the fact that his work has been important for the psychology of cognition and the psychology of perception, as well as for social and cultural anthropology and sociology. The foundational importance of his work in these areas stems from the fact that his works have constituted classic contributions to basic issues in the philosophy of science, the philosophy of language and the theory of symbols, and the theory of conceptual systems and theory construction. They have thus borne on the central problems of the theory of knowledge. His contributions are classic in the precise sense that no subsequent work claiming significance can be reasonably expected to be adequate unless it takes account of Goodman's achievements.

More specifically, Goodman's contributions are to areas that include aspects of inductive logic involving the justification of hypotheses and theory acceptance. Those contributions pertain also to the analysis of causal and, more broadly, nomological regularity, dispositionality, possibility, resemblance, and similarity, as well as to the theory of psychological (especially sensory or phenomenal) orderings. Of similar importance have been his contributions to the theory of definition and explication, to constructional methods for systematic theorizing in science and philosophy, and to the theory of conceptual systematization in its most general aspects. One contribution, especially noteworthy because of its prospects for relatively direct applicability to the social sciences (e.g., see Rudner 1966, pp. 40-53), is his provision of a rigorous theory of the measurement of the formal, structural simplicity of conceptual systems (a theory which may be applicable to the assessment of the relative simplicity of rival conceptual systems). His theory of reference in various modes of symbolization (and particularly in the arts), developed in his later writings, has particular significance for problems of relativism and objectivity in the social sciences, including problems of the scientific penetrability of alien symbol systems.

An aspect of Goodman's work, which has kept him very much a philosopher's philosopher and relatively unstudied outside of a small (but, in his later career as may now be seen, widening) circle of philosophers, scientists, and critics, has been his remarkable combination of rigor, precision, and clarity in presenting insights and arguments—typically, counterexamples—that, while complex and subtle, are profoundly radical. The effect of such results, once their radical import is understood, is to force other philosophers, scholars, or scientists concerned with the subject matter to reevaluate basic conceptions of the field; and it has sometimes led to a major restructuring of the field or its problems. The publication and subsequent discussions of his *Fact, Fiction, and Forecast* (1954) and *Languages of Art* (1968a) provide two examples for the fields of inductive theory and the theory of symbol systems respectively of just such effects.

Goodman was born in Massachusetts in 1906, received a B.S. from Harvard University in 1928, and a Ph.D. in philosophy of science in 1941. His doctoral dissertation, "A Study of Qualities," became the substantial basis for his first pub-

lished book *The Structure of Appearance* (1951). An indication of the importance of this work is the fact that several years before it was published, the dissertation was cited in a survey essay by Herbert Feigl (1943). During the more than ten years that "A Study of Qualities" was in preparation, Goodman worked as an art dealer and operated an art gallery in Boston. His strong professional ties with the art world have been a pervasive part of his life as well as of his philosophic thought. These ties have not been confined to the world of painting. During the early 1970s, for example, he conceived, staged, and directed multimedia productions at the Dance Center at Harvard.

Among the philosophers who have been primary influences—and whom, it must be added, Goodman reciprocally influenced—have been Henry S. Leonard, W. V. Quine, Rudolf Carnap, and Carl G. Hempel. (C. I. Lewis, one of Goodman's teachers who influenced his concern with inductive theory, was perhaps more impervious to such reciprocal influence.) Although Goodman has had important substantive differences with all of these philosophers (including Quine, to whom he is closest philosophically) and has written penetrating, sometimes devastating, critiques of their views, he has shared with them a dedication to clarity and rigor in analysis and a conviction of the efficacy of constructional methods in philosophy and science.

Among Goodman's earliest published works in philosophy, "The Calculus of Individuals and Its Uses" (Leonard & Goodman 1940) and "Elimination of Extra-logical Postulates" (Quine & Goodman 1940) sound themes and exhibit concerns that are enduring and pervasive in his work.

Several generations of distinguished students, collaborators, or colleagues have been interpreters, extenders, or even critics of Goodman's work. The group includes, but is not exhausted by, Robert Ackermann, Jerome S. Bruner, Noam Chomsky, Lawrence Foster, E. Galanter, E. H. Gombrich, Vernon Howard, Paul Kolers, Howard Kahane, Sidney Morgenbesser, Hilary Putnam, Beverly Robbins, Robert Schwartz, Wolfgang Stegmüller, and Morton G. White. Three persons who also belong here could be singled out: first, Geoffrey Hellman, whose introduction to the third edition of *The Structure of Appearance* ([1951] 1977) provides both a helpful interpretation of that work and also points out the unity of Goodman's philosophy; second, Israel Scheffler, who has provided illuminating

explications and extensions of Goodman's work (especially Scheffler 1963; Rudner & Scheffler 1972) and who has collaborated with Goodman on important advances (Goodman et al. 1970); third, Joseph Ullian, who has coauthored with Goodman (1977) an important article that, among other things, integrates Goodman's earlier results on recalcitrant semantical concepts with Goodman's later work on the philosophy of symbolism.

In the course of his career, Goodman's honors have included invitations to visit and to deliver special lectureships (e.g., the Sherman lectures at the University of London in 1953 which formed the basis for *Fact, Fiction, and Forecast*; the A. N. Whitehead lecture at Harvard in 1962; the John Locke lectures at Oxford University in 1962 which formed the basis for *Languages of Art*; and the Immanuel Kant lectures at Stanford University in 1976 which furnished a substantial part of *Ways of Worldmaking* [1978b]). Recognition of his work was also signaled by a Guggenheim fellowship in 1946, his elections as vice president of the Association for Symbolic Logic (1950–1952) and as president of the eastern division of the American Philosophical Association (1967). His academic posts include an instructorship at Tufts College (1945–1946) and an associate professorship (1946–1951) and professorship (1951–1964) at the University of Pennsylvania. In 1964 he accepted the Harry Austryn Wolfson professorship at Brandeis University and remained its incumbent until 1967, when he organized and became director of "Project Zero," whose locus was Harvard's Graduate School of Education and whose aims involved basic studies in education for the arts. He remained director of "Project Zero" until 1971. In 1968 he accepted the position of professor of philosophy at Harvard, with joint membership in the departments of philosophy and psychology.

Goodman's philosophical, methodological, and scientific views are complex, far reaching, and insightful. In *Problems and Projects* (1972) and *Ways of Worldmaking*, Goodman reflects on his own work—particularly in the latter, which shows self-consciousness of its span to date. These reflections clarify the interconnections of themes and results in the entire body of his work. Secondary sources of commentary, explication, and criticism are copious, but few commentaries have dealt with comprehensive stretches of Goodman's work, and even fewer have seriously explored its unity.

The study of Goodman's comprehensive philosophy involves attention to at least three of its major aspects: his inductive theory, his constructionalism, and his radical epistemological relativism.

Inductive theory. The core of Goodman's inductive theory is presented in *Fact, Fiction, and Forecast*. It embodies three achievements: (1) a brilliant analysis and dissolution of the "old" problem of induction; (2) a radical critique of the concepts of *confirmation* and *confirming instance of a hypothesis*, which shows that extant theories of confirmation or evidential support of general hypothesis by instances are profoundly vulnerable to classes of devastating counterexamples; (3) the foundation for a positive theory of *projection*. (The notion of projection is more general than that of prediction.) Where scientific prediction may be construed as an inference, from examined cases in the past or present, to future unexamined cases, a scientific projection would be an inference to *any* unexamined cases from *any* examined instances. The theory includes, pivotally, his analyses of the concepts of similarity and of natural kind; these analyses are given important refinement and articulation in the "Seven Structures on Similarity" (1970, reprinted in [1972] 1977), *The Structure of Appearance*, and *Languages of Art*.

Constructionalism. This aspect of Goodman's philosophy is often held to be *methodological*, as distinguished, presumably, from *substantive*. Goodman himself holds that the techniques and principles for the construction of conceptual systems, systems of definitions, and constructions of theory are, in a certain sense, neutral. That is, they can be neutrally employed by widely divergent, or otherwise conflicting, philosophical positions. On the other hand, Goodman would not hold that his constructionalism was merely "methodological" in the sense that it was neutral among *all* philosophical positions. More characteristically, he would reject the method-substance distinction as an untenable dichotomy and look, rather, for the specific reasons why some techniques of theory construction remain neutrally applicable across disciplines and positions while others do not. Goodman's constructionalism places him in a tradition that not only includes Bertrand Russell, Ludwig Wittgenstein, and Rudolf Carnap, but also, along with them, in a tradition that includes Gottfried W. von Leibnitz, George Berkeley, David Hume, Immanuel Kant, and John Stuart Mill, and surely stretches back to the Eleatics and Pythagoreans,

and surely embraces Plato, Aristotle, and the Stoics. This is the tradition concerned with conceptual systematization—system building—while not eschewing the resources of formal logic (in contemporary times, the resources of the first-order predicate calculus) in fashioning systems of definitions and systematically related principles or statements. Goodman's results in providing principles and methods of systematization and in furnishing varieties of systems, are powerful and far reaching. They are presented in *The Structure of Appearance* and in a set of important auxiliary essays including "Steps Toward a Constructive Nominalism" (1947), "A World of Individuals" (1956), and "On the Way the World Is" (1960), all reprinted in *Problems and Projects* ([1972] 1977). Intimately connected with these essays is the brief but pivotal "On Relations That Generate" (1958) and a later comprehensive and daring essay that breaks much new ground, "Words, Works, Worlds" (1975*b*). These essays contribute greatly both to Goodman's theory of system building and to the final aspect of Goodman's philosophy considered here: his epistemological relativism. Another landmark contribution stemming from Goodman's concern with the theory of systems, however, is his theory of the formal or structural simplicity of theories. The calculus of simplicity presented in *The Structure of Appearance* underwent considerable refinement (for a tracing of the subsequent discussions and criticisms in the literature, see chapter 7 of Goodman 1972) and appears in an improved version in the third edition. Here Goodman develops a rigorous technique for measuring the relative simplicity of explicitly formulated conceptual systems (e.g., systems of definitions or constructional systems which effect systematic analyses of concepts)—a measure permitting judgments of the degree of systematicity or the degree of systematization of such systems.

Epistemological relativism. A striking feature of Goodman's first book is its epistemological antiabsolutism. Although Goodman shares a long tradition of constructionalist penchant for systematizing through the use of the techniques of formal logic, he breaks radically with much of that tradition by denying that there are any empirical or rational (incorrigible) "givens." He has sometimes erroneously been assimilated to the camp of the early logical positivists because the central system he constructs in *The Structure of Appearance* is (not accidentally) like the phenomenalist system of Carnap's *Logische*

Aufbau (see Carnap 1928). But Goodman makes it abundantly clear that the choice of a phenomenalist basis—i.e., the choice of phenomenal predicates as systematically primitive—carries no commitment to a doctrine of the epistemological primacy of the phenomenal. Any inference to such a conclusion is at the least a *non sequitur*. The *Structure* in fact furnishes a lucid analysis of the difficulties that must beset attempts to found any systematic theory of knowledge on any foundations claimed to be epistemologically uniquely incorrigible or uniquely warranted. But Goodman's radical relativism goes beyond epistemological relativism—at least of the sort that deals just with phenomenal or physical or other systematic theories of the world. For theories are linguistic systematizations, and in *Languages of Art*, Goodman, attempting to fashion a general theory of symbol systems which will govern nonlinguistic and nonverbal, as well as verbal or linguistic, systems of symbols, addresses problems connected with symbolic processes that are referential without thereby describing. These are symbolic processes that, like paintings or diagrams, may (still systematically) show the way the world is rather than describe it. The penetrating analyses and creative verve of *Languages of Art* provide Goodman with precise and rigorous means for presenting, in *Ways of Worldmaking*, the theses that make up his radical relativism. In this view, truth can be neither a necessary nor a sufficient condition for the acceptability of accounts of the world. Truth is not sufficient because many other criteria, like simplicity or systematicity, must be satisfied for an account to be acceptable. Nor is it necessary, since truth—a criterion applicable to linguistic symbols—cannot be characteristic of nonverbal symbolic accounts of the world. A central tenet of his position is that there is no uniquely correct way of “rendering” the world (“constructing a world”). Goodman holds that even two incompatible, or otherwise conflicting, accounts or versions may both be acceptable. But his radical relativism is “under rigorous restraints”; thus he is at considerable pains to point out that it is not the case that “anything goes.” Not every version of the world—or, more precisely, not every world-version—or some aspect of it is as acceptable as every other world-version. He meticulously develops criteria that permit rejection for good cause. Although there can be no uniquely acceptable version, many versions are rationally rejectable as inadequate.

Ways of Worldmaking puts together a daring philosophical system, which is likely to have profound effects on the work and thought of intellectuals who encounter it. It by no means solves all the problems it poses, which are at least as numerous as the “answers” it provides. Goodman continues to work on these problems.

RICHARD S. RUDNER

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GOTTSCHALK, LOUIS

Born in 1899 in Brooklyn, New York, Louis R. Gottschalk graduated from Cornell University with an A.B. in 1919 and a Ph.D. in history in 1921. After teaching briefly at the universities of Illinois and Louisville, he became associate professor of history at the University of Chicago in 1927, chairman of its history department for a five-year tour of duty in 1937, and president of the American Historical Association in 1953. At his retirement from the University of Chicago in 1964 he was Gustavus F. and Ann Swift distinguished service professor. After his retirement he taught full time for several years, and then part time, at the University of Illinois at Chicago Circle. He died in Chicago on June 23, 1975.

Methodologically, he was a transition figure from the "Rankean" tradition of writing narrative history of the political elite from critically analyzed documents to the social scientific history of a moving synthesis of social forces. At Cornell, Gottschalk had been taught in the Rankean

tradition by several professors, but chiefly by Charles Hull, whose historical seminar emphasized the intense, critical scrutiny of the evidence. "I learned historical method from Hull," Gottschalk later remarked, using the term "historical method" in a very special sense. In his view historians endeavor to recreate only so much of the past as "can be meaningfully reconstructed" (1950*b*, p. 48). That means that they (1) seek out the records and survivals of mankind's past; (2) critically examine those records and survivals; (3) from the information the records and survivals provide endeavor to imagine what the past was like; and (4) present the results of their imaginative reconstruction of that past in ways that do no violence either to the records or to the canons of scientific imagination. According to Gottschalk: "The process of critically examining the records and survivals of the past is called *historical method*. The imaginative reconstruction of the past from the data derived by that process is called *historiography* (the writing of history)" (*ibid.*, p. 48). Historical method, then, included the analytical procedures for testing the authenticity of documents, restoring the text, identifying the author, establishing the approximate date and place of composition, and determining the author's general credibility. Those procedures Gottschalk learned in Hull's seminar, and he applied them and taught them throughout his scholarly career.

At Cornell, Gottschalk also absorbed ideas about *historiography*: that is, about historical interpretation, synthesis, and writing. Here the major influence was Carl Becker, who was interested in all sorts of ideas, from the formal ideas of abstract speculation to the images, common prejudices, and half-spoken notions of everyday life. However, he was not interested in ideas apart from the individuals who held them. In these years he was also conducting another crusade against the naïve assumption that there was a one-to-one correspondence between the statement of the historian and what had actually occurred. Becker crusaded in addition for excellent writing—for a perfect matching of each word with the image in the historian's mind, for a perfect fusion of form and content, of sound and sense, in a composition that flowed.

Anyone who attended Becker's seminar in those years could not fail to emerge with a lasting interest in the role of ideas in history and in writing well. Anyone who had the seminars

of both Hull and Becker could not fail to have an abiding concern with the practical and theoretical aspects of historical method. The two teachers gave Gottschalk questions to ask of history, and some of the means for answering them. They endowed him with an initial philosophy to exploit, before he developed one of his own.

Gottschalk began his first two studies of Jean-Paul Marat and of the Marquis de Lafayette while he was still under the dominating influence of the Cornell experience. His biography, *Jean-Paul Marat: A Study in Radicalism* (1927), was distinguished by its nonpartisan tone, the cool, competent appraisal of contradictory evidence, and lucid exposition. It was in essence a discriminating delineation of the interplay of temperament, events, and ideas in the evolving career of a popular leader. In this evolution, temperament and circumstances counted for somewhat more than ideas. A successful one-volume biographical study of radicalism in the person of Marat suggested to Gottschalk the preparation of a similar one-volume biographical study of liberalism in the person of Lafayette. However, events involving this hero of two worlds were so numerous and the sources so voluminous that a one-volume essay burgeoned into a monumental multivolume enterprise. Gottschalk published six volumes, carrying Lafayette from 1754 to July 1790. One or two additional volumes covering his career to July 1792 are scheduled to appear posthumously. Although the book on Marat, when necessary, displayed the skill in handling sources that Gottschalk had learned from Charles Hull, its dominant interests were Becker's—the interrelations of personality, formal ideas, and events. The Lafayette series smoothly fused the thoroughness and techniques of technical historical method with the interests of Becker. The Lafayette story presented several technical historical problems—the authenticity of documents, the dating of letters, the accuracy of memoirs, the question of intent, the reliability of oral tradition. These problems were solved in the Lafayette volumes with professional assurance and proficiency, in the best traditions of the Rankine school. One reviewer observed that the manner in which Gottschalk managed to gather his material and sift it "by the most enlightened processes is a model for all research historians." However, the verity of historical statements was not established simply from delight in establishing verities. The statements were used to pre-

sent a full-bodied biographical narrative of an elite figure and to define delicately and subtly the moving interrelations of ideas, characters, and events in the evolving destiny of a historical personality.

While Gottschalk was working on the Marat and Lafayette volumes, he was as a scholar having a series of maturing experiences that led him to become a leader in the comparative study of revolutions, and methodologically, a specialist on generalizations in history. When he entered the academic world in the 1920s the predominant mode of historical understanding, old as Herodotus, was still the narrative story of events, of persons and their motivations and actions, considered in their unique aspects and assumed to be connected. In this world of persons, contingency, and accident, the narrative explained as it described and had its own dignity and importance. It did not need the aid of the social sciences. This was the establishment view, and a young scholar who affronted it was considered bold, daring, and perhaps suspect.

However, from 1923 to 1926 Gottschalk prepared six booklets on the history of the French Revolution, which were later expanded into a textbook, *The Era of the French Revolution* (1929). Preparation of this general work, along with his own research and teaching, led him to reflect on the causation of the French Revolution. The results of these reflections were published, notably, in four works: "The French Revolution: Conspiracy or Circumstance?" (1931), "Some Recent Countersocialistic Literature" (1945), "Philippe Sagnac and the Causes of the French Revolution" (1948a), and *The Place of the American Revolution in the Causal Pattern of the French Revolution* (1948b). Reflections on the comparative study of revolutions and the generalizations that might be derived therefrom were scattered throughout his writings but appeared notably in four articles, "Revolutionary Analogies" (1926), "Leon Trotsky and the Natural History of Revolutions" (1938), "Revolutionary Traditions and Analogies" (1939c), and "The Causes of Revolutions" (1944).

In 1933 he began to collaborate with several colleagues at the University of Chicago in teaching a "Laboratory Course in Historical Method." Several years later he took over a course in modern European historiography. In the 1940s and 1950s he became associated with the Social Science Research Council in various capacities. He was present at the charter conference of November 8, 1942, when a group of historians

met with members of the SSRC's Committee on Problems and Policy to consider some of the relations between historical studies and other fields in the social science area. He was also a member of the SSRC's first Committee on Historiography which grew out of the conference. Other members of the committee included Merle Curti, chairman, Charles A. Beard, Shepard Clough, Thomas C. Cochran, Jeanette Nichols, Richard H. Shryock, and Alfred Vagts. For the SSRC Committee on the Appraisal of Research, he helped to prepare Bulletin 53 on *The Use of Personal Documents in History, Anthropology, and Sociology* (1945). As member of the Committee on Historiography he participated in the preparation of Bulletin 54, *Theory and Practice in Historical Study: A Report of the Committee on Historiography* (1946). Later, as chairman of the Committee on Historical Analysis, he edited a report on *Generalization in the Writing of History* (1963). The fruits of this teaching and committee discussion appeared in the above publications and also in remarks at a round-table discussion on "Generalization in the Social Sciences" (1940), in a manual for apprentice historians, *Understanding History* (1950b), in his presidential address to the American Historical Association, "A Professor of History in a Quandary" (1954), and in three articles, "The Scope and Subject Matter of History" (1941), "The Historian's Use of Generalization" (1956), and "Categories of Historiographical Generalization" (1963).

So far as can be judged from the published evidence, Gottschalk played a mediating role in this ongoing methodological discussion. During the 1940s, 1950s, and the early 1960s, historians who were interested in the social science approach to history felt it important to define their terms, to define their approach, and to crusade for their point of view, against the narrative-establishment school. This they did, most notably in SSRC Bulletin 54 (1946). In this movement to social science history, Gottschalk insisted that social science historians incorporate into their approach what seemed most useful in the Ranke tradition: the intense scrutiny and validation by well-tested procedures of each source and statement. Much of his essay in Bulletin 53 on the use of personal documents in history as well as much of his manual on understanding history is devoted to these procedures for testing authenticity and credibility. On the other hand, to the establishment-narrative-of-unique-events school, he led a

personal crusade in behalf of generalization in historical study. In the use of generalization Gottschalk divided "historians" into six categories: "the school of the unique," the narrative-descriptive historians of very limited generalization, the "school of generalizations on the bases of trends," the comparative historians, the nomothetic historians, and the cosmic "philosophers of history" (1963, pp. 113-114). Gottschalk's favorite tactics showed that even historians of the unique inevitably generalize if they use language, while other types and levels of generalizations, if analytically tested, are useful for the insights they yield. "The world has room and the profession has need for all kinds of historians" (1963, p. 129). He himself had operated in all categories except the last. By 1963, when *Generalization in the Writing of History* appeared, the double crusade for incorporation of the best traditions of critical analyses of documents into the social science approach and for an ever-widening hospitality to generalizing historians in the profession had been won, and to younger historians Gottschalk's remarks seemed platitudinous. To a degree the reputations of Gottschalk and his associates as methodologists were the victims of their success. Once their battles had been won, people forgot there had ever been any enemies to fight. Gottschalk was aware of this; his crusading stances and phrases had become familiar, but as he once remarked: "the familiar is not always contemptible."

By the 1950s and 1960s Gottschalk was articulating the working eclecticism of the American research historian of those decades. In so doing he altered some of the questions that had been traditionally asked of historical writing. Instead of asking, "Is history an art or a science?" he assumed that writing history is now a profession and inquired, "How can historians maintain high professional standards?" With apologies to the Dutch historian, Johan Huizinga, he offered another definition of history as an intellectual form whereby each individual renders unto himself an account of his past, including the past of his civilization. Some accounts will be better than others. To assure that a historical account was the best possible and met the highest professional standards, Gottschalk recommended the Ranke-like critical evaluation of evidence. He also adopted the fundamental observation of Becker and other relativists that the historian writes in the present, and that in selecting and arranging his validated statements he is necessarily affected by present conditions,

including his own biases. However, according to Gottschalk, to minimize the distorting effect of the present, the professional historian "will try to give to each piece of testimony in his collected data its full and no more than its full weight"; he "will also make a conscious effort to lean over backward against his own national, religious, racial, party, class, professional, or other biases"; "in cases where the testimony is unavailable or, if available, inadequate for a definite conclusion, he will be careful . . . to suspend judgment"; "finally, he will studiously avoid gratuitous assumptions or inferences and will endeavor to present only such conclusions as logically proceed from the evidence." Gottschalk also adopted the view of Frederick Jackson Turner, James Harvey Robinson, and others that history is not only past politics and past economics, but also past literature and past religion, in fact, past everything. In reconstructing this comprehensive past, the historian will seek the aid of social scientists. He will check their generalizations against his data and utilize them when they seem valid and relevant. Indeed, he himself will "attempt contrasts and comparisons of historical episodes, situations, and institutions and advance generalizations of his own." A comparison of the major revolutions in Western civilization would seem to reveal that back of each one lay a class or classes that were dissatisfied with the existing order, ideas about what a new order might be like, and a government incompetent for discipline or reform. Gottschalk would contend that of the three conditions, formal ideas are the least important: "Though intellectuals usually have a role to play in causing revolutions, it is seldom decisive." They are primarily critics. They do not "father new intellectual attitudes but work upon the raw material of independent hostility" around them. They stimulate, energize, verbalize, and organize this material, and only secondarily add to it. The American Revolution obviously aggravated all three basic conditions in France. The example of a freer society intensified dissatisfaction with the Old Régime; the state constitutions suggested measures of reform; the expenses of French participation in the war rendered unmanageable the recurring deficit and the royal debt, and this weakened the royal government.

If in his mediating role between the old historiography and the new, Gottschalk in the methodological statements of his middle years may have sown seeds for the new, in his later years he may have tended to return to the faith

of his scholarly fathers. In the preface to the 1967 reprint of his biography of Marat, he referred to his "increasing conviction that the indeterminables (perhaps indescribables) which for want of a better name we call *chance* and *choice* or *free will* play roles—sometimes large, sometimes small, but rarely negligible—in historical and particularly in biographical developments." Of all his works, perhaps those that will endure longest will be the most old-fashioned: the narrative volumes on Lafayette. They may have errors or disputed judgments, but because of their vast and intelligently critical erudition, scholars will have to refer to them for years to come. If there is a lesson in this, let others figure it out.

HAROLD T. PARKER

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GUILFORD, J. P.

Joy Paul Guilford was born on March 7, 1897, on a farm in Nebraska. After teaching in public schools and serving a stint in the U.S. Army during World War I, he completed his B.A. (1922) and his M.A. (1924) at the University of Nebraska. In 1927, he received his doctorate in psychology from Cornell University, where he studied psychology with Edward B. Titchener and Kurt Koffka. He taught briefly at the University of Illinois and the University of Kansas before returning to his alma mater, the University of Nebraska, where he headed the psychology department until 1940. In that year, he moved to the University of California and remained there, except for his service in the U.S. Army Air Force during World War II, until his official retirement in 1969.

Guilford has dedicated his life to making psychology a quantitative, rational science. Not only has he made numerous contributions to experimental psychology, psychometric and statistical methodology, the measurement of human abilities, and the assessment of personality, he has also written two classic texts, *Psychometric Methods* (1936) and *Fundamental Statistics in Psychology and Education* (Guilford & Fruchter 1942) and influenced generations of students.

Early in his career, the emphasis in Guilford's research was on experimental psychology and psychophysics, and the publication of *Psychometric Methods* culminated this period. He also reviewed previous investigations and carried out experimental studies on the fluctuation of attention, working with weak visual stimuli, eye movements, and apparent motion phenomena, pointing out the implications of his findings for current theories. Other experiments during this period concerned visual perception, reaction time, color preferences, facial expression, memory, and experimental aesthetics.

Then, in 1940, he began to gradually shift the emphasis of his work toward the measurement of personality traits and mental abilities, mental testing, statistics, and factor analysis. This trend was accelerated, and perhaps in part motivated, by his assignment during World War II to direct a psychological research unit of the U.S. Army Air Force in the development of group aptitude tests for the classification of cadets (1947). In this historic test development research program, Guilford put to large-scale practical test, often for the first time, the best available psychometric techniques, theories, and procedures of the day.

In addition, he developed innovative procedures, statistical techniques, and research methods. For example, he showed that after determining the criterion necessary successfully to complete a specific task, factor analysis could be used to determine the components of that criterion, and test batteries could then be developed that would mirror the factor structure of the criterion under consideration. These procedures substantially improved the chances of finding the best test measures for predicting the degree to which an individual met any particular criterion.

After the war, Guilford continued his work with aptitude tests and factor analysis. Many of these later studies were published by the Psychological Laboratory of the University of Southern California (e.g., Guilford et al. 1960) and were supported in part by a series of contracts with the Office of Naval Research. These postwar studies increased psychologists' knowledge of human abilities in the areas of spatial perception, reasoning, evaluation, planning, problem solving, and creativity. Guilford's approach to human abilities, like that of L. L. Thurstone, contrasted sharply with that of Alfred Binet, C. E. Spearman, Lewis M. Terman, David Wechsler, and Raymond B. Cattell. The latter have emphasized the importance of general intelligence as the dominant concept in the measurement of human abilities. Although they acknowledge the need to divide intelligence into its component parts to some extent—e.g., verbal versus quantitative, fluid versus crystallized—only a few scores are deemed necessary to describe the intellectual status of an individual.

Although recognizing the possibility of a general factor of intelligence, Guilford has preferred to break down intelligence into finer constituents, as well as to measure variables not covered by intelligence quotient tests. He maintains that an adequate description of human intellectual capacity requires the measurement of more than one hundred variables. To provide a theoretical framework for his findings, Guilford has constructed his famous structure of intellect model (1959), which does for human abilities something analogous to what the periodic table does for the chemical elements. Any given factor occupies a cell within a box representing a three-dimensional classification of factors. Each factor deals with a certain type of material, called "contents": visual, auditory, symbolic, semantic, and behavioral; deals with a certain type of operation: evaluation, convergent production, divergent production, memory, and cog-

niton; and involves a certain type of product: units, classes, relations, systems, transformations, and implications. This box allows for 150 factors. A good deal of work since the middle of the 1950s has been devoted to developing measures that would define relevant factors that should exist in the model, but that have not yet been identified in empirical research studies. Guilford's views on abilities and intelligence, as well as summaries of his and his collaborators' work in these areas, are summarized in *The Nature of Human Intelligence* (1967b), *The Analysis of Intelligence* (Guilford & Hoepfner 1971), and *Way Beyond the I.Q.* (1977). In the 1970s, Guilford has also turned his attention to using the structure of intellect model in the development of an operational-informational point of view for understanding many important phenomena in cognitive psychology. He has shown the relationship between many traditional points of view in psychology and his more comprehensive theoretical system (1974).

Although Guilford has devoted less of his energy to the study of the nonintellectual aspects of human personality than he has to the study of abilities, his work in the former area began in the early 1930s. He first proved that the then popular concept of extraversion-introversion could be broken down into several distinct and relatively independent component factors. This work was followed by the research and test development that led to the *Guilford-Zimmerman Temperament Survey* (Guilford & Zimmerman 1949) and the personality taxonomy upon which it was based. Again, factor analytic procedures were used to develop these tests, just as in the area of human abilities. His work in personality and that of other investigators is presented in his book *Personality* (1959). Guilford's multiple-factor personality taxonomy contrasts markedly with Cattell's 16 factor taxonomy (1950) and with Hans J. Eysenck's 2 factors of personality, extraversion-introversion and neuroticism (1953). The differences in these competing taxonomies result at least in part from different factor analytic philosophies in research and test development. Eysenck extracts few factors in his analyses, whereas Guilford and Cattell extract many; Cattell relies on blind rotation to oblique simple structure, whereas Guilford leans toward orthogonal constructs and uses simple structure as a guide only when it does not contradict other knowledge he has about the test variables. The test of time alone will determine

which of these competing typologies will ultimately prove to be the most useful.

During his long career, Guilford has authored more than three hundred books, monographs, and articles on a wide variety of scientific topics in psychology and related fields. Although he has never been inclined to seek recognition for himself, wide appreciation of his outstanding contributions has resulted in his being honored again and again. He has been president of the Psychometric Society, the Midwestern Psychological Association, the Western Psychological Association, the California Color Society, and the American Psychological Association. He is one of the comparatively few psychologists elected to the National Academy of Sciences and is the first president of the International Society for Intelligence Education.

ANDREW L. COMREY

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H

HALLOWELL, A. IRVING

Although the bulk of Alfred Irving Hallowell's scholarly career was devoted to the study of the Northern Ojibwa, a people whose society, culture, and thought he described and analyzed in meticulous detail, his primary concern in this ethnographic effort was to discover and display the general through the particular. When one also considers the remarkable array of theoretical issues to which he made original and pioneering contributions—social organization, culture and personality, acculturation, behavioral evolution, law, religion, myth, world-view, cultural ecology, and the history of anthropology, among others—it becomes clear why his influence has been felt in disciplines beyond his chosen field of cultural anthropology.

Hallowell was born in Philadelphia in 1892, an only child in an old Philadelphia family, and he died in that city in 1974. His early education did not augur the pursuit of a scientific career. After graduating from a manual training high school, he enrolled in the Wharton School of Finance and Commerce of the University of Pennsylvania, and upon receiving his degree he became a social worker in the Family Society in Philadelphia. While employed as a social worker, Hallowell took graduate courses in sociology and anthropology. Under the influence of Frank G. Speck, his teacher in anthropology (and a distinguished American Indianist), Hallowell developed his interest in American Indians and decided to become an anthropologist. From Speck and from Franz Boas (the leading anthropologist of his time, whose weekly seminar

Hallowell attended at Columbia University), Hallowell acquired his lifelong view of anthropology as a holistic discipline, embracing ethnology, archeology, physical anthropology, and linguistics.

Hallowell's work can be chronologically divided into three phases: an early ethnological phase; a middle "culture and personality" phase; and (toward the end of his career) a "historical" phase centering on the history of anthropology and on behavioral evolution. Although the achievements of each phase were both original and (especially in the cases of the last two) pioneering, those of the middle phase were perhaps the most significant. His delineation of the relationships between culture and personality—in particular, his analysis of the interpenetration of the "inner world" of social actors with the "outer world" of the social order—constitutes a signal contribution to the understanding of the cultural dimension of human experience and the psychological dimension of human culture.

In a period in which behaviorism was a dominant intellectual mode in the social and behavioral sciences, Hallowell was attempting to show, using Ojibwa data as his vehicle, that social action is not so much a function of the properties of the physical and social environments, as objectively described by natural and social scientists, as of the meanings these properties have for the social actors. Taking his point of departure from the gestaltist notion of the "behavioral environment," Hallowell went beyond the gestaltists in his demonstration that perceptions are mediated not through perceptors

alone, but through cognitive orientations that organize and shape them, and that these latter orientations are acquired in large part from cultural symbol systems. From this demonstration it then followed that the *human* behavioral environment was (as he put it) "culturally constituted." In demonstrating how the various members of a social group, each initially encapsulated in the privacy of his own inner world, nevertheless come to share a common behavioral environment, Hallowell identified and described the process by which an aggregation of private individuals becomes a group of social actors. In brief: through the explication of his concept of a culturally constituted behavioral environment, he also demonstrated, in part, how a human social order becomes possible (1955).

Unlike many social scientists, however, for whom the social order is a given, Hallowell always saw it as problematic. While stressing that regularities in social behavior are made possible by culture, Hallowell (who had early discerned the importance of Freud for anthropology) also stressed that there are dimensions of the inner world of social actors—impulse, affect, imagination, fantasy, and the like—that are not derived from (and that indeed elude the influence of) the outer world of culture. These dimensions are a potentially disruptive threat to the social order, for even when they do not lead to the construction of private behavioral environments (as they do in mental illness), they may lead to contemplated action that violates the normative requirements of the social order. To cope with this problem, Hallowell elaborated his seminal concept of the "self," a concept correlative to, and as important as that of, the behavioral environment (*ibid.*).

Calling again upon his detailed knowledge of the Ojibwa, Hallowell argued that the self, a cognitive structure unique to man, develops by means of a set of basic orientations acquired from cultural symbol systems. Lying at the intersection of the inner and outer worlds, and mediating between them, the self protects the social order from the potentially disruptive threat of the former world in two ways. First, as a result of achieving self-awareness, and thereby the ability to distinguish inner and outer stimuli, social actors can both adapt their perceptions to culturally constituted cognitions and monitor their behavior by reference to cultural norms. Second, when the demands of inner urges give evidence of becoming more powerful

than those of outer norms, the attendant moral threat to the actor's self-conception typically assures the enactment of norm-governed action.

Although Hallowell was a prolific scholar, the bulk of his work consisted of research papers and theoretical essays rather than of books and monographs. This work is typically marked by a bifocal perspective because even when focusing on a specialized topic, Hallowell is aware of the widest focus of anthropological inquiry, the nature of man. Hence, although much of his writing dealt with specific ethnographic contexts, his concern with the uniquely contextual was based on and informed by an interest in the generically human; and the latter interest projected a vision of what anthropology at its best, as "the study of man," could be.

MELFORD E. SPIRO

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HANCOCK, WILLIAM KEITH

William Keith Hancock, known for his research and teaching in Australian and Commonwealth history, was born in Melbourne on June 26, 1898, the son of an Anglican clergyman. He studied at the University of Melbourne, where Ernest Scott and Harrison Moore were among his most influential teachers. After a brief period as assistant lecturer at the University of Western Australia under Edward Shann, he received a Rhodes scholarship that took him to Balliol College, Oxford. Soon afterwards he became the first Australian fellow of All Souls College, where he wrote his first book, *Ricasoli* (1926), a study of the Risorgimento in Tuscany.

In 1924, he returned to his native country as professor of history at the University of Adelaide, where his seminal work *Australia* was published in 1930. He returned to Britain in 1934 as professor of history at the University of Birmingham, where he began a massive *Survey of British Commonwealth Affairs* (1937–1942) for the Royal Institute of International Affairs. In 1941 he began editing the Civil Series of the official *History of the Second World War*, contributing an initial volume on the *British War Economy* (Hancock & Gowing 1949). In 1949, after five years as Chichele professor of economic history at Oxford, he became the first professor of British Commonwealth affairs in the University of London, and director of the newly founded Institute of Commonwealth Studies.

He returned again to Australia in 1957 as director and professor of history of the Research School of Social Sciences at the Australian National University. Before his retirement he completed a two-volume biography of *Smuts* (1962–1968), and he later published *Discovering Monaro* (1972), a new departure into ecological history. He also published two volumes of memoirs and reflections, *Country and Calling* (1954) and *Professing History* (1976), and selected essays in *Politics in Pitcairn* (1947). His 1960 Wiles lectures at Queen's University Belfast were published as *Four Studies of War and Peace in this Century* (1961). He was knighted in 1953, being known as Sir Keith Hancock.

Hancock's one excursion into policymaking, outside of his work on the official war histories, took place in 1954, when he accepted an invitation from the governor of Uganda to mediate in a crisis that had arisen over the governor's withdrawal of recognition of the Kabaka of Buganda. In effect, Hancock's role was that of a constitutional expert who would persuade the leaders of Buganda to agree to constitutional revision and to reconciliation with the governor's hopes for self-government for Uganda as a whole. Hancock was successful in this enterprise, but later developments in both Britain and Uganda rendered his work futile.

Such a long, varied, and productive career gave Hancock a reputation in more than one field, based not only on his ideas, but also on his pioneering achievements in Australian and Commonwealth history. To this research, he brought a fresh eye, a rigorous method, a special concern for economic influences, a wide range of allusion, and a determination not to be fet-

tered by conventional wisdom. His treatment of the springs of action of contemporary Australian society contrasted sharply with earlier narratives of discovery, settlement, and constitutional change. Forty years later, *Discovering Monaro* gave Australians a new insight into their impact on the land. His *Survey* had a profound effect on the study of the dominions and colonies and their relations with Britain. He wrote with a firm underpinning of economic fact, shrewdly analyzed prevailing political trends, and placed constitutional issues in a perspective that had been previously lacking.

Hancock's ideas, from his thirties to his seventies, show remarkable consistency. They can be summarized in terms of his doctrines about history, his dominant themes, and his sense of morality, all of which are united in his writings.

Three cardinal virtues, in Hancock's view, distinguish the great historian. They are attachment, justice, and span. Attachment is "getting close to people, getting inside situations." Justice is fairness—"fairly to report each point of view . . . ; fairly to relate this point of view to the situation in which it arose; fairly to measure its effect upon the new situation that was arising." Span is remoteness; it involves a full awareness of backgrounds and the capacity to extricate oneself from the particular and see it in relation to other things: "Getting inside the situation is the opening movement; getting outside it is the concluding one" (1954, pp. 220–221). These three virtues are antecedent to moral judgment, which Hancock sees as a necessary function of the moral sense, but as applied to men, not to situations. The historian's job is first to establish a situation by using attachment, justice, and span, and then to decide how to judge it in terms of approval or condemnation. The situation is analyzed in terms of what is; the men involved in terms of what ought to be. Thus Hancock introduces morality, but not at the expense of realism. Once we know what happened, we can judge it, but we should not exercise judgment in ways that prevent us from discovering the true situation.

In the writing of history, Hancock distrusts reliance solely upon documents. He is fond of quoting R. H. Tawney's remark in the 1930s that "what historians need is not more documents but stronger boots"—i.e., that they should see things and people, and not confine themselves to libraries. He exemplified this maxim in his many travels to Commonwealth countries before writing his *Survey*, and in his frequent

trips over the ground surveyed in *Discovering Monaro*. He respects documents and is glad that he was properly schooled in their use, but "the historian needs also a lust for life" (1954, p. 95). Moreover, the historian should remember his obligation to see every situation in its entirety. He may, for example, be a more useful person in some situations than the economist, "because he is rather less of a scientist, rather more of a humanist; his concern is not with economic man but with men and women in the round" (1976, p. 91). He may use the material and sometimes the language of the social sciences, but he should concentrate upon simplicity in his own narrative exposition: "to split the *ism* demands clarity of thought and constant watchfulness" (1954, p. 227).

Although Hancock's writing is always about concrete matters—colonial policy, economic development, nationalist urges, constitutional issues—in quite specific situations, two broad themes animate most of it: that of man for and against his environment, and that of man for and against himself. The environmental theme did not become dominant in Hancock's work until comparatively late in his career with *Discovering Monaro*, but it appeared much earlier in his life and writings. In Tuscany, he wrote about Ricasoli's agricultural innovations as well as his political leadership. And, although the first volume of his *Survey* was almost exclusively political in its focus, Hancock in the second volume wrote as much on economic problems from an ecological perspective, and this enriched his discussion of such issues as soil erosion in South Africa and swollen shoot disease in the Gold Coast. When, after retirement, he studied the land use in a small highland area of his own country, he thus had many bases of comparison as well as a deep belief in ecological preservation. Putting his ideas to practical effect, he led a campaign to prevent the Australian government from erecting a vast tower on Black Mountain, overlooking Canberra, where he lived. Significantly *The Battle of Black Mountain* (1974) is the most passionate of his works.

A more important theme, however, is Hancock's concern with man for and against himself, especially in a national context. Here his starting point is Machiavelli, on whom he wrote essays in the 1930s—essays that anticipated the later debate about "idealism" and "realism" in the study of international relations (1947, pp. 18–50). Although recognizing the force of Machiavelli's explanations of international con-

duct in terms of necessity and interest, Hancock repudiated the notion that might should be regarded as right and pointed out the contradictions in Machiavelli's own formulation, particularly Machiavelli's need to abandon his own compelling analysis in designing his ideal commonwealth. Force and fraud were powerful but not the whole of the story. The Hobbesian situation "exists, and it deserves far more rigorous study than it usually gets; but it is never the whole historical situation, which contains from one historical moment to another a varying blend of force and idea, of interest and obligation, of predestination and free will" (1954, p. 208).

Hancock saw the theme of man for and against himself in the political arrangements of the British Empire—particularly where the British had striven to establish liberty under law in their dependencies; but he found the doctrines of self-government and racial impartiality difficult if not impossible to combine in so many different situations. Communalism was a feature of many of the British dominions and colonies, subdued in some, but a constant danger in others. Especially in situations like those of Palestine and southern Africa, Hancock accurately predicted in the 1930s the dilemmas that would continue to be disturbing in the 1960s and 1970s. Nevertheless, he saw British rule as broadly beneficent. Britain had ruled, but in most cases it had not divided: the divisions were there to begin with. The Commonwealth, as "a group of sovereign nations whose members have developed to an unusual degree the habit of mutual consultation" (1961, p. 113), provided an opportunity for amelioration of otherwise troublesome divisions, but it was no cure-all.

Hancock once wrote: "I cannot rid myself, I do not wish to rid myself of my very British preoccupation with right conduct. After all, I belong to a people which by Machiavelli's own tests remains 'uncorrupted', a people which lives in liberty under the law, and for all its conflicts of material interest is capable of passionate response to the call of civic duty" (1947, p. v). The preoccupation with right conduct is fundamental to his basically Christian approach to moral issues, but it is always informed by awareness of the intricacies of politics and the compelling force of economics. Moral judgment is confined to the actions of individuals, especially those in authority. Situations at large, especially those involving large bodies of people

driven by what they conceive to be necessity, are analyzed with compassion and understanding. Hancock balances moral, scientific, and aesthetic concerns more firmly and effectively than do most historians. This balance is aided by Hancock's style. He moves from plain statement to quirky allusiveness, but persistently writes with lucidity, delicacy, and imagination.

J. D. B. MILLER

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HANKE, LEWIS U.

Lewis U. Hanke possesses a fundamental belief in the contemporary importance of historical inquiry. By his actions and his statements, he has expressed the conviction that knowledge of the past represents a basic key to human self-awareness and global understanding and can help in the formulation of messages with profound and practical meaning for the present. He has remarked on more than one occasion: "All history is contemporary history," and he sees the social role of the historian as one of grave responsibility.

Hanke has revealed this commitment throughout his scholarly work, most conspicuously in

his writings on Spanish attitudes and behavior toward Indians in colonial Latin America. In his best-known work, *The Spanish Struggle for Justice in the Conquest of America* (1949), he stressed the efforts of Spanish theologians to define and defend the humanity of the indigenous population. Central to his theme was the description of a debate in 1550-1551 between Juan Ginés de Sepúlveda and Bartolomé de Las Casas, a subject to which Hanke frequently returned in subsequent work. Drawing upon the Aristotelian notion of "natural slavery," Sepúlveda had argued that the Indians were naturally inferior and therefore subject to outright domination and enslavement by Spaniards. Emphasizing the ideal of a universal Christian community, Las Casas insisted that Indians were rational beings, capable of persuasion, and that they should consequently be converted peacefully.

The mere occurrence of such a debate sharply modifies the stereotypical "Black Legend" about Spanish cruelty, and the contradiction between Sepúlveda and Las Casas helps explain the evolution (and vacillation) of sixteenth-century royal policy toward the Indians. But Hanke does more than refine our comprehension of the past. Sometimes citing present-day instances of neocolonial domination, racial turmoil, and ethnic discrimination, he openly expresses his admiration for Las Casas' vision, courage, and tenacity. He has taken a quotation from the friar as the title of a recent monograph (1974): "All Mankind Is One."

Hanke's writings on this subject have prompted excitement, research, and dispute. Some critics have charged that he has replaced the "Black Legend" with an equally simplistic "White Legend" of Hispanic benevolence, where in reality Indians languished in misery. Others have claimed that his focus on ideas and theology has ignored base human motives and the clash of material interests. Still others have alleged that his textual interpretations have overlooked some critical distinctions, specifically the difference between "serfs" and "slaves." But Hanke seems to have thrived on such controversy, and he has remained a dominant figure in the area of colonial Latin American history.

A prolific researcher and writer, Hanke has published on other topics as well. He made extensive investigations into the urban history of Potosí, the colonial mining center of what is now Bolivia, anticipating and precipitating the modern analysis of social history in Latin Amer-

ica. Recently he coedited a massive, multivolume series of Spanish viceregal reports on the Indies. These documents provide rich, detailed, and thorough information on political institutions and everyday life, on individuals and collectivities, and their publication will greatly facilitate historical investigation. The project also reflects Hanke's reverence for primary sources and his insistence that students of the past work, whenever possible, with complete and original records.

Throughout his professional life Hanke has striven to develop Latin American history as a legitimate field of serious scholarship, sometimes in the face of collegial condescension and institutional reluctance. He has served as editor of the *Hispanic American Historical Review*, written a college text, supervised the Alfred A. Knopf Borzoi series on Latin America, and brought together two large anthologies. He has also trained scores of Latin American historians, many of them now prominent scholars in their own right, and has expressed continuing interest in methods of undergraduate teaching.

Hanke was born in 1905, received B.S. and M.A. degrees from Northwestern University, taught for three years at the American University in Beirut, and earned his Ph.D. from Harvard University in 1936. After two years of post-doctoral study he was appointed director of the Hispanic Foundation at the Library of Congress in 1939. In 1951 he assumed a professorship at the University of Texas, in 1961 moved to Columbia University, in 1967 went to the University of California at Irvine, and in 1969 became the Clarence and Helen Haring professor of history at the University of Massachusetts—a position from which he retired in 1975. He has received many awards and distinctions, including honorary degrees from the University of Bahia, Brazil (1965); the Tomás Frías University of Potosí, Bolivia (1965); and the University of Seville (1966). In 1965 he was decorated by the Bolivian government with the Orden del Condor de los Andes and was made a corresponding member of the Royal Historical Society in London.

In 1974 he served as president of the American Historical Association. In his presidential address, he pronounced apprehension about the writing of self-centered "tribal" history and urged his professional colleagues to develop intellectual perspectives and working relationships on a wide international scale. As an example he

cited the case of Bernardino de Sahagún, a sixteenth-century Franciscan whose careful investigations of Aztec customs have left a faithful and sensitive account of precolombian indigenous civilization. There is an urgent need for United States historians to broaden their knowledge of other cultures, Hanke declared, for foreign historians to learn more about the United States, and for all to see that no single history can be properly understood in isolation. "Americans will then be ready for an even more difficult step," he concluded,

the initiation of fundamental revisions in their own views of the world, man, and the future, which began in the century of the great discoveries and for which Bernardino de Sahagún showed the way by his studies of Aztec culture. If American historians are fully aware of their opportunities and responsibilities in the world today, they can exert a powerful influence by their teaching and research to the end that we are able to appreciate the history of other peoples without losing allegiance to our own. By studying the history of their own tribes and other tribes as well, historians should be in the forefront of all those who would seek to understand the common elements in all cultures. (1975, p. 20)

In keeping with his own counsel, Hanke would later formulate a proposal for the compilation of a bibliographical guide to the writings of foreign historians on the history of the United States.

In a time when many historians feel unsure of their role and vague about their sense of social purpose, Lewis Hanke remains optimistic and enthusiastic. He is a man of humor as well as fervor. In his mind history lives.

PETER H. SMITH

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HARLOW, HARRY F.

Harry F. Harlow was born in 1905 in Fairfield, Iowa. Most of his undergraduate and all of his graduate education was at Stanford University, where he received his PH.D degree in psychology in 1930. His chief mentors there were Calvin P. Stone, a comparative psychologist, and Lewis M. Terman, who is best known for his development of the Stanford-Binet intelligence test and his studies of gifted children. Neither figure, however, seems to have exerted a dominating influence on Harlow. Indeed, although Harlow (1977) credits Stone with teaching him scientific techniques, many of Harlow's own students will recall his own reaction against Stone's methodological biases. The latter emphasized the systematic but slow process of pushing back the frontiers of science "step by step and stone by stone," whereas Harlow, noting that progress in science has often come in quantum jumps, vehemently downgraded parametric research and tried to instill in his students the knack of simply identifying and attacking important problems in behavioral research. Harlow credits Terman as his primary tutor in scientific theory, but since Harlow has never displayed a strong appetite for formal theorizing, Terman's influence seems to have been mainly on Harlow's choice of topics to study, best represented by his lifelong interest in primate intellectual capacities.

Harlow spent his entire academic career up to his semiretirement in 1974 at the University

of Wisconsin in Madison. He taught the introductory psychology course during most of those years, and it seems likely that this constant contact with diverse areas of human psychology eventually had a greater effect than did his graduate training upon his choices of subjects in the latter part of his research career. Although his doctoral dissertation dealt with social facilitation in rats, and a few studies with cats and even goldfish can be found among his more than three hundred articles, virtually all his research has been conducted with rhesus monkeys. In an informal autobiography, Harlow (1977) recalls that the lack of facilities for rodent research when he arrived in Madison prompted his initial primate studies at the local zoo. Soon afterwards, in 1932, the Primate Laboratory was established on the campus. In the course of thirty years, the laboratory was transformed into a large and interdisciplinary research complex that included the Wisconsin Regional Primate Research Center as well as the psychology department's Primate Laboratory; from 1961 to 1971 Harlow directed the entire complex.

In his first two decades of research Harlow carried out more than fifty studies of learning and cortical lesions in rhesus monkeys. This work laid down many of the standard procedures of laboratory primate learning research, later applied by researchers to species ranging from rats to children. During these years, Harlow and his students also produced many valuable empirical findings that shed light on the nature and limits of rhesus monkey intelligence; their animals performed impressively on high-order multiple-sign learning problems that had been thought to be beyond the learning capabilities of rhesus monkeys (e.g., Harlow 1943).

By far the most significant discovery of this period was the formation of learning sets (Harlow 1949). Departing from the usual procedure in research on rodent learning, of testing many animals on only one or two learning problems, Harlow trained each of a few monkeys on literally hundreds of visual discrimination problems and found that each monkey "learned to learn" new problems of the same type in a single trial. This learning set principle helped bridge the gap between slow, trial-and-error learning and rapid, insightful learning, a dichotomy that had been puzzling learning theorists for some time. Characteristically, Harlow called attention to the probable operation of this principle in everyday learning by humans in the socialization process,

suggesting that the achievement of high social intelligence may require the acquisition of multiple learning sets relating to classes of social stimuli and social-emotional reactions.

The learning set phenomenon itself presented a challenge to the formal stimulus-response theories that were being developed out of simple conditioning principles at the time by Clark L. Hull, Kenneth W. Spence, and others: Could the extremely rapid learning exhibited by learning-set-experienced animals be derived from the postulates of such theories? Not satisfactorily, according to impartial referees (e.g., Mackintosh 1974), and in fact it was not until conditioning-based theories shifted to more explicitly cognitive assumptions in the early 1970s that they could begin to handle the learning set phenomenon, which many theorists have come to believe requires a concept of strategy-learning for its explication. Harlow himself was led to some quasiformal theorizing on what he called "error factor theory" (Harlow 1959*b*), which was extended by others (e.g., Levine 1965) to strategy-learning conceptions (e.g., the monkey acquires and generalizes a "win-stay, lose-shift" strategy in ordinary discriminations) in mathematical form; however, empirical applications were probably more important.

Species comparisons in many different laboratories showed that learning sets were acquired with great difficulty, at best, in most subprimate species, in contrast to the relative ease of their formation by macaques, apes, and children. When Harlow established a breeding program in his laboratory in the 1950s, work on the ontogeny of learning capacities (Harlow 1959*a*) became possible, and it quickly demonstrated that learning set formation is possible in the rhesus only after a certain age (one to two years, depending on testing procedures). Thus, after similar species and age comparisons were made with respect to other learning tasks, it was only natural for Harlow to construct a battery of tests for monkey intelligence measurement in which discrimination learning set was a major ingredient. This test battery proved useful in his work on the effects of early brain lesions (Harlow, Thompson, Blomquist, & Schiltz 1970) and of enriched versus impoverished social rearing conditions (Gluck, Harlow, & Schiltz 1973), and in his collaboration with Harry Waisman, a biochemist and pediatrician, on studies of simulated phenylketonuria in rhesus monkeys (Waisman & Harlow 1965). The array of tasks, known as the Wisconsin General Test Apparatus stan-

dard battery, is probably the nearest thing to human intelligence quotient testing that exists in animal behavior research.

Harlow added another line of work, on curiosity and manipulation motives (Harlow 1953), soon after the learning set discovery. His collaborative studies with Robert A. Butler showing the extreme persistence with which monkeys will work on mechanical puzzles or open windows to observe other animals, without the conventional food or water incentives, presented a challenge to adherents of the homeostatic model of motivation, and gave Harlow another opportunity to polemicize against the myopic views of learning and motivation that were emerging from rat psychologists' work. As significant as the curiosity-exploration-manipulation research was, it now appears as a relatively small point against the homeostatic model compared to Harlow's voluminous contributions in the area of affectional systems, undoubtedly his best-known work.

The latter began with the surrogate mother studies (Harlow 1958). With little ambiguity, these showed that an infant monkey's strong attachment to its mother is based much more upon tactile sensations ("contact comfort") provided by the mother than upon the association of the mother with the reduction of such primary drives as hunger or pain. This conclusion probably came as no surprise to human mothers around the world who, through countless magazine articles and television presentations, became acquainted with Harlow's wire-and-terry-cloth mother surrogates, but it clearly ran counter to the position held by many psychologists and sociologists at a time when both Hullian learning theory and Freudian psychoanalytic notions were predominant.

For Harlow, the main significance of the surrogate studies was that they opened up the whole realm of primate affectional (love) systems to experimental dissection in laboratory settings. His wife, Margaret Kuenne Harlow, who was highly respected for her work at the University of Iowa, where she tested Spence's transposition theory in children (Kuenne 1946), and who had occasionally shared authorship with her husband in reports from the Wisconsin Primate Laboratory (e.g., Harlow & Harlow 1949), joined him fully in these social behavior studies. Until her death in 1971, she contributed immensely to the planning and execution, as well as the publication, of the affection studies. The direction of these researches was clearly given

by the Harlows' classification of the affectional systems into mother-infant, maternal, peer, heterosexual, and paternal types.

Inevitably, artificial and real monkey mothers had to be compared in their effects upon infant monkeys' social development, and from this work came detailed analyses of maternal love and of infant love for its mother (Harlow 1963). Besides demonstrating the superiority of the real monkey mother for the healthy social development of the offspring, and indicating several reasons for this, the studies articulated a crucial role of the real mother in regulating her infant's early play activities so as to foster effective peer interactions. The all-importance of play in the socialization process generally was emphasized in further studies concentrating on peer attachments (Harlow 1969). These studies also yielded the conclusion that peer interactions were more important than mother-infant interactions in terms of the monkey's whole life span, because of the role of social interactions with agemates in determining basic social roles, inhibiting aggression, and sexual maturation. The Harlows also described the heterosexual affectional system in adolescent and adult monkeys (Harlow 1962; Harlow & Harlow 1965), detailing its dependence on prepubescent peer interactions. The paternal love system was studied in a "nuclear family" setting designed by Margaret Harlow (1971). With the usual degree of planned serendipity that characterized the Harlows' social researches, this setting provided fringe benefits in the form of novel data on sibling interactions, the influence of offspring upon parental sexual motivation, and the effects of an unusually enriched social rearing environment upon learning capacities, social adjustment, and heterosexual maturation in offspring.

Certainly more dramatic than the normal social interactions described in the love researches were the forms of maladjustment discovered when social deprivation conditions were imposed. It is here that the Harlows exploited most effectively the laboratory control of antecedent conditions in psychosocial development. Reared for the first 6 or 12 months in total social isolation, and thus unable to form normal infant-mother or peer attachments, monkeys displayed severe and permanent social impairments, including stereotypes resembling autism, extreme fear, inappropriate aggression, and sexual ineptness (Harlow & Harlow 1965). Even partial isolation, in which the monkeys were reared alone in wire cages but allowed to see and hear other

monkeys nearby, was found to be deleterious in many of the same ways. "Motherless mothers"—that is, isolation-reared females forcibly impregnated in adulthood—proved to be grossly deficient in maternal care of their first-born infants, usually ignoring and often abusing them.

Harlow's social isolation studies ended on a happier note—successful psychotherapy. After some years of assuming that isolation-reared monkeys were hopelessly incurable, and after some fruitless attempts at rehabilitation by others, the Harlows and their students Melinda A. Novak and Stephen J. Suomi devised a procedure in which monkeys isolated from birth to six months were exposed, immediately after the isolation period, to socially normal monkeys only half their age. The younger "therapist" monkeys were still at an age of persistent contact clinging, in the early developmental stage of play, yet too young to display aggression. Over another six-month therapeutic period, these attributes, plus, probably, the smaller size of the therapists, gradually converted the isolates from fearful retreat and stereotyped rock-and-huddle to nearly normal agemate attachment behaviors, vigorous and mutual play, and essentially complete and lasting rehabilitation (Suomi, Harlow, & Novak 1974).

Related to the isolation studies was a research program on depression that occupied most of Harlow's attention in his last five years at Wisconsin, but that had really begun with the earlier mother-infant separation studies (e.g., Seay, Hansen, & Harlow 1962). The separation studies had disclosed a characteristic pattern of reactions by infant rhesus monkeys after removal from their mothers that consisted of a highly vocal, agitated protest phase followed by withdrawal and despair. In the despair phase, the infants became less active, and their social interactions, including play with agemates, declined sharply. The behavioral depression in these animals closely resembled the "anaclitic depression" described by René Spitz (1946) and others in human infants separated from their mothers. In the later depression studies, severe protest-and-despair reactions were observed in peer-reared infant monkeys when they were separated for four days from their playmates, and these reactions persisted over twenty weekly separations (Suomi, Harlow, & Domek 1970); long-lasting depressions were also produced in young monkeys when they were housed in stainless steel chambers that severely limited their visual exploration and vertical locomotion

(Suomi & Harlow 1969). By the time of Harlow's departure from Wisconsin in 1974, the research on depression was well advanced, with possible monkey models of human depression being examined in terms of neurochemical correlates and effects of antidepressant drugs, and of behavioral variables (McKinney, Suomi, & Harlow 1971).

Harlow's over-all research accomplishments exhibit not so much a theory or a "school," but a series of empirical discoveries, descriptions, and generalizations that are remarkable in number, scope, and impact. His greatest discovery, in a sense, may have been the rhesus monkey itself as an experimental subject, for he showed that virtually any topic of human behavior that might appear in an introductory psychology textbook could be profitably researched with monkeys, often producing results of seemingly undeniable applicability to humans. Here, of course, is where the main impact of Harlow's work lies—his findings demand the attention of anthropologists, sociologists, and psychiatrists, as well as psychologists—and where much controversy is centered. In hundreds of speeches and articles, Harlow has boldly generalized from monkey to man (or vice versa; see Harlow, Gluck, & Suomi 1972), and at times, as when he has presented evidence that sex-typing of behavior in monkeys has a genetic rather than a learned-imitational basis, he has not exactly endeared himself to certain groups. (On at least two occasions, feminist groups demonstrated at his public lectures on this topic.) He has, however, indicated where limits to generalization from nonhuman to human animals may lie, and how these limits may differ from one behavioral category to another (Harlow, Gluck, & Suomi 1972).

Professional recognition and honors came to Harlow well before his researches on the affectional systems. In 1951 he was named to a twelve-year term as editor of the *Journal of Comparative and Physiological Psychology*. In the 1950s he was also awarded the Howard Crosby Warren medal by the Society of Experimental Psychologists, and was elected president of the American Psychological Association. Shortly thereafter he received that organization's distinguished scientific contribution award, was elected to the National Academy of Sciences and the National Academy of Arts and Sciences, and was twice named Sigma Xi National lecturer. In later years he was awarded the national medal of science (1967), the gold medal award of the American Psychological Foundation (1973),

and the International Kittay Scientific Foundation award (1975).

JOHN W. DAVENPORT

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HARROD, ROY F.

Roy Forbes Harrod was born in Norfolk, England, on February 13, 1900, and died there on March 8, 1978. He considered himself best qualified as an economist in the workings of the international monetary system; his most important contributions to economics, however, have been in the development of the theory of imperfect competition, the promulgation of Keynesian economic thought, and the formulation of a theory of macrodynamics. Despite his acknowledged expertise in the theory and practice of economics, Harrod maintained that his most important achievement was a contribution to philosophy—his attempt, in the *Foundations of Inductive Logic* (1956), to refute the skepticism of David Hume.

Born into a literary and artistic home in which good conversation was deemed a noble pursuit, Harrod found college life appealing. By the time he was thirty he had become a university lecturer, a member of the governing body of Oxford University (the Hebdomadal Council), and, effectively, the head of his college (senior censor of Christ Church). Eschewing the belief that universities were solely the providers of courses and examinations, he displayed a pen-

chant for the company of aesthetes who could discourse knowledgeably in the fields of arts and letters. In his early days at Christ Church he associated extensively with the undergraduate "Oxford Set" surrounding Harold Acton, and by dining regularly at the Cranium Club he maintained contacts with the "Bloomsbury Set" quite apart from his association with Keynes.

Although his education at Westminster School and New College, Oxford, had been in classics, philosophy, and modern history, Harrod, at age 22, was appointed to a lectureship in economics and modern history at Christ Church, Oxford. Before taking up this post, however, he chose to spend some time studying economics and, through a fortuitous introduction by Walter Runciman, was able to spend one term at King's College, Cambridge, under the tutelage of John Maynard Keynes. At Cambridge he came into contact with a brilliant circle of students (including Frank P. Ramsey, Richard Braithwaite, Adrian Bishop, and Steven Runciman) and also with Keynes's flourishing Political Economy Club. Apart from a few months spent studying in Germany in 1923, this was his only formal education in economics, but it made a lasting impression on him. Greatly impressed by the theoretical knowledge and practical expertise of the members of the Political Economy Club, Harrod determined never to retreat from the problems of the world. Though he rejected pragmatism as a philosophy for action, he remained a realist, and all his economic theories had to be tested for the realism of their assumptions.

His academic output was immense. In one nine-year period (1951-1959) when his "stamp collector's urge to be inclusive" allowed a count of his publications, Harrod published seven books and 349 signed articles—a count that did not include the numerous unsigned memoranda he submitted to the European League for Economic Cooperation, his anonymous book reviews for *The Times Literary Supplement*, and his monthly letters circulated by a firm of stockbrokers. Somehow this prodigious output left time for him to be coeditor of the *Economic Journal* and an economic adviser at both the national and international level.

Harrod was a Keynesian economist who believed in free trade, but his loyalty to Keynes came first, and he strongly opposed what he termed "Cobdenite laissez-faire." These beliefs often placed him in the role of a political gadfly, but he played the role with the same energy he

applied to his academic work. For most of his life he was a member of the Liberal party (being in the Liberal Shadow Cabinet and running, unsuccessfully, for Parliament in 1945). In the 1930s he spoke on Labor party platforms (against the deflationist policies of the National government), and ultimately he joined the Conservative party. During World War II he served under Lord Cherwell on Winston Churchill's private statistical staff, at first in the Admiralty and later in the Prime Minister's office.

Imperfect competition. Harrod's interest in imperfect competition stemmed from his concern for the macroeconomic aspects of the expansionist policies proposed by Keynes in the 1920s. Keynes was advocating public works as a solution to unemployment, but a strong opposition developed based on the notion that any such increase in demand would lead to increased prices. This view was greatly bolstered by the publication of Piero Sraffa's famous article (1926) claiming that only in the presence of monopoly was equilibrium compatible with decreasing costs of production. This conclusion seemed wrong in the light of Harrod's practical knowledge of industry. Conducting his own survey of business, he found that even businessmen in competitive industries believed that increases in demand might lead to reductions in prices. Harrod's findings led him into a research of the pricing policies of industry, a research that resulted in many significant contributions to the theory of imperfect competition, the drawing of the marginal cost curve, the correct drawing of the long-run average cost curve as the envelope of the short-run average cost curves, and a solution of the duopoly problem.

It is common to attribute the discovery of the marginal revenue curve to several people working independently in the 1930s (see Robinson 1933), but Harrod could claim some priority in the matter, inasmuch as the publication of his article "Notes on Supply" (1930) had been delayed for two years because of a misunderstanding by the referee, Frank Ramsey. Harrod had originally called the curve "the increment of aggregate demand," but it was later renamed at the suggestion of Austin Robinson. The mathematical concept had been known to at least Antoine Augustin Cournot and to Alfred Marshall before him (see Shackle 1967), but it was Harrod who first drew the familiar textbook diagram that locates profit-maximizing output at the intersection of the marginal revenue and marginal cost curves.

The marginal revenue curve and the long-run

average cost curve were the basic tools used in the discussion of imperfect and monopolistic competition, and Harrod was at first optimistic of the uses to which the concepts had been put (1934). He was greatly impressed by the way it could be shown that with free entry under conditions of imperfect competition, equilibrium output would be at the point of tangency of the demand curve and the short-run and long-run average cost curves. At this point the plant would be at less than optimum size, and would not be used to optimum capacity. In addition, if optimum conditions of production were achieved under imperfect competition, profits would have to be above the standard level.

Harrod's optimism, however, gave way to a feeling that there was something wrong with a theory that had much of modern industry operating either under conditions of supernormal profit or under a tendency to build up an excessive number of businesses. In what Edward H. Chamberlin chose to call a "recantation," but Harrod preferred to call an extension of the theory ("Theory of Imperfect Competition Revised," in 1952*a*), Harrod proceeded to revise the basic concepts of imperfect competition, under the influence of the famous "Oxford Interviews" of businessmen in which he had participated. Harrod thought it would be irrational for firms to charge prices high enough to attract others into the industry to compete away their future profit. When maximizing profit, a firm would therefore take into account the present value of future receipts, so that output would be determined where the average cost curve was tangential to what he called the "average net proceeds curve." At this output the demand curve would intersect the average cost curve from above, and marginal revenue would be below marginal cost (see "Increasing Returns," in Kuenne 1967).

Keynes's *General Theory*. In 1935 the first draft of Keynes's *General Theory of Employment, Interest and Money* (1936), which had been written with "the constant advice and constructive criticism" of R. F. Kahn and Joan Robinson, was circulated for comments to Ralph G. Hawtrey, Dennis H. Robertson, and Harrod. Harrod immediately recognized the book as an important contribution but tried to temper Keynes's attack on classical economics. To his dismay, Keynes replied that Harrod must be "preoccupied with the old beliefs" and "only half shifted away from (the Classical School)." However, Harrod's response, stating what he thought to be the essentials of the *General Theory*, so im-

pressed Keynes that he absolved Harrod from all misunderstanding. Harrod became an official exponent of Keynes's thought. A diagram used by Harrod was appropriated by Keynes and stands as the only diagram in the final version of the *General Theory*. With Keynes's blessing, Harrod published "Mr. Keynes and Traditional Theory" (1937), a clear exposition of the differences between classical and Keynesian doctrine, and he also received the imprimatur of the master on his own extensions into dynamics. For the next ten years Harrod became so identified with Keynes and Keynesian thought that, even though an Oxford man, he was chosen as Keynes's biographer (1951*b*).

Macrodynamics. Even before the *General Theory* was published, Harrod recognized Keynes as the master of macrostatics—the investigation of the way the components of aggregate demand affect the utilization of productive resources. He realized from the beginning, however, that Keynes had no systematic theory of macrodynamics—the investigation of the determinants of the *rates of change* of the components of aggregate demand. Harrod's earliest attempts to fill the lacuna resulted in a theory of the trade cycle (1936) derived from a combination of J. M. Clark's acceleration principle (1917), referred to by Harrod as "the relation," and Keynes's multiplier. This same concept was later to be elaborated and refined by Paul Samuelson (1939) and J. R. Hicks (1950).

Within three years Harrod's views on the matter had matured considerably, and he presented a paper on the fundamental equations of economic growth (1939). These relationships were independently discovered much later by Evsey Domar (1946) and, as the Harrod-Domar relationships, laid the foundation for modern growth theory.

Harrod defined the rate of growth of income ($G = \Delta Y/Y$) as equal to the saving ratio ($s = S/Y$) divided by the incremental capital output ratio ($C = \Delta K/\Delta Y$). Thus $G = s/C$ is the dynamic version of the familiar static equality of saving (S) and investment ($I = \Delta K$). From this basic equation Harrod derived two normative growth rates: the warranted rate (G_w) and the natural rate (G_n):

$$(1) \quad G_w = S_d/C_r$$

$$(2) \quad G_n = S_\sigma/C_r$$

The warranted rate is derived when the saving ratio is s_d , the fraction of income that people currently wish to save, and the incremental

capital output ratio is C_r , the amount of additions to fixed capital and inventories that suppliers consider convenient in relation to the current or projected change in output. The saving ratio necessarily includes government saving, but the government's saving must be counted net of any saving or dissaving that is done to regulate the economy's output level. Given that this equation has savers content with their saving and investors content with their investing, the warranted rate of growth would obtain in a régime of laissez-faire capitalism.

Though there has been some argument as to whether the warranted rate can be called an equilibrium (see Alexander 1950), Harrod maintained only that, without some management the warranted rate would be unstable and that this instability was the fundamental explanation of the business cycle. The instability is clear from the definitions of G and G_w . If $G > G_w$, then $s > s_d$, $C < C_r$, or both, and the actual growth rate will move further away from the warranted rate. However, Harrod protested the notion that the instability was so extreme that the term "knife-edge" could be used. He believed that the instability principle would come into play only in cases of extreme deviations in s and C .

The natural rate of growth (G_n) is the social optimum rate of growth derived from the rate of increase in the working population (assuming full employment) and the rate of improvement in available technology. Thus equation (2) may be used to derive the socially optimal rate of savings (S_σ) to be brought about by monetary and fiscal policy. Emphasis was placed, however, on a *combination* of monetary and fiscal policy, inasmuch as large changes in private saving could be achieved only through monetary policy if there were excessive changes in the interest rate.

Once the place of the incremental capital output ratio in the fundamental growth equations is established, the relationship of technological progress to growth becomes clear. Using this relationship, Harrod was able to define a neutral stream of inventions as one that, for a given rate of interest, requires a rate of increase of capital equal to the rate of increase of the income to which it gives rise. If a stream of inventions requires capital to grow faster than income, then that stream is labor-saving or capital-requiring. If a stream of inventions requires capital to grow slower than income, then it is capital-saving or labor-requiring.

These definitions differ from those proposed

by Hicks (1932), where neutrality is defined as a stream of inventions that changes the marginal productivity of labor and capital in equal proportions. The differences in categorization may not be great, but Harrod claimed superiority for his method on the grounds that his classifications depended on the intrinsic character of the invention, whereas Hicks's depended on circumstances unrelated to the invention itself.

IVAN C. JOHNSON

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HAYEK, FRIEDRICH A. VON

Friedrich August von Hayek's work encompasses, besides economic theory and policy, the methodology of science; social, legal, and political philosophy; psychology; and the history of ideas. He has been a main protagonist of liberalism in the sense now called libertarianism, and a leader of the fourth generation of the Austrian school of economics.

Family background and education. Hayek was born on May 8, 1899, in Vienna, Austria, the son of August von Hayek, a senior municipal health officer who also taught biology (plant geography) at the University of Vienna, and of Felicitas, née von Juraschek, a daughter of a

professor of public law at the university who also was president of the Austrian Central Statistical Authority. In March 1917, Hayek was called to military service and became an artillery officer on the Italian front. In November 1918 he returned to Vienna and registered at the university to study law.

At that time, economics was taught only as part of the law curriculum. Hayek had earlier developed some interest in this field, had for a while been attracted by some moderate socialists (Karl Renner, Walter Rathenau), and even read a textbook on economics while in the army. At the university, he attended the lectures of Friedrich von Wieser and Othmar Spann but was more interested in methodology—inspired by reading Ernst Mach and Max Weber—and in psychology; indeed, in 1920 Hayek wrote a draft of a paper on psychology to which he returned 32 years later with a book on *The Sensory Order* (1952*b*).

Upon his return to civilian life, Hayek founded, together with some friends, the Association of Democratic Students—his first action in his life-long battle against the forces of nationalism and socialism. In November 1921, he obtained the degree of DR. JURIS and started to work in the war-claims settlement office, directed by Ludwig von Mises. At the same time, he continued to study at the university for a second doctorate, in economics. He wrote a dissertation on the theory of imputation and in March 1923 obtained the degree of DR. RERUM POLITICARUM.

Immediately afterwards Hayek took a leave of absence from his job in order to continue his studies of economics in the United States. He worked as a research assistant to Jeremiah W. Jenks of New York University, audited lectures and seminars of Wesley C. Mitchell and John Bates Clark at Columbia University, and assisted Willard Thorp with material for *Business Annals*. He returned to Vienna in May 1924, resumed his government work, finished articles on the incompatibility of stable price levels and fixed exchange rates (1924), American monetary policy (1925), and imputation (1926*a*), and worked on the concept of neutral money. In 1926 he married Helene von Fritsch; the couple had two children, Christine and Lorenz.

Vienna, 1924–1931. In 1921, still before his first university degree, Hayek had helped to found a circle of young scholars, whose influence on his thinking he often acknowledged. The group included not only economists (such as Gottfried Haberler, Fritz Machlup, Oskar

Morgenstern) but also social philosophers (such as Alfred Schutz), some members of the “Vienna Circle” of logical positivists, sociologists, historians, art historians, musicologists, literary critics, and lawyers (including Herbert Fürth, cofounder of the group). Of the twenty-odd members of the circle, many emigrated later to the United States and some of them exerted a strong influence on social science and philosophy.

In 1924, von Mises invited Hayek to join his private seminar, probably the most important center of economic discussion in Austria. It was a selected group of postdoctoral economists and methodologists, including many members of Hayek’s “circle,” all of them engaged in professional or avocational research. Its members later formed the core of the Nationalökonomische Gesellschaft (Economic Society), which was founded in 1927 at the initiative of von Mises and Hayek. The society met two or three times a month and many foreign visitors presented important papers.

Both Hayek and von Mises had seen, on their visits to the United States, the new empirical research programs on industrial fluctuations. Von Mises persuaded Austrian financial, industrial, and labor organizations as well as the government to join in the establishment and funding of the Austrian Institute for Business Cycle Research, with Hayek as its director. It started its monthly reports in 1927; after three years, with the help of an American grant, Hayek obtained Oskar Morgenstern as collaborator.

Von Mises, as Hayek’s patron and superior—first in the government office, then in the research institute—as his fellow officer in the Economic Society, and as discussion partner in the von Mises circle, could not help influencing Hayek’s thought and work. But far beyond these institutionalized associations go the intellectual ties of shared interests in specific economic and philosophical issues: the theory of money and the trade cycle, the problem of economic calculation under socialism, and the foundations of classical liberalism. These had been the major subjects of von Mises’ research and they became the foci of Hayek’s work. Three other economists were among Hayek’s closest associates in his Vienna period: Haberler, Morgenstern, and Machlup.

In 1929 Hayek submitted his book, *Geldtheorie und Konjunkturtheorie* as *habilitation* to the University of Vienna and was admitted as *Privatdozent* (lecturer). His trial lecture on “The

Paradox of Saving" (1929c) came to the attention of Lionel Robbins, who invited Hayek to present four lectures at the London School of Economics. These lectures on *Prices and Production* (1931b) led to the offer of a visiting professorship, which was followed by a regular appointment to the Tooke professorship of economic science and statistics at the University of London.

London, 1932–1950. Hayek's tenure at London began with a dramatic controversy with John Maynard Keynes. Hayek had been asked by Robbins to review Keynes's *Treatise on Money* (1930) for *Economica* (1931–1932), and Keynes had asked Piero Sraffa to review Hayek's *Prices and Production* for the *Economic Journal* (1932a). Keynes replied (1931) to Hayek's review, Hayek wrote a rejoinder to Keynes (1931c) and a reply to Sraffa (1932c), and Sraffa wrote a rejoinder to Hayek (1932b). The controversy was joined by Ralph G. Hawtrey, Arthur C. Pigou, Dennis H. Robertson, Arthur Marget, Alvin Hansen, and others in notes, reviews, and replies. In addition, Hayek published, between 1932 and 1937, ten articles on such controversial issues as the pure theory of money, the relation between saving and investment, the formation and maintenance of capital, and the causes of industrial fluctuations. The "drama," as John Hicks called it (1967), ended with a majority decision of the profession in favor of the modified expansionist views of Keynes, as expressed in his *General Theory of Employment, Interest and Money* (1936), which fitted the times of deflation and mass unemployment better than Hayek's monetary temperance did.

Although Hayek continued to work on the theory of capital, leading to his books on *Profits, Interest and Investment* (1939c) and *The Pure Theory of Capital* (1941b), he devoted increasing amounts of effort to philosophical problems and intellectual history. In 1935 he edited a volume on *Collectivist Economic Planning* for which he wrote two essays. In 1937 appeared his presidential address to the London Economic Club on "Economics and Knowledge"; in 1940, "Socialist Calculation: The Competitive 'Solution'"; in 1941, "The Counter-revolution of Science," largely an analysis of the teachings of Saint-Simon and Auguste Comte; in 1942–1944, in three parts, "Scientism and the Study of Society"; and in 1943, "The Facts of the Social Sciences." In 1944 Hayek published his bestseller, *The Road to Serfdom*, a severe con-

demnation of socialism in all its forms. The book, dedicated to "the socialists of all parties," was lavishly praised and roundly panned, and often distorted by admirers as well as critics.

In October 1940, the London School of Economics (L.S.E.) was evacuated to Cambridge, which afforded Hayek closer relationships with economists at Cambridge—including Keynes—when they were not involved in wartime duties.

In 1941 Hayek was awarded the degree of D.Sc. (Econ.) of the University of London. His closest associates at the London School of Economics were Lionel Robbins, Arnold Plant, and Karl R. Popper. Robbins was a faithful friend, an invaluable critic, and a treasure house of knowledge on the history of doctrines. Popper, the Austrian-born philosopher, was helpful in destroying the myth of the so-called scientific method in empirical disciplines. Plant earned Hayek's gratitude as the man who guided him towards rediscovering David Hume, who "not only laid . . . the foundation of the liberal theory of law, but . . . also provided an interpretation of English history as the gradual emergence of the Rule of Law" (Hayek 1978a, p. 124). Besides Hume, the voices of the past that seem to have had the most profound influence on Hayek's thinking on society, law, and liberty were John Locke, Bernard Mandeville, Adam Smith, Edmund Burke, Alexis de Tocqueville, Lord Acton, and Albert Venn Dicey.

In 1945 Hayek published an article on "The Use of Knowledge in Society," another fundamental disquisition on the division of knowledge in society. In a pamphlet on *Individualism: True and False* (1946a) he contrasted a voluntaristic, spontaneous, undesigned order of freedom of the individual with a rationalistically designed and constructed system. "True" was the undesigned individualism described and esteemed by Hume, Smith, Burke, Acton, and de Tocqueville; "false" the individualism designed and promoted by the encyclopedists, Jean Jacques Rousseau, and the Physiocrats. Most of Hayek's essays completed in these years were collected in a volume on *Individualism and Economic Order* (1948a).

In his last five years at the L.S.E., Hayek made several foreign trips, mostly to the United States. The success of *The Road to Serfdom* led to a lecture tour in the spring of 1945. He came again in 1946, to lecture at the University of Chicago in April and May and at Stanford and in Mexico during the summer. In

April 1947, Hayek convened a group of 39 liberal thinkers in a conference on Mont Pélérin, near Vevey, Switzerland. (The participants—economists, philosophers, jurists, historians, political scientists, literary critics, and publicists—founded the Mont Pélérin Society. Hayek served as president for more than 12 years, and as honorary president since 1960. This exclusive society has admitted some 400 members from 33 countries.) In 1948 Hayek spent the spring in Chicago and the summer at the University of Vienna; in 1950, January to March in Chicago and April at the University of Arkansas in Fayetteville. It was then that he obtained a divorce from his first wife to marry his Viennese cousin and childhood sweetheart, Helene Warhanek, née Bitterlich, and resigned from the L.S.E. to assume a professorship at the University of Chicago.

Chicago, 1950–1962. At the University of Chicago Hayek was professor of social and moral sciences and a member of the committee on social thought. He offered courses in the department of economics, but his major function was to conduct a seminar, mostly on intellectual history. It was attended by senior specialists in various fields, including physics, classical and modern literature, art, archeology, history, sociology, as well as economics (Letwin 1976). His closest associates were John V. Nef, Frank H. Knight, Aaron Director, George Stigler, and Milton Friedman. The first book Hayek published in this period was the product of research done previously in England and in America, *John Stuart Mill and Harriet Taylor: Their Friendship and Subsequent Marriage* (1951*b*). It was an “impartial presentation of documents,” virtually all letters they exchanged during the first 18 years of their friendship and some after her first husband’s death in 1849 and their marriage in 1851. Hayek had himself followed Mill’s itinerary on a journey through Italy and Greece, as described in Mill’s letters. Three other books followed in quick succession: *The Counter-revolution of Science* (1952*a*) containing his earlier articles; *The Sensory Order* (1952*b*), his tract in psychology elaborating his draft paper of 1920; and *Capitalism and the Historians* (1954), a collection, edited and introduced by Hayek, of essays by economic historians exposing the anticapitalist bias of much historical research. Important new articles and papers in epistemology, methodology, and political philosophy were completed in subsequent years

(1955*a*; 1956; 1958; 1963*c*). The chief work of Hayek’s Chicago period was *The Constitution of Liberty* (1960), an ambitious “restatement of the basic principles of a philosophy of freedom” (p. 3).

After 12 productive years in Chicago, Hayek returned to Europe in 1962 as professor of economic policy at the University of Freiburg im Breisgau, in Western Germany.

Freiburg, 1962–1968. Hayek’s inaugural lecture at Freiburg, on “The Economy, Science and Politics” (1963*a*), was a very personal statement of his scientific and political philosophy. He paid his respects to the late Walter Eucken, the eminent representative of libertarian economics, who had taught at Freiburg for many years, and to Max Weber, the influential social scientist who had forcefully explained the role of value judgments in academic teaching.

During his 6 years at Freiburg, Hayek published 2 books, 5 pamphlets, and 24 articles, not counting numerous reproductions and translations. Of the articles, 17 were collected in a volume of *Freiburger Studien* (1969*a*), and 6 of these were also included in an English volume of 29 collected papers, *Studies in Philosophy, Politics and Economics* (1967*b*).

In Freiburg, the Austrian Erich Streissler (later at Vienna) was Hayek’s closest associate. Both the university and the Walter Eucken Institute gave technical assistance to Hayek’s publishing activities.

Salzburg, 1968–1977. Professor emeritus of Freiburg University in 1968, Hayek accepted an invitation to the University of Salzburg. His inaugural lecture, on “The Errors of Constructivism,” was published (1970). The move to Salzburg proved rather unsatisfactory. The university had no degree program in economics, and few law students had serious interests in either economics or political philosophy. Thus he was cut off from the scholars who had everywhere been his stimulating and sympathetic discussion partners. This, combined with ill health, delayed completion of his ambitious three-volume work on *Law, Legislation and Liberty*. The first volume, *Rules and Order*, appeared in 1973, the second, *The Mirage of Social Justice*, in 1976; and the third, *The Political Order of a Free Society*, is expected in 1979.

The biggest event during Hayek’s Salzburg period was the award of the Nobel Prize for economic science, shared with Gunnar Myrdal. His Nobel lecture on “The Pretence of Knowl-

edge" (1975) was a fervent condemnation of the propensity of economists to predict on the basis of too limited knowledge macroeconomic results of expansionary policies. Hayek's prescription for ending the ongoing inflations of moneys and prices was to terminate the governments' monopolies in the creation of money, a *Denationalisation of Money* (1976). The Nobel award led to such a flood of invitations that even selective acceptances put Hayek "on the road" for large portions of the years, traveling to all continents.

Back to Freiburg, 1977. Disappointed with his working conditions in Salzburg, Hayek returned to live in Freiburg. Another volume of collected essays, *New Studies in Philosophy, Politics, Economics and the History of Ideas* (1978a), contains six pieces previously published in German in the *Freiburger Studien* (1969a), and other papers, including his Salzburg inaugural lecture and several contributions to intellectual history. In 1978, he also completed the third and last volume of *Law, Legislation and Liberty* with an epilogue on "The Three Sources of Human Values," which he regards as his intellectual last will and testament.

Honors and awards. Besides his three earned doctorates, Hayek received honorary doctorates from Rikkyo University (Tokyo, 1964), University of Salzburg (1974), University of Dallas (1975), Marroquin University (Guatemala, 1977), Santa Maria University of Valparaiso (Chile, 1977), and University of Buenos Aires (Argentina, 1977); he also was made honorary senator of the University of Vienna (1971). He became fellow of the British Academy (1945), honorary member of the Austrian Academy of Sciences (1976), honorary fellow of the London School of Economics (1976), fellow of the Econometric Society (1947), foreign honorary member of the American Economic Association (1976), and honorary fellow of the Argentine Academy of Economic Science (1977) and of the Academia Sinica (Taipei, 1969). After receiving the Nobel Prize in economic science in 1974, he was decorated with the Austrian medal of honor for science and art (1976) and the German order *pour le mérite* for science and arts (1977).

Highlights of Hayek's work

With a publication record of 185 titles—18 volumes of sole authorship, 10 volumes edited and/or introduced, 15 pamphlets, and 142 articles in journals and chapters in collective books (not counting new editions and translations)—it

would be impossible to give a digest of Hayek's ideas in a short space. Some highlights, however, may be noted.

Money, prices, investments, and fluctuations. Preoccupation with price levels and disregard of relative prices obscure the effects of money creation upon investment and the structure of production. Stability of the price level does not prevent credit expansion from "distorting" prices and production (1925; 1929b; 1931b). A rate of investment accelerated through monetary stimulus is not sustainable and the inevitable retrenchment generates unemployment of both labor and specific capital goods. Thus, monetary causes can lead to structural disturbances (1929b). The natural rate of interest (Wicksell 1898) is not the same as the rate that stabilizes the price level when physical output increases and increased demand for credit is met by a supply of loanable and investible funds in excess of voluntary saving (1931b). Changes in the ratio of demand for investment goods and consumer goods can cause prosperity and depression. Since the depression is usually associated with an induced reduction in investment, an additional extension of consumption is not the appropriate remedy; it may reduce employment even further (1931b).

Capital theory. Internal rates of interest are reflected in the margins between the costs of inputs and the prices of (intermediate) outputs in various stages of production. An increase in interest rates will be indicative of a need to widen the margins between costs and prices, and the production stages requiring relatively much capital become unprofitable (1931b). This is equivalent to a "shortening" of the period of production or investment (Böhm-Bawerk). Hayek's *Pure Theory of Capital* (1941b) provides lucid expositions of his notions of "inter-temporal equilibrium," the "physical productivity of investment," and the "vertical or successive division of labor" (pp. 72–73). Although Hayek rejects the concept of a "supply of capital" as a measurable quantity (p. 147), he derives a meaningful "marginal productivity of investment" (p. 179).

The popular notion that an expansion of consumer demand will always increase demand for and production of investment goods is shown to be a fallacy. Under conditions of full employment it is obvious that either of the two departments of production can increase only at the expense of the other. However, when wage rates behave as if there were full employment—

i.e., when they are raised whenever total demand is expanded—the same condition holds: an increase in consumption will reduce real investment (1969c, pp. 284–285).

Socialism, planning, and competitive capitalism. Hayek added important arguments to the problem of the possibility of rational economic calculation in socialist central planning (1935a; 1940). Although he had in 1935 anticipated the so called “competitive solution,” or “market socialism,” Oskar Lange and Henry D. Dickinson actually proposed this “solution” in 1937 and 1939. This prompted Hayek to explain why the required decentralization of decision making would be too complicated, and too unorthodox to be acceptable to hard-line socialists (1940). (One may hold that the Czechoslovakian events of 1968 proved this hypothesis.)

More fundamental was Hayek’s realization of the role of the “division of knowledge” in economic society (1937a; 1945b), not of scientific or technological knowledge but of the unorganized “knowledge of the particular circumstances of time and place.” Practically every individual “possesses unique information of which beneficial use might be made” (1945b, p. 521) but which “cannot be conveyed to any central authority in statistical form”; decisions based on such knowledge must be “left to the ‘man on the spot’” (p. 524). The problem is not that a unique rational solution *could* be derived from a complete set of data but “how a solution is produced by the interaction of people each of whom possesses only partial knowledge” (p. 530). The price system is the “mechanism for communicating information.”

As to the organization of competitive capitalism, Hayek is not impressed with the modern theories of imperfect and oligopolistic competition. Important are only the institutional and contractual obstacles to competition as a dynamic process, restrictions on entry and on attempts to “discover new ways of doing things better than they have been done before” (1948a, p. 101). He condemns the “mechanical extension of the property concept by lawyers” to non-tangible things such as inventions (p. 114). In a lecture on “Competition as a Discovery Procedure” (1968b), Hayek holds that competition is important “as a process of exploration in which prospectors search for unused opportunities that, when discovered, can also be used by others” (p. 188). Competition produces “a kind of impersonal compulsion . . . for numerous individuals to adjust their way of life in a man-

ner that no deliberate instructions or commands could bring about” (p. 189).

Liberty and the law. Hayek showed the conceptual and empirical links between an economic system based on free markets and a political system based on “liberty under law.” The latter rules out coercion and arbitrariness; the former, i.e., the impersonal mechanism of market prices, communicates dispersed knowledge to masses of free individuals acting and reacting to it without commands. These ideas were gradually refined, starting from the essays on the division of knowledge in society (1937a; 1945b) and a pamphlet on *Freedom and the Economic System* (1939a), continuing with the books *Road to Serfdom* (1944b) and *Constitution of Liberty* (1960), and culminating in *Law, Legislation and Liberty* (1973–1979). There he showed the fundamental contradiction between the idea of constitutionalism—“limited government”—and the [mis]conception of a democracy “where the will of the majority on any particular matter is unlimited” (1973–1979, vol. 1, p. 1); he emphasized the difference between the rules of a spontaneous order and the rules of organization (p. 48), i.e., between *cosmos* (“the law of liberty”) and *taxis* (“the law of legislation”).

Of all of Hayek’s pronouncements the one that most flagrantly negates the tenets of dominant schools of social philosophy is his claim that in a free society the concept of “social justice” is void of meaning. It may have meaning in a “command economy”; but where people are free, guided only “by rules of just individual conduct,” the notion of social justice—epitomized by a call for equality of incomes—is meaningless (1973–1979, vol. 2, pp. 62–100).

Philosophy of science. Hayek’s condemnation of “scientism” (1942–1944) was originally a protest against the fallacy of regarding certain procedures of the natural sciences as the only “scientific method.” Later he learned from Popper ([1935] 1959) “that natural scientists did not really do what most of them not only told us that they did but also urged the representatives of other disciplines to imitate” (1967b, p. viii). Hayek emphasized the differences between the kinds of “facts” observed, described, and explained by the physical and the social sciences. The facts of the social sciences are “opinions—not opinions of the student of social phenomena, of course, but opinions of those whose actions produce his ‘object’”; moreover, we cannot “observe” these opinions directly “in the

minds of the people but [only] recognize [them] from what they do and say merely because we have ourselves a mind similar to theirs" (1942–1944, p. 279).

In his essay on "Degrees of Explanation" (1955a), Hayek distinguished positive and negative predictions, with those of disjunctive alternatives between the two. He stressed the difficulty, in as complex situations as in economics, of ascertaining "by observation the presence and specific arrangement of the multiplicity of factors which form the starting point of our deductive reasoning" (1955a, p. 216). He did not, however, deny the possibility of testing and falsifying propositions about such complex situations. Yet he stressed the importance of "orientation" where prediction is not possible, and of "cultivation" where control is beyond our capacity (p. 225).

Hayek warned of the consequences of excessive specialization:

The physicist who is only a physicist can still be a first-class physicist. . . . But . . . the economist who is only an economist is likely to become a nuisance if not a positive danger. The degree of abstraction which the theoretical disciplines in our field requires makes them at least as theoretical, if not more so, than any in the natural sciences. This, however is precisely the source of our difficulty. Not only is the individual concrete instance much more important to us than it is in the natural sciences, but the way from the theoretical construction to the explanation of the particular is also much longer. (1956, pp. 463–464)

In an important paper on "Rules, Perception and Intelligibility" (1963c) Hayek pointed to "rule-guided perception" and "rule-guided action," neither of which presupposes that we can state or describe the guiding rules. He elaborated the existence of subconscious rules that guide perception and defended the proposition that perception of the concrete presupposes an organizing capacity of the mind. Against the numerous believers in the primacy of the concrete, Hayek went back to the tenet of major philosophers and psychologists who recognized the primacy of the abstract (1969b).

History of ideas

As a dedicated historian of ideas Hayek presented the descendancy of virtually every major issue that figured in the development of his thoughts. Besides such incidental intellectual history, he wrote about the lives and works of Mandeville (1961; 1966), Cantillon (1931a),

Thornton (1939b), Ricardo (1950), John Stuart Mill (1942; 1943b; 1945a; 1951b), Macleod (1934d), Gossen (1932a), Carl Menger (1934b; 1934c; 1968d; 1972), Wieser (1926b; 1929a; 1968e), and Philippovich (1934f); also obituaries for Cannan (1935b), Strigl (1944a), Mitchell (1948b), and Leoni (1968a); he appraised schools of thought such as the École Polytechnique (1941a), the L.S.E. (1946b), and the Austrian School (1965; 1968c); noted some parallelisms in Comte and Hegel (1951a); examined the methodological position of Mach (1967a) and the philosophical and historical ideas of Hume (1955b; 1960; 1963b; 1967a). Hayek's writings on intellectual history are testimonies to his profound and assiduous scholarship.

Memorable traits noticeable in almost all of his writings are Hayek's chivalry and tolerance in criticism and polemics, and his modesty and humility not only in acknowledging the contributions of his intellectual forebears but also in arguing against the views of his intellectual opponents.

Fritz Machlup

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HEBB, D. O.

Donald Olding Hebb, the Canadian psychologist who spurred the mid-century resurgence of neuropsychological theory, was born in Chester, Nova Scotia, in 1904. He received his B.A. degree from Dalhousie University in 1925 and worked for several years as a teacher in Canada. Having developed an interest in psychology through reading Sigmund Freud, he completed

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an M.A. degree (1932) as a part-time student at McGill University, where he was influenced by the works of Ivan Pavlov, Wolfgang Köhler, and Karl S. Lashley. Deciding at the age of thirty on a career in physiological psychology, Hebb chose to study with Lashley, first at the University of Chicago, then at Harvard University, where he received his Ph.D. in 1936. Hebb's dissertation on the innate organization of visual perception included a comparison of rats partially blinded at maturity (through Lashley's cortical excision method) with rats reared in darkness. In its concerns and methods this study prefigured his later work.

Hebb's general orientation toward psychology has echoed that of his mentor, Lashley. Its main tenets are that psychology (including social psychology) is a biological science; that while psychological functions cannot be reduced to physiological functions, psychological ideas must demonstrably be compatible with physiological mechanisms; that the integration of functions is a more critical question than the localization of functions; and that organismic functions must be understood as shaped by both heredity and environment, both nature and nurture. However, new, distinctive problems and data that were to prove vital to Hebb's mature scholarship arose during two subsequent research appointments.

In 1937 Hebb became a research fellow at the Montreal Neurological Institute, where one of his tasks was to assess the effects of the excision of cortical tissue (by neurologist Wilder Penfield) on measured intelligence in human subjects. To his surprise, he discovered that removal of extensive portions of the frontal lobes of adult patients produced no decrease in Binet-type intelligence. Although this finding created little stir among psychologists, Hebb remained deeply puzzled by the fact that mature intelligence did not seem to depend closely on the amount of brain tissue.

This line of investigation also led Hebb into extensive psychometric research on the measurement of specific intellectual abilities. While serving as lecturer at Queen's University in Kingston, Ontario (1939-1942), he and N. W. Morton completed the development of the McGill Adult Comprehension Examination.

In 1942 Lashley became director of the Yerkes Laboratories of Primate Biology and recruited Hebb as a research fellow to undertake naturalistic studies of intelligence and temperament among captive chimpanzees. During this period

Hebb published a number of influential papers on the nature and study of emotions and intelligence in man and animal, a second enduring theme in his works.

Hebb's rise to eminence followed his appointment in 1947 as professor of psychology at McGill, where he assumed departmental chairmanship the following year and created a major research laboratory.

In 1949 his bold and creative *magnum opus*, *The Organization of Behavior*, appeared and immediately stimulated widespread scholarly interest. This work presented a comprehensive theory of behavior that was based as far as possible on the physiology of the nervous system; its central distinctive feature was a novel "physiological theory of thought." To Hebb, thought is psychologically a type of process that is not fully controlled by environmental stimulation, yet cooperates closely with that stimulation. Physiologically it is the transmission of excitation from sensory to motor cortex via association cortex.

The key concept in Hebb's theory of thought is the *cell assembly*, a closed system of recruited neurons within which neural activity can "reverberate" and thus continue after the initiating sensory event has ended. Such assembly activity is the simplest case of an image or idea. Any cell assembly will form neural connections with other assemblies through the mechanism of association, and may thus be activated by an associated assembly in the total absence of adequate sensory stimulus. Moreover, these representational activities of cell assemblies (each corresponding to some very specific property of environmental stimulation) will similarly form connecting links with, and tend to produce, particular motor activities.

Hebb's theory invokes a postulate of synaptic resistance to explain the formation of neural connections within and between cell assemblies: When an axon of cell A is near enough to excite a cell B and repeatedly or persistently takes part in firing it, some growth process or metabolic change takes place in one or both cells such that A's efficiency as one of the cells firing B is increased. As applied to the formation of a cell assembly (involving recruitment of a fairly large number of neurons), this postulate clearly implies that building up such numerous and complex interconnections is a very slow process, requiring many repetitions of the stimulating conditions. These conditions are quite specific and molecular, corresponding to dis-

crete perceptual elements, relations, and properties.

The very simple kinds of images or ideas constituted by cell assembly activity do not themselves go far toward explaining thought or behavior. Accordingly, a second key concept in Hebb's theory is the *phase sequence*,

a temporally integrated series of assembly activities [amounting] to one current in the stream of thought. Each assembly activity in the series might be aroused (1) sensorily, (2) by excitation from other assemblies, or (3) in both ways. It is assumed that the last, (3), is what usually happens in an organized flow of behavior. Each assembly must establish connections with a number of other assemblies, at different times; which of these others it will arouse on any specific occasion will depend on what other activity, and especially what sensory activity, is going on at that moment. Assembly A, tends to excite B, C, and D; sensory activity tends to excite D only, so A is followed by D. At each point in time, behavior would thus tend to be steered both sensorily and centrally, jointly controlled by the present sensory input and the immediately prior central activity. (1959, p. 629)

In Hebb's theory, attention (set, attitude, expectancy) is, physiologically, this influence of the preexisting central activity on the next link in the phase sequence. The motor response of an animal presented with a stimulus is in part selectively determined by excitation from cell assemblies already active.

Perception—of even a simple pattern—is the activation of a phase sequence integrating the perceptual elements, or cell assemblies, that correspond to the several parts of the pattern, through sequencing of the motor components of these assemblies into scanning behavior.

Hebb proposes that an animal's early learning involves a period of establishing perceptual elements, or cell assemblies, and a subsequent period of establishing perceptual and conceptual sequences, or phase sequences. The establishment of such neural structures, as noted above, is presumed to require many experiences of a particular, or of a closely similar, stimulus. Hebb regards this stage of primary learning as the period of establishing a first environmental control over the association areas of the cerebral cortex and thus, indirectly, over behavior. Among those animals in which the association areas are large, absolutely and relatively, primary learning will thus require a long period.

The characteristic learning of the mature

animal, on the other hand, is rapid and conceptual, seeming always to involve a recombination of familiar perceptions and familiar patterns of movement. Prompt learning—including insight—is possible only when stimulation sets off two or more well-organized phase sequences. Being organized, these phase sequences are capable of continued existence after stimulation ceases, thus providing time for the structural changes of permanent learning to take place. Further, the prompt learning of maturity does not involve the establishment of new connections but a selective reinforcement of connections already capable of functioning, based on association by conceptual similarity.

To Hebb, this contrast between types of learning has profound implications for the growth and decline of intelligence. He first distinguishes two meanings of "intelligence." The first is an innate potential, the capacity for behavioral development that resides in possession of a good brain and neural metabolism. The second meaning refers to the functioning of a brain already developing, hence in turn the average level of performance or comprehension of the partly grown or mature animal. Hebb then infers that an intact brain is vital to the first sense of intelligence but not necessarily to the second. The highly organized cell assemblies and, even more, the phase sequences of the adult are characterized by such levels of redundancy and overdetermination that a loss of neurons—for example through aging or brain damage—may not be seriously or permanently disruptive of familiar activities. The formation of new neural structures, however, is importantly a function of the number of potential connections, so that loss of cells does affect the capacity for development, or further development.

Motivation, in Hebb's theory, refers to (1) the existence of an organized phase sequence; (2) its direction or content; and (3) its persistence in a given direction—that is, its stability of content. Items (1) and (2) imply that the waking, normal adult animal always has some motivation, although its persistence in any one direction (3) is not always great. Pleasure is fundamentally a directed growth or development of a phase sequence, "a transient state of affairs in which a conflict is being reduced, an incipient disorganization being dissipated, or a new synthesis in assembly action being achieved" (1949, p. 232). Pain, on the other hand, is a cortically

disruptive activity, capable of channeling motivation by disrupting some phase sequences while facilitating others.

Emotional disturbance, too, is a disorganization of integrated central activities. Two or more phase sequences that are concurrently active may conflict in the sense that they may produce incoordinated behavior. For example, motor components belonging to one sequence may alternate randomly with those belonging to the other. Or one may inhibit the other, with the result that all motor outflow may be nullified in a paralysis of emotion.

Hebb's book marshalled an encyclopedic array of physiological and psychological data to lend impressive plausibility to his key conceptions and to the various theoretical implications drawn from them. Hebb's insistence on the central role of concepts in the behavior of animals (from rats to men), persuasively grounded in contemporary physiology, gave timely support to the growing interest in cognitive psychology and, among more traditional learning theorists, in mediating processes.

Almost immediately, however, new physiological data, particularly the discovery of the diffuse arousal system, and new psychological data, particularly the findings of sensory deprivation experiments conducted by Hebb's own laboratory, dictated significant reformulations of the theory with respect to specific relations among thought, motivation, and emotion. Over the next 25 years Hebb contributed heavily to reformulations of neuropsychological theory in a series of major papers and in successive revisions of his *Textbook of Psychology*, drawing particularly on the directed program of research of the McGill laboratory.

During this period, Hebb was increasingly concerned with the dependence of higher cognitive activities on a varied sensory environment. Animals reared in restricted environments during the primary learning stage were shown to be socially, motivationally, intellectually, and emotionally abnormal in adulthood. Normal mature animals subjected to short-term sensory deprivation were shown to develop temporary abnormalities in the same functional spheres. Hebb's interpretations of such findings contributed importantly to social psychology as well as to developmental and clinical psychology.

Hebb was elected president of the Canadian Psychological Association (1952) and the American Psychological Association (1959), and in

1961, received the APA's award for distinguished scientific contribution. He has received many scholarly honors from several nations.

After serving as chancellor at McGill from 1970-1974, Hebb retired near his birthplace in Nova Scotia, remaining professionally active after his formal retirement.

GEORGE J. MCCALL

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HEIDEGGER, MARTIN

Martin Heidegger was born in 1889 in Messkirch, southwest Germany. He showed an early religious inclination and studied Catholic theology and philosophy at the University of Freiburg, where he was deeply influenced by the eminent psychologist and Catholic philosopher Franz Brentano. After earning his doctorate, Heidegger was for many years closely associated with Edmund Husserl. He adapted Husserl's phenomenological method for his own purposes, but retained Husserl's sharp distinction between a purely phenomenological and a psychological or sociological study of man. He published his main work *Sein und Zeit* (Eng. trans. *Being and Time*) in 1927, but continued to write and publish all his life.

From his close study of the pre-Socratic philosophers, including Parmenides, Heraclitus, and others, Heidegger concluded that they had a profound insight into the special features of human existence, an insight that he attributed in part to the fact that their thinking did not yet basically separate poetry, religion, philosophy, and science from each other. If man were to regain an adequate understanding of himself and his situation in the world, this separation would somehow have to be overcome.

As a consequence, Heidegger largely rejected the terminology of professional philosophy, adopting an original style that requires special efforts to understand. Heidegger deplored this, but thought it necessary in order to erase preconceptions and avoid misinterpretation on the part of his readers.

He felt that Western man, in order to regain self-understanding and eliminate his profound alienation, had to seek deeper than did the political programs of liberalism and Marxism. Somehow the rebirth had to proceed through an awakening of the people ("*das Volk*") conceived along Germanic lines. This conviction explains in part why Heidegger in 1933-1934 gave speeches in favor of Hitler. He was, however, soon led to reject the possibility of cultural rebirth through National Socialism.

In 1945 Heidegger's association with National Socialism was investigated by a specially appointed tribunal. Not classed as an "activist," he was categorized among the "less responsible" and the "sympathizers," and he was permitted to retain his professional rights. He declared later that Hitlerism was a poison whose effect

would require a difficult and lengthy process to eliminate, and that it revealed a structural weakness in mankind as a whole.

Heidegger continued his work after the war until shortly before his death in 1976. He influenced European thinking profoundly, especially in the years of existentialism. In his later works his thought was deeply inspired by the German romantic poet Friedrich Hölderlin, the Danish existentialist Søren Kierkegaard, and by the critical genius of European culture, Friedrich Nietzsche.

The following formulations, written in part in his own kind of terminology, may offer some idea of Heidegger's philosophy: Man exists, stands out from things, in a sense projects, but he is nothing apart, he is no-thing. He finds himself thrown into the world as a being-there (*Da-sein*), always on the verge of being completely absorbed and of falling away (*Verfall, Aufgehen in*). Being absorbed, he is no one particular, nothing authentic, but "the they," *das Man*. However, through a mood of existential dread (*Angst*), an authentic, free way of being is disclosed (dis-closed) as a possibility. Man's finite existence is grasped as a continuous preparedness for his own death. All everydayness disappears when he faces death and no-thing-ness (*das Nichts*). He understands that as existing he is nowhere at home (*Un-heimlichkeit, Un-zu-hause*). But anxiety also reveals Being itself, as the light and the joyful (*das Heitere*). Man has the possibility of an active, knowing joy (*das wissende Heiterkeit*), a possibility of being the shepherd of Being (*das Hirt des Seins*).

Fundamentally Heidegger's basic question is what it means to be a human being. He tries to bring to light something that is so fundamental that it escapes notice and is hidden from both everyday and scientific consciousness. He invites the reader to ask with maximum engagement, and he does not intend to provide answers.

In his last writings, Heidegger's style is more poetic and oracular, retaining only traces of scientific phenomenology. He now presents himself as an interpreter of modern industrial society: Modern life relates to one and is only one way through which Being reveals itself. Man has at least temporarily lost contact with other possibilities and consequently suffers a profound estrangement from Being, as the source of those possibilities. The revealed world now is one of objects exploited technologically by sub-

jects. Nature has lost its dignity. Because man is basically the "shepherd of Being," his estrangement (alienation) is self-estrangement. The "path into the next world era" can be pointed out only by those who in the present "worldnight" are capable of seeing alternatives. Nature will reveal itself, with "earth, sky, mortals and gods" in an original unity. Poets like Hölderlin are best able to assist mankind in its present crisis.

Heidegger's interpretive approach, critical of Freud and stressing meaning relations, inspired philosophical anthropology and theological and psychiatric thinking. In psychiatry, Ludwig Binswanger used Heidegger's hermeneutic phenomenology in shaping an original conceptual and therapeutic approach. Ronald D. Laing and his school have been influenced through the mediation of Jean-Paul Sartre.

Hermeneutical approaches in cultural and social studies have been generally influenced by Heidegger, in part indirectly through his former students, notably Hans-Georg Gadamer. In the English-speaking world, his influence has, on the whole, been indirect, partly because of difficulties of translation. It can, however, be traced through the works of writers on the borderlines between philosophy and social science. One example is Herbert Marcuse's idea that "rationalization" in Max Weber's sense is also rationalization in Freud's sense of the term, and that scientific rationality is a historical phenomenon peculiar to technological society.

ARNE NAESS

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HEIDER, FRITZ

Fritz Heider was born in Vienna in 1896, spent his childhood in Graz, and received a classical education at the official state Gymnasium. Unable to serve in the army during World War I because of a childhood eye injury, he entered the University of Graz. He did not have a professional goal in mind, and in the European tradition, he attended a wide range of lectures open to all university students. His interests eventually centered on the study of philosophy and psychology.

As a student, Heider met Alexius Meinong, a widely influential and dominant figure in European philosophy, who suggested the topic of Heider's doctoral thesis with the question: Why do we say, we see the house and do not say, we see the sun? (The reflected rays of the sunlight are the stimuli which actually strike the eye.) In discussing this problem Heider distinguished between the "thing" or object and the "medium" or that in the environment which provides the information about the object, i.e., the mediators (1927). He carried this distinction into his later analysis of person perception. In Graz, Heider was also influenced by the psychologist Vittorio Benussi, who was one of the

first to conduct and publish experiments in the field of gestalt perception.

After Heider finished his dissertation, written during the winter months of 1919, he worked for a year as an applied psychologist, but soon became restless. He decided to go to Berlin where his uncle was a zoologist. Benussi suggested that while there Heider make contact with psychologists at the University of Berlin.

An air of excitement permeated the Berlin department of psychology. The lectures in perception, especially those of Max Wertheimer, were very popular among students and intellectuals from widely differing fields. There was a definite feeling that gestalt research and theory would have an important influence on the development of psychology. Heider attended the lectures of Wertheimer, as well as those of Wolfgang Köhler, and found many gestalt concepts, for instance laws governing "unit formation," useful in his later work on the perception of interpersonal relations. He also formed a lasting friendship with Kurt Lewin, a younger member of the Berlin faculty.

After Berlin came the *Wanderjahren*, a period when Heider spent a good deal of time reading philosophy, especially Spinoza and Nietzsche; psychology; and literature. Then, in 1927 he became assistant to William Stern at the University of Hamburg, and during this time came to know Heinz Werner and Ernst Cassirer; he learned a great deal from his contacts with all three. In 1930, he published "Die Leistung des Wahrnehmungs-systems," and took leave from Hamburg to accept what he thought of as a one-year position with the gestalt psychologist Kurt Koffka at Smith College in Northampton, Massachusetts. There his principal work was at the Clarke School for the Deaf, where Grace Moore, also a member of Koffka's research group, was working. They were married shortly after and Heider remained in the United States. In the years that followed he combined research at the school for the deaf with teaching at Smith College and published jointly with Grace Heider two monographs on the psychology of the deaf (1940; 1941). Observations that Heider made in attempting to understand the thought processes of children who were just beginning to learn speech inspired the film he later made with Marianne Simmel; findings based on this film were subsequently incorporated into his work on perception of personal causality. In his last year at Smith College, Heider was awarded a Guggenheim

fellowship to continue work on what was to become his widely read book on interpersonal relations.

In 1947 Heider went to the University of Kansas. Roger G. Barker, the new chairman of the psychology department, brought with him a group of people who, like himself, had been closely associated with Kurt Lewin. In the following years, Heider spent a semester at Cornell University, a year at the University of Oslo as Fulbright professor, and a year at Duke University as William Preston Few distinguished professor. In 1951 he received a second Guggenheim fellowship and in 1956 received a grant from the Ford Foundation which allowed him to complete his book *The Psychology of Interpersonal Relations* (1958). In 1959 he received the Lewin memorial award given by the Society for the Psychological Study of Social Issues; in 1963 he shared the Byron Caldwell Smith award given by the University of Kansas; and in 1965 he was also given the distinguished scientific contribution award by the American Psychological Association.

Heider's most influential contribution is *The Psychology of Interpersonal Relations*. In this work Heider applied the laws of perception of physical objects, developed by gestalt psychologists, to the perception and motivations of social "objects," or persons. Synthesizing object and person perception is one of Heider's important scientific achievements.

Two central and associated concerns that have been influential in the development of psychology were also articulated in *The Psychology of Interpersonal Relations*. One of these relates to the concept of balance. Heider reasoned that object configurations, including social objects, could be classified into those which are in a balanced (steady) or in an imbalanced (unsteady) state. Since balanced states are conceptually preferable to imbalanced states, new relations formed according to the principle of balance must be applied to any imbalanced structure. Furthermore, if an imbalanced state exists, there is a force to change it in the direction of balance. For example, in a two-person system, if a likes b and b likes a, the system is in balance. But if a likes b and b does not like a, there is disequilibrium. A force then exists in a to change the attitude of b, misperceive b's sentiments in a biased way, or change the attitude toward b. Configurations with three entities such as those involving two persons and an object are more complex. If, for example, a likes b and

object x (e.g., the elected president), then if b does not like x , the situation is imbalanced and a force will be created to bring the system into a state of equilibrium. In Heider's characteristic manner of integrating everyday observations, literary examples, and scientific analysis, he noted that many literary tragedies make use of balance principles (e.g., Romeo loves Juliet, Juliet loves and obeys her parents, but her parents do not accept Romeo). An example of "latent" imbalance which contributes to dramatic tension in Ibsen's *Wild Duck* occurs when a scrupulous husband loves his wife who, unbeknownst to him, has engaged in a dishonest act for his benefit.

Causal attributions, the second major focus of *The Psychology of Interpersonal Relations*, pertain to perceptions of causality, particularly the relations between actions or behavior and the motives and intentions of others. In a precursory experiment, Heider and Simmel showed experimental subjects three moving objects on a screen (a large and small triangle and a circle). They reported that interpretations of the film were almost always made in terms of interactions between persons with feelings, for instance impacts were interpreted as fights generated by jealousy, joint movements as evidence of liking and "belonging together," and so on. Thus, perceived causality organized perceptions and eliminated the distinction between physical and person perception.

Heider postulated that people are motivated to understand and to master their environment, partly because understanding is adaptive and instrumental to future behavior, but also because of a basic curiosity and desire "to know." Pursuing the distinction intimated in his thesis and in "Ding und Medium" (1927), Heider stated that we are often only in contact with immediate facts or raw data, but we search for the underlying core processes or dispositional properties to explain these facts. These more basic facts are the generally enduring aspects of the world. Heider suggested that in "naive analysis of action" the main causes of events are perceived as either internal to the person (ability and effort) or as external factors (particularly task characteristics). Attributions of causality to these disparate factors have far-reaching consequences. For example, if a person is enjoying his meal at a restaurant, it may make great differences in our behavior (e.g., deciding to dine at the restaurant) if the enjoyment is ascribed to his hunger or to the quality of the meal.

Scientific method. The sources of Heider's ideas were largely observational and literary; his method was often to begin by making "mental experiments" in which interpersonal situations were conceived and altered in a systematic manner (e.g., Romeo does not like Juliet, or Juliet does not care about her parents), and inferring the psychological consequences. The ideas generated in this way have led to further experiments, some by Heider himself, more by others, which in turn have stimulated him to further theorizing.

Scientific heritage. Since 1955, there have been two dominant paradigms in social psychology. The first was cognitive dissonance, a theory of cognitive dynamics proposed by Leon Festinger that was part of the general principle of balance. The second was (and remains) attribution theory, which received its impetus from the writings of Heider. Attribution appears to be the more robust of these paradigms. It dominates social psychology and has made inroads into the fields of motivation, personality, and clinical psychology. Part of the richness of attribution theory is to be attributed to Heider's personality. He has encouraged new directions and alternative paths, rather than dogmatically enforcing or insisting upon one particular set of ideas.

MARIJANA BENESH-WEINER
AND BERNARD WEINER

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HEMPEL, CARL G.

Carl Gustav Hempel was born in 1905 in Oranienburg, Germany. After studying mathematics and physics, he turned to philosophy and was associated with the logical empiricist movement then centered in Vienna and Berlin. He studied at the universities of Göttingen, Heidelberg, and Berlin, where he received his PH.D. in 1934. Hempel then left Germany, did research in Belgium until 1937, and then came to the United States, where he taught at Yale University from 1948 to 1955 and then at Princeton University. Since 1977 he has been a university professor at the University of Pittsburgh. He has made contributions to logic, philosophy of mathematics, and philosophy of science; in the latter area, he has done important work on concept formation, confirmation, and explanation. His own views, worked out with characteristic precision and attention to detail, lie within the tradition of logical empiricism, but he has also been one of that tradition's most perceptive internal critics. On many points his views are similar to those of Ernest Nagel (1961), whose intellectual roots lie closer to American pragmatism.

The logical empiricists are so-called because they share an empiricist attitude toward epistemological problems and the belief that modern mathematical logic is a tool which can be employed to good effect in the analysis of philosophical problems. Apart from Hempel himself, some prominent figures in this movement have been Rudolf Carnap, Richard von Mises, Otto Neurath, Karl Popper, and Moritz Schlick. The scientific interests of most of these philosophers

centered on logic, mathematics, and the physical sciences.

Neurath, who was trained in economics and sociology, was the only one among them who had specialized knowledge of the social sciences. He believed that human individuals and societies were nothing more than complex physical systems, and for this reason, he envisaged a reformulation of all of empirical science in a unitary physicalistic language.

Carnap, in turn, tried to work out the analytical details of this physicalistic program. Although he began by maintaining that all scientific terms were fully definable by means of physical, observational terms, he soon discovered that it was impossible to construct such a definition for each scientific term. So he proposed that many scientific terms should be regarded as theoretical. Carnap eventually came to believe that all laws of nature, including those which hold for people and their societies, are logical consequences of the physical laws which explain inorganic processes. He has always tended to stress the logical unity of empirical science, and the main thrust of his physicalism has been that all empirical science is in principle reducible to physics, either by translation or by logical derivation.

By contrast, Hempel's work in the philosophy of science has consistently emphasized the methodological unity of the empirical sciences. He is acutely aware of the diversity of subject matters that are amenable to scientific study, and he does not insist that all empirical sciences can be reduced to, or will eventually be replaced by, one fundamental discipline such as a perfected physics. He does hold, however, that all empirical sciences have important similarities of method. His studies of the logic of scientific explanation are attempts to formulate general models of explanation that will reveal some of these similarities. The use of these models to answer certain much disputed questions about methods of explanation in social science constitutes Hempel's main contribution to the philosophy of social science.

The concept of explanation outside of science is vague. Broadly speaking, to explain something to someone is to make it plain or understandable to that person. So explanations are person-relative in the sense that what counts as a satisfactory explanation for one person will not do so for another. A satisfactory explanation for a given person will depend upon his antece-

dent beliefs, intelligence, and other personal idiosyncrasies. But scientific explanations are supposed to be independent of such personal factors. So Hempel proposes to explicate the notion of scientific explanation by replacing the ordinary notion with one more precisely characterized. Whether or not such an explication is successful depends upon whether the more precise notion accounts for cases which are generally agreed to be paradigmatic instances of scientific explanation and also provides a useful tool for analyzing the logical and methodological procedures actually used in science. In Hempel's view, a condition of adequacy for a scientific explanation of a particular event is that it provides information which shows objectively (i.e., in a person-independent manner) that the event in question was to be expected. Like other philosophers in the empiricist tradition, Hempel holds that such information has been provided only when the event to be explained has been subsumed under general laws.

Since laws may be either strictly universal or statistical, there will be two basic patterns of explanation for particular events. The first, worked out by Hempel (1948) in collaboration with Paul Oppenheim, is *Deductive-Nomological* (D-N) explanation. In a D-N explanation, the explanandum is a sentence which describes the event to be explained. The explanans consists of two kinds of sentences. Some state specific antecedent or initial conditions; others express universal laws. Many explanations in classical physics exemplify this pattern perfectly. An adequate D-N explanation must satisfy four conditions: (1) the explanans must be true; (2) the explanandum must be logically deducible from it; (3) at least one law in the explanans must be required for the deduction; and (4) the explanans must in principle be capable of being empirically tested. The first two adequacy conditions insure that the explanans provides information which shows that the explanandum-event was to be expected; this is because a valid deductive argument with true premisses guarantees the truth of its conclusion. The third condition insures that the explanandum-event has really been subsumed under a law, and the fourth guarantees that the explanans is subject to confirmation or disconfirmation by observational evidence.

The second pattern, which Hempel (1962a; 1965) elaborated later, is *Inductive-Statistical* (I-S) explanation. In an I-S explanation, the

explanandum is a sentence which attributes to a particular thing a certain characteristic A. The explanans comprises both a sentence expressing a statistical law to the effect that the probability that a thing has A, given that it has some other characteristic B, is close to one, and a sentence stating that the thing in question has B. Some explanations in genetics, for example, conform well to this pattern. In an adequate I-S explanation, the explanans must be testable and true. Moreover, because each of two incompatible explananda can be rendered highly probable by different true I-S explanations if these are cleverly selective, an adequate I-S explanation must be based on all available information of potential explanatory relevance to the explanandum. Hence, the adequacy of an I-S explanation is epistemically relative and may alter as the available information alters. Because the relation between explanandum and explanans in an I-S explanation is not one of logical deducibility, the truth of the explanans does not guarantee the truth of the explanandum. But the explanans does provide strong inductive support for the explanandum and, thereby, shows that the explanandum-event was to be expected.

In addition to explanations of particular events, scientists also explain laws. A universal law may often be deduced from the basic principles of an even more general theory. Sometimes such deductions succeed in showing that the original lawlike sentence was only an approximation. On the other hand, a statistical law may be deduced from others by means of the mathematical theory of statistical probability.

Hempel has applied this general philosophical analysis of scientific explanation to four topics that concern social scientists: (1) the function of laws in historical explanation; (2) the function of ideal types in sociological explanation; (3) the role of reasons in the explanation of human action; and (4) the problem of functional explanation in sociology and anthropology. In each case he argues that proposed scientific explanations are successful only to the extent that they can be understood as conforming to one of the patterns he has described.

Hempel (1942) holds that explanation in history must, as elsewhere in the empirical sciences, involve subsumption under laws. To be sure, historians do other things besides explaining events. Such activities as description, classification, and simple narration are the legitimate

province of historians, but they differ in logical structure from explanation. Hempel is not committed to the view that there are sweeping laws of historical development as proposed by Oswald Spengler or Arnold J. Toynbee. He recognizes that the laws explaining historical events are usually borrowed from such disciplines as economics, psychology, and sociology, or are common-sense generalizations based on everyday experience. Obviously, historical explanations are seldom, if ever, proposed in a form that fits one of the patterns Hempel has described, because, he suggests, historians usually set forth only sketches of explanations, which provide no more than vague indications of the laws and initial conditions considered relevant to the explanandum. Such sketches would have to be expanded before Hempel would regard them as full-fledged explanations. Hempel also argues that empathetic understanding of events and people is not a method of historical explanation; rather it is a heuristic device for suggesting psychological hypotheses which might serve as explanatory principles.

According to Hempel (1965), the intent of constructions of ideal types is to provide explanatory theories. When fully elaborated, they should consist of both a list of characteristics with which the theory proposes to deal and a set of sentences which purport to express laws, universal or statistical, connecting these characteristics. Such connections by means of laws are nomological connections. Hempel carefully shows that his interpretation is consistent with much of what Max Weber and Howard Becker have said about ideal types. But he explicitly rejects Weber's claim that the nomological connections in such a theory must be subjectively meaningful because many events of interest to social scientists can only be explained by reference to factors that have no subjective meaning. Hempel suggests that such ideal constructs might eventually be incorporated, as idealized approximations, into a comprehensive theory of social action, if and when one is constructed. In addition, he warns that if the requirement of testability is to be satisfied by the nomological sentences of such a construct, their empirical domain of applicability must be clearly specified.

Explanation of action in terms of reasons must refer to at least the following factors: (1) the agent's goals; (2) the agent's beliefs about the means available for attaining those goals; and (3) the agent's commitments to normative constraints on actions. Suppose all such

factors are put together and thought of as making up the agent's situation. Some philosophers have held that a rational explanation for an action shows why the action was the appropriate one for the agent to take in his particular situation. Hempel (1962c; 1965) argues that this is not sufficient for scientific explanation because it provides no grounds for thinking that the agent did, in fact, do what was appropriate. He construes the explanans of a full-fledged explanation in terms of reasons as consisting of two parts: (1) a sentence expressing a nomological connection between, on the one hand, being rational and in a certain type of situation and, on the other, performing a certain kind of action; and (2) sentences stating that the agent was rational and was in a situation of the specified sort. In this interpretation, explaining actions by reasons also involves subsumption under laws. Hempel notes that the characteristic of being rational must in such explanations be understood, not normatively but descriptively, as a broadly dispositional trait. Otherwise the testability requirement cannot be satisfied. He distinguishes between actions that can be explained in terms of the specific aspects of the situation that the agent consciously considers in his deliberations and actions that can only be explained by reference to unconscious motivational factors. But he emphasizes that attributions of unconscious motives must be subject to control by empirical testing, and so he takes such motives to be linked to behavioral dispositions. For example, attributions of unconscious motives of the Freudian sort are subject to empirical testing just insofar as they are related to such behavior as slips of the tongue and reports of dreams in a fairly precise manner.

Hempel (1959) takes the object of a functional analysis to be some item which is a relatively persistent trait or disposition occurring in a larger system. For example, the beating of the heart is a persistent trait of the body of a living vertebrate. Hempel argues that the analysis aims to show that the system is such that under specified conditions the item in question has effects which satisfy a condition necessary for the system to remain in adequate or proper working order. For instance, when the body of a living vertebrate is in normal conditions, the beating of the heart has the effect of circulating the blood, and this contributes to the satisfaction of certain conditions, such as supply of nutriment and removal of waste, which are necessary for the proper working of the or-

ganism. This is the content of the claim that the beating of the heart in vertebrates has the function of circulating the blood. Hempel notes that an item may have many effects that are not among its functions. For example, the production of throbbing sounds is an effect of the beating of the heart in vertebrates that is not a function because it normally contributes nothing to keeping vertebrates in adequate working order.

Anthropologists such as Bronislaw Malinowski and A. R. Radcliffe-Brown have supposed functional analyses to have explanatory import. Hempel shows that although functional analyses do presuppose nomological connections, they typically do not explain the presence of an item in a system because the presence of the item under analysis is sufficient but usually not necessary for maintaining the system in working order. In most cases there are other items functionally equivalent to the one under analysis which might have been present instead. Hempel also points out that unless precise empirical criteria for the application of terms such as "adequate or proper working order" are laid down, functional analyses will not be testable. For this reason, he suspects that what Robert K. Merton has called the postulate of universal functionalism—the view that every item in a culture has a function—may be empirically vacuous rather than merely premature. Hempel cautiously interprets functional analysis in social science as a research program aimed at determining the respects and degrees to which various systems are self-regulating and takes the pursuit of this aim to require the formulation of precise and empirically testable hypotheses about specific functional relationships.

Hempel's work on explanation has inspired an enormous body of philosophical literature and has also been taken seriously by social scientists, such as Paul F. Lazarsfeld, who are concerned with methodological problems. Some of the more instructive philosophical criticism of Hempel's views has come from William Dray (1957), Wesley C. Salmon (1971), and Michael Scriven (1959; 1962). The favored strategy of most critics has been to elaborate examples of explanations in some empirical discipline such as history or evolutionary biology that seem to be perfectly respectable and yet do not appear to conform to either Hempelian pattern of explanation. Adolf Grünbaum (1963) and John Passmore (1958) have defended some of the leading Hempelian ideas against such

objections. Hempel (1965) himself has responded to many of his critics. Nicholas Rescher (1970) has edited a volume of essays in Hempel's honor in which prominent philosophers discuss several facets of his work. Over the years, it has become increasingly clear that Hempel's work is a major contribution to the philosophical treatment of the logical and methodological problems of the empirical sciences.

PHILIP L. QUINN

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HERRING, PENDLETON

(Edward) Pendleton Herring had two successive careers, first as an influential political scientist, then, after 1948, as head of the Social Science Research Council (SSRC), one of the leading research organizations in the United States.

Born in Baltimore, Maryland, in 1903, Herring received his A.B. (1925) and Ph.D. (1928) in political science from the Johns Hopkins University. He served as a member of Harvard University's department of government from 1928 to 1947, and as secretary of its Graduate School of Public Administration from 1936 to 1947. In the latter capacity he developed the "case method" of teaching public administration, which grew into the Inter-University Case Program in Public Administration.

During his 18 mainly academic years (1928-1946) Herring published 6 books, 5 of which have remained in print long after their publication. *Group Representation Before Congress* (1929) took its place alongside Peter Odegard's *Pressure Politics* (1928) and E. E. Schattschneider's *Politics, Pressures and the Tariff* (1935) as the foundation of the study of pressure groups as a new field of political science. *Public Administration and the Public Interest* (1936b) was one of the first and most influential analyses of the interactions among government agencies and their organized and unorganized clienteles. *Federal Commissioners: A Study of Their Careers and Qualifications* (1936a) pioneered studies of the relationships between administrators' backgrounds and their behavior in office.

Herring's next two works were more general

in scope and impact. *Presidential Leadership* (1940b) was the first major study of the politics of presidential leadership written since Woodrow Wilson's *Constitutional Government in the United States* (1908). Perhaps his most important work, *The Politics of Democracy* (1940a) has remained a principal source of ideas and inspiration for political scientists who, like Jeane Jordan Kirkpatrick, Seymour Martin Lipset, Nelson Polsby, Aaron Wildavsky, and Austin Ranney, believe that America needs a politics of moderation and consensus more than one of sharp choice between irreconcilable alternatives, and that the decentralized, pragmatic parties of American tradition are more likely to meet that need than are the disciplined British-style parties so admired by advocates of "responsible party government." His last book, *The Impact of War* (1941), was an analysis of American war mobilization.

In 1953 Herring was elected president of the American Political Science Association. But, when he took office he was already well launched on his second career.

After serving from 1942 to 1947 in a variety of administrative positions in the national government and the United Nations, Herring in 1948 became president of the Social Science Research Council, which had been founded in 1923 to promote the organization and funding of social science research. Herring held the presidency from 1948 to 1968, and in those twenty years the council became the prime organization shaping the nature of research in most of the social sciences.

At the beginning of Herring's presidency the SSRC's annual budget was under \$500,000; when he left office it was nearly \$2 million. Under his leadership the council's growing resources were used to pursue two main goals. The first was to improve the quality of social science knowledge by encouraging researchers to acquire more and better data, analyze them by more rigorous quantitative methods, and develop more general and systematic theories with predictive power. The second goal was to focus and report research in ways that would make it more accessible and useful for the makers of public policy. The council worked mainly through its committees of scholars, chosen and encouraged by Herring and his staff. Among the more noteworthy may be mentioned the committees on mathematics in social science research, on economic stability, and on biological bases of social behavior. In Herring's own discipline the committee on polit-

ical behavior and its derivative committees on comparative politics and on governmental and legal processes played key roles in the "behavioral revolution" that transformed political science from the 1950s to the 1970s.

In the postwar era, Herring succeeded in attracting the country's leading social scientists to serve on the SSRC's committees, conferences, and board of directors. To mention only a small sample: in economics, R. A. Gordon, Lawrence R. Klein, Simon Kuznets, and James Tobin; in history, Philip Curtin, Louis Gottschalk, Roy F. Nichols, and Edwin Reischauer; in law, Willard Hurst, Edward H. Levi, and Leon Lipson; in political science, Gabriel A. Almond, Robert A. Dahl, V. O. Key, Jr., David B. Truman, and Robert Ward; in psychology, Angus Campbell, Lee J. Cronbach, Leon Festinger, Gardner Lindzey, Charles E. Osgood, and Herbert A. Simon; in sociology, James S. Coleman, Paul F. Lazarsfeld, and Robert K. Merton; and in statistics, Otis Dudley Duncan and Frederick Mosteller.

Herring retired from the SSRC in 1968 and became director of the Foreign Area Fellowship Program, which administered programs of training and research in foreign countries organized in close cooperation with scholars in the host countries. As president of the Woodrow Wilson Foundation after 1962, he played a major role in persuading Congress to establish the Woodrow Wilson International Center for Scholars in Washington.

Probably no one in his generation had as great and as varied an impact on social science as Herring. Postwar studies of political parties, pressure groups, and public administration took their departure from his prewar books. Postwar research in all the social sciences owes much of the broadening of its objectives, the refinement of its working methods, and the greater utility of its products to the work of the SSRC under his direction. Those who seek his monuments will find many in the other pages of this encyclopedia.

AUSTIN RANNEY

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HERTZ, ROBERT

Robert Hertz (1881-1915) was one of the most brilliant and promising members of the younger generation of Émile Durkheim's *L'année sociologique* school—a group of scholars that was to be devastated by World War I (Mauss 1925).

Born at St. Cloud, France, Hertz was educated at the École Normale Supérieure, from which he graduated in first place as *agrégé de philosophie* in 1904. In the two succeeding years he did research in the library of the British Museum in London, and in 1906/1907 was philosophy master at the Lycée de Douai. In 1907 he was appointed to the École Pratique des Hautes Études in Paris, where he remained until the war, in which he was commissioned as a second lieutenant of infantry. He was killed by German machine guns, at the age of thirty-three, leading his men in an attack on Marchéville.

In his lifetime Hertz had published essays on death (1907), the preeminence of the right hand (1909), and the cult of St. Besse (1913), and a pamphlet on socialism (1910). At the front, he

had collected from his soldiers a number of country sayings, and these were later printed (1917). The introduction to a major work on sin and expiation was edited by Marcel Mauss (1922), and a long review on Russian ecstatic sects was included in a volume of his collected writings also brought out by Mauss (1928). Hertz also left a study, the product of two years' work, of the myth of Athena. This was complete (Mauss 1925, pp. 24-25), and Mauss announced his intention to publish it (in Hertz 1922, p. 4), but it did not appear and the manuscript has never been traced. In addition Hertz had embarked on another investigation, similarly inspired by the case of St. Besse, of legends and cults of rocks, mountains, and springs, but he had not the time to carry it out before the war took him (A. Hertz, in R. Hertz 1928, pp. xii-xiii).

In his published works Hertz exhibited a masterly command of the sources, an acute sense of problem, and a sure style of analysis; he wrote with clarity and remarkable authority. Nevertheless, and in spite of the courses of lectures based on his study of sin which Mauss delivered at the Collège de France between 1932 and 1937, as well as more sporadic attention by others (cf., Needham 1973, p. xiii), Hertz remained largely unknown until after World War II. His contributions were then brought to the notice of social anthropologists in lectures given at Oxford University by E. E. Evans-Pritchard, and at the latter's suggestion the essays on death and the right hand were brought out in an English edition (Hertz 1960). These led directly to a resurgence of interest in symbolic classification, especially that concerned with lateral values. In 1973 a substantial volume of studies of the symbolism of right and left was published (Needham 1973), inspired by Hertz's essay on the topic and dedicated to his memory.

During his university years Hertz long hesitated between committing himself to religious or economic sociology on the one hand, or else to ethics and politics on the other, but he made no decided choice (Mauss, in Hertz 1922, p. 2) and his publications eventually covered a range of apparently disparate subjects. His essay on the collective representation of death, an analysis of secondary burial with special reference to the Dayak peoples of southeastern Borneo, was a singularly accomplished piece of work for a man in his twenties. His argument was that in primitive societies death was not a unique event but an initiation; secondary burial, as a rite of

transition, was thus comparable with rites of birth and marriage. This was essentially the argument that Arnold van Gennep generalized, with acknowledgment to Hertz, in his global survey of rites of passage (1909). In the paper on the right hand, Hertz argued that the universal preeminence of the right over the left was a particular case and a consequence of the dualism inherent in primitive thought, a religious polarity that symbolically partitioned the universe and with it the human body. The study of the alpine cult of St. Besse, an enigmatic figure armed as a Roman legionary and associated with an isolated rock high in the mountains, was a profound combination of local inquiry, historical research, and sociological speculation. Finally, the projected work on sin and expiation was an attempt, by means of comparative ethnography, to define a form of religious experience. Starting from the domination of Christian Europe by the ideas of the sin of the first man and the redemptive passion of Christ, Hertz sought parallels in less advanced civilizations, especially among the Maori, and he intended to prove that the Polynesian ideas, while not strictly primitive, were in a sense antecedent to the Jewish, Greek, and Roman ideologies from which the modern European concepts emerged.

Hertz was more fundamentally concerned, however, to detect certain abstract features of collective representations and social conduct underlying these particular institutions: transition, opposition, the symbolic power of rocks, and the puzzle of how society could both castigate sin and absolve the sinner as if the offense could be effaced. In this focusing of analysis Hertz was a methodological successor to his teachers Durkheim and Mauss. To some extent also he shared their weaknesses—for example, an excessive reliance on the opposition of sacred and profane, and a metaphoric tendency to hypostasize society. In other respects, by contrast, he was in advance of his seniors, not least in abstaining from Durkheim's typical vices of *petitio principii* and the explaining away of contrary evidences by resort to conjectured changes, and from Mauss's inclination to derive complex social facts from a single causal factor; also, he got his references right.

Hertz's characteristic method was to explicate the interconnections among various aspects of an institution, to press his analysis in progressively more fundamental terms, and finally to relate the social facts to a specific ground or occasion. Thus the physical phenomena of death

do not determine the collective representations and emotions, but they contribute to the form of the latter and lend them material support; the superior agility of the right hand is not the cause of lateral values and religious polarity, but it provides the occasion and a paradigmatic case of opposition; the significance of the rock of St. Besse is not its anecdotal association with the cult, but its value as an emblem of the collective existence and continuity of the community of the faithful. An especially important feature of this style of interpretation is that Hertz makes no doctrinaire disjunction between social facts and inner states: thus the institution of secondary burial helps to overcome grief and to bring the survivors gradually to terms with the reality of the separation; the ideology of dualism, as an essential source and pattern of order, is protected by individual reactions of contempt for the left and aversion from what it symbolizes; and the social organization of a saint's cult derives from the adherents' wish to endure and from their imaginative response to the local emblem (the rock) of their persistence and unity; the regulation of mystical crime has to do with a distinct form of experience.

Connected with this approach, perhaps, was Hertz's attitude toward ethnographic research. Whereas neither Durkheim nor Mauss had made studies in the field, Hertz at least based his study of St. Besse on six weeks spent in the alpine hamlet of Cogne. He took part in a pilgrimage to the rock, and in September 1914 he was to have made a journey to the mountainous regions of Greece in order to learn directly, not merely through books, the background to the myth of Athena and to cults of rocks in general (A. Hertz, in R. Hertz 1928, p. xiii). He urged hagiographers not to neglect two "precious instruments of research": a pair of good boots and an iron-tipped walking stick (1928, p. 188). In many respects Hertz was a direct precursor, theoretically and practically, of much that is best in modern social anthropology. Mauss (in Hertz 1922, p. 1) found that time only confirmed and perpetuated Hertz's methods, the directive value of his analyses, and his style of exposition: "They enter the serene sphere of the classical."

RODNEY NEEDHAM

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HESCHEL, ABRAHAM J.

Abraham Joshua Heschel (1907-1972) was born and raised in Warsaw, acquired his academic training in Berlin, and wrote most of his mature works in the United States, to which he emigrated in 1940. From 1945 until his death he held the position of professor of ethics and mysticism at the Jewish Theological Seminary of America in New York. His work as a philosopher of religion and a theologian is best understood as an attempt to achieve a creative synthesis between the two different worlds in which he lived before he came to the United States. Born into an intensely traditional Hasidic milieu, he counted among his paternal ancestors Rabbi Dob Ber of Mezeritch, "the Great Maggid," successor to the Ba'al Shem Tov, founder of the Hasidic movement that swept across eastern European Jewry in the eighteenth century, and Rabbi Abraham Joshua Heschel, known as "the Apter rebbe." On his mother's side he was also descended from rabbis. Growing up in the world of piety and learning, he acquired a thorough training in the study of the Talmud as well as a

knowledge of the world of Jewish mysticism, the Kabbalah. Determined to receive a modern Western education, he spent time at a secular Yiddish Realgymnasium in Vilna, and in 1927, moved to Berlin, where he attended the Hochschule für die Wissenschaft des Judentums and the University of Berlin. In February 1933 the university awarded him a doctorate for his dissertation on prophetic consciousness (1936).

Arriving in Germany with an impressive accumulation of rabbinical learning, he was confronted at the University of Berlin with the critical methods of modern philology and source criticism, while his ancestral piety was challenged by the approaches of modern secular philosophy, science, and psychology. Heschel was especially impressed by the works of the neo-Kantian philosophers and by such phenomenologists as Eduard Husserl and Max Scheler. Against the neo-Kantians he defended the claims of Biblical and rabbinical Judaism that God is more than an idea, a postulate of reason, or a logical possibility. From the phenomenologists he learned to analyze the constitutive traits and structures of experienced reality in their essential purity, without reducing them to alien categories that distort their uniqueness. Thus, in his doctoral dissertation, he refused to force the phenomena of Biblical prophecy into the fixed categories of Aristotelian metaphysics, as the medieval scholastics had done, or to "explain" them in terms of modern psychological or sociological notions. As a result of his attempt to analyze the prophetic consciousness of the Bible he developed a novel conceptual framework that became the nucleus of his later philosophy of Judaism.

Heschel saw the task of the philosopher of religion neither in the construction of a "religion of reason," such as the neo-Kantian philosopher Hermann Cohen had done, nor in the analysis of "religious experience," such as Rudolf Otto had achieved in *The Idea of the Holy* (1917). The first substitutes philosophy for religion; the second tends to replace it with the psychology of religion. If religion is *sui generis*, it must be studied in its own terms, and the interpreter, while making full use of the tools of philosophy, must never forget that the main task is to understand and illumine the reality underlying religion, the living and dynamic relationship between God and man disclosed in the classical documents of Judaism and the lives and experiences of pious men.

Heschel therefore pursued two parallel, com-

plementary tasks: he did research in the sources of classical Judaism to gain valid and objective knowledge of that tradition, and he formulated his own philosophy of religion based on the results of his specialized studies. These, in addition to his work on prophecy (later expanded into his English book *The Prophets* [1962]), included a biography of Maimonides (1935), articles on medieval philosophy, Kabbalah, and Hasidism, as well as a major work on the doctrines of revelation in talmudic thought written in Hebrew (1962–1965). A two-volume work on the thought of Rabbi Menahem Mendel of Kotzk (1973a), written in Yiddish, and an English volume on the Kotzker and Kierkegaard (1973b) appeared posthumously.

Heschel used the results of these researches to construct his own philosophy of religion, which is developed in *Man Is Not Alone* (1951a) and its successor volume, *God in Search of Man* (1955). In these books, Heschel defined religion as an answer to man's ultimate questions. Since modern man is often alienated from the reality that informs genuine faith, Heschel thought it inadequate to present traditional answers to these questions. Instead, he tried to recover the significant existential questions to which Judaism offers answers. Delving beneath the surface phenomena of modern doubt and rootlessness, he confronted his readers with the living God of the Bible. This God is not "proved" as is the conclusion of a syllogism; to the religiously sensitive man God is an "ontological presupposition," the ultimate reality, which is then crystallized by discursive thinking into the concept of a power, a principle, or a structure.

Heschel described three ways in which man could reach an awareness of God. The first is by going through wonder beyond the mere givenness of the facts to an awareness of the grandeur and mystery of reality. Such wonder can take different forms: as curiosity it becomes the starting point of science, which looks beyond the given facts (data) to the laws they exemplify; as radical amazement it points to the ground and power that stand behind all facts and perceptions of facts. This evocative approach yields a *panentheistic* outlook: through created things man reaches an awareness of the God who is within, but is also beyond all finite existence.

In the second way, man reaches an awareness of God by delving into the recesses of his own being, thus realizing that the self is not a discrete, independent, and self-sufficient entity, but part of something greater and more encompass-

sing than one's individual ego. This second way tends to a *quasimystical* world view, but it stops short of the danger of mystical absorption and annihilation in the Godhead by articulating God as the subject of all reality and man as the object whose dignity and worth derive from his consciousness of being the goal of divine concern and expectation.

In the third way, man becomes aware of the *voice* of God. The "holy dimension" discovered by listening to the voice and acting responsively and responsibly characterizes the Biblical view of man as the recipient of divine revelation. In facing the transcendent God and his demands one becomes a moral agent. By observing the commandments the Jew enters this holy dimension of God's challenge and guidance, and by obediently responding to the divine imperative he experiences himself as the object of God's address and concern.

Heschel's emphasis that faith is not so much assent to a proposition as an attitude of the whole person, a response and an attachment to God's demands, is not only important for his understanding of the significance of *halakhah* (law). It is also crucial for his interpretation of God as personal outgoing ("transitive") concern and for his critique of the main trends of philosophical theology that have tried to assimilate the living God of the Bible to impersonal categories of Greek metaphysics or to some types of modern process philosophy. To raise the concept of God from the level of crude anthropomorphism, the medievals defined him as Being Itself, while the modern inclination is to describe the Deity as the power that makes for goodness, the underlying structure or the *nisus* of the universe or the moral dimension of reality. This however is not only detrimental to religious life, watering down the "religious availability" of God, but in direct conflict with the Biblical and rabbinic outlook. Therefore Heschel emphasized the divine concern of the personal God as essential to any adequate Biblical theology. He developed the concept of the "divine pathos," which he had first presented in his doctoral thesis, and made it the cornerstone of his later thought. In fact, the concept of *personal concern* can be considered the key to Heschel's whole system: The Ultimate is not Being but concern or *directed attention*. Few of Heschel's readers realize that he propounded a doctrine that ran counter to the whole venerable tradition of Jewish and Christian metaphysical theology from Philo Judaeus, Maimonides, and Thomas Aquinas to Hermann

Cohen, Étienne Gilson, and Paul Tillich. He declared that the Greek category of "being" and eternally frozen perfection is inadequate to Judaism and must be replaced by a new set of categories derived from Biblical thinking. Aristotle's Unmoved Mover must give way to the Bible's Most Moved Mover, the God of pathos and transitive concern who stands in a dynamic and reciprocal relationship to his creation. "Being through creation," through the divine act of freedom, expresses in symbolic form that reality is not a self-sufficient, fixed, mechanical order. It is a process in which responsible man freely reacts to the challenges of life and in which surprise, novelty, and unexpected creative possibilities always occur. Through sympathy, compassion, and sensitivity to the divine demands, man can overcome his egocentric predicament and fulfill his true potential.

Man as created in the image of God must find his goal in *imitatio Dei*. Heschel's teachings on the nature and worth of man are perhaps his most important contribution to the social sciences. Animate is distinguished from inanimate existence by reflexive concern: every living organism abhors its own destruction. God, who faces no threat to his being, is free of reflexive concern and is characterized by transitive or outgoing concern for others. Man, who as a living creature shares reflexive concern and anxiety for his own future with the animals, is distinguished by sharing transitive concern with God, a regard for others. "Man cannot even be in accord with his own self unless he serves something beyond himself." In ethics the idea of concern helps to explain the ideal of caring for the fellow creature. Animals are content when their needs are satisfied; man cannot find true happiness unless he is also able to satisfy, to *be a need*, not only to have needs.

In exploring basic human attitudes the idea of active concern enables us to overcome routine dullness and alienation by reminding us that man is a self-surpassing creature, the being who is always beyond himself, in *ek-stasis*, transcending his loneliness and isolation in knowledge, action, art, and worship. Space and time take on a new meaning in Heschel's Biblical thinking: things are merely frozen processes; life itself is a process gathering the past into itself, reaching out into the future. Reality is not like a stone sculpture, but like a symphony. The Sabbath, whose celebration is the weekly renewal of the divine-human covenant, is an edifice in time, a cathedral of the spirit.

For the student of the social sciences the most challenging and significant aspect of Heschel's thought may lie in his frankly theocentric viewpoint. Where naturalists have tried to understand religion in terms of human interests, categories, and needs, he maintained the traditional Jewish doctrine that man cannot understand his own nature, his needs and fulfillment, in isolation from God. He once remarked that the Bible is not man's theology but rather God's anthropology. His philosophy of religion expounding the thesis that God needs man and that man finds his deepest fulfillment in sharing in the divine concern, was an attempt to work out the implications of this Judaic world view for modern man.

FRITZ A. ROTHSCHILD

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HICKS, JOHN R.

John Hicks was born in Leamington Spa, England, in 1904. He was an undergraduate at Balliol College, Oxford, from 1922 to 1926, and studied mathematics for a year, then philosophy, politics, and economics. He was Jevons professor of political economy, Manchester (1938–1946), fellow of Nuffield College, Oxford (1946–1952), and Drummond professor of political economy, Oxford (1952–1965).

Hicks is one of the leading economic theorists of the twentieth century. He was created Knight Bachelor in 1964. In 1972 he was awarded the Nobel Prize in economics (jointly with Kenneth J. Arrow) for his work on "general equilibrium and welfare economics." His *Value and Capital* (1939b) is an economic classic. His major writings have become thoroughly absorbed into teaching at the university level, most notably those on indifference curve analysis, and on the IS-LM curve presentation of the Keynesian system.

In his early years as a junior lecturer at the London School of Economics (L.S.E.), Hicks had intended to specialize in labor economics, and his earliest publications were on institutional aspects of the labor market. However, by 1929 he had begun to explore economic theory and to read Gustav Cassel, Léon Walras, Vilfredo Pareto, Francis Y. Edgeworth, Knut Wicksell, and the Austrians. At the same time, he was lecturing on general equilibrium theory, in the tradition of Walras and Pareto, and on risk. The influence on Hicks of Friedrich A. von Hayek (who arrived at the L.S.E. in 1931) was strong and may be detected in varying degrees in all his major works. By 1934, Hicks was independently writing on money in a distinctly Keynesian vein. He subsequently became a noted expositor and developer of Keynesian ideas. Starting from Hicks's first book on wages, D. H. Robertson was an important private influence, something which is perhaps most obvious in Hicks's trade cycle volume.

Hicks's first book, *The Theory of Wages* (1932), is not typical of his total work, adopting, as it does, the assumption of "neutral" money. Even so, it has been influential in a number of respects, including its discussion of inventions, and its introduction of a new measure of the substitutability of factors, the "elasticity of substitution." Various generalizations of the elasticity of substitution have since been developed; by Hicks himself and by Paul A. Samuelson (Wolfe 1968).

It was Hicks's second book, *Value and Capital*, which established his reputation as a theorist of the first water. Its major contributions are the elaboration and development of Paretian value theory and Walrasian general equilibrium theory. The early chapters build on the work jointly developed with R. G. D. Allen in the article "A Reconsideration of the Theory of Value" (1934). This paper abandoned the concept of measurable utility, and showed that to derive many operational results of demand analysis, it was sufficient to assume simply that consumers were able to rank bundles of goods in order of preference. Although the apparent novelty of the Hicks-Allen theory was diminished by the discovery of an earlier mathematical version developed by Eugen Slutsky, the clear literary way in which it was developed in *Value and Capital*, and the skill with which it was used to illuminate such concepts as complementarity and substitutability, made it essentially a Hicksian theory. In *A Revision of Demand Theory* (1956), Hicks revised the first three chapters of *Value and Capital* in terms of the "revealed preference" theory of Samuelson.

In the 1950s, Hicks was one of a group of economists who were concerned with developing compensation criteria in welfare economics. The theoretical underpinnings of this work are contained in the previously mentioned two books. His approach was much criticized by theorists. It has been, at least partly, replaced by the more concrete policy approach of Jan Tinbergen; the social welfare function approach of Abram Bergson and Samuelson; and the approach by I. M. D. Little and others giving distribution effects paramount place. Nevertheless, the compensation approach may remain the best rationale for modern work in cost-benefit and cost-effectiveness analyses.

Another accomplishment of *Value and Capital* was to investigate the stability of Walrasian general equilibrium. Hicks went beyond the "equation counting" approach of Walras, to investigate how the system would react to changes in tastes and resources. However, the stability criteria he developed failed to provide a satisfactory theory of behavior out of equilibrium. This deficiency was noted by Samuelson who was able to demonstrate that Hicks's conditions were neither necessary nor sufficient for dynamic stability. There followed a period of intense research into the properties of general equilibrium models by economists such as Lloyd A. Metzler, J. L. Mosak, and P. K. Newman (a student of Hicks), culminating in the important

works of G. Debreu, Arrow, and F. H. Hahn. On the whole, this research has reconfirmed the value of the Hicksian contribution. For example, the work of D. McFadden (Wolfe 1968) has shown that certain processes which are stable, in the sense of Samuelson, are "close to" the sequential adjustments to temporary equilibria described by Hicks.

The work of Hicks on value and risk led him to develop an early version of portfolio selection theory in the paper "A Suggestion for Simplifying the Theory of Money" (1935). This theory has flourished in the hands of James Tobin, H. Markowitz, and others, though elements of it were already present in Keynes's writings. Perhaps as a result of this 1935 paper, Hicks was asked to review Keynes's *General Theory* (1936) for the *Economic Journal* in 1936. This review was very much a first impression, but it led on to what is essentially a second review, "Mr. Keynes and the 'Classics'" (1937), in which Hicks introduced the IS-LM curve diagram which has come to be regarded as the essence of the Keynesian theory. Useful though this model has been as an expository device, it had the undesirable effect, recognized by Hicks himself, of causing Keynesian analysis to relapse into statics. Don Patinkin maintained that the Keynesian model was best viewed in a framework of disequilibrium dynamics, and this is the view that has prevailed, largely through the intellectual leadership of Robert W. Clower, a former student of Hicks.

The first major attempt by Hicks at disequilibrium dynamics was his *Contribution to the Theory of the Trade Cycle* (1950), which built on the multiplier-accelerator framework established by Samuelson, and the growth model of Roy F. Harrod. In the Hicksian model, two effects mitigated the instability which the accelerator induces in a Harrod-type model of equilibrium growth. Firstly, the accelerator was assumed to work with a lag, and secondly, a part of investment was assumed to be autonomous. The path that the cycle took was constrained from below by a "floor" caused by autonomous investment and its consequential consumption, and was constrained from above by a full capacity "ceiling." R. M. Goodwin (1951) simultaneously published a theory of the cycle which may have been more satisfactory, from a technical point of view than that of Hicks, but since that time, relatively less interest has been shown in theoretical models of this kind, although cycles remain a major field of interest for the econometrician.

Hicks's next major investigation of dynamics was *Capital and Growth* (1965), a book which he himself described as "critical and expository, rather than constructive." Even so, the early chapters have become influential as a methodological study of the "microfoundations of macroeconomics." The book made a simple, but fruitful, distinction between "fixprice" and "flex-price" markets: a distinction which was used to great effect in Hicks's analysis of *The Crisis in Keynesian Economics* (1974). Paradoxically, the most important aspect of *Capital and Growth* was a flaw in the second part of the book which dealt with a steady-state growth model. It was pointed out by C. M. Kennedy (Wolfe 1968), that Hicks had assumed that capital goods which were being made in a given period were also used to produce output. This criticism led to the writing of *Capital and Time* (1973a) in which Hicks developed a model which was based on the Austrian tradition of Eugen von Böhm-Bawerk, Wicksell, and Hayek. Hicks regarded each production process as a time profile of inputs and outputs. The model was one of complete vertical integration in which, at no point, were intermediate products referred back to the market. This model was shown by E. Burmeister to be a particular case of a John von Neumann model in which input and output matrices have a special form. If this were the only point, the main advantage of the neo-Austrian approach would be pedagogic. However, the deeper purpose of *Capital and Time* was to provide a theory of the effects of innovation. Such a theory was implicit in Hicks's *A Theory of Economic History* (1969) and was discussed again in the guise of "impulse" (1973b) in Hicks's Nobel lecture in 1972.

For many years, Hicks contributed to policy discussion—not always with unanimous agreement. One must note, however, his important applied work on taxation, much of it in collaboration with his wife, Ursula Hicks. Since 1965, his work on policies toward wage inflation has commended itself widely, even in political circles. Much of this discussion has been particularly concerned with taxation policies toward excessive wage claims.

The range of activity of Hicks has been immense, and is not easily summarized. His own subtitle to *Value and Capital* provides perhaps the best description of his life's work: *An Inquiry Into Some Fundamental Principles of Economic Theory*.

G. C. REID AND J. N. WOLFE

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HILGARD, ERNEST R.

Ernest Ropiequet Hilgard is best known among his scientific colleagues as an experimental psychologist who has worked deeply in two very different fields, human learning and hypnosis. In a much wider circle he is equally well known as an educator and a scientific statesman. Through his authoritative textbooks he has become the leading generalist—and possibly one of the last—in the gradually fragmenting psychology of the latter half of the twentieth century.

Hilgard was born in Belleville, Illinois, on July 25, 1904. His father was a physician and his earliest vocational thoughts were toward medicine; his father died in France in early 1918 while on army medical service, however, destroying the possibility of a normal family succession in practice. Medicine lost its appeal to him, but his scientific orientation was reflected some years later when he chose chemical engineering as his undergraduate major at the University of Illinois.

A second decision, soon after graduation, revealed an underlying conflict between science and humanitarianism that characterized Hilgard's career from then on. He chose to spend a year as a Young Men's Christian Association counselor rather than to go immediately to graduate school. The following year he enrolled in the Yale Divinity School, strongly attracted to the study of social ethics. So far as his education was concerned, the conflict was resolved a year later, when he transferred to psychology at Yale University, a compromise between hard science and liberal humanitarianism. The latter remained a clear part of his life, however, and in later years he participated in a succession of social betterment activities such as work with a consumers' cooperative, and support of labor schools and of the American Civil Liberties Union. Indeed, because of these interests he briefly became a victim of McCarthyism in the early 1950s.

Hilgard was attracted to both vocational and experimental psychology, but the latter field was Yale's strength. His dissertation on eyelid conditioning, performed under the direction of Raymond Dodge, was the first major study of the differences between reflexes, conditioned reflexes, and voluntary action. After receiving his PH.D. (1930), he remained at Yale as an instructor for three years, completing a number of studies on occipitally decorticated dogs with his colleague, Donald G. Marquis. This collaboration

led eventually to their classic volume on *Conditioning and Learning* (1940), an exhaustive and perceptive review that set the stage for the vigorous development of that field during the postwar decade.

In 1933, Hilgard was offered an assistant professorship at Stanford University. He hesitated to leave the eastern academic scene, but on being advised by his department chairman that only a man with private means should remain at Yale, he accepted the invitation. With the exception of a sabbatical year at the University of Chicago and a three-year stint in Washington during World War II, he remained at Stanford throughout his career. During the first few years he studied the problems of learning as these related to the conditioning process, examining the relation between principles of learning revealed by conditioning and by more conventional methods such as rote learning. In 1940 the Society of Experimental Psychologists awarded him its Warren medal for his research.

At the same time, Hilgard's interest in the relationship between learning and education was renewed. Since the beginning of his Stanford career he had been a courtesy member of the education faculty. In 1940, he was invited to the University of Chicago to participate in a one-year study of the application of knowledge about child development and learning to the education of young children. This year marked a significant change of direction and broadening of Hilgard's career. Several factors were responsible. His wife, Josephine Hilgard, a Gesell-trained PH.D. in developmental psychology, had secured a medical degree at Stanford in preparation for a career in child psychiatry. During the year at the University of Chicago she began her training at the Psychoanalytic Institute, thus bringing her husband into contact with a kind of psychology entirely new to him. Likewise, the work with Daniel Prescott's child development group opened another new scene to him. A third important influence soon followed; with the outbreak of World War II he went to Washington to participate in a program of civilian surveys, first working with Rensis Likert and successively in other agencies with many other leading social psychologists. These were the experiences—psychoanalysis, child development, and applied social psychology—that converted Hilgard, the conditioned reflexer, to Hilgard, the generalist, whose introductory textbook on psychology would become the leader in its field less than a decade later (Hilgard 1953).

During the war years, Lewis M. Terman had retired and Hilgard was appointed, in absentia, executive head of the psychology department. On his return to Stanford, he began enlarging the faculty in order to cope with the tremendous influx of returned war veterans in the graduate program. Not surprisingly, his expansion was in the direction of child and clinical psychology. There was one remaining job for him to do in the field of learning, however, and he largely put aside further experimentation to complete a critical treatise comparing the many divergent learning theories that had grown up during the previous two decades. *Theories of Learning* (Hilgard 1948) became the definitive statement on theories that *Conditioning and Learning* had been on empirics.

Hilgard was appointed dean of graduate studies in 1951, but found time to complete his introductory text (Hilgard 1953) while guiding the university's increasing participation in externally funded research and strengthening the faculty in the social sciences. The textbook was notable for a return to a more Jamesian style than had been customary in the texts of the 1930s and 1940s. Particularly in the areas of developmental, social, and motivational psychology it provided a broader and somewhat more applied—or real life—emphasis, avoiding the limitations that would have been imposed by a close reliance on purely experimental data.

It was during the war years that Hilgard began his role as a national statesman for science. He was a key member of the committee that reorganized the American Psychological Association (APA) to bring together the academic and applied psychologists. He was elected to the APA council soon thereafter, and to the presidency in 1949. In the meantime he served on Harvard University's review committee designed to make recommendations for the future of the psychology department. In the subsequent two decades he served on the National Advisory Mental Health Council, was an advisor to the Ford Foundation, and was an APA representative on both the Social Science and National Research Councils. From 1967 to 1969 he was chairman of the joint committee of the National Academy of Sciences and the Social Science Research Council that appraised (in nine volumes) the current status and future needs of the social and behavioral sciences. He was also elected to membership in the American Academy of Arts and Sciences, the National Academy of Education, and the American Philosophical Society.

In 1969, he was given the APA's distinguished scientific contribution award.

Fifteen years of national service, university administration, and book writing had left Hilgard with a strong desire to resume his research career. In 1955 he resigned the graduate deanship, and with initial funding from the Ford Foundation, established a laboratory for hypnosis research. During the first six years, he and his collaborators—graduate students and junior faculty—explored thoroughly the phenomena of hypnosis and constructed reliable scales for measuring a subject's susceptibility to hypnotic induction. A major finding was that hypnotizability is a highly stable personality quality, even over a ten year period. He published a full review of this work under the title *Hypnotic Susceptibility* (1965).

By this time Josephine Hilgard had joined him in the developmental study of hypnotizability, and her psychodynamic investigations paralleled and enriched his more experimental researches. Again Hilgard's abiding interest in application gave direction to his work; during the next decade the Hilgards examined intensively the use of hypnosis in alleviating both chronic and experimentally induced pain (Hilgard & Hilgard 1975). From this work has come a substantial theoretical contribution to the problem of pain itself, a sorting out of the independent contributions of anxiety and of direct sensory perception. The former source can be reduced by both hypnosis and placebos, but only hypnotic analgesia can reduce the latter, and then only in highly hypnotizable persons. These findings, coupled with many others from the laboratory's two productive decades, led Hilgard to construct a neodissociation theory of the divided conscious and unconscious processes represented by the classical phenomena of amnesia, analgesia, fugues, multiple personalities, and induced hallucinations. The theory rests on a distinction between monitoring and executive functions in mental operations, the tripartite fractionation of the former allowing for independent occurrence of mental processing (1977, chapter 11).

In his autobiography (1974), Hilgard referred to himself as an eclectic with respect to psychological theories. In support of this assertion is the fact that *Theories of Learning* contained no definitive chapter that could be called Hilgard's theory. Likewise, one of the significant qualities of the *Introduction* was its easy movement back and forth between the languages of consciousness and of behavior, a point of view

anticipated in his 1949 APA presidential address, in which he discussed the importance of the self-concept in psychology. This quality of eclecticism—a common sense empiricism that draws on whatever is needed to construct a useful psychology—has made his introductory text widely influential in its field. Curiously enough, it also provided the essential ingredient needed to permit Hilgard to develop a theory in his second research area, hypnosis. Neodissociation theory does succeed in integrating the many complex findings that stem from studies in three parallel realms: consciousness, overt behavior, and physiology.

ROBERT R. SEARS

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HOCART, A. M.

Arthur Maurice Hocart (1883–1939), a British anthropologist of French descent, was born in Belgium and attended Exeter College, Oxford. After receiving his degree in classics and ancient history, he went to the Solomon Islands to carry out ethnographic research with W. H. R. Rivers

as a member of the Percy Sladen Trust Expedition (1908/1909). Alfred C. Haddon, professor of anthropology at Cambridge University, then recommended Hocart for the post of headmaster of a school in the Lau Islands, Fiji, where he remained for three years (1909–1912) and acquired a profound knowledge of Fijian civilization. There followed investigations on Rotuma, Wallis Island, Samoa, and Tonga before he returned in 1914 to Oxford, where in the succeeding year he lectured on psychology and anthropology. From 1915 to 1919 he served in the light infantry, fought in France, and rose to the rank of captain. After the war, in 1921, he was appointed archeological commissioner for Ceylon, a post he retained until 1929, when poor health forced him to retire. For the next four or five years he was honorary lecturer in ethnology at University College London, and librarian and a member of the Council of the Royal Anthropological Institute. In 1934, at the instance of E. E. Evans-Pritchard, he was elected professor of sociology at Fuad I University, Cairo, a post he held until he died in 1939 of an infection contracted during field work in upper Egypt.

Although for most of his life Hocart held no academic position, he engaged the professional respect of many prominent figures in anthropology, including Rivers, Haddon, Grafton Elliot Smith, William J. Perry, Evans-Pritchard, Lord Raglan, and also Marcel Mauss. He published five books and nearly two hundred lesser items (Needham 1967). In spite of Hocart's scholarship and numerous achievements, he made little contemporary impression on an academic anthropology dominated by Bronislaw Malinowski and A. R. Radcliffe-Brown, and in the subsequent development of the discipline he was accorded no prominent recognition. His main theoretical concern was with the history of culture, studied by the comparative and inferential methods of philology, and this was not fashionable in a period when academic efforts were most committed to the prosecution of intense functionalist studies of single societies. Religion and symbolism, his chief forms of evidence, were set at a discount against the sociological analysis of kinship, segmentary organization, and political systems. In later decades, however, his work and style of thought attracted greater attention. *The Life-giving Myth* (1952a) was reissued in 1970, and in the same year there appeared a critical edition of *Kings and Councillors* (1936), his major monograph. With the development of a less doctrinaire conception of anthro-

pology came a revival of interest in cultural origins and ritual, as well as in speculation about the foundations of culture, that gave Hocart's ideas a new relevance.

Hocart's thought was quick and kaleidoscopic, his method of advancing an argument was disciplined but unpredictable (like a fencer in attack), and he did not often work out a theory by way of a lengthy sequence of validated grounds. The vehicle that best suited his purposes was a brief essay or even a letter to a journal, and his argumentation was often terse and unconventional to the point of seeming erratic. His subjects varied extraordinarily, ranging over monotheism and money; snobbery and saviors; baptism, covenants, and decadence; kinship, sacrifice, and taxation. In whatever matter he took up, he was guided by the habit of "taking all beliefs seriously" and also of "taking nothing for granted"; he methodically distrusted the generalizations of anthropology and the definitions concocted by academics; and he relied on the precept to assume "nothing which we do not know actually to occur." His questions could sound simple or even naive—"why should a custom . . . not stay as it is?" "once man has come to believe in gods, why should he ever cut their number down to one?" "how did men first conceive the idea of melting stone?"—but they were provocative and fundamental. His conclusions could sound blunt and implausible, but they seldom failed to challenge accepted ideas and to stimulate thought about topics of enduring interest. If his evidence was adduced sporadically, from India, Fiji, or anywhere at all in the world, it nevertheless was intended to establish principles: for example, "the first kings must have been dead kings," "a single family of rituals has spread to the remotest parts of the world," "myths and miracles are reliable history, not of events but of customs."

Hocart propounded no general theory, and it is hardly possible to systematize his disparate ventures, but he was constantly inspired by the intention to "reconstruct the history of thought" and the "development of human institutions." He had no dogmatic method for achieving these ends, but urged that anthropologists could not afford to neglect any possibly fruitful hypothesis: "We shall adopt the obvious hypothesis of a common origin and see where it will lead us." The form of thought that he tried to discern was a religion, the main features of which could be traced across a considerable part of the world. In his longest and most integrated argument,

Kings and Councillors, he contended that the common origin of government itself, and of its various specialized offices, resided in an ancient organization not of the utilitarian management of social affairs but of a symbolic quest of life. Although this book was not likely to supplant the theories of historians and political philosophers, it made a novel and arresting case that was admirable in itself, and it pointed to a metaphysical aspect of social life that surely had some part in the development of society and its proliferating apparatus of administration. It stands, moreover, as the foremost example of Hocart's ambition to formulate "a theory that will explain all the variations in the simplest possible way without invoking any processes that have not been observed" (1952a, p. 170).

In many regards Hocart's qualities are those of a maverick, and his writings make a special appeal to a particular type of intellectual temperament for which sound method and sober conclusions can be secondary to provocative changes of aspect and the adventures of ideas. Yet for some, Hocart was also the ideal type of scholar; as Evans-Pritchard described him (1939): "He did not acquire knowledge to advance himself, but because he had the true scientist's craving to understand the causes of things."

RODNEY NEEDHAM

WORKS BY HOCART

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HOEBEL, E. ADAMSON

E. Adamson Hoebel, born in 1906, is a major figure in the anthropology of law, having pointed the rediscovery of law as a valid and significant topic for anthropological study after decades of almost total neglect. His theoretical and methodological contributions have moved that area of study far enough for it to drop the label "primitive law," and, saliently, to see itself as "the ethnography of law" or "the anthropology of law." In his work with the famed jurist, Karl N. Llewellyn, Hoebel established a model for collaboration between lawyers and anthropologists and the utilization of analytical and methodological concepts from both disciplines. Partly because of this collaborative, interdisciplinary approach, his work also has seized the attention of lawyers and led them to recognize the legal systems of nonliterate societies as law and as a source of insight into Western legal systems. Hoebel is also well known as an ethnographer, especially of the Cheyenne, although he has done field work in and published on four other cultures, especially Comanche, Shoshone, Keresan Pueblo, and Pakistan. As an anthropological educator he has had an impact primarily through his nonlegal books, his administrative roles, and his teaching.

Hoebel was born and raised in Madison, Wisconsin. His father was a traveling salesman for a harness manufacturing company founded by his grandfather and his mother was state civil service commissioner for Wisconsin. He received his Bachelor's degree from the University of Wisconsin in 1928 where he majored in sociology and was strongly influenced by E. A. Ross, who whetted his interest in law and social control. As a Wisconsin undergraduate he was a contemporary of anthropologists-to-be John Gillin, Lauriston Sharp, Sol Tax, and Clyde Kluckhohn. Like the latter, Hoebel's work mirrors a deep orientation to both the social science and humanistic dimensions of anthropology.

Hoebel's major writings reflect this in that, to a greater degree than those of most anthropologists, they are simultaneously analytic and poetic. His exposure to these two strains of the anthropological ethos continued in his graduate training at Columbia University where, after completing an M.A. in sociology, he turned to anthropology and was a student of Franz Boas and Ruth Benedict.

In 1933, when Hoebel expressed an interest in doing his doctoral research on a field study of a Plains Indian legal system, Boas professed disbelief that those cultures had legal institutions and Benedict indicated both disinterest and ignorance. But Boas arranged for Karl Llewellyn, who was then holder of the Betts professorship at the Columbia University Law School and well known as a leading figure in the jurisprudential school of "legal realism" and for the breadth of his outlook on law, to serve as Hoebel's thesis advisor. The dissertation on law of the Comanche was later published as *The Political Organization and Law-ways of the Comanche Indians* (1940). Hoebel received his PH.D. in 1934. Llewellyn also advised Hoebel in his early studies of the Shoshone and, later, he and Hoebel collaborated in the study of Cheyenne law that was published as *The Cheyenne Way* (1941), which became a germinal work in both anthropology and jurisprudence. Although Llewellyn was the senior author, the book was truly collaborative.

Many of E. Adamson Hoebel's major ideas were developed in the collaboration with Llewellyn. They must be viewed in the context of the position of the law in anthropology in 1933 when the two men first met. Not only Boas, but many other prominent anthropologists (e.g., Ralph Linton) gave short shrift to the notion that tribal peoples had anything that, in the formal sense, could be called "law." And in the forty years since the heyday of the evolutionary school there had been virtually no anthropological publications on tribal law and hardly more by nonanthropologists. An exception was Bronislaw Malinowski's *Crime and Custom in Savage Society* (1926), which Llewellyn thought was analytically careless, but which suggested to him that a collaboration between anthropology and law might be fruitful.

The most germinal concept of the Comanche law monograph, of *The Cheyenne Way*, and of all of Hoebel's other legal writing is the "trouble case method." The method, which has become the hallmark of all modern anthropological

studies of legal systems, focuses on actual disputes as the heart of the law and the key to the rules or normative aspects of any legal system and its procedures. Hoebel (1964) acknowledges the origins of the method in Llewellyn's legal realism, but it is also rooted in Hoebel's training in the meticulous ethnography of Boas. It has been discussed and refined by Epstein (1967), Gulliver (1969), Twining (1973a; 1973b), Abel (1969), and Gluckman (1973).

The second major idea that grew out of the collaboration with Llewellyn was the concept of the functions of the law, the "law jobs" as Llewellyn called them. These functions, Llewellyn and Hoebel argued, were shared by both "advanced" and "primitive" legal systems. They go beyond dispute settlement to include the "functional prerequisites of a society specifically shouldered by law." Although the major role in articulating the law functions was Llewellyn's, Hoebel's acceptance of the concept was rooted in his own knowledge of Malinowski's and Radcliffe-Brown's then current work.

Throughout the collaboration Llewellyn and Hoebel viewed law in its cultural matrix, an idea that Hoebel refined later in his postulational analysis. Viewing law in its cultural matrix often is attributed to Llewellyn's legal realism which, in turn, has intellectual roots in his exposure to William Graham Sumner and Edward S. Corwin, who emphasized the importance of folkways and mores and the cultural dependence of law. But to view law in its cultural matrix was, of course, completely in line with all of Hoebel's anthropological training, and he helped Llewellyn to see cultural roots of law that he would have overlooked.

As an anthropologist, Hoebel subjected the juristic generalizations of Western law to comparative testing on the Plains and Shoshone material, and Llewellyn was enough of a comparativist, a skeptic, and, of course, a realist, to concur. This dimension of the collaborators' work has had an especially important impact in juristic studies, by extending the scope of what is considered a relevant comparison in law.

The last phase of the Hoebel-Llewellyn collaboration involved a study of the law of the Keresan-speaking pueblos which began in 1944 and was unfinished at the time of Llewellyn's death in 1962. The bulk of that material remains unpublished, although Hoebel has published alone several papers on Keresan pueblo law (e.g., 1969).

Hoebel has made major contributions to the

anthropology of law in his own right that go significantly beyond those developed in collaboration with Llewellyn. He has laid the groundwork for a science of law by the comparative study of a range of non-Western legal systems in order to derive empirical generalizations about law and to refine analytical concepts. So, although *The Cheyenne Way* contains no definition of law, Hoebel's second major work, *The Law of Primitive Man* (1954), does. Focusing on norms, Hoebel's much quoted definition ties law clearly to its cultural and social context. It also focuses on process, both hallmarks of Hoebel's thinking. But it has been criticized for limiting sanctions to physical ones.

Hoebel (1954) demonstrates that cultural norms can be derived from "postulates, broadly generalized propositions held by the members of a society as to the nature of things and as to what is qualitatively desirable and undesirable." Law, then, functions to give expression to and uphold some of the postulates of a society. This postulational approach is one of Hoebel's major contributions to comparative law and he has used it in his ethnographic writings as well, most notably his short general monograph on the Cheyenne. This approach has intellectual roots in the work of Ruth Benedict and the concept is similar to one postulated by Morris Opler, "themes." The approach is documented in *The Law of Primitive Man* with brilliantly pithic profiles of the "primitive" legal systems of five non-literate societies. The methodology for inferring postulates is somewhat intuitive and difficult to replicate, so the concept has not been as widely adopted by legal anthropologists as that of the trouble case method.

The Law of Primitive Man offers an evolutionary analysis of what Hoebel calls the "trend of the law," empirical covariations of the subsistence mode of a society with the content of legally-sanctioned norms—or the rules of law—and legal procedures. This schema, which has been widely accepted, complements Hoebel's insightful survey of the universals—as opposed to the variations—in the content of law found in the same volume. *The Law of Primitive Man* refines the statement of the functions of law first developed in *The Cheyenne Way* but still concludes that the major functions of law are universal and that "primitive" law is primitive only in procedure. That impact of the monograph has been extended by its translation into German and Italian.

The young Hoebel had enough ego strength

and knowledge of his discipline to be comfortable in the collaboration with Llewellyn. That partnership became a prototype for later relationships and Hoebel has collaborated with a series of other scholars (Jesse D. Jennings, Michael G. Smith, A. A. Schiller, Ernest Wallace, Everette L. Frost, Shirley Holt, and Thomas Weaver), some of them peers; and others, younger scholars for whom he was the Llewellyn.

Hoebel's impact in the anthropology of law has been less through his students than through his influence on peers and younger colleagues. In spite of the very general acknowledgment of the role of his works, he is not intellectually imperialistic. Having been a student of the authoritarian Boas and, initially, a student of the forceful Llewellyn, he was, perhaps, wary of imposing his ideas on his students too strongly. They are not disciples. The inherent significance of his ideas more than his own advocacy of them has significantly influenced in acknowledged ways the work of a generation of legal anthropologists, including Max Gluckman, Philip Gulliver, Paul Bohannan, Laura Nader, Leopold Pospisil, William Twining, James Lowell Gibbs, Jr., and others.

As an ethnographer, Hoebel is a careful fieldworker, but he is particularly gifted in the other portion of the ethnographer's role—describing a culture in a written portrait. He writes about a culture with conceptual veracity but, at the same time, conveys a sense of the reality of culture. He has a gift for anecdote, for the apt example, and for a poetic turn of phrase, all of which characterize *The Cheyenne Way* and the later, more general monograph on that culture which he authored alone, *The Cheyennes: Indians of the Great Plains* (1960). This was an inaugural volume and an immediate best-seller in a series of short ethnographic monographs published in the 1960s. The flair for ethnographic writing marks Hoebel's more conceptual works also, e.g., *The Law of Primitive Man*.

E. Adamson Hoebel has had notable impact as an educator. Wide reading in ethnography and early fieldwork in three cultures equipped him to write *Man in the Primitive World* (1949), designed as a textbook for the undergraduate students who flocked to anthropology after World War II. As *The Cheyenne Way* stimulated a new generation of anthropologists to turn to the anthropology of law, so the textbook spawned a host of imitations and stimulated young anthropologists to think self-consciously about the role of anthropology as a vehicle for liberal

education in the general education model dominant in undergraduate education of the time and in the variations that have followed since. The volume has gone through four editions, the third and fourth as *Anthropology: The Study of Man*, and it has been translated into Spanish and Finnish. A fifth reworking with Thomas Weaver as coauthor was published in 1979 as *Anthropology and the Human Experience*.

Concern with education per se, both liberal education and graduate education, led Hoebel to accept administrative positions as head of the department of anthropology and dean of University College at the University of Utah, and as chairman of the department of anthropology at the University of Minnesota where in 1969 he was named to a distinguished professorship, regents professor in anthropology. He has been regents professor emeritus of anthropology since 1972 and adjunct professor of law since 1969.

JAMES LOWELL GIBBS, JR.

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HOFSTADTER, RICHARD

When Richard Hofstadter died in October 1970 at the age of 54, he was at the height of his powers but not of his influence. His rare combination of qualities—literary elegance, playful intelligence, and a civilized perception of politics—was less than welcome at the end of a decade overheated by the frustrations of the civil rights movement and of an unpopular war, when many intellectuals and professors, guardians of reason, called its values into question.

It was not a decade for subtle discrimination or inconvenient self-appraisal, let alone humor. But while the dominant passions and the mood of the 1960s have faded, Hofstadter's writings have retained their pertinence. Some of them, notably *The Age of Reform: From Bryan to F.D.R.* (1955), which won a Pulitzer Prize in 1956 and spawned a sizable controversy, bid fair to become classics of American historical writing.

Hofstadter's work was pervaded by two pairs of tensions: he was at once writer and scholar, engaged polemicist and detached professional. As critics have not failed to point out, he spent less time in the archives than other historians. This is why, though intensely receptive and impressively well read—few historians have studied Sigmund Freud and Karl Mannheim to greater advantage, or learned more from fellow intellectuals, like his colleagues Lionel Trilling and C. Wright Mills at Columbia University—he sometimes questioned his place in the ranks of historians, wondering whether he was not, rather, a publicist. He had undertaken *The American Political Tradition* (1948), which made his national reputation, preparing to enter a journalistic career.

These tensions expressed themselves in a certain ambivalence toward the issues of his day. Though not an activist, he had an impressive fund of civic courage: in spring 1968, when Columbia University was engulfed by student unrest and the traditional commencement ceremonies had to be moved, Hofstadter, defying the anxious warnings of his friends, delivered the commencement address, a sturdy defense of free inquiry in an academic setting. He wrote occasional pieces, like his article on Catholic presidential candidates during John F. Kennedy's campaign for the Democratic nomination (1960); and his celebrated dissections of the McCarthy phenomenon are thoughtful interventions in public matters that gravely concerned him (1953; Hofstadter & Metzger 1955). Practically the last thing he published was an anthology on *American Violence* (Hofstadter & Wallace 1970). Certainly several of his most important books, notably *The Age of Reform* and *Anti-intellectualism in American Life* (1963), which won him a second Pulitzer Prize, bear the visible imprint of the decade in which he wrote them. Yet they are far from being frantic for relevance; they are, rather, sober historical re-examinations of aspects of the American past which (like the Progressive era) had been much,

if superficially, studied, or (like the pervasive hostility to high culture and science) had not been studied at all. If Hofstadter drew the impulse for his writing from his turbulent present, he scrupulously tried to see the past as past.

Some of Hofstadter's critics in the 1960s accused him of "neo-Conservatism," of betraying the progressive forces in America; they objected to his discovery of what united Americans rather than that which divided them and called him a "consensus historian." They also criticized him for venturing psychological and sociological explanations for—and hence presumably denigrating the cause of—American radicals (see Williams 1956; Pollack 1960). These charges puzzled and irritated Hofstadter, who rightly thought himself a liberal historian-critic, "critical" of the Progressive tradition, as he had written, "but not hostile, for I am criticizing largely from within" (Hofstadter 1955, p. 12), and free of its easy optimism and facile generalizations of that tradition. In *The American Political Tradition*, he had deftly thrown darts at the complacency, the unwearied self-congratulation endemic in the official version of America, employing a wit that, though it owed something to H. L. Mencken, was his own. In turn, and with far more benevolent intent, he questioned some of the righteous and simplistic images that most previous historians had drawn of the American Populists' and Progressives' passion for reform. Part of the revisionist, rather sardonic, portrait that Hofstadter drew of the Populists' nativism and anti-Semitism may need to be revised in turn (Woodward 1959–1960), but as a corrective and as a deep reading of an important American epoch, *The Age of Reform* stands. Later, in *The Progressive Historians* (1968), an intensive study of Frederick Jackson Turner, Charles A. Beard, and Vernon L. Parrington, Hofstadter gently rejected the charge of being a "consensus historian" by noting that the Progressive historians, long dominant in the profession, had sharply overemphasized the element of conflict in American society, and that at the same time conflict was hardly absent from his own historical thinking. Hofstadter was above all the historian of complexity.

Richard Hofstadter was born in Buffalo in 1916, graduated from the University of Buffalo in 1937, and went to New York that year to do graduate work in history at Columbia University, where he received his PH.D. in 1942. Except for a four-year stint (1942–1946) at the University of Maryland, he lived in New York from then

on, and soon became part of the New York cultural scene; his origins in upstate New York generated another of the fertile tensions in which his life abounded. He was at home among New Yorkers yet distant from their parochial cosmopolitanism. His dissertation (and first book), *Social Darwinism in American Thought* (1944), was still a traditional exercise, but it showed something of the energy and control that would characterize all his later work. The book was, as Stanley Elkins and Eric McKittrick have noted, "a spirited account of the life, movement and interpenetration of ideas; it also showed an extraordinary adeptness at setting forth lucid and compact expositions of ponderous bodies of thought" (1974, p. 304). And it showed Hofstadter able at the beginning of his career to distill first-rate history from second-rate minds—a capacity he would demonstrate over and over again with minds like those of Senator Joseph McCarthy or popular preachers and charismatic demagogues.

His second book differed from *Social Darwinism* not merely in tone but in substance; it was far more mordant and far more political. For while his published thesis was rooted in the familiar soil of orthodox intellectual history, *The American Political Tradition* was highly unconventional political history: it gathered a collection of a dozen portraits, ranging across almost two centuries, of the men who had generated the political style in which Americans were still working late in the 1940s. From this book onward, his unique blending of two specialties would define the area in which Hofstadter was most at home. Reflecting on his work, he described his historical interests as "marginal to both political historians and to practitioners of the history of ideas" and situated himself "between the two fields, at the intersection of their perimeters."

This self-chosen marginality gave Hofstadter the intellectual scope he needed. All his mature work—and he matured early—gives evidence for his abiding interest in the nonrational side of social, especially of political, life; he was as sensitive to the cultural matrix as to the private dimensions of ideas. He had an unwavering respect for the life of the mind; when in 1953 he defined the intellectual, in Weberian terms, as a man who lives for, rather than off, ideas, he was defining himself. Intensely at home with ideas, he still could not treat them as abstractions, mere counters in a lofty debate among philosophers. His debt to Freud and Mannheim (already

noted) and to contemporary political sociologists was extensive, though lightly borne. When in 1963 he lectured on "The Paranoid Style in American Politics," he protected this technical word drawn from psychiatry with an elaborate disclaimer: he was not speaking, he wrote, "in a clinical sense, but borrowing a clinical term for other purposes" (1965, p. 3). Yet, while his most famous and most controversial thesis, "status politics," pays its respects to man's irrational drives by stressing the projection, into the political arena, of private anxieties generated by fears of social decline or worries about social ascent, Hofstadter never questioned the potency of rational self-interest in human decisions: "We have, at all times, two kinds of processes going on in inextricable connection with each other: *interest politics*, the clash of material aims and needs among various groups and blocs; and *status politics*, the clash of various projective rationalizations arising from status aspirations and other personal motives" (1963, p. 53). His concentration on psychological strategies and sociological mechanisms did not prevent him from alternating his analytical pages with explicit and powerful narrative passages, or from writing straightforward institutional history, such as his volumes on *The Development and Scope of Higher Education in the United States* (Hofstadter & Hardy 1952), and *The Development of Academic Freedom in the United States* (Hofstadter & Metzger 1955). Still, his first and controlling interest was the human comedy, principally as it played itself out in the United States, an interest to which he brought a measure of irony and sympathy unsurpassed in his generation. His intellectual style was too personal to permit a Hofstadter school to form, and so his work continues to exercise its influence largely by example and as a demonstration of how deeply a civilized mind, disciplined stylist, and skeptical observer can penetrate the human past.

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HOIJER, HARRY

Harry Hoijer (1904-1976) was the son of John Oscar and Agnes Sophia (Peterson) Hoijer. Hoijer received his A.B. (1927), A.M. (1929), and Ph.D. (1931) from the University of Chicago. During his Chicago period, Hoijer benefited from the training and friendship of two distinguished pioneers in anthropological linguistics, Edward Sapir and Leonard Bloomfield,

as well as from collaboration with Morris E. Opler, an anthropologist central in Apachean studies.

After 1929 Sapir's research concentrated on Indian languages. In collaboration with Sapir, Hoijer, from 1930 on, studied Navajo as well as Chiricahua Apache and other Apache dialects, all closely related to Navajo. Although Hoijer's dissertation project was the rescue of Tonkawa (1933), an Indian language of Texas now extinct, the focus of his research during most of his career was the Athapaskan language family, part of Nadene, with Apachean languages at the center.

Hoijer taught anthropology at the University of Chicago from 1931 to 1940, when he left to enjoy thirty years (1940–1970) of productive scholarship at the University of California at Los Angeles. He was chairman of the anthropology department from 1948 to 1951. He served as memoirs editor of the American Anthropological Association (1948–1952), as well as vice president of that association (1949), president of its western states branch (1950), and its national president (1958); he then became president of the Linguistic Society of America (1959). Hoijer was book review editor of the *International Journal of American Linguistics* and associate editor of the *Journal* from 1964.

In private he had a mordant, hearty sense of humor; in public his stance was conservative and guarded. His name became an anthropological household word, partly through his coauthorship of a best-selling text, *An Introduction to Anthropology* (Beals & Hoijer 1953). In this text, he proved a descriptive and historical linguist and comparative grammarian. Less constrained and more intuitive was his enthusiasm for problems and solutions in the field of language and culture. If, like Bloomfield, Hoijer was cautious in interpreting linguistic data and inflexibly dedicated to productive drudgery, he was also, like Sapir, willing to speculate about the influence on perception and cognition of grammatical categories, especially those of the Navajo. He became the perfect referee on matters Athapaskan, as well as the prime target of Athapaskanists and others who failed to understand his principles of scholarship. In his lifetime he achieved the status of a scholar who cannot be ignored, whose work future perspectives on prehistoric Athapaskans, no matter how different, would have to consider.

Hoijer extended confidentiality to his infor-

nants, and did not document his many field trips. Asked in a questionnaire to name three publications that reflected his basic interests, he chose six: a sketch of anthropological linguistics, 1930–1960 (1961a), a study of Athapaskan kinship systems (1956a), an article on language in relation to culture (1953a), and three contributions to *Linguistic Structures of Native America* (1946c; 1946d; 1946e). The categories he selected as his special provinces in linguistics were theory and method, descriptive linguistics, historical and comparative linguistics and linguistic reconstruction, language and culture, language typology, lexicography and lexicons, linguistic change and language acculturation, phonetics, phonemics, morphophonemics, phonology, semantics, structural linguistics, and syntax. His special provinces in anthropology were kinship and marriage, oral literature, and change (acculturation). Hoijer left a manuscript bibliography of published works, published with additions and annotations and a technical review of his linguistic contributions. His collection of manuscripts went after his death to the library of the American Philosophical Society in Philadelphia (see Kendall & Landar 1977).

Empiricism and structuralism. Among Hoijer's contributions to anthropological linguistics are studious caution, organization, and clarity. He reminds us constantly that his conclusions are "tentative." At the same time he begs the adjective because his conclusions were reflections of enormous care in assembling, inspecting, and revising linguistic data.

Quality was to his taste. He mistrusted Athapaskan vocabularies of anthropologists or missionaries unexposed, or poorly exposed, to classroom instruction in linguistics. The Tainaina vocabularies of Cornelius Osgood (1937), for example, failed to please him. He preferred not to speculate about spellings, wearing philological glasses; excellent phonetic and phonemic transcriptions were his *métier*. He defended his standards and theoretical orientation in 66 reviews between 1941 and 1973. Typical are three reviews of contributions to Navajo linguistics. Hoijer (1945b) criticized Gladys Reichard for disorganization in her 1944 essay on the hail chant, evoking a scarcely cordial reply; he devastated her for the ineptness of her Navajo grammar (1951), leaving no room for a reply (1953b). He also found Berard Haile's Navajo dictionary poorly organized and difficult for students to use (1952).

Pragmatism in empirical research was institutionalized in anthropology as the discipline developed in America. Hoijer's theoretical orientation was nourished on this tradition, structured largely by Sapir, Bloomfield, Franz Boas, and Alfred L. Kroeber. All were dedicated to the collection of data and the assessment of patterns. Bloomfield, especially, tried to axiomatize procedures and to exploit structural principles developed by such European scholars as Ferdinand de Saussure and Nikolai S. Trubetzkoy.

Boas, one of the great teachers of American anthropologists, particularly during his professorship at Columbia University (1899–1937), taught the importance of a model of grammatical description involving base forms and processes that transformed them. Some students of Bloomfield, however—Zellig Harris, C. F. Hockett, and Floyd G. Lounsbury—for example, explored a model limited to items and arrangements, in which structure was defined by meaningful contrasts, of phonemes with phonemes on the phonological level of analysis, of morphemes with morphemes on the morphological level. Items were classified in terms of shared criterial properties on the one hand and noncontrastive distribution on the other. Transformational processes were not permitted in this model. Bloomfieldian structural analysis in the item-and-arrangement mold stimulated Clyde Kluckhohn, Claude Lévi-Strauss, and others to seek implications for anthropology in general. If linguists could group alternants, variations of a structurally distinctive sound into phonemic classes, variations of semantically similar utterance-fractions into morphemic classes, why could anthropologists not do likewise? In myth, ritual, and kinship, explorations were undertaken that were patterned on the principles of distribution and classification of some of Hoijer's linguistic contemporaries.

Against this background one can understand Hoijer's cordial disagreements with Harris (Hoijer 1945*d*) and Lounsbury (Hoijer 1954*a*). Hoijer followed Boas and Sapir and favored an item-and-process model. He refused to accept Harris' revision of his inventory of phonemes in *Navaho Phonology* (1945*d*), in which, for example, aspiration was transcribed as [h], pitches were reduced from four tones to two, high and low, and unitary phonemes were respelled as clusters (Hoijer's /c/ became /tsh/ and so on). Hoijer used various spellings for Athapaskan forms, but he insisted on the unitary nature of phonemes in his inventories; he held to the

position inherited from Sapir that Proto-Athapaskan could be reconstructed with morphophonemic pitch notation and ideal analogues of morphemes, instead of with lists of allomorphs of morphemes and the environments of those allomorphs (Hoijer 1943; Hoijer 1954*a*).

Hoijer's conservatism manifested itself also in historical and comparative grammar. Against the fashion of scholars like Sapir, Morris Swadesh, Paul Radin, and Joseph Greenberg in America, and Paul Rivet in France, Hoijer aimed at caution, refusing to accept rash assignments of language families into ever-larger assemblages. For the contrast between Sapir and Hoijer, see Sapir's list of American language families in the *Encyclopaedia Britannica* (1929), Hoijer's enumeration in *Linguistic Structures of Native America* (1946*c*), and Hoijer's revision of Sapir's *Britannica* presentation (1961*b*; 1961*c*; 1961*d*). Hoijer emphasized the tentative nature of Sapir's six groups for all languages north of Mexico: Eskimo–Aleut, Nadene, Aztec–Tanoan, Algonkin–Wakashan, Penutian, Hokan–Siouan. Hoijer accepted Eskimo–Aleut, Nadene, and Aztec–Tanoan, leaving it for future determination, after scientific comparative work, whether Sapir's reductions would survive.

Theories, models, and paradigms. Hoijer admired Bloomfield and Kroeber, who characterized data, described patterns, and worked from demonstrable bodies of detail. Hoijer's grammars came from records, not intuitions. In the 1950s Noam Chomsky and other pioneers in generative transformational grammar brought to the linguistic scene two claims, among others, observing that some models in social sciences played upon patterns behind observable data, patterns not directly demonstrable: (1) all—and only—the sentences of a language can and should be characterized by mathematical logic, by semigroup theory as distinguished from Carnapian use of metalanguages; and (2) phrase–structure grammarians (among whom Hoijer was exemplary) did not go far enough. They could parse a sentence, naming parts of speech, labeling phrases and clauses, and tallying types of constructions, but they failed in not recognizing transformations, additions, deletions, and permutations in sentence derivation and amalgamation. They failed in not positing “deep structures,” engrams whose composition by the mind resulted in the sentences that found their way into the anthropological linguist's corpus.

Many anthropologists of Hoijer's generation saw in Chomsky's approach a reversion to the mentalism decried by Bloomfield. Since it was impossible to correlate claims about "deep structures" with neurobiological mechanisms, some disagreements of generative grammarians exposed anti-empirical bias and true decadence in theory construction. Other anthropologists saw in the best of transformational research an important stimulus to developing models, relating old data to new perspectives in ways that made discoveries significant, and providing "paradigmatic" quality in the sense—or one of the senses—popularized by Thomas Kuhn (1962). Hoijer's response to this dramatic, controversial new paradigm lay primarily in continued dedication to the Bloomfieldian paradigm. Hoijer worked with his hands, not with computers.

Language and culture. Sapir said that one is "at the mercy" of his language. Each language represents its own social reality and defines experience. Benjamin L. Whorf claimed that when grammars differ radically, speakers who use those grammars systematically observe and evaluate experience differently. Hoijer tried to integrate both views under the rubric of the "Sapir-Whorf Hypothesis."

Hoijer's aim in ethnolinguistic research was, in part, to show correlations between grammatical categories and nonverbal cultural sensitivities—e.g., sensitivities of Navajos, for whom motionful activity is salient, in consonance with a grammatical system of verbs of motion (1951). He outlined his approach thus: (1) determine the structural patterns of a language; (2) determine the semantic patterns "that attach to structural patterns"; (3) isolate active structural categories with "definable semantic correlates"; (4) isolate "fashions of speaking," through comparison of "active structural-semantic patterns," to get a partial description of the "thought world" of a speaker; (5) identify indirect connections between the ethnolinguistically derived thought world and other cultural areas.

Hoijer illustrated his approach by analyzing Navajo verbs to show that "the Navaho speaks of 'actors' and 'goals' . . . not as performers of actions or as ones upon whom actions are performed, as in English, but as entities linked to actions already defined in part as pertaining especially to classes of beings" (1954*b*, p. 102). He tied this manner of speaking to the Navajo religious motif of compromising with nature, rather than trying to control it.

Hoijer qualified his view by calling for em-

pirical tests. Generalizations about "the" Navajo, however, oversimplify; there are difficulties in cross-cultural testing of claims about differential perception and cognition. Ironic contrast inheres in Hoijer's impressionistic efforts to follow Sapir and Whorf while refusing to accept "meaning as a primary guide to analysis," which is "analysis by fiat" when used in descriptive linguistics (1953*b*, pp. 80, 83).

Lasting impact. Hoijer's impact on anthropological linguistics was strongest where he was most dedicated to framing scientific statements founded on carefully prepared data of high quality. He recognized that any subgrouping of Athapaskan languages, considering present imperfections in our knowledge, could only be tentative. He experimented with lexicostatistic subgrouping and subgrouping by criterial sound laws. Also tentative was his work with Athapaskan kinship systems, stimulated by Kroeber (Hoijer 1960*b*, p. 32). Hoijer contributed to the reconstruction of 73 Proto-Athapaskan kin terms, using 14 subgroups. But he had misgivings about lexicostatistics in subgrouping or in dating separations of tribes (1956*a*; 1956*b*; 1956*c*; 1962*a*; 1962*b*; 1974*a*).

Hoijer's most durable contributions lie, perhaps, where his emulation of Bloomfield is most apparent. Like Bloomfield, he strove to show regularity of sound change for Apachean (1938); Pacific Coast Athapaskan (1960*a*); and all Athapaskan (1963; 1971*c*). Sound laws that he established permitted the reconstruction of hypothetical prehistoric forms—e.g., kinship systems (1956*a*) and morphological systems (1971*a*). Just as Bloomfield was a founder of Algonquian studies, so Hoijer was a founder of Athapaskan studies on a scientific basis, with texts (Hoijer & Opler 1938; Hoijer & Sapir 1942), grammatical sketches (1946*d*; 1966*a*; 1966*b*; 1973; Hoijer & Joel 1963), and signal contributions to Navajo grammar (1945*a*; 1945*c*; 1945*d*; 1946*a*; 1946*b*; 1948; 1949*b*; 1967; 1971*b*; Hoijer & Sapir 1967). He pioneered also in the study of Athapaskan classificatory verbs (Davidson et al. 1963). With Edward P. Dozier (like William Bittle, who worked on Kiowa Apache, one of Hoijer's distinguished students), he published on Santa Clara Tewa phonemes (1949). In Tonkawan studies Hoijer is unique, having rescued the language with a grammar (1933), a sketch (1946*e*), a dictionary (1949*a*), a study of suffixes and particles (1949*c*), and a collection of texts (1972). His most polished work, perhaps, a pattern and guide for all future

Athapaskan lexicography is *Navajo Lexicon* (1974b).

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HOOK, SIDNEY

In the *Handbook* to his lectures on logic, Immanuel Kant distinguished between a philosophy in the scholastic sense (making, and sometimes endlessly multiplying, distinctions) and a philosophy in the universal sense (*in sensu cosmico*), which is "the knowledge of the ultimate aims of human reason," or the "knowledge of the highest maxims of the use of our reason." In this cosmopolitan sense, the field of philosophy, according to Kant, may be divided by four questions: What can I know? What ought I to do? What may I hope? What is man? Metaphysics deals with the first, ethics the second, religion the third, and philosophical anthropology the fourth. Kant added: "Fundamentally all this could be reckoned as anthropology, since the first three questions are related to the last."

Except for the phrases "the ultimate aims" and "highest maxims," Kant's program for philosophy can be said to describe Sidney Hook's intentions and concerns. "Philosophies," John Dewey declared, "are different ways of construing life. . . ." As Hook elaborated this statement, in *Pragmatism and the Tragic Sense of*

Life (1974), his 1960 presidential address to the eastern section of the American Philosophical Association:

Here lies the important task for the philosopher. To be wise he must immerse himself in the actual subject matters (not necessarily experiences) out of which life's problems arise. To be wise about economic affairs he must study economics, to be wise about the problems of law he must study law, to be wise about politics he must study history, sociology and other disciplines. . . . It is philosophy not as a quest for salvation but as a pursuit of understanding of great cultural issues and their possible upshot. It does not start with a complete stock of philosophical wisdom . . . but with an initial sense of concern to meet the challenges of the great unresolved problems of our time, offering analyses of these problems which will win the respect of the specialist and yet command the attention of everyman. . . . It is philosophy as *normative* social inquiry. (1974, pp. 7-8)

This willingness—more, eagerness—to enter into the fray of mundane affairs has distinguished Hook's career, for he believes that science is on a continuum with common sense, and that knowledge, as an instrumental activity, emerges from the confrontation of problems, rather than being derived from first principles. As Paul Kurtz, who edited a *Festschrift* for Hook's 65th birthday, remarked in his preface:

Hook speaks to the actual conditions of contemporary life; and he has persistently attempted to apply pragmatic intelligence to concrete issues of practical concern. The breadth of his critical analysis has been very wide, and includes education, politics, Marxism, morality, metaphysics, religion, civil disobedience, democracy, science, psychology, art, economics, law, nuclear weapons, racial segregation, the Supreme Court, the Bill of Rights, and international affairs. . . . Sidney Hook, virtually alone among contemporary American philosophers, has not abandoned ethical, political or social philosophy, in the original sense of these disciplines. (1968, pp. 11-12)

To a considerable extent this is true, though the hint of anti-intellectualism (in the guise of anti-scholasticism) in Kurtz's remarks minimizes the enduring contributions to the linguistic clarification of truth and empirical inquiry of more technical philosophers such as Alfred Tarski, Rudolf Carnap, and Hans Reichenbach, or of pragmatists such as C. I. Lewis, and the somewhat younger generation of W. V. Quine, Nelson Goodman, and Ernest Nagel. A useful distinction is made by one of Hook's students,

Raziel Abelson, who wrote: "Philosophy, for Hook, is primarily concerned with 'value facts' rather than linguistic facts, and it mainly clarifies and criticizes values, although on occasion it may clarify and criticize meanings" (Kurtz 1968, pp. 203–204).

Sidney Hook was born on December 20, 1902, of immigrant Jewish parents, and grew up in the working-class neighborhood of Williamsburg, in Brooklyn, graduating in 1919 from Boys' High School, which was then, along with Stuyvesant High School, one of the incubators of young New York intellectual Jews and radicals. World War I roused a sympathy for socialism in Hook, and the reading of W. E. H. Lecky, H. C. Lea, John W. Draper, and other rationalists who counterposed science to religion made him an agnostic; Hook has retained this double stance all his life.

As a student at the City College of New York, he encountered Morris Raphael Cohen, whom he admired, as he wrote in a memoir fifty years later, not only because of his tough-minded, relentless questioning, and sharp wit (a typical Cohen story: "Professor Cohen, prove to me I should study logic." "How will you know it is a good proof?") but also because Cohen was the first teacher to respect Hook's questioning of America's entry into the war "with anything other than the ferocious antipathy of my high school and City College teachers." (Hook's classmate was Ernest Nagel, with whom he formed a lifelong friendship.) Following graduation in 1923 (he won the Ward medal for logic), Hook began teaching in the public schools of the city while attending Columbia University, part-time, as a graduate student in philosophy. After obtaining his M.A. in 1926, he was awarded a university fellowship and finished his Ph.D. a year later.

At Columbia, Hook came under the influence of John Dewey and became his friend and foremost expositor. Yet Hook, who believes that there is an ascertainable connection between temperament and philosophy, has written, in a memoir of Dewey: "Temperamentally, I have found Russell and Cohen more congenial than Dewey. They are philosophers of the clean line, incisive, witty, rarely hesitant about sacrificing the dull or exact truth for the sake of a bon mot" (1974, p. 113). In 1927 he became an instructor in philosophy at New York University, where he remained for the rest of his academic life, retiring in 1972. There, he achieved the legendary qualities of his first teacher, a wit and sharp-

ness as quick as Cohen's tempered by compassion.

Hook's great passion has been radical politics, and his involvement first with Marxism and later as the intellectual leader of the anticommunist left has given him his largest public prominence. In the early 1930s, Hook was close to the Communist party, and was instrumental in gaining a large degree of intellectual support for the Communist presidential candidate in 1932. But his philosophical views on Marx, which were at variance with party orthodoxy, and his dismay at the communist theory of "social fascism," which in Germany labelled the Social Democrats, not the Nazis, as the prime enemy, and which brought the Communist party into practical alliance with the Nazis at key points in opposing the Weimar Republic, turned Hook against the communists, and toward an appreciation of the need for democracy as an integral aspect of socialist belief.

In the mid-1930s, Hook was close to the Trotskyites, and in 1934, together with James Burnham, a fellow instructor in philosophy at N.Y.U., he was instrumental in the formation of the short-lived American Worker's party, an effort at an indigenous American radicalism; but the party was captured by orthodox Trotskyites and soon dissolved. Hook, however, then took the initiative in setting up the John Dewey commission of inquiry, which, following the Moscow trials, in the course of which many of the original leaders of the Russian revolution were shot by Stalin, examined the charge that Trotsky was a Nazi agent, through a series of hearings held in Mexico, where Trotsky then lived. These hearings resulted in a two-volume report demonstrating the Moscow trials to be a frame-up. By 1940 Hook had broken with all variants of Bolshevism, though, out of respect for Trotsky, he dedicated his *Reason, Social Myths and Democracy* (1940) "to the memory of a Great Adversary."

Within the milieu of the New York Jewish intelligentsia, a remarkable group of men and women rising to prominence in American cultural life (for example, Ernest Nagel, Meyer Schapiro, Lionel Trilling, William Phillips, Herbert Solow, Elliot Cohen), Hook became a leading figure. He was for many years the political mentor of *Partisan Review*, which became the dominant cultural periodical in the United States. In 1939 Hook took the lead in organizing the Committee for Cultural Freedom, headed by John Dewey, to combat all varieties of totalitar-

ianism and to oppose a larger communist intellectual front, headed by Franz Boas, which expired on August 23, 1939, a week after many of its leading members signed a statement denouncing the charges that the Soviet Union was a totalitarian society. Ten years later, during the cold war, Hook again was instrumental in helping form the American Committee for Cultural Freedom. In 1950, with Arthur Koestler, Ignazio Silone, Raymond Aron, Michael Polanyi, and other European and American intellectuals, he helped found the world-wide Congress for Cultural Freedom (C.C.F.), to oppose totalitarianism and communist influences in intellectual life. In 1967 it was revealed that the C.C.F. had received funds from the CIA, though there was little question of its intellectual independence. Hook has made no apology for his activities in that period. In an article, "What the Cold War Was About" (1975), Hook, who called himself an "unreconstructed 'Cold Warrior,'" stated that when asked to justify his position, the shortest answer he gives is "Read Solzhenitsyn's *The Gulag Archipelago*."

Despite his intense activism, Hook has been remarkably prolific. A bibliography in the 1968 *Festschrift* listed 21 books by Hook, 13 edited or coedited by Hook, 62 books to which he has contributed (excluding encyclopedia articles), 516 articles in various periodicals, and 107 "selected" book reviews, in a period beginning with his first essay, "Philosophy of Non-resistance" (1922). Since that bibliography appeared, Hook has published, as of 1978, six more books. A large number of his articles have appeared in general intellectual periodicals, such as *The New Leader*, *Partisan Review*, *Commentary*, and *Encounter*, and Hook's relish for intellectual battle has often led to polemics in which an article is countered by a reply and a riposte by Hook, a counterreply and a counterriposte—all of which becomes a robust way of adding to a bibliography. Yet an extraordinary number of the essays, particularly in the books to which Hook has contributed, are serious philosophical articles on the value questions with which he has been concerned all his life.

What is most remarkable, however, is the consistency of his philosophical viewpoint. Hook has been described, variously, as a pragmatist, a naturalist, an experimentalist, an instrumentalist, even an "epistemological activist" (Arthur O. Lovejoy once said that there were 13 varieties of pragmatism). What unifies these positions is the persistent attack on metaphysical absolutes,

"ultimates of human reason," first principles or final causes, because they provide no guides to cognitive validity or practical conduct, and are incapable of being tested or disproved in experience, but are only given some counterlogical formulation. Hook has argued, for example, that it is possible and not unusual for individuals to agree on matters involving values while disagreeing on metaphysical doctrines, or to agree on metaphysics and disagree on values; so nothing immediately follows from metaphysics (Kurtz 1968, p. 183).

Central to his thinking is the attack on the idea of "Being," as such, or on a priori categories. This argument is the leitmotif of Hook's first book, his PH.D. thesis, *The Metaphysics of Pragmatism* (1927), which unites Charles Pierce's "fallibilism" with Dewey's "instrumentalism," and whose target is Kant. It is reiterated in the paper "The Quest for Being" (1953; written for the 11th International Congress of Philosophy, in Brussels, and reprinted in the book of that title, in 1961), whose target is Martin Heidegger. In his first book, in a chapter on "categorical analysis," Hook wrote:

Categories are bound up with distinctions we make in analysis. Analysis starts *from* something given and *with* something taken. . . . It follows, therefore, that Being is not a category and Experience is not a category. Neither is Space-Time nor subsistence nor any other denotative indication of the sum total of actual and possible existents. A fundamental term which has no intelligible opposite describes nothing because its apparent import is to describe everything. (1927, p. 116)

In renewing his attack on the revived interest in such metaphysics, Hook wrote:

In ordinary discourse every significant word has an intelligible opposite. Being, however, as an all-inclusive category does not seem to possess an intelligible opposite. Not-Being is not the opposite of Being, because when it is taken as equivalent in meaning to Nothing, and Nothing is interpreted as a substantival entity, then Nothing is a Something . . . and hence possesses Being too. . . .

Precisely for this reason the question originally asked by Schelling and repeated in our time by Heidegger: "Why is there something: why is there not nothing?" is devoid of sense except as a sign of emotional anxiety.

For Hook, as for Dewey, an idea is an instrument, a sign or a tool, a "mental blue-print which directs its exercise and interprets its effects" (1927, p. 18), ideas are "implicit guides to ac-

tivity and behavior, and knowledge is dependent on the experiment and reconstructive activity" (1974, p. 21), philosophy is an empirical discipline, its methods of inquiry continuous with those of the natural and social sciences (1961, p. 50).

The emphasis, then, is almost entirely on *method*—not Cartesian method, in which, from axioms of undoubted doubt, knowledge is derived by rationalist deduction, or the phenomenological method of Husserl, a logical technique for the "bracketing" of experience, but the *scientific method*, as the one reliable method. In fact, at one point Hook says, in an italicized statement, "All knowledge that men have is scientific knowledge" (1961, p. 214).

What is less clear, however, is the meaning of scientific method and scientific knowledge. Hook has distinguished between the "scientific method as the procedures pursued in the study of nature"—in particular, physics, with its search for quantitative and mathematical precision—from the "scientific method as a general pattern of inquiry whenever we seek knowledge." The latter does require "the recognition and formulation of a problem, the formulation of an hypothesis, the elaboration of its logical implications, the performance of an experimental act, and the observation of its consequences" ([1946] 1963, pp. 174–176).

But when Hook also states that the "experimental act" may be "just looking," and that the consequences of a hypothesis may include "logical consequences," and that "there is no opposition between common sense knowledge and scientific knowledge as knowledge, but only a difference in their objects and problems" (1961, p. 214), then the initial precision seems to have dissolved into a defense of any systematic empiricism that begins with some hypothesis and looks for some consequences. It is striking that Hook, for all his defense of the scientific method, has rarely engaged in the contemporary discussions of the philosophy of science (e.g., those initiated by Karl Popper), perhaps preferring to leave this to his friend and colleague Ernest Nagel (Nagel 1961; 1968).

Hook's forte has been the exposition of ideas, and nowhere is this more evident than in his works on Marx and Dewey, works that have rendered two ambiguous and opaque writers lucid and comprehensible. His first book on Marx, *Towards the Understanding of Karl Marx* (1933) is a *tour de force*. Having spent an academic year (1928/1929) in Berlin and Moscow working through the Marx–Engels archives,

Hook was among the first to read the newly "discovered" early philosophical writings of Marx, and he pointed out that the "young Marx" had an activity theory of knowledge that made intelligible the emphasis on praxis running like a thread through Marx's writings. At the time, Hook was accused of reading Dewey into Marx, for the received notion of that period was the epistemology proposed by Engels in *Anti-Dühring* (1878)—that sensations and knowledge are "mirror reflections" of a material world, a view which Lenin took over in his attack on Mach in the *Materialism and Empirio-criticism* (1909) and which had become the orthodox Soviet position. Yet the view first propounded by Hook is now widely accepted, though it remains an intellectual mystery why Marx, who had read and even contributed to a section of *Anti-Dühring*, allowed this idea to be proposed if it in fact contradicted the views he had come to hold. One answer may be that Marx had changed his mind somewhat, since *Das Kapital* (1867–1879), is written in a determinist mode, in which the "laws of motion" operate and tendencies are "inevitable."

Hook's second book, *From Hegel to Marx* (1936), is still unsurpassed in its meticulous tracing of the relation of Marx to Hegel, and to the group of young Hegelians with whom he jostled so bitterly. What is striking in the book is Hook's neglect of the idea of alienation, which after 1950 became so fashionable that for some writers "alienation" was the master key to all of Marx's thought. In an introduction written for the 1962 edition, Hook pointed out that the concept of alienation was foreign to Marx's thought because its origins were religious; that it implies an essence to man from which man would be alienated and that the term, when used, as it is in the *Economic and Philosophic Manuscripts* (1844), was completely Feuerbachian. In fact, Marx abandoned the term, and later mocked it, as in *The Communist Manifesto* (Marx & Engels 1848), because he had found in economics the material embodiment of philosophy, and "alienation" had been replaced by "exploitation." In two subsequent works, the long essay "What Is Living and Dead in Marxism" (printed in 1940, pp. 105–142) and *The Hero in History* (1943), Hook evaluates and refutes the thesis of social determinism, and provides, through the case study of the Russian revolution, a fruitful analysis of the interplay of structural constraints, contingency, and personality, as the way in which social events can be understood.

Hook's book *John Dewey: An Intellectual Por-*

trait (1939) is a comprehensive survey of Dewey's ideas. The heart of the book is the defense of the argument that values are open to scrutiny and empirical testing, like any other mode of empirical knowledge. What Hook seeks to show is that all values, even those claimed as "ultimate ones," can be judged (evaluated, not just described) on the basis of relevant consequences, for there is no Good in the large any more than there is Truth in the large; and that all statements of value exist in determinate contexts whose claims can be tested. The second theme central to Hook's discussion—and it becomes relevant time and again in his subsequent judgments on Bolshevism and all its variants—is the interconnectedness of ends and means. The notion that the end justifies the means often overlooks the fact that the means used become ends in themselves, or, in different situations, an emphasis on a single end overrides the "irreducibly plural ends" of human beings and distorts the quest that initiated the desire for change in the first place.

This theme is expanded with great sensitivity and poignance in the book that serves as Hook's credo, *Pragmatism and the Tragic Sense of Life* (1974). Rejecting the existential and mystical views that define a tragic sense of life as the ineluctable intrusion of evil into human affairs, or the role of death in defeating the aspirations of men, Hook defines the tragic sense as "a very simple thing which is rooted in the very nature of moral experience and the phenomenon of moral choice." Every such experience, Hook argues, takes place in a situation in which good conflicts with good, and any resolution involves some surrender of a moral position: "No matter how we choose, we must either betray the ideal of the greater good or the ideal of right or justice. In this lies the agony of the choice." The tragic sense of life is the realization of the guilt inherent in making a choice: "In its starkest form [this] is the theme of Sophoclean tragedy, but the primary locus of the tragic situation is not in a play but in life, in law and in history."

DANIEL BELL

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- 1927 *The Metaphysics of Pragmatism*. Chicago: Open Court. → This was Hook's Ph.D. thesis at Columbia University. Includes an introductory word by John Dewey.
- 1933 *Towards the Understanding of Karl Marx*. New York: John Day.
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HORKHEIMER, MAX

Insofar as the members of the Institute for Social Research constituted a coherent "Frankfurt School," their acknowledged "master" was the philosopher and social theorist Max Horkheimer. Although less widely known than sev-

eral of his junior institute colleagues, in particular Theodor W. Adorno and Herbert Marcuse, Horkheimer was the major organizer of the institute's intellectual life and the chief promulgator of its common theoretical perspective, known as "Critical Theory." Although by the time of his death in 1973 Horkheimer had repudiated many of "Critical Theory's" more radical dimensions, his reputation as one of the founding fathers of the tradition of Western Marxism has remained intact.

In common with many other contributors to that tradition, Horkheimer came from a non-proletarian background. He was born in Zuffenhausen near Stuttgart, Germany, in 1895, the son of a wealthy Jewish textile manufacturer, Moriz Horkheimer. His father insisted on his entering the family business, and after graduating from a Gymnasium in 1910, the young Horkheimer reluctantly agreed. But like another son of a successful businessman, Arthur Schopenhauer, to whose ideas Horkheimer was particularly drawn, he entertained hopes of pursuing a less mundane career. In a series of novellas and extended diary entries (1974), he expressed his adolescent yearnings and utopian hopes. His tone was similar to that of the expressionists, whose moral idealism he would come to scorn after drawing closer to Marxism.

His interest in Marxism matured slowly over the next decade. Emboldened by his friendship with Friedrich Pollock, another restless son of a bourgeois household, and made more independent by a year of army service during World War I, Horkheimer left his father's factory for academic life. From 1919 to 1925 he studied in Munich, Freiburg, and Frankfurt, earning his doctorate in 1922 and *Habilitation* in 1925 at the last of these universities. Written under the direction of Hans Cornelius, his dissertation dealt with Kant's *Critique of Judgment* and its implications for overcoming the contradictions of bourgeois life and thought (1925). Although never joining any of the Weimar Republic's radical parties, Horkheimer was drawn increasingly to the left during his student days. Among his friends were a group of similarly inclined intellectuals, including Theodor W. Adorno, Leo Lowenthal, and Felix Weil, who were all excited by the new stimulus to Marxist thinking in the work of Georg Lukács and Karl Korsch. In 1923, supported by Weil's wealthy father, Horkheimer, Pollock, and Weil founded the Institute for Social Research to provide an independent scholarly setting for the pursuit of their radical goals.

Lowenthal soon joined them, as did the economist Henryk Grossmann and the Sinologist Karl A. Wittfogel. Although loosely associated with the University of Frankfurt and maintaining informal ties with several of the leftist parties, the institute was able to achieve an autonomy that it maintained throughout its history.

Horkheimer was too young to become the institute's first director, a post that went to the Austrian Marxist Carl Grünberg, because the director also had to have a chair at the university. But in 1930, when Grünberg's health had deteriorated, Horkheimer had completed *Anfänge der bürgerlichen Geschichtsphilosophie* (1930) and was thus able to assume a professorship in social philosophy, and with it, the institute's leadership. From that date the history of the Frankfurt School may be properly traced. In his first three years as director, the institute started a new journal, the *Zeitschrift für Sozialforschung* ("Journal for Social Research"), opened branch offices in Geneva and London, and welcomed several new members, most notably the psychoanalyst Erich Fromm and the philosopher Herbert Marcuse. It began as well to gather materials for its first collective project, a study of the relations between authority and family in the modern world, which was designed in part to explain the proletariat's inability to assume the revolutionary role assigned it by orthodox Marxism.

By the time the *Studien über Autorität und Familie* (1936) appeared, the institute itself had been victimized by that inability. With the Nazi seizure of power in January 1933, Horkheimer and his colleagues, most of whom were Jewish as well as socialists, were forced to flee. After a year in Geneva they went to New York, where they established a loose affiliation with Columbia University. In that same year, a collection of Horkheimer's aphorisms appeared pseudonymously under the pointedly ambiguous title *Dämmerung* (1934), the German word for both dawn and twilight. Written when the fate of the Weimar Republic was still uncertain, they expressed a militancy that would wane in his later writings.

In New York the institute was able to resume a large part of its earlier work. The *Zeitschrift* continued to be published in Paris until 1939, and then for two more years in the United States under the title *Studies in Philosophy and Social Science*. New members joined the institute's ranks, most notably Franz L. Neumann, Otto Kirchheimer, and, still in his Paris exile, Walter

Benjamin. Although the late 1930s saw the departure of Fromm and the loosening of ties with Grossmann and Wittfogel, Adorno joined the institute officially in 1938. For Horkheimer, his collaboration was to be of particular importance; in later years, they would be more closely identified with the Frankfurt School than would any other institute members.

During the institute's first years in exile, Horkheimer spelled out the tenets of Critical Theory in a series of articles in the *Zeitschrift*, later collected in book form (1968). Drawing on the Hegelianized Marxism introduced by Lukács and Korsch, but hostile to its inherent identity theory, loyal to the classical German idealist notion of reason, but wary of its disdain for concrete human happiness, open to insights from psychoanalysis, but disturbed by its tendency to naturalize the contradictions of bourgeois society, "Critical Theory" opposed itself to what Horkheimer called "Traditional Theory." Most important, it claimed that the goal of theory was social emancipation, not abstract truth, and thus that the theorist could not be detached and "objective," as traditional theory contended he should. Although reluctant to endorse conventional metaphysics, Horkheimer was particularly scornful of positivism, which he accused of accepting the reified "facts" of the *status quo* as eternal and immutable. Instead, "Critical Theory" stressed their historical and therefore changeable nature.

By the 1940s, however, Horkheimer and his colleagues had grown more cautious about the possibilities of significant change. In two books written after he left New York for California in 1940, *Dialectic of Enlightenment*, coauthored by Adorno (1947), and *Eclipse of Reason* (1947), Horkheimer voiced his increasing pessimism about the current situation. Moving beyond a strictly class analysis, he stressed the importance of instrumental rationality and the domination of nature as central elements of Western culture from its beginning. Fascism could be explained in part as a revolt of dominated nature against its subjugation. Even traditional Marxism, Horkheimer suggested, had not entirely escaped the Enlightenment's ambiguous dialectic.

As his hope in the proletariat's ability to reverse this unprogressive progress of domination waned, Horkheimer increasingly stressed the importance of rescuing and nurturing critical, negative, and utopian impulses wherever they might be found. In an increasingly bleak anal-

ysis of what he called our "administered world," Horkheimer came to identify these impulses more with artistic and philosophical than with directly political phenomena. Most art, to be sure, was manufactured by what he and Adorno called the "culture industry," and most philosophy was a variant of positivism or idealism, but there were certain counterexamples that allowed some hope for the ultimate emancipation of mankind from the sinister dialectic of the Enlightenment. Ironically, they were most likely to be those cultural phenomena, such as modernist art and "Critical Theory" itself, that eschewed any direct and immediate political efficacy. For Horkheimer and the Frankfurt School in general, there was no viable political practice that could lead to qualitative change, at least for the foreseeable future.

In their empirical work, Horkheimer and his colleagues probed the reasons for this pessimistic prognosis. Using methods they had first employed in their studies of fascism in Europe, institute members joined with American researchers to explore the potential for a similar disaster in America. Under the auspices of the American Jewish Committee, whose director for scientific studies Horkheimer became in 1943, the institute helped write a five-volume series, *Studies in Prejudice* (1949–1950). The central volume was *The Authoritarian Personality*, co-authored with the Berkeley Public Opinion Study Group, which quickly became a controversial classic of social scientific research. Although the institute's more radical impulses seemed to be submerged in the predominantly psychological methodology of the work, the social origins of the phenomena under discussion were not slighted in the sections of the volume written by Adorno and inspired by "Critical Theory." Read in conjunction with the more theoretical writings of the late 1940s, *The Authoritarian Personality* did little to dispel the sense of mounting despair that ran through the Frankfurt School's work. Although the study did spell out the components of a nonauthoritarian personality syndrome, it never ventured an estimate of the relative prevalence of each. Even with the defeat of fascism, Horkheimer and his colleagues were not sanguine about the direction in which the world was moving. What Marcuse was later to make famous as "one-dimensional" society meant that even in the seemingly liberal democracies of the West, true emancipatory impulses were suppressed.

Although Horkheimer had felt grateful to his

American hosts, he recognized that "Critical Theory" had little chance of breaking through the prevailing philosophical and cultural discourse of this country after the war. He was thus willing to consider, and finally accept, the blandishments of the city of Frankfurt to return with his institute and begin where he had left off in 1933. Accompanied by Pollock and Adorno, he reestablished the institute in its original home in 1950. From 1951 to 1953 he served as rector of the university. Although returning to America sporadically for the next five years to act as guest professor at the University of Chicago, Horkheimer quickly established himself as a leading intellectual presence in the Federal Republic. In 1959 he became professor emeritus and moved in the following year to Montagnola, Switzerland, where he lived until his death in 1973.

The last decade of Horkheimer's life was perhaps his most controversial. While many of the institute's students were rediscovering his earlier and more radical works, he was becoming increasingly concerned with the totalitarian threat to individualism. His lifelong interest in Schopenhauer came to the surface and pessimistically colored his later writings, collected in *Gesellschaft im Übergang* (1972a), *Sozialphilosophische Studien* (1972b), and the posthumously published edition of *Dawn and Decline* ([1934] 1978). A new concern for religious issues, in particular for Jewish questions, also emerged in ways confusing to his more politically minded disciples in the German New Left. Although he was spared the direct harassment suffered by Adorno and their younger colleague Jürgen Habermas, Horkheimer was accused of betraying the link between theory and practice that "Critical Theory" had originally stressed. Ironically, conservative elements in German society soon afterwards berated the Frankfurt School for fomenting the student unrest that led to the terrorism of the 1970s.

In neither case was the subtlety of Horkheimer's position fully appreciated. For although he condemned the instrumentalization of theory in a way that seemed to preclude virtually any political activism, he never abandoned his hope for a future practice that might satisfy his yearnings for what he called "the entirely other" (*das ganze Andere*). He was thus never as resigned as his students claimed, nor as irresponsible as his establishment detractors charged. And yet both were correct in sensing the impasse at which his version of "Critical Theory"

had ultimately arrived. Schopenhauer and Marx were not, after all, very compatible bedfellows. It is, however, the special good fortune of masters of intellectual schools to have students who develop their ideas in new and creative ways. In the work of Jürgen Habermas and other still younger "Critical" theorists in Europe and America, Horkheimer's influence remains very much alive.

MARTIN JAY

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HOTELLING, HAROLD

Harold Hotelling (1895-1973), a pioneer in statistical theory and mathematical economics, began his career as a journalist. After completing high school in Seattle, he worked on small weekly newspapers in the state of Washington in 1915-1916, then took up journalism as an undergraduate at the University of Washington, where he earned a bachelor's degree in 1919. While completing that program he took several mathematics courses with Eric Temple Bell. Under Bell's influence, he undertook graduate study in mathematics, obtaining an M.A. degree at Washington in 1921. He went to Princeton

University from Seattle and completed a doctorate in mathematics in 1924.

From the beginning of his study of mathematics, Hotelling was interested in its applications. He once said that he had hoped to study both statistics and mathematical economics in his doctoral program and was surprised to find that extensive offerings in these subjects were not available in the United States in 1921. His own career both as a researcher and as a teacher did much to overcome those deficiencies.

After completing his doctorate at Princeton, Hotelling went to Stanford University's Food Research Institute, where he remained until 1927. It was here that he published the first of the many scholarly papers that brought him international recognition. His early interests in the application of mathematics included the fields of journalism and political science, population, and food supply; he then turned to mathematical economics while simultaneously producing fundamental research in the developing theory of statistics.

Hotelling was associate professor of mathematics at Stanford University from 1927 to 1931. During this period he spent six months with R. A. Fisher, then the dominant figure in mathematical statistics, at the Rothamsted Agricultural Experiment Station in England. This visit reinforced and further developed Hotelling's interest in mathematical statistics. In 1931 he moved to Columbia University as professor of economics, remaining for 15 extremely productive years during which he became an internationally recognized authority in both mathematical economics and statistics. In 1946 he moved to the University of North Carolina, founding the department of statistics on the Chapel Hill campus, where his uncanny ability to recognize talent in others led him to attract an outstanding faculty. He later became also professor of economics, Kenan professor of statistics, and was Kenan professor emeritus from his retirement in 1966 until his death in 1973.

Although Hotelling published a few papers in theoretical mathematics (1925c; 1926), the bulk of his teaching and research efforts was devoted to statistics and mathematical economics. Even in economics he published a relatively small number of papers, but they had a major impact. Hotelling's contributions to economics may be classified under five headings: optimization over time, imperfect competition and location theory, the incidence of taxation, demand theory, and welfare economics.

One of Hotelling's first papers (1925*b*) dealt with the problem of finding a logical basis for methods of depreciation. The problem was solved by relating depreciation methods to profit maximization. The paper also suggested that the calculus of variations could be applied to problems of temporal optimization in economics including the problem of depreciation, but this suggestion was not acted on in this paper. A later paper (1931*a*) dealt with the economics of exhaustible natural resources. In this paper the calculus of variations was used to obtain the socially optimal rate of exploitation of irreplaceable natural resources. After decades of neglect, this paper has received a good deal of attention since the petroleum crisis of 1973.

A single paper (1929*b*) made contributions to the theory of imperfect competition and to the theory of the location of economic activity. Some earlier economists had argued that oligopoly situations were inherently unstable—that is, that an equilibrium could not be reached and maintained. Hotelling developed an oligopoly case that attained a stable equilibrium, thus showing that oligopoly equilibrium was possible although it need not hold in every oligopoly situation. The case that was developed involved two competing firms connected by a linear thoroughfare (main street in a small town or a transcontinental railroad). Hotelling demonstrated that profit maximization would cause the two firms to locate near each other on the thoroughfare, rather than at the quartile points, which would minimize transportation costs for the customers. This aspect of the paper has made it interesting to scholars of the locations of economic activity. This tendency of competitors to draw near each other was generalized by Hotelling to explain similarity of competing products and of the platforms of political parties.

Hotelling then studied Edgeworth's (1897) argument that an excise tax on one of two related goods may result in a lowering of the prices of both goods. This thesis had produced a seemingly consistent and reasonable numerical example in which price reduction for both goods followed the levying of an excise on one of them. Edgeworth regarded this as a mere "curiosum." Hotelling investigated this phenomenon and found that it could occur if the two goods were substitutes in both consumption and production; he thus raised the phenomenon above the category of a curiosity (1932). In this paper he also developed a measure of the social loss due to the levying of an excise tax. The measure

was to be developed further in his work on welfare economics.

Hotelling also worked out the mathematical characteristics of demand functions under the assumption that the consumer maximizes satisfaction subject to a budget constraint (1935). This work was in the same general area as that of J. R. Hicks and R. G. D. Allen, and it came at about the same time as their work and the discovery of Eugen Slutsky's work. Since the 1950s this type of material has been taught to virtually all first-year graduate students in economics, but in 1935 it was considered advanced economics.

Perhaps Hotelling's most noteworthy contribution to economics was in welfare economics (1938). For some time economists had been aware that pricing at marginal cost was advantageous from the social viewpoint. Hotelling provided a mathematical demonstration of the validity of the principle of pricing at marginal cost. This principle, together with its variations and limitations developed by others, forms an important part of the theory of welfare economics. Hotelling also refined the concept, first developed in 1932, of the social loss due to levying an excise tax.

When Hotelling began his career, mathematical economics was regarded as a recondite specialty. He lived to see economic theory established on a firm mathematical basis. Through his teaching and research efforts he contributed a great deal to this reformation of economic theory.

His role in the dramatic development of mathematical statistics in the United States since 1930 was also a most significant one, involving important contributions in diverse areas. For example, his early work on differential equations subject to error (1927*b*) pioneered in a field of great subsequent, and indeed current, research interest. His work in the nonparametric area involving rank correlations (Hotelling & Pabst 1936) stimulated much later research, as did that on statistical prediction (1940*a*) and on experimental determination of the maximum of a function (1941).

The work for which Hotelling will be especially remembered, however, is in the field of multivariate analysis, an area in which he became the acknowledged leader. He was responsible for a number of fundamental developments in this specialty, which is concerned with the treatment of the simultaneous measurement of multiple characteristics (vector random varia-

bles). His first major contribution involved a "measure of deviation" now known as "Hotelling's T^2 ," by which two populations may be compared by the simultaneous comparison of a number of their characteristics. This generalized the "Student- t -statistic" introduced earlier to compare single characteristics of two populations.

In 1933 Hotelling developed the idea of "principal components analysis" (a concept introduced originally by Karl Pearson). Basically this method describes the dispersion of n points in a k -dimensional space by a convenient transformation giving new orthogonal axes and is now a widely used method of multivariate analysis.

Canonical correlation analysis developed by Hotelling (1936a) provides another method for describing associations between two sets of random variables. In this method, the two linear combinations of the variables of each set are found for which the correlation is maximum. Then the process is repeated among the linear combinations that are orthogonal to those already considered, and so on. In this way a set of "canonical variates" may be formed and natural measures of association obtained.

Hotelling's contributions to the teaching of statistics were also noteworthy. His generous manner extended to all situations, including the classroom, where his students became genuinely involved in the subject. His informal style, encouragement of class discussion, and his clear authority as a leader in the field instilled in students a keen desire to perform at their best.

Hotelling's influence on the teaching of statistics reached far beyond his personal classroom involvement. He gave deep consideration to the general issues surrounding the teaching of the subject, being especially concerned about the qualification of instructors, balance between theory and practice, and the organization of statistics instruction on a university campus. Many of his views on these subjects are contained in a famous paper (1940b) delivered first as an address at the 1940 annual meeting of the Institute of Mathematical Statistics. It is indicative of Hotelling's almost prophetic nature that many of the issues he raised are still discussed.

Hotelling was a member of the National Academy of Sciences and of the Accademia Nazionale dei Lincei, an honorary fellow of the Royal Statistical Society, a distinguished fellow of the American Economic Association, and a

fellow of the Econometric Society, the Institute of Mathematical Statistics, and the Royal Economic Society. He served as president of the Econometric Society in 1936/1937 and of the Institute of Mathematical Statistics in 1941. He received an honorary LL.D. degree from the University of Chicago in 1955, an honorary D.Sc. degree from the University of Rochester in 1963, and, in 1972, the North Carolina award for science.

RALPH W. PFOUTS AND M. R. LEADBETTER

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HOWELLS, WILLIAM W.

Characteristically in America, the anthropological view of *Homo sapiens* is one incorporating prehistory and physical (biological) anthropology as well as cultural and social anthropology. William White Howells exemplifies that tradition from the perspective of physical anthropology. He was born in New York City in 1908, the son of John Mead and Abby Macdougall White Howells and the grandson of the novelist William Dean Howells. He married Muriel Gordon Seabury in his senior year at Harvard College. His concentration as an undergraduate was anthropology, and this focus continued on through his PH.D. at Harvard University in 1934. Earnest A. Hooton had made Harvard the United States center for academic training in physical anthropology after World War I, and Howells' entry into the field was attributable to Hooton's stimulating presentations of anthropology to

undergraduates and to the public. Some of Howells' professional interests in physical anthropology, for example quantitative methods in dealing with morphological variation in populations, derive from the approaches that Hooton pioneered at that time.

Apart from three years of wartime service in Washington in the U.S. Office of Naval Intelligence, Howells has held only three professional positions. The first was in the American Museum of Natural History in New York City, where he, along with Hooton's first doctoral student, Harry L. Shapiro, investigated racial variation in Pacific island populations. In 1939 he accepted a teaching position at the University of Wisconsin at Madison, where he remained until he returned to Harvard on the death of Hooton in 1954. He retired in 1974. At both Wisconsin and Harvard, Howells excelled in undergraduate instruction. At the former institution he was a leader in the development of the integrated liberal studies curriculum, and in the latter university he originated one of the most popular courses in the natural sciences division of the general education program. In doctoral training at Harvard, he has produced more than a score of physical anthropologists whose strong methodological training under him is displayed in the diverse subject matter in physical anthropology that they have pursued in their research (Giles & Friedlaender 1976).

An engaging lecture style has carried over to his popular books, which cover primitive religion in *The Heathens* (1948), world archeology in *Back of History* (1954), and physical anthropology, with an emphasis on fossil man, in *Mankind So Far* (1944), followed by a book along similar lines, *Mankind in the Making* (1959). These books, widely used as texts, have received extensive critical praise in the profession. Their translations, variously, into French, German, Spanish, Japanese, Dutch, and Arabic, have made Howells the most diverse and accessible American physical anthropologist explicating the discipline.

Howells was elected to the National Academy of Sciences and the American Academy of Arts and Sciences, and, in 1954 received the Viking Fund medal in physical anthropology. He was editor of the *American Journal of Physical Anthropology* from 1950 to 1954, and in 1951 was president of the American Anthropological Association, a position reflecting his esteem in the general anthropological community.

Howells' early research was spread among

topics common to physical anthropologists: technical reports on modern human skeletal populations recovered by archeologists, refinements of measurement techniques in osteology, and the use of anthropometry, blood groups, and other traits to determine racial groups and their history and interrelations. The main themes that characterize his research developed out of these interests. The studies perhaps most clearly associated with him in physical anthropology are innovative elaboration and application of multivariate statistical techniques to problems of morphology: understanding individuals in terms of physique, living populations in terms of anthropometric measurements, fossil specimens in terms of affinities, and world populations in terms of cranial variation.

Howells' involvement in primary description of fossil material has been minor but significant (e.g., the Kanapoi humerus fragment from east Africa); his syntheses and reviews in paleo-anthropology have been acclaimed as authoritative and even-handed. His research has been most extensive and influential in the examination of human cranial variation, culminating in "Cranial Variation in Man: A Study by Multivariate Analysis of Patterns of Difference Among Recent Human Populations" (1973a). For this analysis, Howells first collected data from 17 well-defined populations representing all regions of the world. This required him personally to take more than 100,000 measurements in various museum collections. His thorough multivariate discriminant analysis of these cranial dimensions convincingly demonstrated that such variation is not, as he once put it, merely a taffy-pull with the skeleton of the head. Howells' work provides at least as appropriate a base for assessing the relationships among the world's populations as any other means, and is a major contribution to studies of prehistory as well as to physical anthropology. Cranial variation has been the subject of attention, often inconclusive or at worst irresponsible, in physical anthropology for more than a century. No one has succeeded like Howells in bringing credibility and modern evolutionary thinking into such investigations.

From his earliest papers, Howells has evinced a particular interest in the populations and prehistory of Oceania. He has visited almost all of the major Oceanic groups and conducted research in the Solomon Islands and elsewhere as part of a medical-human biology research team organized by Albert Damon. Among the

modern populations of this region, Howells has emphasized the concept of biological distance, a measure of interpopulation affinity in a restricted area based upon multivariate analysis of similarities and differences in single-gene traits (e.g., blood groups) and polygenic ones (e.g., finger- and handprints). Such studies can be complementary to, or include, similar investigations of linguistic, geographic, and other measures of relationship in determining patterns of colonization and the dynamics of population interaction in more recent periods.

In *The Pacific Islanders* (1974), Howells has synthesized an immense amount of diverse and sometimes conflicting evidence from linguistics, social anthropology, prehistory, and physical anthropology to present a comprehensive picture of the indigenous people of the major regions of this area: Melanesia, Polynesia, Micronesia, and Australia. The evidence suggests to Howells two ancient population complexes: that of what he calls old Melanesia, extending into the Philippines as well as Australia, New Guinea, and other modern-day Melanesian islands, and the Proto-Mongoloids, diversifying later into all the island groups. Despite the great diversity of physical types in Oceania, Howells sees the ultimate derivation of these peoples from eastern Asia. In this volume as well as others, Howells is characteristically modest about his own substantial contributions of data and synthesis, preferring to emphasize the work of others and the problems that remain.

EUGENE GILES

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HUNT, J. McVICKER

Joseph McVicker Hunt, American psychologist, has contributed to education and public policy as well as to his own field. He has influenced psychology primarily through syntheses of a wide range of ideas and data suggesting new directions for theory and practice. His work has been characterized by broad scholarship, innovative integration of ideas, and a concern with the application of knowledge to practical social problems.

Hunt was born in 1906 on a farm in Scottsbluff, Nebraska, and attended local public schools. As an undergraduate at the University of Nebraska in Lincoln, he sampled many courses, including biology, sociology, and economics, but without a clear focus of interests or a career goal. Psychology was not then prominent at Nebraska, although the philosophy department since 1899 had had a laboratory of psychology. In 1928, when Hunt was a senior, J. P. Guilford returned to direct the laboratory and to teach the general psychology course. Hunt enrolled in it and his interest in psychology, sparked by the course, was solidified by Guilford's invitation to pursue graduate study in psychology as his assistant. Although Hunt's later work in psychology does not reflect Guilford's direct influence, a concern for measurement and empirical verification may derive from the two years of graduate work under him.

To complete his studies, Hunt went to Cornell University in 1931 to work with Madison Bentley and, two years later, received his PH.D. for research on characteristics of the process of observing. Bentley's theoretical orientation did not have a long-lasting influence on Hunt's thinking, but their encounter may have sustained Hunt's interest in the problem of the determinants of both adaptive and disorganized human behavior. A quest for factors affecting behavior, particularly in the long term, is reflected in most of Hunt's work. At Cornell he also enrolled in several courses in neuroanatomy and physiology. These courses gave him the background necessary to follow neurophysiological literature, which he continuously attempted to integrate

with theoretical formulations stemming from observation of behavior.

Hunt made his first major contribution to psychology while teaching at Brown University. He organized and edited a handbook in the field of personality psychology that was published as *Personality and the Behavior Disorders* (1944). This two-volume compendium brought together psychoanalytic understandings of human functioning with behavioristic formulations, research on experimentally-induced disorders in animals with interpretations derived from clinical work in psychiatric settings, and discussion of biological, organic determinants of personality with cultural and experiential determinants in an effort to forge a cross-disciplinary, scientific approach to the study of personality. The impact of this work resulted from the counterpositioning of topics and viewpoints within the covers of one book, not from any integrating section written by the editor. As a whole, the work embodied the view that human personality is amenable to systematic assessment, rigorous study, and deterministic understanding.

Hunt brought to this undertaking considerable experience beyond his graduate training. He had been a postdoctoral fellow at the New York Psychiatric Institute, where he had learned of different approaches to the study of personality, and at the Worcester State Hospital, where he had been exposed to a lively group of professionals who had recently emigrated from Europe. There he had expanded his knowledge of psychoanalytic theory, gestalt psychology, and the Rorschach technique. At St. Elizabeth Hospital in Washington, he had pursued his interest in various methods of psychiatric treatment and had continued his study of Sigmund Freud's writings. Most important, he had been influenced by the group at Yale University led by Clark L. Hull, and including John Dollard, Neal E. Miller, O. Hobart Mowrer, and Robert R. Sears, and had adopted their conviction that clinical insights into personality dynamics could be translated into propositions consonant with the drive-reduction theory of behavior and rigorously tested. *Personality and the Behavior Disorders* managed to convey this view.

The book was widely used in the growing field of clinical psychology and brought Hunt recognition within the profession. He was elected president of the Eastern Psychological Association for the 1947/1948 term and of the American Psychological Association (APA) in 1950. He served as the editor of the important *Journal*

of *Abnormal and Social Psychology* from 1949 to 1955, and later was accorded various posts and honors in professional organizations. He received the distinguished contribution award of the APA's division of clinical psychology in 1973 and the G. Stanley Hall award of its division of developmental psychology in 1976.

In 1946 Hunt left the academic world to become director of the Institute of Welfare Research in New York. Believing sound evidence to be the best guide for policy, he proceeded rapidly to develop methods for assessing conditions under which social casework is effective (Hunt, Blenkner, & Kogan 1950; Hunt & Kogan 1950). It was one of the first attempts to evaluate the effectiveness of social intervention. In the process he discovered and disapproved of the political nature of social policy decisions and he began to look again to academia, moving in 1951 to the University of Illinois as professor of psychology. In later years his interest in the psychology of personality was expressed in collaborative work with Norman S. Endler on the conceptualization and measurement of personality traits (1968), but his main effort was directed elsewhere.

While still at Brown University, Hunt had begun work in the second area to which he has made significant contributions. He saw the possibility of using laboratory animals to investigate the effects of experience on behavior. With the collaboration of Harold Schlosberg and others, he carried out a series of studies on the effects of feeding frustration in infant rats on their later hoarding and eating behavior (Hunt 1941). The studies were designed with the implications of psychoanalytic theory clearly in mind. Although this research was interrupted by World War II and Hunt went on to pursue this issue through other means, the method of controlling the life histories of animals in order to study the effects of rearing conditions on later functioning became the mainstay of a lively area of research.

Hunt pursued the question of the role of experience in ontogenesis through a critical examination of the existing literature and through field research. In 1961 he published *Intelligence and Experience*, in which he marshalled evidence against the twin assumptions that the level of adult intelligence as measured by the intelligence quotient is fixed at birth and that intelligence develops at a predetermined rate. In doing so, he brought together Donald O. Hebb's (1949) theorizing on the neurophysiological correlates

of information processing and problem solving with Jean Piaget's (1947) views on the course of cognitive development. From the evidence, Hunt argued for accepting the significance of continuous interaction between environmental demands and the problem-solving capacities of the organism, with optimal growth attributable to some degree of discrepancy between them. He suggested that intelligence might be usefully conceived as a hierarchically organized system of central processes involved in problem solving, processes that were constructed in the course of interaction with the environment. Since the optimal range and sequence of interactions for the development of intelligence has not been determined, he claimed that the potential level of average adult intelligence also remains unknown.

The viewpoint expressed in *Intelligence and Experience* differed considerably from the Hullian position on the attainment of competencies, but it did not merge with genetic structuralism either. Hunt seemed to be groping for a position giving greater importance to specific interactions with the environment than was acknowledged by structuralists, and one giving more initiative to the organism than was granted by learning theorists.

While reviewing the literature on the effects of experience, Hunt concluded that the prevalent conception of motivation needed revision as well. He came to recognize the limitations of the drive-reduction position, even though it meant a basic change in his own theoretical orientation. He tried to unite cognition and motivation through the concept of intrinsic motivation—that is, motivation inherent in information processing and action, independent of external reward. This conception was first sketched in 1960 and was elaborated in a Nebraska Symposium paper (1965) and in chapters of books published later. Hunt's concept of an active comparison process between input and internal standards resembles the notions of dissonance, discrepancy, and incongruity that became current around the same time. However, his developmental perspective encouraged him to consider the epigenesis of intrinsic motivation and permitted him to find a way to reconcile a feedback mechanism with hierarchically ordered change.

The study of the literature on the effects of experience led Hunt to make a major commitment to the field of developmental psychology. From his base at the University of Illinois, he began with students and collaborators a program

of research into cognitive and motivational development during infancy and early childhood. The search for opportunities to introduce development-fostering experiences experimentally led him to undertake a research project at an orphanage in Iran, where sequential groups of infants could be provided with progressively modified living régimes. The progress made by children growing up under different conditions was assessed not by psychometric tests, but by a set of ordinal scales constructed to permit evaluation of development in different domains of functioning (Užgiris & Hunt 1975). The results obtained in these studies convinced Hunt of the plasticity of early development, not only in terms of over-all rate but also in terms of a selective effect on specific aspects of competence (Hunt et al. 1976). He explicitly rejected Arnold Gesell's (1946) stress on maturational control of behavioral development and implicitly questioned Piaget's notion of holistic stages in early functioning.

On the basis of the evidence for behavioral plasticity, Hunt suggested that the concept of a "norm of reaction" used in genetics was of much greater potential value to psychologists than the concept of heritability. Not only did he reject Arthur R. Jensen's (1969) conclusion that individual differences in intellectual functioning are largely genetically determined, but he also argued that psychology lacks knowledge about the range of achievements open to particular human genotypes. In 1967, as a result of his increasing concern with the educational aspects of child rearing, he changed his position at the University of Illinois to become professor of psychology and early education.

Intelligence and Experience was perhaps Hunt's most influential work. It acted as a catalyst on research concerning intellectual development, in shifting the orientation from psychometric to Piagetian. Moreover, it indicated the need for an educational psychology of infancy and early childhood, apparently the period of the most rapid intellectual development. Its implications matched the national inclination in the 1960s to do more for the poor and the underprivileged. When Project Head Start was launched, Hunt was drawn into the national advisory network for early education. In 1966 he was invited to chair President Lyndon B. Johnson's Task Force on Early Childhood, which in its report "A Bill of Rights for Children" recommended such subsequently implemented programs as neighborhood parent and child centers

and Follow Through classrooms (White House Task Force 1967). In 1967/1968, he served as director of the National Laboratory for Early Childhood Education Coordination Center. He continued to write and lecture on the relation of experience to development and educational achievement in school. A selection of these papers was published as *The Challenge of Incompetence and Poverty* (1969).

Throughout Hunt's career, theory and practice were intimately related. He used incidental observations to confront theoretical beliefs, and he was mindful of the practical implications of theoretical ideas. When he was working on the psychology of personality, he was also involved in devising ways to assess the outcome of the interventions that constituted psychotherapy and social casework. When concerned with the study of intellectual development, he was also involved in the evaluation of educational programs for infants and young children. His influence comes largely from breadth of perspective, a willingness to modify his beliefs on the basis of evidence, an ability to integrate and reformulate existing information to suggest new paths for the field, and, not the least, from enthusiasm about the value of psychological knowledge.

INA Č. UŽGIRIS

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JAKOBSON, ROMAN

Roman Jakobson was born in Moscow on October 11, 1896. In 1914, he enrolled in the philological faculty of Moscow University, where he joined a number of fellow students in founding the Moscow Linguistic Circle, of which he was president from 1915 to 1920. The circle included among its members persons from a variety of backgrounds: linguists, philosophers, students of literature, and several poets. Its purpose was "the elucidation of linguistic problems of both practical and poetic language as well as questions of folklore and ethnology" (*Selected Writings*, vol. 2, p. 531; hereafter cited as *Writings*). In the 1920s, the circle's interests became more literary, and together with the Society for the Study of Poetic Language in Leningrad, it was instrumental in the development of the important school of literary criticism known as Russian Formalism (Erlich 1955, pp. 44 ff.).

With regard to linguistics, the circle represented a reaction to the then dominant neogrammarian school, which had come into being in the late 1870s in the wake of a number of spectacular breakthroughs that clarified many obscure points in the phonological evolution of the Indo-European family of languages. These advances so deeply impressed workers in the field that for close to half a century the majority of linguists devoted most of their energies to the search for sound correspondences among cognate languages and to the reconstruction of forms in temporally ever more remote protolanguages. With the passage of time, attention naturally began to shift to those many other aspects of

language that had been slighted during the preceding period. Jakobson and his colleagues in the Moscow Linguistic Circle were among the scholars to whom the questions left unanswered by their teachers and their predecessors appeared more interesting than what was already known. Looking back in 1962 to this early period, Jakobson wrote: "Though the linguistic textbooks of our college years used to define language as an instrument of communication, chief attention in these manuals was paid to the pedigree [historical origins] of its *disjecta membra*. No answer appeared to the crucial questions: how do the diverse components of this tool operate? What is the multiform relationship and interplay between the two sides of any verbal sign—its sensuous, perceptible aspect . . . *signans* . . . and the intelligible or, properly, translatable aspect . . . *signatum* . . . ?" (*Writings*, vol. 1, p. 631). Since the linguistic literature was silent on these questions, the young researchers were forced to look elsewhere, and it was the experimental procedures of the avant-garde artists of the 1900s that provided Jakobson with a clue to the answers to these questions. He was particularly impressed "by the pictorial theory and practice of cubism, where everything is based on relationship and interaction between parts and wholes, between color and shape, between the representation and the represented" (*ibid.*, vol. 1, p. 632). And it was in the study of the productions of the Russian poets, especially those of Velemir Khlebnikov and Vladimir Mayakovsky, whose bold linguistic experiments were the counterparts of the cubists' experiments with visual forms, that the new

methods were first developed and tested. According to Jakobson, "poetic language, disregarded by neogrammarian doctrine but presenting the most patently deliberate, goal-directed, and integrated linguistic species, was a field that called for a new type of analysis and particularly required us to study the interplay between sound and meaning" (*ibid.*, vol. 1, p. 633).

What attracted Jakobson's immediate attention in the linguistic experiments of the poets was that not all phonetic components were treated on a par, but that a special status appeared to be assigned to those sounds and sound qualities which by themselves were capable of signaling that two utterances represented different words. (Ferdinand de Saussure had made similar observations in his lectures in the early 1900s. Jakobson became acquainted with Saussure's ideas in 1917 through the reports of the linguist Sergei Karcevsky, who had studied in Geneva.) This distinction among phonetic entities provided Jakobson with the means to overcome the apparently endless accumulation of facts without rime or reason that characterized the study of the sounds of speech (phonetics) during much of this period. It directly led to the recognition that each language had its own system of distinctive sounds, its own *system of phonemes*. It was these insights that led Jakobson, in his essays on Khlebnikov, to suggest that "phonetic texture does not deal with sounds but with phonemes" and "to examine the basic prosodic elements (of Czech and Russian verse) from a phonological angle" (*ibid.*, vol. 1, p. 633).

In 1920 Jakobson moved to Czechoslovakia, where he remained until 1939. These years saw the full development of his scientific genius. The 134 items in Jakobson's bibliography that were published between 1920 and 1939 include studies on all of the major topics that have occupied Jakobson during his entire career. They include studies on the theory of linguistics, especially phonology and linguistic geography, contributions to the historical phonology of various Slavic languages, synchronic descriptions of phonological phenomena in different languages, including also their prosody, inquiries into morphology, investigations of medieval Slavic culture and literatures, especially Old Church Slavonic and medieval Czech, studies in folklore, sociolinguistics, poetics (especially metrics), literary criticism, and essays on the film, painting, and the theater. In addition, Jakobson was instrumental in the founding of the Prague Linguistic Circle in 1926, of which he was vice

president for the first 13 years. Many of the most important theoretical papers of the period are to be found in the eight volumes of *Travaux*, published by the circle. Among them, a considerable number were by Jakobson. During this period, he was in close contact with Nikolaj S. Trubetzkoy, professor of Slavic philology in Vienna, who also participated actively in the activities of the Prague Circle. As the correspondence between Trubetzkoy and Jakobson shows, the two scholars exercised a profound influence on each other, especially in the area where both were to make their most significant contributions: the development of the modern study of the sounds of speech, phonology (Jakobson 1975).

Having tested the usefulness of the phoneme concept for the description of various poetic devices (rime, alliteration, meter), the natural next step was "to attack . . . that aspect of language, which had been traditionally monopolized by the neogrammarians," the phonological evolution of a language. The monograph *Remarques sur l'évolution phonologique du russe comparée à celles des autres langues slaves* (1929, in *Writings*, vol. 1, pp. 7-116) represents such an attempt. Rather than follow the procedure that since the neogrammarian days has been used in almost all studies of historical phonology and enumerate the reflexes of each sound of the protolanguage in its (more) modern descendants, Jakobson presents the phonological system of proto-Slavic and discusses each of the steps (and intermediate systems) through which the protosystem has evolved into that found in modern Russian, for what is most important in Jakobson's view is not the sounds themselves but the structure of their interrelation.

The idea that the sounds of a language are not just an arbitrary aggregate of entities, but constitute a system with its own imminent structure appears to have first been suggested by the American linguist, Edward Sapir, in his 1925 essay, "Sound Patterns in Language" (*Selected Writings* . . . 1949, pp. 33-45). Jakobson and Trubetzkoy read this paper in 1928 and were much impressed with it (Jakobson 1975, p. 144). Like Sapir, they insisted on the importance of the patterns that were constituted by the phonemes of different languages. In a 1930 monograph, Jakobson wrote that phonemes constitute a system, and that even though a phoneme of one language may be physically identical with the phoneme of another language, the place that the two phonemes occupy in their

respective systems may be different (*Writings*, vol. 1, p. 151). While in Sapir the nature of the properties that made up the different patterns remained somewhat obscure, Jakobson and Trubetzkoy saw that the relationships between phonemes in different languages reflected well-known phonetic properties. For example, the consonant [č] as in *chew* differs from [j] as in *Jew* by the property of voicing, whereas [č] differs from [š] as in *shoe* by the property of affrication. They correctly pointed out that the same distinctive properties were to be found among other pairs of phonemes: e.g., [č] : [j] = [š] : [ž] = [s] : [z] = [f] : (v). It was noted that languages differed from one another with regard to the extent to which they illustrated particular contrasts. Thus, Russian differs from English in that it lacks the phoneme [j] and thus provides no illustration of the contrast [č] : [j]. Hence the position of [č] in the Russian consonant system is quite different from that of [č] in the English system. It was said in the 1920s and 1930s that the structure of the phonemic system of every language was determined by the oppositions (the distinctive contrasts) that its phonemes entered into. It was believed then that the oppositions themselves exhibited marked structural differences. Some, like voicing and affrication (the two contrasts illustrated above), were binary in that they admitted only two values: voiced : voiceless and affricate : fricative. Others like the so-called point of articulation, the place where the vocal tract is maximally narrowed in the production of the sound in question, could assume a multiplicity of values. The former (binary) oppositions were termed by Jakobson and other Prague school phonologists, correlative, whereas the latter (multivalued) oppositions were called disjoint.

While this relational model of the phonological system represented substantial progress over the traditional phonetic doctrines embodied, for example, in the international phonetic alphabet, it had a number of flaws. In particular, the oppositions utilized in the characterization of vowels were separate and distinct from those utilized in the characterization of consonants. There appears to be little motivation for this since both classes of speech sounds are produced and perceived by the same organs. Moreover, the distinction between disjoint and correlative oppositions, which has been introduced into phonology by Jakobson and Trubetzkoy, was more of a formalization of traditional practice than a move justified on its own grounds. In addition, Jakob-

son's studies of the utilization of phonetic features by the poets (meter, assonance, rime) had strongly suggested to him that binary features (i.e., correlative oppositions) occupied a special, privileged status. He, therefore, attempted to reduce all oppositions to a single binary type.

These efforts resulted in Jakobson's communication to the 1938 International Congress of Phonetic Sciences (*ibid.*, vol. 1, pp. 272-279), which breaks with an important tenet of almost all previous phonetic research that speech sounds are further undecomposable entities which can be characterized with the help of phonetic properties, much as chemical elements were once characterized with the help of such properties as valence, atomic weight, etc. Instead, Jakobson proposed that phonemes are nothing but complexes of features, much as chemical atoms are now seen as specific configurations of protons, electrons, etc. And just as the behavior of chemical atoms is directly determined by their subatomic structure, Jakobson shows that it is the feature composition of the different phonemes that determines their behavior in individual languages. Moreover, he uses the latter to make inferences about feature composition and about the nature of the features themselves where direct phonetic evidence is not unequivocal. Jakobson also shows that the multivalued dimension of points of articulations—the points of maximal constriction in the vocal tract—by means of which consonants have been traditionally classified can and must be replaced by a number of binary features and that this replacement results in a notable gain of exploratory power. Finally, he describes the hierarchical interrelations among features and directs attention to the need for a search for the invariant properties of the signal.

Although Jakobson's ideas, writings, and activities in connection with the Prague Linguistic Circle won him considerable international recognition, they also aroused noticeable opposition from older professional colleagues and administrators both inside and outside of Czechoslovakia. Because of this opposition it was only in 1937 that Jakobson received a tenure appointment in the Masaryk University in Brno (Jakobson 1975, p. 293, note 1). Jakobson was not fated to remain long in this position; the Nazi take-over of Czechoslovakia in the spring of 1939 forced him to flee to Scandinavia. He worked some months in Denmark, then proceeded to Norway, where he was warmly received by the local linguists and remained until the Nazi in-

vasion in April 1940, from which he escaped by walking across the border into Sweden.

It was in Sweden that Jakobson published *Kindersprache, Aphasie und allgemeine Lautgesetze* (1940, in *Writings*, vol. 1, pp. 328–401), his most popular and influential study. In this book, he attempts to justify further his conception of phonemes as complexes of binary distinctive features. In addition to the evidence previously adduced, Jakobson puts forth facts from the acquisition of language by children and its loss in aphasia. Jakobson argues that the facts of aphasia and language learning are governed by certain broad principles that imply that phonemes are complexes of binary distinctive features and that the same principles also account for the distribution of phonemes among the languages of the world. *Kindersprache* was not only an important step in the development of the theory of distinctive features, it was also one of the earliest linguistic studies of language pathology and in many of its insights anticipated the development of what now is known as neurolinguistics. Moreover, it continues to exercise a profound influence on studies of language acquisition by children, being widely quoted and discussed to this day (Luria 1977).

In June 1941, Jakobson came to the United States. Although structural linguistics had gained a firm position in the United States as a result of the work of Sapir and Leonard Bloomfield, the narrowly empiricist approach that became predominant in American linguistics in the 1940s contrasted sharply with Jakobson's flexible and far-ranging perspective, which excluded nothing from its purview on a priori grounds. These differences account in part for the rather inhospitable reception that was accorded Jakobson by some American linguists. An equally significant role was played by their concern—as one of them was to describe it years later—lest “positions (for which they had been trained and were eminently qualified) be snatched from under their noses and given to European refugees” (Hall 1969, p. 194, note 3). These “chauvinistic protectionists who launched quasi-ideological arguments in order to repress competition” (Jakobson 1973a, p. 17) contributed materially to the difficulties that Jakobson experienced all through the war years in obtaining a regular university appointment in linguistics.

Like several other European academicians who had taken refuge in the United States, Jakobson became a faculty member of the “uni-

versity in exile,” *École libre des hautes études*. Here, together with the Belgian Byzantinist, Henri Grégoire, Jakobson conducted a working seminar on the Russian medieval epic, the *Igor Tale*. This poem, which recounts a military disaster of a minor Russian prince of the end of the twelfth century, has come down to us in two late eighteenth-century copies of a single sixteenth-century manuscript which was burned in the Moscow fire of 1812. Certain contradictions and obscure statements in the account of the discovery of the burned manuscript prompted the French Slavist, André Mazon, to question the authenticity of the poem. Mazon suggested that like the Ossian poems published by James MacPherson and the archaic Bohemian poems published by Vaclav Hanka, the *Igor Tale* was a modern forgery. These doubts seemed unjustified to Grégoire and Jakobson, and their seminars at the *École libre* were devoted to a detailed examination of the questions raised by Mazon. These seminars showed that in the grammar and vocabulary of the poem, in its historical and geographical references, in its poetic conventions, in the customs and traditional beliefs reflected in it, and in its utilization of contemporary literary sources, there was nothing that was implausible or that conflicted with information available to us from documents of unimpeachable authenticity. Since much of this information was totally unknown in 1800, and since, moreover, there were serious flaws in Mazon's own argumentation, the seminar confirmed the authenticity of this masterpiece (*Writings*, vol. 4, p. 294; Grégoire, Jakobson, & Szeftel 1948).

The rapid expansion of Slavic studies in the United States that took place immediately after the end of World War II enabled Jakobson to obtain a regular professorship at an American university. In the fall of 1946 he became T. G. Masaryk professor of Czechoslovak studies at Columbia University. The cold war period of the late 1940s did not fail to affect him. He was denounced as an individual of doubtful loyalty by supporters of right-wing causes in America, whereas in the Soviet Union, where culture and education were suffering through the Zhdanov period, his activities were described in the following terms: “One of the founders of the structuralist direction in linguistics, R. Jakobson was in the beginning of the 1920s closely connected with the ‘Moscow Circle,’ which . . . was a fortress of formalism, that most reactionary

bourgeois movement which during the NEP years was attempting to poison the consciousness of the Soviet intelligentsia. It is significant that at present Jakobson has migrated to the USA, the stronghold of bourgeois reaction, the chief source of imperialist aggression" (Desnickaja 1949, p. 342).

In 1949, Jakobson left Columbia for Harvard University, where he taught until his retirement in 1967. From 1957 on, he held a concurrent appointment at the Massachusetts Institute of Technology. An immediate result of this move, which included most of Jakobson's graduate students at Columbia, was to give Harvard the leading Slavic department in the country. A large percentage of American Slavic scholars of the last quarter century were trained by Jakobson at Harvard, as well as a fair number of important linguists. What attracted students to him was not only his extraordinary knowledge, scientific imagination, and his dramatic lecture style; much more important were the close personal relationship into which he involved almost every one of his many students, the genuine interest he took in their scholarly efforts, no matter how elementary, and the assistance and encouragement he gave to all who came.

During the Cambridge period, he again took up work on several topics that had attracted his attention earlier. He extended his inquiries into Slavic mythology and folklore, and uncovered in this domain previously unnoticed relationships between the Slavs and other Indo-European peoples (*Writings*, vol. 4; Jakobson 1969).

Of even greater interest, though perhaps somewhat overlooked by the wider public, are Jakobson's contributions to sociolinguistics. These concern primarily the activities of the ninth-century Byzantine apostles to the Slavs, Cyril and Methodius. Jakobson showed that the apostles developed Old Church Slavonic as a separate liturgical language and fought successfully to have it recognized as the equal, for ritual purposes, of the three traditional languages of the Church, Hebrew, Greek, and Latin. This struggle, which anticipated by many centuries the linguistic battles of the Reformation, had enormous historical consequences, and furthered the religious, cultural, and political self-determination of Slavic peoples (Jakobson 1965).

Since the beginning of the 1960s, Jakobson has been publishing a series of analyses of poetic texts composed in a great many languages in different countries and time periods. The pur-

pose of these is to illustrate the crucial role that is played by the grammatic structure of the texts. These original analyses have elicited lively reactions from many quarters, from linguists and literary scholars as well as from writers and journalists. They have been especially influential in France, where a collection of these articles, plus a number of his earlier pieces on poetry, were published in translation in 1973 (Jakobson 1973*b*; *Writings*, vol. 5).

In the early years at Cambridge a very major part of Jakobson's research efforts were directed towards further development of the distinctive feature model that had formed the core of *Kindersprache*. What absorbed his particular interest in those days was the acoustic facet of the different features and the relationship between the acoustic signal and the motor events in the human vocal tract which produced the signal. Much of this information was being developed by Gunnar Fant, who at that time was working at the Massachusetts Institute of Technology. Fant worked out in great detail the systematic interrelationships between the articulatory gestures involved in the production of different speech sounds and the acoustical signal that these gestures elicit. In a special monograph Jakobson, Fant, and Halle (1952) catalogued for the first time these systematic interrelationships for all known phonetic features. The monograph also showed that the number of features that were required to characterize the sounds of all languages was quite small and that this same set of features was also fully adequate for characterizing the various rules in which speech sounds are involved. The last insight was of great importance for the subsequent development of generative phonology by Noam Chomsky, Halle, and others. Generative phonology was directly influenced by Jakobson in yet another respect. His 1948 account of the Russian conjugation (*Writings*, vol. 2, pp. 119-129), with its explicit recognition of the distinction between surface and underlying forms of words that are related to each other by means of a set of synchronic sound laws, provided an essential model for much subsequent work (Halle 1959; Chomsky & Halle 1968).

In the early 1950s there was great interest in the Boston scientific community in the ideas developed by the mathematician Norbert Wiener in his book *Cybernetics*, and in the formalization by Claude E. Shannon and Warren Weaver of the concept of information. Jakobson found in

these ideas much that was congenial. "I am convinced," he wrote in 1960, "that methods newly developed in structural linguistics and in communication theory, when applied to verse analysis, and to many other provinces of language, are capable of opening up wide perspectives for further coordinated efforts of both disciplines" (*Writings*, vol. 2, p. 579). Jakobson employed concepts of information theory in a description of the phonological pattern of modern Russian (*ibid.*, vol. 1, pp. 449–463), and he adapted the terms "code" and "message," and utilized them in discussion of linguistic phenomena (*ibid.*, vol. 2, pp. 130–148).

Communication is for Jakobson the nexus of all social phenomena, and with Claude Lévi-Strauss, he takes this insight quite literally: "One cannot but follow Lévi-Strauss's triadic conception that in society communication operates on three different levels: exchange of messages, exchange of commodities (namely goods and services), and exchange of women (or, perhaps, a more generalizing formulation, exchange of mates). Therefore linguistics (jointly with other semiotic disciplines), economics, and finally kinship and marriage studies 'approach the same kinds of problems on different strategic levels and really pertain to the same field'" (*ibid.*, vol. 2, p. 663). In Jakobson's scheme, linguistics occupies the central core because it deals with language, the human communicative medium par excellence. The science of semiotics deals with the communication of messages of all kinds, not only verbal messages, and thus includes linguistics as a special case; while the general theory of communication, the basis of all social science, studies exchanges of all kinds, not only that of messages, and includes, therefore, also anthropology and economics. Moreover, Jakobson sees important parallels between the genetic code and human language: "among all the information-carrying systems, the genetic code and the verbal code are the only ones based upon the use of discrete components which, by themselves, are devoid of inherent meaning but serve to constitute the minimal senseful units; i.e., the entities endowed with their own intrinsic meaning in the given code" (Jakobson 1973a, p. 50). He draws attention to a series of other similarities between the genetic code and the entities of phonology that lead him to the even more basic question: "whether the isomorphism exhibited by these two different codes, genetic and verbal, results from a mere convergence induced by similar needs, or whether, perhaps, the founda-

tions of the overt linguistic patterns superimposed upon molecular communication have been modelled directly upon its structural principles" (*ibid.*, p. 53).

MORRIS HALLE

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JASPERS, KARL

Karl Jaspers (1883–1969), a German philosopher, was, for most of his career, professor of philosophy at the University of Heidelberg. After the end of World War II and the collapse of the Nazi régime, he accepted a professorship at the University of Basel in Switzerland, where he completed his career. He was a special target of the Nazis, largely because his wife was Jewish. They were, however, allowed to live in seclusion in Heidelberg throughout the period of the Nazi rule.

It is interesting that Jaspers came to philosophy from medicine and psychiatry. Before medicine, he had studied law at the universities of Heidelberg and Munich in 1901 and 1902. For the next six years, he studied medicine at the universities of Berlin, Göttingen, and Heidelberg. In 1908, he passed the state exam to practice medicine and in February 1909, he was registered as a doctor.

Jaspers could very well have made his career in psychiatry. As it was he wrote a general book, *Allgemeine Psychopathologie* (1913), which went through several editions and was importantly influential. This background is surely significant for the nature of his first major philosophical work, namely the *Psychologie der Weltanschauungen* (1919), which, however, was not a clinical study of personalities, although he did this for such figures as Strindberg, Van Gogh, and Nietzsche, but was rather a typological study of possible kinds of world view. One unsolved problem for the researcher is to define Jaspers' relation to Freud. In his writings, Jaspers makes relatively sparse references to Freud's work, and most of them are on the derogatory side. It seems likely that Jaspers never made a careful study of Freud and was not attracted to his ideas. Perhaps Freud struck him as too biologically oriented.

In the 1920s, Jaspers was one of the few most prominent personalities in the University of Heidelberg community. He particularly interested sociologists because he was a special ad-

mirer and had been a close personal friend of Max Weber.

Jaspers' place in philosophy is linked to the concept of existentialism. Perhaps his best-known book is *Reason and Existenz* (1935). It is notable that the translator and publisher retained the German form of the word *Existenz*, which Jaspers treated as definitely a technical term. The usual German translation of "existence" would be *Dasein*, which for Jaspers was quite different.

In terms of most classifications of "schools" of philosophy, Jaspers belongs in the German "idealist" tradition, but this statement needs careful interpretation. Basically he was a Kantian, and in that role he attempted further to develop some of Kant's most important philosophical approaches and leads. In any case he was clearly not a Hegelian idealist; indeed he speaks of Hegel as having "built a tower of Babel where everything was absorbed into a reason which now had a sense far beyond that of Kant. This reason is 'mysticism for the understanding'; . . . its philosophizing wanted to be the absolute knowledge of an evolving rationality of all Being, the unity of the rational and the irrational" ([1935] 1955, p. 154). He also repudiates the other two "classical" idealists, J. G. Fichte and F. W. J. von Schelling.

Central to Jaspers' position was his treatment of the subject-object relation and his attempt to maintain a very careful balance between them. Indeed his conception of mysticism, whose possible authenticity he does not dispute, is of a state, if it can be attained, in which the distinction disappears. Indeed he insisted that such states could not be objectively proven beyond the assertions of those who claimed to have experienced them—surely a Kantian position.

In connection with this problem, Jaspers presented an illuminating discussion of the relationship between positivism and idealism. He speaks of both as closed systems of thought which squeeze out the element of freedom that was so essential to Kant. He treats positivism as the attempt to "absolutize" the objective, and idealism as the attempt to absolutize the subjective, sphere. Jaspers explicitly repudiates both attempts at "closure" and assumes a definitely pluralistic position, leaving open many possibilities, but at the same time being careful *not* to undermine the centrality of the subject-object relation.

Jaspers' underlying position here seems es-

entially correct and exceedingly important. To a social theorist, however, his treatment of these issues is in some respects disappointing, as in his identification of positivism with "materialism" and mechanism. Surely the object world in the context of the sciences includes a much wider range of object types than the "mechanistic." Surely such scientific objects as organic systems, including human organisms, are not now properly called "mechanistic," and certainly what we call *action* systems are also capable of objectification.

Then there is a particular difficulty in the status of what are often called "cultural" objects, such as documents with meanings or works of art. Cultural objects are *always* the products of human action, but they may exist both as objects external to individual selves—that is, they are "externalized"—and can be "internalized," becoming parts of the personalities of human individuals. Thus Jaspers' writings are cultural objects that can be found and observed in books, but as studied and understood, they become part of the student's personality and hence "subjective" to him. Of course this problem area, which includes "motives" as well as cultural objects, involves the problem of subjective *Verstehen*, which was so important to Weber, and which is also discussed in a variety of ways by Jaspers.

When from the purely "mechanistic" level through that of organic systems we reach the level of action, which is in principle *Verstehbar*, there is a double involvement of the category "subjective." This is to say that what are, epistemologically speaking, members of a class of objects must, to be understood in the cognitive sense, be treated as having "meanings" to concrete human actors, meanings that can be grasped only through processes of *Verstehen*. It is this complication that makes it so essential, in the analysis of action phenomena, to distinguish clearly between the "point of view of the actor" and that of an "observer." This of course should not be interpreted to preclude self-observation and self-understanding by any given actor. Jaspers treats the latter problem area quite extensively. It would be illuminating to compare his treatment of this complex with that of George Herbert Mead on the one hand and Freud on the other.

In order to avoid the misunderstandings that may arise in this field, it is necessary to supplement the underlying conception of subject and object and their relationships with a classifica-

tion of *levels* of empirically existent reality or being, which can, through human cognitive processes, in Kantian terms, come to be treated as objects. Such a classification would have a hierarchical structure, starting with the world of physical objects and going on to the organic world. Then, with the level of action, "consciousness" emerges, and with it a "subjective" point of view. Furthermore, as Kant so clearly worked out, this classification implies a dual structure of the components, not only of empirical knowledge, but of "orientation" more generally—in the cognitive case the combination of sense data and what Kant called the "transcendental" component, here the categories of the understanding.

Although even Weber did not work out a satisfactory hierarchical scheme of the organization of the object world in this sense, both he and Jaspers, leaning on Kant, did think in closely related terms that can be readily linked with such a scheme (most fully presented in Parsons 1978, chapter 15). In Jaspers' version it involves three components that in turn are related to "levels" in the above sense. The two limiting categories are directly related to the distinction between positivism and idealism mentioned above. They are "empirical existence" (*Dasein*) and "spirit" (*Geist*). The intermediate, and in an important sense mediating, category is "consciousness as such" (*Bewusstsein überhaupt*), which was a famous category of Kant's.

In this three-fold schema, consciousness as such comes to focus in the category of cognitive function. It is the point at which "motivational" factors, which are in a special sense "psychological," meet with cultural factors, which are clearly transpsychological. Since Jaspers wrote, much has come to be understood about these relationships, especially through improved knowledge of language. In particular, there is one German word that is translated as "reason" or, sometimes, "understanding"—namely *Verstand*. This is the reference of Kant's categories and also of Weber's very central concept of *Verstehen*. In most German literature, *Verstand* is explicitly contrasted with the term translated as "reason" in Jaspers' title, namely *Vernunft*. There is an important sense in which this concept is the key to Jaspers' extension of his consideration beyond the object world, in the sense in which we have used that term.

This extension Jaspers works out through the use of the striking concept that he calls "the

Encompassing" (*das Umgreifende*). The central concept is that any formed or "structured" experience of Being (*Sein*), whether of objects external to the self or of subjective experience, implies that there are limits bounding the range of that "structuring," and the concept of a limit implies that what is beyond the limit is not simply "nonbeing," to use the current philosophical term. Jaspers' exploration of the implications of the limit and what can conceivably lie beyond it constitutes his most distinctive contribution.

In developing these themes Jaspers used a basically Kantian strategy, "relativizing" without losing anchorage in firm philosophical commitments. In this connection Jaspers' statement (some would say "outburst,") against Hegel, quoted above, is a major key to understanding him. He objected above all to *Absolutizing*. But his extended discussion of skepticism and nihilism makes it quite clear that his relativizing did not go over to a totally "relativistic" position. On both the subjective and the objective sides he held firmly to the view that structured—indeed we may say "institutionalized"—orientations could in principle be regarded as fully "authentic"; they should not—for example, in the case of scientific theory—be treated as "fictional," as these were treated by Alfred Schutz.

Indeed, there is a notable section in *Philosophie* (Jaspers 1932*b*, vol. 1, chapter 7) in which he discusses the relationships of philosophy to religion, the intellectual disciplines (*Wissenschaften*), and art. Here he carefully distinguishes philosophy from theology. The latter is part of a religious system, whereas philosophy should be regarded as independent. Similarly, philosophy is not the summary integration of our intellectual knowledge of the objective world, but is distinguished by a distinctive approach or "orientation." A philosophy, finally, is not a "work of art," though some philosophers have attempted to achieve that status for the discipline.

Jaspers' central formula for the distinctiveness of philosophy is its concern with *reflection* about the meaning of human experience, both of objects of the various meaningful categories, and of the subject—the philosopher himself as a human person. It is in this context that the concept *Existenz* fits. Thus reason as *Vernunft* is the boundary concept for reflection about the object side of the subject-object distinction, whereas *Existenz* is the boundary concept on

the subjective side. It is most definitely not confined to treatment of the self as object but transcends that. Indeed, Jaspers introduces the concept of transcendence to denote the "final" boundary in what can be called the "telic" direction, which synthesizes the objective and the subjective. It is as the summit of whatever is conceived to be transcendent in this sense that Jaspers treats the concept of God.

Jaspers considered the need for reflection in the above sense to be the most distinctive feature of philosophy in his time. He devoted the first chapter of *Reason and Existenz* to Søren Kierkegaard and Nietzsche, stressing that they had in common the relentless pursuit of reflective exploration of the human condition. He endorsed, however, not the final positions at which either one arrived, but, rather, approved the example they both set in their reflections on the meaning of experience.

Jaspers seems to have been consistent in maintaining that the problem of analyzing the "encompassing" arose at *all* of the limiting boundaries of human experience. Thus physical science faces the boundary problems of "cosmology," touching the origins, scope, and nature of the physical "universe," which are actively discussed in astronomical and philosophical circles. In biology similar problems have arisen and been actively discussed about the origins and nature of organic life, ranging from the most primitive microorganisms to the status of man as organic species and to the possibilities of organic evolution beyond the human level.

Then man, as conscious being with knowledge not only of the outside world but also of self, becomes the focus of the reflective endeavor. After all, his *awareness* both of himself and of the world external to him, which includes other human individuals, can be meaningful only from the "perspective" of the human condition, and indeed, "in the last analysis," of particular individual human beings. Relative to the positivistic components of our cultural background this is perhaps the predominant note, brought in with German idealism but greatly sophisticated in Jaspers' work. The "world" is not simply "given" to the human observer and experiencer but must be "processed" and interpreted in complex ways if it is to "make sense." Indeed many, like the nihilists whom Jaspers discusses, have come to the conclusion that the world is "absurd" and have abandoned the attempt to make sense of it.

This is by no means Jaspers' position. It is not fortuitous that in his important title "Reason" is placed first, before *Existenz*. Again in something like the Kantian sense, Jaspers was a "rationalist." However, he qualified his position in this respect very carefully. He definitely did not share in a variety of movements of this century that might be called "anti-intellectual"—for example some branches of phenomenology (reference has been made to Schutz on the fictional character of scientific theory.) Indeed, for him it was only through reason, as integrated with *Existenz*, that another phenomenon, which he called *Kommunikation*, was conceivable. This integration of course occurred in the individual, who in one sense and at one level was conceived to be unique and "alone," being at the same time in a position to relate to others—potentially not only other humans but other "sentient" beings. This potential for relating involved *both* levels of the English concept "reason," which are differentiated in German terminology, namely *Verstand* and *Vernunft*, and some kind of synthesis between them.

In other words, to Jaspers, the other, with whom one communicates, must be *object* as well as subject, and he who communicates must also be an object to others and to himself. Their common and mutual "objectness" is, precisely in the Kantian sense, an essential ground or condition on which they can communicate with each other. But the possibility implies an integration, not only through the categories of understanding, that is *Verstand*, but also as related to "transcendental" considerations, as *Vernunft*, and mutual awareness of each other at the level of *Existenz*.

Jaspers also holds that the boundaries between areas known and experienced and the encompassing which lies beyond them should not be regarded as fixed. They may shift with a wide variety of changes in human society or culture, including the "advancement of knowledge," and with new forms of subjective experience that would not ordinarily be called knowledge, such as philosophical reflection of the sort that Jaspers practiced. Such reflection, however, can also help men adjust to the many changes in significant aspects of culture and of the human condition that are continually occurring in complex societies.

Thus Jaspers maintained a careful and judicious balance in at least two related fields. The first was the very central relation between subject and object. The second was that be-

tween rationality and those aspects of the humanly significant world that must be classed as non- or irrational. This balance of course includes that between reason and *Existenz*.

Still another was the balance between openness and flexibility on the one hand and rigidity of crystallization on the other. The latter is in particular a danger of rationalistic theories, as in philosophy, and also of institutions. It is notable that Jaspers, in discussing such rationalisms on both the positivistic and the idealistic sides, refers to them as producing *Gehäuse*. This is the same word used by Weber at the end of his study of the Protestant ethic, which Parsons translated as "cage" and which has come into the English-language discussion in the term the "iron cage." Jaspers' warning about the dangers of rationalistic *Gehäuse*, however, should not be interpreted to mean that he advocated a Heraclitean doctrine that "all is flux" and that such concepts as structure should be abolished.

Finally, another balance Jaspers was careful to maintain in subtle and delicate ways was that between individuality and the collective aspects of the human condition, which in turn involves a balance between the "historically" unique, bound to particular times and places, and the universal or general, independent of such particularities. Only particular individuals can practice the art of philosophical thinking, though philosophical tradition has become a major component of human culture more generally.

A particularly striking expression of this theme of both individuality and historical temporality was Jaspers' statement that, from one perspective, thinking philosophically could be considered "a way of learning how to die."

In the years following World War II, Jaspers wrote a number of relatively public statements on the moral and existential aspects of that grandiose historic episode and the involvement of Germany in it. His writings in this vein attracted widespread attention, including in the English-language world through translations. His voice was a voice of sanity which was unhesitating in its condemnation of this episode not only in German but in world history. At the same time his was one of the most serious attempts to understand what had gone wrong and why.

In his career as a philosopher, rather than a psychiatrist, Jaspers was the author of two major works. The first, *Psychologie der Weltanschauungen*, may be regarded as marking the

transition between the two phases of Jaspers' career. The use of the term "psychology" in the title is not particularly felicitous. In any usual sense it is not a psychological study, but rather one at the "general action" level of possible types of "orientation" in the human situation to what, following Weber, Jaspers called problems of the "meaning" of being human.

The second major work was his three-volume *Philosophie*, the first volume of which was published in 1932, just on the eve of the Nazi takeover. As the most systematic statement of his mature work, it may be noted that the main aspects were incorporated in the subtitles of the three volumes: *Philosophical Orientation to the World*, *The Clarification of Existenz*, and *Metaphysics*; the latter deals mainly with his concept of transcendence. For purposes of general interpretation, Jaspers' one short book, translated as *Reason and Existenz*, is particularly useful.

It is appropriate, perhaps, to conclude with the remark that Jaspers has a special place as a social scientist's philosopher. He was very careful not to merge the philosophical and the scientific enterprises, yet at the same time to treat them as intimately interdependent. A good scientist, then, especially at the level of general theory, must be aware of, and acutely sensitive to, the philosophical order or problems that lie on the boundaries of his field and intertwine with it.

A further word is in order concerning the description of Jaspers as a "social scientist's philosopher." Jaspers' grounding of his position in the Kantian tradition has become the focus of a major "metatheoretical" controversy in the social sciences. The controversy centers on the challenge from phenomenology to what may be regarded as the "main line" of sociological theorizing associated with the heritage of Émile Durkheim and Weber. This challenge concerns above all the status of the "subjective point of view" in relation to what Weber called the *Verstehen* ("understanding") of the intentions of human actors. This was a central problem area in the German intellectual world of Weber's day and had a substantial influence on Jaspers' thinking. For example, Jaspers gave special attention to the subject-object relationship. Concern for the philosophical side of the "subjective" aspect underlay Jaspers' emphasis on the concept *Existenz* and its complicated context. This can be regarded as an attempt to provide a further philosophical grounding for Weber's position on *Verstehen* and its

many ramifications. Specifically, in relation to the phenomenological challenge, theoretically minded social scientists would do well to give careful consideration to Jaspers' writings.

TALCOTT PARSONS

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JESSUP, PHILIP C.

Philip Caryl Jessup, born in 1897, has been one of the most esteemed and influential international lawyers of this century. As a professor of international law, a prolific writer, and a judge on the International Court of Justice in the Hague, he has produced studies and opinions on virtually all of the major issues in the international legal field. He has combined scholarship with active participation in public affairs, serving as a United States delegate to

the United Nations, as an ambassador-at-large, and as a prominent advocate of stronger international institutions based on law.

Jessup has not been a proponent of any grand theory; rather, in the common law tradition, he has addressed himself mainly to specific problems of current concern. In his writing and lectures, he characteristically relies on particular cases and on concrete facts to illuminate principles and procedures. He discusses cases with a relish for the odd fact and personal idiosyncrasies, conveying amid the abstractions and sonorities of legal doctrine, a sense of the strange ways of individuals. As a judge he went deeply into the facts of the case before him and into the origins and social function of the legal rules at issue. His presentation of concrete facts has often been accompanied by an emphasis on historical trends in law and society. He tends to perceive specific legal events in terms of evolutionary developments responsive to changing social needs and moral ideals. In this respect, a distinct teleological element is evident in his thought. The trends of international law are seen largely as a progressive development toward a more comprehensive and effective world legal order. The significant features of that world order include: recognition of the interest of the international community, protection of the basic rights of individuals, greater recourse to judicial procedures for resolving conflicts, the strict prohibition of the use of force except in self-defense, and the extension of international regulation in areas of interdependence. These social ends are virtually axiomatic for Jessup; he assumes their desirability and he is, on the whole, optimistic about their eventual adoption (Jessup 1948; 1959a).

Jessup's evolutionary optimism may be linked to the early influences in his career and to his personal involvement in the dramatic expansion of international institutions and international law in the period following World War II. Jessup's choice of a career in international law was suggested to him in 1918 by Elihu Root, America's most eminent international lawyer, a former secretary of state and a fervent advocate of international judicial settlement. Jessup was then a college senior, having returned to Hamilton College in upper New York state after service in the infantry in World War I. His war experience left him with a strong desire to take part in efforts to end wars. Root, then in residence on the Hamilton College campus, stimulated his interest in international law and put

him in touch with James Brown Scott, then editor in chief of the *American Journal of International Law* and an active proponent of international legal institutions. Jessup also consulted John Bassett Moore, the professor of international law at Columbia University Law School. Thus, even before entering Columbia Law School, Jessup received guidance and inspiration from the three acknowledged leaders of the field. These leaders shared a belief in the evolution of international society to a stage in which international law, and judicial and arbitral institutions in particular, would have the principal role in preventing war and settling disputes. At Columbia Jessup studied international law with Moore and Edwin M. Borchard, who was both scholar and practitioner, specializing in international claims. Jessup produced his first written piece, a note for the *Columbia Law Review* on maritime jurisdiction in regard to rum smuggling. That note led him later to select the subject of the law of territorial waters for a doctoral dissertation and his first book (1927). Thus begun, his interest in the law of the sea and related questions of resource exploitation remained with him throughout his career. After receiving a law degree in 1924 from Yale Law School (to which he had transferred for his third year), Jessup served as an assistant solicitor in the State Department under Charles Cheney Hyde. When Hyde returned to Columbia Law School in 1925, Jessup accompanied him and began teaching international law and doing graduate work toward a Ph.D. In 1927 he became a member of a New York law firm, an association that continued until 1943, although his activity in practice was sporadic. In 1929 he served as an aide to Root at the conference of jurists in Geneva on the revision of the Statute of the Permanent Court of International Justice. That same year he delivered lectures at the Hague Academy of International Law—an unusual distinction then for one 32 years old—on the exploitation of ocean resources (1929a). While on the Columbia faculty, he took leave in 1930 to serve as legal adviser to the United States ambassador in Cuba and later, during World War II, to work in the State Department and the United Nations Relief and Rehabilitation Administration (UNRRA) on postwar relief (Jessup 1944a; 1944b). Even earlier, Joseph Chamberlain of the Columbia faculty had stimulated his interest in international organizations. Jessup was an assistant to the United States delegation at the 1945 San

Francisco Conference on the UN charter. In 1946 he was named the Hamilton Fish professor of international law and diplomacy at Columbia University, a chair he held until 1960 when he became an "associate" of the Rockefeller Foundation. In 1947 he was appointed United States representative on the UN Committee on the Progressive Development of International Law and Its Codification, which drafted the plan for the International Law Commission. From 1948 to 1953, he also served as a United States representative at several sessions of the UN Security Council and General Assembly and as an ambassador-at-large (Acheson 1972). In 1960 he was elected by the UN to serve as a judge on the International Court for the period 1961-1970. After retirement from the International Court in 1970, he devoted himself to lectures, research, and writings; from time to time he gave legal advice to governments and private clients. He continued to play an active role in professional and scholarly societies, particularly in the American Society of International Law, of which he had been president (1954/1955), the curatorium of the Hague Academy of International Law, and the Institut de droit international, of which he was a vice president. Numerous honorary awards were bestowed on Jessup, and he delivered several series of lectures under distinguished auspices. These honors confirmed his long-standing preeminence in the community of American international lawyers (Kennedy 1962).

No account of Jessup's career would be adequate without mention of his extraordinary personal qualities. Endowed with a commanding presence, a resonant voice, a gift for direct and lucid expression, a lively mind, and human warmth, Jessup was a natural leader in conferences and organizations: "His wit, gay and ready, and his irrepressible humor would survive the longest and dullest meetings" (Acheson 1972, p. 6). He was much admired for speaking out on behalf of his ideals irrespective of governmental or popular attitudes. Within his wide circle of friends, professional colleagues, and students, he was known for generosity and innumerable kindnesses; he was always ready to devote time and effort to those who sought his help and counsel, and one must regard this as a significant part of his contribution to international law.

Jessup's intellectual contributions to international law fall into three categories. One category includes his ideas about the role of

international law, its aims, values, and historical trends. A second category contains his opinions on the content and interpretation of existing legal principles, rules, and concepts. In the third category are his ideas on measures and policies that should be taken to enhance the effectiveness of international law.

Within the first category, the most prominent ideas are those relating to the reality and social ends of international law, the application of the international "community interest," and the concept of transnational law. Jessup's concern with the reality of international law had a strong practical dimension. He felt that those who denied the reality of international law lacked practical knowledge of the workings of foreign offices and international bodies in numerous areas of interstate relations. Quoting Moore, Jessup observed that international law was as well observed as national law (Jessup 1948, p. 7), but he recognized that the absence of compulsory judicial settlement and the deep attachment to national sovereignty limited the use of law in international disputes. Although hopes for an early achievement of world rule of law were unrealistic, this did not mean that one should minimize the role of treaties and international custom in the conduct of the world's business in orderly and predictable fashion (Jessup 1959a).

Jessup's treatment of values in international law exhibits a sophisticated blend of positivism, idealism, and pragmatism. He is careful to distinguish positive law—the *lex lata*—from recommended or future law. In many of his writings, notably in *A Modern Law of Nations* (1948), Jessup was concerned primarily with indicating desirable future legal developments, making it clear that he was writing *de lege ferenda*. But it is noteworthy that he generally linked future law to concepts and ideals that had been accepted in existing law. Thus in advocating new rules and procedures, he related them to the principles and purposes of the UN Charter, especially those principles recognizing the equal rights of states and peoples, respect for the dignity of the individual, and the obligations of peaceful settlement and collective security. There was, therefore, no sharp break between existing and proposed law. Existing law was itself an authoritative source of the ideals that had been validated by international consensus and that therefore served as a basis for new law. Jessup also turned to those ideals for guidance in interpreting existing law, thus

transcending strict positivism. He did so with lawyerlike caution, aware that broad principles of social ends cannot be pressed too far without losing the consensus on which their authority rests. He was also conscious that in many cases conflicting principles had to be satisfied. These essentially pragmatic concerns led him, especially in his judicial opinions, to devote considerable attention to the particular facts of the case at issue and to the social function of rules in that and similar cases. Instructive examples of Jessup's treatment of the facts and of his functional approach can be found in his individual opinions in the International Court cases on the North Sea continental shelf (International Court of Justice 1969, pp. 67–84) and on Barcelona Traction (International Court of Justice 1970, pp. 162–221).

Illustrative of Jessup's careful value-oriented approach is his advocacy and use of the notion of the international community interest. In presenting the concept he observes that the basic idea was expressed in 1915 by Root, who called for a change in the legal theory that an international wrong could be redressed only at the instance of the nation injured. Root had argued that the maintenance of the international order was a community interest and that law violations that threaten peace and order should be treated as injuries to all on the analogy of domestic criminal law. Jessup suggested that Root's projected idea of community interest was adopted in some degree in the UN Charter, which allows all states to take up threats to peace and security (Jessup 1948, p. 11). He noted, however, that the application and limits of the international community interest remain uncertain, dependent on future decisions by states and international tribunals. Later, as a judge in the International Court of Justice, Jessup traced the notion of community interest to Article 11 of the League of Nations Covenant and on that basis supported the right of members of the League to invoke the jurisdiction of the Court even though their own interests or their nationals were not affected (International Court of Justice 1962, pp. 387–436). In the subsequent phase of the same case on southwest Africa, Jessup concluded (in a dissenting opinion) that the condemnation of apartheid by the General Assembly was decisive proof of contemporary international community standards which the Court had to take into account in deciding whether South Africa met its obligation under the mandate to promote the well-being of

the inhabitants of southwest Africa (International Court of Justice 1966, p. 441). Jessup was careful to avoid the thesis that the General Assembly may create new law by majority resolutions; his view was that the repeated resolutions of the General Assembly (adopted by near unanimity) expressed the international community's attitudes, and that such contemporary community attitudes were relevant in applying a legal standard, such as well-being, laid down in an earlier treaty. Jessup thus sought to give legal effect to the strong views against apartheid held by nearly all states without endowing the General Assembly resolutions with general legislative character.

The importance attached by Jessup to the idea of the interest of the international community did not mean that he regarded all international communities as universal. Particularly in his later writings, he took note of nonuniversal "selective" communities, joined by common interest, geography, or shared values. These diverse communities have been and are increasingly the basis of new legal arrangements; notable examples are the European Economic Community and the functional groupings concerned with maritime and economic arrangements (Jessup 1964; 1973a). Though Jessup recognizes the expanding role of nonuniversal communities, he has continued to stress the universal character of the basic principles of the Charter and of general international law. Thus he argues for the universal application of the rule against the use of force (except in self-defense) and against the idea that armed force may be used in wars of liberation or by third parties in civil wars (Jessup 1973a, pp. 423–429). He also considers that the law of state responsibility for injuries to aliens has—and should have—universal application in its essential aspects, noting that abuses by powerful states typified by "gunboat diplomacy" have now been eliminated (International Court of Justice 1969, p. 164; Jessup 1973a, pp. 420–421). In regard to the law of the sea, he asserts the need for general rules to safeguard navigation, fishing, and environmental protection, but he accepts the necessity of nonuniversal arrangements to manage resources in particular regions. His general approach is strongly value-oriented, underlining what he considers to be universally accepted ideas of order, responsibility, and justice, yet recognizing that diversity and special conditions create many different kinds of international communi-

ties that must have their own interests and legal arrangements.

Jessup is also commonly credited with the concept of "transnational law," a term he did not invent but developed and popularized in his Storrs lectures at Yale in 1956 (Jessup 1956*b*; 1973*b*). Through that conception, Jessup sought to show that the international legal realm could no longer be compartmentalized into its two classic divisions of public international law, applicable only to relations among states, and private international law, governing choice of law and enforcement of national judgments in cases involving nationals of two or more states. The legal rules and processes applicable to situations that cut across national lines must now be sought in both public and private international law and to a significant degree, in new bodies of law that do not fit into either traditional division. As examples of the latter, Jessup cited the growing areas of European community law, maritime law, international administrative law, international law on war crimes, the law of economic development, and the emerging law applicable to multinational enterprises. Jessup's aim was not merely to chart the new fields of law but to underline the extent to which these new fields involve law directly applicable to the individual. Outmoded conceptions of international law as law applicable to states alone had to be modified, and the "mysteries" of the distinction between public and private law could not be allowed to determine the rights of parties (Jessup 1973*b*, p. 343). One significant consequence of increased recognition of "transnational law" has been the growth of law school courses and research concerned with international transactions, human rights, international economic law, and other subjects that are much broader in scope than the traditional law of state-to-state relations.

Jessup's interest in the widening scope of international law included particular emphasis on recognition of the rights of individuals. True to his evolutionary perspective, he saw the gradual progression of such rights as proceeding from relatively isolated cases of humanitarian intervention, through the adoptions of special treaties (as those on minorities) to the Charter obligation to respect and promote human rights, and eventually the more detailed covenants with provision for complaints by individuals. He maintained that the Charter articles 55 and 56 imposed obligations in regard to human rights and removed human rights from the exclusive

sphere of domestic jurisdiction (Jessup 1948, pp. 87-89). But he also foresaw difficulties in universalizing national practices, and he cautioned against efforts to extend American concepts to the rest of the world: "The human rights to be defined and protected must be considered not in a vacuum of theory but in terms of the constitutions and laws and practices" of all the states of the world (Jessup 1948, p. 92).

Throughout his active career, Jessup was an outspoken advocate of a wider use of judicial settlement by governments. No single subject occupied him more than the International Court of Justice. In addresses, editorials, and books, he explained why the Court should be used and he cited the many instances in which third-party settlement through the Court or other tribunals had been successful. He strongly criticized the United States for adhering to the self-judging reservation of the Connolly amendment (Jessup 1945). While there was at times almost an evangelical flavor to his advocacy of judicial settlement, he recognized that many disputes are best left to negotiation or mediation. His practical and specific approach led him to propose several measures to enhance the use of the International Court to a modest degree. He advocated the use of small special chambers by the Court to speed decisions and to accommodate regional or functional interests, for example, a chamber for African disputes or one for environmental issues (Jessup 1971, pp. 61-70). Knowing that governments are reluctant to submit to binding judgments, he proposed to increase the nonbinding advisory opinions of the Court by extending the right to request such opinions to the UN Secretary-General, the International Law Commission, and a small committee of the General Assembly. He supported a proposal under which the highest national courts would be able to seek advisory opinions from the International Court on questions of international law before such national courts (Jessup 1970, pp. 5-20). Although these proposals won favor in academic and professional circles, they have received little support from the major governments. They remain on the international agenda to await a more propitious climate for international judicial settlement.

Jessup's advocacy of international cooperation has also extended to proposals for international regulation and, in some degree, administration of areas outside of national jurisdiction. He sees the vast area of outer space as a *res*

communis requiring international administration to avoid national conflict and has advanced a similar conception for Antarctica (Jessup & Taubenfeld 1959). These proposals represent for Jessup an evolutionary advance from a decentralized system based on reciprocity to more centralized regulation on matters "which escape the old territorial realm of national sovereignty" (Jessup 1959a, pp. 140–153). Science and technology are seen as the moving forces in expanding the area of community interests and requiring international administration to serve the needs of all. Jessup does not attempt to demonstrate why or how the recognition of the common interest will prevail over conflicts of ideology or national interest. He rests his case on an assumption of rationality rather than a theory of historical causation. Whether or not his hopes are realized, his eloquent espousal of the "practical necessity" for international administration in the common interest may itself be a factor in influencing future decisions.

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JOHNSON, HARRY G.

Harry Gordon Johnson was born in Toronto, Canada, on May 26, 1923, and died in Geneva, Switzerland, on May 9, 1977. During this relatively short life, he wrote prodigiously: 526 professional articles, 35 books and pamphlets, and more than 150 book reviews straddling a number of corners of intellectual concern in the theory and policy of economics. In addition, he edited 27 books and wrote for popular and in-

tellectual magazines. He had an immense impact on the economics profession, for this vast outpouring of publications was characterized not merely by creative insights, but also by the unique capacity to synthesize apparently untidy and unyielding masses of unrelated and abstruse contributions. Then again, his impact was enhanced by his ceaseless travels to conferences around the world, picking up and adding to the development of new knowledge and disseminating it in improved form, and by his willingness to lecture at the smallest campus or institute, for he perceived this as a professional obligation, no matter what the personal cost in time and health.

Johnson was a cosmopolitan economist, reflecting the richness of his own education and teaching career. He graduated from the University of Toronto in 1943, then spent a year at St. Francis Xavier University in Nova Scotia as acting professor of economics (at the age of twenty), to proceed, after military service in the Canadian infantry, to Cambridge, England, to take his B.A. in 1946. He taught the ensuing year at the University of Toronto, where he also earned his M.A., specializing in economic history, and then spent 1947/1948 at Harvard University. This was followed by a year at Jesus College, Cambridge, after which he was elected to a Berry-Ramsey fellowship at King's College, Cambridge, in 1949. He remained a fellow of King's, teaching also at the London School of Economics (L.S.E.), until he left for the University of Manchester as professor of economic theory in 1956. In 1959, he joined the University of Chicago as professor of economics; he later became the Charles F. Grey distinguished service professor of economics, remaining at Chicago until his death. However, he was soon to combine this with a chair at the L.S.E. (1966-1974) and then with teaching at the Graduate Institute for International Studies in Geneva (1976/1977).

These shifts in location and the associated changes in intellectual environment inevitably proved important in shaping Johnson's education as an economist. In particular, the years in Cambridge and Chicago were the most significant. For both campuses had, in addition to Johnson himself, remarkable figures in economics: Dennis H. Robertson, R. F. Kahn, Nicholas Kaldor, and Joan Robinson at Cambridge; Milton Friedman, George J. Stigler, and Theodore W. Schultz at Chicago. The strong pro-

fessional and political views and interests of these major economists undoubtedly deepened Johnson's interest in developing theory as a tool of policymaking as well as his disdain for logically imposing but irrelevant theoretical constructs. At the same time, they certainly influenced the evolution of his own views and attitudes toward the various approaches to economics, resulting in his eventual antipathy to Marxist viewpoints and to the economics of the New Left.

How does one present in a few pages a set of writings whose major characteristic is the extraordinarily broad scope of its topics? Topics that range from the history of economic doctrines to the economics of the price of gold; from the theory of international commodity agreements to the theory of preferences and consumption; from an analysis of Keynesian economics to the theory of income distribution. They cover, too, the economics of reparations, public goods, common markets, monetary reform, basic and applied research, the brain drain, poverty and opulence, bank mergers, the North-South relationship, student protest, the multinational corporation, universities, libraries, the international monetary union, dumping, smuggling, speculation, bluffing, patents, licenses, innovations, and reversed welfare and revised transfers; the theories of growing productivity and balance of payments, tariffs, excise taxes, inflation, index numbers, nationalism, advertising, the demand for money, distortions, money and economic growth, effective protection, and human capital; analyses of the efficiency of monetary management, minimum wages, the infant-industry argument for protection, the role of uncertainty, income policy, mercantilism, equal pay for men and women, monetarism, buffer stocks, and legal and illegal migration; economic policies for Canada, Britain, the United States, and developing countries; the state of international liquidity; the relationship between planning and free enterprise; the choice between fixed and floating exchange rates; and the monetary approach to the balance of payments and the exchange rate.

Four areas of interest and impact, clearly the most important, deserve to be highlighted: the pure theory of international trade, macroeconomics, international monetary theory, and writings on economic policies and issues of political economy.

Johnson's work on the pure theory of international trade constitutes perhaps his most im-

portant scientific contribution. That trade theory clearly established him as a leading theorist is evident from his papers in *International Trade and Economic Growth* (1958). Characteristically, his work in this area revealed an uncanny and quick perception of the importance of a new idea, and a remarkable ability to shape it into a major contribution of his own that would catch everyone's imagination. Thus, for example, he saw early on the significance of the idea of effective protection (1965*b*). In the theory of optimal policy intervention, he discerned the apparent discrepancy between the use of a production tax-cum-subsidy and the use of a tariff. He showed perceptively (1965*a*) that the former was suited to a production-augmenting objective and the latter to a trade-restraining objective. Again, when the theoretical literature on incorporating illegal transactions into general equilibrium theory began in the early 1970s, Johnson immediately developed interesting extensions of the current analysis into such questions as the effect of smuggling on the optimal- and maximal-revenue tariffs. Moreover, his important and highly original papers on the theory of trade and growth (1953*a*; 1954), written at the time of the dollar shortage after the war, addressed the issues from the viewpoint of differential growth of productivity among trading countries, and put the entire theoretical discussion into a form that dominated the work of trade theorists for years.

His writings on the general equilibrium analysis of international trade include two influential companion articles on income distribution (1959*b*; 1960*b*). Among his best writings on the theory of trade, however, are those that belong to what James E. Meade called the theory of trade and welfare. Here, mention must be made of at least four analytically basic contributions which, at the same time, have had considerable impact on policy discussions. These are his papers on optimum tariffs and retaliation (1953*b*); the cost of protection and the scientific tariff (1960*a*), building on his earlier work measuring the gains from trade; optimal trade interventions in the presence of domestic distortions (1965*a*); and the possibility of income losses from economic growth of a small, tariff-distorted economy (1967*c*).

Written in the first flush of his youth, the paper on optimum tariffs and retaliation is a splendid piece of theorizing: subtle, elegant, economical, and addressed to an important policy problem. Basically, Johnson analyzed whether a

large country could "get away with" exercising its monopoly power. If a foreign country retaliated, would any gain be left for the large country from its departure from free trade? If the country was worse off as a result of the retaliation than under free trade, the case for first-best departures from free trade would collapse completely: John Stuart Mill's celebrated exception to the case for free trade for a large country would no longer be tenable. What was at stake, therefore, was a truly significant cornerstone of the theory of trade policy. Tibor Scitovsky had argued, following on earlier writers, that retaliation would indeed leave everyone worse off than they were under free trade. Johnson used a Cournot-type of retaliation mechanism, in which each country imposed an optimal tariff in turn to show that a country could benefit more from imposing an optimal tariff than from free trade. From the perspective of Johnson's evolution as an economist, two things are notable: (1) This early vintage Johnson was clearly intrigued by analytical complexities that he found less interesting later: thus, he discussed at great length how the retaliation process could lead to a tariff cycle. (2) More interestingly, the policy implication of this early analysis is to resurrect the classic case for the exercise of monopoly power by a large country; Johnson's later writings leaned in the opposite direction, highlighting the great potential cost of departing from free trade.

This shift in Johnson's emphasis to the advantages of free trade is seen most directly in his work on the theory of optimal policy intervention in the presence of distortions and his work on the theory of immiserizing growth. In both instances, Johnson opposed the use of tariffs, utilizing the insights of the theory of second-best as applied to the problems of trade and welfare. His 1965 paper on distortions was built squarely on two propositions: (1) The only *first-best* case for tariffs was the Mill argument for exercise of monopoly power by a large country; and (2) The *second-best* use of a tariff to offset the distortion caused domestically need not be welfare-improving. This paper became a classic reference in support of free trade, reducing the argument for tariffs to a second-best case in the presence of all distortions other than that implied by the presence of monopoly power.

The later article on immiserizing growth (1967c) presented yet another influential and novel argument against protection. Johnson showed that if a small country grew subject to a distortionary tariff, it could experience real in-

come losses. He linked it to the case for an infant industry tariff, which often rested on protection-induced gains in technical efficiency in the industry, and concluded that the case was weak. If technical change in the protected industry was in fact the source of growth, the likelihood of immiserizing growth was enlarged. Equally important have been the subsequent applications of this contribution to (1) the question of measuring growth at world, rather than domestic, prices in trade-distorted economies and (2) the question whether tariff-induced capital flows would be welfare-improving.

Finally, Johnson wrote a paper on the scientific tariff (1960a), whose impact was in two areas: (1) the measurement of protection and (2) analytical propositions regarding optimal tariff structures. Johnson's influence on empirical work, on measuring the cost of protection, and on measuring the gains or losses to Britain from joining the European Economic Community was important, and he took pains to make his theoretical work in this area directly usable by econometricians. Thus, the Johnson who was interested in alternative measures of the gains from trade corresponding to the different Hicckian concepts of compensating and equivalent variation and their relationship to the historic Marshallian measure, was also successful in translating this into practical guides for measurement of the cost of protection and indeed carried out some of these empirical applications himself. Many of Johnson's contributions to the theory of tariffs and commercial policy are reprinted in his *Aspects of the Theory of Tariffs* (1971a).

Johnson's early contributions to macroeconomics were made during his tenure at Cambridge. In "Some Cambridge Controversies in Monetary Theory" (1951a), he clarified the essence of the controversy between the Keynesian and the Robertsonian approaches to such key issues as loanable funds versus liquidity preference, the saving-investment identity, and the Gibson paradox, and distilled and integrated complex issues into a coherent framework. His major contributions during that period, however, were his study of the implications of secular changes in United Kingdom banks' assets and liabilities as a result of the replacement of private by public debt (1951b), and his active participation in the discussion surrounding Britain's revival of monetary policy. Johnson was critical of the quality of British monetary statistics and argued that improved monetary

statistics were essential for a well-managed monetary policy. In "British Monetary Statistics" (1959a) he published his own laboriously constructed monetary aggregates for the period 1930–1957, which stimulated further research.

Johnson's move to the University of Chicago (to which he was invited as a "Keynesian" economist) marked an increased research interest in monetary theory. In the early 1960s, he wrote "The 'General Theory' After Twenty-five Years" (1961), the survey article "Monetary Theory and Policy" (1962b), and "Recent Developments in Monetary Theory" (1963b). These three articles, classics in the field of monetary economics, established Johnson's reputation as a mature scholar with broad scientific and historical perspectives. Johnson's survey suggested a list of issues that would benefit from further research and, in retrospect, this list served indeed as the agenda for research in the following fifteen years. One of the notable issues on the list was his early skepticism about the stability of the Phillips curve in the face of macroeconomic policies. His evaluations of the major developments in monetary economics (as of the early 1960s) have been influential and perceptive. These developments include the application of capital theory to monetary theory and the shift from a static to a dynamic analysis (1962c; 1967b). Following the pioneering analysis by James Tobin, Johnson's analysis of money and economic growth extended Robert Solow's neo-classical barter growth model to a monetary economy by considering the effect of changes in the rate of monetary expansion on the steady-state stock of capital and consumption per head when money is viewed as a consumer's and producer's good.

The dynamic context for the monetary analysis led Johnson to analyze the optimal rate of growth of the money supply (1969b; 1970). He noted that the typical analysis rests on the assumption that money does not bear interest and that the issue of the optimal money supply can be analyzed in terms of the efficiency of the monetary and banking organization.

As a result of his interest in the Keynesian revolution and his deep historical perspective, Johnson wrote his controversial article "The Keynesian Revolution and the Monetarist Counter-revolution" (1971b), which was first presented as the prestigious Richard E. Ely lecture in 1970 and was reprinted in *Further Essays in Monetary Economics* (1972a). This article is an exercise in the history of economic

thought and scientific evolution. Following Axel Leijonhufvud, he drew a sharp distinction between Keynes and the Keynesians, sharply criticizing the latter but not sparing the former. His continuing interest in the various aspects of Keynes and his economic thought resulted in a series of provocative articles, some of which appeared posthumously in his joint book with Elizabeth Johnson, *The Shadow of Keynes* (Johnson & Johnson 1978).

Johnson's major criticism of the Keynesian model was its failure to deal with the problem of inflation at the level of both economic theory and economic policy. He was extremely critical of the "sociological" noneconomic theories of inflation, as well as of price controls and income policy as the proposed remedies to inflation. His analysis of inflation was approached from the perspective of an international economist who views inflation (under a fixed exchange rate régime) as a global phenomenon, a proper analysis of which requires a shift of focus from the concept of monetary developments in individual countries to the concept of the aggregate world money supply (1972b).

Throughout his professional life, Johnson continued his research on international monetary economics. Three articles in 1950 set the stage for what later became the typical characteristics of his style of research: courage to take positions not always popular with others, the application of relatively simple economic techniques to a new range of problems with resultant important insights, and an unabashed love for geometry as a tool of analysis. He took an early stand against raising the price of gold in terms of all other currencies (1950a), analyzed the destabilizing effect of international commodity agreements on the prices of primary products (1950b), and produced an early diagrammatic analysis of income variations and the balance of payments (1950c)—an analysis that was conducted within the then typical Keynesian framework.

In his writings on the theory of the transfer problem, originally developed in the context of the postwar reparations, Johnson synthesized the earlier work of Paul A. Samuelson, Lloyd A. Metzler, Fritz Machlup, and James E. Meade, and demonstrated his philosophy that individual research effort is most productive when it utilizes the work of previous theorists as a foundation for new construction. Johnson's theme was that of "continuity and multiplicity of effort." In "The Transfer Problem and Exchange

Stability" (1956*b*), he proved that the problems of transfers and exchange stability are formally the same, and that all possible methods of correcting balance-of-payments disequilibrium can be analyzed effectively with the apparatus of the transfer problem. Almost two decades later (1974), he returned to the analysis of reversed transfers with greater emphasis on the monetary aspects of the problem.

Johnson's most important contribution to the understanding of international monetary economics is "Towards a General Theory of the Balance of Payments" (printed in 1958). His key insight here was his emphasis on the monetary nature of balance-of-payments surplus or deficit: "[A] balance-of-payments deficit implies *either* dishoarding by residents, *or* credit creation by the monetary authorities"; the former is inherently transitory and the latter has the obvious policy implication. As for policy, Johnson distinguished between "expenditure reducing" policies and "expenditure switching" policies. The originality of this article is all the more remarkable considering the intellectual environment in the mid-1950s, when to a large extent, the balance of payments was viewed as a "real" (in contrast with "monetary") phenomenon. This article may be considered the intellectual precursor of what 15 years later would be termed "the monetary approach to the balance of payments."

Over the years, Johnson focused increasingly on issues, with special reference to Canada (1962*a*; 1963*a*; 1965*c*). Johnson was a supporter of flexible exchange rates as revealed in "The Case for Flexible Exchange Rates, 1969" (1969*a*), which builds on Friedman's classic contribution but incorporates the important lessons from the literature on optimum currency areas. Thus, while he supported the move to a flexible exchange rate régime, he recognized circumstances under which a small country (like Panama) might be better off maintaining fixed parities.

His analysis of the international monetary system revealed his strength as a realistic political scientist. Monetary reform is not carried out in a vacuum; it is implemented or frustrated by representatives of independent nation-states, to whom international commitments are likely to be secondary to national commitments. This realization is reflected in his numerous commentaries on current international monetary crises, his doubts about the prospects of a stable European monetary union, his appraisal of the Bretton Woods system, and his article "Political Economy Aspects of International Monetary Re-

form" (1972*d*). He took a hard line on schemes designed to solve the international monetary problems by methods that would channel resources to less developed countries. Although he was aware of the unpopularity of such a stance, integrity and courage always dominated his position: "My reason for refusing to endorse such schemes is not that I am opposed to the less developed countries receiving more development assistance but that I think that no useful purpose is served by misapplying economic analysis for political ends" (1967*b*, p. 8).

As world inflation accelerated in the 1960s, Johnson recognized that in a world integrated through international trade in goods and assets, national rates of inflation cannot be fully analyzed without a global perspective: "I have become increasingly impressed in recent years with the conviction that the traditional division between closed-economy and open-economy monetary theory is a barrier to clear thought, and that domestic monetary phenomena for most of the countries with which most economists are concerned can only be understood in an international monetary context" (1972*a*, p. 11). This perception of world inflation, along with the analytical insights from his earlier work "Towards a General Theory of the Balance of Payments" (printed in 1958), paved the way for his work on the monetary approach to the balance of payments, which he viewed as the crowning achievement of his career. The intellectual roots of the monetary approach run back to the classic writers (David Hume and David Ricardo), and its early developments can be found in the work of economists associated with the International Monetary Fund (e.g., Jacques Polak). Johnson, however, along with Robert A. Mundell and other members of the International Trade Workshop at the University of Chicago, introduced new and significant dimensions to the approach. He was the most prolific writer and the most devoted contributor to the new approach, having been quick to understand the fundamental policy implications of the simple key proposition that the balance of payments is essentially a monetary phenomenon. Among these is the implication that balance-of-payments policies will not produce an inflow of international reserves unless they increase the quantity of money demanded or unless domestic credit policy forces the resident population to acquire the extra money wanted through the balance of payments *via* an excess of receipts over payments. He saw himself as a missionary, and he

took the lead in developing and disseminating this approach by encouraging and guiding theoretical and empirical research in this field in Chicago, London, and Geneva (Frenkel & Johnson 1976).

The evolution of the international monetary system into a régime of flexible exchange rates led to further extensions of the monetary approach and resulted in a new direction of theoretical and empirical research on the economics of exchange rates. Johnson stimulated much of the early research in the area and coedited *The Economics of Exchange Rates* (Frenkel & Johnson 1978), which contains some of the resulting work.

While the foregoing review of Johnson's major theoretical contributions amply shows the strong policy relevance of his best work, he also wrote directly on policy. He reacted quickly to major policy issues, as well as to theoretical developments. Thus, when the question of responding to the needs of the less developed countries became important during the 1960s, he published *Economic Policies Toward Less Developed Countries* (1967a), a comprehensive and masterly study for the Brookings Institution of foreign economic policy issues facing the United States in regard to the less developed countries. He analyzed such proposals as commodity schemes and preferential entry for manufactured exports of the less developed countries. The originality and independence of his thinking were further evident in many of his policy analyses, where he often departed from the conventional view. This is best illustrated by his work on the brain drain, where he was most influential, through his own writings (1964; 1967c) and those of his associates, in propounding the view that the brain drain might be welfare improving for the countries from which it occurred. This is one example of how, in his later years, his analyses increasingly questioned interventionist policies. Thus, the brain drain was beneficial rather than harmful; the multinational corporations were part of a non-zero-sum game, and so on. The United Nations Conference on Trade and Development, which addresses the less developed countries' problems and demands, and which he viewed benignly in the early 1960s, came under his withering criticism as he came to feel that professional economists, influenced by their sympathies for the poor countries, had been led into empathetic and nonscientific research on trade and development.

Three additional aspects of Johnson's work deserve mention. He was a humane social scientist, interested not merely in understanding social phenomena, but also in contributing to the improvement of well-being (1975a; 1975b). Moreover, he was a gifted teacher with a deep sense of mission and responsibility. He devoted great effort to the preparation of his lectures and always accepted a heavy teaching load. Some of his lucid and insightful lectures are published in *Macroeconomics and Monetary Theory* (1972c) and *The Theory of Income Distribution* (1973). Then again, he was widely respected as an editor, who demonstrated considerable judgment and a talent for recognizing and encouraging the development of original lines of thought. In addition, he was devoted to his sustained role as an editor of the *Journal of Political Economy*. He also served on the editorial boards of *Economica*, the *Journal of International Economics*, the *Review of Economic Studies*, and the *Manchester School*.

Many honors came Johnson's way. His combination of sociological insights with professional economic expertise, and the elegance of his style, made him a popular lecturer, and he was invited to deliver many of the prestigious public lectures in economics: the Ely lecture, the Wicksell lectures, the de Vries lecture, the Ramaswami lecture, the Johansen lectures, and the Horowitz lectures. He was president of the Canadian Political Science Association (1965/1966) and the Eastern Economic Association (1976/1977), chairman of the (British) Association of University Teachers in Economics (1968–1971), and vice president of the American Economic Association (1976). He was also a fellow of the Econometric Society, the British Academy, the Royal Society of Canada, a distinguished fellow of the American Economic Association and of the American Academy of Arts and Sciences. He was the holder of honorary degrees from St. Francis Xavier University, University of Windsor, Queen's University, Carleton University, University of Western Ontario, Sheffield University, and the University of Manchester, and was awarded the Innis-Gérin medal of the Royal Society of Canada, the prix mondial Messim Habib by the University of Geneva, and the Bernhard Harms prize by the University of Kiel (Germany) just prior to his untimely death. The Canadian government named him an officer of the Order of Canada in December 1976, a fitting tribute from his native country

for a fully internationalist economist who had brought great distinction to his discipline.

JAGDISH N. BHAGWATI AND
JACOB A. FRENKEL

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JOUVENEL, BERTRAND DE

For assessing Bertrand de Jouvenel's life and works, and the intimate relationship between them, the best tools are provided by the French writer himself. His major works of the 1960s and 1970s have the double merit of being future oriented and opening new avenues on the one hand, yet, on the other hand, referring explicitly to questions raised twenty, thirty, and sometimes fifty years before. Three books constitute a particularly useful introduction to his thinking, since they are collections of essays written from the 1950s to the 1970s, grouped according to themes: *Arcadie: Essais sur le mieux-vivre* (1968) deals with the quality of life, environment, and the nonquantifiable dimensions of economic life; *Du principat* (1972) deals, in a more classical vein, with problems of political philosophy, particularly the growth of governmental power in modern societies; *La civilisation de puissance* (1976a) deals essentially with the growth of technological power and man's mastery over nature.

Each of these three books contains a preface which, almost apologetically, acknowledges their lack of systematic character, but maintains their unity of inspiration. In the preface to *La civilisation de puissance*, Jouvenel wrote:

In the course of a long life, I was never capable of devoting myself to a particular "intellectual dis-

cipline": I was always fascinated by the phenomena of my time and the most diverse ones. Out of this dispersion of my attention, a certain idea of the civilization within which my life was unfolding tended to take shape in my mind. To present a synthetic view of it, would be an ambition beyond my reach. Besides, a mind which would be powerful enough to provide such a vision would risk imprisoning other minds within it: this is the drawback of large syntheses. This is why I do not regret my limits.

What I am inclined to do is simply to shed light on certain features of our civilization, features which became printed in my mind through successive encounters. (1976a, p. ii)

And in the preface to *Arcadie*, he wrote:

I speak here not as someone who aspires to the position of guide to the human caravan, but as a traveler among others, who signals pot-holes here and attractive panoramas there to his companions as they spring to his eyes, and without forgetting that his view is partial and selective. Perhaps a certain indiscipline qualified me for this role of explorer. This does not mean that my mind is repelled by rigorous modes of thought; to the contrary, I like these beautiful nets cast upon reality, but we all know that part of reality escapes every net, that there is no formulated knowledge which is not incomplete by definition. It follows, it seems to me, that the vision obtained through a system of definitions and measures must always be completed by views taken from outside the system. (1968, pp. 7-8)

The metaphor of the traveler exploring uncharted roads, announcing opportunities and dangers ahead, and looking back to assess whether directions chosen were the good ones, is indeed appropriate for characterizing Jouvenel's approach and experience. His life coincides almost exactly with that of the century; his social background, his literary style, and his personal experience all help make his intellectual approach and his published work highly unconventional by academic standards, yet all the more able to reflect the contradictions of an epoch.

Jouvenel was born in 1903, the son of Henry de Jouvenel des Ursins, a well-known French public figure who was an ambassador, a senator, an editor of a major Parisian daily, and, above all, a constant fighter for liberal causes, from the Dreyfus affair to the League of Nations, where he inspired the Protocol for the Peaceful Settlement of Disputes, which was ultimately to be rejected. Jouvenel's mother, Claire Boas, was the daughter of a Jewish industrialist. The

entire Jouvenel family was aristocratic, political, and literary. Jouvenel's uncle, Robert de Jouvenel, was the author of a well-known political pamphlet, "La République des Camarades," and his stepmother was the great novelist Collette. The idea of a Czechoslovak republic is presumed to have been spread in French circles through his mother's "salon," at which Thomáš G. Masaryk, who was to become president of Czechoslovakia, was a frequent visitor.

Jouvenel's early studies showed the eclecticism that would characterize his interests. He graduated in law and mathematics from the Sorbonne, but his interest soon turned to social and economic matters, particularly in the United States. His first important book, *L'économie dirigée* (1928), coined the term adopted in French usage for economic planning. The work starts with an insistence on the importance of the technological revolution for changing the nature of social life and ends with a discussion of the inevitable and desirable increase in the economic role of the state and its power to assess economic trends. Perhaps his most formative experience in social and economic matters was the great depression in the United States, which shaped Jouvenel's permanent horror of the human costs of unemployment and his preference for growth-oriented economic policies, and which produced, as well, *La crise du capitalisme américain* (1933), one of the major books interpreting the depression.

Much of Jouvenel's political thinking and activities in the 1930s revolved around the search for some kind of a French and European New Deal, capable of overcoming both the economic, political, and ultimately, the spiritual crisis, by calling the state to fundamental reform, and, above all, by uniting and mobilizing popular energies, particularly those of the young. This quest involved him in such efforts as the founding of a short-lived weekly, significantly entitled *La Lutte des Jeunes* in 1934, the support of Gaston Bergery, a precursor of Mendès-France, and even of Jacques Doriot, the former communist and future collaborationist leader. Although these activities seemed to fluctuate in direction, all had in common an attempt to overcome the rigidities of classic right and left thinking and to learn from new experiences, including those of the fascist states. This last aspect is particularly apparent in *Le réveil de l'Europe* (1938), as it was to be in *Après la défaite* (1941).

Jouvenel's main activity during that period,

however, was as a journalist specializing in international affairs; he was both one of the most influential diplomatic commentators and one of the most famous special envoys of the time. His interview with Hitler in 1935 was a world scoop. His books of the time—for example, *D'une guerre à l'autre* (1940–1941)—are based essentially on his newspaper chronicles.

The outbreak of World War II brought about a fundamental change. Dropping his journalistic activities, Jouvenel enlisted as a volunteer. He published still another political tract, *Après la défaite*, which was widely—and rather erroneously—interpreted as a call to France to learn the lesson of its victors, but then withdrew into scholarship. He published two studies of economic history, *Napoléon et l'économie dirigée* (1942) and *L'or au temps de Charles-Quint* (1943). At the same time, he engaged in resistance activities that soon forced him to quickly go into hiding in France and then to take refuge in Switzerland, where he remained for the first years after the war ended. This period seems to have marked the turning point of his career. He did resume writing political columns, this time in the Swiss press, dealing particularly with the fate of Europe in the postwar world, calling for moderation and hard work and warning against the excesses of left-wing ideologies that were prevalent in France at the time and that he felt could lead to the repetition of the evils of fascism (1947c). He also continued to write books evaluating current political, social, and economic experiences. Examples are his analysis of the Labour experience in England in *Problèmes de l'Angleterre socialiste* (1947b) and of the Marshall Plan in *L'Amérique en Europe* (1948). However, his major work shifted from journalism and concrete economic history to political philosophy.

Jouvenel's most voluminous and best-known work, *Du pouvoir: Histoire naturelle de sa croissance* (1945), was followed by *De la souveraineté: A la recherche du bien politique* (1955) and by *The Pure Theory of Politics* (1963). Despite their great separation in time, the three volumes follow each other logically. The second and least-known work, *De la souveraineté*, holds the key to the passage from the historical approach of the first to the analytical perspective of the third. The collection of articles entitled *Du principat* offers a further key to Jouvenel's thought on the nature and evolution of politics, as does his work on Rousseau (1947d, 1965).

Besides those of the youthful socioeconomic observer, glamorous international correspondent, and meditative political philosopher, two other roles established Jouvenel's position and influence in French and international cultural life. The first was that of social and economic expert and adviser. As a founder and chairman of the SEDEIS (Société d'Études et de Documentation Économiques Industrielles et Sociales) he may have helped more than any other man, through innumerable meetings, round tables, and publications, to acquaint French elites both with modern, particularly Anglo-American, economic discussion and with the broader social implications of economic policies. For years the SEDEIS bulletin combined extremely close coverage of economic events and trends with longer-range or theoretical speculations. Later these two orientations gave rise to two different publications: the *Chroniques SEDEIS*, continuing the running coverage of trends and events, and *Analyse et Prévision*. The latter is the fruit of still another pioneering initiative of Bertrand de Jouvenel: the encouragement and, to a great extent, the launching in France and in the West, of future studies as an organized intellectual enterprise. Jouvenel coined the term "futuribles" which provided the title, first of a series of studies published by the *Bulletin SEDEIS*, next of an international association, and then of a series of books, of which the most rigorous methodologically is probably Jouvenel's own *L'art de la conjecture* (1963–1965). His twin preoccupations—with broadening the scope of economics to the quality of life, and with positive utopias and visions of the future—led to *Arcadie*, based on essays written as early as 1957.

Jouvenel's last role has been that of the academic. During the early postwar period he was a frequent visiting professor, largely to British and American universities. In the 1960s, he was appointed an associate professor at the Faculty of Law and Economic Sciences of the University of Paris, where he was given the chair of social forecasting. He also taught the history of political thought, however, and from this teaching has emerged *Les débuts de l'état moderne: Une histoire des idées politiques au XIX^e siècle* (1976b), which, in a way, returns to the themes studied thirty years earlier in *Du pouvoir*.

This alone would show that Jouvenel's intellectual travels, if they have often carried him

away from well-charted roads, have been far from rambling. What, then, is their logic and what is his basic contribution?

First, as has been said of Charles de Gaulle, Bertrand de Jouvenel is a man "d'avant-hier et d'après-demain," of the day before yesterday and the day after tomorrow. Although he follows and analyzes the present, his peculiar mark is to see it in the light of the past and of the future. Jouvenel's background, style of writing, and, to some degree, themes point toward the past, when the power of the state was more limited and man's contact with nature more satisfying; yet his best-known contribution, along with *Du pouvoir*, may be the impulsion he gave to future studies and his constant preoccupation with the consequences of our present activities for the future of our planet.

Second, his contradictions, while reflecting different facets of his personality and different phases of his life (emphasis on dynamism and mobilization in his youth and on prudence and procedure more recently) express above all the contradictory or complementary features of reality itself. One of Jouvenel's greatest contributions is his warning about the necessity for countervailing measures to trends he considers inevitable or to policies he favors. As an economist, he is for increasing salaries rather than reducing prices, yet he warns against the dangers of the inflationary society. He has warned, successively, against insufficient and excessive quantification. He thinks that growth and the satisfaction of the consumer are the imperatives upon which our society is built, but that they risk destroying both nature and individual responsibility. He warns that politics cannot be reduced to institutions, that power lies above all in the initiative of men moving other men; but this reality, in his eyes, justifies all the more the advocacy of new institutions better able to contain abuses of personal power from above or the rise of personal power from below.

To Jouvenel, the theme of the development of power is a guiding theme in his thinking. He cites the development of the state as the theme of *Du pouvoir* and the increase in the forces used by human societies as the theme of *La civilisation de puissance*.

For his political thought, the important point is to recognize both the generality of the phenomenon and the crucial theoretical and practical importance of distinguishing between its various levels or dimensions. *Du pouvoir* is a

generalization of Alexis de Tocqueville's idea that the French Revolution, rather than breaking the absolutism of the state, further concentrated power in the hands of the state—a work that the French kings had undertaken and that would be pushed even further by Napoleon. In this monumental book, Jouvenel considers power as a living being, whose growth he examines through the ages, along with the justifications, revolutionary or other, which have accompanied its concentration. He shows how modernity does away with the moral, legal, and social restraints that traditional régimes put in the way of the concentration of power. However, Jouvenel does make a distinction—which he makes even clearer in further works—between the growth in the functions of the state, the growth in its power over men, and the concentration of this power in the hands of one man. This is why twenty years later, in *Du principat*, one of his most impressive essays, he insists that the broader the scope of power, the more it must be controlled, and the more inevitable the growth of the executive at the expense of the legislative and the judiciary, the more one must find, within the executive itself, new checks and balances that can come only from the bureaucracy. This would be embodied in what Jouvenel calls “a new constitutionalism,” a policy needed to check the growth of personal power, given the decline of traditional forms of representation.

The reason why this new constitutionalism has not been forthcoming is the change in the legitimation of power. Instead of being justified by the way it is used, power, Jouvenel claims in *Du pouvoir* and in *De la souveraineté*, has been justified in terms of its origins: popular sovereignty or ideological orthodoxy are assumed to legitimate any use of power. An even more telling aspect of the decline in the notion of the rule of law is what Jouvenel calls the change from *nomocracy* (rule of law) to *telocracy* (rule of goals). Modern governments are justified by the expectations they fulfill or the goods they deliver. This is what gives the state, which has the initiative of planning for growth and welfare, the edge over parliament and the judiciary, which enunciate universal laws or are the guardians of a permanent constitution.

Much of this change in the functions and power of government is due to the change in the nature of society. In this aspect of his analysis, the unity of Jouvenel's thought, despite its apparent dispersion, becomes apparent. Much of

it consists in drawing the consequences from three characteristics of our societies: they are complex, open, and moving. These tenets are what renders unfeasible a Platonic republic; socialism; a fascist or anarchist society based on the model of the small community; and any conception of the common good based on a blueprint pretending to regulate the whole of society. Moreover, any attempt to achieve perfect justice through the redistribution of resources or perfect community through common enthusiasm is conducive to tyranny. Only through the just behavior of individuals, through the reconciliation of initiative and procedure, of action and negotiation, can progress toward freedom and justice be hoped for.

An analogous duality between power and moderation characterizes Jouvenel's thought on economic and social matters. Here too, the basic question is that of the human consequences and the metaphysical implications of the technological revolution. In *La civilisation de puissance*, Jouvenel suggests calling our time “the era of Watt,” for of the three great events of the mid-1770s—the invention of the steam engine by James Watt in 1774, the publication of Adam Smith's *Wealth of Nations* in 1776, and the American Declaration of Independence—the most important is the first one. It signaled the passage from biological to physical force as the latter was harnessed to extend man's power over nature and society. This increased enormously man's ability to achieve goals he had always desired, but it also led him to become a prisoner of his own power rather than to use technology and economic efficiency to achieve consciously chosen aims for the improvement of the quality of life.

This theme may not be Jouvenel's most original. His peculiar contribution, beyond a general critique of man's enslavement by his own machines, is fourfold. First is his great attention to the role of the organization of labor, of research and development, and, more generally, of the conditions of efficiency in modern industrial economies. Second is his insistence on the sometimes voluntary, sometimes unconscious, narrowing of the horizon implied by the very methods that produce this efficiency. In particular, Jouvenel may have been the first to point out the misleading aspects of the notion of gross national product, which includes only quantifiable data and ignores whether they are productive or destructive; to criticize the monetary bias

of economic science and its ignorance of physical data; and to point out the necessity of including political economy in a broadly conceived political ecology.

Third, the latter preoccupation, which Jouvenel expressed as far back as 1957, leads substantively to a concern with the deterioration of man's environment, and to a call to people to become "gardeners of the earth." Finally, it also leads to a call for conscious utopias and to the importance of alternative models of the future, or "futuribles."

Jouvenel's two most abstract and general books are *Futuribles: L'art de la conjecture* and *The Pure Theory of Politics*. The unifying concept of the two works is probably that of decisions. What worries Jouvenel in both the growth of the state and the growth of technology is the progressive constriction of the area of choice for the individual. The only area in which modern man exercises his choice is as a consumer. But he cannot fulfill himself unless he participates in social and political life as well. And he cannot do that unless he coordinates his preferences and expectations with those of other men.

This is why a preoccupation with the future is so important. Society is a system of decisions; the problem is to make these decisions compatible with each other and thus to reconcile freedom, which means individuality, and predictability, which implies coordination. It is at this point that Jouvenel's practical preoccupation with making social, economic, and political decisions as transparent as possible, and his theoretical one, with showing the preinstitutional bases of politics, coincide.

Jouvenel identifies the basic political phenomenon, power, as "man moving man." But there are two distinct sources of power of men over men: what he calls "potestas," or power based on institutional authority, and "potentia," or power based on interpersonal influence. And there are two different styles and tasks of power: that of moving or mobilizing, that of the "dux," of the charismatic leader or that of the team, and that of regulating or reconciling, that of the "rex," the wise and benevolent judge, or the committee.

Ever since his youth, Jouvenel has been sensitive to the appeals and dangers of the former, and his *Pure Theory of Politics* reads like a prophetic message to established elites that more dynamic and less controllable forces are

again afoot. Increasingly, however, his preferences go to those who define their task as maintaining the fragile consensus on which liberal institutions are based, as well as man's fragile coexistence with his planet. But for both tasks, a reawakening of personal responsibility and a restoring of the affectionate respect of individuals for each other, their institutions, and their natural environment are the ultimate preconditions of legitimacy.

PIERRE HASSNER

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K

KAHLER, ERICH

Erich Kahler (1885–1970) can be described as a historian, a philosopher, a sociologist, and a literary scholar. His work embraced all these fields, and whether he wrote a historical analysis of the Prussian state or a literary analysis on the changing form of the novel in the twentieth century, he was always studying the role of the phenomena at hand in the evolution of humanity and of human consciousness. As a philosopher of history, he deplored the compartmentalization and specialization of scholarship in the twentieth century, and insisted that the intellectual's role was to synthesize the relationships between all facets of human experience.

Kahler's holistic and humanistic perspectives are best reflected in *Man the Measure* (1943), one of his major studies in the philosophy of history. In it, he surveys the development of the Western world, a development he sees as "representative of human evolution as a whole" ([1943] 1967, pp. 24–25). In the course of this survey, he attempted to define what is specifically human about human beings, to uncover the roots of the crisis he saw facing the world in 1943, and to use the knowledge so derived for creating the future of man.

Kahler defined man's exclusively human quality as the "*faculty of going beyond himself*, of transcending the limits of his own physical being" (*ibid.*, p. 11). The transcending process involves two major acts:

the establishment of these limits by detaching and discerning a non-self from a self, and the establish-

ment of a new and conscious relationship with the clearly conceived non-self by transcending self toward the non-self.

History as the evolution of the human quality, then, is the *successive development of these two acts or faculties of man*. In its early periods, human history is mainly the formation of the faculty of detaching and discerning. The later part of human history is the genesis and perfection of the quality of transcending. (*ibid.*, pp. 18–19)

In this perspective, human history breaks down into three major phases. In the first period, which reaches from man's primitive state to the end of antiquity, man completes the separation of the inner and outer worlds. In so doing, he leaves behind both his unity with the physical universe and his membership in what Kahler called the "pre-individual" community of tribe and species. As a fully formed human individual, man enters the second evolutionary phase, extending from the end of antiquity to the Renaissance, in which the intellect is completely liberated and the human individual completely freed from superhuman rule. The third phase, the modern era, is characterized by man's struggle toward a collective order, a "post-individual community," and toward "reintegration in a clearly conceived universe" (*ibid.*, p. 21).

Under the impact of capitalism and capitalistic means of production, the development of modern man has been toward ever greater collectivization and loss of individuality. Paradoxically, the human being no sooner reached the height of his individuality at the end of the Renaissance than he was forced to collectivize, economically and politically, to protect that in-

dividuality. The result—and a major cause for the crises of the twentieth century—was the formation of “private collectives,” which pit interest groups against each other and are governed by things for the welfare of things. Kahler called instead for “public collectives” governed by man for the welfare of man.

The intellectual's job is a revolutionary one. He must determine what the future demands and help to bring it about. “We cannot go back, we can only go forward; historical processes are irreversible. We cannot preserve, we can only conquer, and, in the face of change, reconquer what is worth preserving, thereby making it new” (1969, p. 13). The idea of revitalizing what is worth preserving is crucial, for in Kahler's view of history nothing is ever left behind. History is neither linear nor cyclical; it proceeds like a spiral, expanding and advancing yet also circling back on itself. Kahler's allegiance to the guiding ideas of the classical and Judeo-Christian traditions was deep and abiding, but his conception of the ideas of universal peace, justice, democracy, and brotherhood of man was not static or absolute. These ideas always had to be “made new.” In *The True, the Good, and the Beautiful* (1960), he wrote that “there is a certain constancy of basic values which, at a certain point of maturity, emerged into human consciousness, and subsequently, in the process of history, blended with changing forms according to changing circumstances” (1960, p. 11).

Given Kahler's background and education, it is not surprising that these ideas were part of his very nature and that his work represents a lifelong effort to realize them in thought and action. The son of a Prague industrialist, Kahler grew up in intellectual and artistic circles. He studied in Vienna, Berlin, Munich, and Heidelberg, receiving his doctorate from the University of Vienna in 1910. During his residence in Germany from 1912 to 1933 he was in touch with the Max Weber circle and was a close friend of Friedrich Gundolf. Later, in his emigrant years in Switzerland and the United States, he formed close friendships with Thomas Mann, and especially, with Hermann Broch. Broch and Kahler shared both a strong moral impulse and a “striving toward unity” that has been attributed to the Judaic tradition.

Humanity's need for unity in its thinking and institutions pervaded Kahler's life and work. Man has to understand himself, his past, and his present as a whole if he is to shape his future in a humane way. The “public” and “post-

individual” collective that Kahler projected for this future would have to be a product of all the intellectual disciplines working together and would have to be based on the idea of man as a constantly evolving, historically created entity.

ROBERT KIMBER

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KALDOR, NICHOLAS

Nicholas Kaldor is probably one of the most original and provocative theoretical economists of the twentieth century, as well as one of the most radical advisers on taxation policies to

many governments. His contributions to economic theory range over a wide field: from the theory of the firm to welfare economics, from capital theory, international trade, and tariff policy to trade cycle and economic growth theory and monetary and taxation policy. But he probably will be most remembered for his theory of income distribution and his advocacy of an "expenditure tax."

Nicholas Kaldor was born in Budapest, the son of a barrister, on May 12, 1908. His parents, Dr. Julius and Joan Kaldor, very much welcomed this son, as two elder sons had died in infancy. They brought him up in a relatively well-to-do Jewish family in one of the major capitals of central European culture during the splendid last stage of the Austro-Hungarian empire. This privileged childhood affected Kaldor's attitudes throughout his life and may help explain his exuberant, egocentric, and undisciplined character. In 1934, Kaldor married Clarissa Goldschmidt, who graduated in history from Sommerville College, Oxford. They had four daughters.

Kaldor's education was elitist in nature. He attended the famous "Model Gymnasium" in Budapest (from 1918 to 1924). Then, in 1925/1926, he attended lectures at the University of Berlin, but soon moved to London. In 1927-1930, he was at the London School of Economics (L.S.E.), where he graduated with a brilliant record in 1930. In those years the chair of economics was occupied by Allyn Young, an American from Harvard University, who had a great influence on Kaldor. Young died in the winter of 1928/1929 and his place was taken by the active, flamboyant Lionel Robbins, well known for his efforts to build up contacts with continental Europe, especially with the Viennese circle of Ludwig von Mises.

In the very early 1930s, the L.S.E. teaching staff was joined by the Austrian Friedrich A. von Hayek. These surroundings could not help but to enhance Kaldor's career. He returned, for short periods, to Budapest, where he met and became a friend of John von Neumann. In 1932, he was appointed to an assistant lectureship at the L.S.E. Among his colleagues were J. R. Hicks and Tibor Scitovsky. He remained at L.S.E., as a lecturer, and then as a reader of economics, until 1947.

The early L.S.E. years were for Kaldor a period of strict orthodoxy in the marginal economics tradition. His 1930s writings are on the problems that were under discussion at the time—mainly the theories of imperfect and monop-

olistic competition and the theory of capital. They immediately revealed distinct originality. It was Kaldor (1933) who gave the name "cobweb theorem" to the theorem, by now well known, concerning the conditions of market stability in terms of relative elasticities of demand and supply. Later, he proposed (1939) a "compensation of the loser's test," in the hypothetical event of personal income redistribution; it has since become known as "Kaldor's compensation test" in the literature on welfare economics.

The crucial turning point came with the publication of Keynes's *General Theory* (1936). Kaldor's reaction was slow, but it went very deep. It materialized at first in a change of interest, mainly from micro- to macroeconomic problems, then as a complete and radical change in thinking on all of economic theory. He first wrote a series of articles on macroeconomic problems, the most remarkable of which is "A Model of the Trade Cycle" (1940), that relies on nonlinear investment and savings functions to produce "limit cycles." The idea turned out to be very fruitful and was further developed by Hicks (1950) and Goodwin (1951). Later, the break with tradition became wider, and a parting with the L.S.E.—the major center of opposition to Cambridge University—was finally inevitable. Kaldor had definitely moved "into the opposite (Cambridge) camp."

Kaldor's contacts with Cambridge had begun in 1940, when the L.S.E. had moved there physically because of the war. But the favorable opportunity arose only after World War II. Kaldor spent two years in Geneva as a director of research at the Economic Commission for Europe, and had to leave the L.S.E. On his return to academic life in 1949, he joined the economics faculty of Cambridge University, where he was appointed a fellow of King's College (Keynes's own college). He then became a reader of economics and, in 1966, a professor of economics, a position he held until his retirement in 1975. During this period he also became economic and taxation adviser to an extremely large number of governments (India, Ceylon, Mexico, Ghana, British Guyana, Turkey, Iran, Venezuela), to some central banks, and to the Economic Commission for Latin America. But, most important was his position as special adviser to the chancellor of the exchequer of the British Labour governments during the eventful years 1964-1968 and 1974-1976. He was elevated to the House of Lords as a life peer in 1974, when he took the name Baron Kaldor of Newnham in the City of Cambridge.

At Cambridge, Kaldor did most of his mature work. He was one of the major authors—along with Richard Kahn, Joan Robinson, and Piero Sraffa—of what has become known as the post-Keynesian school of economic theory (see Eichner & Kregel 1975). Kaldor's original contributions, which are numerous, can be found in a series of polemical papers and in his three versions of a model of economic growth (1957; 1961; Kaldor & Mirrlees 1962). His major original contribution, however, is in a 1956 article, in which he proposes a "Keynesian" theory of income distribution. The theory is in fact distinctly Kaldor's, and has correctly been so-called since.

Kaldor's theory of income distribution is based on the idea that profit receivers have a much higher propensity to save than wage earners. Therefore, in an economic system in which entrepreneurs carry out investments that correspond to full employment, there exists a distribution of income between profits and wages that—owing to the differentiated propensities to save—will generate precisely that share of profits into national income that is necessary to sustain the predetermined investments. This macroeconomic theoretical conception of income distribution is reminiscent of David Ricardo's. But Kaldor reverses Ricardo's chain of causation. For Ricardo, wages were the exogenous magnitude (determined by the sheer necessity of workers' subsistence), and profits were a residual, or rather a "surplus." For Kaldor, profits take up the character of an exogenous magnitude (determined by the necessity of capital accumulation), while wages become a residual. The consequences are far reaching, both on a theoretical level—for a critique of the marginal theory of income distribution—and also on a practical level, because of its implications for taxation policy. It can be no surprise that Kaldor's theory was immediately the target of bitter attacks. There is by now an enormous literature on the subject, which developed further after the publication of a related article by Pasinetti (1962), concerning a long-run theory of the rate of profit.

Along lines parallel to those on income distribution, Kaldor developed his idea of an "expenditure tax," proposed in a book (1955), which has become a classic and has served as the basis for other major works on the subject (for example, Meade et al. 1978). Kaldor points out that the present tax system, based on personal *income*, is inequitable in many respects. People

who inherit private fortunes have enormous spending power without contributing to the state's needs. On the other hand, thrifty people are taxed twice—once on the income they save and a second time on the income they derive from accumulated savings. Kaldor proposes a radical change in the system so that people would be taxed not on their incomes but on their actual expenditures.

In the latter part of his life, Kaldor was increasingly involved in British politics. His position as special adviser to the chancellor of the exchequer brought him into contact with practically the entire economic policy of the Labour government. He was surprisingly successful in this capacity, especially on matters concerning taxation. He devised ways to reform in detail British tax policies—to detect loopholes and eliminate them. He also invented new taxes, such as the selective employment tax.

Not surprisingly, this activity made Kaldor unpopular with the British establishment, but he did not mind. He was firmly convinced of the inequitable and unjust distribution of income prevailing in capitalist societies, and he wanted to have the rich pay more and the poor less. Being in the fortunate position of belonging to a family with considerable private means, he was often hit, sometimes rather badly, by the very taxes that he had helped to devise.

Kaldor took a keen interest in the economic problems of less developed countries. He wrote at length on economic development, as well as on the reform of the international monetary system.

His involvement with practical economic policy led him, in the late 1960s, to look more and more at empirically observed correlations among economic phenomena, trying to discover theoretical hypotheses that might explain them. His inaugural lecture (1966) is an example of this effort. But he also attempted a new theoretical construction based on the idea that the "Keynesian" features of an economy apply only to its industrial sector, which enjoys increasing returns to scale, while alongside that sector, another sector, the primary sector, which provides food and raw materials, operates with "non-Keynesian" features and decreasing returns. These ideas are clearly reminiscent of Allyn Young's, whose lifelong influence is explicitly brought out, especially in Kaldor's 1975 article.

As will appear, even from this brief survey, Kaldor's original ideas are numerous. They are

scattered in a remarkably large number of articles, papers, notes, memoranda, and reports. Fortunately, at the end of his career, Kaldor was persuaded to collect his writings into a series of eight volumes (1960–1979). To read these books is extremely rewarding for any economist. Unexpected clever insights, unorthodox remarks, original ideas are disseminated everywhere, even in the most “applied” writings; they will remain a rich source of ideas for years to come.

Kaldor's attitude was much like that of such great economists as Ricardo and Keynes. He was an innovator in economic theory, with his mind always intent on concrete situations and actual applications. Like Ricardo and Keynes, he was not a revolutionary. But he was a great believer in reforms. He believed in the possibility of even single persons to modify situations and influence events. And he actually did influence events, or acted always as if he had a deep conviction that he did and could.

LUIGI L. PASINETTI

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KALECKI, MICHAŁ

Michał Kalecki (1899–1970) earned an important place in the history of economic analysis by his anticipation of the Keynesian revolution and by his original contributions to the theory of economic dynamics. His ideas (even if sometimes not directly identified with him) have had a lasting imprint on diverse branches of modern economics. His theory of dynamics and fluctuations of national income and its partition between profits and wages is more general than the Keynesian system and more relevant to the contemporary scene.

John Maynard Keynes's epoch making *General Theory* was published in 1936. But, in 1933, Kalecki had already discovered the basic components of Keynes's analytical system. He avoided the distinction between macro- and microeconomic theories. He constructed his macroeconomic model on the basis of a more realistic theory of the firm that incorporated imperfect competition and income distribution as integral parts of his analysis. He elucidated the dynamic properties of the economic process and dealt with an open economy. His model provides a starting point for understanding the contemporary problem of simultaneous occurrence of inflation and recession.

Kalecki studied engineering at Warsaw Poly-

technic and at Gdansk Polytechnic, but never received his degree. In economics, he was self-taught. He worked independently and outside the mainstream of economic theory. His pre-1936 writings were mainly in Polish and those in English and French were highly technical. Soon after the publication of the *General Theory*, Kalecki visited Cambridge University and met Keynes and his group. Although Kalecki had informed Keynes's younger colleagues of his own prior publication, he never mentioned it to Keynes. In fact, Kalecki's precedence was publicly acknowledged only after Keynes's death.

Kalecki's contributions are of particular importance because of modern assaults on the Keynesian revolution. They include:

(1) The model of perfect competition used by Keynes was foreign to Kalecki, who dealt with imperfect competition and oligopoly. Kalecki's macro model in this respect has a more useful microeconomic account. To build a realistic theory, Kalecki explained how industrial prices are formed by markups on costs and distinguished between "cost-determined" and "demand-determined" prices. The markups depend on the relative strength of market imperfection and oligopoly, which he termed the "degree of monopoly." The intensity of the "degree of monopoly," together with other distributional factors, is a key for the determination of macro-distribution. The distributional factors are essentially pertinent to effective demand and to fluctuations in aggregate output and utilization of resources.

(2) Kalecki's theory of profits was based on the principle that wage earners do not save, but spend what they get, and that entrepreneurs get what they spend. Thus entrepreneurs' profits are governed by their propensity to invest and consume and not the other way round. Kalecki's treatment of the consumption function is attractive because his analysis is based on behavior patterns of classes rather than on a "fundamental psychological law."

(3) Kalecki's model not only described a wide range of economic phenomena, but it presented the economic process in motion—i.e., how one sequence develops from the preceding ones. The model encompassed long-run dynamics and the capacity effects of investments and some other supply considerations.

(4) Kalecki did not approach the theory of effective demand through the multiplier, but through the theory of the business cycle, which

established two basic relations: (a) the impact of effective demand generated by investment on profits and national income and (b) the investment decision function, where the rate of investment decisions at a given time is roughly determined by the level and the rate of change in economic activity at some earlier time. Kalecki's model is distinguished by his clear separation between investment decision and actual implementation. He used the investment realization lag to explain the cumulative character of expansionary and contractionary processes.

According to Kalecki, if the current rate of investment surpasses that of the preceding period, the level of current profit will rise, profit expectations will improve, investment demand will increase, and more orders will be placed. This will be followed by an increased rate of investment activity and enlarged income. The income generating capacity of investment is the source of prosperity and encourages a further rise in investment. But investment has also a capacity creating effect; every completed investment adds to productive capacity, competes with the stock of equipment of older vintage, and discourages further investment. Sooner or later investment stops rising and so does the level of current profit. The rate of profit falls. The rise in investment is transitory and the boom cannot endure. A process of cumulative contraction takes place. Thus the growth of national wealth contains the seeds of retardation.

Kalecki modified, reformulated, and improved his business cycle model many times, seeking to bring it closer to reality. While his earlier writings were clearly influenced by the severity of the depression of the 1930s, his subsequent development of the argument made allowances for the relatively weak impact of the capital destruction effect. He introduced a certain "corrective"—a trend factor that shifts investment upward as the cycle continues. In a growing economy investment fluctuates along the long-run trend line. Innovations raise the prospects for profit, thus stimulating investment and engendering an ascending trend. Innovation becomes another weighty factor in the determination of the investment function, together with the change in the rate of profit, the rate of change in the stock of capital, and the "internal" gross savings (depreciation and undistributed profits) of firms.

In recasting the argument Kalecki continued his search for a more satisfactory formulation of the investment function, allowing for reper-

cussions of technical progress on the dynamic process as a whole. He sought to develop a theory integrating growth and cyclical processes. While he never reached a fully satisfactory solution, the investment function he initially propounded, and his analysis of the capital stock adjustment mechanism, provided the basis for modern business cycle theory and closely foreshadowed many contemporary developments in econometrics.

(5) To Kalecki the key prerequisite for becoming an entrepreneur was the ownership of capital which determined the amount of outside finance that could be secured. Moreover, entrepreneurs tend to be unwilling to use their full borrowing potential because the risk increases with the amount invested. In the case of a bad investment, the higher the ratio of borrowing to the entrepreneur's own capital, the greater is the decrease of the entrepreneur's income or the risk of wiping out his equity. These considerations cannot be ignored in the theory of investment decisions and in the analysis of factors circumscribing the size of the firm. Such decisions are related to the firm's "internal" accumulation of gross savings. These savings allow the firm to make new investments without facing the problems of the limited capital market, or "increasing risk."

(6) Kalecki dealt with an open system. He treated the rate of export surplus as a promoter of prosperity and the balance-of-payments difficulties that tend to accompany an upswing as a factor limiting expansion.

(7) Whatever the rationale of the *economics* of full employment, the *political* problems are formidable. Kalecki realized that full-employment policies could be used to reform the capitalist system toward beneficial welfare-oriented growth, whose fruits would be directed to the advancement of living standards of the lower income groups. He saw the opportunity, but was mindful of the grave political problems and, in 1943, predicted the emergence of the political business cycle. He argued that opposition by the "leaders of industry" to full employment stimulated by government spending may be expected because of the inherent fear of government interference (especially opposition to deficit spending), opposition to the objects of government spending (particularly to public investments and the subsidizing of consumption), fear of inflationary pressures, opposition to sustained full employment (as against mere prevention of deep depressions), and the dislike of the social

and economic changes resulting from the maintenance of full employment (including laxity of workers' discipline). He felt that business cycles in milder form would continue and result in a sort of stop-go economy. The government would stimulate business activity, then in the upswing it would withdraw under the clamor of an "unsound" financial situation (and even undertake deflationary policies near the peak) only to re-emerge as a stimulating agent when unemployment again rose above an "acceptable level."

Because of his silence about his priority of publication and, in general, his distaste for "promoting" or "advertising" his ideas, Kalecki has always been regarded as a very modest man. But he was totally free of false modesty. He was a stern moralist whose sense of rectitude, forthrightness, and integrity permeated his personal and intellectual life. While in personal dealings he was careful not to offend, in economics he was stubborn and merciless when discussing his economic theories. His ready wit could be destructive, thereby antagonizing many who were offended by his polemics. He courted controversy and was well known for courageously voicing unpopular views.

On the surface Kalecki's economic writings were dispassionate exercises in economic analyses of a highly technical nature. All of his major contributions, however, have their social and ethical underpinnings. The main concerns of his life's work were full employment, its composition and distributional equity. He was not impressed with huge sacrifices made by the present generation for future ones. He was outraged at high employment contrived by means of armaments buildup. He was disturbed by the blunders of socialist planners at the cost of the worker-consumer.

Kalecki spent the war years at the Oxford Institute of Statistics, where he developed the subtle device of general (expenditure) rationing to expand the range of economic choice under the trying conditions of a war economy. His proposal was designed to allow the consumer the widest possible choice under restrictive conditions; it would divert real resources from consumption to the war effort while keeping down inflationary pressures and reducing inequalities of distribution. In addition to his work on the war economy, he produced during this period a number of papers including a major essay on ways to full employment, modified the Gibrat distribution, and pursued his theoretical work on economic dynamics.

Until his return to Poland in 1955 he was an international civil servant mainly at the UN. During the remaining years of his life he served as economic adviser, preparing what is probably the most exemplary perspective plan (1961–1975) ever devised for a socialist economy. This plan, like the other constructive alternative plans suggested by Kalecki in later years, was rejected by the Polish leadership. At the same time he pursued an active career as teacher and researcher. He laid the foundations for a theory of growth under central planning, contributed to advances in planning methodology and investment and foreign trade efficiency calculations, wrote about the Third World, and advanced his theory of modern capitalism. He also resumed his interest in pure mathematics and made a significant contribution to the theory of numbers.

Despite many disappointments, until 1968 Kalecki led a relatively happy life, sustained by the general esteem of colleagues and students. He died on April 17, 1970, a deeply saddened man whose pessimistic views of humanity and its frailties were reinforced by the 1968 wave of repression in Poland and the cowardly behavior of many of his colleagues. It is unfortunate that the life of so productive a man should have been plagued by so many crises and bitter disappointments. He simply would not compromise his principles, and looking back over his troubled years, Kalecki observed that his life could be compressed into a series of resignations in protest—against tyranny, prejudice, and oppression.

GEORGE R. FEIWEL

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KATZ, DANIEL

The career of Daniel Katz spans the development of social psychology as an empirical discipline, and his work has been of major importance in that development. He was among the earliest users of survey methods to study social psychological problems, and he improved the methods with which he worked. His early studies of racial stereotypes illuminated the nature of prejudice as his later research clarified the different forms of nationalism. His theoret-

ical work on attitude formation and change has provided a framework for relating methods of change to the different motivational bases of attitudes. His work on formal organizations has brought to social psychology a level of phenomena typically neglected by psychologists. One of the unifying characteristics of his varied work is a concern for the interplay of social structure and social process, the explanation of human behavior in terms of the structure of events in which it occurs. This emphasis reflects the theoretical views of Katz's influential teacher, Floyd H. Allport, and is explicit in Katz's own development of open-system theory.

Daniel Katz was born in Trenton, New Jersey, on July 19, 1903. The family later moved to Buffalo, New York, and Katz graduated from the University of Buffalo in 1925. His first published research dates from this undergraduate period; it is a study of the acculturation of Polish immigrants and their children (Katz & Carpenter 1927). Katz conducted this research jointly with Niles Carpenter, chairman of the sociology department, who had studied at Harvard University and was committed to empirical research in a field still predominantly social-philosophical.

The findings of this intergenerational study show a complex familial pattern: the Polish-born parents retained their stereotypic values and attitudes about parental authority, but their American-born children were more expert about the new society. The children acted on their expertise, and the parents adjusted to their doing so, accepting in pragmatic terms what they could not acknowledge ideologically. Apart from being an undergraduate publication, the work is of interest in forecasting several of Katz's enduring research concerns: the quantitative measurement of values and attitudes, the explanation of group differences in terms of differing external influences, and the conduct of social research in real-life settings.

In 1925 Katz went from Buffalo to Syracuse University to start his doctoral work on a fellowship that the chancellor of the University of Buffalo had helped to arrange. Syracuse was in many ways a fortunate choice. Floyd H. Allport had moved there a year earlier to take a chair in the university's new social science enterprise, the Maxwell Graduate School of Citizenship and Public Affairs. Allport's pioneering *Social Psychology* had been published that year, and he was already launched on a lifelong effort in the theoretical integration of individual and social phenomena. Katz committed himself to social

psychology and became Allport's first doctoral student at Syracuse. His dissertation, published jointly with Allport under the title *Students' Attitudes: A Report of the Syracuse University Reaction Study* (1931), was a quantitative description of the student culture of that university and its several component subcultures.

On completion of his doctorate in 1928, Katz moved to Princeton University, where he remained for 15 years. His publications during those years range widely in psychology and social psychology—experiments on eye movement and the Phi phenomenon, reviews of psychology for annual volumes of the *Britannica*, and articles on aesthetics and epistemology in the social sciences. His work on attitudes and on attitude measurement, especially by means of survey methods, continued during these years, and he became recognized as a survey methodologist.

The best known work of his Princeton years may well be the experiments on racial prejudices and racial stereotypes he conducted with Kenneth W. Braly (1935). The authors showed that students agreed among themselves in their preferential ranking of ethnic groups, that their attribution of characteristics to these groups was consistent with these rankings, and that this pattern (stereotypy) could not be plausibly explained in terms of the students' actual contact with or direct knowledge of the groups in question. During this period Katz published *Social Psychology* (1938), coauthored with another of Allport's early students, Richard L. Schanck. The book was characterized by a strong emphasis on research data and by an attempt to deal with different levels of social phenomena without such familiar reifications as "group mind" or "collective conscience."

In the early 1940s many social scientists were leaving their universities to take on work more directly related to the war effort. Several groups of quantitatively oriented sociologists and psychologists were forming in government agencies. Katz joined one such group that had been founded at the U.S. Department of Agriculture by Rensis Likert, and he maintained close ties with it after moving to the Office of War Information. Katz's research in organizational settings dates from this period and is well exemplified by a study of worker morale in wartime, conducted jointly with Herbert H. Hyman (1947). This research was ahead of its time in using a multiorganizational design (five shipyards), a representative sample of workers

within organizations, and a combination of self-reported and independent measures of the major variables. The findings emphasized the objective organizational determinants of worker satisfaction and morale (earnings, supervisory behavior, promotional opportunities, and the like), and the complex reciprocal relations between worker attitudes and productivity.

At the end of World War II, Katz went to Brooklyn College as the first chairman of its department of psychology, an appointment that he had accepted in 1943 while still in government service. That service had ended with an overseas assignment as senior analyst on the U.S. Strategic Bombing Survey, a set of studies of the effects of bombing on civilian attitudes and morale. The time at Brooklyn was successful in creating a strong department and training students who went on to prominence as social psychologists. It was, however, a brief period; in 1947 Katz went to the University of Michigan, where he has remained.

The move to Michigan was something of a reunion. The group of social psychologists led by Rensis Likert at the U.S. Department of Agriculture had just established the Survey Research Center at the University of Michigan; the department of psychology at Michigan had begun an extended development; and an interdisciplinary doctoral program in social psychology had been created under the direction of Theodore M. Newcomb.

Katz entered into all these activities, concentrating initially on the development within the Survey Research Center of a research program on social-psychological aspects of large-scale organizations. He was the first director of this program of research, which has continued and grown, and along with Likert he set its initial theoretical and methodological orientations. After he left the formal direction of the program in order to give more time to teaching and departmental activities, his commitment to organizational research continued. He was instrumental also in establishing organizational psychology as a field of doctoral study that stressed the interaction of organizational structure and individual behavior rather than the more conventional problems of selection, testing, and the like.

Katz's career spans more than fifty years. His writings are unusual in range, responsiveness to large social issues, and choice of problems at the boundary of psychology and other fields of social science; nevertheless, certain themes

and commitments persist. Especially since 1947, three substantive concerns are dominant:

(1) organizational life and its ramifications, including the nature of leadership, the determinants of organizational effectiveness, and the nature of satisfactions and deprivations at work;

(2) political structure and process, especially the phenomenon of nationalism, the resolution of conflict, and the nature of identification with political parties;

(3) attitude formation and change, including specific studies of prejudice and the development of a general theory of attitude change.

These areas of research are linked by two other persistent emphases:

(4) a concern for the improvement of research methods; and

(5) the use and development of open-system theory as a framework within which more specific substantive interests could be expressed. For example, in each of his substantive areas of work—organizational life, political behavior, and attitude change—we also find methodological articles. The book on research methods in the behavioral sciences, edited jointly with Leon Festinger (1953), set research standards for a generation of graduate students and research workers.

Katz's development of open-system theory is stated in *The Social Psychology of Organizations* (Katz & Kahn 1966). It owes much to other field theories and most, perhaps, to Allport's general theory of event-systems (never fully published). Katz's use of the open-system framework, however, is specific to the social psychological level. In his view, social psychologists had taken too little account of social structure and had been especially neglectful of organizational influences on human behavior. He proposed the open-system approach as a means of overcoming these limitations and created a social psychology concerned with social structure. The second edition of *The Social Psychology of Organizations*, published in Katz's seventy-fifth year, expresses this theoretical position most fully.

Katz's main efforts have been original rather than critical, but he has been influential as editor and critic. As editor of the *Journal of Abnormal and Social Psychology* from 1962 to 1964, and of the *Journal of Personality and Social Psychology* from 1964 to 1967, he played a leading editorial role during a transitional period in which the main journal for social psychologists was separated from the field of abnormal psychology and established in

its own right. He wrote several essays on the state of social psychology as a discipline, two of them undertaken as reviews of editions of the *Handbook of Social Psychology* (Lindzey 1954). His earlier articles on survey methods had appeared when standards were first sought in that rapidly expanding field.

Academic biographies are notoriously impersonal, but accurate description of Katz's place in social science requires some mention of his personal qualities. Colleagues know and students quickly recognize his absolute integrity, commitment to the intellectual life, and unflinching consideration for others. The example of high character may not be a requirement for scientific contribution, but it enriches those who encounter it.

ROBERT L. KAHN

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KENNAN, GEORGE F.

As diplomat, historian, foreign affairs analyst, and publicist, George Frost Kennan enjoyed a varied and controversial career. Whether as practitioner or intellectual, bureaucrat or critic, he has demonstrated enviable traits: brilliant analysis, elegant language, introspective ability to adjust his ideas to the changing times, courage to speak out, and a studied, mannered style bespeaking reflection and authority. These qualities often propelled him after World War II to the center of national debates on the goals and methods of American foreign policy and prompted America's political leaders to entertain, if not always heed, his counsel. He has been widely recognized as the father of the "containment doctrine" formulated in the 1940s, the foiled advocate of "disengagement" in the 1950s, an eloquent spokesman for *détente* and "neoisolationism" and against global interventionism in the 1960s-1970s, the steady critic of the inner decay and mediocrity of American society—and its consequent flawed diplomacy—and the Cassandra who warned against the "cloud of danger" hovering over a world burdened with a nuclear arms buildup, the spoilation of the environment, and short-sighted leadership.

Career. Born in 1904 to a middle-income family in Milwaukee, Wisconsin, Kennan grad-

uated from Princeton University and entered the Foreign Service in 1926. He became an area specialist, concentrating on Soviet Russia, and mastered the German, French, and Russian languages. He served a short stint in Riga, Latvia, where he interviewed anti-Soviet *émigrés* from Russia, and went on to the University of Berlin to study Russian history, literature, and language. Always impatient with routine bureaucratic chores and the insufficient attention he thought superior officers paid to his work, Kennan spent the depression decade at posts in Riga, Moscow, Washington, and Prague, and World War II in Portugal, Germany, and England. In 1944 U.S. Ambassador W. Averell Harriman selected him to be a key adviser in the Moscow embassy. Until 1946 Kennan made no appreciable impact upon American foreign policy, although he himself developed a profound pessimism about Soviet-American relations and plucked the sleeves of all who would listen. But with a February 22, 1946 "long telegram" to Washington on the wellsprings of Soviet behavior, Kennan emerged from relative obscurity to join the American diplomatic hierarchy. "My reputation was made," Kennan recalled. "My voice now carried" (1967, p. 295). He was soon assigned to the National War College in Washington, D.C.

In 1947 Kennan was ordered to create and direct the policy planning staff, an in-house "think tank" designed to provide the secretary of state with the long-range perspective and policy recommendations. An essay Kennan had written earlier for Secretary of the Navy (later Defense) James V. Forrestal appeared in the July 1947 issue of *Foreign Affairs* magazine anonymously under the name "X." This highly influential article on "The Sources of Soviet Conduct," urging a policy of containment of communism for the United States, created a public sensation, and Kennan was soon revealed as its author, establishing him as the most prominent American expert on contemporary Soviet affairs. Until 1950 he and his staff turned out numerous secret reports on cold war crises and helped launch the Marshall Plan. But Kennan fell out with Secretary of State Dean Acheson, who, to Kennan's seldom concealed chagrin, increasingly implemented containment through military means and seemed to abandon diplomacy itself. Kennan moved from Washington to the scholarly quietude of the Princeton-based Institute for Advanced Study. He soon lectured Americans that they

were too emotional and simple-minded in their responses to the complexities of world politics and published these views in *American Diplomacy, 1900-1950* (1951), a book whose sales numbered more than a million copies by the early 1970s when its message was still being read by countless college students. In 1952 he served briefly as ambassador to the Soviet Union. That tour of duty was cut short when the Kremlin declared him *persona non grata* for his uncharacteristically undiplomatic remark to newsmen that living conditions for foreigners in Moscow were not unlike those for diplomats trapped in central Europe by the Nazis at the start of World War II.

Kennan again retreated to the institute, turned enthusiastically to historical research, and wrote two well-respected volumes on *Soviet-American Relations, 1917-1920* (1956-1958). In November-December 1957, Kennan brought himself once again to the center of public discussion on two continents with his Reith lectures broadcast over British Broadcasting Corporation radio and published as *Russia, the Atom and the West* (1958b). Therein Kennan advised American and European officials to seek a mutual disengagement of Soviet and American forces from the heart of Europe and to defuse the cold war. Senator John F. Kennedy was impressed by Kennan's reasoning and upon becoming president in 1961 appointed him ambassador to Yugoslavia. Kennan resigned in quiet protest two years later because the United States, still gripped by the cold war mentality which did not distinguish among different types of communist governments, attempted to punish Tito's Yugoslavia by cancelling the "most favored nation" provision in trade relations. During the troubled 1960s, when the nation was mired in tortuous war in Indochina, Kennan once again became headline news when he declared that the containment doctrine, meant for a European context, did not fit Asia. "I find myself," he said, "in many respects sort of a neo-isolationist" (quoted in Paterson 1973, p. 160). His prolific writing and lecturing thereafter expressed that sentiment. His summing up, *The Cloud of Danger* (1977), was a plea for restraint in foreign policy, for minding one's own business in a world of complex problems for which the United States had few answers.

The containment doctrine. By the mid-1940s George F. Kennan had come to intensely held ideas about the Soviet Union and American policy toward it. "Never," Kennan has written,

“did I consider the Soviet Union a fit ally or associate, actual or potential, for this country” (1967, p. 57). He judged Joseph Stalin a ruthless realist and Marxism a “pseudo-science, replete with artificial heroes and villains.” “I felt,” Kennan explained, “that it [communism] must some day be punished as all ignorant presumption and egotism must be punished” (quoted in Paterson 1974, pp. 252, 256). Having witnessed the ghastly purges and repression of the Soviet system in the 1930s, he considered it morally repugnant. As for Soviet diplomats, they were blunt and rude, violating Kennan’s sense of the gentlemanly profession he himself practiced with such devotion and pride. The Soviet Union could not be trusted; it seemed bent on global preeminence; it posed a supreme threat to the United States.

Kennan summarized his thoughts in an 8,000-word telegram he dispatched from Moscow on February 22, 1946. Marxist–Leninist ideology, Kennan advised, was a driving force behind Soviet foreign policy. The Soviets foresaw inexorable capitalist–socialist conflict. Yet at the bottom of the Kremlin’s “neurotic view of world affairs is [the] traditional and instinctive Russian sense of insecurity.” Communist dogma, Kennan wrote, was the new vehicle for perpetuating this very Russian tradition necessitating geographical expansion. “In summary, we have here a political force committed fanatically to the belief that with [the] U.S. there can be no permanent *modus vivendi*, that it is desirable and necessary that the internal harmony of our society be disrupted . . . if Soviet power is to be secure” (1967, pp. 549, 557). Kennan’s alarmist language was tempered by his conclusion, which received far less attention, that Russia would not unleash war because it was too weak. In Washington, high-level officials relished Kennan’s message, for it captured their moods and fears after Soviet–American diplomatic clashes over eastern Europe and Iran. It was embraced, too, because Kennan seemed to absolve Americans of any responsibility for the cascading cold war. The “long telegram,” Kennan admitted later, read “exactly like one of those primers put out by alarmed Congressional committees or by the Daughters of the American Revolution, designed to arouse the citizenry to the dangers of the Communist conspiracy” (1967, p. 294). Indeed, at the time, in 1946, it contributed to the “get tough” posture of the Truman administration.

Kennan’s widely read and reprinted “X” ar-

ticle in *Foreign Affairs* the following year repeated many of the themes of the earlier cable, highlighting implacable communist hostility, Stalinist totalitarianism, and the duplicity and suspiciousness of Soviet conduct. And the essay recommended, in language that would henceforth become an American cold war liturgy, that “the main element of any United States policy toward the Soviet Union must be that of a long-term, patient but firm and vigilant containment of Russian expansive tendencies.” Indeed, American “counter-force” had to be applied to “constantly shifting geographical and political points” to curb Russia and to create strains in the Soviet system which might weaken or break it up (1947, pp. 575, 576).

Kennan’s analysis of Soviet behavior and his prescription of containment met with wide approval at the time. One of the few critics was Walter Lippmann, who, in *The Cold War* (1947), found Kennan’s containment doctrine a “strategic monstrosity,” because it did not, at least as spelled out in the “X” article, discriminate between areas vital and peripheral to American national security. Containment, the venerable journalist and political commentator predicted, would test American resources and patience without end or limit. Lippmann also observed that the policy would probably have to be implemented distastefully through American coddling of “satellites, clients, dependents and puppets” (Lippmann 1947, pp. 18, 21). In 1952, during the presidential campaign, the Republicans, led by John Foster Dulles’ attack, scored the containment doctrine for being too defensive, too negative, leaving the initiative to the Soviets. Dulles said he would substitute “liberation” for Kennan’s vague formula.

In the years after, some scholars found Kennan’s explanations of Soviet behavior too speculative, placing too much stress on communist ideology and thereby positing a mechanistic view of Soviet foreign policy: it flowed not from interaction with other nations but from internal imperatives. Doubted was his conclusion in the “X” article that Soviet power “moves inexorably along the prescribed path, like a persistent toy automobile wound up and headed in a given direction, stopping only when it meets with some unanswerable force” (1947, p. 574). Rarely did Kennan suggest that the actions of noncommunist, or anticommunist, countries, such as the allied military intervention in the Russian civil war beginning in 1918, influenced Soviet decisions. He glaringly ignored American

power and expansionism as contributing factors to Soviet-American tension. Most important for the long-term application of containment, Kennan used words like "force" without precise meaning, thus inviting a host of methods for implementing his recommendation of "counterforce." Again and again after the 1940s, and especially in Kennan's memoirs, he claimed that he never meant that the United States should emphasize military over economic or political means or that crusading globalism and knee-jerk anticommunism should distinguish American diplomacy. Contemporary documents suggest that Kennan entertained a variety of methods, including military, but that, applying a pragmatic yardstick, he largely excluded Asia from the purview of containment. Yet Kennan's ideas and policy prescriptions, cast as they were in imprecise language and based on the assumption that because the threat was global, the effort to curb it had logically to be likewise universal, lent themselves to American decisions that the diplomat himself opposed, such as military aid to the French in their war at midcentury to shore up their crumbling colonial empire in Indochina. The father of containment would spend much of his life defining the purposes of his own offspring. As he wrote in his memoirs, he felt "like one who has inadvertently loosened a large boulder from the top of the cliff and now helplessly witnesses its path of destruction in the valley below, shuddering and wincing at each successive glimpse of disaster" (1967, p. 356).

Disengagement. After Stalin's death in 1953 and an apparent thaw in the cold war suggested by the peace settlement over Austria, the Soviet appeal for "peaceful coexistence," and Nikita Khrushchev's "de-Stalinization" program, a number of European leaders called for a reduction of armaments and troops on the continent. Other events at the same time, however, heated up the cold war: West Germany's entry into the North Atlantic Treaty Organization, the Soviet crushing of the Hungarian rebellion, the Suez crisis, and the firing of the missile *Sputnik*. Fearful that the United States might undertake feverish military missile development and an enlargement of NATO, and believing that the United States should undertake negotiations leading to the withdrawal of American troops from West Germany and a Soviet military withdrawal from eastern Europe, George F. Kennan spoke out in late 1957 in six Reith lectures. The architect of containment, read the headlines,

seemed to be abandoning his creation in favor of "disengagement." Kennan urged upon his listeners and readers a unified, nonaligned Germany, withdrawal of foreign troops from eastern Europe and Germany, and restrictions on the deployment of atomic weapons there. He was critical of strengthening NATO and pled that diplomacy replace an apparent American military fixation. The Stalinist Russia he had sketched in the "X" article was not the Russia of Khrushchev. The time was pregnant for tempering the cold war, Kennan argued.

A flurry of speeches, serial articles in such opinion magazines as *The New Republic* and *Foreign Affairs*, and stinging comments from Harry S. Truman and Dean Acheson jolted Kennan. The counterattack was formidable: Russia was still a military threat; American troops should remain in Germany as a deterrent; Russia could not be trusted in any agreement; Soviet political control of eastern Europe would not end with the removal of military control; if the eastern Europeans tried to throw off the Soviet yoke, Moscow's soldiers would fight back; a united Germany might become a new and threatening empire that would arouse fear in the Soviets or might even, as in 1939, join Russia in a menacing anti-American pact. Kennan fought back, finding his detractors too attached to a hardened cold war mentality. "Nothing, for example, in Mr. Acheson's attack on the lectures would suggest that he sees the faintest difference between the Soviet government as it is today and the same government as it was eight years ago" (January 18, 1958, Kennan Papers, Princeton University Library). One of Kennan's allies now was a former critic, Walter Lippman. They agreed that military disengagement was a necessity and that the time was ripe for diplomacy. Anyway, said Kennan, the West could not know Russian intentions and could not know whether his suggestions would work unless officials sat down at the negotiating table to find out. In those days of intense cold war passions, however, the weight of opinion went against Kennan.

The conduct of American foreign policy and neoisolationism. In 1951, in a set of lectures published as *American Diplomacy, 1900-1950*, Kennan expressed views to which he would return time and time again: Americans were too emotional, too afflicted with a "legalistic-moralistic approach" (1951, p. 95), too susceptible to stampedes of opinion, too tolerant of narrow special-interest lobbyists, too easily swayed by

simple answers. During the era of McCarthyism Kennan vigorously inveighed against mindless anticommunism. Later he deplored the "congenital subjectivity of the American perception of the outside world" (1976, p. 678). The congress and the president, people of mediocre talent, played politics with foreign policy, creating a misleading bogeyman image of the Soviet Union as a military monster. Russia, Kennan always insisted, did not pose a military threat to United States security. In 1951 and after, he appealed for a professional foreign service, an elite corps of bright, imaginative people undisturbed by the buffeting winds of public opinion and democratic politics. In 1976 he set the reformist tasks for American statesmanship:

It will have to overcome that subjectivity that caused Americans to be strongly pro-Soviet at the height of the Stalin era and equally anti-Soviet in the days of Khrushchev, and to acquire a greater steadiness and realism of vision before the phenomenon of Soviet power. It will have to make greater progress than it has made to date in controlling the compulsions of the military-industrial complex and in addressing itself seriously to the diminution, whether by agreement or by unilateral restraint or both, of the scope and intensity of the weapons race.

American politicians will have to learn to resist the urge to exploit, as a target for rhetorical demonstrations of belligerent vigilance, the image of a formidable external rival in world affairs. And American diplomacy will have to overcome, in greater measure than it has done to date, those problems of privacy of decision and long-term consistency of behavior which, as Tocqueville once pointed out, were bound to burden American democracy when the country rose to the stature of a great power. In all of this, American statesmanship will need the support of a press and communications media more serious, and less inclined to oversimplify and dramatize in their coverage of American foreign policy, than what we have known in the recent past. (1976, p. 689)

Kennan was also a persistent critic of American life, finding it by the 1970s ill suited as a model for other nations to imitate. "Foreign policy, like a great many other things," he wrote in *The Cloud of Danger*, "begins at home" (1977, p. 26). Only if America reformed itself, attended to its myriad internal problems of environmental pollution, commercialism, waste of natural resources, rampant pornography, urban disintegration, and political disunity and mediocrity, could it influence other peoples. During the 1960s and 1970s, a very pessimistic Kennan

spoke and wrote against American interventions in the Third World (especially in Indochina), foreign aid programs, the continued militarization of the cold war, and the sad neglect of diplomacy as a means to reduce crises. He complained that the United States had assumed the role of world policeman, thrusting itself into regions and problems where it inevitably stumbled because it lacked the necessary power, intelligence, inner strength, and cultural affinity to provide solutions.

The label "isolationist," a term often used pejoratively after the 1930s, attracted Kennan. He took it to mean, in a constructive sense, a reduction of the military establishment, more serious attention to domestic ills, and a frank recognition that Washington had neither the duty nor the capability to serve as mankind's guardian and savior. As for the global economic crisis—energy and food shortages, overpopulation, monetary disorder, and trade wars—Kennan argued against interdependence. He advised that the United States deal with other states individually, eschewing the multilateral arrangements that limited America's freedom of action. The United States had few vital interests abroad; only western Europe and Japan warranted a protective American shield. "The first requirement for getting on with most foreign peoples," Kennan concluded, "is to demonstrate that you are quite capable of getting on without them" (1977, p. 70). The emergence of Soviet-American *détente* in the early 1970s met with Kennan's approval, yet he thought the Nixon administration had oversold it, causing Americans to expect too much and then to suffer disappointment, if not anger, over the slow movement toward diplomatic accommodation. And in the later years of the decade he criticized President Jimmy Carter for attempting the impossible: reform of the Soviet system through a vocal "human rights" policy.

Kennan, as always, had his critics in the 1970s. Many commentators found him too gloomy, futilely trying to recover a simpler, rural life of the past. They thought him too elitist, too out of touch with the ways of a democratic society. Some analysts argued that the United States, whether it wished it or not, was profoundly intertwined in the world's problems, ensnared in interdependence, incapable of significant retrenchment. Others believed, unlike Kennan, that the United States had a humane responsibility as a wealthy nation to meet the needs of less fortunate peoples, especially for

food. Russia was still a threat, other analysts intoned, and they quoted Kennan's 1947 "X" article back at him. Kennan, the realist of the 1940s, had become, it seemed to his detractors, the dreamer of the 1970s. Still, Kennan's cry for restraint and humility in foreign policy, for neoisolationism, met a sympathetic reception in an America sobered by its bloody and costly failure in Indochina and beleaguered by a rash of domestic troubles long ignored. Yet Kennan, indulging his habit of self-anguish, was sure he had aroused no followers. He was wrong, of course, but from his Olympian perch, so far removed from the many people who admired his wisdom and his eloquence, he underestimated his influence upon a generation of Americans.

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Otto Klineberg, born in 1899, is, understandably, best known as an American social psychologist because of the many contributions he made to that field during his 37 years of residence in the United States, first as a member of the Columbia University department of psychology, and toward the end, as the first chairman of a separate department of social psychology. Equally well, he could be labelled a Canadian psychologist, having been born in the city of Quebec and having done his undergraduate studies in philosophy and psychology at McGill University (B.A., 1919). Equally, he deserves the title physician, having completed the M.D. at McGill in 1925, two years before receiving his Ph.D. from Columbia. He also deserves the label anthropologist, having been a student and then, from 1929 to 1931, a research associate of Franz Boas in the Columbia department of anthropology. He did his first field work with Yakima Indian children at a reservation in the northwestern United States and with students from 36 Indian tribes enrolled at the Haskell Institute in Kansas (1926–1927). At the urging of Ruth Benedict, his later field work was in Mexico among the Huichol Indians (1974, p. 172).

In 1973, he referred to himself as "an international psychologist of Canadian origin," and this surely describes the global course of his career: from 1927 to 1929 research in Italy, France, and Germany under a National Research Council fellowship; from 1935 to 1936 research in China under a Guggenheim fellowship; from 1945 to 1947 in Brazil at the University of São Paulo as the first professor of psychology; from 1948 to 1949 and again from 1953 to 1955 in Paris as director of various international research projects and activities of the United Nations Educational, Scientific and Cultural Organization (UNESCO). Upon retiring from Columbia University in 1962, he became visiting professor in social psychology at the University of Paris, the Sorbonne, where, as of 1979, he still held a chair in ethnic psychology as well as the directorship of the International Center for the Study of Intergroup Relations. From Paris, he commuted monthly for many years to Rome as the visiting lecturer in intergroup relations at the Università Internazionale degli Studi Pro Deo.

His multidisciplinary training and multicultural experiences influenced the style and

themes of his work. This, however, is not to suggest a doctrine of crude geographical determinism, that Klineberg happened to be blessed by birth and growth in two cultures and the gift of a ticket around the world, complete with admission to lectures by Boas, Edward Sapir, Floyd Allport, and Robert S. Woodworth, and simply reaped the benefits of his good fortune. Others sat in these same lecture halls and have never been heard from since; others got a ticket to travel the world and never came back to science.

Although Klineberg does see chance as having played an important part in his career, he always made the most of his opportunities. By graduating from McGill University with first class honors and the Prince of Wales gold medal, he was able to obtain a scholarship to Harvard University, where he received his M.A. in 1920. Although he planned to study for the Ph.D. and then pursue a career in Canada in academic psychology, his former professors discouraged him because of the lack of job opportunities and urged him to obtain a professional degree. He thus decided to study psychiatry at the Medical School at McGill. By the time he became a physician, he knew that in spite of the scarcity of jobs he really wanted to teach psychology. So, he immediately enrolled in the Columbia University department of psychology as a doctoral candidate. In the process, however, he discovered anthropology courses by Sapir and Boas, and became as deeply involved in that discipline as in psychology.

Studies of race differences. In 1926, Klineberg began his work in the field of race, which continued throughout his life, and deservedly, brought him great recognition. His own studies plus his comprehensive reviews of the evidence led him to conclude "that psychological research gives no justification for the belief in a racial hierarchy of abilities" (1974, p. 167). Actually, his studies of race began accidentally when he was invited to ride with a graduate student in anthropology to the West coast, where Klineberg hoped to do research among the Indians. When Klineberg requested research funds for this purpose, Woodworth agreed to secure him a small sum if he used the research as the basis of his doctoral dissertation. Klineberg accepted this condition and thus shifted the emphasis of his work from abnormal psychology to race (*ibid.*, pp. 166-167).

The money and the free ride to the Yakima reservation in the state of Washington was the

source of his work. Klineberg began to administer performance tests of intelligence to Indian children selected at random, and to white children, also chosen at random, who lived in the town of Toppenish in the heart of the reservation. All of the children attended the same school. An unanticipated finding—that Indian children performed tasks much more slowly than did white children, but made fewer mistakes (1928, pp. 27-31)—led Klineberg to focus "on the factor of speed in group comparisons." Other observers, less acute and less sensitive to the role of culture, might have neglected the finding. As he later noted in *Race Differences* (1935*b*), many intelligence tests depend to some extent on speed and the scores are affected by the speed at which the questions are answered (p. 159). This leads to the unwarranted inference of racial or innate inferiority in intelligence among groups whose cultures have not taught speed and who therefore are unfairly penalized by such tests.

The test performances of the white and Indian children were indeed congruent with the respective patterns of their cultures: the hustle and bustle of American life and the slow tempo of Yakima life, the proverbial American value that the race is to the swift and the Indian precept to think carefully before one acts. But Klineberg did not rest his case solely on the ground of congruence. He carefully examined the possibility that the differences in tempo might in fact be racial, rather than cultural, in origin. As a trained physician, he could analyze competently the literature on group differences in basal metabolism and other physiological processes, and concluded that environmental factors also had considerable influence on the very tempo of the body. His direct approach to the cause of the differences was a research design in which groups of the same race, but living in contrasting environments, were tested and compared. He found that Indian children at the Haskell Institute, where white teachers encouraged them to work as quickly as possible, performed much more quickly on his tests than did the Yakima children.

Thus, the study of race differences, begun as a chance encounter, became a lifelong passion for Klineberg. But the style he displayed then, as in his later works, involved the careful presentation of alternative hypotheses, however uncongenial. He invented ingenious quasi-experimental designs, rendered judgment only after scrutiny of substantial and varied evidence, and

kept a sharp watch for artifacts that might result in misleading findings. One particular design proved to be the archetype of his later studies. Just as he compared the scores of Indian children contrasted in length of time at Haskell, he also tested a group of black children in Harlem and related their scores to their length of residence in New York City. By this design he tried to establish the claims of a theory of cultural determination as opposed to a theory of selective migration.

In 1927, after completing his dissertation, Klineberg was offered an instructorship at the City College of New York. Although jobs were still difficult to find, he decided instead to accept a National Research Council fellowship to study in Europe.

His research in Europe was a test of the theory or myth of Nordic superiority (1931). Later, in *Race Differences*, Klineberg specifically reviewed the long history of that myth and similar racial theories. It is perhaps a bitter lesson for scientists to recall that Klineberg's refutation of the theory during 1928 and 1929 occurred so shortly before Hitler resurrected the myth. More generally, he tested whether differences in the intelligence of racial groups were innate in origin or caused by the environments and cultures in which they were reared, and the design he employed was a more elaborate version of the basic design of the first study.

Earlier, he had compared members of the same racial group, the American Indian, living under contrasting conditions. Now, the size and heterogeneity of France, Germany, and Italy permitted him to locate and compare children from the same racial group—Nordic, Alpine, or Mediterranean—who were living in the contrasting environments of those three nations, and also to compare the test scores of the different racial groups when they were living in the same national environment. All the children in this quasi-experimental design, however, had in common the characteristics that they were boys aged 10 to 12, living in small communities in rural areas, and attending school. Most were from peasant or farm families. Since these factors were held constant, and since the tests were performance tests, always administered individually in the native language by Klineberg, methodological variables could not invalidate the comparisons or unfairly handicap any group.

A fair test of the theory also required that the

groups not be selected in an arbitrary or inaccurate fashion, and that the children be as representative as possible of "pure" racial types. Klineberg's medical and anthropological training again aided him. Boas had created a revolution in physical anthropology by rejecting subjective classification of races and by his further objection to the use of a single, arbitrarily chosen physical characteristic as the basis for the classification. Klineberg, correspondingly, chose his sites by reference to anthropological maps of the distribution of the various racial types, and then, within the most typical rural areas, chose his subjects by the combination of hair and eye color, cephalic index computed by head measurements, and by the inclusion of only those children who had been born there and whose parents had been born there.

Klineberg concluded that "the concept of 'race' is quite incapable of explaining the results, since one and the same racial group will show up very differently depending on which national sample is taken as representative" (1931, p. 29). When he compared the different racial groups within the same nation, the Nordics were in no instance superior. In one nation, the differences were not statistically significant; in one, the Alpines scored the highest; in another, the Mediterranean group scored the best. "As far as these results go, they offer no substantiation of a definite racial hierarchy" (*ibid.*, p. 35).

In 1935, Klineberg published *Negro Intelligence and Selective Migration*, which was a large-scale, crucial test of his theory. In his first study, the fact that the Indian pupils at the Haskell Institute were quicker than those on the reservation led Klineberg to conclude that the environment accounted for the pattern of behavior. In his European study, his analysis of the intelligence of the various racial groups led him again to the same conclusion. The many earlier studies of the comparative intelligence of blacks and whites, which found that Northern blacks scored higher than Southern blacks and higher than whites from some Southern states, also suggested to Klineberg that environment and not innate endowment accounts for the relative intelligence of racial groups.

Since none of these studies, however, are true experiments in which groups are assigned at random to live in various environments, the alternative explanation has been advanced that the smart families migrate selectively to the better environments of the city or the North or

the favored region of a country, and their children correspondingly inherit that innate superiority. So, too, the critic might assert that the smart Indian senses the advantage of sending his child to the Haskell Institute. Although Klineberg notes that it is equally plausible that those who are not intelligent enough to succeed in harsh environments are the ones who selectively migrate, it is clear that "nothing of any scientific value can be obtained from this type of subjective" speculation (1935a, p. 5).

Anticipating the susceptibility of his basic design to this alternative argument of selective migration, Klineberg had devised in his first study the more refined design of comparing subgroups varying in the length of exposure to the better environment. If the scores show an improvement with increments of exposure, the alternative explanation becomes dubious, because their innate endowment should have made all the migrant children smart to start with, no matter how long they had been there. Klineberg therefore decided to use this refined design on a grand scale in a study of the intelligence of black children. The monograph, based on 9 comparable master's theses, presents findings on more than 3,000 black boys and girls in New York City between the ages of 10 and 12, whose intelligence (as measured by various verbal and performance tests) and school grades were examined in relation to their length of residence in the North. In general, the findings were that the "rise in intelligence is roughly proportional to length of residence in the more favorable environment" (*ibid.*, p. 59).

Klineberg realized, however, that his conclusions could still be attacked on the ground that selective migration, in a more subtle form, was the explanation. The smarter the family, the sooner they might sense the wisdom of moving. Correspondingly, the children of the earliest migrants could have been the smartest right from the start, and not because they had been in the better environment longer. Klineberg therefore used three supplementary designs to buttress his conclusions. The reasons for migration to the North were examined historically by an analysis of old newspapers and by data from interviews with migrant families. The analysis suggested that the move was mainly the result of coercive or accidental factors rather than a rational decision made on the basis of intelligence.

In a more direct approach to the problem, Klineberg examined school records in three

Southern cities of approximately six hundred black children, all of whom had migrated to the North, to see if they differed in ability from their classmates who had remained in the South. After the raw scores were transformed to provide a proper index of class standing, the average score of the migrants was almost exactly the same as that of the nonmigrants. The evidence, refined to see whether the earliest movers were the smartest, indicated that there had been a gradual improvement in the intellectual level (relative class standing in the South) of the migrants during the 15-year period examined. Klineberg's earlier conclusion about the gradual influence of environment was thus more compelling.

The third approach involved a variation on the basic design. Two of the master's theses which studied the relation of intelligence to years of residence in New York, were replications of each other, except that one was conducted in 1931 and the other a year later. Thus, in the second study, a child with any given length of residence was always a later migrant than the child with the same length of residence in the first study. The differences observed were nonsignificant, but the fact that the earlier migrant children tended to be inferior to their later counterparts made the environmental explanation even more compelling.

In 1935, Klineberg also published a comprehensive review of the literature in medicine, physical and cultural anthropology, and psychology, which dealt with the general problem of racial differences. Two chapters deal with the question he had already examined in his earlier researches of "the inherent intellectual superiority of certain races over others" (1935b, p. 152). As he lists the many factors that affect such test scores and that remain uncontrolled in comparisons between racial groups, it becomes obviously hasty to attribute such observed group differences to innate endowment. Klineberg also reviews the questions of race differences in personality, mental abnormality, crime, patterns of growth, bodily functioning, and physical endowment. He analyzes the many factors other than race that have been found to affect such observed differences, shows that invidious conclusions drawn from such studies are often arbitrary, and introduces proper cautions on hasty inferences from the physical to the psychological.

In the first two sections of the work, Klineberg uses mainly what one might call a "method

of negation," undermining the claims for racially determined psychological differences by a critical analysis of the evidence that had previously been used in support of such a theory. But the fact that races do differ in their behavior cries for an explanation. If it is not physical type or innate endowment, then what causes the differences? The final section of the book is, in effect, a brief, systematic text—trailblazing for its time—in social psychology and reveals the dramatic cultural patterning of the most basic psychological entities: motivation, emotion, cognition, mental organization, and personality. Understandably, the book leads to the conclusions "that there is no adequate proof of fundamental race differences in mentality, and that those differences which are found are in all probability due to culture and the social environment" (*ibid.*, vii). Klineberg, the physician-psychologist-anthropologist, had just the right combination of technical skills for the task, and he used his firsthand knowledge of many cultures tellingly in the book.

At least three more times in the course of his later life Klineberg reviewed the accumulated literature on race. Probably no one has examined the literature as often and as carefully as he has. Under the auspices of the Carnegie Corporation, he edited a volume, *Characteristics of the American Negro* (1944a), containing chapters by various authorities on mental disorders among blacks, stereotypes and attitudes about race, and his own chapters on black intelligence and personality. This book was prepared as essential background and documentation for Gunnar Myrdal's *An American Dilemma* (1944). Then, as director of the project on "Tensions Affecting International Understanding" for UNESCO, Klineberg proposed a series of publications by biologists and social scientists on race and science, and later wrote the book in the series on *Race and Psychology* (1951). Later, at the suggestion of the Society for the Psychological Study of Social Issues (SPSSI) of the American Psychological Association, he again reviewed the literature and wrote an article, "Negro-White Differences in Intelligence Test Performance" (1963). When he had finished this review of the long and confusing literature, he concluded that there was still "no scientifically acceptable evidence for the view that ethnic groups differ in innate abilities" (1963, p. 202).

Klineberg's contributions in the field of race have not been limited to research and scholar-

ship. Although the nature of his writings did have some ultimate influence on race relations, his impact may be seen more directly in the part he played in the events that led to the United States Supreme Court's 1954 decision on desegregation. In one of the five major cases in which the National Association for the Advancement of Colored People made a frontal attack on state laws requiring racially segregated schools, and which were later argued before the Supreme Court, Klineberg acted as an expert witness. In the Delaware case, his testimony went unchallenged and served to establish that "racial classification for the purposes of educational segregation was arbitrary and irrelevant since the available scientific evidence indicates that there are no innate racial differences in intelligence or other psychological characteristics" (Clark 1953a). Then in connection with the reargument before the Supreme Court, Klineberg and Robert K. Merton, in consultation with Kenneth B. Clark, planned the early work preparatory to the statements presented to the Court by social scientists in answer to the questions it had posed. In recognition of this and his other contributions to race relations, Howard University gave Klineberg an honorary degree.

Studies in the relations of groups and nations. In his textbook, *Social Psychology* (1940), Klineberg completed the formulation he had begun in the final section of *Race Differences*. He established the ways in which the fundamental psychological features of the individual are influenced by others as well as by his group and culture, using mainly a comparative method and drawing upon a wealth of anthropological evidence on cultural patterning of behavior. His first contact with anthropology had affected him "somewhat like a religious conversion" and had made him muse philosophically: "How could psychologists speak of *human* attributes and *human* behavior when they knew only one kind of human being?" (1974, p. 166). Now he had answered that question on the basis of knowing many kinds of human beings, and had provided what was perhaps the most powerful of justifications for social psychology by showing the force of culture upon even such apparently individualistic, rudimentary processes as the perception of size or color or time.

A different and more obvious agenda for social psychologists is the study of what is manifestly social, the specific ways in which individuals orient themselves and act toward others. Klineberg treats these topics in a long section

on "Social Interaction," using as specific vehicles the examples of racial stereotypes and prejudiced attitudes, and their modification. In this sense, the book is the bridge between his earlier and his later work: The interest in "alleged genetic psychological differences between ethnic groups led directly to a concern with intergroup relations . . . and from this it was an easy step to the study of relations between nations" (1973, p. 43). In some respects, the book is also a bridge between the old and new eras in social psychology. When one notes that some of the most fashionable topics of the 1970s—sex differences, psycho- and sociolinguistics, social behavior among animals—are the topics of chapters Klineberg wrote almost forty years ago, and that emotional expression—indeed a form of body language or communication—is also treated extensively in the text and was the problem he examined in his field work in China more than forty years ago, one can advise the young to cross that bridge and re-read this now ancient text.

During World War II, despite his duties in various United States government posts, Klineberg began his studies in the relations of groups and nations. In "A Science of National Character" (1944b), his address as the retiring president of the SPSSI, he focused on the character of contemporary nations. No matter that the origins are complex and cultural, and not racial, the character of nations as well as the false stereotypes about national character influence the course of their relations. These phenomena require scientific description, and Klineberg tried to build a framework for further studies.

In the quarter of a century since 1950, Klineberg has made many more contributions to the study of group and international relations. Only a few can be mentioned and briefly summarized. *Tensions Affecting International Understanding* (1950), a monograph prepared under the auspices of the Social Science Research Council, deals at length with approaches to the measurement of national character, with racial and national stereotypes and their origin and modification, and with factors making for aggression. This latter theme has been a recurrent concern of Klineberg's through the years (1970; 1972). In "The Role of the Psychologist in International Affairs" (1956), his address when the SPSSI gave him the Kurt Lewin memorial award, he reviewed ways that the action-oriented social psychologist could contribute to the improvement of international relations, and some of the barriers impeding performance of

that role. *The Human Dimension in International Relations* (1964) is a brief, semipopular textbook covering the various sources of tension reviewed in the earlier monograph, and also the contributions psychology can make to the effective operation of technical assistance programs and their evaluation. Two monographs deal specifically with cultural exchange programs. *International Exchanges in Education, Science and Culture: Suggestions for Research* (1966) is a critical survey of the literature, containing an interesting section on the discrimination nonwhites may experience in host countries with a tradition of race prejudice. *Étudiants du tiers-monde en Europe* (Klineberg & Brika 1972) reports research among students exclusively from developing countries attending European universities.

In *Religione e Pregiudizio* (1968), Klineberg and three Italian colleagues report the results of a content analysis of Catholic texts used in Italian and Spanish schools. References to Jews and Protestants in the period brought under study were predominantly negative, a finding having obvious implications for tensions between ethnic groups. In a brief paper, written in 1971, "Black and White in International Perspective," Klineberg returns to his earlier concerns with race relations, but looks at such patterns within other countries as well as within the United States, and treats their implications for international relations. In "The Multi-national Society: Some Research Problems" (1967), Klineberg examines societies that are heterogeneous in other respects than race to explore such sources of conflict and their reduction.

Two research monographs that may be seen as contributions to the improvement of relations between nations also have a special importance for methodologists concerned with cross-national surveys and for theorists concerned with problems of identity, reference group processes, and the developmental processes underlying national stereotypes. In *Nationalism and Tribalism Among African Students* (1969), Klineberg and Marisa Zavalloni explore the relative importance of tribal, national, and pan-African reference groups among students at African universities from six nations: Ethiopia, Ghana, Nigeria, Senegal, Uganda, and Zaire. By comparing the responses of such students resident in France, factors affecting the relative saliency of reference groups are established. In *Children's Views of Foreign Peoples* (1967), Klineberg and Wallace E. Lambert report findings

from a cross-national survey of an urban sample of more than 3,000 children at 3 developmental stages, ages 6, 10, and 14, in 11 nations. Self-conceptions, reference groups, feelings of similarity and difference from people of other countries, and stereotypes and attitudes toward people of other nationalities are examined within the comparative developmental framework of the design. Apart from the substantive importance of the findings both for theory and application, the problems of organizing and operating such a world-wide enterprise, the methodological problems involved in standardizing the open-ended interviewing technique, and the coding of data in so many languages are carefully reviewed and ingeniously handled.

In action as well as in scholarship, Klineberg has sought to foster international cooperation. He helped organize the World Federation for Mental Health, and later served as its president. He served for a long period as an officer of the International Union of Psychological Science. As a researcher and teacher in so many countries, he has personally transmitted a message of good will around the world and sown the seeds of social psychology over thousands of miles. The vocation he had chosen in his early days, to teach psychology, which he has pursued for almost fifty years, is perhaps what he cherishes most. The social psychologists whom he taught or whose researches he guided, including Solomon E. Asch, Robert Chin, Kenneth B. Clark, E. L. Hartley, Herbert H. Hyman, H. N. Schoenfeld, Muzaffer Sherif, and many others, have carried those seeds of a scientific and relevant social psychology to other places.

Klineberg has received many honors: The Butler medal of Columbia University, honorary degrees from Drew University and the University of Brazil, as well as from Howard University and his alma mater, McGill University. Gardner Murphy, in conferring the Kurt Lewin memorial award on Klineberg, described him as having "seen the intricacies, the triumphs, and the tragedies of human relationships as a challenge not only to the possibilities of a greater human brotherhood, but to the possibilities of a rigorous scientific method," and as being honored by "a permanent and illustrious place in the development of social psychology."

HERBERT H. HYMAN

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KOHN, HANS

From the publication of *The Idea of Nationalism* (1944) until his death, Hans Kohn (1891-1971) was the most prolific and influential student of this subject in the United States. The fifty books he authored or edited since 1913, and published in German, French, and Hebrew as well as English, measure his energy and erudition. Kohn's 45 reviews in the *American Historical Review* from 1944 to 1965, his 150 reviews in the *New York Times Review of Books* during the same period, and his dozens of articles in the *Britannica*, including the articles on "Communism," "Fascism," and "Democracy" in the *Britannica* yearbooks from 1942 to 1948, indicate his significance as an opinion molder and publicist (see Wolf 1976, p. 651).

Kohn's later works, especially those in which he discusses Germany, are notable for the dichotomy that he made between a liberal, rational, enlightened "Western" variety of nationalism and an illiberal, nonrational or romantic "Eastern" European variety. His historical thesis (see 1942; 1946; 1949; 1953; 1956; 1960; 1962; Snyder 1954, pp. 117-122) views the nationalist movements in England, France, Switzerland, and the United States as "forward-looking" and progressive because they maintained certain characteristic beliefs of the eighteenth-century European Enlightenment, which based human progress on individual liberty and held that the power of the state ought to be limited to protect this freedom. Thinkers and statesmen in "Eastern" nations, primarily in Germany and Russia during the nineteenth century, reacted against the Enlightenment, glorified state power and the collective unit as the agency of progress. Nationalists such as Heinrich von Treitschke in Germany and the Pan-Slav Fyodor Dostoevsky in Russia were both "statist" and "romantic": they favored a strong state government that could base its claim to greatness on appeals to a supposedly "heroic" past. That reactionary variety of nationalism grew up in countries with a weak middle class; it was narrow and egocentric and unlike its liberal counterpart, offered no "universal message" to mankind.

For all its imprecision, Kohn's dichotomy was popular because it helped explain how men as ideologically diverse as Johann G. von Herder, Otto von Bismarck, and Adolf Hitler could all be accounted German nationalists. It was simply a matter of noting how nationalism had, progressively and detrimentally, separated itself from liberalism. Indeed, Kohn's famous book *The Mind of Germany* (1960) is an excellent analysis of why and how German thinkers moved chronologically and ideologically from Johann Wolfgang von Goethe to Josef Goebbels. The divisiveness that Kohn saw among German intellectuals reflected his own preoccupation with "unity in diversity" throughout his intellectual life. Kohn's interpretation of nationalism cannot be understood without a prior appreciation of the tension between "the one and the many" in Kohn's thought.

Paradoxical as it may seem, nationalism was always associated in Kohn's works with the movement toward world unity. During his early years, Kohn, like Herder, understood nationalism to be primarily a cultural and only secondarily a political force. The title essay in *Nationalismus* (1922) argued that nationalism could be depoliticized in the wake of the World War just as religion had been depoliticized at the end of the Thirty Years War. As his early studies on nationalism in the Middle East make clear, Kohn soon accepted the inevitability of political nationalism. He remained convinced throughout his life, however, that nation-states should *not* be based upon what divided men from each other, such as language, religion, common traditions, or other so-called "objective" criteria of nationality. Rather, nations should be united around moral and political principles that not only make it possible for people to cooperate with each other but leave room also for cooperation with others outside the national unit. The nation-state was only the means to the end of international cooperation and harmony. Kohn was an avid supporter of world federalism in the 1940s, and in the 1960s he spoke of the age of "pan-nationalism" that was "merging into the age of pan-humanism" (1962, p. 166). As men, organized into nation-states, came to understand how much they had in common with each other, they could move into the next stage of "global history," an era of international cooperation in which men might yet "outgrow" nationalism ([1949] 1966, p. 235).

Of course, for nations to become—even potentially—a unifying world historical force, they

would have to be established on the basis of liberal principles. Such principles Kohn saw growing directly out of the "three great revolutions" of 1688, 1776, and 1789—the English, American, and French—each, to one degree or another, influenced by the social, intellectual, and political ideals of the Western European Enlightenment. The liberal beliefs in tolerance, the civil and political rights of individuals, and some form of representative government were founded upon the eighteenth-century assumption that man was fundamentally rational. Only this "faith in reason" could serve as an adequate basis for nationalist movements that would allow people to find meaning for themselves as members of a group without denying members of other, different groups that same right. Only a liberal could allow many different national flowers to bloom.

Kohn's often-used phrase, "unity in diversity," described for him the ideal political and cultural situation *among* nations as well as the best condition *within* a given national state. His concern for unity *with* freedom developed out of the experiences of Kohn's early years; he spent the first 25 in the multinational Austrian Empire. Following five years of grammar school education, Kohn entered the Altstädter Gymnasium in Prague in 1902, graduating in 1910 at the head of his class. Although he spent the years from 1910 to 1914 as a law student at Charles University in Prague, during these years his "chief interests were philosophy, literature and theology." Kohn's study for the bar examination was interrupted by World War I but his education as a "world citizen" was furthered by service in the Austro-Hungarian army, several years spent as a prisoner of war in Russia, and a period of time (after the 1917 revolution) serving with the Czech Legion. It was 1923 before Kohn finally returned to Prague to acquire his J.D. degree.

In 1908, while still a Gymnasium student, Kohn had joined the Zionist movement, and later traveled to Jerusalem. He remained an active member until 1929, when he left Jerusalem to begin a trek westward that ended with his appointment as a history professor at Smith College in 1934. In the decade before he settled permanently in the United States, Kohn traveled extensively in the Middle East, Russia, and Western Europe (Kohn 1964).

The most lasting influence upon his thought, however, was provided by a number of men whom Kohn later loosely described as "cultural" or "ethical" Zionists. One of Kohn's major works during this early period was an intellectual bi-

ography (1930) of Martin Buber, the most eminent of this group and a man whose version of Zionist nationalism and vision of human community strongly influenced Kohn (Wolf 1976, pp. 652ff.). The cultural Zionists, who also included Ahad Ha'am, Asher Ginzberg, Gustav Landauer, and A. D. Gordon, differed from Theodor Herzl and the political Zionists in emphasizing cultural and spiritual renewal within Judaism as a precondition for the establishment of a "national home" in Palestine. Though each approached the question slightly differently, all agreed that cooperation with the Palestinian Arabs was important for ethical as well as economic and political reasons. The Jews had the responsibility to show the world how to live by establishing a "true community" and a binational state in Palestine.

The cultural Zionists served as a "prophetic" force within the larger Zionist movement during the first two decades of the twentieth century. They not only challenged their more "practical" or "priestly" contemporaries, but Buber, at least, tried to synthesize in his writings and addresses during this period a concern for individual spiritual development with the growth of community; he also tried to link ethics and politics, mysticism and rationalism. In a similar fashion, Kohn, whose early essays were never specifically religious, tried to show that the "prophetic ethics" of the Old Testament were really very similar to the moral ideals of the eighteenth-century Enlightenment: he spoke of the "fusion of Jew and European" and compared the "universalism" or "universal messianism" of Isaiah with that of the eighteenth-century French *philosophes* (see 1924; 1931a). The Old Testament prophets from Amos to Jeremiah preached ideals of justice, equality, and community, making it clear that these were universal, not merely national, ideals. By linking this prophetic tradition to the secularized (but also universalist) rational ideals of justice and liberty "preached" by the eighteenth-century reformers, Kohn could synthesize nationalism and liberalism. More importantly, he could maintain his cultural Zionist conviction that nationalism could be a force promoting liberty and world unity.

By the late 1930s, Kohn's transition from a Zionist to a defender of the Western Enlightenment was complete. The dichotomy between "good" and "bad" nationalism that he began to develop by this period first appeared clearly in an essay on "Democracy: The Way of Man" in *World Order in Historical Perspective* (1942).

Kohn here saw American liberalism and fascism as contrasting prototypes. The American political system and values were an outgrowth of the eighteenth-century revolutions, while Nazism was, significantly, "the most audacious counter-revolution ever undertaken, not only against the last three revolutionary centuries, but against the whole development from Socrates and the Hebrew prophets to the present" (1942, p. 57). By drawing upon both English and French ideas, the Americans had devised a political system that rationally combined concern for the individual with necessary attention to the need for security for all members of the community. This was a national community at its best, a form of nationalism that could be copied by others to the benefit of all (see 1957, chapter 1). Nazism, by contrast, was an outgrowth of an anti-intellectual tradition that specifically despised Enlightenment nationalism and universalism. Fascism, Kohn wrote, generally "rejects all absolute standards of ethics and law and . . . the oneness of the human spirit. . . . Fascism believes in the immutable status of man . . . it denies the perfectibility of man" (1942, p. 37).

Hans Kohn remained committed to "liberal nationalism" and the dichotomy (of good and bad nationalism) in all his post-World War II works, despite charges from critics that he oversimplified a complicated subject. By the end of World War II, no other major student of nationalism was as willing to defend the indiscriminate combining of these two terms. The approach of the other major American historian of the subject, Carlton Hayes, for example, was quite different (Hayes 1931).

Kohn could not and would not admit that "liberal nationalism" was an oxymoron because, throughout most of his life and certainly for his last 35 years, it was not primarily nationalism that he was defending, but liberalism. This was clear at least as early as his first major American work; in the concluding pages of *The Idea of Nationalism*, he wrote: "In the word 'liberty' vibrates the message which pervades all human history and makes it human: the promise of the dignity of man, of his rights as an individual, of his duties to his fellow man. . . . Compared with it, nationalism is only a passing form of integration, beneficial and vitalizing yet by its own exaggeration and dynamism easily destructive of human liberty" (1944, pp. 575–576).

It was this "integration," call it world unity or "pan-humanism" or world community, that re-

mained the goal of Kohn's life and this goal gave unity to his many diverse works. "The individual liberty of man has to be organized today on a supra-national basis. Democracy and industrialism, the two forces which rose simultaneously with nationalism and spread with it over the world, have both today outgrown the national connection," he argued in the introduction to *The Idea of Nationalism* (1944, pp. 22–23). Thus, in both the opening and closing pages of Kohn's *magnum opus*, the work that, more than any other, made his reputation as a scholar, he proclaimed that a dispassionate study of nationalism was not his only aim. He made no secret of the fact that he wished to see (and would do what he could to encourage) a world where both individual freedom and human community were protected, a world that promoted both the freedom of the one and the security of the many, a world that manifested "unity in diversity," a world that could accommodate, by that name or by some other, "liberal nationalism."

While Hans Kohn's work was less a detailed study of nationalism than it was a protest against the way the term "nationalist" was used by many students of the subject, especially Americans, his appeal to many Americans in the post-World War II generation may reveal that he was saying what his cold war audience wished to hear, about themselves and about the Eastern "others" who were so unlike them. Kohn, like many of the other cultural Zionists of the early years of this century, was a utopian reformer. He found his audience, not among the Jews or Europeans, but, finally, among the Americans, who for him had come to typify the "liberal West."

KEN WOLF

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KRECH, DAVID

David Krech was born Isadore Krechevsky in 1909. In 1913 his family moved from Russia to New England where he received his early education: an education that led to the development of a set of strong moral, social, and political values that influenced much of his later personal and professional life. He enrolled at New York University, where he became interested in psychology, partly through the influence of T. C. Schneirla. Near the end of his undergraduate studies he read Karl S. Lashley's just published *Brain Mechanisms and Intelligence* (1929), and this influenced him to conduct a series of experiments that demonstrated the existence of "hypotheses" in rats. Following the completion of his M.A. (1931) at New York University, he entered the University of California at Berkeley in 1931 and studied with Edward C. Tolman, who was just completing the final draft of his *Purposive Behavior in Animals and Men* (1932). At this same time Robert Tryon was performing his classic experiments on rats selectively bred to run mazes. Tolman's insistence that behavior could be understood best as dynamic and purposive (even when applied to the laboratory rat), and Tryon's concern for the importance of individual differences, influenced Krech's subsequent career.

Following the completion of his Ph.D. in 1933, Krech spent four years with Lashley at the University of Chicago, in the company of such people as Robert Leeper, Donald O. Hebb, and Frank A. Beach. As a result of his growing political awareness, he was one of the organizers of the Society for the Psychological Study of Social Issues. He then taught briefly at Swarthmore College, where he came under the influence of Wolfgang Kohler.

In 1942 he was drafted into the U.S. Army and assigned to the Office of Strategic Services' assessment station under the command of Henry A. Murray. During this period he married Hilda Sidney Gruenberg and changed his name to David Krech. He returned to Swarthmore after the war as a social psychologist and, with Richard Crutchfield, wrote *Theory and Problems of Social Psychology* (1948). In 1947 he joined the faculty at Berkeley, where he remained through his early retirement in 1972 and until his death in 1977. In 1970 he received one of the highest honors awarded by the American Psychological Association, the distinguished scientific contribution award.

Krech's professional contributions were multifaceted. He began with a search for the principles of learning in rats. This search gave way to an intensive period during which he was concerned with social psychology, political activity, and the application of gestalt psychological principles to the understanding of behavior. Finally, he focused on biological questions and developed a research program based on the relationships between brain chemistry and behavior. This latter pursuit was coupled with a concern for the ethical and moral implications of "mind control" through chemical intervention.

Four major threads run through all three phases of Krech's development. First, he had a strong concern for individual differences. Rather than characterize behavior on the basis of group trends he analyzed in detail the performance of each animal as it negotiated the "hypothesis box"—an unsolvable maze. In this way he was able to detect and characterize the strategies used by each animal in its attempts to solve the maze. He moved from this level to the formulation of an over-all schema that related behavior styles to genetic factors and to alterations in the nervous system. In his later work on brain chemistry, he again adopted this approach in order to relate brain chemistry to rat hypotheses.

Second, he insisted on the primacy of broad-ranging theoretical concerns. He realized that data are only as good as the schema by which they can be organized. He scorned those scientists who engaged primarily in amassing experimental facts—"pebble-pickers" as he called them. In all of his writing, research, and teaching, he emphasized the bearing of particular data on larger conceptions of behavioral significance.

Third, Krech strongly believed that molar behavioral concerns could be understood by molecular reduction. He thought that the smaller the unit of analysis, the greater the generality of the findings. This conviction led to his early studies of the behavior of selectively bred rat strains, of the effects of brain lesions on the nature of hypotheses, and of the effects of experience on the structural and chemical constitution of different parts of the brain.

Finally, he maintained an overriding commitment to the essential views of gestalt psychology. He accepted the gestalt emphasis on the organized unity of behavior and resisted attempts to fractionate this unity into static, molecular pieces. Even his physiological studies of the brain and behavior had a molar emphasis

that respected the inherent complexity of behavior.

Krech's contributions to psychology can be divided into four areas. The first is his concern for general behavior theory. He rejected the traditional behavioristic notions of John B. Watson, E. L. Thorndike, and Clark L. Hull that an organism approached a problem with a formless, random array of responses that were forged into a successful set of habits by reward and punishment on the anvil of trial and error. Krech demonstrated that rats attempted systematic solutions to problems, rejecting one after another until the correct response was selected. The organism's past experience and biological heritage affected the nature of the attempted solutions. This view of learning as a noncontinuous process provoked a great deal of controversy (especially with Kenneth W. Spence of the University of Iowa) and stimulated valuable research.

Krech also made major contributions to the field of social psychology, including the textbook he coauthored with Crutchfield (1948) and his political activities. Stressing phenomenology and cognitive organization, the theoretical orientation of the text helped to unify the subject matter and theory of what was then a fragmented field. In addition to providing a scientific framework for social psychology, the authors argued that a theoretically sound social psychology is also practically valid and immediately useful. Toward this end they presented intensive treatments of measurement techniques, public opinion survey methods, sampling problems, and action programs designed to minimize industrial conflict, racial prejudice, and international tensions.

His political involvement ranged from early membership in socialist organizations to a leadership role in the organization of the Society for the Psychological Study of Social Issues; now a formal division of the American Psychological Association. He also testified against the separate but equal doctrine in a 1951 suit brought by the National Association for the Advancement of Colored People on behalf of parents of black children in Clarendon County, South Carolina. This was the first time a federal court allowed social psychologists to testify as expert witnesses.

The third topical area, to which Krech devoted much of the last two decades of his life, was the relation of brain chemistry to behavior. This work began with the encouragement of the

biochemist Melvin Calvin. A research team consisting of Krech, the biochemist E. L. Bennett, and the physiological psychologist M. R. Rosenzweig began to study the effects of experience on the anatomy and chemistry of the rat brain. Later, they were joined by the neuroanatomist Marian Diamond and produced an extensive series of empirical and theoretical papers—some of which were greeted by a critical storm. The research indicated that the environmental conditions under which rats are housed produce differences in brain cholinesterases, weight of brain sections, and thickness of cerebral cortex. They demonstrated that the brain grows with use and that brain nerve cells can increase or decrease in size depending on whether or not the environment is stimulating. The provocative nature of these findings and their potential value in promoting an understanding of the substrates of behavior resulted in a flurry of research activity throughout the world. Within a few years of the inception of the project a large number of laboratories were actively inquiring into the relations between brain chemistry and behavior.

Above all, however, Krech was an educator. He had an extraordinary ability to teach. He was a brilliant, exciting, and exacting leader of a graduate seminar, a challenging teacher of advanced psychology classes, and an inspired lecturer in introductory psychology. His commitment to education is attested to by three major textbooks: the previously mentioned *Theory and Problems of Social Psychology*; an introductory psychology text, *Elements of Psychology* (Krech & Crutchfield 1958); and an introductory statistics package, *Statlab* (Hodges, Krech, & Crutchfield 1975). He was concerned with the quality of education and the quality of academic life at the university and took an active role to help the university steer a reasoned course through the social turmoil that embroiled the Berkeley campus in the 1960s.

LEWIS PETRINOVICH

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KUZNETS, SIMON

An economist, statistician, and economic historian, Simon Kuznets' foremost contribution, for which he won the 1971 Nobel prize in economic science is the comparative study of the economic growth of nations. Kuznets' work was the fruit of a carefully designed and imaginatively conceived research program involving scholars throughout the world and extending over the three decades since World War II. The program centered on the quantitative measurement of the rate of economic growth and of associated shifts in internal conditions and external relations for as many nations as possible. It encompassed also exploratory studies of underlying causes. The culmination of these efforts appear in volumes written by Kuznets in the 1960s and 1970s. Here one may find most of what is known today about rates of economic growth; how they vary among nations and how they differ from premodern growth rates; the systematic changes in internal economic, social, and political structure that have accompanied modern economic growth; and the international economic and political ramifications of the spread of modern economic growth (for a brief summary, see Easterlin 1968).

This work is a logical outcome of a long record of distinguished scholarship. Kuznets'

first contributions in the late 1920s and early 1930s were in the analysis of economic time series movements—seasonal fluctuations, cyclical changes, and secular movements. From this he moved on to the measurement of the national income and product of the United States. This research is a landmark in the evolution of economic science. Today, figures of gross national product (GNP) are taken for granted, but before World War I there was almost total ignorance of such elementary data on the economy's size and structure. Kuznets was not the first to seek to close this gap, but his work on national income and product was so distinctive that it became the benchmark in the field. It encompassed estimates of total output and income by final product, industry of origin, and type of income; of capital formation and savings; and of the distribution of income between rich and poor. This work, coinciding with the new demands for economic information generated first by the depression of the 1930s, and then, by the mobilization requirements of World War II, laid the foundation for the establishment of official estimates of total GNP and its components by the federal government, a task in which Kuznets played a leading role. It provided, too, the basis for Kuznets' subsequent program of research on economic growth, which was built upon historical series of national income and product for as many countries as possible.

Kuznets' work on national income played a crucial role in the transition of economics from a deductive to a quantitative science. This transition required a union of theory, economic measurement, and statistical methodology. In the 1930s the new macroeconomic theories of John Maynard Keynes had aroused much interest because of their relevance to the worldwide economic crisis. Kuznets' concurrent and independent effort to develop measures of the consumption, savings, and investment components of national income provided the empirical counterparts of the Keynesian concepts. This advance in economic measurement and its concordance with new theoretical formulations was a key step in the development of econometrics, the systematic quantitative modeling of the economic system pioneered by Ragnar Frisch and Jan Tinbergen.

In his early work on secular movements, Kuznets identified fluctuations of 15 to 25 years' duration in a number of economic time series in the United States. Subsequently he returned

to this subject several times, widening the range of observation to other developed countries and incorporating demographic as well as economic time series. These movements, although still somewhat controversial, are today commonly referred to as "Kuznets cycles" in recognition of his pioneering contribution.

Kuznets was born in Kharkov, Russia, in 1901 and emigrated to the United States in 1922, where he took up studies at Columbia University, receiving his B.A. in 1923, his M.A. in 1924, and his Ph.D. in economics in 1926. In 1927 he became a member of the research staff of the National Bureau of Economic Research (NBER) where his work on national income was conducted. Although he remained an active member of the NBER staff until 1960, the primary base for his research effort from the 1950s was the committee on economic growth of the Social Science Research Council (SSRC). Kuznets rose to the position of professor of economics and statistics at the University of Pennsylvania during his tenure there from 1930 to 1954, and subsequently held professorial appointments at The Johns Hopkins University from 1954 to 1960, and Harvard University from 1960 until his retirement in 1971. His primary teaching areas followed the development of his interests: economic statistics (especially time series analysis), business cycles, economic development, and economic-demographic interrelations. His influence as a teacher extended beyond formal course work and the many dissertations that he supervised. Kuznets was teaching by example in the research projects that he directed as well as through the many professional conferences in which he participated throughout his career, particularly the NBER Conference on Research in Income and Wealth and its offshoot, the International Association for Research in Income and Wealth, both of which he helped found; and the many conferences organized under the auspices of the SSRC committee on economic growth.

Kuznets served two periods in government. Before his emigration to the United States, he was the youthful head of a statistical office in the Ukraine during the early chaotic post-Czarist years. During World War II he was associate director from 1942 to 1944 of the Bureau of Planning and Statistics of the War Production Board. Kuznets served as president of the American Economic Association (1954) and the American Statistical Association (1949). Since

retiring in 1971, his research efforts and publications have shifted increasingly to the field of population.

Kuznets' convictions about the importance of economic and quantitative measurement as the basis for understanding social problems antedated his emigration to the United States, but they received strong reinforcement from his mentor at Columbia, Wesley C. Mitchell. Mitchell's skepticism about the reliance of economics on deductive economic theory and his belief in the need for quantitative facts had been instrumental in the establishment in 1920 of the NBER, which was a nonprofit research organization devoted to the study of basic economic science and the first of its kind in America. Mitchell brought Kuznets into the NBER, where before long he came to head the bureau's program of national income studies. This project, along with the study on business cycles, headed by Mitchell and Arthur F. Burns, were the central pillars of the bureau's work, and the basis for the national and international reputation that the bureau established in the 1930s and 1940s. A crucial opportunity for maintaining the NBER's leadership was lost after World War II when the bureau rejected Kuznets' proposal to undertake the comparative study of economic growth of nations. As a result, Kuznets' primary research commitment was eventually shifted to the SSRC committee on economic growth.

In the development of American economics, the NBER is an offshoot of institutional economics. The leading features of Kuznets' approach to economic research are best appreciated in terms of this intellectual heritage. Two themes of the NBER's work—expressed in titles of the bureau's annual reports, "A Respect for Facts" (1960) and "The Cumulation of Economic Knowledge" (1948)—form key ingredients in Kuznets' research style. In Kuznets' work, as more generally in that of the NBER, economic science starts with measurement based on careful definition and classification. The underlying philosophy, in Kuznets' words, is that "statistical data are susceptible of cumulation to the highest degree. As statisticians so well know, a series that is twice as long possesses more than twice the analytical value—provided continuity and comparability are preserved. It is this advantage of statistical measurement and research that assures its fruitfulness . . ." (1947, p. 34).

The quintessential criticism of the NBER ap-

proach was captured in the title of T. C. Koopman's famed "Measurement Without Theory" review (1947) of the Burns–Mitchell treatise on business cycles. In Kuznets' view, however, measurement can never be divorced from economic theory and is necessarily guided by theory: "A major task is involved in the attempt to pass from the scattered incomplete, primary data, affected by all the peculiarities of both the country's economy and its data gathering institutions, to measures that reflect as clearly as possible the rigorously defined concepts of economic analysis" (1957, p. 553). Indeed, in the late 1940s Kuznets broke with the official estimators of GNP because he held that their increasing emphasis on "social accounting" was an abandonment of the fundamental Marshallian and Pigovian theoretical concept of national income as a measure of economic welfare. Throughout his career, Kuznets' monographs, although structured around tables of data, have been infused with "tentative" interpretations and explanations based on economic analysis.

Admittedly, Kuznets is reserved in his use of economic theory and skeptical of formal mathematical and econometric models. This stems, however, not from a rejection of theory, but from the notion of the historical relativity of economics. He applied this feature of his approach both to measurement and theory. To Kuznets, "much economic writing and theorizing . . . [is] geared to the current conditions and oversimplified to the point of yielding a determinate answer. . . . Such theories . . . tend to claim validity far beyond the limits that would be revealed by an empirical test" (1955, p. 76). Even "quantitative measures may lose part of their value because the object they measure may seem, in the light of objective changes and changes in theory, less strategic than it seemed before" (1947, p. 33). Kuznets' view of the historical relativity of economics partly explains why, aside from his periods of wartime service, he remained aloof from work on public policy, though recognizing its necessity.

Kuznets' reservations about economics stem too from what he feels is its limited coverage of social reality: "[E]ither [economics] must admit that none of its results have validity until they are supplemented by findings of other disciplines on the processes impounded . . . in *ceteris paribus*; or it must state where, in real life, *ceteris paribus* begins and ends . . ." (1955, p. 73). Particularly in the study of economic

growth is an expansion of disciplinary boundaries necessary: "Much as one may regret leaving the shelters of the accustomed discipline, it does seem as if an economic theory of economic growth is an impossibility, if by 'economic' we mean staying within the limits set by the tools of the economic discipline proper . . ." (*ibid.*, p. 73).

Writing of Joseph A. Schumpeter, Kuznets once observed that "strong minds are guided by their own interests," a statement that applies equally to him. In a discipline where deductive analysis is the hallmark of accomplishment, Kuznets, though himself a creative and original thinker—witness his sweeping program for the study of economic growth—is notable for his insistence on facts and measurement. In a field that prides itself as "queen of the social sciences," Kuznets has reached out to other disciplines both in teaching and in research. And in a subject in which sweeping ideological prescriptions for reform abound, Kuznets has been, both in words and in example, a passionate believer in the ultimate value of science, as evidenced in his 1949 presidential address to the American Statistical Association:

Under the shock of the catastrophic events of recent decades, belief in the possibility and usefulness of scientific study of human society has grown perceptibly weaker. There are, and will be, many to doubt that the search for objectively found patterns of stability and change is likely to yield significant knowledge in the field and who will turn to other ways in which men may reconcile themselves to the apparently capricious turbulence of human history. We can easily understand and sympathize with such doubts. But it is difficult for me to see how there can be any guide for scientific work and development other than a belief in the existence of some order in the seemingly chaotic jumble of history; in the demonstrability of such order in empirical terms; and in the ultimate social usefulness of the resulting body of tested theory. These three basic beliefs warrant examination of conditions of statistical research . . . in the spirit of setting a task for the future rather than of apology for the past and for failure to go on; in terms of hope rather than of despair.

RICHARD A. EASTERLIN

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LAKATOS, IMRE

At the time of his death Imre Lakatos (1922–1974), who had been professor of logic at the London School of Economics since 1969, was a foremost philosopher of mathematics and of science. He had a vivid personality and had led a turbulent life.

He was born in Hungary of Jewish descent; the family name was Lipsitz. His father was a wine merchant in Budapest, but as a result of anti-Semitism, his mother took him to live with her mother in Debrecen, where he entered the Gymnasium. He attended the University of Debrecen and graduated in 1944. During the Nazi occupation of Hungary he changed his Jewish name to Molnar (= Miller) and joined the underground resistance. After the war, he changed his name again—this time to Lakatos.

At the end of the war, he became a research student under György Lukács at Budapest University. A convinced communist, he was also very active in party politics. In 1947 he was made a secretary in the Ministry of Education and put in virtual charge of the democratic reform of higher education in Hungary. He also wrote a doctoral thesis on concept formation in science and received his degree in 1948.

In 1949 he spent a year at Moscow University. In June of that year the terror campaign under the Stalinist Rakosi against Rajk and the “Hungarian-Tito-ists” began. Soon after his return to Hungary in the spring of 1950, Lakatos was arrested and imprisoned for nearly four years. He later attributed his survival to two things. First, his communist faith never wavered (though

this was also true of many who did perish); second, though willing to confess any moral view which his interrogators might require of him, he absolutely refused to fabricate evidence for them.

When he was released in late 1953, he found that as an ex-political prisoner he was virtually unemployable. However, in 1954 the mathematician Alfréd Rényi obtained a position for him in the Mathematical Research Institute of the Hungarian Academy of Science in which he could use his linguistic skills; he spoke German and Russian and read English and French. His job was to translate important mathematical works into Hungarian. For the first time since his early days at university Lakatos was in a state of undistracted academic seclusion. His concentrated study during the ensuing two years provided a basis for the highly original work he did later in the philosophy of mathematics. One of the works he translated, György Polya’s great book on mathematical heuristic, *How to Solve It* (1945), had a great impact on him. In his preface Polya had said: “Mathematics presented in the Euclidean way appears as a systematic, deductive science; but mathematics in the making appears as an experimental, inductive science” (1945 [1957], p. vii). This contrast between a formalized mathematical system and its informal prehistory was to become a *motif* of Lakatos’ “quasi-empirical” philosophy of mathematics. His period in the institute had another important result. Hitherto he had regarded Marxism as the only scientific theory of society; but he now had access to a library containing Western books and journals

not generally available in Hungary; and he discovered works that shook that idea. In particular, he found in the works of Karl Popper both a clear understanding of the nature of scientific theory and a devastating criticism of Marxist historicism as hopelessly unscientific. His communist certainties, having withstood four years of communist imprisonment, began to crumble.

In November 1956, after the Soviet suppression of the Hungarian uprising, Lakatos, along with many other Hungarian intellectuals, fled the country. First he went to Vienna; then he was awarded a three-year Rockefeller fellowship and went to King's College, Cambridge, to research under R. B. Braithwaite. In 1958 he met Polya, who advised him to prepare a case study of the history of the Descartes–Euler conjecture that for all polyhedra $V - E + F = 2$, where V , E , and F are the number of vertices, edges, and faces respectively. His PH.D thesis was devoted largely to this problem. He presented part of it at Popper's seminar at the London School of Economics in 1959 and joined Popper's department in 1960. His *Proofs and Refutations* (1976a) presents a view of mathematics very different from the "Euclidean" view in which theorems are infallibly proved by deduction from axioms that are certainly true, the whole deductive system containing only terms that are either perfectly clear and undefined or else are exactly defined by such undefined terms. In Lakatos' rational reconstruction, presented as a fast-moving, incisive, and occasionally hilarious classroom dialogue of the history of the Descartes–Euler conjecture, refutations play a decisive role. Moreover, the refutations, for the most part, are not discovered by accident but *as a result of the "proof."* Very briefly, the pattern is this. A "naive" conjecture C is proposed, and a proof is sought for it. The proof consists of a thought experiment in which C is decomposed into a number of lemmas that jointly entail C . This process prompts and guides the search for counterexamples. Some counterexamples are only "local": they hit one of the lemmas without hitting C itself; others are "global": they are cases in which C itself, as well as at least one of the lemmas, is refuted. (The "guilty" lemma may have been a hidden one, brought into the open as a result of the refutation.) Various responses are made to the counterexamples produced. One response, "monster-barring," is to reject the alleged counterexample as not being a genuine instance of C 's antecedent clause. Thus if C says, "All poly-

hedra satisfy $V - E + F = 2$," and it is found that for a cube with a hollow cube inside it we have $V - E + F = 4$, the latter may be rejected as not being a genuine polyhedron ("A woman with a child in her womb is not a counterexample to the thesis that human beings have one head"). A more sophisticated version of this response, "monster-adjustment," is the claim that the apparent monster, when properly seen with mathematically trained eyes, is no monster after all. Thus the hollowed cube is really two cubes, one inside the other; and for *two* polyhedra C of course gives a value of 4 instead of 2. Another response, "exception-barring," is to concede the genuineness of the counterexamples and try to restrict C to a safe domain with all exceptions outside it. One may try to achieve this in one step by looking for a set of characteristics shared by all instances and no counterinstances of C , and then reformulate C so that it applies just to the thus characterized polyhedra. Or one may proceed step by step by a process of "lemma-incorporation." Suppose that a global counterexample to C has been found and that the guilty lemma (perhaps a hidden one) has been identified. This lemma will say that all polyhedra have a certain property P (for instance, that if made of stretchable rubber they can be pumped into a ball). One now incorporates P into a modified version of C which now says that all P -type polyhedra satisfy $V - E + F = 2$.

All these responses reduce the content of the original conjecture; yet there seems to be no guarantee either (a) that there are not still counterexamples within the shrunken domain, or (b) that there are no examples outside it. Indeed, the further one goes in trying to achieve (a), the greater the likelihood that one fails to achieve (b). So there is now a move in the opposite direction, an attempt to replace the original naive conjecture C with a deeper, more comprehensive, and more sophisticated conjecture that, instead of giving a fixed value to $V - E + F$, predicts its values for all varieties of polyhedra according to certain parameters, say n and e , where n and e stand for the number of certain abstract properties that any given polyhedron has. If this can be done, the domain of the original C may be reconquered, perhaps even extended.

In this dialectical development, the process of "proving" ideas is really a process of *improving* them. The concepts of "polyhedron," etc., are continually stretched and modified. Lakatos complained that when the end product of all

this mathematical endeavor appears in a mathematics textbook as a formally proven theorem with its exciting informal prehistory suppressed, it will be practically unintelligible; the proof will be preceded by laborious definitions (he mentioned a 1962 definition of "ordinary polyhedron" which fills 45 lines!) which cannot be grasped intuitively and the historical rationale for which is not given, but which trivially guarantee the validity of the theorem.

The Descartes–Euler conjecture was an excellent vehicle for Lakatos' philosophical ideas because its subject matter is relatively concrete (mineralogy suggested some of the counterexamples) and easily visualizable, and the non-mathematician can follow the story through most of its stages. Critics, however, have suggested that this case study was not a representative piece of mathematics, and that Lakatos placed too general an interpretation upon it. But he did not confine himself to this one case. He exhibited a similar pattern in the history of an abstract mathematical idea, namely that the limit of any convergent series of continuous functions is itself continuous, an idea that was proved by Augustin L. Cauchy and refuted by Charles Fourier. In his "A Renaissance of Empiricism in the Recent Philosophy of Mathematics?" (1976*b*) he claimed, with supporting quotations from Bertrand Russell, A. A. Fraenkel, Rudolf Carnap, Haskell B. Curry, Willard Quine, J. B. Rosser, Alonzo Church, Kurt Gödel, Hermann Weyl, John von Neumann, Paul Bernays, Andrzej Mostowski, L. Kalmar, and others, that David Hilbert's program of establishing "once and for all" the certitude of mathematics has broken down irretrievably, and that mathematics as a whole must now be regarded as quasi-empirical: its axioms are hypotheses whose truth, and even consistency, are not guaranteed. Truth-values are injected not at the top but at the bottom of the system, as in the empirical sciences.

Having extended Popperian conjecturalism from empirical science to a field to which Popper himself had not ventured to extend it, Lakatos began to shift his philosophical interests back from mathematics to empirical science. In 1965 he organized a remarkable colloquium at Bedford College, London. As well as including many of the world's leading mathematical logicians, it brought together in an amicable confrontation, for the first time since the far-off days of the Vienna Circle, the inductivist and anti-inductivist methodologies of Carnap and

Popper. Thomas Kuhn was also there, a third pole in the discussion. The proceedings of this colloquium, published in four volumes edited by Lakatos in cooperation with Alan Musgrave (1967–1970), were valuable for, among other things, their critical exchanges of opposing viewpoints. The last, *Criticism and the Growth of Knowledge*, became a best seller. It included a long essay by Lakatos in which he developed his "methodology of scientific research programmes." Although still in the Popperian tradition of critical rationalism and fiercely opposed to Kuhn's psychosocial approach to science, it involved a move towards Kuhn's governing idea of a scientific *paradigm*: "my concept of a 'research programme' may be viewed as an objective, 'third world' reconstruction of Kuhn's socio-psychological concept of 'paradigm'" (p. 179, note 1). In Lakatos' methodology the basic unit and object of appraisal in science is no longer a scientific theory, as it stands at a given time, but rather a research program that results in a sequence of theories over a period of time. What gives a research program its identity is its "hard core," a set of tenaciously retained fundamental ideas, and its "heuristic," which advises how the "protective belt" of subsidiary assumptions round the hard core should be progressively modified in response to recalcitrant evidence. A research program is progressive so long as such modifications are not *ad hoc* but content increasing, resulting in predictions of novel facts at least some of which are verified (falsifications being put on the agenda to be dealt with later). The program is degenerating if the modifications are content decreasing or yield no successful novel predictions. Where Popper had seen the basic choice as between rival theories, perhaps decidable by crucial experiments, Lakatos saw it as between rival research programs; and no quick decision between them is possible, since a degenerating program may stage a comeback; a "crucial experiment" is not crucial at the time; at most it confronts one of the programs with one more anomaly. However, if it constitutes no anomaly for the other program, and if this one continues to progress while the other degenerates, then that experiment may subsequently, "with long hindsight," come to be regarded as crucial.

This methodology leads to a new, more exacting, and more revealing historiography of science. It requires that many new questions be answered. For instance, it is no longer enough for a historian to ascertain that a certain pre-

diction, made on the basis of a theory, was verified on a given date. He must investigate whether the prediction was made only after certain modifications in the theory, whether these modifications had been in some way *ad hoc*, whether the fact predicted was novel or already known, whether it was or could have been predicted by the rival program, and so on. Lakatos, who relished case studies, inaugurated this new historiography. His example was infectious and he inspired many of his students and colleagues, including Peter Clark, John Worrall, Alan Musgrave, and Elie Zahar, to develop it further. Both as a supervisor and as editor of the *British Journal for the Philosophy of Science*, he devoted hours of patient criticism to younger people's work.

Although he disparaged much of what was being done in sociology and social psychology, he regarded some developments in the social sciences, especially economics, as promising; and he claimed that his methodology would not have the destructive effect of Popper's less tolerant methodology on budding research programs in this field. His methodological ideas have been applied to the IQ (intelligence quotient) debate by Peter Urbach (1974) and to theoretical developments in economics by Spiro Latsis (1976) and Axel Leijonhufvud.

A lecture by Lakatos was always an occasion: the room crowded, the atmosphere electric, and from time to time a gale of laughter. From 1964 on he often lectured in the United States. He had friends all over the world with whom he corresponded indefatigably. He also had enemies; for his sharp wit could be biting and he was sometimes a ruthless campaigner. A rift opened between him and Karl Popper (see the exchange between them in Schilpp 1974). The political issues that most exercised Lakatos were either geopolitical (especially the vulnerability of the Western world to the forces threatening it) or academic (especially threats to academic autonomy, whether from governments or from student militants). During his life in England his political views veered from left of center towards the right; but he always retained a democratic informality in his personal relations.

JOHN WATKINS

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LANGER, WILLIAM L.

William L. Langer (1896–1977) was a leader in a generation of scholars who established European international history as a professional discipline in the United States. In a series of classic works, he brought to the highest level of systematic illumination the traditional narrative realism of diplomatic historiography. At the same time, his sense of the limitations of diplomatic history as an autonomous field led him to break open new areas and methods of historical inquiry.

The second of three sons of a struggling German widow, Langer grew up in South Boston under conditions of poverty and ascetic discipline. A discerning grammar school teacher sent him to the Boston Latin School, from which he entered Harvard University. Though his real interests had become history and the classics, the German-speaking boy, compelled by iron economic necessity to be practical, concentrated in modern languages with an eye to schoolteaching as a career. The rigors of his experience as child and youth gave Langer a vivid sense of reality: delight in its variety, respect for its

power, alertness to its unexpected transformations, and strong empathy for men trying to cope with it.

Enlistment in the U.S. Army in World War I liberated Langer from a confining career as a preparatory school teacher. At the same time, the great issues of war and peace turned him definitively to history as the center of his intellectual life.

Even before he entered graduate school at Harvard in 1919, Langer had published his first work of history, that of his army company (1919). In reintroducing this youthful work, reissued in 1965, Langer recreated with charming self-irony the naive cultural climate in which, as actor and as author, he had produced it. The defeat of Wilsonian hopes in the early 1920s, however, darkened Langer's initiation to the study of modern European history. His two principal teachers, Archibald Coolidge and Robert Lord, gave Langer more than a solid training in the scholar's craft. Having actively participated as historical experts in Wilson's "Inquiry," which prepared the United States for its role in peacemaking, they showed Langer by their example that a historian could lead a life both "in and out of the ivory tower," as he was later to say of himself. As editor of *Foreign Affairs*, Coolidge enlisted his young disciple as bibliographer, beginning his lifelong association with the Council on Foreign Relations, a forum where the business, academic, and governmental elites met to explore international issues. Scholarship, government service, and participation in shaping political opinion thus combined early as the constituents of Langer's vocation as historian.

The burning issues of the 1920s—the Treaty of Versailles, the League of Nations, and responsibility for the European war—attracted to diplomatic history many of the most vital young minds in the profession. Where most students of European diplomatic relations before 1914 became involved in the more immediate origins of the war and in the war guilt question, Langer raised more fundamental questions, those concerning the structure of international politics. In the introduction to his pathbreaking *European Alliances and Alignments* (1931), he wrote: "I have written this book not with the outbreak of the World War especially in mind, but as a study in the evolution of the European states system." This emphasis had two consequences for the character of his work. First, by eliminating diplomatic events not germane to

the changing configurations and power groupings, Langer achieved both empirical compression and structural clarity. Thus he could effectively employ diagrams of power alignments to conceptualize the yield of his narrative. Second, his approach allowed him to identify readily with the statesmen operating within the system, regardless of what states they served.

In the conviction that "a just appreciation of the past can be gained only if the events of the past are viewed with the eyes of the actors," he analyzed the international system essentially as the product of men playing a game whose rules were known, but whose stakes became fully evident only in the course of events. Within Langer's cool analytic concentration on the power system, the diplomatic actors could be shown in all their concreteness as they unconsciously prepared the destruction of the international order and their own security. Where the more moralistic historians of his generation asked of their recent history, "Who played foul?," Langer pursued a deeper question: "What happened to this game, that it should destroy its players?"

Langer devoted three major works to the European international system. In the second of these, *European Alliances and Alignments*, is found the touchstone of his interpretation: the international system at its Bismarckian apogee (1871–1890). By an exemplary playing off of international tensions, Bismarck, once his German aims were accomplished, is shown by Langer to have established a multilateral equilibrium, absorbing new pressures into old order. Bismarck emerges not only as a diplomatic virtuoso, but as a statesman who knew how to actualize the positive potentialities of a politics of national interest for general European stability.

The slow disintegration of this system occupied Langer's attention in two other works: *The Franco-Russian Alliance* (1929) and *The Diplomacy of Imperialism* (1935). The first of these analyzed the removal of the Russian keystone of Bismarck's edifice of peace. The second, *The Diplomacy of Imperialism*, Langer conceived as a direct sequel to *European Alliances and Alignments*. He pursued the dynamics of the breakdown of the European system into the wider world. Here Langer dissolved the simple picture of the incipient polarization of Europe into two camps, current among historians whose vision was fixed on the alignments of World War I, into a series of eccentric criss-

crosses of imperialist power partnerships as the measured *allemande* of the Bismarckian era turned into a *danse macabre*.

Even as he deftly articulated the new complexities of the international system, bringing Rankean realism to a kind of Ptolemaic perfection, Langer developed doubts about the efficacy of diplomatic history as an autonomous field, and a sense of uneasiness about his own achievement in it. How could one account for the extraordinary aggressivity of the European great powers, at once so predatory and so self-destructive, as they fanned out into the hapless world of weaker societies? Within *The Diplomacy of Imperialism*, Langer devoted a chapter to the internal social and cultural dynamics of imperialism, using British society as a testing ground. It was the first but not the last time that Langer, speaking with all the authority of a master-consolidator of a historiographical tradition, pointed beyond his own work to new questions and new approaches.

The ink on *The Diplomacy of Imperialism* was scarcely dry when its chronicler found himself confronting new outbreaks of the disease in Italy, Germany, and Japan. For almost two decades thereafter Langer devoted much of his life and writing to the contemporary international crisis, bringing his historical perspective to questions of American international policy, first in the public arena, later as public official. As chief of the Research and Analysis Branch of the Office of Strategic Services (1941–1946) Langer effectively organized scholars of the most diverse intellectual skills and social outlooks for political and economic intelligence work.

His wartime service reactivated Langer's interest in diplomatic history, this time with a contemporary focus on American foreign policy. At the initiative of the Department of State, he produced *Our Vichy Gamble* (1947), a work much attacked both because it justified a controversial American policy toward France and because of the questions it raised concerning privileged access to documents not open to the historical profession at large. At the request of the Council on Foreign Relations, Langer wrote, in collaboration with S. Everett Gleason, two basic works on the shaping of American foreign policy in the final pre-war years: *The Challenge to Isolation, 1937–1940* (1952) and *The Undeclared War, 1940–1941* (1953).

With government service and the attendant reengagement in diplomatic history behind him,

Langer took up again the more universal historical problems upon which he had embarked before the world crisis absorbed his energies. He brought to completion *Political and Social Upheaval, 1832–1852* (1969*b*), his own contribution to the influential series which he had launched in 1931 under the title, *The Rise of Modern Europe*. In his own volume and as editor of the series, Langer sought to reinvigorate the tradition of European universal history by comparative analysis of European national societies. Using synchronic cross-sections for each volume, Langer defined a new scale of time and social space which made possible a textual treatment of European culture beyond the prevailing limitations of political narrative.

Langer devoted the last two decades of his life to the intellectual explorations that one normally associates with youth, opening one door after another into the historiographical future. His pioneering papers, collected in Part 4 of *Explorations in Crisis* (1969*a*), entered such disparate fields as the historical uses of psychoanalysis, epidemiological history, and demographic history.

A few days before his death, Langer received the first copy of his last book, an autobiography, *In and Out of the Ivory Tower* (1977). Its title expressed well Langer's conception of the historian's vocation, one centering on the reciprocal illumination of past and present, scholarship and politics.

CARL E. SCHORSKE

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LASSWELL, HAROLD D.

Harold D. Lasswell's voluminous writings may be described as "prose mathematics." Throughout his career, Lasswell's guiding purpose has been to develop a theory about man in society that is comprehensive and that draws on all of the social sciences. Accordingly, one looks first for those key terms that are the basic components of Lasswell's system of thinking, for his lifework has produced just that—a system of thinking about the private and public lives of *Homo sapiens*. The key terms are spelled out explicitly by Lasswell himself.

How did Lasswell, a many-faceted, perfectly polished gem emerge from the rich but rough-cut ores of the Middle West? For, Lasswell, who was born in 1902 and died in 1978, had no doubts about the importance of his Middle Western childhood to his subsequent development. His father was a Presbyterian minister and his mother a schoolteacher. Because his father's ministry kept changing, the family often moved from one small town to another in Indiana and southern Illinois. As a result, Lasswell acquired a vivid sense and a deep knowledge of the whole Ohio River valley, especially of its small mining and farming communities.

During his early years, the Middle West was an extraordinary mix of diverse peoples and cultures, who, in order to live together constructively, had to develop an open society—a series of communities receptive to all other cultural ways and eager to find similar values in belief and behavior. This experience was one of the factors that enabled Lasswell to realize that the priority task of social science should be to make explicit the theory of human interaction.

When Lasswell entered the University of Chicago at the age of 16, he had already read deeply in modern philosophy. He had mastered Kant as well as the works of Freud, which his uncle had acquired to seek guidance with some difficult cases in his medical practice. Lasswell found nothing especially odd about these books and was surprised to learn of the furor that Freud's writings were causing in the academic community. At the university, Lasswell was at the core of the intellectual ferment that existed during the decade following World War I. He studied a variety of disciplines, ranging from medicine and the life sciences to economics and the social sciences. His dissertation, *Propaganda Technique in the World War* (1927), became a major work in the development of communications research.

It is worth noting particularly how Lasswell handled the philosophy of "logical positivism," which was then ascendant in the United States and Europe. Lasswell was deeply impressed by the application of mathematical (symbolic) analysis to complex propositions. How early and well he restricted their ideology by partial incorporation of their insights into his own systematics is illustrated by his famous (and deliberately insoluble) protomathematical equation: $p \} d \} r = P$. This signifies that when private motives (p) are displaced (d) in public rationale (r), the product is political man (P). Lasswell's intention, in using the inoperative symbol $\}$ to mean "transformed into," was to focus attention on the complex process by which subjective events enter the arena of public policy.

This illustrates the important Lasswellian concept of "restriction by partial incorporation." The work of Rudolf Carnap, Alfred Tarski, and Hans Reichenbach (who, later, contributed an important paper to *The Policy Sciences* [Lerner & Lasswell 1951]) certainly figured in Lasswell's subsequent development of quantitative semantics. Yet, since he was already moving toward his own systematics, young Lasswell rejected the "ism" associated with this philosophical school "because it treated subjective events as second-class citizens in the commonwealth of scientific inquiry."

When one compares the published work of all these thinkers with Lasswell's, one finds little evidence of direct influence as revealed by "borrowings." Even his use of Freudian ideas involved transformations, not transfers. Although scrupulous in his footnote references,

Lasswell always used them mainly to share his intellectual discoveries with less voracious and wide-ranging readers. The use of thoughts formed by others has always involved, for Lasswell, a "transformation" of their ideas into his own terminology and systematics. Thus his transformation of Charles E. Merriam's "credenda and miranda," and of Karl Mannheim's "ideology, utopia, and myth." Thus, supremely, his transformation of logical positivism into content analysis and of Alfred North Whitehead's "manifold of events" into the Lasswellian system of "configurative analysis."

At a time when many people thought that values were God-given and enshrined in patristic texts, and many others were embroiled in such controversies as nature *vs.* nurture, Lasswell's incisive formulation pointed to the path that social scientists have followed to their benefit ever since. In his years at the University of Chicago as a teacher (1924–1938), he worked mainly with the three basic value categories of "income, safety, and respect." In his later years at Yale University (1945–1975), he developed an apparently simple, but richly productive matrix of eight values that endlessly interacted upon each other. These values were power, wealth, well-being, enlightenment, skill, affection, respect, and rectitude.

Although reticent about his own development, Lasswell could be generous in evaluating the intellectual contributions of other scholars. Thus, in recalling his years at Chicago, he spoke warmly of his many great teachers, classmates, and colleagues—notably such figures as George Herbert Mead, Robert E. Park, Frank H. Knight, Charles E. Merriam, and T. V. Smith. In speaking of his early *Wanderjahre* in Europe, he gave high praise to Graham Wallas, the "creative evolution" thinkers around Henri Bergson, and the vigorous investigators activated by Sigmund Freud. In his travels between Chicago and Europe, Lasswell developed serious intellectual interactions with scholars on the East coast, notably with Elton Mayo, Karen Horney, and Harry Stack Sullivan.

Of Lasswell's many contributions to twentieth-century thought, a number have earned permanent places in the development of the social sciences. In an effort to describe Lasswell's intellectual history, Bruce L. Smith, his long-time collaborator, divided his major contributions into seven categories: (1) organization of the research field now known as communication; (2) qualitative and quantitative content analy-

sis; (3) developmental constructs of world elites and social orders; (4) specification of a theory of values; (5) linkage of classical political thought with empirical research; (6) studies in legal theory; (7) collection of world trend data (Rogow 1969).

Using pragmatic philosophy, as expounded by George Herbert Mead and John Dewey, Lasswell began to evolve the mode of thinking which, over the next sixty years, he developed into his own systematics, known as "configurative analysis." Central to his analytic system has been the clarification of values as a motivating force in human life. Building his own systematics during the decades when scientism dominated each of the social sciences in turn, Lasswell always stressed the importance of normative functions in human enterprises. Although a behaviorist, he never became an addict of behaviorism (or of any other ideological "ism"). His role as a founding father of behavioral science was certified from the moment in his youth when he defined values as "objects of desire—what people want."

A central term in Lasswell's system is "context." In his perspective, any detail of human behavior became significant only when it was located in an appropriate time-space configuration. Within such a context, analysis becomes possible, for then behavioral details can be described and interpreted in terms of a natural and historical "manifold of events"—a concept that struck Lasswell as particularly productive in his reading of Whitehead, because it required researchers to amplify as fully as possible the context of all relationships under study.

Working with complex patterns of behavior, then handled separately in different academic departments, Lasswell's first priority was to locate diverse items that could be studied and observed under a unifying context. Once a configuration was established, he searched for intellectual tools that would permit valid comparison with other configurations developed according to different terminologies and referring to different time-space frames. The tools that served this purpose grew out of Lasswell's concept of "functional equivalence." Through the idea of "equivalencies," Lasswell had, by 1935, in *World Politics and Personal Insecurity*, developed his own rich configurative version of structural-functional analysis long before this phrase became commonplace in the postwar social sciences.

An example of the research tools that

Lasswell invented to bring scientific exactness into the empirical study of equivalencies is content analysis, alternatively known as quantitative semantics. Lasswell was one of the first social scientists to use content analysis to examine the formation of public opinion. His famous definition of communication as "who says what to whom with what effect" is still used as a basis for research in the field.

During the decade before World War II, he used content analysis to study political propaganda. After providing the key ideas of propaganda analysis (1927), he was later to invent the research tools of content analysis in the 1930s and perfect them during World War II, when the United States government, out of necessity, encouraged such research. Lasswell served as director of a wartime project at the Library of Congress, which produced the unprecedented studies published as *Language of Politics* (Lasswell et al. 1949). In the immediate postwar years, he was the intellectual mentor of the research project on "the world revolution of our time" at Stanford, which compared the political symbols used in the editorials of the major newspapers of five countries from 1890 to 1950. Lasswell devised the symbol code that was instrumental in the search for equivalencies across these many nations and decades. Originally published at Stanford as separate monographs, these studies have since been republished in a single volume as *The Prestige Press* (Pool, Lerner, & Lasswell 1970).

In the course of his studies in communications and international relations, Lasswell concluded that only a small group of elites in any country is aware of developments or pays attention to changes in foreign policy, and this finding is still supported by the results of more recent research (1935; Lasswell, Lerner, & Rothwell 1952).

As indicated, the core of Lasswell's system is the philosophical analysis of the time-space configuration which shapes every "manifold of events" in terms that make them amenable to empirical inquiry. For Lasswell, the world was the appropriate spatial unit for political analysis. Long before it became fashionable to speak of a "global village," Lasswell perceived that both the centripetal and centrifugal trends of current history (what he has called the "zigs" and "zags") were operating on a worldwide basis. His earliest published paper was entitled "Political Policies and the International Investment Market." His most famous political fore-

cast—that the postwar world would be bipolarized in a cold war led by the two superpowers—was presented in *World Politics Faces Economics* (1945).

To understand the global texture of Lasswell's thinking, we must take into account his multi-versal personality and culture. As a contemporary Renaissance man, he was at home in every region of the world, was a gifted amateur of all the arts, and was a systematic student of architecture. While Lasswell's place in intellectual history is assured by his prose mathematics—his rigorous logical analysis of empirical propositions in a contextual framework—it is doubtful that a person of less global interests could have conceived so richly textured a systematics as configurative thinking. Witness his creation of "plain language" radio shows, such as the National Broadcasting Company's series on "Human Nature in Action" in the late 1930s. Witness also his *Politics: Who Gets What, When, How* (1936), possibly the best selling book among comprehensive treatises of political science in the twentieth century. Both the high standards of scholarship in his professional writings and his capacity for "plain talk" are essential in understanding why Lasswell, when World War II erupted, was equipped to turn his attention to any type of problem that he thought was significant for the future of the world.

After World War II, Lasswell decided to concentrate on law, and at Yale University, he began his famous collaboration with Myres S. McDougal, which reshaped much of current legal thinking and education in terms of a continuing concern with "law, science, and policy." They produced, among other books during the past three decades, the landmark *Law and Public Order in Space* (McDougal, Vlasio, & Lasswell 1963). At no point did Lasswell lessen his concern with systematizing the world arena as the appropriate observational unit for all the social sciences, as is indicated by the publication of the *World Handbook of Political and Social Indicators* (Russett, Lasswell et al. 1964) and his *World Revolutionary Elites* (Lasswell & Lerner 1965). This theme is continued in his three-volume work entitled *Propaganda and Communication in World History* (Lasswell, Lerner, & Speier 1979).

Lasswell's idea of context required systematization of time as well as space. For space, "the world" would do until some larger and more useful observational unit became apparent

to him (as in projecting the characteristics of "outer space"). For time, the mechanical units of year, decade, and century really did not suit Lasswell at all. In the pragmatic tradition, he saw time as a continuous process which could be periodized meaningfully only in relation to an appropriate "manifold of events." Some such time units may be short, as in dealing with the phases of world revolutionary communism initiated in Russia by the Bolshevik revolution of 1918. Other time units may be very long, as in characterizing the equivalencies that developed over centuries among the valley empires along the Nile, Tigris-Euphrates, and Ganges rivers.

Once the appropriate time-space dimensions were brought into configuration, Lasswell was ready to test all systems for readiness and to arrange all technical details for the planned empirical exploration. Lasswell always kept the human factor in the center of configurational analysis. Since the appearance of *Psychopathology and Politics* (1930), so much has been written about Lasswell's pioneering integration of psychoanalytic insights with political analysis that one need here only refer to the vast literature by and about Lasswell in this area (Lasswell 1948*b*; 1972*a*). How Lasswell transformed Freudian concepts to serve political analysis is most clearly illustrated in his paper on "The Triple Appeal Principle" (reprinted in Lasswell 1948*a*). Here Lasswell transforms Freud's triad of id-ego-superego into instruments of content analysis for the policy sciences. Let us note, in now turning to the policy science dimension of Lasswell's systematics, that he early developed a hominocentric-contextual definition of the policy process: "Man acting upon resources through institutions to achieve goals." The policy sciences seek to integrate all of the behavioral sciences in behalf of human values and social goals, and to evaluate the applicability of our current knowledge for the solution of specific problems.

As the creator of the policy sciences, Lasswell achieved his own goal of integrating knowledge and action—the goal that Plato set in *The Republic* as the one that could be achieved, in the simpler context of democracy at Athens, by unifying a single class of "philosopher-kings." Lasswell, deeply aware of the modern division of labor and committed to the values of pluralism, early perceived that the Platonic solution was no solution at all for his time-space configuration. Nor was Marx, who read all signs portending revolution—often in the wrong places

at the wrong times—able to think of alternatives to his own prediction (a key phrase in Lasswell's policy sciences). Lasswell could think beyond Marx's time-space limits, while respecting Marx's scholarship and acumen, because he perceived them as a transient historical configuration. Lasswell, well versed in the *Streit um Marx*, therefore had to create alternative ways of looking at the world-in-progress. The impact of Marx upon intellectuals in our century has been great, but Lasswell was not abashed.

Lasswell's conception of the policy sciences brought together the main strands of his earlier thinking that we have sketched above and which was foreshadowed in his great, perhaps greatest, book, *World Politics and Personal Insecurity* (1935). This major work set forth precepts of configurational analysis and illustrated them in a series of brilliant chapters that have taken social scientists nearly half a century to absorb. Known as his most difficult book, this volume exemplifies his skill as a master of prose mathematics. Actually, the vocabulary employed is very simple—words such as income, safety, and respect—but there are two features of the style that were virtually unknown in the social sciences: (1) the use of simple terms from and across many different fields of knowledge; (2) the use of these terms in a rigorous manner, so that each meant the same thing at all times.

Lasswell's key terms were presented most fully in *Power and Society* (Lasswell & Kaplan 1950), and most recently and compactly in a *Pre-view of Policy Sciences* (1971). According to Lasswell, the five main components of policy thinking were (1) setting the goals; (2) identifying the major trends; (3) studying the prevailing conditions; (4) projecting future changes; and (5) considering alternatives. He also defined the seven main phases of every decision making process as (1) intelligence (explaining the problem); (2) promotion (discussion of alternatives); (3) prescription (choosing one of the alternatives); (4) invocation of the alternative; (5) application of the alternative; (6) appraisal of the results; and (7) termination of the decision process. In every configurative analysis, special attention must be given to identification, expectation, and demand, which are the three principal components of individual and public opinion.

Since the appearance of *World Politics and Personal Insecurity* more than forty years ago, Lasswell has articulated and explicated its many meanings. In an article, "Sino-Japanese Crisis:

The Garrison State *vs.* the Civilian State," (1937), he presented his major formulation of a "developmental construct"—a basic concept for the policy sciences since it brings together the five essentials of configurative analysis: goals, trends, conditions, projections, and alternatives. Hardly a year went by without Lasswell's contributing some further elaboration and refinement of the basic concepts and techniques of policy thinking. All of his wartime work in developing content analysis demonstrated empirically a systematic way of handling the daily flow of world verbiage for the rigorous analysis of policy processes.

During World War II social scientists from many disciplines were pressed into wartime service under circumstances that obliged them to blend policy relevance with scientific rigor. During the years following the war, Lasswell undertook to integrate these wartime efforts across the full range of the social sciences, from anthropology through psychology to zoology.

Following Lasswell's preoccupation with spatial units of observation, he brought together a volume entitled *The Policy Sciences: Recent Developments in Scope and Method* (Lerner & Lasswell 1951). The contents ranged from the person (Ernest R. Hilgard) through the group (Edward Shils), the nation (Mead), culture (Clyde Kluckhohn) to the world (Lasswell). Devoted as always to the integration of concept and method, Lasswell designed the second half of the book to make explicit the methodological advances achieved during the preceding decade. These papers were often written by younger scholars—many of them barely known outside of their own disciplines at the time—such as Kenneth J. Arrow, Alex Bavelas, and Herbert H. Hyman. With the publication of this book, Lasswell's system of thinking was baptized—or at least named.

After the publication of *The Policy Sciences* and his commitment to the term policy sciences, Lasswell developed a strategy of collaborating with outstanding specialists in many disciplines. Himself a master of many fields of knowledge, and always a reliable collaborator with people working on problems he considered important, Lasswell's production over the past 25 years may be considered as one man's demonstration of the effectiveness of multidisciplinary efforts. His many books foresaw and subsumed virtually all of the major intellectual currents that have recently emerged in the social sciences around the world.

One example is "development." As the first flush of enthusiasm over American aid faded, Lasswell spelled out the inadequacies of a policy based on transfers of technical assistance. In *World Politics*, he published a masterful review of the literature of the development of the policy sciences, which provided a framework for development thinking. Always concerned with interweaving broad concepts with empirical details, Lasswell also summarized the highly innovative studies he was making in the Andes Mountains of Peru with anthropologist Allen Holmberg (1965).

A second example is "futurology." This had long ago been articulated in Lasswell's idea of the "developmental construct" and exemplified in his own projections of the "garrison-prison state" and the "bipolar world arena." Scholars will long remember Lasswell's presidential address about the future to the American Political Science Association, which he expounded further in *The Future of Political Science* (1963). The futurological emphasis has continued throughout his more recent writings, such as *The Future of World Communication* (1972a) and "The Future of Government and Politics in the United States" (1975).

This brief essay has given only the barest sketch of an intellectual giant. Lasswell was the American of our century about whom it can be said as truly as the Renaissance said of its greatest thinkers: "Nothing human is alien to him." What Lasswell accomplished went even beyond the erudition of the great men of the Renaissance, who sought to encompass all knowledge within a single cerebrum. Lasswell did this at a time when knowledge had expanded by several orders of magnitude and when the information explosion had weakened many lesser spirits. The conclusion is borne upon us that, in Lasswell, we are dealing with a true original in the classic sense.

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LAZARSFELD, PAUL F.

Paul Felix Lazarsfeld was born in Vienna on February 13, 1901. In 1933, he went to the United States as a Rockefeller Foundation fellow. He remained in America after the end of his fellowship; became a naturalized citizen; and for three decades was a professor of sociology at Columbia University. He died in New York City on August 30, 1976.

Trained as a mathematician, Lazarsfeld first thought of himself as a psychologist; only in midlife did he become identified as a sociologist. His major interests were the methodology of social research and the development of institutes for training and research in the social sciences. Because of the originality and diversity of his ideas, his energy and personal magnetism, his unique style of collaboration with students and colleagues, and the productivity of the research institutes he established, his influence upon sociology and social research—both in the United States and in Europe—has been extraordinary.

The Vienna years

Lazarsfeld came from a professional family active in the musical, cultural, and political life of turn-of-the-century Vienna. His father, Robert, was a lawyer in private practice who, in 1908, published a book on jurisprudence; it is reported that he often defended young political activists without fee. His mother, Sofie, was trained in analytic psychology by Alfred Adler. She wrote a book on the emancipation of women and practiced Adlerian individual psychology much of her life, still seeing patients up to her death in her early nineties in New York City, a few weeks after the death of her son. Lazarsfeld's father died in Paris in 1940; his sister Elizabeth moved to Paris when the Nazis came to power in Austria and was awarded French citizenship for her work with the Maquis.

Lazarsfeld had three successive marriages: to Marie Jahoda, Herta Herzog, and Patricia L. Kendall—all his students, all his co-workers, and all accomplished social scientists. His daughter

Lottie Bailyn is a social psychologist; his son Robert, a mathematician.

Socialist youth. Sofie Lazarsfeld's association with another, unrelated, Adler—Friedrich—had more of an impact upon her son than did her association with Alfred Adler. Friedrich was a mathematician and physicist. During the war years, Sofie Lazarsfeld's home became a social center for the socialist leaders of Vienna, and Friedrich Adler became a close friend of the Lazarsfeld family and something of a hero and surrogate father to Lazarsfeld, whose father was at the front. (It was Adler who was largely responsible for Lazarsfeld's lifelong interest in mathematics.) Adler was the leader of the anti-war faction of the socialist party. When the parliament was suspended, and heavy censorship prevented access to public opinion, Adler assassinated the prime minister, Count Karl Stürghk, in August 1916, in order to be able to voice his concerns at the trial. Lazarsfeld both visited him and corresponded with him while he was in prison awaiting trial. Later he attended the trial, and was arrested for taking part in a courtroom demonstration when Adler was convicted.

Austrian socialism in those years was not just another political movement, particularly for the Lazarsfeld family and its friends. Recalling that time and place, Lazarsfeld's boyhood friend Hans Zeisel has noted that "for a brief moment in history, the humanist ideals of democratic socialism attained reality in the city of Vienna and gave new dignity and pride to the working class and the intellectuals who had won it" (1979*b*). Socialism was the familial, social, intellectual, and political environment of Lazarsfeld's early years; he once said that he was a socialist the way he was a Viennese: by birth, and without much reflection. He was active as a leader in socialist student organizations, he created a monthly newspaper for socialist students, and he helped found what Zeisel recalls as "a political cabaret that was to play a seminal role in the development of both the political and theater history of Vienna" (1979*b*). Lazarsfeld's first publication, coauthored with Ludwig Wagner and published when he was 23, is a report on a children's summer camp they had established, based on socialist principles.

Although Lazarsfeld often stressed the importance of his early immersion in the socialist movement, his political activism did not survive his move to the United States. In later life he used to say that he was still a socialist "in my heart," and he once remarked that his intense

interest in the organization of social research is "a kind of a sublimation of my frustrated political instincts—as I can't run for office, I run institutes" (1962*b*, p. 56). His American students and colleagues found him to be essentially apolitical, with the significant exception that he felt very strongly that politics and scholarship should be kept apart. In his memoir he recalls that his academic supervisor at the University of Vienna, Charlotte Bühler, objected to the compassionate tone used in the draft of one of his contributions to *Jugend und Beruf* (Lazarsfeld et al. 1931)—a tone that reflected his belief that social research should be used to improve the conditions of the working class and to achieve other socialist ends. Years later he recalled that this criticism led to his subsequent insistence that professional and political roles should be kept separate (1968*a*, p. 285). For example, while he was at Columbia University he found the political ideas of his colleague C. Wright Mills intellectually acceptable, sometimes stimulating, but he abhorred the way that Mills injected them into his teaching and sociological writings (1962*b*, pp. 149–150).

The Wirtschaftspsychologische Forschungsstelle. Lazarsfeld received his PH.D. in applied mathematics from the University of Vienna in 1925. While a student, he had studied and worked with the psychologists Karl and Charlotte Bühler, who had been invited to come from Germany to Vienna in 1922. Lazarsfeld studied with Karl Bühler, who influenced his ideas about research on individual actions, and he worked at the Böhlers' Psychological Institute as Charlotte Bühler's assistant in her studies of early childhood and youth development. In 1925, he established a division of the Psychological Institute dedicated to the application of psychology to social and economic problems—the Wirtschaftspsychologische Forschungsstelle. Years later, Lazarsfeld recalled (1968*a*, p. 272) that at the time he established the Forschungsstelle he also created a formula to explain his interest in applied psychology: "a fighting revolution requires economics (Marx); a victorious revolution requires engineers (Russia); a defeated revolution calls for psychology (Vienna)."

Karl Bühler became the Forschungsstelle's first president; a board consisting largely of prominent citizens was recruited; Lazarsfeld became the research director; and scores of small research projects were carried out—chiefly for business firms, but some for trade unions and city agencies. "[The Forschungsstelle] came to

life in 1925," Zeisel later recalled, "and sustained itself mainly on ideas, all of them more or less Paul's, on the unabated enthusiasm of its members, and on no money worth talking about" (1979*b*). As in the case of most of Lazarsfeld's projects, the participants never forgot the experience. Ilse Zeisel (Hans's sister, who had been an employee of the Forschungsstelle in the 1930s) remarked at the time of Lazarsfeld's death in 1976 that "in the end it is to the Forschungsstelle and to Paul that we owe our existence if not more" (quoted in Zeisel 1979*b*).

The Forschungsstelle was the first of four university-related applied social research institutes founded by Lazarsfeld; the others were the Research Center at the University of Newark, the Office of Radio Research at Princeton University, and finally the Bureau of Applied Social Research at Columbia University. (The Office of Radio Research was transferred to Columbia in 1939, and the Bureau always cited 1937 as the year it was founded, since this was the year that the Princeton Office was established.) "University-related" is an integral part of the conception, since Lazarsfeld considered such institutes to be essential for the proper teaching and study of the social sciences (1962*c*). Because these four institutes were the embodiment of his intense interest in the institutionalization of social research, more attention is given here to their history and activities than is normally the case in the biography of a scholar and institute director. "The formula of the Viennese Forschungsstelle," Lazarsfeld said years later, "remained absolutely the same whatever I have done since" (quoted in Morrison 1976, p. 94). The formula is still in use in universities throughout the United States and Europe; Glock (1979) presents a list of the major ones. (For accounts of the Forschungsstelle and its activities, see Lazarsfeld 1971; Rosenmayr 1962; and Zeisel 1968; 1969; 1979*b*.)

The Marienthal study. The Forschungsstelle's most ambitious project resulted from Lazarsfeld's interest in undertaking a community study. In order to get started, Lazarsfeld and his associates sought the advice of Otto Bauer, a family friend who was both an eminent historian and the leader of the Social Democratic party. The Austrian trade unions had managed to eliminate the ten-hour working day, and Lazarsfeld told Bauer that they intended to study community leisure patterns. Bauer thought it was ridiculous to study leisure when so much of the population was unemployed; *that is the*

leisure to study, he said, the social and psychological effects of unemployment. He suggested that they study *Marienthal*, a nearly totally unemployed one-industry village 24 kilometers southeast of Vienna (Lazarsfeld 1968a, p. 275; Zeisel 1979b).

The *Marienthal* study was directed by Marie Jahoda, Lazarsfeld, and Hans Zeisel. The methods used were both imaginative and eclectic: participant observation, life history analysis, and a variety of unobtrusive measures. In order to obtain measures of the effects of unemployment, for example, the researchers calculated the speed at which people walked, finding that men walked more slowly on the average than did women, since they had more time to kill. They noted that the circulation of the socialist party newspaper declined more during the years of widespread unemployment than did the circulation of a sports and entertainment newspaper—interpreted as a measure of withdrawal from participation in political affairs. The circulation of books from the workers' library was also examined; although the borrowing fee was abolished during the years 1929–1931, the circulation declined by almost half—a decline that was interpreted as an indication of apathy.

The *Forschungsstelle* was “a sequence of improvisations” (Lazarsfeld 1968a, p. 287), but it carried out a great deal of innovative consumer research and it contributed importantly to the development of this field by making the study of consumer decisions academically respectable. However, it is *Marienthal*, a slim, clearly-written volume, that remains the *Forschungsstelle*'s most memorable product (Jahoda, Lazarsfeld, & Zeisel 1933). The study has impressed generations of social scientists by its integrated use of quantitative and qualitative observations; the Lynds, for example, in their *Middletown in Transition* (1937), repeatedly refer to the methods and findings of *Marienthal*. It contributed substantially to the methodology of community studies (see the review of the history of what the authors called “sociography” in Zeisel's “Afterword” to *Marienthal*); and its major finding—that prolonged unemployment of workers leads to apathy rather than to revolution—foreshadowed the lack of resistance to Hitler (Zeisel 1979a). *Marienthal* was banned by the Nazis soon after it was published, but by 1978 it had become part of the sociology curricula in Austrian universities. In 1979, a group of young Europeans began a restudy of the village, which is now defunct as a political

entity, using as far as possible the original methods, plus a new one—video recording (Freund 1978).

Career in America

Lazarsfeld first went to the United States in September 1933 as a Rockefeller fellow; the *Marienthal* study had brought him to the attention of the foundation. He spent the academic year 1933/1934 visiting universities where he had been told that social research was being done: Columbia, Harvard, Pittsburgh, Ohio State, Rochester, Chicago—the itinerary is listed by Morrison (1976, appendix A). In most places, he tried to learn by attaching himself to one or more of their research projects. With the enthusiasm, energy, and imagination that characterized his entire career, he sent a questionnaire to the eight other European fellows in his group in order to study their adjustment to America (1968a, p. 299).

Lazarsfeld's fellowship was extended for a second year, at the end of which he decided to remain in America. The political situation in Austria following the defeat of the Social Democrats in the civil war of February 1934 had made his return to the University of Vienna impossible. The *Forschungsstelle* was in the same deficit state he had left it in two years earlier; and his marriage to Marie Jahoda—who had remained in Vienna with their daughter—had ended. So he accepted a position, for which Robert S. Lynd of Columbia University had recommended him because of the *Marienthal* study, studying unemployment—analyzing some ten thousand questionnaires from young people that had been collected by the New Jersey Relief Administration. Lazarsfeld soon transformed the project into the University of Newark Research Center, whose director he became. At the Center, as well as at its successor institutes, Lazarsfeld employed a large number of refugee social scientists, often giving them their first job in the United States.

The Newark Center survived its first year by carrying out studies for the public school system, the Works Progress Administration, and the Frankfort Institute for Social Research—then in exile in Paris. Located on the fringes of a small university, with only a handful of staff members, the abiding meaning of the Center is that it was for Lazarsfeld the American rebirth of the *Forschungsstelle*.

The Princeton radio project. In 1937, the Rockefeller Foundation approved a proposal by

Hadley Cantril, the Princeton psychologist, for a large-scale study of the social effects of radio. At that time, radio seemed to many observers to be an enormous source of social, cultural, and political change; in the 1930s, both Franklin D. Roosevelt and Adolf Hitler were using the radio as a direct means of communicating with the public. Again on the recommendation of Lynd, Lazarsfeld became director of the study. An Office of Radio Research was established at Princeton, with a two-year grant of \$67,000, but the project itself was located at the Newark Center. Cantril and Frank Stanton, then the research director, later the president, of the Columbia Broadcasting System, were appointed associate directors, and a broad study of radio programming, radio audiences, and the preferences of radio listeners was begun. The emphasis was on the secondary analysis of existing survey data, the content analysis of programs, and the Lazarsfeld-Stanton Program Analyzer (a jointly-developed device for recording the expressed likes and dislikes of experimental audiences). Many journal articles and a series of monographs were published (see especially Lazarsfeld 1940c; Lazarsfeld & Stanton 1941; 1944); numerous students were given on-the-job training; and the new field of mass communications research became greatly enriched. Wilbur Schramm dedicated his influential reader, *Mass Communications* (1949), to Lazarsfeld for what he had done "perhaps more than any other man toward bringing the social sciences to bear on the problems of communications."

If Harold D. Lasswell may be said to have created the agenda for the field of communications research with his question "Who says what to whom with what effect?" (1932), Lazarsfeld extended the agenda in important ways by asking why messages are introduced into the media and why people attend to them, that is, what gratifications, what rewards, people get from the media and what functions the media serve in their lives. Herta Herzog's studies of the audiences of daytime radio "soap operas" and of the radio listeners who believed the famous 1938 Orson Welles broadcast about an invasion from Mars (Herzog 1938; 1943), are examples of this extension, as are the studies of Edward A. Suchman (1941) and T. W. Adorno (1941a; 1941b) on the social roles of popular and serious music. Other research projects carried out by Lazarsfeld's associates are Bernard Berelson's study of "What 'Missing the Newspaper' Means" (1949), which used the occasion of a newspaper

strike in New York City to ascertain the functions that newspapers serve in the lives of their readers, and Leo Lowenthal's (1944) study of the functions served by biographies of contemporary persons in popular magazines. Lazarsfeld's own research on comparing the effects of radio listening and reading (1940c) was the first serious examination of this important question.

Some of the conclusions of this research were summarized in an essay by Lazarsfeld and his Columbia University colleague Robert K. Merton, "Mass Communication, Popular Taste and Organized Social Action" (1948), that discusses the social functions performed by the media, noting that they both confer status upon people and enforce social norms, while at the same time serving a "narcotizing dysfunction." The essay concludes that the effect of the media is largely confined to peripheral social concerns and that the media do not exhibit the degree of social power, the capacity to change social attitudes, that is often attributed to them.

The "Princeton radio project" was a misnomer; it was neither located at Princeton nor confined to radio ("I use just everything on which I have worked for the past years and call it radio now," Lazarsfeld wrote in a 1938 memorandum to Cantril and Stanton; quoted in 1968a, p. 308). Its work greatly enlarged Lazarsfeld's stature in the world of applied social research and led directly to his appointment at Columbia.

The Bureau of Applied Social Research. The Office of Radio Research soon acquired what Lazarsfeld later called "an institutional life of its own" (1968a, p. 309), but the University of Newark's depressed financial straits forced the Research Center to move to rented space in New York City in 1938. In 1939, the Rockefeller grant was renewed but transferred from Princeton to Columbia University, where Lazarsfeld was appointed a lecturer and soon thereafter an associate professor of sociology. In 1944, the Office of Radio Research was renamed the Bureau of Applied Social Research, and in 1949 the offices were moved to one building, and in 1957 to another building, both adjacent to the Columbia campus. During the 1950s and 1960s, the Bureau expanded its program and grew steadily in terms of both income and staff; by the mid-1970s, its gross annual income was more than a million dollars and it employed at any one time more than a hundred people, half of them full time (A. H. Barton 1979). How-

ever, in 1977, a year after Lazarsfeld's death, the Bureau was closed and its legacy and library were transferred to a new Center for the Social Sciences, located on the Columbia campus.

The Bureau's offices were temporary and makeshift throughout its entire life span. In 1938 it had moved from an abandoned brewery in Newark to an old office building on Union Square in New York City and in 1940 to a building that was formerly part of the Columbia medical school; in 1949 it moved to a former faculty residence and in 1957 to a former women's dormitory. The physical locations are indicative of the Bureau's never quite becoming the established university-based social research institute that Lazarsfeld had first dreamed of in Vienna. Although it was extremely productive, it was destined to remain, like its founder, somewhat marginal to the mainstream of American academic life (Lazarsfeld 1968a, p. 302). It survived for forty years, generally amidst administrative chaos, and with conspicuously little financial support from the university. But the research ideas it created, the leading social scientists that it trained, the innovative research it carried out, and its distinctive organizational structure have greatly influenced the social sciences throughout the world. (For historical accounts of the Bureau, see A. H. Barton 1979 and Glock 1979; for a bibliography of its publications, see J. S. Barton 1977).

Lazarsfeld remained at Columbia from 1940 until his retirement in 1969; after 1962, he held the chair of Quetelet professor of social sciences. This chair was created specially for him at the suggestion of his colleague, Robert K. Merton—and was so named because Lazarsfeld believed that the Belgian statistician Adolphe Quetelet was the originator of empirical social research (see Lazarsfeld 1961b, pp. 164–181; Landau & Lazarsfeld 1968). Unwilling to give up teaching after his retirement, he traveled almost weekly to the University of Pittsburgh, where he served from 1969 until his death as distinguished professor of social sciences.

The interaction of theory and method

During his 52 years of active professional life, Lazarsfeld contributed importantly to 4 substantive areas in the social sciences: the social effects of unemployment, mass communications, voting behavior, and higher education. These contributions were not the result of a grand design, but of historical accidents. Lazarsfeld studied the effects of unemployment in an Aus-

trian village in the early 1930s because Otto Bauer ridiculed his plan to study leisure; he studied the impact of radio in the late 1930s because he was an immigrant in need of a job; and his study of the 1940 U.S. presidential election grew out of a planned evaluation of U.S. Department of Agriculture radio programs directed at farmers. All his life he was interested in university organization, but his major study of higher education—a study of how college and university teachers reacted to “McCarthyism” in the early 1950s—came about because Robert M. Hutchins, then president of the Fund for the Republic, asked him to undertake it.

It is incorrect to say that Lazarsfeld was not interested in substance; rather, for him method was inseparable from substance. Accordingly, it is also incorrect to call him a “mere” methodologist. In the first place, there is nothing “mere” about methodology (Jahoda 1979) and Lazarsfeld's effort to combine quantitative and qualitative methods constitutes one of the most ambitious of all undertakings in the social sciences. In the second place, methodology for Lazarsfeld was far broader than method, and he believed that the methodologist does much more than introduce quantification into a research project. Rather, a methodologist is a person who “tells other scholars what they have done, or might do, rather than what they should do” (Lazarsfeld & Rosenberg 1955, p. 4). And in the third place, Lazarsfeld's students were often shocked to learn through their association with him that theory and method are not two branches of sociology but often much the same thing.

Methods of survey research. When Lazarsfeld undertook his major study of the impact of radio in 1937, he realized that since radio listening left no public records, such as circulation data, it needed new methods of study. He took the opinion poll—at that time used largely for descriptive purposes, for example, to measure popularity or audience size—and by the multivariate analysis of responses developed ways to identify causal relationships. This transformation of the opinion poll into “survey research” constitutes one of Lazarsfeld's major accomplishments. For this reason, Charles Y. Glock's *Survey Research in the Social Sciences* (1967) is dedicated to Lazarsfeld.

Several important procedures to follow in the analysis of survey data are described in “Problems of Survey Analysis” (Lazarsfeld & Kendall 1950), a classic article that codifies and clarifies some of the techniques first developed in *The*

American Soldier volumes. The article is in effect a manual on how to avoid spurious relationships in survey analysis and how to make causal attributions by straightening out the time sequence of the variables involved. Herbert Hyman's *Survey Design and Analysis* (1955) is an extended codification of these principles. Years later, Lazarsfeld developed algebraic ways of expressing many of these ideas; see, for example, "The Analysis of Attribute Data" (1968b).

In studying radio audiences, Lazarsfeld encouraged the use of open-ended, detailed interviews to uncover the subjective experiences and motivations of the persons interviewed (see Merton, Fiske, & Kendall 1956), just as he urged the use of content analysis to provide a more precise measure of the nature of the stimulus. His procedure was essentially sequential: his interest in methods influenced his choice of research topics; the substantive findings of his research generated more theoretical findings; and these findings often pointed out the need to develop still other methods (see A. H. Barton 1979 for an illustration of this process).

The panel method for the study of change. A major finding of Lazarsfeld's research on radio audiences is the tendency of audiences to be self-selected (see, for example, Lazarsfeld, Berelson, & Gaudet [1944] 1968, pp. 120-136; Suchman 1941). Accordingly, in order to sort out the causal sequences of such problems as the effect of listening upon attitudes versus the effect of attitudes upon patterns of listening, a method of studying the time order of variables was required. Drawing upon his research in Vienna with the Bùhlers, in which repeated observations were made of the same children over time, as well as on the earlier research of Stuart A. Rice among Dartmouth College students and Theodore M. Newcomb among Bennington College students, Lazarsfeld developed what he called the panel method, in which a sample of respondents is reinterviewed at intervals of time (Kendall 1954; Lazarsfeld & Fiske 1938; Levenson 1968).

The panel method is essentially a field experiment in which a "natural" rather than an experimental population is studied. Although Lazarsfeld cannot be said to have invented the panel method, it was his use of it, and particularly his innovative ways of analyzing data derived from panel studies, that made him its earliest and most effective exponent.

The study of interpersonal influence. Lazarsfeld used the opportunity of his famous study

of the 1940 U.S. presidential election, *The People's Choice*, another slim, elegant volume (Lazarsfeld, Berelson, & Gaudet 1944), to test and extend the panel method as a field technique. The substantive findings of the study are also important. First, a great deal was learned about the psychological and social processes that delay, inhibit, reinforce, activate, and change voting decisions; people subject to cross pressures, for example, delay making a decision. Second, the study revealed a great deal about the phenomenon called opinion leadership: it was found that opinion leadership is horizontal as well as vertical in a community and that there is a flow of opinion from the mass media to persons who serve as opinion leaders and then to the public. This process was termed the "two-step flow of communication."

Traditional methods of sampling populations are inadequate to study interpersonal influence, since their design makes it nearly impossible for people who know each other to turn up in the sample (Lazarsfeld, Berelson, & Gaudet [1944] 1968, pp. 49-50). Accordingly, new methods of sampling had to be developed. The first method used was called "snowball" sampling, in which informants from different strata in the community are asked to name people who have influenced their decisions in concrete ways. (The sample thus grows the way a snowball does, incrementally.) The persons who are cited repeatedly are termed opinion leaders, and a sample is selected from this group and then interviewed (see Katz & Lazarsfeld 1955; Merton 1949).

"Snowball" sampling locates opinion leaders, not networks of individuals, so new methods of finding networks by the use of sociometric-type nominating questions ("Who have you talked to about this recently?") were developed. These techniques (which were influenced by Jacob L. Moreno's use of the sociogram; see Borgatta 1968) were further developed in a study, directed by three of Lazarsfeld's students and associates, of the networks of influence among physicians in a community that determine the diffusion of the prescribing of new drugs (Coleman, Katz, & Menzel 1966).

These techniques for measuring interpersonal influence, opinion leadership, and networks of influence stimulated a wide variety of studies by Lazarsfeld's students and their students, each of whom developed a variant of the method and a new field of substantive application. Examples are Peter H. Rossi on interpersonal environ-

ments (1966); Rossi's student W. L. Wallace on the student culture of a liberal arts college (1966); Richard Alba and Charles Kadushin on social circles (Alba & Kadushin 1976; Kadushin 1968a); Kadushin on intellectual elites (1974); and Alba and Gwen Moore on elite social circles (1978).

The empirical study of action. Throughout his long professional life, Lazarsfeld was intrigued by the problem of how to study "action" from the point of view of the actor. (He contended that "action" was a correct translation of the German *Handlung*, a concept that had stimulated a great deal of psychological research in the Europe of his youth.) In 1958, he wrote a long essay on the problem, "Historical Notes on the Empirical Study of Action: An Intellectual Odyssey" ([1958] 1972); although well known to many of his students and colleagues, the essay was not published for 14 years, ostensibly because it was "too long for an article, too short for a book" ([1958] 1972, p. viii). In the essay, he traced some of the ideas on action that had influenced him, from Karl and Charlotte Bühler's work on goals to Kurt Lewin's study of intentions to his own work on the analysis of consumer behavior. The discontinuity between these ideas and empirical research in his adopted country became one of his major pre-occupations.

Much of the research done by Lazarsfeld and his students on this problem concerned the study of the "reasons" people give for their behavior—a research procedure that came to be known as "reason analysis." At the heart of the procedure is the development of what is called an "accounting scheme"—a model of the action being studied that incorporates the various dimensions of the act on which data are needed (see Zeisel [1947] 1968, pp. 153–170). Many of the data in an accounting scheme are obtained by personal interviews, and in a crucial part of the interview the interviewer asks the questions necessary for the analyst to do what Lazarsfeld (1940a) called "discerning": determining not only that a person was exposed to a given influence, but that he or she acted in a certain way because of that exposure (see Lazarsfeld 1942; Smith [Lazarsfeld] & Suchman 1940).

Lazarsfeld's classic article on reason analysis, "The Art of Asking Why" (1935), was published shortly after he arrived in the United States, and is based largely on consumer studies that he had carried out at the Forschungsstelle in Vienna. The basic premise of the article is that

there are three types of data that need to be obtained by asking "why" questions in studying consumer purchases: data that report on (1) influences that lead toward action, (2) relevant attributes of the product, and (3) acted-upon impulses of the purchaser. This formulation has a generality that goes far beyond consumer research, and has been used—with adaptations and extensions—by Lazarsfeld, his students, and others not only for the study of consumer purchases (Kornhauser & Lazarsfeld 1935) but also for studies of changes in vote intentions (Gaudet 1939), choosing or not choosing trial by jury (Zeisel [1947] 1968, pp. 161–164), choosing an occupation (Lazarsfeld et al. 1931), getting married or divorced (Goode 1956), going to a psychiatrist (Kadushin 1958), joining a voluntary association (Sills 1957; 1960), moving from one house to another (Rossi 1955), and not practicing contraception (Sills 1961a). Summaries of these research procedures are provided by Kadushin (1968b), Lazarsfeld and Rosenberg (1955, pp. 387–391), and Zeisel ([1947] 1968, chapters 6, 7).

The intensity of Lazarsfeld's interest in the study of action is indicated not only by his 1958 historical essay but also by the attention given to it in his methods reader, *The Language of Social Research* (Lazarsfeld & Rosenberg 1955), in his autobiographical memoir (1968a), and in his essay "Working With Merton" (1975). Since he viewed the analysis of action as a way of merging the study of individuals with the study of the aggregate effects of individual actions, and thus as a way of merging psychology and sociology, the centrality of the problem to his intellectual career is evident.

The relationship between individual and collective properties. Techniques for relating the characteristics of individuals to those of collectivities were termed by Lazarsfeld "contextual analysis." They involve characterizing individuals by some characteristic of the group to which they belong (the context), and then noting how individuals who are similar in other ways differ in their opinions or behavior in accordance with the group context in which they are located (Sills 1961b). The characteristic of the group may be an aggregate of individual characteristics (as in "climate of opinion" studies) or it may be a so-called "global" characteristic that describes the collectivity as a whole. Contextual analysis was used at Columbia in a study of the membership of the International Typographical Union (Lipset, Trow, & Coleman

1956); Lazarsfeld at first resisted applying it to his own work, but was forced to give in at the insistence of his students. He first made systematic use of the procedure in a 1955 study of social science faculty members in American colleges and universities (Lazarsfeld & Thielens 1958).

The 2,451 faculty members who were interviewed in the study constitute a probability sample of all American college teachers. Lazarsfeld was interested not only in the opinions of these faculty members but in the institutional determinants of their opinions. Accordingly, the sample was designed so that interviewing took place at only 165 colleges and universities, at each of which at least half of the social science faculty was interviewed. It thus was possible to examine variations in faculty opinions and behavior according to variations in individual characteristics; according to variations in the "climate of opinion" of different institutions; and according to variations in the "global characteristics" of institutions—or by various combinations of these variations.

The classic analytic paper on the study of collective properties is Lazarsfeld and Menzel (1961); a section of *Continuities in the Language of Social Research* (Lazarsfeld, Pasanella, & Rosenberg 1972, pp. 219–320) contains ten selections that describe the study of collectivities and refers the reader to many other examples of research.

Mathematics in the social sciences. Lazarsfeld never abandoned his early interest and training in mathematics, and he sought for many years to introduce improved mathematical methods into the social sciences. His own work was primarily in mathematical psychology, since he sought to model processes within the individual. His impact on mathematical sociology was of a different kind: he posed problems, he raised questions, and he organized the efforts of others. The papers by T. W. Anderson, James S. Coleman, Leo A. Goodman, R. Duncan Luce, and Herbert A. Simon in the Lazarsfeld *Festschrift* (Merton, Coleman, & Rossi 1979) demonstrate his influence on the use of mathematics by this generation of social scientists; see also Lazarsfeld (1954*b*) and Lazarsfeld & Henry (1966). Coleman (1972) provides a useful overview of Lazarsfeld's mathematical work.

Latent structure analysis. One aspect of Lazarsfeld's mathematical work had its origins in his wartime association with Samuel A. Stouffer on studies of the American army. He invented

at that time an analytical procedure he called latent structure analysis, a procedure that is designed to ascertain the structure of attitudes or other unobservable entities that can only be observed through their probabilistic connection with observed data. Stated differently, Lazarsfeld's starting point in developing latent structure analysis was "the problem of how concepts may be *inferred* from indicators" (Lazarsfeld & Henry 1968, p. 3). Although he spent decades developing the procedure (rather than the long weekend that at one point he thought it would take), the complex computational procedures required initially discouraged the use of latent structure analysis in research. More recently, the development of high-speed computers has reawakened interest in this type of analysis (see, for example, Henry 1973; for examples of earlier work, see Lazarsfeld 1950*a*; 1959; Lazarsfeld & Henry 1968; Madansky 1968).

Other interests

The history of empirical social research. As early as the Marienthal study, Lazarsfeld was fascinated by the history and development of research methods. But he did little systematic work on the topic until a 1959 interdisciplinary conference led him to prepare a paper (1961*b*) in which he traced the history of quantification in sociology. In 1962/1963, he gave courses and led seminars at the Sorbonne and at Columbia on the history of quantification; the topic, originally sketched out under Lazarsfeld's prompting by Zeisel in his "Afterword" to *Marienthal*, became one of his major interests during the remainder of his life. Under his direction, original research was carried out on the history of quantification in Germany (Lazarsfeld & Oberschall 1965; Oberschall 1965; Schad 1972); France (Clark 1965; 1967); Belgium (Landau & Lazarsfeld 1968); the United Kingdom (Cole 1972; Elesh 1972); and Europe generally (Lécuyer & Oberschall 1968; Oberschall 1978). Lazarsfeld's interest in the topic made him into something of a reverse missionary during the last 15 years of his life, attempting to convince Europeans that "American-style" empirical social research was strongly influenced by a European empirical tradition. He was primarily responsible for—or exerted a strong influence on—the establishment of research institutes in Oslo, Vienna, and Jerusalem, and his visits to Paris and Warsaw greatly altered the nature of social research in these cities. He visited Paris frequently, and Reid Hall at the

Sorbonne became almost his second home. He invited a number of Europeans to spend a year at Columbia, and in this way enriched sociology on both sides of the Atlantic. When he died, Raymond Boudon and Jean Stoetzel wrote memorial articles for the Paris press, and almost every sociological journal in Western Europe published an obituary.

The utilization of social research. Lazarsfeld's career began with his founding of an institute for applied social research in 1925, and he never lost his interest in the practical applications of research. For him, clients with problems to be solved were more than a source of funds; they were a source of real problems and new methods. Most of his research was sponsored by an organization that had a problem and he did extensive consulting work with a number of industries, firms, and advertising agencies—not only because he needed the money but also because he appreciated the intellectual stimulation. When as president of the American Sociological Association he had the opportunity to set the theme of the annual meeting in 1962, he chose "the uses of sociology." Many of the papers given were subsequently published in a volume with that title (Lazarsfeld, Sewell, & Wilensky 1967).

The Uses of Sociology contains 31 chapters on an enormous range of applications—from social work, to law, to public health, to desegregation, and its "Introduction" by the editors is a manual on the nature, the problems, and the prospects for the application of sociology to real-life problems. The book was a publishing success, but Lazarsfeld was not satisfied that it had captured what he thought was most important about applied research. He continued to work on the problem for the rest of his life, and his last book was *An Introduction to Applied Sociology* (Lazarsfeld & Reitz 1975).

Influence on the social sciences

Students, colleagues, and other associates. Throughout his life, Lazarsfeld worked intensively with students and colleagues, and a full-scale intellectual biography of him would of necessity also be a biography of his associates. As he freely admitted, his early years as an organizer of socialist youth activities established a pattern of leadership that he never fully abandoned: he was skilled at telling others what they should do, and then helping them do it. Most of his major writings are coauthored, and much of his work day consisted of listening to, talking

to, and instructing his students, colleagues, and co-workers: in class, in his office, in taxicabs, in his apartment, in a succession of summer houses in New Hampshire; at breakfast, at lunch, and at dinner; at the blackboard, or pacing his office with a cigar, or seated in the faculty club with a double Manhattan cocktail in hand, Lazarsfeld seldom was or worked alone, and he was always working. What Allen H. Barton termed "the hectic Lazarsfeldian life style" (1979) went on to midnight or later; only then did he work for hours alone. (For another description of this life style, see Bailyn 1979, in which a visit by Lazarsfeld to his daughter's and son-in-law's house is compared to a "wonderfully benign hurricane.")

Lazarsfeld's need for so many close associates was a consequence of his massive research agenda, which was so long (and grew ever longer) that he could not possibly carry it out by himself. Accordingly, he was continually in search of new associates, and whenever he found a person who had skills and what he called a "latent interest" in one of his problems (1964*b*, p. 19), he sought to draw him or her into his orbit. These scores of individuals may be said to have been exploited in the sense that they were persuaded to work on Lazarsfeld's problems, but in the process they found problems for themselves and learned how to work on them: the subsequent publications of the persons "exploited" by Lazarsfeld testify to the two-way nature of the relationship. The "Supplementary Bibliography" of this article, kept to a manageable length only by exercising strict discipline, is a testament to the influence of Lazarsfeld's ideas upon the publications of others. His characteristic method of teaching a graduate sociology course was not to lecture at all, but to describe a series of problems to which he did not know the answer. The problems were divided up among the students, and the purpose of the course was to help the students and Lazarsfeld at least to clarify the problems.

Coleman (1979) presents an inventory of some of Lazarsfeld's major associates and what they contributed to his work and he to theirs; here only a few can be mentioned. Hans Zeisel, now emeritus professor of law at the University of Chicago, worked with Lazarsfeld in Vienna at the Forschungsstelle, with Jahoda and Lazarsfeld on the study of Marienthal, and later with Lazarsfeld at the Bureau of Applied Social Research; his *Say It With Figures* (1947), which is a textbook that is more than a textbook, a

manual that is more than a manual, and has now been translated into six languages, is a product of their long collaboration. Zeisel's essay, "The Vienna Years" (1979*b*), in the Lazarsfeld *Festschrift*, is both a record of and a tribute to their association.

Robert S. Lynd, then professor of sociology at Columbia, befriended Lazarsfeld from the time of his first visit in 1933; for decades, the Lazarsfelds went to the Lynds' apartment each November for Thanksgiving Day dinner. Bernard Berelson, Frank N. Stanton, and Edward A. Suchman were early collaborators in his work on mass communications. Allen H. Barton, professor of sociology at Columbia, first studied with Lazarsfeld in 1947 and accompanied him to Oslo in 1948 to help establish a research institute at the university. They coauthored an important article on qualitative measurement (1951) and Barton was director of the Columbia Bureau from 1962 to 1977. Other important students at Columbia during the productive decade of the 1950s were James S. Coleman, Charles Y. Glock, Elihu Katz, William N. McPhee, Peter H. Rossi, Hanan C. Selvin, and Lee M. Wiggins. Seymour M. Lipset was at Columbia during the 1950s, and has provided a personal account (1979) of the complexities of being a junior colleague of Lazarsfeld.

In Paris (and for one year in New York) Lazarsfeld worked closely with Raymond Boudon (see Boudon 1976); in Warsaw, where he was fascinated by the social research that was being done in the late 1950s in order to test the efficacy of various socialist programs, he worked particularly with Stefan Nowak. His collaborative relationship with his three wives has been mentioned previously. For years, he and the Columbia philosopher Ernest Nagel taught a successful graduate seminar on the logic of various methods of social research. He had intense and complex relationships with two sociologists whose approaches to scholarly work were totally different from his: T. W. Adorno (see Lazarsfeld 1968*a*, pp. 322–326; Morrison 1978) and C. Wright Mills (see Lazarsfeld 1962*b*, pp. 352–375; Mills 1959). Both were critical of him, and he of them, but he went to great lengths to try to find common ground. But more important than any of the individuals named above in their impact upon Lazarsfeld and in his impact upon them are two eminent sociologists—Samuel A. Stouffer and Robert K. Merton.

Samuel A. Stouffer was a 36-year-old University of Chicago professor when Lazarsfeld first met him in 1936; at that first meeting they agreed to collaborate on a book on the family in the depression that was a part of a Social Science Research Council inquiry into the depression directed by Stouffer (Stouffer & Lazarsfeld 1937). They established at that time what Lazarsfeld called "an alliance" that lasted until Stouffer's death in 1960. Their most notable collaboration was on the wartime research on the U.S. Army that led to the four-volume series that included the two volumes called *The American Soldier*, published in 1949 and 1950; Lazarsfeld wrote the introduction (1962*a*) to a posthumously published selection of Stouffer's papers.

It was Stouffer who first introduced Lazarsfeld to the four-fold table—a concise way of demonstrating the cross-tabulation of two dichotomous variables—by drawing one on a luncheon table cloth one day in Newark in 1937 (Lazarsfeld 1962*b*, p. 145); Mirra Komarovsky was a witness. Later, during the war, Lazarsfeld's ideas on latent structure analysis (1950) and the use of surveys for causal analysis (Lazarsfeld & Kendall 1950) were worked out in discussions with Stouffer. Their personal and research styles were totally different. Lazarsfeld was the somewhat flamboyant, cultured European, Stouffer the homespun, modest Midwesterner; Lazarsfeld the leader of research teams, Stouffer more the loner, famous for creating his own statistical tables on the IBM counter-sorter outside his office door. Both were totally absorbed in obtaining ideas and findings not from speculation but the hard way, from data. Perhaps because he saw in Stouffer a more disciplined and self-effacing reflection of himself, Lazarsfeld considered Stouffer "the most important man of all of us . . . an outstanding mind in our generation" (1962*b*, pp. 174–175), and Stouffer's effect upon his thinking, although difficult to specify, was enormous.

Merton joined the Columbia faculty at the same time as Lazarsfeld. In fact, the Merton and Lazarsfeld appointments were designed to resolve an internal dispute over whether the next major appointment to the Columbia sociology faculty was to be a theorist or a methodologist: with Merton and Lazarsfeld, Columbia thought to get both. It did get both, and more, but in a complex way. Lazarsfeld pulled Merton into many of his research projects during their thirty

years together in the department, and Merton encouraged Lazarsfeld to develop methods as a way of building theory.

The relationship between Lazarsfeld and Merton was intense, personal, and deeply meaningful both to each other and to their students. Speaking for generations of graduate students at Columbia, Selvin wrote that "we were satellites, not of one sun, but of two, for Robert K. Merton and Paul F. Lazarsfeld so dominated sociology at Columbia during these three decades that no lesser figure of speech would do" (1975, p. 339).

Although they worked closely together for years, Lazarsfeld and Merton were seldom coauthors—that is, in print. In fact, many of Lazarsfeld's writings during the Columbia years were written in close collaboration with Merton. On the title page of the copy he gave Merton of a long chapter of his on latent structure analysis (1959), Lazarsfeld wrote: "Bob, this is the first item in 20 years you did not have to work on. P." And each of their six published collaborative efforts reveals something important about their relationship. They coedited *Continuities in Social Research: Studies in the Scope and Method of "The American Soldier"* (1950), a brilliant attempt to enrich social theory by reanalyzing the attitude surveys that Stouffer had conducted during the war. Their three coauthored articles on mass communications (1943; 1944; 1948) are on the social and cultural meaning of the radio research that Lazarsfeld carried out for a decade. In their "Friendship as Social Process" (1954), Lazarsfeld recast a number of Merton's sociological propositions about friendship into formal, deductive mathematical terms and indicated their research relevance. And in "A Professional School for Training in Social Research" (1950), they reviewed plans for a new educational institution that they talked about to each other and to others for many years but never managed to create.

Each of these publications is important, but none is as important as the 35-year-long conversation they carried on with each other. They respected each other's judgment, they tested ideas with each other, and they reviewed, often in great detail, the ideas of their colleagues and students. In his article in the Merton *Festschrift*, "Working With Merton" (1975), Lazarsfeld has provided a uniquely detailed recollection of their close relationship during these three decades; in the Lazarsfeld *Festschrift*, Merton refers to

him as a "brother" (1979); but their students and colleagues know that these words only hint at the depth and complexity of their working relationship and their intellectual companionship.

Recognition. Lazarsfeld received many acknowledgments of his accomplishments during his lifetime. He was president of both the American Association for Public Opinion Research (1949/1950) and the American Sociological Association (1961/1962), and he was elected a member of both the National Academy of Education and the National Academy of Sciences. He received honorary degrees from Chicago and Yeshiva universities in 1966, from Columbia in 1970, from Vienna in 1971, and from the Sorbonne in 1972, the first American sociologist ever so honored. In 1955 he was the first recipient of the Julian L. Woodward memorial award of the American Association for Public Opinion Research, and in 1969 the Austrian Republic awarded him its great golden cross, largely for his help in establishing the Institute for Advanced Studies in Vienna in 1963. He was a much sought-after consultant, speaker, and teacher. Shortly after his death, a memorial service was held in St. Paul's Chapel, Columbia University, attended by hundreds of his students and associates: Bernard Bailyn, William J. McGill, Robert K. Merton, Ernest Nagel, and Hans Zeisel were the speakers. His students and colleagues have established a Paul F. Lazarsfeld memorial fund in order to sponsor a series of lectures in his honor. Hans Zeisel gave the first lecture in October 1978.

Lazarsfeld's innovations in consumer research, and his impact upon the business and advertising community, were influential; he was a major academic model for the generation of advertising and market researchers that matured in New York City in the decades following World War II. His work in communications research helped create it as a field of scholarship, and through his analyses of propaganda during World War II (Lazarsfeld & Merton 1943) and his influence upon the research activities of the Voice of America (exerted largely through his colleague Leo Lowenthal and such students as Marjorie Fiske and Joseph T. Klapper), he helped create the field of international communications research (1952). His ideas about the institutionalization of training and research in the social sciences (1962c; 1964b; Glock 1979; Lazarsfeld & Merton 1950) are embodied in

dozens of thriving research organizations around the world. (He was one of the founders of the Center for Advanced Study in the Behavioral Sciences in Stanford, California, but he failed in his efforts to make it a training institution at which junior people would study with senior professors.) His use of the sample survey as a tool for causal analysis (see especially Lazarsfeld & Kendall 1950) helped transform opinion polling into a scientific method; and his development and use of the panel method (see Levenson 1968) has enormously influenced the currently active field of evaluation research (the study of the impact of educational or social reform programs).

In spite of these achievements, Lazarsfeld felt that he was somehow an outsider in America, a marginal person. Why did he feel this way? In his oral history statement, he mentioned such factors as his Jewishness, his foreignness, his heavy accent, and his interest in such a low-status activity as market research, but these reasons are not fully convincing. He believed that his marginality fed on itself: "I think any new situation forces you again to do something marginal, because you are in some degree handicapped by the past up to this moment. In order to overcome the handicap, you have again to do something which is somewhat different. . . . So you get into a new marginal situation, and that forces you, at every moment, into new ones" (1962*b*, p. 171). He lived his life, as he once put it, like a bicycle rider, always compensating so as not to fall off. He left his marginal position at the University of Vienna for marginal positions at Newark, Princeton, and (initially) Columbia; he was a mathematician who never quite believed that life had fully transformed him into a sociologist; he approached every new research topic from a startling new direction; and he took pride in the originality of Bureau studies in contrast to the more traditional research carried out at centers such as those at the universities of Chicago and Michigan. Like an expert skier, who knows that the best snow is generally at the edge of the trail, his genius kept him carefully away from the accepted center of most topics. "But look," he would say with his hand raised, and then proceed to outline a highly original plan of action.

Lazarsfeld's self-perception of marginality led to his conception of his own role in the social sciences: to be on the margin is also to be on the frontier. It can also be argued that his marginality encouraged the intellectual traffic

between ideas and methods that made him a singularly influential figure in the recent history of social research. In a memorial article published in *Le Monde* shortly after Lazarsfeld's death, Raymond Boudon noted that "his work has attained the most noble form of marginality: many of the ideas which he introduced have become so familiar that hardly anyone bothers to attribute their paternity to him" (1976, p. 7).

The search for convergences. One consequence of Lazarsfeld's sense of marginality for his intellectual activity was his never-ending search for convergences between different intellectual traditions—convergences that could serve to enrich both traditions. (His search for convergences was undoubtedly also a result of his being Viennese: syntheses are characteristic intellectual products of Vienna.) His collaboration with the theorist Merton is the most obvious of these convergences. Other convergences that he encouraged were between social sciences: psychology and sociology (1964*a*); mathematics and sociology (1954*b*); anthropology and media research (1952); and sociometry and survey research (Katz & Lazarsfeld 1955). He sought both a convergence and a mutual understanding between the critical sociology of the Frankfurt school (see especially the writings of T. W. Adorno, Jürgen Habermas, and Max Horkheimer) and the dominant positivistic trends in American sociology (1941*a*; 1970*b*); as well as between Marxist sociology and European-American sociology (1970*b*, pp. 94–103).

Other convergences he sought were between the social sciences and the humanities. He used his early studies of radio to build bridges between the social sciences and such fields as literary analysis (Arnheim 1944) and music (Adorno 1941*a*; 1941*b*); he sought to relate the philosophy of science and empirical social research (1962*c*); historical analysis and opinion research (1950*b*; 1957; 1964*b*; 1970*a*); and logic and concept formation (1966). While some of his critics were accusing him of mindless quantification, he was spending time reading and talking with humanists, historians, and philosophers.

Finally, he sought convergences between different research traditions and methods. He made connections between small group research and the use of sample surveys to study interpersonal influence (Katz & Lazarsfeld 1955) and between the use of multiple-choice questions in surveys and so-called open-ended interviewing (1944). His work on concept formation (1966)

and index construction (see Lazarsfeld, Pasanella, & Rosenberg 1972, pp. 9–118) is a monument to interdisciplinary relationships and connections. With Barton, he took a polemic of C. Wright Mills against the decline of “craftsmanship” and developed it into a scheme for studying the man–job relationship (Barton & Lazarsfeld 1955, pp. 339–340). And he encouraged the foremost qualitative researcher of the 1950s—David Riesman—to join him in the interviewing and analysis phases of his study of American social scientists (Lazarsfeld & Thielens 1958; Riesman 1979). A volume of interdisciplinary essays edited by Mirra Komarovsky, *Common Frontiers of the Social Sciences* (1957), was inspired by him and was prepared under his general direction.

The convergence in the social sciences that Lazarsfeld tried hardest to make is that between quantitative and qualitative research. In almost every field in which he worked he tried to relate these two modes (see 1972*b*; Barton & Lazarsfeld 1955; Lazarsfeld & Barton 1951); it was the theme with which he ended his presidential address to the American Sociological Association (1962*c*); the journal *Quality and Quantity* was founded in 1967 under his direct influence (Capecchi 1978); and for all these reasons the *Festschrift* in his memory is entitled *Qualitative and Quantitative Social Research* (Merton, Coleman, & Rossi 1979).

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LEAKEY, L. S. B.

Louis Seymour Bazett Leakey was born in Kabete near Nairobi, Kenya, on August 7, 1903, and died of a heart attack in London on October 1, 1972. In his career he devoted nearly half a century to paleontology, archeology, and anthropology, pioneering in the uncovering of man's past in Africa.

Biographical sketch. Louis Leakey was a son of Kenya. His parents were missionaries who left England in 1901 to set up a station for the Church Missionary Society at Kabete. Leakey was nurtured among Kikuyu neighbors with whom the family was closely associated and whose language they all mastered. With the exception of one year in England, he received most of his schooling at home from his father and tutors. He also learned a great deal from his Kikuyu playmates and mission adherents, and from a Ndorobo hunter who taught him to track patiently, stalk wild animals, and make traps.

The outbreak of World War I prevented

Leakey from resuming schooling in England. He later mused in his autobiography, *White African* (1937), that if the war had not broken out, he would have become a typical product of the English public school system, "and this book would never have been written." The title of his autobiography comes from something said of him by Kikuyu chief Koinange: "We call him the black man with a white face, because he is more of an African than a European, and we regard him as one of ourselves." Years later Leakey wrote: "I have always considered myself more of a Kikuyu than an Englishman in many ways. I still often think in Kikuyu, dream in Kikuyu."

From 1913 to 1919, Leakey's education continued informally at Kabete. In these years he developed a passion for natural history and especially ornithology, though he hoped to become a missionary. In 1915, he received as a gift a book called *Days Before History* by H. R. Hall. This account of late Stone Age people sparked a new enthusiasm in him. He soon found that the area around his home was teeming with evidences of the African Stone Age, and he amassed a large collection of artifacts. He collected animal bones and was encouraged and helped by Arthur Loveridge, curator of the first Kenyan Museum.

After the war Leakey resumed his formal education at Weymouth College in England. In 1922 he gained entry into St. John's College, Cambridge. For part 1 of his tripos he decided to take modern languages. He was proficient in French, but it came as a surprise to that institution when, for his second language, he chose Kikuyu. His tutor, W. A. Crabtree of St. Catharine's College, knew Luganda, another east African language, so Leakey had to instruct his instructor in Kikuyu.

After head injuries sustained in a rugby match in 1923, Leakey was advised to take a year's leave. So it happened that in 1924 he assisted the Canadian paleontologist W. E. Cutler on an expedition to collect dinosaur bones in Tanganyika. This gave Leakey the chance to learn Ki Swahili (the *lingua franca* of east Africa) and to gain experience in the handling of fossils and in the logistics of field work. Leakey's lifelong battle against time is evident in his account of this first expedition.

Leakey returned to Cambridge to take a first in languages and, in May 1926, a first in archeology and anthropology in the second part of

the tripos. Subsequently he led east African archeological research expeditions in 1926–1927, 1928–1929, 1931–1932, and 1934–1935. These ventures in a geographical zone that till then had been virtually unknown in prehistory laid the foundations for much that has later been learned of the archeological and paleontological sequence in east Africa.

In 1929, on his second expedition, he drove from Nairobi to Johannesburg to attend the joint meeting of the British and South African Associations for the Advancement of Science. On the road south he visited Broken Hill (now Kabwe in Zambia) where, eight years earlier, a fossil human cranium had been found. This was the famous *Homo rhodesiensis*, later known as *Homo sapiens rhodesiensis*. Leakey collected stone implements in old river gravels below the Victoria Falls; in caves in the Matopo Hills south of Bulawayo where A. L. Armstrong was excavating; at Hope Fountain where Neville Jones had done much archeological work; and he visited Zimbabwe where Gertrude Caton-Thompson was excavating.

From 1929 to 1934 Leakey held a fellowship at St. John's College, Cambridge, and in 1934 he was Jane Ellen Harrison memorial lecturer there. He was a Leverhulme research fellow from 1933 to 1935. In February 1936 he was Munro lecturer at Edinburgh University; the ten lectures formed the substance of *Stone Age Africa* (1936b). In later years, he was Herbert Spencer lecturer at Oxford (February 10, 1961) and Thomas Huxley lecturer at the University of Birmingham (March 3, 1961). These lectures were published under the title *The Progress and Evolution of Man in Africa* (1961). Subsequently, Leakey was regents' lecturer at the University of California (1963), Silliman lecturer at Yale University (1963/1964), George R. Miller professor at the University of Illinois at Urbana (1965), Andrew R. White professor-at-large of Cornell University (1968), and honorary professor of anatomy at Nairobi University College (1969).

His first marriage was to Henrietta Wilfrida Avern (1928), by whom he had two children. After the dissolution of this marriage, he married Mary Douglas Nicol, an archeologist, and his partner in life and work until his death. They had three sons: Jonathan, who discovered at Olduvai the type specimen of *Homo habilis* and became a snake farmer at Lake Baringo, central Kenya; Richard, on whose shoulders

Leakey's mantle descended and who, as director of the national museum of Kenya, had scientific control of the rich fossil hominid areas east of Lake Turkana, the potential of which he discovered and systematically laid bare; and Philip.

Leakey identified himself with Africa and its peoples. His knowledge of Kikuyu ways led the Kenyan government to enlist his aid during the years of the emergency and the Mau Mau movement. These experiences were reflected in *Mau Mau and the Kikuyu* (1952), *Defeating Mau Mau* (1954), *First Lessons in Kikuyu* (1959), and *Kenya: Contrasts and Problems* (1936a). Following independence of the republic of Kenya in 1963, he became a Kenyan citizen and was a staunch supporter of the state.

During World War II, Leakey was officer-in-charge of Special Branch 6 of the Nairobi Criminal Investigation Department. He took up calligraphy and became a consultant on handwriting. Leakey was a founder trustee of the Kenya National Parks, trustee and executive member of the East African Wildlife Society, and council member of the East African and Uganda Natural History Society.

From 1941 to 1961 he served as honorary curator, then as full-time curator, of the Coryndon Memorial Museum, Nairobi (later the National Museums of Kenya). He initiated, and served as secretary-general of, the Pan-African Congresses on Prehistory and Pleistocene Studies; the first was held in Nairobi in 1947.

In 1962 Leakey set up the Centre for Prehistory and Palaeontology under the trustees of the Kenya National Museums and was its director until 1972. In ten years the center, housed in old buildings and temporary structures, became a base for paleoanthropologists from many areas and an important repository of hominid fossils.

Within his lifetime, Leakey saw his center grow until it was bursting at its seams. After his death, Richard Leakey and the trustees of the National Museums established in Nairobi, as successor to the center, an International Louis Leakey Memorial Institute for African Prehistory. The institute was opened on September 3, 1977, and the first director was Bethwell Alan Ogot, former professor of history at the University of Nairobi.

Leakey's contributions were recognized by many awards and honors including the Cuthbert Peek prize of the Royal Geographical Society (1933); the Andrée medal of the Swedish

Geographical Society (1933); the Henry Stopes memorial medal of the Geologists' Association, London (1962); the Hubbard medal of the National Geographic Society, Washington, jointly with Mary Leakey (1962); the Richard Hopper Day memorial medal of the Philadelphia Academy of Natural Sciences (1964); the Viking Fund medal of the Wenner-Gren Foundation for Anthropological Research (1961-1965); the Haile Selassie award (1968); the Welcome medal of the Royal African Society (1968); the science medal of the Academy of Biological Sciences, Italy (1968); and the Prestwich medal of the Geological Society of London, jointly with Mary Leakey (1969). Honorary doctorates were conferred on him by the Universities of Oxford (1958), California (1963), East Africa (1965), and Guelph (1969). The L. S. B. Leakey Foundation for Research Related to Man's Origin, Behavior, and Survival was established in the United States in 1968; a European chapter of the foundation was opened in London in 1977.

Personality. Three abiding impressions of Leakey are of his singular energy, enthusiasm, and vision. The energy is reflected not only in the tally of his excavations and discoveries, or in the 20-odd books and more than 150 articles he wrote. On safari he walked hurriedly and worked indefatigably and singlemindedly. His enthusiasm repelled some, but inspired many more people to take an interest in man's past and to support his efforts.

Leakey's farsightedness lent direction to his energy. His almost visionary perspective gave purpose to the efforts of the family team. His mind comprehended detail, but could also grasp the larger scheme. His syntheses were not always correct, but then, in this discipline as in others, he who never makes a mistake never makes a major contribution. Romantic and imaginative, he was the kind of scientist who could balance and lend perspective to the pursuit of the detail in the scientific community.

He was sometimes criticized for seeking publicity. Yet his finds and his concepts were always news. If he was quick to publicize his discoveries, it may be argued that this was a necessary means of arousing interest and of eliciting financial support. Funding of his research was always a problem in a field whose practical applications are not readily apparent. This was a prime motive for his exacting lecture tours of America, which undoubtedly shortened his life, but which were necessary fund raisers.

Leakey's public appeal was greatly enhanced by his skill in communication. In speech and in writing he was fluent, impressive, and persuasive.

His strong will, positive personality, and often dogmatic manner brought him into conflict with fellow scientists. Those who judged him hastily missed the substance of the man. Those willing to look beneath the surface dispassionately, to explore his thoughts and claims and not rest with their first expressions, found wisdom, sometimes genius, and often an inspired and inspiring element to be discerned.

Ethnology. Fluent in Ki Swahili and Kikuyu, Leakey was a lifelong observer of the east African peoples with whom he came into close contact: the Kikuyu, Masai, and Luo. In 1929, he served on a government committee that reported on Kikuyu land tenure. From 1937 to 1939 he investigated Kikuyu social organization and culture. His thousand-page report, prepared for the Rhodes trustees and unpublished in Leakey's lifetime, provided a comprehensive record of the ways of the Kikuyu people before and during the period of European influence in the late nineteenth and early twentieth centuries. His manuscript, edited and published posthumously as a three-volume work entitled *The Southern Kikuyu Before 1903* (1977-1978), is an important primary source for social scientists. Younger Kikuyu often complained that Leakey's knowledge of the people and their ways was out of date. Indeed, the knowledge they spoke of was that of older days, before their earlier social and cultural system had been transformed under the influence of colonization.

Primatology. Leakey's zoological interests were not confined to fossilized animals. He believed that much could be learned about the probable behavior of early hominids through the detailed study of living chimpanzees and gorillas in the wild. Accordingly, he initiated studies of free-ranging African primates, especially anthropoid apes. Preliminary studies of the mountain gorilla were undertaken by Rosalie Osborn and Jill Donisthorpe in Uganda, and a definitive study was completed by Dian Fossey in Rwanda. He initiated the long-term researches on chimpanzees conducted by Jane Goodall in Tanzania. At Tigonini near Nairobi, he established a National Primate Research Center.

Prehistoric archeology. Leakey's early researches in east Africa opened up archeological deposits now known to have spanned the entire Pleistocene period. In most of his excavations

after the four early expeditions, Mary Leakey played a large part; often they became her field projects, while he turned his attention elsewhere.

From the earliest expeditions Leakey started to piece together an archeological sequence for east Africa. His "Outline of the Stone Age in Kenya" was published in 1929. It was later expanded into his first book, *The Stone Age Cultures of Kenya Colony* (1931), followed by *The Stone Age Races of Kenya* (1935). He classified the stone industries he had excavated into the hand-axe culture (the African Acheulean), a flake culture, a blade-and-burin culture, and mesolithic and neolithic industries. Later excavations at Kanjera, Olorgesailie, Olduvai, and other sites were to add new evidence on the African Acheulean and to uncover an earlier culture, the Oldowan.

Olduvai Gorge, Tanzania. The gorge was a cornucopia of fossils and implements discovered in 1911 by the entomologist Kattwinkel. Before World War I, Hans Reck of Berlin had collected fossilized bones there; since they included extinct forms, they aroused considerable excitement that was heightened by Reck's discovery of a human skeleton. At first the skeleton was thought to belong to the middle Pleistocene, but it was later shown to be a relatively recent burial into older deposits. The outbreak of war and the transfer of Tanganyika to Britain as a mandated territory put to rest any further plans by Reck to explore the gorge.

On his third east African expedition in 1931-1932, Leakey was accompanied by Reck, as well as by A. T. Hopwood, D. MacInnes, and Vivian Fuchs. Reck had previously found only faunal remains; stone artifacts had eluded him. Shortly after arriving at Olduvai, they found a fine hand-axe. This was the forerunner of thousands excavated in the gorge; so many, in fact, that in *Olduvai Gorge* (1951), Leakey could recognize 11 stages in the evolution of the hand-axe culture.

The excavations in the gorge by Louis and Mary Leakey, their sons, and their assistants, including their African aides, exposed in the walls nearly two million years of human history. Within the lowest layers were sandwiched implements of one of the earliest stone cultures, the Oldowan. Then followed African Acheulean handaxe industries, and finally more advanced cultures. Entombed within the same beds were the fossilized remains of more than 150 different species of animals.

Leakey's 1965 volume, *Olduvai Gorge 1951-1961*, inaugurated a series of monographs to chronicle the new discoveries. The work at Olduvai remained for a number of years in the hands of Mary Leakey, while he gave his full attention to Fort Ternan in Kenya and to the raising of funds abroad.

Hominid evolution. An early chapter on the higher primates was written through Leakey's discoveries. In 1923 Koru in western Kenya had yielded the first known fossil apes from east Africa. On Rusinga Island in Lake Victoria Nyanza, Leakey found a Miocene fossil ape, *Proconsul*, on his 1931 expedition. Other finds of Miocene pongids came from Rusinga, Koru, Songhor, Fort Ternan, and elsewhere. Another early hominoid was found by Leakey on a farm at Fort Ternan. Leakey called it *Kenyapithecus wickeri*, but it became widely accepted that it was not different generically from *Ramapithecus* of India. The work of Leakey, amplified by that of Elwyn Simons and David Pilbeam, suggested that *Ramapithecus* was an early genus of the hominid family. *Ramapithecus* it was thought, might well be the Miocene ancestor of *Australopithecus*.

Among Leakey's earliest discoveries of fossil hominids were the crania of Kanjera and the mandible of Kanam, both discovered in 1932. Leakey shocked the scientific world by claiming that they were respectively middle and lower Pleistocene members of the species *Homo sapiens*, or at least of a form of man close to *Homo sapiens*. They were found on the Gulf of Kavirondo, northeast of Lake Victoria. The Kanam jaw turned out to be more primitive than Leakey had supposed; its features were reminiscent of *Homo erectus*, and it appeared also that it was not as old as the fauna among which it was found. The Kanjera crania, with surprisingly "modern" brows, for long remained an enigma; then Kenneth P. Oakley in 1974 disproved Leakey's claim that they were contemporary with the middle Pleistocene fauna. However, throughout his life, Leakey accepted them as evidence for the early appearance of a sapient kind of man. Indeed, for him, the brutish-looking *Homo erectus* was a side branch of human evolution. So was Neanderthal man, with his flat-topped, heavy-browed, and large skull.

It sometimes seems that for Leakey, sapient man had always, or almost always, looked like modern man. That is why he welcomed the apparently modern-looking Kanam and Kanjera men as direct ancestors, and the miniaturized

though modern-looking *Homo habilis* as another direct ancestor.

Among the fossil mammals recovered at Olduvai were at least three extinct species of hominids. From the earlier layers, between 1.8 and about 1.6 million years before the present, the Leakeys recovered two kinds of synchronic hominids. One was a big-toothed, heavy-jawed, robustly built creature that Mary Leakey discovered in July 1959; he called it *Zinjanthropus* (man of east Africa, for which "Zinj" is an old name). It was an australopithecine, bigger and heavier than the specimens that Robert Broom and John T. Robinson had found in the Transvaal. Today it is known as *Australopithecus boisei*.

The second earlier hominid was called *Homo habilis* by Leakey, P. V. Tobias, and J. R. Napier (1964). It was a bigger-brained, smaller-toothed miniature of a man, who lived in Africa from just over 2 million to about 1.6 million years ago. This species is represented by specimens from Olduvai, Koobi Fora, and Ileret in northern Kenya, Omo in Ethiopia, and probably, too, from an upper member of Sterkfontein in the Transvaal. *Homo erectus*, the third hominid, appeared at Olduvai somewhat later.

Leakey was obsessed with the prospect of finding the earliest true man. Shortly before he left for London on his last journey in September 1972, he was shown a big-brained cranium (1470 man) that his son Richard had found east of Lake Turkana. It had weathered out of a deposit that was at first dated to about 2.9 million years, though more recently it has been shown to be probably up to 1 million years more recent than its earlier claimed age. Leakey was delighted with the discovery, for it seemed to provide further evidence for the very early emergence of the genus *Homo*. Later investigations indicated that the earliest fossils classifiable as members of *Homo* are probably no older than 2.2 or 2.3 million years before the present.

Paleoecology. From his first expedition in 1926-1927, Leakey was impressed by old raised lake levels, well above the present-day levels of neighboring lakes. The gravels deposited on these high-level terraces contained identifiable stone implements. To explain the formerly greater extent of the lakes, Leakey had recourse to past climatic changes. Building on the work of J. W. Gregory, E. Nilsson, and E. J. Wayland, Leakey proposed that the Pleistocene of east Africa be subdivided into two moister pluvial phases, the Kamasian and Gamblian, separated

by a drier, interpluvial phase. The Kamasian was named after the Kamasia Hills west of Lake Baringo and the Gamblian after former lake levels evidenced at Gamble's Cave and Gamble's Drift.

The use of the terms Kamasian and Gamblian, and the addition of a third term, Kageran, based on the Kagera River valley in Uganda, were ratified by the first Pan-African Congress on Prehistory at Nairobi in 1947. The use of the 3 terms was ratified, too, by the 18th International Geological Congress in London in 1948, though it rejected Leakey's proposal that the Kamasian be subdivided into an earlier Kamasian and a later Kanjeran Pluvial (after Kanjera). Nevertheless, Leakey's proposed four-fold subdivision of the Pleistocene came for a time into widespread use. Indeed, the term "Kanjeraan" was formally ratified by the third Pan-African Congress on Prehistory at Livingstone in 1955, apparently under the erroneous impression that "Kanjeraan" had been accepted by the Geological Congress in London! Yet, critics pointed out, these proposed subdivisions were not based on stratigraphic units, such as those on which geologists usually base subdivisions. The concept has been much criticized by geologists (e.g., J. D. Solomon, H. B. S. Cooke, R. Pickering, R. F. Flint, and W. W. Bishop).

In sum, Leakey's proposal to subdivide the Pleistocene into climatic phases was abandoned by most geologists, who were unanimous in recommending that his scheme be replaced by one founded on rock sequences or stratigraphic units. That some stratigraphic units do have a climatic explanation seems undoubted in the light of newer work. Indeed, Leakey's notion of some past climatic changes in the African Plio-Pleistocene has stood the test of time, even though his detailed scheme has not. In another respect, Leakey's claims were heuristically valuable: they provoked a search for newer geochronological and paleoecological techniques and a reappraisal of the concepts of paleoclimatology and paleoecology.

PHILLIP V. TOBIAS

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LEIBHOLZ, GERHARD

Gerhard Leibholz was born in 1901 in Berlin. His life, in many ways interwoven with the fate of twentieth-century Germany, is marked by varied experiences of great significance to his development as scholar, publicist, and jurist. Jewish by heritage he converted in early life to Christianity. Born into a family of industrialists, he was attracted to the life of the mind, going on to earn doctoral degrees in philosophy (Heidelberg University) and law (Berlin University) by the age of 23. Raised in the Wilhelmian period of growing militarism and nationalism, he embraced the principles of democracy and equality. Loyal to the Weimar Republic, he witnessed its destruction from his chair in constitutional law at Göttingen University. Forced to abandon his professorship, he fled to England in 1938, where as a refugee he turned to political and religious subjects, becoming a close personal confidant of George Bell, Lord Bishop of Chichester, and, during his years as a guest lecturer at Oxford University, an intellectual

confreere of Christopher Dawson. Linked to the anti-Nazi resistance movement through his brother-in-law, Dietrich Bonhoeffer—the well-known Protestant theologian imprisoned and executed on Hitler's order for his part in the conspiracy against him—he labored tirelessly but unsuccessfully with Bell to convince the British that it was necessary to cooperate with the resisters inside Germany. Finally, notwithstanding the execution of many close relatives—most in the Bonhoeffer circle—at the hands of the Gestapo, he returned to Germany in 1947 to resume his professorship at Göttingen and to contribute, as a scholar and jurist, to the reconstruction of Germany's new constitutional order.

Leibholz served for twenty years (1951-1971) as judge of the Federal Constitutional Court, the Bonn Republic's highest tribunal. For 26 years he was editor of the *Jahrbuch des öffentlichen Rechts*, a distinguished annual review of constitutional law and development around the world. Leibholz' international reputation was underscored in 1966 with the publication of a massive two volume *Festschrift*, on the occasion of his 65th birthday, that included contributions by noted philosophers, theologians, political scientists, historians, international lawyers, and public servants from 23 nations (Bracher et al. 1966). The *Festschrift* was appropriately subtitled "Modern Constitutionalism and Democracy," the main theme of more than two hundred articles, monographs, and books that Leibholz had written in the course of his career.

Although Leibholz' publications fall neatly into three stages—those of the rising young German jurist and scholar, of the refugee in England, and of the distinguished professor and judge in the Bonn Republic—they are marked by an uncommon unity and continuity of thought. They reflect his philosophical opposition, as a legal theorist, to the prevailing positivism and nominalism of his time; his faith, as a constitutional liberal, in the capacity of institutions to shape the politicolegal order; and his conviction, as a Christian intellectual, that the principles of liberty and equality could not be maintained without their anchorage in the spiritual and religious tradition of the West.

His most important early works were *Die Gleichheit vor dem Gesetz* (1925) and *Das Wesen der Repräsentation* (1929), both ardently debated among the legal and political theorists of the Weimar Republic. Analyzing the historical and structural basis of modern democracy, these studies examined the evolution

of the idea and reality of political equality and traced the transformation of parliamentary democracy from its early modern, indirect forms of representation to what Leibholz perceived as its contemporary plebiscitary form, dominated by mass political parties. What distinguished this theory from the prevailing view of Western, and especially American, political scientists was his contention that "plebiscitary mass party parliamentary democracy"—in Leibholz' view a historically determined and irreversible reality—was a variant of direct (not indirect) democracy, with a clear potential for tyranny. Thus for him the central problem of politics was to keep democracy from becoming totalitarian with the consequent crushing of personal liberty despite equal representation of citizens in parliament. Rejecting Western models of political representation based on interest-group liberalism as well as egalitarian models based on class rule, he proposed a system of responsible party government buttressed by election procedures and constitutional devices calculated to guarantee both limited government and political equality for all citizens.

During his exile in England, Leibholz reflected more deeply on the meaning of constitutional government. Writing a series of articles and lectures on Germany and its rebuilding after the war, he turned also toward the larger issues of power, justice, and world order. From the pages of the *Contemporary Review*, the *Hibbert Journal*, the *Fortnightly*, and the *Dublin Review*, the main vehicles for publication of his work between 1938 and 1948, he sought to develop a political science based on an explicitly Christian understanding of man and history. The influence of Dawson and Reinhold Niebuhr seemed evident in his tracing of the rise of "secular totalitarianism" to the growth of moral relativism and the total separation of religion and politics in the public life of Western nations. The essence of his thought is captured in his 1946 essay "Politics and Natural Law," reprinted in his *Politics and Law* (1965), in which he used the term "theonomic thinking" to describe his approach to political power. Such thinking "requires that political power be brought into the service of God to fulfill His purposes" (p. 20). Although an adherent of natural law, he no longer believed it possible, because of the erosion of common values, "to derive the conception of justice from human reason and morality alone," for "theonomic thinking requires that [the principles of natural

law] be not separated from their dependence on the eternal and sacred justice which has been revealed by God" (p. 21). He argued at length for the reconstruction of politics on a Christian basis and, with Bonhoeffer clearly in mind, for a vigorous reassertion of the political role of the Christian church.

Back in Germany, Leibholz remained firm in these convictions but adopted a more institutional perspective in his defense of constitutional procedures—one that would protect the vitality of the nation and the moral order on which it was presumably based. He became at length the Bonn Republic's most persistent advocate of the juridical democracy created by the Basic Law (the West German Constitution). Controversial on and off the bench, he ardently defended the institutions of judicial review, federalism, the multiparty state, proportional representation, and the Federal Constitutional Court as preeminent guardians of liberty, equality, and political democracy. Many of his early views on political parties and equal representation he helped to write into ruling constitutional law, although he was only partly victorious in his long fight to win the court's approval for the public financing of political parties. Several reviews of his works during this period, while praising him for his judicial pragmatism, tended nonetheless to find large gaps between his juridical theory of the party state and empirical theories of democracy advanced by leading political scientists. But his main intellectual *tour de force* of this period was doubtless *Das Grundgesetz für die Bundesrepublik Deutschland* (Leibholz & Rinck 1966), a comprehensive commentary on the Basic Law, based entirely on the decisions of the Federal Constitutional Court.

DONALD P. KOMMERS

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LEONTIEF, WASSILY

Wassily Leontief, the father of input-output analysis, was born in St. Petersburg (now Leningrad) on August 5, 1906. Educated at the University of Leningrad and the University of Berlin, he received his PH.D. from the latter institution in 1928. Leontief was a research economist at the Institut für Weltwirtschaft at the University of Kiel in 1927/1928. He moved to Nanking the following year to become an economic adviser to the Chinese government, and to the United States in 1931 to join the National Bureau of Economic Research as a research associate. That year he became an instructor in economics at Harvard University, moving through the ranks to achieve a full professorship in 1946. He was appointed Henry Lee professor of economics in 1953. Leontief established the Harvard Economic Research Project (HERP) in 1946 and served as its director until it was closed in 1972. After resigning from Harvard in 1975, he became professor of economics at New York University. Leontief was awarded the French Legion of Honor in 1967, the Bernhard-Harms prize by West Germany in 1970, and the Nobel Prize in economic science in 1973.

Leontief's first publication on input-output

analysis was the article entitled "Quantitative Input-Output Relations in the Economic System of the United States" (1936). This was followed by other journal articles and his first book, *The Structure of the American Economy, 1919-1929* (1941). Leontief's early work attracted relatively little attention. His first article appeared less than a year after the publication of Keynes's *General Theory of Employment, Interest and Money* (1936), and the pressing problem of the day was chronic, long-term unemployment, which appeared endemic to all mature capitalist economies. Keynes's work, addressed to this problem, attracted immediate and widespread attention, not only because of its theoretical innovations but because of its policy implications.

World War II solved the unemployment problem, at least temporarily, and convinced many economists of the correctness of Keynes's views. But it was widely believed that after the war capitalist economies would once again slide into chronic depression unless appropriate policy measures were taken. Concern about postwar full employment led to the first application of input-output analysis to an important policy issue. Input-output tables for 1939 were constructed by the U.S. Bureau of Labor Statistics under Leontief's direction and were used to project full employment patterns to 1950. These were published in the February and March 1947 issues of the *Monthly Labor Review*. Within a decade, input-output analysis—or interindustry economics—had become a major specialty. New developments appeared with increasing frequency, often stimulated by the publication of seminal papers by Leontief and his associates at HERP.

The classical economists—Adam Smith and his immediate successors—had dealt with the economy as a whole. However, for more than a century before the appearance of Keynes's *General Theory*, the focus of neoclassical economics had been on the household and the firm. The dominant method was that of partial equilibrium analysis—that is, the changing of variables one at a time on the assumption that everything else remained constant. When Léon Walras published his *Éléments d'économie politique pure* (1874-1877), it was a major departure from the traditional approach. Walras was concerned with the *interdependence* of economic activities, or with *general equilibrium*. He was a pure theorist, however, and did not attempt to develop an empirical model. Leontief's input-

output model is a lineal descendant of the Walrasian theory of general equilibrium. It represents a quantum jump in the evolution of economic thought, however, because Leontief produced empirical versions of his theoretical model. Today, input-output models are used to analyze a wide variety of economic problems.

Leontief was critical of abstruse mathematical models that had no counterpart in the real world. In his presidential address to the 83rd meeting of the American Economic Association, he criticized the use of models based on highly simplified assumptions. "By the time it comes to interpretation of the substantive *conclusions*," he stated, "the assumptions on which the model have been based are easily forgotten. But it is precisely the empirical validity of these *assumptions* on which the usefulness of the entire exercise depends" (1971, p. 2). Himself a skilled mathematician, and an acknowledged theorist, he preferred to devote his talents and energy to the development of models that could be statistically implemented and applied to the analysis of significant policy issues.

The original input-output model. The first tables constructed by Leontief, for 1919 and 1929, represented a *closed* model. The basic table—now called a *transactions* table—divided the economy into 44 sectors. These were listed along the left-hand side and across the top to form a two-way grid. Each *row* recorded sales from the sector at the left to the sectors at the top. Each *column* recorded inputs purchased by the sector at the top from sectors at the left. The sum of each row was that sector's *total gross output*; the sum of each column was its *total gross outlays*.

From the transactions table Leontief derived a second grid of *technical coefficients*. Each column shows how much is required by the sector at the top from sectors at the left to produce one dollar's worth of its own output. The coefficients were assumed to be stable. Finally, these technical coefficients were inserted in a system of equations that were solved simultaneously to obtain what is now called a *Leontief inverse*. This table, which completes the basic set comprising a static input-output model, shows the direct and indirect requirements each sector needs to produce an *additional* dollar's worth of output. Because the equations in Leontief's original model were solved by desk calculators, the 44-order tables were reduced to 10 rows and columns for computation.

The 1939 table, constructed by the Bureau of

Labor Statistics under Leontief's direction, was larger than his original tables. It had 96 *processing industries*—that is, industries for which intermediate transactions were recorded as described above. A much larger table, for 1947, with data collected for 500 industries (later aggregated to 190) was also constructed by the Bureau of Labor Statistics. This table was completed in 1952. By that time digital computers were available to solve the system of equations, and the era of applied input-output analysis had arrived.

The 1947 table represented a new departure. It described an *open*, static input-output system. This system distinguishes between interindustry sales and *final sales* (to households, government, and export). There are corresponding rows showing purchases of *primary inputs* (from households and imports, as well as payments to government). The final sales columns are referred to collectively as *final demand*, the primary inputs as *value added*. Changes in final demand are determined by forces "outside" the system. Once these are given, the equations are solved again to show how each sector is affected by any specified change in final demand—in one, two, or any number of sectors. In practice, changes in final demand are estimated by conventional statistical forecasting procedures.

Static, open input-output models are used to make detailed forecasts and to calculate various types of *multipliers*. The basic model yields *output multipliers*. If households are treated as an "industry," however, the model can generate *income multipliers*. It is also possible to calculate labor coefficients from employment-output functions and, from these, to derive sets of *employment multipliers*. These various multipliers show total changes in output, income, and employment, by industry, after the economy has reached a state of equilibrium following specified changes in final demand.

Later innovations. After the 1947 tables were published (in 1952), there was a hiatus during which the federal government was not involved in preparing input-output tables. Later, an Interindustry Economics Division was established in the U.S. Department of Commerce, which publishes sets of tables at approximate five-year intervals. The most recent tables were for 1967. During the hiatus, Leontief and a growing number of research associates were working on the Harvard Research Project. The result was a major book by Leontief and nine associates, *Studies in the Structure of the American Econ-*

omy (1953). It includes a chapter by Leontief discussing changes in technical coefficients over time. Early input-output studies had been criticized because they assumed constant coefficients. This was not a necessary assumption, however, and some later models allowed coefficients to vary as they were affected by changing technology, relative prices, and in some cases by changing trade patterns.

Two major advances were made by Leontief in the new volume. One was the development of dynamic input-output theory, the second a lengthy exposition of interregional theory. A dynamic input-output model is more complex than the static model. It includes a matrix of *capital coefficients* relating expansion investment requirements to changes in each sector's capacity. The static model is represented by a system of ordinary linear equations. But a dynamic model consists of a system of linear difference equations—or, in Leontief's original form, a system of linear differential equations. Systems of difference or differential equations may or may not be stable. After the publication of Leontief's theoretical paper, there was some discussion in the literature about the feasibility of constructing operational dynamic models. Such models have been constructed, however. They have proved to be stable and have been used for a variety of analytical purposes. Dynamic models extend the application of input-output to a number of new uses, including analysis and simulation of the processes of economic growth and development.

Leontief's paper on interregional theory (printed in Leontief et al. 1953), together with a companion piece on regional input-output analysis by Walter Isard (1953), had an immediate and widespread impact on regional economics and the newly emerging discipline of regional science. Within two decades, scores of input-output tables had been constructed for cities, states, and regions. Regional and interregional input-output analysis has become a major subdivision of interindustry economics, with a burgeoning literature of its own.

Other evidence of progress in input-output analysis is found in the proceedings of a series of international input-output conferences. The 1st, held in the Netherlands in 1950, was attended by only 15 participants. The 6th, with 320 participants, took place in Vienna in 1974. Their respective proceedings show a gradual shift from an early preoccupation with definitions and measurement to the development of

sophisticated theoretical models, and the application of input-output techniques to a wide variety of social and economic problems. Among other contributions to these conferences was a paper by Leontief demonstrating the use of an input-output model for the economic analysis of air pollution abatement.

The most ambitious interindustry study conducted to date was made by Leontief and several associates for the United Nations. The results were presented in *The Future of the World Economy* (Leontief et al. 1977). The nations of the world were divided into 15 regions and economic activity into 45 sectors; thus the global model belongs to the family of interregional models. Among the objectives of the study was estimating the economic and environmental impacts of alternative development strategies on the world economy to the year two thousand.

Conventional economics, whether neoclassical or Keynesian, presupposes a market economy. Interindustry economics, however, is not constrained in any way. It can be used for the analysis of any type of economic system from laissez-faire capitalism to a controlled, centrally planned economy.

Not all planning is centralized. France, for example, has experimented with *indicative* (as opposed to coercive) planning. Production goals needed to reach the target levels of output were set by a committee representing business, labor, and government. An input-output model was used to calculate the inputs, including investment requirements, that were needed to reach the stated goals. Some American economists have argued that the next stage in the development of capitalism will require comprehensive national planning. Leontief has been a leader of this group. Input-output analysis would play a central role in such planning, which would be noncoercive. In Leontief's words: "Choice among alternative scenarios is the cue to rational national economic planning" (1976, p. 8).

For many years input-output analysis was considered to be an esoteric subject of limited interest to most economists. Today, interindustry economics is an integral part of graduate training in economics, generally as part of econometrics, where mathematical and statistical analysis are conjoined with economic theory. Increasingly, the rudiments of input-output analysis are being taught at the undergraduate level.

Interindustry economics is anything but static. Input-output models are being merged with

other types of econometric models. New techniques for estimating coefficients appear; some are tried and discarded, others are modified by additional research findings. New theoretical developments and applications are regularly reported in the literature. It is a vibrant and expanding branch of economics. Leontief now ranks with Adam Smith, Keynes, and other leading economists whose ideas have left a permanent imprint on the history of economic thought.

WILLIAM H. MIERNYK

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- 1936 Quantitative Input-Output Relations in the Economic System of the United States. *Review of Economics and Statistics* 18:105-125.
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- 1966 *Input-Output Economics*. New York: Oxford Univ. Press. → Contains 11 essays by the author published over a period of 20 years.
- 1966-1977 *Essays in Economics*. 2 vols. White Plains, N.Y.: M. E. Sharpe. → Volume 1 was first published in 1966 with the subtitle *Theories and Theorizing* and was reprinted in 1977 as volume 1 of Leontief's *Essays*. Volume 2 was subtitled *Theories, Facts and Policies* and was first published in 1977.
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WALRAS, LÉON (1874-1877) 1954 *Elements of Pure Economics: Or, the Theory of Social Wealth*. Translated by William Jaffé. Homewood, Ill.: Irwin; London: Allen & Unwin. → First published as *Éléments d'économie politique pure*. The definitive French edition appeared in 1926.

LERNER, ABBA P.

Abba P. Lerner was born in Bessarabia in 1903. Shortly after his birth his family immigrated to England and he grew up in London. After a series of false starts to a career, he became a student at the London School of Economics (L.S.E.) in 1929. His ability was recognized by J. R. Hicks and was called to the attention of Lionel Robbins. Lerner entered the L.S.E. as a convinced socialist and was to become a leading, and early, exponent of Keynesian economics. Yet it was Robbins, an advocate of market mechanisms based on private property, and, in the 1930s, one of Keynes's most able opponents, whom Lerner later credited with having "made me an economist." Robbins, still in his early thirties, though skeptical of the role of empirical testing in economics, was convinced of the social relevance of rigorous deductive economic theory. During the 1930s his department attracted as staff and students an array of extraordinarily talented young economists: Friedrich A. von Hayek, Hicks, Roy Allen, Marion Bowley, Victor Edelberg, Nicholas Kaldor, and Tibor Scitovsky, among many others, not to mention Lerner himself. Lerner's own enthusiasm for economic theory, his concern that it be applied to problems of social importance, and his passionate advocacy of individual freedom, clearly mark him as Robbins' student, as indeed do his lucid exposition and the absence of any predominantly quantitative work in his contributions.

Lerner has made important contributions to the pure theory of international trade, welfare economics, the economics of socialism, theoretical macroeconomics, and the policy application of Keynesian economics. His earliest papers, written within three years of his taking up economics, were exercises in the application of geometric techniques, which were then at the technical frontier of economics, to problems in the pure theory of international trade. What might have become the most influential of these papers went unpublished for 19 years. In it Lerner derived a set of conditions under which,

in the absence of factor mobility, the existence of free trade in outputs between two countries would serve to equalize factor rewards between them. It was only after Paul A. Samuelson's publication of similar results in 1948 that Robbins rediscovered Lerner's paper in his files and in 1952 had it published in *Economica*.

The most typical of Lerner's early papers is his "Concept of Monopoly and the Measurement of Monopoly Power" which appeared in the 1934 *Review of Economic Studies*, a journal that he helped found, and also, with Ursula Webb (Lady Hicks), edited between 1933 and 1937. His proposal that monopoly be measured not in terms of some purely descriptive statistic such as a concentration ratio, but rather in terms of the effect of its existence on economic welfare, is notable in two respects. First it contains the germ of the idea that a policy or institution should be judged by its effect on the well-being of individuals, a notion that was later explicitly to underpin his writings on "functional finance." Second, Lerner explained that monopoly maintained the market price of a good (the amount that consumers were just willing to pay for a marginal unit of output and hence a measure of the social benefit conferred by that unit) above its marginal cost of production (this in its turn being a measure of the benefit being sacrificed elsewhere to obtain that marginal unit). Competition on the other hand forced output to expand until marginal costs and benefits were equated, with a net gain in consumer satisfaction arising from this expansion. For Lerner, the amount by which a monopolist sets his price above marginal cost was thus an index of the social harm that he did and hence an appropriate measure of his power. This was a particular application of the general principle that deviations between marginal cost and marginal benefit lower economic welfare, a concept developed extensively in Lerner's most important book, *The Economics of Control* (1944).

This work had first been proposed in 1932 as a PH.D thesis. An early version of it did earn Lerner his doctorate, but the book itself was not published until 1944, seven years after Lerner's first arrival in the United States from Britain. Lerner at first intended, as a contribution to the economics of socialism, to develop the rules whereby the managers of collectively owned enterprises could act so as to ensure that scarce resources would be allocated to promote the general economic welfare of the community. In the course of developing his analysis, Lerner be-

came involved, along with Oskar Lange and H. D. Dickenson among others, in a debate with Ludwig von Mises and von Hayek about the very possibility that a collectivist society could organize its economic life coherently. The upshot of the debate was that the possibility did exist, provided that managers of socialist enterprises were instructed to adopt the principles of marginal cost pricing—principles that profit-maximizing competitive firms under capitalism, would automatically adopt—a solution that von Hayek remarked would probably not appeal to socialists who were not also trained economists.

Lerner's involvement in this debate convinced him that a system based either on private or on collective ownership of resources could conceivably produce a coherent solution to the fundamental problem of scarcity. The performance of National Socialism in Germany and of communism in the Soviet Union led him to fear the totalitarian potential of collectivism. Thus, by 1944, Lerner's socialism stressed the ends of individual freedom and democracy rather than the means of collective ownership of property. Lerner had come to conceive of the "economics of control" as a set of general principles, based on the equation of marginal cost and benefit, that could be applied by policy makers to improving the performance of any economy, collectivist, capitalist, or mixed.

The bulk of *The Economics of Control* provides a thorough working out of the principles whereby a Pareto optimal allocation of resources, under which no one can be made better off without someone being made worse off, can be achieved. It is notable for the stress that it places on the ways in which a laissez-faire economy can fail to achieve such an optimum and on the design of state intervention to help to solve these problems. Such matters had of course been analyzed by Arthur Pigou in his *Economics of Welfare* (1920), but he had used Marshallian partial equilibrium techniques. Lerner used the more powerful general equilibrium tools that had been developed on the Continent and brought into English economics by Robbins, Hayek, Hicks, and their associates. Lerner's 1944 analysis pays little attention to the relationship between the extent to which unregulated market mechanisms can achieve an optimal allocation of resources and the structure of property rights and legal obligations prevailing at any time. This disregard is surprising because pioneering work on these matters was done at the L.S.E. in the

1930s by Arnold Plant and his associates. The omission was made good in Lerner's later writings on consumer sovereignty, where the influence of Ronald Coase, one of Plant's students, is evident. It is symptomatic nevertheless of what some regard as an undue neglect on Lerner's part of the political and institutional background against which economic activity takes place and economic policy is made. Although *The Economics of Control* has come to be thought of mainly as a contribution to theoretical welfare economics, Lerner himself was inclined to regard it as a guide to practical policymaking.

In addition to its exposition of the principles of marginal cost pricing, *The Economics of Control* is also notable for an impressionistic argument, part probabilistic and part Benthamite, to the effect that an equal distribution of income is likely to maximize social welfare. Furthermore, its final chapters contain one of Lerner's several accounts of his own particular version of Keynesian macroeconomics. Lerner seems to have been attracted to Keynesian economics during a term spent at Cambridge University in 1935. Richard Kahn and Joan Robinson played a role in convincing him of the importance of Keynes's then unpublished work, but he gives the major credit for this to Robert Bryce, Alec Cairncross, and Lorie Tarshis, who were then graduate students at Cambridge. Lerner wrote an early (1936a) review of Keynes's *The General Theory* (1936) and his theoretical papers on Keynesian economics deal with the distinction between a stock of capital and a flow of investment in models of the influence of interest rate on the level of aggregate demand; they also elaborate the insight that money wage rigidity, inasmuch as it promotes price level predictability, is a desirable feature in a monetary economy. However, the policy doctrine of "functional finance" is undoubtedly Lerner's main contribution to Keynesian economics.

"Keynesian economics" is often thought of as a set of propositions concerning the use of fiscal and monetary policy for the pursuit of high employment and price stability, but such propositions derive only indirectly from Keynes's *General Theory*. They were, in fact, developed by Lerner and also by Alvin Hansen. According to Lerner the principles of "sound finance," under which the government attempts to avoid increasing its debt by matching its expenditures with tax revenue, derive from a false analogy with behavior appropriate to an individual

firm or household and serve no useful social function. Internally held national debt is both an asset and a liability to the community and hence represents no burden to it. It is only debt held overseas that is burdensome to a country in the way that private debt is burdensome to an individual. However, the scale of government expenditure, and the extent to which it is financed by taxes, borrowing, or money creation, do influence the community's welfare through their influence on income, employment, and prices. The basic principle of "functional finance," then, is that fiscal and monetary policy are to be judged solely in terms of their effects on these variables. Because Lerner believed that the automatic forces tending to bring any actual economy to full employment and price stability are weak, he held that fiscal and monetary policies should be actively and continuously deployed toward the achievement of these goals.

Unlike most American Keynesian economists, Lerner explicitly recognized that there could be a conflict between achieving domestic goals and maintaining balance-of-payments equilibrium under fixed exchange rates. Hence, like Milton Friedman, whose views on stabilization policy were otherwise diametrically opposed to his, he advocated exchange rate flexibility. Moreover, Lerner recognized the interdependence of fiscal and monetary policy that arises from the fact that expenditure not financed by taxation must be met by borrowing or money creation; and when he dealt with inflation was always explicit that money creation was a necessary condition for it to persist. His stress on these matters, and the absence from his work of any touch of the pessimistic "secular stagnation" thesis, sharply differentiate his contribution to Keynesian economics from that of Hansen.

It was in the late 1940s that Lerner became concerned with price stability problems. He paid considerable attention to them in *The Economics of Employment* (1951), in which he noted the theoretical possibility that inflation, once under way, could generate expectations that would cause it to accelerate; thus to an important extent he anticipated the "accelerationist hypothesis" of Friedman and Edmund S. Phelps. However, he doubted the empirical importance of this effect. Moreover, he did not associate the onset of accelerating inflation with the achievement of a particular "natural" unemployment rate. Rather, he conceived of two levels of "full employment equilibrium"—a "high" level that could, in the absence of monopoly power and other in-

stitutional rigidities in output and labor markets, be achieved without generating inflation; and a "low" level, above which inflation would in fact set in because of such rigidities. For Lerner, for whom inflation that did not accelerate was not in any event a major problem, this "low" level of full employment involved an intolerably large number of unemployed: six million was his 1951 guess for the United States. Hence at that early date he advocated the use of wage and price controls, perhaps enforced by tax incentives, to counteract what he later named the "sellers' inflation" that would ensue if functional finance were used, as he believed it should be, to achieve a "high" level full employment target of about two million.

Much subsequent Keynesian discussion, both academic and practical, of anti-inflation policy, clearly had its roots in Lerner's pioneering work. Moreover, he continued to study these matters, and during the 1970s inflation, when proposals for tax-enforced incomes policies became popular, he was again involved in debates about them. Becoming skeptical of their feasibility, he put forward an alternative plan in 1978. The government would issue to firms a limited number of marketable vouchers permitting a certain rate of money wage increase during a specific period. Firms wishing to grant increases beyond their initial allocation would be free to buy vouchers from others, who would thereby give up the right to increase wages. Whatever view one takes of the administrative feasibility of this plan, there can be no question about its ingenuity.

Throughout his career there have been detractors who have found Lerner naive about the political practicalities of implementing economic policy, but his defenders would argue that his only fault, if indeed it is a fault, has been optimism about the power of rational argument to influence politicians and bureaucrats to act in socially desirable ways. However, there has always been complete agreement about his analytical work: it has consistently been of the highest quality. Though many an economics department is decorated with one or more of Lerner's elegant mobile sculptures, a legacy of his stay there, he never settled long enough in one university to create a readily identifiable group of disciples to develop and propagate his ideas (Milton Friedman at the University of Chicago and Paul A. Samuelson at the Massachusetts Institute of Technology come to mind as examples). It is all the more a tribute to the quality of his work then, that he has nevertheless

had an unquestionable influence on the development of economics during the middle decades of the twentieth century.

DAVID LAIDLER

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LÉVI-STRAUSS, CLAUDE

Claude Lévi-Strauss was born on November 28, 1908, in Brussels, where his parents were residing temporarily; his father, a painter, was then completing a series of portraits. The family returned to Paris two months after his birth. Lévi-Strauss lived in Paris continuously until 1935, with the exception of the World War I years, which he spent in Versailles with his mother and other relatives in the home of his maternal grandfather, who was the city's chief rabbi.

The roots of the Lévi-Strausses were Alsatian (deriving from Strasbourg and its vicinity). The men were mostly rabbis or merchants. Isaac Strauss, Lévi-Strauss' great-grandfather, was an orchestra conductor and composer, who had moved to Paris at the beginning of the nineteenth century. Despite the prominence of rabbis in the family, Lévi-Strauss and his parents were never believers.

Lévi-Strauss took, simultaneously, a licentiate in law and one in philosophy, and became *agrégé de philosophie* in 1931. During his graduate studies he was militant in the Socialist party (S.F.I.O.) and he wrote his first publication, *Gracchus Babeuf et le communisme* (1928).

After his military service, Lévi-Strauss taught lycée for a little more than two years. In 1934 he was appointed lecturer in sociology at the University of São Paulo, Brazil. There, the time not taken up by his teaching was spent on field work among the Amerindians of Brazil. A first ethnographic contribution (1936) and an exhibition in the Musée de l'Homme (Paris) of artifacts he had collected won him recognition as an ethnologist by the French masters of the time—Marcel Mauss, Paul Rivet, Lucien Lévy-Bruhl. As a result, he was able to obtain a research grant for further field work in Brazil in 1938–1939. Drafted in 1939, Lévi-Strauss left for New York after the Nazi occupation of France. There, he joined the *forces françaises libres* while teaching at the École libre des Hautes Études. His New York associates included the surrealists André Breton and Max Ernst and his colleague, the linguist Roman

Jakobson, with whom he established a lasting friendship resulting in a deep reciprocal influence. Lévi-Strauss returned to Paris briefly in 1944 before being sent back to New York as cultural counsellor of France.

In 1943 Lévi-Strauss had begun what became his first monumental work, *Les structures élémentaires de la parenté* (1949). He had by then already published the “charter” paper of structuralism, “L'Analyse structurale en linguistique et en anthropologie” (1945, printed in 1958, chapter 2) and the Nambikwara ethnography (1948). Back in Paris, he was appointed assistant director of the Musée de l'Homme, a post he held until 1950 when he took a chair of directeur d'études at the prestigious École Pratique des Hautes Études. He published the widely known and acclaimed *Tristes tropiques* in 1955 and the important collection of essays, *Anthropologie structurale*, in 1958. Along with *Les structures élémentaires*, these books made him the “father” of structuralism in anthropology.

Lévi-Strauss had planned to continue his analysis of kinship—he had been preparing a second volume, on the complex structures—when he was appointed at the École. Since the chair was of religions of preliterate peoples, he shifted the main focus of his research to mythology and religion (1958, chapters 9–12: see especially chapter 11, the pathbreaking paper “La structure des mythes”).

In 1959 the chair of social anthropology was created for him at the highest French academic institution, the Collège de France. Shortly thereafter, he completed what could be seen as the second phase of his work with the publication of *Le totémisme aujourd'hui* (1962b) and *La pensée sauvage* (1962a). The onset of a third phase, which combined and broadened the two previous ones, was marked with the publication in 1964 of *Le cru et le cuit*, the first volume of a series of four (*Mythologiques*, 1964–1971) devoted to the analysis of almost one thousand Amerindian myths. *Anthropologie structurale deux* (1973) appeared the year he became a member of the Académie française. And *Mythologiques* was extended in *La voie des masques* (1975), a two-volume analysis of Northwest Pacific Coast Amerindian art and ritual; the second edition (1979) will include ground covered in later articles.

Lévi-Strauss holds several doctorates *honoris causa*; he was awarded the Huxley memorial medal (Royal Anthropological Institute of Great Britain and Ireland) in 1965, the international

gold medal and Viking Fund prize in 1966, the golden medal of the Centre National de la Recherche Scientifique in 1967 and of the president of Italy in 1971, and the Erasmus prize in 1973. In 1954, he married Monique, who has authored *Shelia Hicks* (1973), a book on the works of an American artist living in France; she also contributes to anthropological translations.

Intellectual orientation. The most important early influences on Lévi-Strauss' thought were the science of geology and the writings of Sigmund Freud and Karl Marx:

Marxism seemed to me to follow the same procedure as geology and as psychoanalysis as defined by its founder: all three demonstrate that understanding consists in reducing a type of reality to another one; that true reality is never the most evident; and that the nature of what is true transpires already in its very evasiveness. It is the same problem in all cases, that of the relationship between the sensory and the rational, and the sought-for goal is the same: a kind of *superrationalism* aiming at integrating without loss the former into the latter. (1955, p. 44)

This epistemological orientation was more satisfactory to him than that suggested by the stream of academic training at the time of his graduate studies, which he characterized as follows: "In the last analysis, the point was not so much to discover the true and the false as to understand how mankind had little by little overcome contradictions" (1955, p. 38). This position he repudiated: "Knowledge . . . consists in the selection of *true* aspects, i.e., those that coincide with the properties of my thought," not in a neo-Kantian way but "because my thought is itself an object. Being 'of this world', it partakes of the same nature" (1955, p. 42; see also 1964-1971, vol. 4, p. 621).

Despite exposure in college to the writings of the French school, Lévi-Strauss had remained impervious to ethnology. So his reading of Robert H. Lowie's *Primitive Society* (1920) "in 1933 or 1934" was a revelation. He found in that work a "lived experience of indigenous societies whose meaning had been preserved by the participation of the observer" in contrast to the French "metamorphosis into metaphysical concepts" of data coming from published sources (1955, pp. 45-46). Thus, Lowie, Franz Boas, A. L. Kroeber were the most important influences on Lévi-Strauss' formative years as anthropologist.

Later, through the guidance of Jakobson, Lévi-Strauss was influenced by Prague and Saussurian linguistics (1958, chapter 2). But since Ferdinand de Saussure's structuralism derived essentially from the principles laid out by the members of *L'année sociologique* (especially Durkheim and Mauss), the French sociologists did mark Lévi-Strauss' thought, albeit indirectly.

Among social anthropologists, the British A. R. Radcliffe-Brown is probably the one with whom Lévi-Strauss has argued most extensively, both privately and in print, on several basic issues (1949; 1958; 1962*a*). Radcliffe-Brown's intelligent functionalism captivated, irritated, and stimulated Lévi-Strauss' own reflection. Among modern colleagues, Meyer Fortes, Edmund R. Leach, and V. W. Turner were most challenging to him.

Lévi-Strauss has characterized his own cognitive pattern as "neolithic"; by his own description (1955, p. 39), he has practiced the "slash and burn" method of cultivation of ideas. Clearing "sometimes unexplored grounds" he "grows and hastily harvests a few crops, leaving behind a waste land." Actually, this waste land is in the last analysis eschatological. Referring back to chapter 7 of *Tristes tropiques* (1955), where he used a sunset dissolving into darkness as a "model of the facts I was to study later on," Lévi-Strauss wrote in *L'homme nu* (1964-1971, vol. 4, p. 620): "Is not this image [of a sunset] that of mankind itself and beyond mankind of all forms of life . . . which evolution develops and diversifies in order that they abolish themselves and that, in the end, of nature, of life, of man, of all those subtle and refined deeds that are languages, social institutions, customs, art masterpieces, and myth, after their last fireworks, nothing remains?"

Lévi-Strauss' structuralism. In the introduction to *Mythologiques: Le cru et le cuit*, Lévi-Strauss stated: "We are not therefore claiming to show how men think the myths, but rather how the myths think themselves out in men and without men's knowledge" (*Yale French Studies* translation, quoted in Leach 1970, p. 51). Eleven years later, a similar proposition formed the conclusions of *La voie des masques* (1975, vol. 2, p. 124): "Wanting to be apart, the artist is lulled by an illusion that may be fruitful, but the privilege that he endows himself with, is unreal. When he believes that his expression is spontaneous and his work original, he echoes other creative artists past or present, actual or potential. Whether one knows it or ignores it,

one never moves alone along the path of creation."

The major components of Lévi-Strauss' structuralism are culture-specific semantic systems and the universal rules that underlie them. These have structural "inertia," permanence over time, and resistance to change. They exhibit themselves through the people who are largely unconscious carriers. Myth makers, poets, and other creators are those in whose works "collective representations" emerge and are remolded to neutralize history—that is, to reduce the randomness of contemporary events to culturally acceptable patterns (1962*b*; 1964–1971). Social units derive their identities from, and can communicate between themselves through, common paradigmatic rules that enable them to build taxonomies: to agree on resemblances, on resemblances between resemblances, and on resemblances between differences (1962*b*).

In other words, cultures are conditioning mechanisms that structure context-bound human minds according to a syntax of compatibilities (e.g., French cuisine and frogs' legs) and incompatibilities (e.g., taboos on eating dog flesh or on marrying one's own sibling), which it is the task of the analyst to map out and interpret.

Such syntaxes and semantic structures are never directly perceivable. They manifest themselves in all sorts of more or less homomorphic institutions and customs—kinship, marriage, Father Christmas, astronomy, music, cuisine, science fiction, anthropology itself, etc. Lévi-Strauss tackles these diverse dimensions of human culture. He raises such questions as: Does the syntax of encoding operations vary in kind or only in degree from society to society? Does the same set of logical rules control both the "scientific" and the "savage" mind? What are the parameters within which societies are able, and without which they fail, to reproduce themselves? If there are fundamental structures of mankind, what are the models that can best represent them? Are not these structures, in the final analysis, fragile systems of loose oppositions generating dialectic and creative disequilibria that drive the slow growth of cultural helices patterning the fates of human groups until, their ideologies or infrastructures becoming exhausted, they collapse and become mental fossils stored in museums and in history books?

Lévi-Strauss investigates these issues in his continuing research. The structure of his quest has remained consistent from its inception until

today. The principle of inverted symmetry seems to be the dialectic operator constituting the elementary structure of mankind. It was the fundamental proposition of *Les structures élémentaires* to explain the passage from nature to culture, and it recurs in *La pensée sauvage*, in *Mythologiques*, in *La voie des masques*.

For example, mankind is the only animal species to have mastered mating relationships and fire (1949; 1964–1971, vols. 1, 4). Cooking and the management of reproduction forces transformed nature—the raw, the incestuous, the wild—into culture by modifying eating patterns and by defining marriage rules. Accordingly, the incest taboo must be understood first not as a prohibition but rather as a prescription—that of "marrying out." Because of it, humans form and consolidate alliances between groups that are broader than isolated cells. This foundation of political systems rests on the dialectics of inverted symmetry. For a man, the incest taboo means in effect: "The woman I have (my daughter or my sister) I cannot keep for myself (as a wife). The woman he (another man) has, he cannot keep either. Thus we must enter into an exchange, elementary though it be (e.g., sister exchange). This exchange forces us into a partnership." In this way, two symmetrical, unacceptable and inverse propositions (to have a woman and not to keep her—that is, to have a woman yet at the same time not to have her) become complementary when combined with another similar set of propositions, so as to form a structure stronger than each previously isolated one (one does not keep one's own women in order to have women).

Different strategies are available to and used by different societies. Some will opt for a safe, minimal-risk exchange strategy so that immediate return is insured, A giving to B and receiving from B. This strategy Lévi-Strauss has called "restricted exchange." It results in small, relatively stable, low-risk and low-return political structures. Or partners may be bolder. Their network will have at least three nodes. A gives to B, who gives to C, C to N ($N \neq B$, $N = / \neq A$), and C, or N, to A. This "generalized exchange" generates higher-risk and higher-return, complex, and large political units.

Restricted and generalized exchange characterize nontechnological societies, but strategies are more blind in complex societies. We "give away" our daughters and sisters—that is, we comply with the incest rule—but without the assurance, other than statistical, that we will get an-

other woman in return. We can only trust to the demographic pool that our move will be reciprocated.

In this light, our own marriage patterns become interpretable within a broader framework. So do many of our other institutions and even everyday trivial activities—for example, such a simple gesture as the striking of a match, in which “are symbolically arbitrated the most pregnant primeval oppositions, between heaven and earth in the physical world, between man and woman in the natural world, between affines in the world of society” (1964–1971, vol. 4, p. 558).

Responses to Lévi-Strauss’ structuralism. Lévi-Strauss’ books have been translated into all the major languages and have generated a bibliography of commentary in articles and books. According to a compilation by François Lapointe of the Tuskegee Institute, there are more than one thousand pieces of writing devoted to Lévi-Strauss’ works. He has been acclaimed by Amerindians of the Northwest Pacific Coast. The intelligentsia of literate countries acknowledge his influence in the definition of a philosophy for modern times. Numerous articles in news magazines (*Time*, *Newsweek*, etc.) and newspapers; a poem by Robert Lowell (“Lévi-Strauss in London”); a movement in a symphony by Berio, the lyrics of which contain excerpts from *Le cru et le cuit*; even a reference in Agatha Christie’s *Passenger to Frankfurt*, which takes it for granted that Lévi-Strauss is a prophet of youth. These instances and many similar ones indicate that Lévi-Strauss has become a culture hero.

As would be expected, antihero reactions are not lacking (for a compendium of both positive and negative reactions up to 1970, see Hayes & Hayes 1970). But Lévi-Strauss’ sternest critics subscribe to the words of one of the best among them: “The structure of relations which can be discovered by analysing materials drawn from any one culture is an algebraic transformation of other possible structures belonging to a common set and this common set constitutes a pattern which reflects an attribute of the mechanism of all human brains. It is a grand conception; whether it is a useful one may be a matter of opinion” (Leach 1970, p. 53).

Lévi-Strauss has been said to be, along with Freud and Marx, one of the major architects of the thought of our times (Steiner 1972). After Freud’s microscopic investigations of individual history and Marx’s macroscopic investigations of

social history, Lévi-Strauss reminds us that history, like anthropology itself and all other social sciences, is a mythopoeic operation that tries to make sense of mankind.

PIERRE MARANDA

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LEWIS, OSCAR

Oscar Lewis (1914–1970), at his death professor of anthropology at the University of Illinois in Urbana, was renowned for his studies of poverty in Mexican and Puerto Rican families, and for his often misunderstood and politically significant concept of a "culture of poverty." He died suddenly in New York City of a heart attack on December 16, 1970, leaving for posthumous publication a wealth of similar material on the people of postrevolutionary Cuba. Not quite 56, he had also achieved among anthropologists recognition for his earlier studies of cultural change; for important innovations in field work methods; and for the richness of his descriptive data on communities, families, and newly burgeoning studies of urban life.

Lewis was born in New York City on December 25, 1914, to a family recently arrived from Poland with three daughters and a son. He was the only child born in America. His father, a rabbi and cantor, soon moved the family for reasons of health to a small farm in Ferndale in the New York Catskill Mountains, where he served an Orthodox congregation. Here Lewis grew up, familiar with both rural and city life. He enjoyed sports and was also a devotee of music and the performing arts. He had a tenor (later a baritone) voice of operatic quality, and often expressed regret that his parents had dissuaded him from following a musical career.

When he graduated from high school in 1932, he entered the City College of New York, where he majored in history but his teachers included Abraham Edel and the philosopher Morris R. Cohen. These were the deepest years of the economic depression, and while pursuing his studies—then and as a graduate student—he

learned firsthand about poverty. In 1936, having earned his B.S.S., he entered Columbia University's department of history.

While an undergraduate, he had met Ruth Maslow, a student of psychology and education at Brooklyn College, and they were married in 1937. She took an M.A. in special education at Columbia's Teachers College, and spent three years teaching the handicapped while her husband completed his doctorate. They went together on his first field trip in Canada, and their careers thereafter were professionally joined. After she was widowed, Ruth Lewis undertook the task of completing the publication of their most recent data from Cuba.

At Columbia, Lewis was quickly dissatisfied with history and, on the advice of his brother-in-law Abraham H. Maslow (then a postdoctoral fellow in psychology at Columbia), he consulted Ruth Benedict and turned to anthropology. Columbia was at a turning point. Franz Boas had retired (though he was still in residence and taught a class that Lewis attended), and Ralph Linton had moved from the University of Wisconsin to join Benedict and a faculty that included Duncan Strong, Margaret Mead, Alexander Lesser, Gene Weltfish, Gladys Reichard, George Herzog, and others. Among Lewis' fellow students were Joseph Bram, Irving Goldman, Jack Harris, Natalie Joffee, Bernard Mishkin, Marvin Opler, Morris Siegel, and Charles Wagley. Interest in psychological and personality studies was strong in the department, sparked by Benedict (always much admired by Lewis) and Mead, and also by Linton, Abraham Kardiner, Cora DuBois, and others who participated in what became an influential interdisciplinary seminar. Along with Freudian, there were strong Marxist interests at Columbia and in the wider community; these, too, were shared by Lewis. But Columbia then was still the center of Boasian eclecticism, and political commitments to socialism were generally separated from anthropological theory—in the case of Oscar Lewis, throughout his life.

Lewis' talents and promise became evident with his first professional work on Blackfoot Indian culture. In the summer of 1939, the Lewises did their first field work with the Northern Piegan in Manitoba (other Blackfoot groups in Montana were simultaneously studied by other students from Columbia). They found a special personality type, the "manly-hearted" woman, which was the subject of Lewis' first article in the *American Anthropologist* (1941).

In it, he related the personality type to Piegan social institutions and to sex and age roles. For his doctoral dissertation, he then undertook an ethnohistorical study of the effects of United States and Canadian expansion on the Blackfoot peoples. Completed and published in 1942, the thesis was exemplary in method, with precise documentation showing that the much-discussed influence of the horse on Plains culture needed to be balanced by that of the fur trade.

In 1943/1944, after brief interim positions with the Human Relations Area Files project at Yale University and with the U.S. Department of Justice, Lewis first studied what was destined to be his major field of research, the people—mainly the poor families—of Mexico, Puerto Rico, and Cuba. The newly founded National Indian Institute sent him to Mexico City to represent the United States on the staff of the also new Interamerican Indianist Institute. Under its director, the distinguished anthropologist Manuel Gamio, the institute launched a program of publication and encouragement of the application of social science knowledge to the welfare of Indian peoples. As an extra task, Lewis agreed to study an Indian village to learn better what might be involved in applied work. He chose Tepoztlán because Robert Redfield had published (1930) what was then the best monograph on a Mexican village; and Lewis proposed to build his own studies of personality on this foundation. In the end, however, the project became his renowned “restudy,” *Life in a Mexican Village* (1951), a landmark in ethnographic method. It also involved him in his first collection of interviews of entire families, which were to be the basis of most of his later work.

Before his Tepoztlán restudy was published, another useful experience intervened. In 1944–1945 he undertook studies of rural communities in the United States for the Department of Agriculture. In contrast to Tepoztlán, he found here rich statistical sources to lighten the burdens of field work; his excellent study of Bell County, Texas, thus required only 11 weeks in the field. It was also a useful contrast, after the Piegan and Tepoztecan, to work in a segment of his own society. As a result of this variety of experiences, the Wenner-Gren Foundation invited him to write two major methodological reviews, “Controls and Experiments in Field Work” (1953) and “Comparisons in Cultural Anthropology” (1955).

In 1946 Lewis was appointed associate professor at Washington University in St. Louis,

and in 1948 he was called to the University of Illinois to develop its first independent department of anthropology, which began with the addition of Joseph Casagrande and moved into a high rank when Julian H. Steward was added in 1952. By then, Lewis’ reputation was unexcelled, and the Ford Foundation asked for his counsel in its programs in India. Lewis welcomed another opportunity for a comparative research experience (he had also spent some time in Spain in the summer of 1949), and as usual made the most of only one year, managing to include extensive field work (November 1952–May 1953) that resulted in *Village Life in Northern India* (1958), as well as to conduct an “applied” study of factions that was published by the India Planning Commission in 1954. As in Mexico and later in Puerto Rico and Cuba, in India, the Lewises received excellent cooperation from local institutions and were joined by colleagues and students in their complex interdisciplinary field work.

In 1963 Lewis began his study of one hundred Puerto Rican families in San Juan and their kin in New York. At the time of his death, full publication of the data on only one of these families had appeared (1966). In the 1960s, especially with President Lyndon B. Johnson’s war on poverty, Lewis’ concept of a culture of poverty—to which his Puerto Rican studies were relevant—was timely, controversial, and the subject of much debate. The fullest discussion appeared in *Current Anthropology*, in multiple reviews first of Lewis’ work (Book Reviews . . . 1967) and then of his critic, Charles Valentine (Lewis 1969b).

In 1946 Lewis had visited prerevolutionary Cuba briefly and in February 1968 Fidel Castro invited him to study families in Havana. With funding from the Ford Foundation, he and his staff were able successfully to interview hundreds of poor and not-so-poor. Although the Cuban government withdrew its sponsorship, and the project ended prematurely in 1970, shortly before Lewis’ death, most of the field materials were available for study. Posthumously published under the over-all title *Living the Revolution: An Oral History of Contemporary Cuba* (1977–1978) were three volumes (*Four Men; Four Women; Neighbors*) bearing Oscar Lewis’ name as senior author. *Neighbors* reports the lives of five families living in an apartment building in a formerly upper-class Havana suburb. Virtually emptied during the revolution by thousands of homeowners, the furnished houses

and apartments had reverted to the state, which turned them to public purposes and rental housing. Where Lewis' first "family" book had described a day in the life of each of five separate Mexican families isolated from each other, his last described the lives and relationships through time of close neighbors.

Lewis' contribution. Lewis' most lasting achievement may have been to give a voice to people and to families of people. Any doubts that the English sentences of the first published biographies accurately expressed thoughts originally spoken in Spanish were dispelled as soon as the same life histories were published in Spanish directly from the tapes. Lewis was completely honest—his professional interest being to perfect replicable field methods—and was also unusually suited to conduct such personal interviews successfully. Even more remarkable was his apparent ability to select and train staffs to do so, too. The printed interviews have been criticized for not providing information on the questions that had elicited the narratives or on circumstances that might explain the changing moods of the biographees. The number of printed pages of the primary material probably exceeds the volume of primary data published by any other ethnographer in the second half of the twentieth century; yet even the published material is a small fraction of the total corpus of tapes, transcripts, and other materials available to scholars in the Lewis archives at the University of Illinois. At present, there is no richer source to document the thoughts of hundreds of Mexicans, Puerto Ricans, and Cubans in a period of change. And since the subjects were selected as "ordinary" people, the interviews suggest the paradox that humans are somehow all extraordinary.

Before his death, Lewis completed a sample of his contributions up through 1965 in *Anthropological Essays* (1970). The book suggests that Lewis was satisfied with his life's work. He did not regret a lack of interest in grand theory and was proud of being a careful craftsman. He felt that he understood social-historical-cultural processes and their interactions with larger and smaller events, and he was satisfied to explain, at a level close to the data and with a minimum of jargon, the phenomena uncovered by careful reading and field work. He was proud to have developed family biographies as a contribution to method and thought it proper that they should speak for themselves. He worked in the tradition of teachers he admired:

Cohen, Boas, Benedict, and Linton had sought ways to approach the largest human problems, confident that each researcher could provide perspectives that were the pieces that would ultimately produce the larger perspectives of future Darwins, Marxes, and Freuds.

The phenomena that Lewis dealt with were largely societies and people in the process of change from village to city; and he took off from Redfield's construct of the folk-to-urban continuum. It was largely coincidental that Lewis, like Redfield, did his major field work in Mexico and the Caribbean, and that both had a later interest in India. The fact that Lewis' main start was in "Redfield's town," Tepoztlán, itself led to a special interest in Redfieldian thought. In 1926 Redfield had discovered "folk" in Tepoztlán who contrasted with his own people not only in the particular qualities given to each by cultural background (Nahuatl-Spanish versus North European-American) but also by sociodemographic circumstance. Redfield sensed that his own experience with rural life, then rapidly disappearing, had something in common with Tepoztecan village life. When in 1930 he began his research in Yucatan, his discussions with Robert E. Park suggested systematic study of the processes of change from more to less folk-like with the transition from hinterland to city. Applying concepts that had been developed by Tönnies, Weber, Simmel, and others, Redfield constructed a model that posed at one pole of a theoretical continuum an ideal *folk* type of community and at the other pole an ideal *urban* type, theorizing that all actual societies could be placed along that continuum. Lewis in Tepoztlán found himself not only restudying a town, with new perspectives, but also rethinking Redfield's theoretical conceptions, particularly since he soon began to study Tepoztecs (and others) in Mexico City as well. Lewis' development of Redfield's folk-urban theory gained from coincidence of their areas of field work and from personal communication and friendship with Redfield until Redfield's death in 1958. By then, however, Lewis was more and more absorbed in urban family studies and in polemics over his concept of the culture of poverty.

Like most anthropologists, Lewis empathized with and respected the people he came to know in his field work, presumably even those who despised themselves. Like most anthropologists he therefore took a positive view of their beliefs and behavior. Whether they were poor or rich,

their culture differed from his own. Those who were very poor behaved differently from those who were less poor, and Lewis described them as having a subculture of the general culture. A subculture was no less a positive way of life than a culture; and Lewis used the phrase "the culture of poverty" only to avoid pejorative associations. After studying very poor families in Tepoztlán and in Mexico City, he noted similarities to published data on slums elsewhere. The configuration of traits that seemed to be associated with the poor he called the culture of poverty. The term first appeared in the subtitle of *Five Families* (1959). His last explanation of this configuration was published in *A Study of Slum Culture* (1968):

(1) The traits fall into a number of clusters and are functionally related within each cluster. (2) Many, but not all, of the traits of different clusters are also functionally related. For example, men who have low wages and suffer chronic unemployment develop a poor self-image, become irresponsible, abandon their wives and children, and take up with other women more frequently than do men with high incomes and steady jobs. (3) None of the traits, taken individually, is distinctive per se of the subculture of poverty. It is their conjunction, their function, and their patterning that define the subculture. (4) The subculture of poverty, as defined by these traits, is a statistical profile; that is, the frequency of distribution of the traits both singly and in clusters will be greater than in the rest of the population. In other words, more of the traits will occur in combination in families with a subculture of poverty than in stable working-class families. Even within a single slum there will probably be a gradient from culture of poverty families to families without a culture of poverty. (5) The profiles of the subculture of poverty will probably differ in systematic ways with the difference in the national cultural contexts of which they are a part. It is expected that some new traits will become apparent with research in different nations.

Several criticisms leveled at the popular phrase would have been avoided had Lewis with its first use in 1959 clarified what he meant by it. For example, ink was wasted on his use of the word "culture" (implying to some that the poor constituted an independent society) rather than a subculture. He would also have disarmed many critics had he indicated how the biographies in his published works matched or diverged from the poverty pattern—even at the expense of spoiling the drama. However, he could not have avoided criticism associating his views with

those of reactionaries who blamed the poor for their poverty. Although Lewis thought positively of the poor, he hated poverty, and of course he saw the poor as victims. But he was criticized for publishing a view that was being misused against the poor. Even more difficult were critics who, wishing to disarm those who blamed the poor, exaggerated small weaknesses in Lewis' concept and his data.

Lewis answered answerable critics briefly in his reply to reviewers (Book Reviews . . . 1967, p. 499) as follows:

. . . it is a serious mistake to lump all poor people together, because the causes, the meaning, and the consequences of poverty vary considerably in different sociocultural contexts. There is nothing in the concept which puts the onus of poverty on the character of the poor. Nor does the concept in any way play down the exploitation and neglect suffered by the poor. Indeed, the subculture of poverty is part of the larger culture of capitalism whose social and economic system channels wealth into the hands of a relatively small group and thereby makes for the growth of sharp class distinctions.

But the genuine issue remains. A culture (or subculture) perpetuates itself, and Lewis indeed meant to affirm that it does so in the case of poverty. Although the pressure exerted by the larger society was the main reason for the persistence of the subculture, he wrote:

. . . *this is not the only reason.* The subculture develops mechanisms which tend to perpetuate it, especially because of what happens to the world view, aspirations, and character of the children who grow up in it. For this reason, improved economic opportunities, though absolutely essential and of the highest priority, are not sufficient to basically alter or eliminate the subculture of poverty. Moreover, it is a process which will take more than a single generation, even under the best of circumstances, including a socialist revolution. (Book Reviews . . . 1967, p. 499)

The opportunity to test this thesis by examining the validity of the last sentence came with the Cuban socialist revolution, which changed the social structure that had borne down on the poor. Among the families studied were many who had lived in poverty. What happened to them? Lewis did not live to tell. His co-worker, Douglas Butterworth (1972) writes that at his death Lewis had not yet formulated a final opinion on whether the culture of poverty had survived the revolution, "but my impressions from conversations with him and my own experi-

ences in Cuba. . . . [indicate] that through indoctrination of socialist and nationalist ideals and incorporation into revolutionary organizations, perhaps about a third of those families we studied who had been in the culture of poverty were now outside it" (p. 753). The three posthumous volumes of *Living the Revolution* contain 15 long and 13 partial biographies, too few of whom had lived in poverty to draw statistical conclusions; but the archives contain many more. Oscar Lewis himself (who returned frequently to his Mexican families, as for *A Death in the Sánchez Family* [1969a]) would have advised continuing field work with the younger generations of his families in Cuba, hoping, very probably, that economic security would produce more rapid effects than the culture of poverty thesis predicted. Even as he tried to correct misunderstandings about the concept, he often indicated that for him it had never been a grand theory and was certainly not important enough to overturn his egalitarian social ideals.

SOL TAX

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LICHTHEIM, GEORGE

George Lichtheim (1912-1973) was a younger member of the illustrious group of Jewish upper middle-class scholars and intellectuals whose outlook was formed under the Weimar Republic. Hitler's accession to power drove them into exile, with the result that they made invaluable contributions to the cultural and intellectual life of the various countries in which they settled, as well as to German culture itself when the broken thread was picked up again after 1945. Unlike such older figures as Hannah Arendt, Walter Benjamin, Gershom Scholem, and the philosophers and sociologists of the Frankfurt Institute for Social Research, Lichtheim had not even completed his university education at Heidelberg at the time of the Nazi revolution, and, except for brief visiting appointments at Columbia and Stanford universities in the 1960s, he never returned to the academic world despite many opportunities to do so in the last decade of his life.

Lichtheim joined his parents in Palestine in 1934 and became a journalist on the staff of the English-language *Palestine Post*, writing chiefly about European and world politics in the shadow of the approaching war. He belonged in the 1930s to an unusual intellectual circle of fellow exiles in Jerusalem that included Scholem and the philosopher Hans Jonas, who encouraged him to persevere in his interrupted formal studies of philosophy and the history of Western political thought. In 1943

Lichtheim moved to London, where, except for two long visits to America, he remained for the rest of his life. Writing for a time under the name of "G. L. Arnold" as well as under his own name, he became in the 1940s and 1950s a prolific writer of essays, reviews, and commentaries on current affairs for a variety of journals in England and the United States. He also served for a time as editor in chief of the London magazine *Twentieth Century* and as an associate and later a contributing editor of *Commentary* in New York. His mastery of English prose (rare among German refugees, but he had spent two years in England as a child), his mordant wit, his remarkable but lightly worn erudition, and, most of all, his utter independence and originality of judgment became familiar to a small circle of readers of Anglo-American intellectual journals. These qualities were fully in evidence in his first book, *The Pattern of World Conflict* (1955), published under the name of G. L. Arnold, which was an examination of the cold war and the rise of the underdeveloped countries that anticipated a great many themes later to enter common intellectual parlance.

The publication in 1961 of his major book, *Marxism: An Historical and Critical Study*, brought Lichtheim wide acclaim. This magisterial study of Marxism as a developing corpus of social theory shaped by nineteenth-century European history drew on sources that had previously been scarcely known to English-speaking readers. Lichtheim followed it up with a shorter study, *Marxism in Modern France* (1966), before turning to the history of socialism—essentially of socialist *ideas* in the widest sense rather than of movements—which resulted in *The Origins of Socialism* (1969) and *A Short History of Socialism* (1970b). He also published *The New Europe: Today, and Tomorrow* (1963), a masterful assessment of the trend toward European integration, and two shorter books, *Lukács* (1970a), a volume in the Fontana Modern Masters series, and *Imperialism* (1971b), a brilliantly far-ranging treatment of that ideologically overburdened subject. No less than three essay collections appeared in the last six years of Lichtheim's life: *The Concept of Ideology, and Other Essays* (1967), *From Marx to Hegel* (1971a), and *Collected Essays* (1973). His extraordinary productivity is indicated by the fact that only a single essay appears more than once among the selections reprinted in these volumes. His range is most fully displayed

in the last and longest collection, which includes, along with much else, portraits of Winston Churchill and Charles de Gaulle, assessments of Leninism and Maoism, and critiques of Jean-Paul Sartre's existential Marxism and of Simone Weil's religio-political reflections.

Despite Lichtheim's long residence in Palestine and England and the fact that he wrote almost entirely in the English language, he always defined himself as a central European intellectual from "the old European heartland of Marxism" rather than as a Jew or an Anglo-American. His last book, *Europe in the Twentieth Century* (1972), was an attempt to sum up the rich and complex political, intellectual, and aesthetic history of Europe in the troubled twentieth century, which had long been his primary subject and the ground of all his work. The book is an encyclopedic compendium but suffers from its effort to cover too much too briefly, especially in its treatment of philosophy, Lichtheim's original field of interest to which he increasingly returned in the final years of his life. He is likely to be most remembered for the histories of Marxism and socialism in which he brilliantly succeeded in emulating their chief protagonist, Marx himself, in uniting intensive knowledge of German philosophy, French politics and political thought, and British economics and political culture.

How should we classify George Lichtheim? Was he an intellectual and political historian? A political theorist? A sociologist of knowledge? An interpreter of German and French thought to a parochial English-speaking audience? He was often all of these, but at his best he achieved the stature of an original social theorist in his own right if not quite that of a full-fledged philosopher. He reserved his highest admiration for the creative philosophical synthesis of universal idea and historical insight—Hegel, of course, was his prototype—and was inclined to deprecate his own achievements when measured by so exalted a standard. His modesty, combined with the fluent allusiveness of his style, its lack of any straining toward oracular utterance, and a preference for ironic understatement that perhaps owed something to his partial acculturation to England, tended to obscure his originality. He sometimes dismissed himself as a "mere" recorder and chronologist of other people's thoughts, and his scrupulous concern for the intellectual pedigree of the ideas he articulated may have falsely conveyed this impression to his less learned readers. But his

gift for seeing the exact conjuncture between an idea and its historical setting made him much more than a narrow historian of ideas. He possessed an extraordinary sensitivity to the way in which the fate of ideas and political doctrines was shaped by "the exact moment on the time curve, and the historical situation of which such moments are part" (1973, p. 141). His sense of the interaction of individual creativity, the constraints of social structure, and historical change raises his own work to the level of creative interpretation, although he never embodied his understanding in a formal conceptual apparatus or set of general propositions.

Lichtheim had been a Hegelian and a Marxist since his youth in Berlin; he had studied the heretical Marxist writings of György Lukács and Karl Korsch when they first appeared in the 1920s; and he forthrightly rejected both the official Social Democratic and the Soviet versions of Marxism. His intellectual and political affinities resembled those of the Frankfort School, both in the 1920s and much later in his life. His closeness to public events during his journalistic career and his years in Britain, however, imbued him with an antiutopian political realism and considerable respect for "bourgeois" democracy that were alien to the Frankfort theorists, especially Herbert Marcuse and his youthful "New Left" followers in the 1960s. Although he was often linked to the Frankfort tradition, only in the last few years of his life did Lichtheim establish a direct contact with its heirs when he became close to Jürgen Habermas, whose work he enormously admired and regarded as overshadowing his own, once again minimizing the value and distinctiveness of his special gifts.

Lichtheim advocated and exemplified a subtle, antipositivistic Marxism grounded in the history of the modern West since the French revolution—this at a time when Marxism was still largely identified with the materialistic and deterministic philosophy of progress it had become in the hands of Friedrich Engels and Georgi Plekhanov, or, even worse, with the crude Marxism–Leninism of official Soviet ideology. Lichtheim wrote about the ideas of Lukács, Korsch, Antonio Gramsci, and T. W. Adorno at a time when they were scarcely known in England or America, although their writings later became central to the canon of "Western Marxism" embraced by a considerable number of young academic men and women in both the

social sciences and the humanities. It is an irony of intellectual history that the belated appearance of "Marxists of the chair" in British and American universities tended to eclipse Lichtheim's role in preparing the ground for this overdue development. Lichtheim's increasing pessimism and melancholy in the last few years of his life doubtless owed something to the tension between the utopian and revolutionary temper of the post-New Left Marxist epigoni and his own strong rejection of Soviet and Maoist totalitarianism, his skepticism about the achievements of "Third World" revolutions, and his reverence for the cultural grandeur of European bourgeois civilization. This tension surfaced in the hostile reactions to his short book on Lukács, which was sharply critical of the Hungarian thinker for his conformist apologetics on behalf of Stalinism and his hostility to aesthetic modernism.

At a concrete historical level, Lichtheim's prescience in discerning major trends well in advance of other observers was striking. At the peak of the cold war he insisted that, barring nuclear holocaust, Soviet–American rivalry was essentially a mere incident in the larger struggle of the underdeveloped countries to achieve modernization. Long before the label "technocrat" had entered political debate, Lichtheim argued that the scientific and managerial intelligentsia was the most rapidly expanding and politically important stratum in advanced industrial societies and that socialist movements must appeal to this "new class" and abandon their primary identification with the old working class, or else become obsolete. Lichtheim saw that after World War II Britain and the United States were beginning to suffer the "crisis of values" resulting from the disintegration of bourgeois civilization that had wracked central Europe after 1914. The widespread malaise giving rise to abortive new cults and "countercultures" in the 1960s and 1970s later strongly confirmed this diagnosis. Lichtheim's characterization of the late twentieth century as a "postbourgeois" age was more apt and accurate than the other popular labels with the "post" prefix that were widely applied to it—"postindustrial," "postcapitalist," "postmodern," *et al.*

Without ever having held a regular university post or having become a frequent public lecturer or a mass media performer, George Lichtheim was the teacher, with his pen alone, of a generation of English-speaking intellectuals. In

contrast to some of his more famous Weimarian contemporaries, he did not provide them with a system of thought or even a new vocabulary. He taught them, rather, how to discern historical depths, continuities with the formative events of modern history, in the most ephemeral of cultural and political fashions. He gave tangible reality to the much-vaunted notion of "historical perspective."

DENNIS H. WRONG

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LIKERT, RENSIS

Rensis Likert was born in Cheyenne, Wyoming in 1903. After receiving his A.B. from the University of Michigan in economics and sociology in 1922, he studied psychology at Columbia University, where he received his PH.D in 1932. While a student at Columbia, he married Jane Gibson whom he had met as an undergraduate in Ann Arbor.

Rensis Likert's contributions to social psychology and the social sciences can be summarized under five rubrics: (1) the measurement of attitudes; (2) the development of methodologies of survey research; (3) the creation of the Institute for Social Research at the University of Michigan; (4) the theory of participative management; and (5) human resource accounting.

Measurement of attitudes. In graduate school, Likert gradually moved away from the more traditional fields of psychology toward the newly emerging area of social psychology. He was particularly impressed by Gardner Murphy, who became chairman of his dissertation committee. With Murphy, Likert conducted an extensive study of the attitudes of college students and of the variables affecting these attitudes. Part of this research became his dissertation, which was published under the title *A Technique for the Measurement of Attitudes* (1932). During the course of this research, Likert developed what has become the most widely used procedure for attitude measurement, the well-known "Likert scale." In the late 1920s, L. L. Thurstone had pioneered in the field of attitude measurement with a procedure based on the psychophysical method of equal-appearing intervals. Likert found that a much simpler method, the construction of an attitude scale ranging from favorable to unfavorable, with a neutral midpoint, gave almost the same results as the more cumbersome Thurstone procedure and achieved the same reliability with half as many items.

Development of survey research methodologies. From 1930 to 1935, Likert taught in the department of psychology at New York University. In 1935, he became director of research for the Life Insurance Agency Management Association in Hartford, Connecticut, where he launched a program of research on the effectiveness of different styles of supervision. In September 1939, he was appointed director of the Division of Program Surveys in the Bureau of Agricultural Economics of the U.S. Department of Agriculture in Washington.

The Division of Program Surveys had been established to provide a conduit through which farmers and other citizens could communicate to the department their experiences with the various federal programs that affected them. When Likert entered the division, the procedures of information gathering were crude and in need of improvement. Likert, along with Morris H. Hansen and others from the Department of

Agriculture, the Bureau of the Census, and Iowa State University collaborated in developing a method of sampling households and individuals that was based on the identification and listing of small units of land area throughout the country. The theory underlying this procedure, and the specific techniques of selection, was the basis of what later became known as "probability sampling."

More reliable methods were also needed for obtaining information from individual respondents. Likert discovered that government agencies had been using a "reporting form" that specified only the types of information desired; the interviewers were expected to ask whatever questions they thought best to obtain that information. When the serious problem of interviewer bias, which was inherent in this technique, was demonstrated, the Division of Program Surveys adopted formalized questionnaires which the interviewers were instructed to follow without deviation. Likert introduced the use of "open-ended questions" in these interviews when it was desirable to let the respondent interpret the question in his own terms rather than choose among specified alternatives. These procedures have since become standard practice in survey research.

Establishment of the Institute for Social Research. In the summer of 1946, the University of Michigan invited Likert to Ann Arbor to set up an interdisciplinary institute for research in the social sciences. The Institute for Social Research, which Likert founded and directed, was not the first such institute in the United States, but it differed in critical ways from those that preceded and followed it. Intended from the beginning to be truly interdisciplinary, it was located administratively outside the established schools and departments so that it could achieve this breadth of research interest. The research staff of the institute held their primary appointments in the institute rather than in the teaching departments. Grants and contracts from foundations, governmental agencies, and private profit and nonprofit organizations provided the basis for the institute's support. With the advice of a faculty executive committee, the institute accepted only such research activities that seemed clearly appropriate to the university's general interests and that could be published.

The role of Rensis Likert in the establishment of the institute, particularly during its early, uncertain years, was critical. His engaging personality and talents of persuasion were crucial

in representing the institute—occasionally to skeptical individuals with the university, but constantly in the world of grants and contracts where competition for research support was always brisk. His unfailing optimism and refusal to believe that any obstacle could impede the growth and progress of the institute often left his associates a little breathless, but ultimately his expansive predictions proved realistic. Under Likert's direction, the institute grew rapidly and within a few years became the largest university-based organization for research in the social sciences in the United States.

Theory of participative management. Shortly after he had established the Institute for Social Research in 1946, Likert continued the research program that he had initiated before the war, the study of management. Assisted at first by a grant from the Office of Naval Research, and supported later by contracts with private corporations, he directed a series of studies in business and government that were aimed at discovering the principles used by managers who achieved the highest performance and employee satisfaction and the ways in which these principles differed from those used by managers who obtained only mediocre results. Likert and his major associates, Robert Kahn, Floyd Mann, Stanley Seashore, and David Bowers, directed surveys and experiments that ultimately led to Likert's well-known series of books on participative management.

Likert believed that styles of management went through four phases, evolving from what he called exploitive authoritarian, through benevolent authoritarian, to consultative management, and eventually, to participative management. The central principles of participative management, or System 4, as he described it, were: (1) supportive relationships between organizational members and the avoidance of demeaning punitive behavior; (2) multiple overlapping group structure, with each organizational "family" consisting of a superior and his subordinates; (3) group problem solving by consensus within organizational families; (4) high performance goals; and (5) overlapping memberships between organizational families by individuals who serve as "linking pins." Likert found that organizations operating under these principles were more responsive to basic human motives than more authoritarian organizations were likely to be, and that they were consequently better able to mobilize their human resources to achieve organizational goals. These

concepts and the associated research were laid out by Likert in *New Patterns of Management* (1961) and subsequently in *The Human Organization* (1967). The concepts were developed further and additional research reported in *New Ways of Managing Conflict* (Likert & Likert 1976).

Human resource accounting. In 1967, Likert argued that current methods of accounting gave corporate management inadequate and misleading information on the relative effectiveness of different management styles. To expand these sources of information, Likert proposed that human resource accounting be adopted, and he suggested several possible methods, which he later refined. Human resource accounting computes in dollars the investment a firm has in its people and the changes in the productive capability of these people from one accounting period to another.

Likert's theories of participative management did not revolutionize management practices in American business and industry. Likert himself described the typical business organization in this country as falling within the category of System 2, or benevolent authoritarian. But there is no doubt that his research and writing, along with that of other scholars, raised fundamental questions about the nature of organizational life and contributed to a gradual change toward less authoritarian practices in American management.

Rensis Likert retired as director of the Institute for Social Research in 1970 after 25 years in that position. He continues to work as a private consultant on problems of organization and management, and is engaged in further research on the comparative effectiveness of participative management.

ANGUS CAMPBELL

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LORENZ, KONRAD

Konrad Lorenz, an Austrian zoologist and ethologist, was born on November 7, 1903. In accordance with his father's wishes, he studied

medicine at the University of Vienna, but his interests were always primarily in zoology. As a boy in Altenberg, he built aquaria and bird cages, and soon turned the garden with its ponds and animal enclosures into a small zoo. Thus, after receiving his M.D. in 1928, he studied zoology and received his Ph.D. at the University of Vienna in 1933.

Throughout his research, Lorenz emphasized the importance of gestalt perception. In his opinion, the knowledge acquired by artists and scientists through gestalt perception transcends by far what "rational thought" makes available to us. He disagreed with those who viewed quantification as the only legitimate source of knowledge. This reliance on intuition provides one of the keys to understanding Lorenz' personality. As a scientist, he had an artistic nature; it was his gift to see in ethology relationships that led to new scientific insights.

In medical school, Lorenz kept meticulous notes on his charges, and in 1927, while still a student, he published his first work on jackdaws. He worked as an assistant to the anatomist Ferdinand Hochstetter, whom he revered, and gained a comprehensive knowledge in comparative and functional anatomy. His simultaneous observations on animals taught him that their behavior repertoire included stereotyped movement coordinations that were constant in form and could be used to identify species in the same way as bodily structures. These behavior patterns can be compared across species, just like morphological characteristics, and if one applies the criteria of homology as used in morphological homologies, the evolutionary history of some behavior patterns can be reconstructed. In his papers on the ethology of the social corvidae (1931), the species-specific behavior patterns of birds (1932), and the companion in the bird's world (1935), he presented the first detailed outline of a biology of behavior.

In these original works, Lorenz discussed the concept of the instinctive behavior pattern, which is distinguished from all acquired behavior patterns in that it matures "independent of experience" during the course of embryonic and ontogenetic development. Lorenz emphasized the dichotomy between inborn and acquired behavior, and although he always acknowledged that Charles Darwin, Oskar Heinroth, and others had expressed similar ideas, he was the first to incorporate these ideas into a new and consistent theoretical structure. Much later, when he considered the origin of the adaptiveness of be-

havior, he arrived at an even clearer definition of these concepts.

Instinctive behavior patterns were first thought of as reflex chains. Lorenz pointed out, however, that Wallace Craig had already stated that the readiness to perform a particular instinctive behavior pattern increased if the behavior had not occurred for some time. In an extreme instance, the behavior can run off as a vacuum activity in the absence of the appropriate stimulus situation. This is not the case with instinctive behavior patterns, which in contrast to simple reflexes, are accompanied by feelings and affects that are subjectively experienced. While this cannot be actually proven, no one familiar with animals can overlook the homologies that exist between humans and animals.

Lorenz emphasized the stereotypic, automatic character of instinctive behavior patterns and the fact that animals act instinctively because they are not guided by insight into the purpose of their actions. Instead they react rather blindly, which often leads to inappropriate behavior, particularly in the actions of captive animals.

Instinctive behavior patterns are released by stimuli and stimulus combinations that are simple and unmistakable, and reflect the natural, releasing situation. Lorenz called behavior patterns that serve as signals releasers. The releasing stimuli affect an innate releasing mechanism, which is seen as a perceptual correlate to the stimulus. This is a kind of stimulus filter which, not unlike a lock, responds to only specific key stimuli. The stimulus receiver can be thought of as possessing a schematic (simplified) picture of its companion, which can appear as a distinct entity in several functional systems, for example, as a child, sexual, or parent companion. The conspecific is not perceived as totality, but as the bearer of signals appropriate to the release of various behaviors in distinct functional systems. A female muscovy duck (*Cairina*) will react to the lost call of a mallard duckling by attempting to rescue it from the hands of the experimenter. But if this duckling joins the flock of her own young she will kill it, since she recognizes it as another species by distinct visual or acoustic signals.

In describing the phenomenon of imprinting, however, Lorenz (1935) found that in some species of birds the innate schema of the sexual or parent companion is not very specific. In these cases, the young animal learns during a short, sensitive period by the appearance of its

parents what the appearance of its sexual companion will be later in life. Ducklings and goslings follow practically any moving object after hatching, including humans, but if they are exposed to humans during a relatively short period, they retain their preference for them and are unwilling to follow their own species.

In contrast to the classical reflex concept, according to which all behavior is a reaction to stimuli, the recognition of action-specific fatigue, lowering of thresholds, intention movements, and vacuum activities led to inconsistencies that at first Lorenz was unable to reconcile. Then in 1937 he met Erich von Holst, who had shown in a number of experiments that movements do not always need a stimulus to trigger them in reflexlike fashion. Furthermore, he had shown that coordination of muscle movements is already centrally coordinated and that they can occur without the participation of external and internal stimuli.

The discovery of central excitatory potential by von Holst basically changed our conception of the function of the central nervous system. Behavior was no longer viewed only as a response to external stimuli, because spontaneously active groups of neurons whose coordination occurs without sensory input are also present in the central nervous system. Behavior is thus centrally activated, and the continuously and spontaneously active groups of neurons must be prevented from continuous discharge by some inhibiting mechanism. Von Holst concluded that no stimuli are needed to activate an organism; rather, its internal drive must be inhibited.

Lorenz immediately recognized the significance of this discovery, which enabled him to explain the phenomena of the lowering of threshold, vacuum activity, and action-specific fatigue by assuming that not only locomotor patterns but instinctive behavior patterns in general are activated by motor neuron groups. Lorenz postulated an action-specific energy for specific behavior patterns and thought this energy was used up during the performance of the behavior, thus leading to action-specific fatigue. The concept of action-specific energy did not imply that each movement was based on qualitatively different biochemical processes. Lorenz placed more weight on the specificity of the active groups of neurons. The conception of a continuous production of excitation implied further that there had to be inhibiting mechan-

isms that prevented the continuous discharge of the continuously produced impulses. Such an inhibitory block is only removed when adequate releasing stimuli are mediated by an innate releasing mechanism. Lorenz made one additional assumption, namely that this reflexlike inhibition would lead to a storing of action-specific energy. In this he was supported by the observations of C. S. Sherrington, who had described a storing of excitatory potential for reflexes in the phenomenon of "spinal contrast." This storing of excitatory potential was thought to motivate the animal by means of appetitive behavior to actively seek out the proper releasing stimulus situation. It would react to increasingly less specific stimuli, until finally the behavior would appear as a vacuum activity in the absence of any detectable stimuli. The strength of a reaction would then depend not only on the quality of the releasing stimuli, but also on the internal drives. Lorenz published this synthesis of his ideas with the results of von Holst's experiments in *Folia Biotheoretica* (1937a). With this new concept, the year 1937 thus brought a major advance for ethology.

At the same time, Lorenz developed friendly contacts with Nikolaas Tinbergen who carried out studies in the biology of behavior in Holland. Specifically, he studied the releasing and directing stimuli as well as certain behavior patterns that occur in conflict situations. These he interpreted in line with the new ethological model as a central sparking over of a central excitation that was prevented from running its normal course. He called it displacement reaction. Tinbergen and Lorenz collaborated on an experiment that demonstrated that instinctive behavior patterns consist of a rigid component, the inherited movement pattern (later called a fixed action pattern), and a less rigid taxis component that is controlled by external stimuli and is called an orienting movement. Further investigations by Tinbergen led to the recognition of the fact that the Lorenz-Craig hypothesis was too simple. The consummatory action could be preceded by a chain of appetitive behavior patterns that led from general to more specific behaviors.

In the years after the discovery of the spontaneity of instinctive behavior patterns, Lorenz concerned himself increasingly with the behavior of humans. Although in 1940 he published his comparative study on the movement patterns of swimming ducks, most of his works

thereafter focused on man, even when animal observations provided the basis on which his conclusions were built.

The new drive concept and the concepts of innate behavior patterns, releasers, key stimuli, and innate releasing mechanisms seemed ideally suited to explain certain aspects of human behavior. The fact that humans react rather automatically in the realm of social behavior suggests that inborn constraints may affect the degree of freedom of our actions. The contrast between our achievements in dealing with problems presented by the physical environment and our failure to solve problems arising from human interactions is awesome. In 1943 Lorenz published "Die angeborenen Formen möglicher Erfahrung" in which he pointed out that humans may well respond to stimuli prior to all experience with them on the basis of innate releasing mechanisms in an adaptive, species-preserving manner. This applies to certain basic cognitive structures with respect to space and time as well as to certain reactions to other people. Here too man reacts to very simple stimuli that can easily be imitated in an experiment. Thus, experiments using models show that relatively simple characteristics of human babies elicit emotionally toned approach behavior. Objects that have these cues are considered cute. The baby schema consists of several components that can be presented singly as well. They may be exaggerated, and when several components are combined their effectiveness in releasing a response is additive. For example, babies have comparatively large heads in relation to their bodies and their extremities are short and chubby. Industry produces cute dolls and illustrations for stories in which the head-body proportions are exaggerated as the sole cue of "babyishness." We respond in a similar fashion to stimuli emanating from sexual partners and to certain facial expressions that can be sketched with relatively few lines, and respond blindly to animal facial expressions and consider them friendly, sad, brave, and noble if they include components contained in our own facial expressions. Lorenz also hypothesized that we classify fellow humans quite generally according to such criteria as well-proportioned and beautiful, and that we like people who appeal to our aesthetic sense. Such a sense of values is also projected onto other species as for example when we consider a gazelle noble and the plump hippopotamus ugly.

Besides these aesthetic schemata, Lorenz also suspected the existence of ethic schemata. These provide the blueprints for social behavior by giving people a foreknowledge of what behavior is good and bad.

During his comparative studies of domesticated and wild animals, Lorenz found that the process of domestication resulted in a number of changes in body structures and behavior that are similar in mammals and birds. In domesticated animals, where rapid reproduction is desirable, there is a decrease in the fine nuances of expressive behavior found in the wild ancestral forms. Wild geese are monogamous and mate only after an elaborate courtship ceremony; the domesticated goose is promiscuous and shows little courtship. In the absence of predatory pressure, quick reactions are no longer the basis of selection. The senses, muscles, and even intelligence may atrophy. Weakness of connective tissues and a tendency to deposit fat results. This loss of specificity is adaptive if one considers domesticated animals as symbionts of man. Nevertheless, we still judge these changes subjectively as degenerate. Why? Lorenz saw the reasons for this in the fact that civilized man is also subject to self-domestication, which leads to changes in body structure and behavior. Only our inborn sense of values counteracts these degenerative changes.

Finally Lorenz speculated about the decline of cultures, but not along the lines proposed by Oswald Spengler, who spoke of quasipsychological aging processes. Instead, Lorenz suspected that degenerative changes that result from the process of domestication might be the cause. Humans who have fewer scruples in their dealings with others and are ruthless towards their fellows will at first gain an advantage in certain civilizations. The increase in people who have no consistent set of values, who are incapable of close bonds with others, who do not identify with their community, and who lack the courage to defend their group, will lead to the demise of such civilizations, thus making room for those with more adaptive behaviors. However, Lorenz did not see this as the inevitable fate of civilized peoples, since man can control his fate by insight into these processes.

As a result of these findings, critics in recent years have accused Lorenz of racism. The charge is unwarranted since Lorenz spoke of domestication effects in all civilizations and felt an obligation to call attention to the problem.

The genetic deterioration of civilized mankind cannot be denied in view of the increase in inherited illnesses. As a result of modern medical practices many people, who in the past would not have been able to have children, now reach the reproductive phase of their lives and are acceptable as marriage partners. In presenting these controversial ideas, Lorenz concluded that education was the best means of regulating human reproduction. People should be educated to make an appropriate choice of partners and to exercise self-control in the realm of reproduction in order to avoid tragic afflictions in offspring (Lorenz 1961).

In this same study, Lorenz also discussed the functional analogies of moral behavior in animals. He showed that dangerous animals possess inhibitions against using their weapons against conspecifics in a damaging way. They fight in a ritualized manner and have special submissive postures with which to placate the victor. In man the invention of weapons led to a situation in which such inborn inhibitions cannot always be effective. Possessing an aggressive drive and having weapons that kill quickly, before the opponent has time to make a submissive plea for mercy, man must frankly confront the categorical question posed by his own self-destructive behavior, for example by the development of cultural patterns based on codes of honor.

At the time Lorenz wrote this paper, he was appointed to the chair of philosophy at the University of Königsberg, where he became one of the successors to the chair once held by Immanuel Kant. World War II interrupted his scientific activities. Lorenz was an army doctor from 1942 to 1944, when he became a Russian prisoner of war. In 1948 he was released and returned to his native Austria. That same year, he presented a lecture at the Biological Research Station at Wilhelminenberg outside Vienna; his ideas were unsurpassed in freshness and imagination.

After a period of professional uncertainty, Lorenz was given a research station in Buldern, Westphalia, in West Germany by the Max Planck Society, which in 1957 became the Max Planck Institute for Behavioral Physiology at Seewiesen near Munich in southern Germany. He headed the institute with his friend Erich von Holst, and became the focal point of a European school of ethology.

Following World War II, Niko Tinbergen took the initiative in spreading the ideas of European ethologists to the English-speaking world. In his

book *The Study of Instinct* (1951) he combined the morphology of behavior with the physiology of behavior. When Tinbergen moved to England another focal point of ethology was created.

Soon Lorenz' ideas became the focus of international discussions. Some of his ideas provoked vigorous responses from behavioristically oriented American psychologists. Learning theories had exercised a great influence on American students of behavior ever since J. B. Watson. The notion that man is shaped primarily by his environment and is born with his brain as a kind of blank slate was for a long time the accepted creed in psychology, sociology, and anthropology. Lorenz' findings were in direct contrast to these ideas, for he claimed that the behavior of animals and people was affected by biological inheritance. In addition, classical reflex theory had many adherents in the United States, and those who studied animal behavior by various conditioning techniques were not likely to agree with Lorenz' theory of the spontaneity of behavior. Finally, many American behavioral scientists lacked Lorenz' background in comparative research. In contrast to American psychologists, American zoologists had little difficulty in understanding Lorenz' ideas, since they were familiar with the comparative morphology and behavior of many species.

Most American psychologists, however, were critical of or rejected the ethology of Lorenz and Tinbergen. There were exceptions, such as Eckhard H. Hess, who discovered in his studies on chickens that Lorenz' ideas had great explanatory value. Of all the critics, Daniel S. Lehrman gained the greatest prominence with his article "A Critique of Konrad Lorenz's Theory of Instinctive Behavior" (1953). This critique proved to be of great heuristic value. Lehrman contended that the innate-learned dichotomy had no heuristic value since it can never be proven that a behavior is inborn. All behavior patterns develop continuously and an embryo cannot be isolated from environmental influences even in the egg. Therefore, raising an animal free of external influences is impossible. The growing organism is subject to influences during any stage of its development and can thus have "experiences." Lehrman thought that Lorenz' concept of the innate was negatively defined as "not learned," and since learning experiences could not be excluded, the concept had no value.

This critique pointed out a weakness in the ethological concept of the innate. Actually every

biologist knew what was meant by "innate," but there was no clear definition of the word. This critique caused Lorenz to reexamine his entire system of ideas. In 1961 he replied with his *Evolution and Modification of Behavior*, where he defined "inborn" in a positive way according to the origin of the adaptation.

Lorenz took for granted that behavior patterns, just like morphological characteristics, mirror certain features in the environment to which they are adapted. Such adaptations presuppose that the adapted system copies, however crudely, at some time, the information about the environmental situation. This "acquisition of information" can take place during phylogeny through the mechanism of natural selection of mutations. Thus, proven experiences are preserved in the genome and are decoded during ontogenesis. However, information can also be acquired in the course of cultural and individual development through various learning processes. Information can be stored by the central nervous system, and for man, in written records and by other technical means. The manner in which the adapted system acquired the information for a specific adaptation can be assessed by raising an animal in the absence of specific experiences or information. It is neither necessary nor desirable to withhold all information from an animal during its development. Instead it is important to design an experiment so that only the specific adaptation being investigated is interfered with. If one wanted to find out, for example, whether a stickleback innately reacts to the red underside of a conspecific, it will suffice to withhold the specific stimulus situation from the animal. To raise the animal in complete darkness would be inappropriate, since this would lead to retinal degeneration and subsequent disruptions at an entirely different level of adaptation.

The same is true with respect to motor patterns. If one wanted to know if a bird had acquired its song as a phylogenetic adaptation, it would not be necessary to raise the bird in complete isolation from all stimulation. It is enough to isolate it from all sound. If the bird still sings its species-specific territorial and courtship song when it is sexually mature, the proof has been obtained that the information about the song patterns must have been stored in the genome. This is a logical conclusion and is not dependent on knowledge as to how in the specific case the genetic information is decoded during the course of development.

Lehrman accepted the validity of this argument, but also called for an analysis of the developmental process. Lorenz agreed. In practice, the answer to the question as to whether a behavior pattern owes its specific adaptiveness to a phylogenetic or cultural or individual process is only the first step toward a complete analysis. There exists no logical argument based on the issues against this position. However, there is no lack of the monotonous reiteration of the comment that "the nature-nurture issue is dead." Such remarks merely show that the authors have not understood the issues involved. It is, in fact, very much alive. Lorenz made a decisive contribution to the consolidation of ethological thought with the clear definition of phylogenetic adaptations, and has provided a positive statement of the meaning of "innate."

In the years following World War II, Lorenz also studied ducks, geese, various species of cichlid fish, and coral fish. Among other things, he was interested in the questions of intraspecific aggression, and collected his observations and thoughts in *On Aggression* (1963). The title showed that Lorenz approached the topic from a biologist's perspective. Quite outside of any value systems he unhesitatingly asked the question, what is the biological function of aggressive behavior. He was always searching for the selection pressure responsible for a particular behavior. As with all biologists, he was interested in learning how a particular behavior contributes to the preservation of the species and was not interested in technical explanations. He studied the various forms of aggressive behavior and his main points can be summarized as follows:

(1) Intraspecific aggression evolved as a means of "spacing out" and is found in various functional systems. Feeding and breeding territories are established and maintained; rivals are overcome and temporarily excluded from the reproductive process. Rank-orders are established.

(2) Intraspecific fighting behavior is, with few exceptions, arranged so that the conspecific is not killed. Animals capable of doing great damage usually have ritualized, nondamaging fights. Where this is not the case, means exist to prevent the destruction of or serious damage to a conspecific. Thus many animals are able to prevent further attacks by means of appeasement postures.

(3) These behavior mechanisms are already

present as phylogenetic adaptations with respect to perceptual as well as motor pattern organization.

(4) Most higher vertebrates are motivated by an aggressive drive to fight. However, this drive is only one of many in the "parliament of instincts."

(5) The bond uniting individuals who know one another develops during the course of evolution for mutual defense and protection. Love is, in a manner of speaking, the offspring of aggression.

(6) Humans are also imbued with an aggressive drive. It leads to an increased readiness to fight if not discharged through the performance of aggressive actions.

(7) While humans possess, analogous to higher vertebrates, inhibitions against killing conspecifics, the balance between the ability to kill and the inhibition to do so has been disturbed by the invention of the first weapon. Modern weapons technology further insulates soldiers even more against situations in which compassion for an enemy could be elicited.

(8) Man was evolved for life in individualized bands and the anonymous mass society places excessive demands on people. Thus, "man is not bad from his youth up, he is just not quite good enough for the demands placed upon him by modern society" (1963, p. 372).

(9) The drive to enthusiastically participate in fighting determines the structure of human society and its political organizations. Mankind is not aggressive and ready to fight because it is divided into parties and factions. Instead, mankind is organized to provide the stimulus situation necessary for the discharge of social aggression.

(10) Personal acquaintance and the unifying effect of an enthusiasm for shared ideals tend to blunt the aggressive drive. Furthermore, ritualized forms of fighting, such as sports, may serve to prevent the damaging effects of aggressions.

(11) At the same time, man feels friendship and love only on an individual basis and will power cannot change this. But Lorenz believes in "the power of natural selection . . . reason can and will exert selection pressure in the right direction" (1963, p. 299).

There was a strong reaction to this book. It was criticized by psychologists, sociologists, and behavioristically oriented anthropologists, and one is struck by the highly emotional tone of their rebuttals. They said that Lorenz' refer-

ences to inborn behavior in humans could be used to support arguments in favor of the status quo in human societies, and that Lorenz' hypothesis served as an excuse for past and future aggression. Thus, Erich Fromm wrote (1974): "What could be more welcome for those who are afraid, and who feel unable to change the destructive course of events, than Konrad Lorenz' theory that violence emanates from our animal nature and is the result of an untameable drive for aggression."

The accusation that Lorenz defends aggression and that his position is one of fatalism in the sense that what is inborn cannot be changed, can be refuted by his own statements. Lorenz never spoke of an "untameable" drive for aggression. Instead he pointed out the need for its control, which would require an investigation of the phenomenon: "We have good reason to consider intra-specific aggression in the present cultural and technological situation of mankind as the greatest of dangers. However, we shall not improve our chances to deal with it if we accept it as something metaphysical and unavoidable. However, we may succeed if we can unravel the causal chain of its natural causes. Whenever man attained the power to direct a natural phenomenon into a particular direction, he was able to do so because of his understanding of the underlying causes. The study of normal, species-preserving life processes, called physiology, provides the pre-requisite basis for the study of their pathology" (1963, p. 47). Clearly, Lorenz was not an apologist for aggression as his critics charged. He did, however, point out that the control of aggression cannot be accomplished simply by its suppression because the drive is too strong. Thus, harmless or even useful outlets for the aggressive drive need to be found.

Lorenz is also accused of drawing conclusions based on false analogies, but it is significant that he prefaced his remarks about human behavior with the following self-critical passage: "I may claim that the preceding chapters are natural science: the recorded facts are verified, as far as it is possible to say this of the results of a science as young as comparative ethology. Now, however, we leave the record of facts evidenced by observations and experiments on the aggressive behavior of animals and turn to the question of whether they can teach us something applicable to man and which is useful in circumventing the dangers arising from his aggressive drives" (1963, p. 220). Omission of

references to such statements by his reviewers, with the exception of Theodosius Dobzhansky, indicates that they attempted to build their cases by ignoring relevant facts.

It is unfortunate that Lorenz' critics did not contribute to a constructive evaluation of his hypotheses, since there are a number of points that do need to be questioned. That Lorenz would be the last to object to such criticism is indicated by his reaction to Lehrman's critique of the concept of "innate behavior." He never objected to criticism of his work as long as it dealt with the issues at hand. He continuously exhorted his students that as natural scientists they must be ever ready to throw favorite hypotheses overboard when they are no longer supported by the facts.

Any critical evaluation of Lorenz' ideas on aggression must call attention to the lack of a clear distinction between individualized aggression within and between groups. He later stated that if he were to write *On Aggression* again, he would draw a sharper distinction between individual aggressivity within a society and the collective aggressivity of one ethnic group against another. This is illustrated in Eibl-Eibesfeldt, *Krieg und Frieden aus der Sicht der Verhaltensforschung* (1975). Some of Lorenz' statements are far too general, such as the assertion that "the personal bond love arose in many cases from intra-specific aggression by way of ritualization of a redirected or threatening attack." Another such statement is that man's over-all psychological make-up is not suitable for modern mass society and thus a new man needs to evolve to fit the new condition. True, aggression is an important element of social bonds, but a good argument can be made that individual bonds have their origins in the mother-child relationship. The behavior patterns that maintain bonds within groups seem to derive primarily from behavior patterns involved in the care of the young and from infantile behaviors. With respect to aggression between groups, one can argue that it is possible, through an identification with shared symbols, to become friends with initially strange people; therefore one need not wait for a new man.

The focal point of many of the attacks on Lorenz was his concept of an aggressive drive. Although not all species have such an inborn drive, many do. The behavior of a species depends on its specific ecological demands. Even within the cichlid family, for example, there are great differences from one species to the next.

It is therefore not valid to say that Lorenz' ideas concerning aggression in humans have been disproven merely because it has been discovered that in one species of fish aggressivity is a reaction to stimuli rather than a spontaneous reaction.

In examining the situation in humans, much evidence supports the existence of an aggressive drive (Eibl-Eibesfeldt 1975). Numerous experiments have shown that anger can be dissipated by the expression of aggressive behavior. In addition, many societies have evolved customs that harmlessly channel the expression of aggressive behavior, and the results of neurophysiological studies have provided evidence in support of the hypothesis. In humans, the temporal lobes and the amygdaloid nuclei are implicated in aggressive behavior. If stimulated electrically, rage is exhibited. In temporal lobe epilepsy spontaneous rage occurs, and it is accompanied by an increased electrical activity in this region. It is a fact that neurons fire spontaneously, perhaps because transmitter substances are continuously produced and accumulate. At first, this may merely increase the readiness to respond, as evidenced in von Holst's experiments. The objection has been raised that if aggression were based on a spontaneous, neural motivation for an animal, this would be inappropriate since an animal may be driven to leave a territory in search of an opponent or an opportunity to fight. However, this does not necessarily follow, because the tie to a territory may be so strong that it prevents the animal from leaving. As a rule, spontaneous behavior is also subject to controls. Aggressive behavior is used, in a certain sense, like a tool. It is used in various functional systems to overcome obstacles, just as locomotor behavior can be used for various purposes. Perhaps this explains the similarities in their neural motivation.

Independent of these theoretical interpretations, Lorenz pointed out the need to study the species-preserving function of aggression in order to understand the aberrations of this drive in man in the hope of eventually controlling it. Most of Lorenz' hypothesis listed earlier do withstand critical examination and Lorenz has thus contributed significantly to understanding the phenomenon.

During the last two decades, Lorenz has had extensive contacts with psychoanalytically oriented physicians and behavior students. John Bowlby, for example, based his investigations

into the mother-child relationship on ethological theories, as did Erik H. Erikson. These interactions have led to much mutually rewarding discussion.

In common with Freud's psychoanalysis, ethology shares the view of a dynamic drive model. Freud seems to have been the first to recognize the importance of the dynamic aspects of behavior. Lorenz, building on von Holst, provided a physiological explanation. Furthermore, he interpreted the aggressive drive as an evolutionary adaptation, while Freud saw it as a quasimystical drive toward self-destruction. Besides the death wish Freud postulated the concept of the libido. All pleasurable expressions from sucking in infants to adult sexuality were thought to derive from this drive.

Freud's much quoted passage that a mother would be shocked if she knew that she behaved sexually toward her infant when she fondled and kissed him is wrong. Freud correctly noticed certain connections, but he erred in the direction of the course of evolution. We are witnessing here true parental care behavior which secondarily became a part of the bonding behavior that is part of sexual behavior (Eibl-Eibesfeldt 1970). In fact, no physiological basis has been found for the existence of only one primal, libidinal drive. Lorenz spoke instead of a "parliament of instincts."

Other areas of similar concern shared with psychoanalysis are based on the ethological concept of imprinting. Freud already pointed out one sensitive period during early development as well as the regularities of developmental stages that can be thought of as a kind of program that will run its course. With respect to specifics, ethologists' ideas about the developing human infant have gone beyond Freud's original formulations.

Ethologists find it hard to accept some psychological explanations. Thus, they interpret the Oedipal phase differently from classical psychoanalysts. During this phase of development children certainly experience conflicts based on rank-order, which in boys centers on their relationship with their fathers. To conclude, however, that each boy latently fears castration by his father as punishment for his Oedipal desires is farfetched, especially since there is probably an innate incest taboo. Humans, like other animals living in permanent family groups, seem to possess an inhibition against falling in love with a member of the opposite sex with whom

they grew up during a certain sensitive period in their development (Bischof 1972). A girl may, during this developmental phase, learn that in a patriarchal society boys are preferred. This may lead to conflicts in sensitive girls, but to postulate penis envy is probably more in the realm of a pathologic aberration than a norm. This is undoubtedly true also for a number of other psychoanalytical interpretations, which are reflected in a rather unfortunate terminology that many find objectionable. Thus, psychoanalysts speak of an anal-sadistic phase. With this allegorical terminology an attempt is made to characterize a phase in which the child learns to control his anal sphincter muscle, and employs his exploratory drive destructively by tearing up paper, toys, etc. What actually takes place can hardly be appropriate for the terms used. The term "sadistic" coming from pathology is not appropriate to describe normal exploratory behavior in a child, nor is control of the sphincter muscle the main concern during this period. However, we are dealing with a phase in which first attempts are made by the child to assert independent action by the active exploration of the environment. Curiosity, play, the beginning of speech are much better suited to characterize this stage of a child's life (Hassenstein 1973).

Another important area in which ethological and psychoanalytical theory differ is concerned with ethics. According to Freud, people act altruistically because they are afraid of their all-powerful fathers. According to Lorenz, certain ethical norms are inborn. The conscience and empathy with which people appreciate the situation of a companion are based in part on phylogenetic adaptations.

In his most recent book *Behind the Mirror* (1973), Lorenz attempted to understand the natural history of man's cognitive abilities. This is his most mature and probing work. Based on a kind of "hypothetical" realism, Lorenz tried to show that each adaptation reflects some external reality, however crude the model may be: "Life is not an image of something, it is knowing reality itself" (p. 326). With increasing differentiation, one speaks of higher development; the impressions that an organism has of its environment are also correspondingly more differentiated. This is true for physical characteristics as well as for sensory capacities and the capabilities of the central nervous system. "Adaptation is a kind of growth of the

transformation taking place in the organism and its environment," where the genome "learns" by a kind of teleonomic probing from successes and failures, just as the inquiring human mind learns, especially from its errors. Both processes can be seen as a cognitive matching with a reality that exists outside the organism.

Of course, it is possible to distort an adaptation, as in the case of errors in visual perception as a result of optical illusions. There are pre-conceptions in perception as well as in thought processes of which people need to be aware. Lorenz traced the evolution of cognitive functions through phylogenesis and cultural history and pointed out their achievements and limitations. Lorenz considered this work the basis for a contemplated critical examination of human societies, which should be provocative in its unconventional approach. Its biological epistemology will open new paths to philosophy.

Lorenz' work has left a deep and lasting imprint in the intellectual landscape. Almost all works about man have to deal with his ideas. The line of scientific inquiry that he, Tinbergen, and von Holst developed has long since been recognized, and in 1973, Lorenz, Tinbergen, and Karl von Frisch received the Nobel Prize for physiology and medicine for their work in ethology.

What Lorenz saw as the most important task of ethology, namely the biological investigation of human behavior, has meanwhile begun in a systematic fashion. Psychologists, psychiatrists, psychoanalysts, anthropologists, and pedagogues have had to come to grips with his ideas and an ethology of humans has begun to grow as a new area of scientific inquiry (Eibl-Eibesfeldt & Hass 1966).

IRENÄUS EIBL-EIBESFELDT
(Translated by Erich Klinghammer)

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LOVEJOY, ARTHUR O.

Arthur Oncken Lovejoy (1873-1962), acknowledged founder of the discipline of the history of ideas, organizing spirit of the American Association of University Professors, and for many years America's leading critical philosopher, was born in Berlin, Germany, on October 10, 1873, to a family with a long New England ancestry; his father, Wallace W. Lovejoy, a physician, had gone to Germany to continue his medical studies. Shortly after the family had returned to Boston, Lovejoy's mother died from overdoses of a drug. In grief and guilt, his father, abandoning medicine, undertook theological studies and became an Episcopalian minister. He educated his son along strict religious and classical lines, having him study Hebrew as well as Latin and Greek. During his four years as an undergraduate at the University of California from 1891 to 1895, Lovejoy was an editor of a student publication, wrote poetry, and above all, was inspired to the vocation of historical scholarship and philosophical thinking by his teacher George Holmes Howison. Howison was a spokesman for "personalistic idealism," which Lovejoy later described as an intellectual effort to introduce democracy into metaphysics. Resisting steadfastly his father's plea that he enter the ministry, Lovejoy attended Harvard University, studying for three years until 1898, most notably under William James and Josiah Royce. Lovejoy's first intellectual standpoint resembled Royce's absolute idealism, but gradually James's influence deepened; an eternal idealistic absolute came to seem inconsistent with the temporal character of experienced realities. Lovejoy never took a PH.D. degree; a devoted researcher and a prolific though careful writer, he nonetheless took to heart James's polemic against "the PH.D. octopus." A year in Paris in 1898/1899, spent largely in studying the history of religions, was followed in 1899 by an appointment to his first academic post, as assistant professor at Stanford University in California.

As a result of events at Stanford, academic freedom became a primary theme in Lovejoy's life. In 1900 the sociologist Edward A. Ross was dismissed from the faculty after he had advocated prohibiting the importation of Chinese laborers. Despite the urging of David Starr Jordan, the president of Stanford, that the faculty approve the dismissal, Lovejoy, after studying the evidence, concluded that Ross's academic free-

dom had indeed been violated. All Ross had done was offend Mrs. Leland Stanford, whose husband, a leading railroad builder, had employed imported Chinese labor. To make his protest significant, Lovejoy resigned from his post, but was thereupon summarily dismissed from the university in disregard of his effective date of resignation. He secured a post as professor at Washington University in St. Louis, where he taught from 1901 to 1908. He spent two years at the University of Missouri, then accepted an appointment at the Johns Hopkins University in Baltimore, where from 1910 to 1938 he was professor of philosophy. In 1932/1933 he went to Harvard University as visiting professor to give the William James lectures that subsequently became his great book *The Great Chain of Being* (1936); he was again visiting professor in 1937/1938.

While at Johns Hopkins, Lovejoy was the architect of the American Association of University Professors. In November 1913, he convened a handful of professors from seven universities to plan the organization of a society dedicated to the freedom and well-being of the academic profession. Finally, in January 1915, the organization was formally founded. Lovejoy became its first secretary; John Dewey its president. Soon Lovejoy was serving as a one-man committee, investigating reported infractions of academic freedom across the United States; he became president of the A.A.U.P. in 1919. Because of these activities, a proposal to appoint Lovejoy to the Harvard philosophy department was vetoed by President A. Lawrence Lowell; it had been argued by W. E. Hocking that Lovejoy could not be "domesticated."

The outbreak of World War I evoked a deep moral response from Lovejoy. His sympathies were strongly with the Allied cause; he thought that the famous 1914 manifesto of the 93 German intellectuals was a betrayal of their scientific and scholarly vocation. He availed himself of the pages of *The Nation*, for which he had frequently reviewed philosophical works, to express his views in several long letters. After the United States declared war against Germany in April 1917, Lovejoy worked with the morale section of the War Department and the Young Men's Christian Association in their respective educational activities. His chief contribution was to edit with Albert Bushnell Hart a *Handbook on the War for Public Speakers* (1917). Lovejoy's concern at that time to awaken "the moral vision and the moral passion of the entire

people" has been criticized in recent years, especially by leftist writers, as a departure from his principles, but to Lovejoy the threat to democracy presented by German militarism in 1917 was as real as the threat posed to world civilization in the next generation by Adolf Hitler and the Nazis. As early as 1933, Lovejoy warned the American Jewish Conference in a speech in Baltimore that Hitler was a menace to world peace. When war came again to Europe in 1939, he joined the Committee to Defend America by Aiding the Allies, and became leader of a group referred to as the "Hopkins war-mongers."

To Lovejoy, as a guardian of academic freedom, America's entry into World War I raised difficult issues. In its first four months, at least six cases arose of professors discharged upon alleged grounds of "disloyalty." Lovejoy warned that patriotic zeal should not be used as "a mantle for private intolerance," and as a vehicle for "hysteria" and "spiritual retrogression." Under his chairmanship, the A.A.U.P. Committee on Academic Freedom in Wartime condemned the trustees of Columbia University, whose actions against James M. Cattell in October 1917 for advocating a purely voluntary army resulted in his dismissal. Nevertheless, Lovejoy felt that professors who opposed American military actions should preserve silence during wartime, and he admonished American colleges in March 1918 against "facilitating the efforts of those who would repeat in America the achievement of the Lenines and the Trozskys in Russia."

With World War I over and the cultural stocktaking of the 1920s in progress, the Johns Hopkins University, under Lovejoy's leadership, became a center for the study of the history of ideas. A group of scholars began in 1922 to meet for "interdisciplinary" discussions; the next year Lovejoy was chairman of a committee to organize a History of Ideas Club. Its constitution defined the field as "the historical study of the development and influence of general philosophical conceptions, ethical ideas, and esthetic fashions, in occidental literature, and of the relations of these to manifestations of the same ideas and tendencies in the history of philosophy, of science, and of political and social movements." Lovejoy served as president from 1927 to 1929 and 1938 to 1939; more important, he presented eight papers during the twenty years from 1923 to 1952. He became the patriarch of scholarship, the symbol of scholarly

truth-seeking. A distinguished series of scientists, philosophers, and historians came to present their ideas on ideas, including Niels Bohr, John von Neumann, Marjorie Nicolson, George Boas, Jacob Hollander, Gilbert Chinard, Charles A. Beard, Dorothy Stimson, Carl Becker, and William F. Albright. The Johns Hopkins History of Ideas Club was one of those "invisible colleges" that from time to time fructify the advancement of science and scholarship. When the organizational initiative came from the City College of New York, chiefly from an instructor, Philip P. Wiener, for the founding of the *Journal of the History of Ideas*, Lovejoy served for a year as editor in chief after it began publication in 1940.

Lovejoy's philosophical work revolved around three "unit-ideas": temporalism in metaphysics, dualistic realism in epistemology, and emergent evolution in cosmology. The first was directed against the philosophical idealism that dominated American academic thought at the end of the nineteenth century as the last residue of New England transcendentalism. The tenet of dualistic realism, on the other hand, led Lovejoy into a powerful critique of the pragmatic theories of both James and Dewey. In his essay "Thirteen Pragmatisms" (1908, in 1963), Lovejoy began to develop a remarkable method for distinguishing the ambiguities in the pragmatic thesis; some of its variants, he held, led to doctrines that confused sociological utility with objective truth, while others confused mental with physical events, implying either solipsistic or extreme physicalistic consequences. The method was most trenchantly used in Lovejoy's volume of Carus lectures, *The Revolt Against Dualism* (1930a), in which he analyzed with the skill of a logical anatomist the efforts of Bertrand Russell and Alfred North Whitehead to overcome the distinction between mental and physical entities and to achieve "the unification of mind and matter." Lovejoy reiterated his standpoint of epistemological dualism—that the knowing event remains existentially distinct from the known—and his psychophysical dualism—that is, the irreducible distinction between mental and physical entities.

The human race, according to Lovejoy, had at a very early stage in its intellectual evolution arrived at "an unformulated working epistemology," its natural realism. Men founded their lives on five assumptions: that known objects were external to their own bodies; that in their memories and anticipations they were conceiv-

ing of events in a past and future not presently coexistent; that their knowledge did correspond to objective realities; that social experience brought them indirect knowledge of other people's feelings and ideas; and that phenomena, such as dreams, that did not satisfy a common verifiability belonged to a private, nonphysical domain. Such later discoveries as Olaus Roemer's, in 1675, of the finite velocity of light simply reinforced the dualistic distinction between sense data and their objects. These epistemological discoveries of mankind were for Lovejoy a heritage not to be discarded lightly for some philosophical fashion.

Lovejoy's psychophysical dualism meshed naturally with a third tenet that he called "emergent materialism." Lovejoy believed that there were "absolute emergences" in nature, that cosmic history was not a repetitious "barren shuffling-about of the same pieces." With the advent of living organisms and physical events a "transphysical emergence" had taken place. Although Lovejoy could accept no "generalized or cosmic meliorism," he expressed the hope that "there may yet emerge out of the latent generative potencies of matter . . . new and richer forms of being, such as no prescience of ours could foresee" (1927, p. 32). The notion of irreducible, absolute emergence was basic to Lovejoy's conception of human behavior and the social sciences. The human desire for approbation, which was based on self-consciousness, was, Lovejoy held, an emergent not derivable from antecedent physical or biological events; it was a "biological singularity" constituting a "great chasm" between simply conscious and self-conscious organic creatures. The emergence of self-consciousness itself he regarded as a "pure mutation" not genetically explicable, "one of the great points of discontinuity in nature" (1961b, pp. 85, 92, 106). Lovejoy's conception allied him with such social inquirers as Adam Smith and Thorstein Veblen, who had regarded the chief incentive to the accumulation of wealth as the pursuit of honor or as emulation.

During the first part of his life Lovejoy, like many contemporaries, was much devoted to work in social settlement houses. He worked with a Boys' Club and the Manse in Oakland in 1895, with workingmen's classes in Cambridge, with the Boston Children's Aid Society in 1896 and 1897, and as president of the St. Louis Settlement a few years later; at Johns Hopkins he served in a similar, if more "grandfatherly" capacity, with the Baltimore Boys' Clubs, and

afterwards participated in the National Child Labor Committee. Although he would not permit his philosophical views to be shaped by such activities, in the manner for instance of Dewey and George Herbert Mead, he concluded by 1911 that equality of opportunity could be restored only by reforms that "could hardly fall far short of a thoroughgoing system of nationalized industry." He criticized the schemes for profit sharing that became current after World War I; they would not, he wrote in 1921, affect the deeper causes of industrial unrest, and he concluded sadly that "nothing in modern history" encouraged the belief that the more privileged class would have the "civic virtue" to enact the necessary reforms in the distribution of wealth.

In *The Great Chain of Being*, Lovejoy explicated his seminal concept of "unit-ideas" as the basis for his method in the history of ideas. He defined them as "the elements, the primary and persistent or recurrent dynamic units, of the history of thought," the "implicit or incompletely explicit assumptions, or more or less unconscious mental habits, operating in the thought of an individual or a generation." Their exploration involved a "philosophical semantics," "a study of the sacred words and phrases of a period or movement." Lovejoy meticulously traced the way the notion of a great chain of being, with all the possible varieties of existence actualized, had controlled much of Western philosophical and scientific thought for two thousand years. Several principles were axiomatic in this approach: specifically plenitude, continuity, gradation. In other words, it involved the notion that the entire range of the conceivable diversity of things—species, particles, or societies—had to be realized; that any possible intermediate type between two neighbors had to be exemplified; and that all would be arranged according to a scale of perfection. This model, long employed for a theistic cosmology, had been "temporalized" or secularized in the philosophy of evolutionary progress in the eighteenth and nineteenth centuries.

The history of ideas, according to Lovejoy, was moved by a combination of temperamental, irrational motives interacting with logical "considerations." Lovejoy never developed a theory of what these irrational motives were, but he felt that what made a philosophy attractive to many adherents was the feeling it inspired that they were being initiated into a kind of Eleusinian mystery. Moreover, from one generation to the next, basic shifts in philosophical propensity

would take place, as when his generation, for instance, rebelled against the dominant absolute idealism. "The need for a new sort of philosophic *Eleusinia* is recurrent among the cultivated classes every generation or two; it is a phenomenon almost as periodic as commercial crises," wrote Lovejoy in 1913 (p. 254). Lovejoy, regrettably, never undertook to explain in the 1930s what type of edifying Eleusinian motive was the mainspring of the growing vogue of logical positivism. His conception of human nature was indeed that of a student of the seventeenth and eighteenth century moralists: men, in his view, were mainly irrational; La Rochefoucauld had been right in saying that the head was the dupe of the heart and John Adams correct in asserting that men's arguments were mostly rationalizations. The task of the history of ideas, according to Lovejoy, was akin to that of psychopathology, which helps alleviate complexes by making conscious their origins (1948, p. 234).

It was probably the strange phenomenon of anti-intellectualism among intellectuals, exhibited in such writers as Henri Bergson and Georges Sorel, that led Lovejoy and his collaborator George Boas to probe the varieties of "primitivism" in their book *Primitivism and Related Ideas in Antiquity* (1933). Cultural primitivism articulated "the discontent of the civilized with civilization" and could take "soft" or "hard" forms, a longing either for the idyllic or the harsh. Tracing the protean meanings of the "state of nature," Lovejoy noted the way all left-wing movements included among their ideological ingredients a combination of such longings—whether for the propertyless, stateless, spouseless, sinless, machineless states of nature, and, in a case like Bernard Shaw's, for a vegetarian overcoming an estrangement from animals.

Lovejoy died on December 30, 1962, after stoically enduring several years of total blindness, during which he continued to work with a graduate assistant in preparing his last books. The next decade saw a widespread recrudescence of various forms of primitivism in the New Leftist ideologies.

Convinced that the progress of philosophy depended on cooperative discussion, Lovejoy during his life engaged in some notable debates. A literary controversy with the Harvard humanist Irving Babbitt on the meaning of "romanticism" in 1920–1922 led Lovejoy to argue in behalf of the romantics that "there can be im-

moderation even in the preaching of restraint; . . . [in] the contrary error of an apotheosis of inhibition." Against behaviorism, highly fashionable in the mid-1920s, Lovejoy directed his "paradox of the thinking behaviorist." He debated logical positivism with Rudolf Carnap at the 1935 Baltimore meeting of the American Philosophical Association and critically analyzed historical relativism when that doctrine was becoming influential in the latter 1930s. After World War II he parted company with the A.A.U.P., for he had concluded by 1949 that confirmed communists, as enemies of academic freedom, had no right to membership in the academic community. He felt that the A.A.U.P. had been caught up by the "trick phrase" of "guilt by association" into failing to think through the significance of allegiance to the Communist party. For Communist party members, Lovejoy reiterated in 1952, "are collaborating with an organization which demonstrably seeks to destroy academic freedom wherever it can, and has already succeeded in doing so throughout a large part of the civilized world; and that such collaboration is an act of disloyalty to the first and fundamental obligation of the scholar's profession" (1952, p. 88).

A lifelong student of all climates of opinion, winds of doctrine, slogans, and ideologies, Lovejoy tried as a scholar to maintain a cool, critical detachment from all. He did not believe a scholar should join a trade union or be a partisan activist or office seeker, for he felt that any such affiliation would adversely affect the impartial analysis that was the duty of the scholar and scientist. He argued this standpoint vigorously in the pages of professional journals. When in 1933 he delivered the lectures that became *The Great Chain of Being*, the audience dwindled to a handful; logical positivism was becoming the fashion, and even historians in that Marxist era were indifferent. His book became recognized as a classic only after World War II. His dualistic realism, rejected as "meaningless" by positivists in the 1930s, also returned likewise after World War II in the writings, for example, of Karl Popper on scientific method and the interpretation of theories. Arthur E. Murphy called Lovejoy "the Edmund Burke of the epistemological revolution" (1931); yet Lovejoy thought of himself as more like David Hume, unwaveringly loyal to the understanding and determined not to discard the hard-earned epistemology of the plain man unless the evidence was convincing. In 1976 the logician Jaakko

Hintikka characterized Lovejoy's *The Great Chain of Being* as "the most influential single work in the history of ideas in the United States during the last half century." In philosophy, history, and social theory, Lovejoy was probably the least ideological of American thinkers. That is why his work, though never acquiring a vogue like that enjoyed by some contemporaries, may prove to be more enduring. It embodied, as did Lovejoy in his personality, the principle of integrity in American scholarship.

LEWIS S. FEUER

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LURIA, A. R.

Alexander R. Luria (1902–1977) was born in the provincial Russian commercial center of Kazan. His father, Roman A. Luria, was a physician with a strong interest in problems of

psychosomatic medicine. Before the revolution of 1917 the Luria family was precluded, by virtue of its Jewish origins, from moving to St. Petersburg, where the elder Luria had been invited to a prestigious medical institute. After the revolution both elder and younger Luria moved to Moscow, each in turn taking up a career in medicine. But medicine was not Alexander Luria's first passion. Despite his father's judgments, he was influenced by current enthusiasms to take up a career closer to the affairs of men as he had experienced them.

When Luria entered the University of Kazan, the institution was in turmoil produced by the advent of Soviet authority and its need to reconceptualize higher education in a new Communist society. Classes were held irregularly; professors (especially in the humanities and social sciences) had to reconstruct their courses, discarding a classical education for an unknown, but assuredly radical, future. Many students passed on the strength of their instructors' activism; standards were uncertain. But the chaos also encompassed great energy. A serious student with determination could fashion his own education and construct his own career.

The education that Luria constructed was heavily tilted toward that amorphous set of topics in the social and biological sciences that spanned the discipline now defined as psychology. He was strongly impressed by two seemingly contradictory schools of thought. First, he endorsed the *objective* methods of study that were just appearing in the writings of Vladimir M. Bekhterev and Ivan P. Pavlov in Russia, Wilhelm Wundt in Germany, and Edward L. Thorndike in the United States. Yet much of the objective psychological research he encountered seemed arid and devoid of the complex, emotion-ridden, highly organized behaviors that make people exciting objects of analysis. Reading Sigmund Freud and Carl Jung, he was struck by the possibilities of psychoanalysis as a link between documentary accounts of living individuals and the generalizing, law-seeking goals of experimental, natural sciences. What science needed was an objective *reale psychologie* (a term he borrowed from the German romantics) that submitted Freudian dynamic mechanisms to experimental analysis. Inspired by this goal, he and several fellow students founded the Kazan Psychoanalytic Circle. As a student he conducted research using modified psychoanalytic techniques, combining them with reaction time measures and indices of physical

fatigue as he sought the synthesizing technique to support his early vision.

Only one of these early works on "objective, dynamic, *reale psychologie*" has been translated into English (1978), but this line of research was the basis for the first two-thirds of Luria's first work in English, *The Nature of Human Conflicts* (1932). The central idea of the book is extremely simple. Human emotions are difficult to study in the laboratory for two reasons. First, real emotions cannot be simulated by such artificial events as firing a gun behind someone's head. These evoke only phasic excitation, not the tonic motivation that organizes actual behavior for hours, days, and months. Second, emotions should not be studied directly, as *psychological* phenomena, but rather through their effects on ongoing, voluntarily controlled behavior—in the way that emotions affect behavior outside of the laboratory.

To overcome the difficulty of such empirical study, Luria developed first a laboratory technique designed to trace the influence of emotions on voluntary behavior which he called the "combined motor method." The procedure started with a simple voluntary response on the part of the subject—squeezing a pneumatic rubber bulb—to a signal from the experimenter. When this response to a simple command ("squeeze") was stabilized, more interesting verbal stimuli were introduced, and the subject was required to free associate while continuing to respond to each stimulus by squeezing the bulb. Luria's intent was to determine the way in which emotion aroused by the verbal stimulus disrupted the smooth flow of the voluntary response.

On the basis of this technique, Luria went on to develop laboratory models of real-life conflict situations. When educational authorities at Moscow University decided to purge the student body of those whose poor academic work or whose family backgrounds disqualified them, Luria and his colleagues conducted their research by taking students who were on line awaiting interrogation and plying them with verbal stimuli whose emotion-inducing effects seemed certain; "examination," "expelled," "purge." Later, the same technique was used with criminals who were to be questioned by the district attorney. These studies amply illustrated the objective *reale psychologie* that would be one of Luria's unique contributions to psychology.

Aside from its intrinsic value, the third part

of *The Nature of Human Conflicts* is especially interesting in presenting the earliest formulation in English of the concerns that would dominate the remainder of Luria's scientific career: the role of speech in the development and organization of mental processes; the development of intellect; mental retardation; the brain correlates of behavior; and even the cultural history of mind. In retrospect it is clear that Luria here was introducing the American reader to basic concepts brought into psychology by Luria's mentor and colleague, Lev Vygotsky. Beginning in 1924, Vygotsky, Luria, and Alexei N. Leont'ev had set about to reconstruct psychology on a far broader scale than Luria's efforts to combine laboratory and real-life psychology. Vygotsky's unique contribution was to set psychology in a historical and cultural framework. Psychological processes ordinarily thought of as special "mental" functions were treated as complex forms of activity, the structure of which is provided by the structure of one's social milieu, which in turn has been shaped by the social history of one's culture. The psychologist's task became the study of complex functional systems constrained on the one hand by biology and on the other by society.

In the early 1930s the cultural and historical context of psychological activity was the object of Luria's research in two expeditions to central Asia, where traditional pastoral culture was being transformed by massive collectivization, literacy campaigns, and formal schooling. Luria's reports of a qualitative shift in psychological functioning as a result of the cultural shifts that the peasants were undergoing were consistent with Vygotsky's theory and seemed to justify the Soviet government's program of providing the material basis for modernization in its outlying areas. But critics emphasized the other side of the coin: Luria and Vygotsky were severely criticized as Great Russian chauvinists who had denigrated the status of national minorities. A small volume describing some of this research was published in Russian in 1974 and translated into English in 1976.

The years immediately following Vygotsky's death in 1934 were especially difficult for Luria. Partly as a result of the furor over the work in central Asia, partly because of strong reaction against psychological testing in general, and partly because of repressive measures associated with the "cult of personality," Luria's work veered in a number of unexpected directions.

For a while he worked at the Medico-Genetic Institute in Moscow, where he conducted pioneering studies with identical and fraternal twins in an effort to disentangle natural and culturally shaped psychological processes as they interact over the course of human ontogeny. When this line of work also ran into political difficulties, Luria decided to complete his medical training (he had taken medical school courses on and off since his university days in Kazan). Upon completing medical school in Moscow in 1936, he worked in the neurological clinic.

These vicissitudes in his training prepared Luria to make maximum use of the tragic opportunities for brain research that arose when Germany attacked the Soviet Union in 1941. For the next several years he worked in surgical wards, improving methods for diagnosing and restoring disrupted brain functions caused by head wounds. This experience established his reputation as a neuropsychologist.

In the late 1940s events again intervened, and Luria turned from neuropsychology to the study of developmental abnormalities associated with mental retardation. This period coincided with an era in Soviet medical and social sciences when the use of Pavlovian neurophysiological concepts was virtually demanded of all researchers, and Luria's writings reflect the influence of this requirement. Fortunately, some of Pavlov's speculations about the influence of language on the structure of reflex processes in human beings could be construed as similar to Vygotsky's ideas about the mediated nature of higher psychological processes. Luria found enough similarity to permit him to pick up a line of research that had been dormant for many years.

In the late 1950s, the times again permitting, Luria returned to the area of neuropsychology, which remained his major concern until his death in 1977.

MICHAEL COLE

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LYND, ROBERT S. AND HELEN MERRELL

Robert Staughton Lynd might be described as the Sinclair Lewis of sociology. His books *Middletown* (Lynd & Lynd 1929) and *Middletown in Transition* (Lynd & Lynd 1937) provide the sociological flesh that makes Lewis' satiric classics, *Babbitt* and *Main Street*, comprehensible as studies in the culture and mores of midwestern America.

Lynd was born in New Albany, Indiana, on September 26, 1892. His major research works, undertaken and coauthored with his wife Helen Merrell, were actually performed in Muncie, Indiana, not far from his birthplace. His background was modest, but he graduated from Princeton University in 1914, and from there went on to earn a Bachelor of Divinity degree at Union Theological Seminary in 1923. A doctorate from Columbia University was granted in 1931, after the publication and in recognition of the first Middletown study. Until his retirement in 1965, he was associated with Columbia. He died on November 1, 1970.

Lynd's first major job was as managing editor of *Publisher's Weekly*, the trade magazine of the publishing industry. His next position was director of the small city project of the Institute of Social and Religious Research, which directly, albeit modestly, underwrote the Muncie research. *Middletown* may be the first sociological work to be distributed and promoted to the general public as a trade book. According to Helen Lynd, the book was displayed in bookstore win-

dows alongside the leading novels of the time. The relationship between his sociology and the quality of writing was at least partially due to his vocational background in publishing, then a rare training ground for a sociologist. Recalling him at the time of his death in 1970, Seymour Martin Lipset noted that Lynd not only "devoted an enormous amount of time to his students," but that he was always available to help them rewrite, edit and even restructure their papers. His publishing experience clearly remained with Lynd as pedagogue and as researcher.

Today, Lynd would probably be described as an anthropologist of complex organizations. Certainly his work defies easy labeling. Not since Alexis de Tocqueville's *Democracy in America* (1835) have we had such a careful analysis of the daily life of America; middle America in this case. The book subjects one Indiana community to the same kind of intense scrutiny that de Tocqueville gave the entire United States. *Middletown* illumined, for a generation of social science, the essence of the American way of life. Probably no other single work published between World War I and World War II so precisely and devastatingly delineated what the nation had become. *Middletown* was described by H. L. Mencken as "one of the richest and most valuable documents ever concocted by an American sociologist" (1929, pp. 379-381); and by Stuart Chase as "an unparalleled work: nothing like it has ever been attempted; no such knowledge of how the average American community works and plays has ever been packed within the covers of one book" (1929, p. 164).

Middletown makes little conscious effort to posit the centrality of one variable or factor over another. It is divided into six large sections. The first, on the economy, documents "Getting a Living." The second and third sections are concerned with family life, linking problems of housing, child rearing, food, clothing, and schooling. The fourth, and probably the most innovative section, is on leisure. This material includes early mass communication research. It analyzes the leisure activities of middle Americans in pictures and periodicals, selecting and viewing, precisely in terms of mass communication. The fifth section concerns religious observance and practices, analyzing varieties of Protestant worship, but also, showing how organizations such as the YMCA (Young Men's Christian Association) link religion to community. The notion of community organizes the

final section, showing how community is related to the machinery of government. The very fragmentation of community points out the insignificance of bureaucracy in the social life of Middletown. These last sections clearly owe much to Lynd's activities in the mid-1920s as a missionary preacher in the oil fields of Montana.

The book took several years to produce. Robert Lynd lived in Middletown with his wife Helen for one-and-a-half years; their assistants lived there for an additional half year. Tables imaginatively illustrate the book; and while some of the data provide only a careful reworking of state and national data, other tables concern sources of disagreement between children and their parents, and books borrowed in the adult department of the public library. These show an imaginative concern with intimate detail rare in the annals of sociology up to that time.

What gives added character to *Middletown* is its historical specificity. The Lynds provide a documentary accounting of the life of a town at two selected periods: 1890 and 1924, rather than attempting to do a detailed study of the history of the intervening years. Such cross-sectional analysis provides a sensitive appreciation of the cultural tension between past and present generations. Middletown is in retrospect best seen as an analysis of the secularization process in American society: a veritable model of why modernization occurs and how social change takes place in an advanced industrial society.

Middletown in Transition is another path-breaking effort in a tradition of reevaluating and reanalyzing data. It examines what happened in the decade between 1924 and 1935. The Lynds took seriously critiques of their earlier work. Their follow-up study, while lacking some of the historical possibilities of the earlier effort, builds upon that earlier work and attempts to apply techniques that had evolved in social science in the intervening decade in sociological history. As John Madge (1962, p. 149) sagaciously noted: "If Middletown had changed it is also necessary to substantiate the claim . . . that there had been a profound development in the thinking of the Lynds."

Not only did the Lynds return to study the same town; doing so they clearly changed their own estimates of what was important. The machinery of government, for example, was no longer subsumed under community activities, nor was religion given a whole section. It was

reduced to a chapter. This reflected changing mores of American society: namely, progress in the secularization process that the Lynds discussed at the end of *Middletown*, and also a sense of new problems emerging in the depression of the 1930s. The world of Sinclair Lewis' early novels had broken down. Class bias gave way to class antagonisms, stating the facts yielded to making clear the sources of power. While *Middletown's* citizenry continued to retain the values by which it lived, the impact of economic chaos at home, and fascism and socialism abroad, compelled *Middletown* to face both ways. Trade unionism became acceptable; reluctant adaptation to the new world became inevitable. If the follow-up study ends on a note of uncertainty, taken together the *Middletown* studies remain a most significant record of this period in American social life. The Lynds' ability to weave ethnography, stratification, and quantitative data into a meaningful whole has rarely been equalled in sociological literature. One might wonder if their open-ended choice of methodologies makes such broad-ranging work currently suspect. There is strong evidence that Lynd himself had serious questions about how generalizable such field researches actually are.

In *Knowledge for What?* (1939), a book subtitled: *The Place of Social Science in American Culture*, Lynd attempted to come to terms, indirectly at least, with a new methodological emphasis in American sociology. He argued that scholars have become technicians who would lecture on navigation while the ship goes down. This book has a bitter tone; it reveals pessimism that even if the new methodological sociologists should take the wheel, they would not really know how to steer a meaningful course. It is not that Lynd thought social scientists should go in for "pretentious soothsaying," still he recognized that a sense of the fragility of the future should not result in the sorts of inhibitions that make broad-ranging social science research unpalatable.

Lynd clearly was a sociological pragmatist, urging a careful middle course between what C. Wright Mills was later to call "abstract empiricism" and "grand theory." This approach was underwritten by a strong Columbia tendency to emphasize culture over society—a tendency that Lynd very much shared. From John Dewey in philosophy to Franz Boas and Ruth Benedict in anthropology, the emphasis was on a cultural framework, subsumed under society, economy,

and the polity. In a special way, Lynd was like a swan in a department where he seemed to be increasingly perceived, by some at least, as an ugly duckling. It was his philosophical anthropology, rather than a lack of statistical methodology, that ultimately frustrated Lynd and led him to shift his priorities from intellectual pursuits to departmental matters.

Lynd was involved in bringing the best scholars to the sociology department of Columbia, even when he doubted the efficacy of the methods used. The methodological wing represented by Paul F. Lazarsfeld and the Bureau of Applied Social Research had Lynd's unwavering support. Although they were intellectually on different wave lengths, Lazarsfeld and Lynd remained close personal friends. Lazarsfeld never forgot the role of Lynd in securing him a position at Columbia in 1940, nor the place of the *Middletown* study in his own community research efforts of the 1930s (Jahoda, Lazarsfeld, & Zeisel 1933). Lynd, for his part, made frequent reference to Lazarsfeld's study of the Austrian village of Marienthal in *Middletown in Transition* (Lynd & Lynd 1937, pp. 146, 179, 201, 254–255, 385). Lynd also shared with Robert K. Merton a concern for the middle range of social research. He was probably intellectually closer to Merton than to anyone else among the senior staff in the department of Columbia, and they worked closely on decision-making levels in the department. Lynd was also largely instrumental in bringing Mills to Columbia from the University of Maryland. He was central in a postwar crop of social scientists, headed by Lipset, who in many ways continued the dialogue about culture and democracy in new forms and in a postwar crisis period. Lynd, however, broke his silence between 1939 and 1956 long enough to write an extremely provocative, even crucial, critique (1956) of Mills's *Power Elite* (1956).

When one takes into account the paucity of Lynd's writings between *Knowledge for What?* in 1939 and his review essay on Mills in *The Nation* in 1956, the importance of the critique becomes self-evident. Lynd had been preoccupied with the development of a theory of power and democracy ever since *Middletown*. For Lynd, power as a social resource was absolutely necessary for the operation of society. Like physical energy, power could be harnessed for human welfare or corrupted by misuse. The development of democratic goals and the enhancement of a pluralistic national culture is, there-

fore, a responsibility of any sociological critique of power. Jeffersonian emphasis on democratic life is precisely the most outrageous hypothesis contained in *Knowledge for What?* Lynd shared with Mills a concern for the proper use and applications of power which he too had found much abused by elite groups. Yet he chides Mills for failing to undertake an analysis of power that extends its meaning for democracy. According to Lynd, the chief task for the observer of power is developing a theory of power for a given society. But according to Lynd, this was not what Mills aimed at. He was sorely out of sympathy with Mills's lack of commitment to a liberal democratic ethos and consequently finds that his ambiguous "exposé" lacks concreteness with relation to America as well as any sense of meaningful alternatives. Lynd also found elite analysis in social science limited, if not distasteful, because it obscured or ignored the basic characteristics of a given social system. It bred a superficial analysis that amounted to a way out of dealing with capitalism, socialism, and class structure. In this sense, Lipset picked up on this sense of Lynd's frustration, and his *The First New Nation* (1963) in some sense proceeded along the lines indicated by Lynd in his critique of Mills.

It would be unfair to think of Lynd's contribution as residing solely in the work of his students. Lynd was close to present-day Marxist analysis of American society; certainly his claims that Mills overlooked important evidence linking present-day American capitalism and the capitalism of the nineteenth century struck that note. Lynd indicates that Mills did not systematically analyze the American economy, and that by focusing on great changes, Mills failed to account for property as a power base linking the centuries. For Lynd, social science needed to understand the chief characteristics of the American system and not a given institution within the social order. Finally, Lynd breaks with Mills by assuming that the capitalist character of the United States defines the quality of society in the United States from the outset.

By all reports, Lynd was fair and tough, deeply committed to the idea of graduate education and to sociology itself as a cultural transmission belt. He also had a lifelong commitment to the Columbia style of education as a civilizing process, civilization itself being measured by its advanced educational institutions. He was a plebeian comfortable in a world of patricians.

To his lasting credit, the values he espoused and lived by remained consistent and consonant. He linked the sociological tradition and the problems of social science with the democratic culture and the larger problems of society.

Helen Merrell Lynd, two years his junior and his co-worker, carved out a career in many disciplines. She was born on St. Patrick's Day in 1894. Her parents were devoutly religious, with that element of social justice characteristic of many midwestern Congregationalists. She graduated from Wellesley College in 1917, where the strongest single influence was Mary S. Case who introduced her to philosophy, particularly Hegel, and according to her colleague at Sarah Lawrence, Bert J. Loewenberg, "gave her an abiding zest for both" (1965, p. vi). She married Lynd in 1921, taught for many years at Sarah Lawrence College, and has remained in the New York City area since her retirement in 1965. If the word *polymath*, someone learned in many fields, has any meaning, it certainly applies to her. Not only is she coauthor of the famed *Middletown* series, which alone would make her a figure to contend with in sociology, but she can also claim a place in the disciplines of history, psychology, and philosophy. She was entirely at home with the poetry of Shelley, the plays of Shaw, and the novels of Dostoevsky. She was versed in the technical literature of an amazing variety of fields—from the philosophy of science to experimental psychology. Perhaps this breadth was essential to a work like *Middletown*, which in its very nature transcended many disciplines and many imageries.

After the completion of *Middletown in Transition*, Helen Lynd carved a path of her own, starting with her remarkable book *England in the Eighteen-eighties* (1945), a work in social history done initially as a doctoral thesis under the supervision of the dean of history at Columbia, Carleton J. H. Hayes. The impact of *Middletown* showed in the organization of *England in the Eighteen-eighties*. It is divided into "Material Environment," "Environment of Ideas," "Political Parties," "Organized Labor," "Religious Education," and "Organization for Change." There is the same dialectical tension between the old and the new; the discrepancy between material abundance and satisfaction of human wants on the one hand, and the continued poverty of the masses on the other. Helen Lynd understood England in the 1880s as being involved with problems of social organization compatible with

democratic individualism, a problem in England then and in the United States fifty years later.

Helen Lynd's style is wide-ranging, with a transparent clarity that disguises the seriousness of her efforts. She worked out the essential tension between freedom and authority in a series of discussions of party life: namely, the tension between conservatives and liberals, between organized labor and what might be described as agitators and reformers, and between the High Church and Methodist Quakers and other non-conformists, between the crude barbarism of the private schools and the tragedy of lower-class education, what were called the ragged schools for the ragged classes. The work is informed by a strong sense of the social contradictions in British society; a series of unresolved conflicts looked at from a decade involving the principal political actors, writers, and playwrights of the time, who illuminated the central themes of freedom and authority.

While this work seems remote from her classic volume a decade later, *On Shame and the Search for Identity* (1958), in a way it reveals the same sense of dialectical tension in concrete settings. Even in discussing such psychological categories as shame and guilt, the nature of language, and clues to identity, she retained a lively sense of the concrete, constantly illustrating her theme with wide-ranging references to the scientific and literary leaders of the time. It remained characteristic of Helen Lynd that she referred to work as wide-ranging as that of C. P. Snow, Norbert Wiener, and Alfred Korzybski, all with a gracious weaving of information and ideas that in lesser hands could easily have fallen apart. This book shares with her earlier work a strong democratic impulse. The authority of the earlier work became a search and realization of identity. Helen Lynd distinguishes between guilt, which is a response to standards that have been internalized, and shame, which is a response to criticism or ridicule by others. Guilt, she argues, is centrally a result of a transgression, a crime, a violation of a specific taboo or legal code by a definite voluntary act, whereas shame is linked to uncovering, to exposure, to wounding, to experiences of *exposé*, and to peculiarly sensitive and vulnerable aspects of the self.

This work is far more than a purely psychological account of pleasure and pain, and reward and punishment. It involves a general theory of personality development, linked to the

evolution of historical thought. The work of Georg Simmel and Dorothy Lee plays a large part. Helen Lynd appreciated the extent to which concepts of psychological analysis are linked to mechanisms of social control intended to minimize conflict. But whether such reduction of conflict is good is warranted not by personality adjustment but by historical tendencies. This made Lynd's work quite different from conventional neo-Freudian writings of the 1950s. Her approach to questions of: "Who am I?; Where do I belong?" was strongly linked to sociology and history. Showing how such questions are formulated in ancient, medieval, and modern times, she observed that notions such as pride or shame are linked to general theories of religion, theology, and ideology. For Lynd, it is not the sin of pride but the capacity of pride to transcend shame, and therefore, to reach a new level of identity or even lucidity, that becomes central in raising consciousness. Unfortunately, she provided few clues as to how the guilt axis and the shame axis can be resolved by creating a pride-humility axis. Still, because at the time the social sciences emphasized intense social control and negative reaction to deviance, *On Shame and the Search for Identity* is more than a product of a generation in itself. It is also part of Helen Lynd's long-standing commitment to the idea that individual freedom is integrally linked to social democracy.

Critical acclaim for *On Shame and the Search for Identity* was widespread. Psychiatrists felt her work to be of seminal importance. Franz Alexander noted that "Mrs. Lynd's study goes further in depth and in comprehensiveness than any other contemporary writing on the subject. It is a sensitive, highly suggestive discourse on that most human of all faculties—reflection of the self on the self" (1958, p. 7). And Theodor Reik added that "her perceptiveness and sensitivity, especially felt in her differentiation of guilt and shame, as well as her intellectual sincerity and the originality of her observation, made her book a remarkable work" (1958, p. 19). He might have added doubly so, since Helen Lynd was trained in history, did a pioneering work in sociology, and taught in philosophy. Helen Lynd, like Robert Lynd, revealed that powerful element of free-thinking autodidacticism that was a family trademark.

A number of her important occasional writings were collected in *Toward Discovery* (1965b). In his introductory essay, Bert J. Loewenberg

properly notes that "Helen Lynd is concerned with the context of discovery; the environment of ideas; education in contemporary society; and the nature of historical objectivities" (1965, p. vi). He also understands that ultimately, for Lynd, discovery was really a way of growth as well as a technique of inquiry, and that to discover in a true sense also involves a diversity of methods. In this collection of papers all of these themes are amply illustrated.

In the 1950s, Helen Merrell Lynd achieved political notoriety by becoming courageously involved in the response to McCarthyism within university life. Her essay "Truth at the University of Washington" (1949) took to task university administration and faculty supporters. Various tenured professors were found to be competent scholars, objective teachers without academic fault, but members of the Communist party and hence incapable of objectivity. Such issues deeply divided the academic community. Helen Lynd was always on the side of the victims of McCarthyism; even her occasional papers showed the same tension between freedom and authority, identity and guilt. Her writings went far beyond placid formalism. During a period when nearly any defense of Communist party members was tantamount to inviting disaster, she was able to write:

Freedom and truth must be sought in the world we live in, not in a vacuum. With the worst that anyone can say about the Communist Party, I cannot discover any reading of this evidence about what has happened at the University of Washington that supports the belief that there can be more dictatorial power over teachers in the United States by the Communist Party than by Boards of Regents; or that the search for truth is more threatened by Communists than by arbitrary action of Boards of Regents and Canwell Committees. I cannot discover any readings of this evidence which supports the belief that purging Communists is in the interests of independent teaching, or of democracy. ([1949] 1965, p. 135)

If Robert Lynd had a clear impact on his intellectual progeny, Helen Lynd had an equally powerful impact on familial progeny. They had two children, Andrea and Staughton. The latter in particular, as evidenced through his own writings in history and social science, and his involvement in everything from the anti-Vietnam War movement to legal advocacy of organized labor in the midwest, exhibits a fusion of radical ideas and social action. Helen Lynd's final statement to the graduating class at Sarah Law-

rence in 1964, stands a fit epithet to her careers and writing and those of her husband: "So we cross the bridge into a new country. We go alone. But we take with us some knowledge of what it means to probe deeply into new worlds of learning and to glimpse all that lies beyond and is yet unexplored. And we take with us the gaiety, the delight, the sustenance of having known each other here—a knowing that will continue with us. We go in expectation of what may lie ahead" (1965a, p. 275).

IRVING LOUIS HOROWITZ

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McDOUGAL, MYRES S.

Chronology. Myres Smith McDougal, the founder of the New Haven school of jurisprudence, was born in 1906 in the farming community of Burtons, Mississippi. His father was a country doctor and something of a local political boss. McDougal's childhood blended close, trusting family ties with prudent cautions against the machinations of strangers, and these early impressions seem to have become tenets of McDougal's thought, in his emphasis on the importance of stable relationships on which one can rely, the human agency in political organization, the continuous relevance of power, and the practical importance of learning.

McDougal's father wanted him to enter state politics and become a U.S. senator. After attending local schools, McDougal studied classics and then law at the University of Mississippi, earning a B.A., M.A., and LL.B. After a brief stint teaching Latin and Greek at Mississippi, he devoted his academic career entirely to law. As a Rhodes scholar, he studied law at St. John's College, Oxford, came under the influence of W. S. Holdsworth, and received his B.C.L. in 1930. Though invited to teach at Oxford, he elected to return to the United States to take his doctorate (1931) at the Yale Law School.

McDougal arrived at Yale in 1930, trained in a formal positivism that viewed law as a body of rules and principles and that employed a method of rudimentary, though often elegantly expressed, syntactics not advanced yet to even the crude and exaggerated empiricism of C. C. Langdell's case method. Yale was then the cen-

ter of legal realism, a diversified jurisprudential movement most of whose members sought to annihilate positivism, expose rules as camouflage, and reveal law as no more than decisions of human beings who responded, with varying degrees of consciousness, to political, economic, psychological, and even organic stimuli. McDougal was converted to realism and published prolifically, with a convert's zeal and intimate knowledge of the enemy's mind.

As the realists demolished the traditional tools of the law, those who wished to rebuild turned to the social sciences for new ones. Yale became particularly hospitable to anthropologists and social psychologists. McDougal, who returned there in 1934 as an associate professor after three years at the University of Illinois, came under the influence of the anthropologist Bronislaw Malinowski and, to a lesser extent, the psychologist Edward S. Robinson, and continued to read widely in the social sciences and philosophy. By the late 1930s he had concluded that realist iconoclasm was only prologue. Now that the inadequacies of the traditional theories had been exposed, it was necessary to create a jurisprudential system appropriate to democracy and to facilitate the performance of the intellectual tasks that he believed lawyers would perform in the post-war reconstruction of American and world society.

In 1935 he became associated, quite fortuitously, with Harold D. Lasswell, and the two began a close collaboration. McDougal was instrumental in bringing Lasswell to Yale, and in 1943 they published "Legal Education and

Public Policy," their first joint exposition. Lasswell's insistence on contextuality, his wide-ranging and creative use of methodology, his psychoanalytic applications, and his refined conception of power, combined with his problem-solving orientation, provided myriad points for common interest and collaboration.

During World War II McDougal worked in the departments of State and Justice and in the Lend-Lease Administration, an experience that gave him firsthand knowledge of the methods of mobilization, manipulation, and control in modern society. He was greatly influenced by Oscar Cox, lawyer for Harry Hopkins. The Lend-Lease experience rekindled, among other things, his interest in international law, dormant since he had studied with James Brierly at Oxford. Thereafter, he devoted more and more time to what he called the public order of the world community.

McDougal was never the model of the retiring scholar. At different times he was active in politics at the state and national levels, and he took keen interest in the power struggles that often seem to be the *raison d'être* of scholarly organizations. He served as president of the American Society of International Law and of the Association of American Law Schools, was active in the American Law Institute, and was elected to the Institut de Droit International. He was counsel in a major international arbitration, represented the United States at diplomatic conferences, and advised corporations. Since his retirement from Yale in 1975, he has visited at other law schools and has continued to write and practice law.

His relationship to his students was close, usually dominating, always protective. He put a tremendous premium on loyalty, giving it unstintingly to friends, colleagues, and students. As a matter of course he expected it in return and could be deeply puzzled and hurt when he felt betrayed. He followed closely the careers of many students who achieved high positions in the United States and abroad. McDougal married Francis Lee of Virginia in 1933.

Publications. McDougal's key areas have been property law, jurisprudence, and international law, but many of his monographs have also made recognized contributions to constitutional law and legal education. With the exception of his innovative property law casebook, most of the work he did before his fifties was monographic. His major collaborative treatises in international law were all produced after that

period, and in his view represent the application of his matured theory to major problems. His *oeuvre* is enormous, a fact due as much to his energy as to his capacity to attract gifted younger scholars in collaborations on which his intellectual imprint is always evident. He has also been able to work simultaneously on several projects, each with a separate team. This influence has been further extended by the work of many students who wrote under his strict supervision in virtually all fields of law.

Many of his early international monographs were events in themselves, prompting rebuttal and often angry debate. His work on the lawfulness of testing hydrogen bombs over the Pacific, a detailed study of the basic traditional policies governing the law of the oceans, was immediately attacked. His theories on aggression and self-defense, now widely accepted, occasioned angry debates at the American Society of International Law. Part of the excitement and antagonism may have derived from the issues he chose; he always wrote on topical and explosively controversial subjects, for he was concerned with the application of law to current problems. Although he explored all issues thoroughly, he always reached conclusions about the lawfulness of contemporary acts that many colleagues rejected. His methodology stung some, for it focused not on rules and their logical interrelations, but on claims and decisions, the latter tested by their contribution to all the public policies raised in that particular instance. This was a distinctive approach, and McDougal insisted that it be viewed not merely as an alternative, but as the only meaningful way of approaching law. He set out in detail and with fairness the theories, methods, and conclusions of others, but indicated with precision where he differed and why he believed they were wrong.

Legal theory. McDougal called his jurisprudence "configurative" and "policy-oriented"; the theory and its proponents are generally referred to as the New Haven school. His theory of law can best be appreciated by contrasting it with the operational jurisprudence of most lawyers. The latter define a "problem" as essentially a dispute between parties. Analysis teases out the legal issues, and answers are found by reference to given rules of varying origin, applied through logical derivation. Law in this traditional analysis is taken as a body of rules. The origin of the rules, the political agitation that may establish, maintain, or change them, and the quality

of the rules as tested against other criteria are "political" and not legal problems. McDougal, by contrast, views law not as a body of rules but as a process of making decisions about how values—power, wealth, enlightenment, skill, well-being, affection, respect, and rectitude—are to be produced and distributed in the community studied. A problem is a disparity between goals and achievements. The function of the lawyer, as advocate or decision-maker, is to influence the process in order to achieve desired results; the scholar's function is to understand and appraise this process in its totality.

Scholar and lawyer share a number of intellectual tasks. Each must identify problems and objectives, chart pertinent past decisions, and determine their conditioning factors in order to assess whether past trends approximated or deviated from goals or objectives. Both scholar and lawyer must also assess likely future decisions, assuming that they do not intervene. According to some views, the advocate is distinct from the scholar in that he must clarify or adopt goals and invent alternatives, but in devising them he may not go beyond the interests of his clients. McDougal, however, requires the most encompassing goal clarification of the scholar, refusing to discharge him from the civic obligations flowing from his potential or actual "impact."

Thus, all who engage in these intellectual operations must postulate and make known the goals to which they are committed. McDougal's goals are those of a public order of human dignity, an order in which concern for realization of individual potential is central, and one that secures a production and distribution of values sufficient for individual growth.

McDougal's emphasis on goal clarification and the testing of past or prospective decisions in terms of their contribution to goal realization has led some commentators to call him a naturalist, since this has been one of the classic functions of natural law. McDougal, however, rejects any natural or transempirical content to law and specifically *postulates* the goals of human dignity; he does not attempt to discover or derive them. However, while he concedes that his goal choices are the products of his past and are basically existential, he believes that they are part of the trend to enlightened demands traceable to the pre-Socratics and that they are widely demanded at the present time.

The goals and five intellectual tasks are a central concern for McDougal. In developing

methods for their performance, he has rejected much of the inherited language and conceptual apparatus of lawyers, redefining, in Kantian fashion, the questions and concepts to be used, and reaching deep into the social sciences for appropriate tools. In performing these tasks, a key focus is on decision, on choices about how values are to be produced and distributed or "shaped and shared" in a given community. Values are things people want—"desired events"—and for their analysis McDougal uses a delineation developed largely by Lasswell, which seeks to identify desired events comprehensively by using the eight categories of values mentioned above. To identify the component operations involved in making a decision, McDougal and Lasswell have developed a sequential analysis of decision-making that yields seven "functions": intelligence, promotion, prescription, invocation, application, termination, and appraisal.

By using these and other concepts, McDougal aims at improving the performance of scholar and lawyer. To be useful, a system must, he believes, be *contextual*—i.e., take account of all the features of the social process of immediate concern, relating them to the larger context of events. It must be *problem oriented*, and, given its emphasis upon contextuality, it must employ *multiple method* or *interdisciplinary* skills. The framework of inquiry for this endeavor is critical, for it must yield realism, permit application to dissimilar situations, and be sufficiently general to meet the requirement of contextuality, but at the same time be economical enough to permit implementation. McDougal is distinctive, even among sociologically oriented colleagues in jurisprudence, in insisting on detailed attention to social and power processes.

Since the conventional study of formal organs and branches of government never tests whether authoritative bodies are actually effective, McDougal uses a functional model. It identifies *participants*; *perspectives* (the value demands, expectations, and identifications of participants); *situations* of interaction; *base values* or the *bases of power* upon which participants draw; the *strategies* or modes by which the bases are used; the *outcomes* or immediate value allocations of the interactions observed; and the longer term *effects* of the outcomes. This model is used to study community process (explained below). It is also cross-cut by value analysis, which permits the observer to examine in more detail the interactions and value dis-

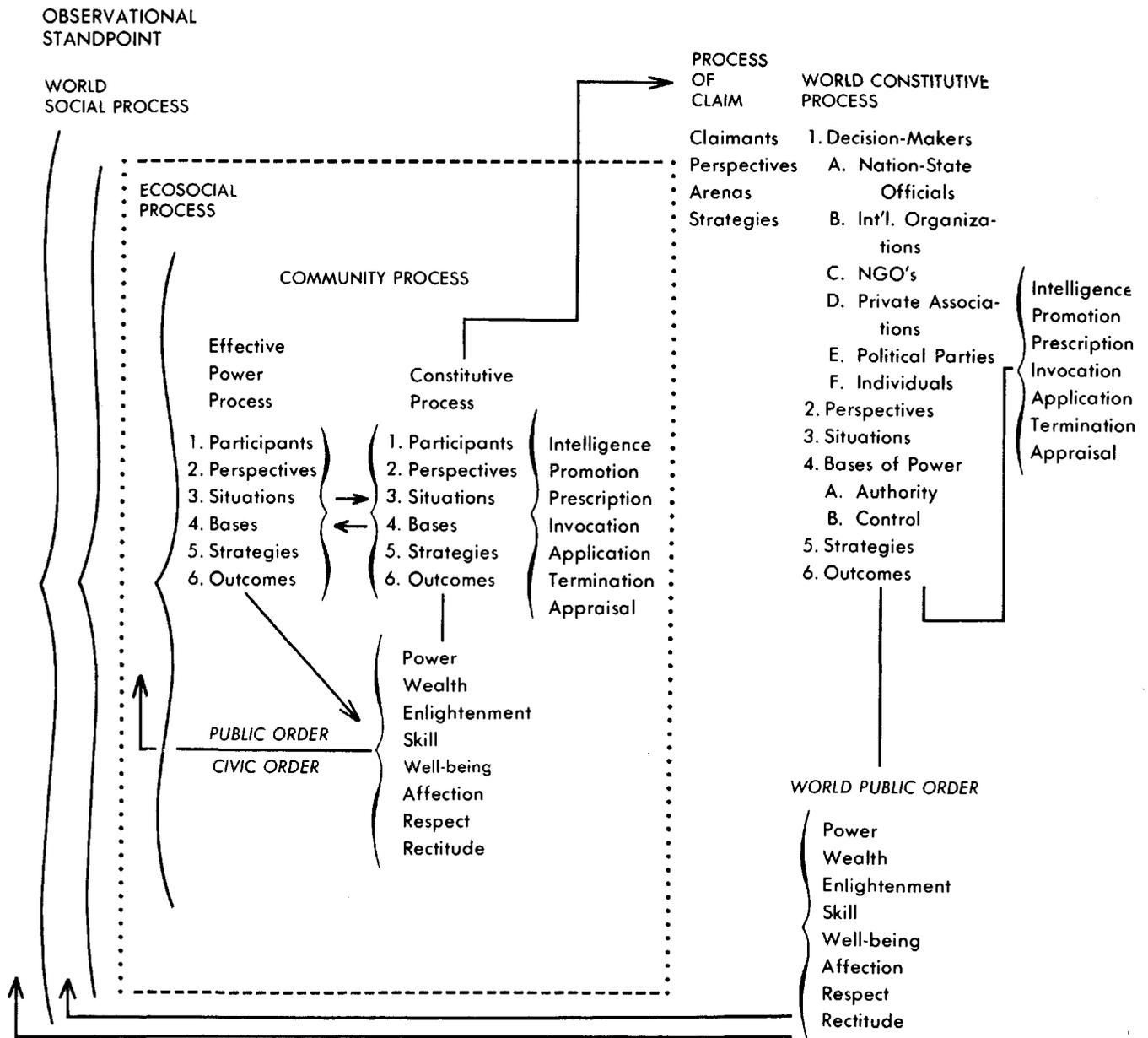


Figure 1. The configurative map

tributions in specialized institutions of power, wealth, respect, and so on. Schematically, McDougal's framework of inquiry can be portrayed as above (see figure 1).

In the use of this model, a number of operations are critical. The first task is to establish an observer's standpoint distinct from the process under scrutiny; otherwise, it is impossible to set criteria for appraisal. An appropriate perspective also requires self-scrutiny to correct for cultural and psychological bias, distortions that are particularly frustrating in transnational and cross-cultural contexts. Sigmund Freud's influence here is manifest, and use of

free association techniques in this and other intellectual tasks is acknowledged.

The second task is to delimit the focus on authoritative decisions in their context, a step akin to the scientist's choice of the proper lenses for his observational instruments. Some lenses magnify key features of the object of inquiry, some distort it, and some, once serviceable, may be too crude for current needs. Hence it is important to have a repertory of lenses and to appreciate the specific functions of each. It is in his choice and evaluation of the components of his focus that McDougal rejects much traditional legal theory, creating new categories,

many of which stress features that jurisprudence ordinarily ignores.

McDougal's focus has two main components—the relevant communities to be studied and the conception of law to be deployed. McDougal insists on a globally comprehensive conception of community, for his work is premised on the interdependence of the entire earth-space arena in which people interact; a corollary of this premise is that efforts to understand or influence decisions cannot succeed without keeping that interdependence in mind. In his community focus, he insists that processes of effective power as well as those of authoritative decision be incorporated. Both of these focal elements represent radical departures from traditional jurisprudence. Positivism and historicism have tended to be national in focus, even questioning the possibility of law between groups, and while natural law theory speaks in universal terms, the content of its focus on community tends to be general and imprecise. Almost all schools of jurisprudence focus only on authoritative decision, the formal institutions of the law, either ignoring processes of effective power or isolating them in closed categories, such as "political question," and then dismissing them as inappropriate subjects for jurisprudence.

McDougal insists that law, the second component of his focus, be conceived as processes of authoritative decision by which people clarify and implement their common interests. This process is investigated by special focal components, some original with McDougal, some incorporated from methodologies of other social sciences. Thus McDougal rejects behavioralism by insisting on a *balanced emphasis on both perspectives and operations*: what people say and think as well as what they do. He rejects formalism by insisting on a conception of law that incorporates both *authority and control*—i.e., the normative expectations of relevant actors as well as their actual participation in decision making. He emphasizes the processes by which the institutions for making indispensable decisions are established, maintained, and changed, but in studying this he dismisses the notion of a constitution as a document, insisting instead on a theory of "constitutive process" in which authority and control actually operate. He distinguishes this *constitutive process* from the *public order* of a community, the aggregate of decisions about the production and distribution of all values *other* than power, and dis-

tinguishes in turn, within the public order, a *civic order* or domain of privacy, in which production and distribution are effected through less severe sanctions.

These focal components have been designed with an intellectual task or a humanitarian goal in mind. The distinction between public and civic order is animated by McDougal's concern for the maintenance of private domains protected by the coercive power of the organized community but insulated from its operation. Here McDougal shares concerns with philosophical radicals and liberals and some common ground with writers like H. L. A. Hart. Conversely, a Marxist conception of jurisprudence might consider civic order a bourgeois atavism and a pathological condition in a just society. But many of the other focal emphases are radical jurisprudential revisions. The insistence on an integration of authority and control as prerequisite to a meaningful conception of law is distinctive and may be one of McDougal's major contributions to legal theory. The conception of the constitutive process is almost Copernican in its insistence that many postulates of right conduct in secular societies be tested with full attention to contemporaneous practices, rather than by reference to a totemized text.

International law. McDougal's treatises in international law are rigorous applications of his jurisprudential theory. In each, a careful statement of the factual problem is followed by a comprehensive critical review of scholarship in the area and a postulation of the goals that should guide decision makers if community responses to that resource or practice are to contribute to a public order of human dignity. McDougal then details with great specificity the factual claims with regard to the resource in question and the *aggregate* international decision processes that respond to those claims. The heart of each treatise is a meticulous review of the decisions for each claim, the environmental and predispositional factors that conditioned those decisions, projections of likely future decisions, and the alternatives thought to give a greater approximation to community goals. The result in each treatise is a picture of the processes that make and apply the law under study, their past and probable future productions, the impact of the decisions on the values concerned, an appraisal of the aggregate performance of the decision process, and recommendations for improved future performance. Legal and social science literature

are incorporated, documentation is enormous, and the total impact of each book is magisterial. To date McDougal has produced treatises on the regulation of international coercion (McDougal & Feliciano 1961), the oceans (McDougal & Burke 1962), outer space (McDougal, Lasswell, & Vlastic 1963), interpretation of treaties (McDougal, Lasswell, & Miller 1967); forthcoming treatises are on human rights, with Lasswell and Chen, and constitutive process, with Lasswell and Reisman. A bibliography published in 1976 listed 17 international treatises by other scholars that were strongly influenced by McDougal and that can be considered products of his school.

Property law. While McDougal's interest in jurisprudence has been consistent, his shift to international law from the field of property law, in which he worked for 25 years and has attained great prominence, is more puzzling. Property law is extremely localized; moreover, the common law variant is a strand in the unique web of English political history and hence shares little with other property law systems. In fact, McDougal's approach to property was radically different from that of his predecessors and exhibits many of the distinctive features that mark his work in international law. His conception of property was not "land" but the aggregate of a community's spatial resources, and he viewed the function of law as the planning and implementation of the uses of these resources in ways that would optimize the common interests of the community. In cooperation with Maurice Rotival, a planner who had worked in many parts of the world, McDougal wrote a book on planning for a river valley (McDougal & Rotival 1947). He published a study of land planning for Connecticut and, with David Haber, a textbook of cases and materials on property law that placed land use in a global context (1948). The book was a popular teaching text, enjoying a long run, but it was so controversial that efforts were made to ban it in two states.

Legal education. Throughout his career McDougal has been concerned with improving legal education. In 1943 he published, with Lasswell, one of his most famous articles, "Legal Education and Public Policy." He argued that one could not speak rationally of professional training without a theory of law and society and a conception of the preferred functions that the lawyer-in-training was to perform in his pro-

fessional career. He applied his method to these issues, dismissing much of the inherited curriculum as obsolete and inappropriate for professional training in a free society, and suggesting a detailed radically innovative curricular process that might better secure the social objectives he had postulated. The article was widely discussed and republished many times.

For 25 years, McDougal chaired the graduate program at the Yale Law School and transformed it into an international teacher's training program. Through it he played a major role in staffing law schools in the United States and abroad and in influencing the choice of deans. He traveled in the United States and abroad frequently, lecturing on legal education and public policy. He was also an active member of the Association of American Law Schools and served as its president in 1966.

Impact and evaluation. Intellectually, McDougal's perspective incorporates major contemporary concepts in several disciplines. His contextualism echoes in Alfred North Whitehead, Ludwig von Bertalanffy, David Easton, and many others. His commitment to social science techniques and to empiricism is contemporary. His theories of purposive decision resemble those in organization theory and in the policy sciences, of which he can be considered a founder, and his rigorously contextual instrumentalism in social design, a feature traceable to his early work in property law, is Gropian, as is his emphasis on teamwork. His skepticism about objectivity, sensitivity to psychological factors, and commitment to action are recognizable components of the twentieth-century intellectual's dilemma. Yet in the field of legal scholarship, a fortress that has long withstood intellectual change, McDougal's work is utterly distinctive. Although he purports to find the origins of his work in pre-Socratic Greece and in some of the medieval Pandectists, the genealogy is hard to trace.

McDougal's intellectual enterprise is at core a method that has yielded insights and spun off some new terminology. In a profession that charges by the hour and is impatient for results, there is a tendency to overlook the methodological component and to concentrate on the exploitable insights. Virtually every lawyer in the United States knows his name, though many older lawyers still attribute his fame to his seminal work in property law. International lawyers around the world consult his work and

respect him. Representatives of his ideas can be found in most leading law schools in the United States, though in many schools his exponents are likely to be considered eccentric by their colleagues.

Some aspects of McDougal's theory, however, have enjoyed wide acceptance. Conceptions of law as a process of decision are common, and policy analysis has become a fixture in much of legal education. Though both of these developments are attributed to McDougal, he has been critical of the style and method with which they are executed. Despite talk of empirical research in law, much research has merely incorporated various types of economic analysis and transformed them into a neoscholasticism. The over-all influence of McDougal's methodology on professional consciousness has been subtle, yet it is evident. One can say of McDougal, as W. H. Auden said of Freud in 1939, that "he is no more a person/Now but a whole climate of opinion/Under whom we conduct our differing lives."

Intellectual innovation always generates resistance. In the case of the New Haven school, language may be a factor. Although law as a discipline is notorious for its jargon, there has been great resistance and confusion about some of the metalanguage McDougal created or adopted from other social sciences. Ironically, many of his metaterms have become common parlance, precisely because they refer to aspects of modern society for which there were no terms in the traditional lexicon. Some contend that resistance is due to the absence of a simple manual setting forth his ideas, though he has in fact published a number of short and simple statements. The engagement of scholarship in contemporary political events, central in McDougal's thought, may also generate resistance, for it challenges the myth of the university's separation from practical life, which has provided it some autonomy. Historically, lawyers have cultivated a comparable technique of self-protection in the notion of the neutral technician, one that McDougal, like many legal realists, explodes. McDougal's open partisanship has generally alienated at least one side in a debate and has at times led to the charge that his theory is no more than an apology for American foreign policy.

Basically, this resistance may be less a matter of intellectual comprehension (his ideas are current and consistent with contemporary so-

phisticated thinking) than of distaste for the task he sets for lawyers. For in the final analysis McDougal demands that lawyers look at phenomena and themselves in a radically different way. Instead of an orderly world of rules and a simple system of logic for their application, he presents a dynamic and shifting process in which certainties are few and the agency and responsibility of the lawyer great. Those who enlist in the legal profession may demand precisely the myth of certainty that McDougal has undercut.

W. MICHAEL REISMAN

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MACHLUP, FRITZ

Fritz (Friedrich Eduard) Machlup was born in Wiener Neustadt, south of Vienna, on December 15, 1902, into the family of Berthold Machlup. He was raised and went to school in Vienna, spending his summers in the village of Timmersdorf, Styria, where he worked in the family cardboard-manufacturing business. In 1920 he enrolled at the University of Vienna and studied under Friedrich von Wieser and Ludwig von Mises. He wrote his dissertation on the gold-exchange standard under von Mises; it was completed in 1923 and published in 1925.

In 1922, while still a graduate student, Machlup had become a partner in a cardboard-manufacturing firm, the Ybbstaler Pappenfabriken Adolf Leitner & Bruder, which owned two cardboard mills in lower Austria. After receiving his doctor's degree in December 1923, he and some of his partners founded a corporation for the production of paperboard in Budafok, near Budapest, Hungary, under the protection of a high tariff. In 1927 he became a member of the council of a cartel of Austrian cardboard producers, assuming responsibility for setting up its accounting system. While pursuing this active business career, which involved weekly visits to the cardboard mills, this "businessman in Vienna," as he was then known (Ellis 1934), also continued research in economics and became treasurer of the Austrian Economic Society, whose other officers were Hans Mayer (president), von Mises (vice president), and Friedrich A. von Hayek (secretary). From 1924 to 1933 he formed a close association with Hayek, Gottfried von Haberler, and Oskar Morgenstern, as well as with the philosophers Felix Kaufman and Alfred Schutz, from whom he derived his lifelong interest in methodology and philosophy of science. These scholars all participated in the biweekly meetings of the von Mises Circle and the monthly meetings of a small group of interdisciplinary intellectuals, the *Geistkreis*. During this period Machlup published his second book on the "New Currency Systems of Europe" (1927), describing how one country after another adopted the gold-exchange standard; two articles (1928; 1929) on the German transfer problem and, in 1931, *The Stock Market, Credit and Capital Formation*, a central theme of which was an

analysis of the alleged tendency of stock-exchange speculation and brokers' loans to tie up or absorb funds that might otherwise flow into real capital formation. This early work and, still more, the expanded 1940 English edition, displayed the unique combination of talents that was to characterize all Machlup's later work: a penchant for abstracting the relevant aspects of the situation being analyzed, and for combining clear logical reasoning with an intimate, first-hand knowledge of the workings of economic institutions.

Following the collapse of the Austrian *Kreditanstalt* in 1931, Austria as well as other European countries introduced foreign-exchange restrictions, making it difficult to carry on a business. During this period Machlup wrote some 150 articles for the newspaper *Neues Wiener Tagblatt* dealing with current economic issues and advocating trade liberalization. As business conditions became progressively worse, he obtained leave from his partners to accept, early in 1933, a Rockefeller fellowship for 1933/1934, subsequently renewed for an additional year. He visited Columbia University, Harvard University, the University of Chicago, and Stanford University. At Harvard he associated chiefly with Frank W. Taussig and Joseph A. Schumpeter, as well as with Edward H. Chamberlin and Edward S. Mason, from whom he derived a lasting interest in the theory of the firm and of industrial organization. At Chicago he was closest to Jacob Viner and Frank H. Knight, and shared an office with three graduate students: W. Allen Wallis, George J. Stigler, and Milton Friedman. Upon receiving an appointment as professor at the University of Buffalo in May 1935, he returned to Austria to liquidate his business interests. He spent the first term of the new academic year on leave from Buffalo, spending October 1935 to January 1936 in Cambridge and London, where he saw much of John Maynard Keynes, Joan Robinson, R. F. Kahn, Piero Sraffa, and J. R. Hicks. He began teaching at Buffalo in February 1936, and remained there, except for the four war years, until June 1947.

At Buffalo Machlup wrote his seminal 1939–1940 papers introducing the concepts of supply and demand for foreign exchange, and relating the forms of these functions to underlying credit conditions and the elasticities of supply and demand for exportables and importables. During these years he also completed the 1940 English-language edition of his book (1931) on

the stock market, adding three new chapters that traced with great precision the financial flows in and through the stock market and their relation (or lack of relation) to the aggregate of brokers' loans. It was a logical step to pursue next his concern with the transmission of money flows in terms of the then fashionable Keynesian concept of the multiplier. In his two contributions on this subject (1939; 1943), he enriched multiplier theory by stamping on it his characteristic imprint: an insistence on providing a precise description of an institutional framework consistent with the formal theory; and of the effects of money flows on the balance sheets and income statements of individuals, firms, and monetary authorities. This led him to a healthy awareness of the model's limitations, which he described in the last chapter of *International Trade* (1943), causing Viner to praise it good-humoredly as the best chapter in the book (cf., John Williamson's chapter in Dreyer 1978). From an analytic point of view the book had considerable influence; its concept of "accommodating capital movements," for example, was subsequently adopted by James E. Meade.

With his lively polemic (1946) against Richard A. Lester (1946), Machlup entered a long period of concern with the theory of the firm and industrial organization, as well as enhanced preoccupation with methodology. To some, the main issue of the debate was the question whether the profit-maximizing theory of the firm was empirically secure; it was presumably this interpretation that led to the widely quoted quip by Schumpeter: "Lester was right but Machlup won the argument." The central issue stressed by Machlup, however, was one that he would later raise to the status of a fundamental problem in philosophy of science (1969): In a field where the objects studied (in this case, firms) themselves have theories (or notions) concerning their own actions, these theories cannot be accepted at face value. The influence of Machlup's 1946 paper can be gauged by the fact that subsequent empirical investigations have rarely made uncritical use of information obtained from questionnaires. Machlup later went on to outline a theory of theories—that is, a theory of the kind of theory economic agents might be expected to adopt in describing their own roles; accordingly, he points out (1960a) that unions "reject the wage-push diagnosis because, understandably, they do not wish to take the blame for the inflation. But they also reject the demand-pull diagnosis, because this . . . would

militate against the use of fiscal and monetary policies to bolster employment. . . . The only way out of this logical squeeze is to blame the consumer-price increase on prices 'administered' by big business" (p. 126). Thus, one must understand and be able to explain agents' theories about their own behavior in order to know how much weight to place on these theories as evidence. Machlup's methodological position is here remarkably close to that of Vilfredo Pareto's *Sociology* (1916), although there is no reason to believe there was any direct influence.

In 1947 Machlup became Hutzler professor of political economy at the Johns Hopkins University, where his main research (and teaching) interest continued to be in price theory. A *tour-de-force* was his 1949 book on the basing-point system, in which the reader is put through his paces to obtain a complete understanding of delivered-price systems from almost every angle. The book contains a sizable methodological component in its attention to persuasive definitions and theories and to the different meanings attached to the same terms by lawyers and economists. The book was written in the wake of the Supreme Court's decision against basing-point pricing in the cement case, and just as Congress was passing a law to legalize the practice; it came out strongly and unabashedly against uniform delivered pricing, and a copy of it was on President Harry S Truman's desk when he vetoed the bill later in 1949. Machlup continued unrelentingly in his analysis of industrial organization and published two major treatises in 1952. These, especially *The Political Economy of Monopoly* (1952*b*), illustrate and carry out Machlup's methodological position to the effect that there can be no reliable factual inference without good theory, and that accounts pretending to be purely factual and empirical invariably contain hidden theory.

A major subject of Machlup's attention during these years was the economics of the patent system and its dual role as restrainer of competition and as promoter of invention (Machlup & Penrose 1950; Machlup 1958*a*; 1958*c*; 1960*b*; 1968*a*). This work had begun when he was on leave from Buffalo at the Brookings Institution in 1942 and in the office of the Alien Property Custodian from 1943 to 1946. As always, Machlup started by acquiring a thorough acquaintance with the facts, and set forth a program for a cost-benefit analysis of the patent system (1958*a*). He perceived (1958*c*) that innovation

must spread to be effectual, but not so rapidly as to wipe out the gains to the innovators. He came to realize that, given the interdependence of education and research, a precondition of a cost-benefit analysis of the production and dissemination of knowledge was a scheme of cost accounting and the accompanying statistics. The outcome was *The Production and Distribution of Knowledge in the United States* (1962*b*), a comprehensive work covering all phases of knowledge and what he styled "knowledge industries." He measured knowledge using two approaches: type of output (the industry approach) and type of labor input (the occupations approach), and found that according to both approaches the production of knowledge had been increasing twice as rapidly as the gross national product. Throughout this period, Machlup was active in the American Association of University Professors and contributed articles on education, tenure, academic freedom, faculty salaries, and (later, in 1976) the organization of libraries, to its *Bulletin*; he was its president from 1962 to 1964.

Perhaps the most significant of Machlup's contributions were those to international monetary economics, a subject to which he returned in 1950 when many countries were experiencing what they considered to be "chronic" deficits in their balance of payments. He coined the term "elasticity pessimism" (1950*a*) for the belief, based on statistical regressions, that exchange-rate changes would have little corrective effect on payments balances, and sorted out and clarified the confused issues surrounding the concept of "dollar shortage" (1950*b*). In his second major controversialist foray (1955*b*), he took on Sidney S. Alexander's 1952 attempt to replace the "elasticity approach" to the theory of balance-of-payments adjustment by the "absorption approach," which treats a country's balance of payments analytically as an excess of its expenditures (absorption) over its receipts. Machlup argued that what was correct in Alexander's account in no way contradicted the traditional approach, properly understood. He returned to the question of disequilibrium in the balance of payments in a brilliant and penetrating methodological essay (1958*b*).

With his appointment in 1960 to succeed Jacob Viner as Walker professor of economics and international finance at Princeton University, Machlup entered a decade in which his attention was to be absorbed very largely in the

problem of international monetary reform. As director of the International Finance Section and editor of its unique series of publications, he was in constant touch with the ablest analysts of international monetary problems. Robert Triffin in 1961 sounded the warning that, with continued deficits on the part of reserve-currency countries as the only way to satisfy the growing needs for international "liquidity," a collapse of the gold-exchange standard was bound to ensue. In two works (1962a; 1963b), Machlup provided his assessment of the situation and an analysis of five alternatives: extension of the gold-exchange standard (by multiple-reserve currencies); mutual credit facilities (swap arrangements) among central banks; creation of an international reserve asset; an increase in the price of gold; and flexible exchange rates. He saw fatal objections to the first, recalling bimetalism and Gresham's Law (1963b); long-run inadequacy in the second; and inequitable distributional problems and chronic speculation in the fourth (since the price of gold would have to keep increasing). Only the third and fifth were left as viable alternatives. In 1963, when Secretary of the Treasury Douglas Dillon announced that a thorough review of the international monetary system would be undertaken without seeking the views of academics (who could never agree among themselves), Machlup's professional pride was challenged. He proceeded to organize a series of conferences in Princeton and Bellagio, calling together academics who became known as the "Bellagio Group." So successful was its report (Machlup & Malkiel 1964), with its distinction of three problems (adjustment, liquidity, and confidence), that officials asked to be included in future meetings. The subsequent joint conferences of officials and academics that Machlup organized resulted in two volumes of papers (Machlup et al. 1966; 1970). The flavor and accomplishments of these meetings have been richly described by Triffin and Williamson in Dreyer (1978). Machlup became, in Triffin's words, "the unquestioned intellectual leader and mentor of our vain efforts to reform the crumbling international monetary system of the postwar years," and he attributed this leadership to "the unusual blend of theoretical rigor and pragmatic realism which characterizes Professor Machlup's methodology and is the key to his unique achievements in this area as well as in . . . many others. . . ."

These years saw a number of significant contributions to the field of international monetary economics. There was the startling theory (1966a; 1966b) that the central banks' need for ever-increasing international reserves is politically based and must be met if restrictions on trade and capital movements are to be avoided. There were penetrating analyses of the nature of international reserve units (1965a; 1965b; 1967a; 1967b) and special drawing rights (1968b), as well as of the seigniorage problem (1965a). One senses Machlup's striving to communicate when he writes in exasperation with the attitudes of central bankers toward exchange-rate flexibility (1966a): "their apodictic claim that it is impractical may . . . be translated into the statement, 'I am against it, but cannot give any good reasons'" (p. 173). So great, however, was the respect that Machlup gained in central banking circles that he was twice offered the presidency of the *Nationalbank* of his native Austria (in 1967 and 1972). Great was the disillusionment (1976a) when, with the Jamaica accord, events forced the recognition of floating exchange rates, but the carefully laid plans for reform (effective adjustment processes and establishment of the SDR as principal reserve asset) were forgotten. Machlup had done all he could.

He had assumed the presidency of the American Economic Association in 1966, and took on that of the International Economic Association from 1971 to 1974. Retiring from Princeton in 1971, he accepted a professorship at New York University. The 1970s saw his brilliant analysis of the way in which the actions of European central banks in what he called the "Xenocurrency markets" led to the creation of additional dollar deposits and official reserves (1970b; 1971; 1972b; 1972c; 1972d), furnishing proof of the success of Machlupian methodology: he was way ahead of "practical men" in understanding how the Eurodollar system actually functioned. His organization of the IEA World Congress in Budapest in 1974 led him to "toss off" an encyclopedic *History of Thought on Economic Integration* in 1977.

Renowned for his teaching, Machlup was also a man of great culture, who would follow radio performances of Wagner operas and Mahler symphonies with the score in front of him. At age 75, a volume was presented in his honor by Dreyer (1978). Far from resting on his laurels, he embarked on the most ambitious project of

his career: the second edition of *The Production and Distribution of Knowledge in the United States*—in eight volumes.

JOHN S. CHIPMAN

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MAHALANOBIS, P. C.

Prasanta Chandra Mahalanobis (1893–1972) was born on June 29, 1893, in a family that traced its roots to the landed aristocracy in Vikrampur in Bangladesh. His grandfather moved to Calcutta in the middle of the nine-

teenth century and became involved in the Brahmo Samaj, a Hindu reformist movement, started by Raja Ram Mohan Roy in 1828 and revived in the last quarter of the nineteenth century by Devendranath Tagore, father of the poet Rabindranath. Mahalanobis had his early education in the Boys' School run by the Samaj and later got his Bachelor's degree with honors in physics from the Presidency College of Calcutta. He left for England in the summer of 1913, joined King's College in Cambridge, took part I of the Mathematical Tripos in 1914 and passed the National Science Tripos, part II, with a first class in 1915. He was elected senior research scholar and returned to India for a short vacation, intending to go back to England to work with C. T. R. Wilson at the Cavendish Laboratory in Cambridge. But on his return, he accepted a teaching position at the Presidency College. He gave up the idea of pursuing his research in physics at Cambridge and became deeply involved in his teaching and in the affairs of the Brahmo Samaj. He was active in the Samaj until his death. He was closely associated with Rabindranath Tagore during the poet's long life, and with Visva-Bharati, a university established by the poet embodying his universal vision. These activities reflected Mahalanobis' deep commitment to public service. He contributed significantly to the Bengali literature on Tagore, of which his pioneering work in the textual criticism of Tagore may be mentioned.

Mahalanobis' interest in statistics was stimulated by his tutor in King's College, who asked his opinion of some volumes of the journal *Biometrika* presented to the college by its editor, Karl Pearson. He became so interested that he bought a complete set of the journal and brought it with him to India. This started his long scholarly career in statistics that was to result in contributions to several branches of natural and social sciences, meteorology, anthropology, educational psychology, sample surveys, and finally to planning for economic development. He founded the Indian Statistical Institute (ISI) in 1931 and served as its honorary secretary-director almost to the end of his life. He was elected a fellow of the Royal Society in 1945.

The ISI, nurtured in no small measure from its early years by the resources of Mahalanobis and his wife Nimal Kumari, came to occupy a unique position among institutions of research and teaching in statistics in India and abroad: unique in that teaching and research in many

branches of natural and social sciences, and not merely statistics, are carried on within the institute. This was due to Mahalanobis' view that not merely statistical tools are essential in any empirical research; it was equally essential that research and training in statistics be informed by research in the sciences.

Mahalanobis' name will always be associated with the emergence of a statistically competent technique of sample surveys. He was instrumental in the adoption in India of statistical sampling on a large scale in the estimation of area devoted to, and yield per unit area of, agricultural crops. He introduced the technique of "interpenetrating subsamples" by which two or more independent random samples were chosen from the population, data from each sample being collected by a different investigator. Each sample provided a statistically valid estimate of the population characteristic and the variance between the estimates from the different samples provided an easily calculated estimate of the sampling and nonsampling errors. In the days before modern electronic computers, this saved the substantial computing effort involved in calculating sampling errors, particularly when complex multistage sampling designs were being used. Following his lead, the statisticians of ISI have contributed significantly to the theory and practice of survey sampling.

He was also a pioneer in the area of multivariate analysis. The statistic Mahalanobis distance or Mahalanobis D^2 , as it is known in the statistical literature, is used for testing the hypothesis that the vectors of means of two multivariate populations are the same. This statistic was first used for problems of taxonomical classification based on anthropometric measurements. It has also been used by some economists for grouping countries according to their state of economic development as measured by more than one indicator.

Mahalanobis was not formally trained in economics, nor did he make any systematic study of it himself. Yet his conviction that statistical methods constituted "a new technology for increasing the efficiency of human efforts in the widest sense" led him to a study of development planning. In addition, he initiated studies in development planning in the ISI and invited economists and planners to the ISI from all over the world, East as well as West. Some of the earliest multisector planning models applying techniques of linear programming were built at the ISI. As a by-product, several input-output

tables for the Indian economy, with differing degrees of disaggregation and for different years, were generated.

While recognizing the utility of formal models in understanding an economic system without getting bogged down in its details, he repeatedly emphasized that a model was only "a scaffolding to be dismantled once the building was erected." His own contributions were the two- and four-sector models. The two-sector model, while similar to those developed by G. A. Feldman in the Soviet Union and Evsey Domar in the United States, was independently formulated by Mahalanobis. The two sectors consisted of an aggregate consumption goods producing sector with an output-capital ratio of β_c and an investment goods producing sector with its output-capital ratio β_i . Given an initial investment rate of a_0 , a one period lag between investment and production and constant proportions λ_c , λ_i ($\lambda_c + \lambda_i = 1$) of aggregate investment going to consumption and investment goods sectors, he showed that national income at point t will be given by

$$Y_t = Y_0 \left[1 + a_0 \frac{\lambda_i \beta_i + \lambda_c \beta_c}{\lambda_i \beta_i} \{ (1 + \lambda_i \beta_i)^t - 1 \} \right]$$

and consumption by

$$C_t = Y_0 \left[(1 - a_0) + a_0 \frac{\lambda_c \beta_c}{\lambda_i \beta_i} \{ (1 + \lambda_i \beta_i)^t - 1 \} \right]$$

It is then easily seen that, given the output-capital ratios, the long-run growth rate of income and consumption is $(1 + \lambda_i \beta_i)$, implying that the long-run growth rate of consumption will be higher the greater is the proportion of investment devoted to building up investment goods industries. This, given the potential of small-scale village industries in India, led Mahalanobis to propose a strategy of development that gave priority to basic and heavy industries in large-scale manufacturing, while labor-intensive small-scale industries were to be the suppliers of consumption goods and generators of employment. This strategy, accepted by the government, has been followed for two decades since the middle of the 1950s to the present with only minor changes in emphasis. But Mahalanobis characteristically resisted becoming formally a member of the Planning Commission, preferring to be a *de facto* member.

His four-sector model is an elaboration of the

above with the consumption goods sector further subdivided into modern industry, small-scale industry, including agriculture, and services. This model was used by Mahalanobis to explore the employment implications of alternative investment allocations.

The Mahalanobis strategy of development has been criticized for its formal inadequacies, its ideological bias, its economic inefficiency, and especially for its neglect of foreign trade. It would appear however that the strategy was appropriate at the time it was formulated. Many of the industries established under the strategy would have been desirable socially, as well as efficient from the point of view of India's long-run comparative advantage. The only major criticism of the government, rather than of Mahalanobis, is that the strategy was inflexibly adhered to for too long a period in the context of technical change in agriculture and the opportunities offered by international trade. Be that as it may, there is no denying the fundamental contribution of Mahalanobis in building a firm foundation for India's industrialization.

Mahalanobis was concerned not merely with economic growth but also with its impact on different socioeconomic groups. In order to monitor the changes in the levels of living of the population brought about by planned development, he initiated an annual household consumer expenditure survey in 1950. Aware of the conceptual and measurement problems in defining household income in economies where self-employment is the dominant mode of employment, these surveys canvassed consumer expenditure instead. A large number of studies based on these surveys have been done at the ISI. These had a direct bearing on the formulation of national development plans. His concerns about distributional aspects of development were shared by Prime Minister Nehru, who appointed him chairman of a committee to inquire into these aspects. The committee submitted its report in 1964, long before such concerns were espoused by international agencies. Worth mentioning is that Mahalanobis emphasized the inequality of access to educational opportunities far more than the inequality of the distribution of physical assets in explaining income inequalities.

In analyzing data on consumer expenditure, Mahalanobis developed a new methodological tool, "Fractile Graphical Analysis," for comparison of socioeconomic groups at two different places or points of time. This tool has since been

applied in demography, psychology, and biometry.

Mahalanobis laid the foundations for continuing collection of socioeconomic data for policy purposes in his many functions, such as chairman of the National Income Committee, honorary statistical adviser to the Indian Cabinet, and the organizer of the Central Statistical Unit. Indeed, because of him, the data base of the Indian economy is far more extensive and reliable than that of many countries, developed as well as developing.

Mahalanobis' interest in science and world affairs was broad. He was a founder member of the Indian Pugwash Committee and was active in the nuclear disarmament movement.

T. N. SRINIVASAN

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MANNHEIM, HERMANN

Hermann Mannheim was born in Berlin in 1889 and died in London in 1973 at the age of 84. His obituary notice in *The (London) Times*

described him as the "father of modern English criminology," a title that reflected the immensely influential role he played in the development of the discipline not only in the University of London, where he had taught at the London School of Economics (L.S.E.), but throughout the academic world and in public life and affairs. An early refugee from Hitler's Germany, Mannheim had been one of a trio of *émigré* criminologists; his fellow countryman Max Grunhut became reader in criminology at Oxford University, while Leon Radzinowicz from Poland was later to become the first Wolfson professor of criminology at Cambridge University.

Mannheim's life had been divided into two parts. Born into a family with mercantile interests in east Prussia, his early education followed the typical bourgeois pattern of private tutorial education at home, moving on to the classical Gymnasium and the university. Although a talented pianist from his earliest years, who might have made his name in the concert hall, he chose to study law and political science. It was in fact in his student days that the polymathic interests that were to be so distinctive a feature of his later academic life became initially evident. At Munich, Freiburg, Strasbourg, and Königsberg he followed courses in economics, philosophy, sociology, psychology, and psychiatry. He was, of course, a student in a world as yet undisturbed by the shattering social consequences of World War I, in which scholars such as Émile Durkheim and Max Weber were contemporary figures. There can be little doubt that in his education Mannheim benefited not merely from the advantages of the continental system, which permitted greater academic flexibility than did the universities of the English-speaking world, but from the stimulation of an intellectual curiosity that was to distinguish him from most lawyers. Although he read Schopenhauer, Kant, and Dostoevsky, and developed an interest in psychiatry, his interests increasingly focused on criminal law and on the phenomena of crime and the penal system. These he approached not simply in the context of "scientific" criminology, but in relation to the study of ethics and social justice.

Mannheim underwent the usual forensic apprenticeship in the courts and lawyers' chambers. His thesis for the degree of DR. JUR. at Königsberg in 1912 was the subject of criminal negligence. Although he was to go on to a distinguished career as a practical lawyer, his aca-

demical training had been substantial. Franz von Liszt, probably the most influential figure in late nineteenth-century German criminology, had established a following of successful teachers whose lectures Mannheim attended. If their names are now less well known, the same cannot be said of Max Weber, whose influence on Mannheim's thinking was as important as that of Durkheim, although the latter addressed himself more directly to problems of social pathology.

When World War I erupted, Mannheim became an artilleryman, first on the Russian front, then, after the collapse of the Czar's armies, in France. For many of his generation, nurtured in a climate of scholarship and culture that was European rather than nationalistic, the war was a negation of values, conducted by interests as Philistine as they were morally suspect. In later years, in private conversation, Mannheim described his intellectual and moral despair, not only at his proximity to the horrors of the war, but with the company of professional soldiers with whom he had so little in common. Toward the end of the war he was made a judge of courts-martial.

The end of the war, the collapse of the Hohenzollern Empire, and the establishment of the ill-fated Weimar Republic gave Mannheim the chance to exercise his practical talents. He served as a legal adviser to local governments and engaged in the practice of labor law. In this area he gained experience in administrative work and with the pressing social problems of postwar Germany. But by 1923 he had returned to academic life at the University of Berlin, where he was appointed *Privatdozent* in the law faculty. At the same time he became a magistrate in Berlin. Although his teaching was essentially restricted to criminal law, his work in the courts provided more scope for his criminological interests. He was progressively promoted toward the superior courts, and his experience along the way as an examining magistrate—a position that has no counterpart in the Anglo-Saxon legal system—gave him unique opportunities to enlarge his criminological interests. By 1929 he had become professor *extraordinarius* at the university and in 1932 he was promoted to a judge of the *Kammergericht*, the supreme court of Prussia, sitting in the criminal division.

Thus, at the age of 44, Mannheim had gained a senior judicial post and a professorship and had completed some 14 major publications.

Then, in 1933, the course of European history changed, and with it, the whole of Mannheim's life. It was clear that men of his moral and intellectual stature were soon to become the prime targets of the beerhall thugs and Jew-baiters who, having brought Hitler to power, ruled Germany. He was soon deprived of his academic post, and rather than accept a transfer from Berlin to the Rhineland—which he knew was but a prelude to his eventual dismissal from office—he retired from the bench and went as an *émigré* to England. In 1951, although by now a British subject, the West German government conferred on him the rank of retired president of a division of the court of appeal.

Mannheim was only one of many refugees to arrive in England in the mid-1930s, and his position was not easy. He was not only in a foreign country but had been deprived of both academic and judicial status. Thus, in addition to being a refugee he experienced a dramatic degree of downward social mobility. This, then, was his challenge. In England he found an entirely different intellectual tradition of social inquiry, essentially empirical rather than theoretical, and directed toward social reform rather than the accretion of knowledge. It was a tradition of long standing and, surprisingly soon, he found a place among its contemporary exponents. For a man of lesser intellectual stature this adaptation might not have been possible, but Mannheim had always been open to ideas in criminology. Among lawyers of his day he was distinctly *avant-garde* in his thinking about the nature of crime and criminal responsibility, and in his last years in Berlin, moving between the university and the courts, he had always based his teaching on his work in the courts.

Criminology in England was, to all intents and purposes, a "fringe" interest of the intellectual establishment. It had no base in any university. The legal profession, officially, was quite unaware of it. Psychiatrists interested in the treatment of mentally disordered offenders were few and were generally regarded as "cranks" by their fellows in the medical profession. As for the work of European criminologists, the natural insularity of British culture ensured for it a place on the hazy margins of academic consciousness.

Mannheim quickly made contact with several influential figures in the world of social work and social reform and, perfecting his knowledge of English, he became involved for a while in

several practical projects, including teaching in Pentonville Prison, London. In 1935 the L.S.E. appointed him a lecturer in criminology, but the post was both honorary and part time. Not until he was awarded a Leon fellowship at the University of London the following year could he devote his time to uninterrupted research. The result of this fellowship was a book entitled *Social Aspects of Crime in England Between the Wars* (1940). *Social Aspects* reflected not only the rapidity with which Mannheim had come to grips with the crime problem in his country of adoption, but the extent to which he had become familiar with the work of sociologically oriented criminology in the United States, exemplified in the work of the Chicago School. Mannheim gave further lectures at the L.S.E. and by 1940 had become a British subject. The L.S.E. had, in the meanwhile, been evacuated to Cambridge, and in its wartime atmosphere Mannheim had the opportunity to develop and expand his criminological teaching with a growing number of students. His researches into juvenile delinquency among the evacuees in wartime Cambridge, published under the somewhat inappropriate title of *Juvenile Delinquency in an English Middletown* (1948), date from that time. Not until 1944 was he appointed to a full-time lectureship, but two years later he was promoted to reader in criminology, the first such named academic post in Britain, which he retained until his retirement in 1955.

He had in the meantime continued to publish research materials, notably *Young Offenders* (1942), a study of juveniles that had been carried out jointly with A. M. Carr-Saunders, the leading demographer of his day who would later become director of the L.S.E., and E. C. Rhodes, then reader in statistics. In 1946 he published what was probably his most widely read and, perhaps, most influential book, *Criminal Justice and Social Reconstruction*. In its preface he wrote: "So far, Criminal Justice has often remained too much behind and out of touch with the progressive elements of social thought, and its approach to the problems of society has been too one-sided to make it a really living force." His observations on such subjects as euthanasia, sterilization, and abortion are astonishingly contemporary after three decades. White-collar crime, the crimes of motorists, they are all scrutinized in a fashion which is all the more interesting when one considers how much of what was then "radical" has become the substance of contemporary orthodoxy.

In the last ten years of his teaching career, Mannheim made his mark not merely as an undergraduate teacher, but as a supervisor of research students. Indeed, it is possible to trace a kind of "genealogy" of his PH.D. students comparable to the "line" established by Edwin H. Sutherland. The impact of his teaching in England and the British Commonwealth was immense, not merely in raising a new generation of academics, but in influencing public servants concerned with the administration of criminal justice and with the social policies allied to it.

Although deeply suspicious of positivism, particularly of the theories of so-called "social defense" that had been attractive to the totalitarian régimes of fascist Italy and Nazi Germany, he had a lifelong desire to see a greater degree of rationality in criminal policy, not least in sentencing. To this end he pursued his researches into prediction techniques published as *Prediction Methods in Relation to Borstal Training* (Mannheim & Wilkins 1955). Since Mannheim fought long and valiantly against the arrogance of a judiciary that considered its prejudices to be encapsulated in the moral conscience of society, it is hardly surprising that he argued continually for sentencing with credible objectives and credible results. That end was entirely consistent with his liberal social democratic philosophy. Perhaps he did not foresee the conflict that would inevitably result from the ascendancy of the "treatment" ideology and its challenge by the guardians of the "rule of law," per Gault (387 U.S. 1 [1967]).

Mannheim's thinking had a strong utilitarian bias. His objections to short prison sentences, for example, were based upon their proven ineffectiveness. Yet he would probably have opposed much current thinking about the need to shorten sentences on the ground that such policies were largely instrumental in character. As an executive member of the Howard League for Penal Reform he played an important part in the campaigns for various penal reforms, notably the fight against capital punishment. He was also a strong public supporter of reforms in the laws relating to abortion and homosexuality. His liberal views were not, however, libertarian. He opposed abortion on demand, and his close association with the forensic psychiatrists of his day confirmed him in the view that homosexuality was an abnormal condition and a proper subject for psychological treatment.

He wrote prefaces to many of the published works of his former students, but his last major

work was the 2-volume textbook *Comparative Criminology* (1965) published when he was 76. An Italian translation appeared shortly after his death. This text, though not of the didactic quality of much of his earlier work, is an important source book, not least of crime in the context of wartime England, a subject that is poorly documented elsewhere. Mannheim also founded, together with some of his prewar colleagues at the Institute for the Study and Treatment of Delinquency, the *British Journal of Delinquency* (later the *British Journal of Criminology*), and he was involved in the formation of the group that would become the British Society of Criminology. He traveled widely in Europe and the United States after World War II. He was awarded the coronation medal in 1953, an honorary doctorate in law at Utrecht in 1957, and the Order of the British Empire in 1959.

Mannheim never achieved professional status at the L.S.E., a fact indicative more of academic parsimony than lack of merit. As an academic politician (rather than a scholar), he was conspicuously unsuccessful, both in winning promotion himself and in founding an elaborate empire of research organizations. A proud man, he suffered his pretentious inferiors less well than would have served his interests. The traumata of his demotion from academic and judicial eminence at the peak of his first career also left its mark. Yet his coolness toward some of his academic colleagues could be contrasted with the warmth that infused his relations with students.

The volume of Mannheim's published work is small compared with his spoken output. His two-hour lectures at the L.S.E. were legendary; he addressed innumerable public meetings to every kind of interested audience. He spoke to social workers, probation officers, prison staff and prisoners, magistrates and judges. It is a measure of his contribution that he reached so wide an audience in his enduring concern to articulate the academic study of crime with the practical operations of the criminal justice system. Although he never penetrated the inner fastness of the legal establishment or the socially exclusive circuits of Oxford and Cambridge, his ascendancy in the world of social administration and sociology was nearly complete. Without his contribution, criminology in Britain would have been retarded for a generation.

TERENCE MORRIS

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MARCUSE, HERBERT

No twentieth-century philosopher, with the possible exception of Jean Paul Sartre, has been the object of such lively public controversy as Herbert Marcuse (1898–1979). Unlike Sartre, Marcuse remained in relative obscurity until well into his seventh decade, when he suddenly emerged as the most potent intellectual force behind the New Left in the United States and much of Europe. Enormously popular with the student generation of the 1960s, he was reviled by a wide variety of political opponents from conservative defenders of capitalism to orthodox Marxists. At the height of the controversy in 1968 he was briefly forced to go into hiding because of threats from right-wing extremists.

In contrast, the earlier decades of Marcuse's life were generally uneventful. He was born in 1898 in Berlin, the first of three children of a Jewish businessman, Carl Marcuse, and his wife, Gertrud Kreslawsky. He studied at the Mommsen Gymnasium in Berlin until conscripted into the German army in 1916. At the end of the war, he was caught up in the short-lived German revolution and was elected a member of the Soldiers' Council for the Berlin suburb of Reinickendorf. Disappointed by the revolution's outcome, he quit the Social Democratic party, to which he had belonged for a short time, and returned to his studies. In 1919, he entered the University of Berlin, where he concentrated on German literature; two years later, he moved to Freiburg and the study of philosophy. In 1922, he received his doctorate with a thesis on the *Künstlerroman*,

novels in which artists played central roles. After a short career in bookselling and publishing, he returned to Freiburg in 1929 to work for his *Habilitation* under Martin Heidegger. His project was a study entitled *Hegels Ontologie und die Grundlegung einer Theorie der Geschichtlichkeit* (1932), which clearly showed his mentor's influence. In a number of long articles published in those years, Marcuse attempted to reconcile Heidegger's version of existentialism with the Marxism he had recently come to adopt as his political and theoretical orientation. Although some later commentators would persist in finding residues of that attempt in his subsequent work, in particular his critique of technology, Marcuse soon abandoned the effort.

Because of the Nazi régime's ascent to power, Marcuse had no chance to receive his *Habilitation*. He left Freiburg in 1933 to join the staff of the Institute for Social Research, then headquartered in Frankfurt. He was immediately assigned to its Geneva office, where he remained until he followed the institute in its migration to New York and Columbia University in 1934. Under the leadership of Max Horkheimer, the institute was then engaged in the formulation of a radical critique of advanced capitalist society, in particular of its fascist and protofascist forms. This critique was grounded in a variant of Hegelian Marxism that became known, after a 1937 essay by Horkheimer, as "Critical Theory." Marcuse's major task was the development of the theory itself, which he pursued in a number of essays in the institute's *Zeitschrift für Sozialforschung* treating such topics as hedonism (1938) and philosophy and "Critical Theory" (1937). Among his initial colleagues at the institute were Frederick Pollock, Leo Lowenthal, Erich Fromm, Karl A. Wittfogel, and Henryk Grossmann. By the end of the 1930s, they were joined by T. W. Adorno, Franz L. Neumann, and Otto Kirchheimer. The inner circle of the institute around Horkheimer would later be known as the Frankfurt School, which achieved public recognition after the institute returned to Frankfurt following World War II. Although Marcuse chose to remain in America, he continued to be identified with his former colleagues as a founding member of the school.

During the war, Marcuse was a senior analyst for the Office of Strategic Services, and then after 1945, the chief of the research division of the Department of State's Central European section. In 1950, he became a senior fellow at the Russian Institute of Columbia University; two

years later, he moved to Harvard University's Russian Research Center, and then in 1954 he joined Brandeis University as professor of politics and philosophy. In 1965, he accepted a position in the philosophy department at the University of California at San Diego, where he was compelled to retire in 1970.

As a writer and teacher rather than as a political activist, Marcuse influenced American political life. His first full-length study in America, written during his tenure with Horkheimer's institute, was *Reason and Revolution: Hegel and the Rise of Social Theory* (1941). It sought to rescue G. W. F. Hegel from his alleged connection with Nazism and to establish his crucial importance for Marx's dialectical thought. Influenced by the recently discovered Paris Manuscripts (Marx 1844), whose significance he had been among the first to recognize, Marcuse stressed the subjective, critical, humanist dimensions of Marxism as opposed to the scientific and economic interpretation of both orthodox Marxists and most anti-Marxists. He linked their distorted interpretation to the success of positivist social theories such as Auguste Comte's and F. J. Stahl's, which developed in response to the "negative" philosophy of Hegel and Marx. He charged positivist social theory in particular with suppressing the crucial negative tension between the Classical German Idealists' notion of Reason (*Vernunft*) and the irrationalities of the given reality. The radical impulse of Hegelian thought lay in the drive to rationalize the world, as the young Hegelians of a century before, with whom Marcuse was sometimes to be linked, had understood.

Marcuse's next book, written after his long hiatus in governmental service, sought to do for Sigmund Freud what *Reason and Revolution* had done for Hegel: rescue him from his conservative interpreters. *Eros and Civilization* (1955) advanced a boldly imaginative thesis about the social and political implications of psychoanalysis, turning even such seemingly pessimistic concepts as the death instinct in a radical direction. The crux of Marcuse's argument lay in his historicization of repression and the reality principle, which allowed him to posit the concept of "surplus repression" produced by the specific reality principle of modern industrial society. He called the latter the "performance principle" whose most basic characteristics were the reification of the body in the service of the work ethic and the concentration of mature

sexuality in the genitals alone. Despite Freud's approval of this condition, Marcuse contended that the current technological capacity to end scarcity rendered the performance principle functionally obsolete and with it the surplus repression entailed by genital sexuality. The alternative he proposed was the release of what Freud had called "polymorphous perversity," a return to an undifferentiated eroticism that would approximate the tensionless state of nirvana that Freud had seen as the goal of the death instinct. What had hitherto been tied to aggression and destruction would thus be rerouted in a life-fulfilling direction and the ultimate unity of the life and death instincts would be achieved. What Friedrich Schiller had called the "play drive" would become the new reality principle of a liberated mankind.

In an epilogue to the book, Marcuse attacked the neo-Freudian revisionists, including his former colleague Erich Fromm, for their desexualization of psychoanalysis and their premature attempt to smooth over the contradiction between society and psyche. For Marcuse, Freud's seemingly conservative stress on biological drives had an ultimately radical function because it implied that corporeal gratification was a necessary component of full human emancipation. Accordingly, *Eros and Civilization* earned him a reputation as a radical Freudian and placed him, at least in the popular consciousness, alongside Norman O. Brown and Wilhelm Reich, whose somewhat overlapping readings of psychoanalysis also nurtured the countercultural impulse of the 1960s.

Three years later, the publication of *Soviet Marxism* (1958), the fruit of Marcuse's tenure at the Columbia and Harvard Russian Research Centers, showed even more clearly than his previous work the extent of his disillusionment with orthodox Marxism and its alleged realization in the Soviet Union. Through an immanent critique based on Marxist principles, Marcuse analyzed Stalinism's systematic betrayal of the promise of socialism. Probing the consequences of trying to build socialism in one country in a hostile world, he showed the ways in which the Soviet Union had come to resemble the societies it allegedly opposed. Perhaps his most telling discussion concerned socialist realism, which he castigated as a harmonistic aesthetic grounded in the fallacious assumption that social contradictions had already been reconciled. Without holding out a promise of future happiness, that

"*promesse de bonheur*" of which Stendhal had spoken, art would become no more than "affirmative culture" serving the preservation of the status quo. True art, Marcuse claimed, must contain a critical, negative moment, even in an allegedly postrevolutionary society.

The disturbing weakening of that moment in prerevolutionary societies, in particular that of the contemporary United States, was the subject of Marcuse's next and perhaps most influential work, *One Dimensional Man* (1964). Subtitled *Studies in the Ideology of Advanced Industrial Society*, it analyzed the means through which potential social protest was contained by apparently noncoercive cultural mechanisms. An outgrowth of Horkheimer and Adorno's earlier work on the "culture industry," *One Dimensional Man* provided a phenomenology of false consciousness in mass society. The term "one-dimensionality" referred to the suppression of that multidimensional dialectic of reason and reality which had characterized the "negative" philosophies of Hegel and Marx as well as the "negative" political practice of the working class in its classical phase. On the level of academic philosophy, it signified the triumph of positivism; on the level of popular ideology, it meant the acceptance of technological rationality as the answer to political problems; on the level of personal happiness, it meant the shortsighted and shallow hedonism Marcuse called "repressive de-sublimation." Against the pervasive power of one-dimensionality, Marcuse could offer little more than an exhortation to think critically and preserve the negative moment of art in its still uncorrupted form. Only through a "great refusal" of the repressive totality of the present might the possibility of a future liberated society be maintained.

The irony of *One Dimensional Man's* essential pessimism lay in its influence on the movement it unexpectedly helped spawn, whose very emergence contradicted Marcuse's belief that the universe of political discourse had been closed to radical challenges. Taking seriously his widely discussed argument in his essay, "Repressive Tolerance" (Wolff, Moore, & Marcuse 1965, pp. 81-117), the more militant members of the New Left practiced the liberating intolerance of denying the right to speak of conservative spokesmen, especially those defending the Vietnam War. Marcuse's justification of a "'natural right' of resistance for oppressed and overpowered minorities to use extralegal means if

the legal ones have proved inadequate" was cited as support for acts as disparate as civil rights sit-ins and terrorist bombings, even though he explicitly condemned the latter.

In part because of the notoriety won him by this stance, several volumes of his earlier essays were reissued as *Kultur und Gesellschaft* (1965), *Negations* (1934-1938), and *Studies in Critical Philosophy* (1932-1969); they were supplemented by newer writings entitled *An Essay on Liberation* (1969) and *Five Lectures* (1957-1967). In the latter he returned to the speculative optimism of *Eros and Civilization* and combined elements of the erotic and aesthetic themes of his previous work to call for a more utopian emphasis in radical theory. Outlining a defense of basic human needs, which he grounded in a loosely defined notion of biology, Marcuse attempted to articulate and crystallize the often inchoate desires of the movement he had helped inspire.

Within a few years, however, Marcuse came to recognize the very serious limitations of the negative impulses he had praised in the 1960s. *Counterrevolution and Revolt* (1972) dealt with the strength of the preventive and cooptive powers of advanced capitalism into whose hands the anti-intellectualism of much of the New Left had played. The emancipation of sensibility he had celebrated in *An Essay on Liberation* had to be accompanied by an emancipation of rational consciousness, which could only be nurtured through rigorous education. To those who had understood him to say that universities were solely the instruments of repression, he replied that without an active intellectual life no serious threat to the status quo could be mounted. And once again, he cited art as a repository of negation in an era of counterrevolutionary consolidation, an argument he elaborated in his 1978 extended essay, *The Aesthetic Dimension*, which repudiated the instrumentalization of art in the name of immediate political efficacy.

However one may contest certain of Marcuse's specific arguments—and there has been no shortage of controversy over their validity—it cannot be doubted that he played a highly crucial role in the reorientation of radical thought in the second half of the twentieth century. By stressing cultural and aesthetic issues, he helped free Marxism from its economic bias. By developing psychological and biological themes, he enriched our appreciation of personal gratification as an ineradicable goal of drastic social change.

By insisting on the utopian moment in the socialist dream, he gave voice to the discontents of those resisting the false comforts of institutionalized socialist régimes. As a result he will remain long identified with the extraordinary generation of the 1960s, whose ideals and strategies he helped formulate. And paradoxically, he will be remembered as well by those eager to understand the ways in which those ideals were thwarted and those strategies undermined. For there have been few utopian writers with as keen a sense of the obstacles to utopia as Herbert Marcuse, at once the bold essayist on liberation and the somber analyst of one-dimensional man.

MARTIN JAY

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MARSCHAK, JACOB

Jacob Marschak was born in 1898 in Kiev, capital of the Ukraine, and died in 1977 in Los Angeles, California, still in active service as professor of economics and business administration at the University of California at Los Angeles and as president-elect of the American Economic Association. These bare facts suggest the long and varied odyssey of his career and his high, though slowly developing, position in American economics.

Marschak's scholarly career spanned 55 years and 3 very different environments: Germany of the Weimar period, the United Kingdom of the great depression, and the United States from World War II on. The world econ-

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omy and economists' perceptions and theories were altering rapidly, and Marschak experienced the additional variance due to international differences in academic environment, especially in traditions and modes of economic thought. To all these influences must be added the fact that he was a Russian and a Jew, whose educational and political formation derived from a particular period of Russian history that had little in common with the life of his host countries.

It is not surprising therefore that his focus as an economist should have showed considerable changes of direction over his long and productive lifetime. His early empirical and practical interests yielded increasingly to theoretical and methodological themes; his youthful political interests subsided, and he became increasingly aloof from political affairs and even from specific proposals of economic policy.

But there was an underlying continuity of purpose and approach through all the changes in Marschak's thought and work. At all times, it was marked by an extraordinary ability to synthesize. The problem in hand, whatever it might be, was to be approached from every useful angle. Every good idea and theoretical presupposition was to be drawn upon while being subjected to severe criticism of its utility, clarity of expressions, and contribution to the understanding of economic issues.

The process was marked by a remarkable openness to new ideas and methods, to which Marschak would add his own improvements and clarifications. These talents enabled him to become a leader of research organizations, first at a relatively young age in Germany, and, with increasing importance, as director of the Oxford Institute of Statistics (1935-1939) and as director of the Cowles Commission for Research in Economics at the University of Chicago (1943-1948), a fertile period of great influence on the course of economic analysis in several diverse fields.

Only after 1948 did Marschak begin to make the contributions to economic analysis that are most distinctively his own. Yet in curious ways the subject matter of his studies was consonant with the characteristics of his earlier career. An organizer of economic research, he became a theorist of organization. A student and critic of new developments in economic analysis, he developed the economics of information. A skeptic distrustful of received dogma, he studied the economics of uncertainty.

Another characteristic of Marschak's work is its recurrent interdisciplinary tendency. Some of his early papers dealt with class structure and with the emerging phenomenon of Italian fascism. From 1928 to about 1953, the titles of his papers were more narrowly within the field of economics as currently understood, though not infrequently their content referred to broader notions derived from politics, sociology, and, later, individual psychology. But his work on information and organization led to a series of experimental and theoretical studies on the psychology of decision making. During his last 15 years, he organized an interdisciplinary seminar in the behavioral sciences that provided a main source of contact among mathematical modelers with widely divergent substantive interests.

A more detailed account of Marschak's evolving contributions and career demands a chronological framework. His parents were well-to-do Russian Jews, assimilated in culture; he learned German and French from his governesses. He received some formal religious education but was never religious. His family sympathized with the revolution of 1905; Marschak in later life remembered the pogrom that followed. Refused admittance to Gymnasium at age nine because of the very small Jewish quota, he was admitted instead to the First Kiev School of Commerce.

After engaging in the very common group discussions about which revolutionary group to join, he became a Marxist in 1915, at the same time that he entered the Kiev School of Technology. He joined the Menshevik Internationalist (antiwar) faction, was arrested with others in December 1916, and was released with the fall of the Czar in February 1917. He joined the municipal government, a coalition that broke up in October in a three-cornered battle among supporters of the Kerensky government, the Bolsheviks, and the Ukrainians, who wanted a separate state; the last group won.

Marschak and his entire family left for a resort in the Terek region in the northern Caucasus. There political activity was also intense. The Bolsheviks took the lead in organizing all the Russian political parties against the Moslem mountaineers; the coalition was also intended as a counterweight to the Cossacks, with whom there was an uneasy alliance. Marschak became secretary of labor in this government. Within it, he led the Mensheviks and Socialist Revolutionaries in opposing recognition of the Bolshe-

vik government in Petrograd until the elected constituent assembly was allowed to meet. By June 1918, there was a three-sided conflict among the Mensheviks, the Cossacks, and the Bolsheviks, now allied with the mountaineers. Marschak was essentially a press relations officer, composing manifestos explaining the Menshevik government's aims. The government gradually fell under the control of a local dictator. Marschak eventually rejoined his family, and they all returned to Kiev, still under the Ukrainian government.

Like other friends and political colleagues, he felt there was no longer any viable political cause with which to associate. After some brief study of statistics at the Kiev Institute of Economics, he decided to take up economics in Germany. He studied for some six months at the University of Berlin in 1919. According to his later accounts, this was an important period; he attended the lectures of the economist and statistician Ladislaus von Bortkiewicz, and first learned from him the importance of mathematical and statistical methods in economic analysis. He then moved to the University of Heidelberg, where he received his PH.D. in 1922.

Economics in Germany was a broad subject; Marschak studied not only with the economist Emil Lederer, a strong advocate of quantitative analysis, but also with the philosopher Karl Jaspers and the sociologist Alfred Weber.

A number of Mensheviks were among the Russian immigrants into Germany. Because of the good relations between the Mensheviks and the German Social Democrats, the latter helped Marschak get started on his career. He was an economics reporter for the famous newspaper the *Frankfurter Zeitung* from 1924 to 1926, after which he joined the staff of the Forschungsstelle für Wirtschaftspolitik (Research Center for Economic Policy) in Berlin, sponsored by the labor unions and the Social Democratic party. He also spent some months in England in 1927 on a Rockefeller Foundation travelling fellowship, the first of several occasions when he was granted support from that source. His interests and aspirations shifted increasingly toward the academic. His Russian accent and Jewish origin were obstacles eventually overcome. He joined the staff of the University of Kiel's Institut für Seeverkehr und Weltwirtschaft (Institute for World Economics), headed by Bernhard Harms, a skillful academic entrepreneur with a good eye for quality in those who had difficulty finding academic posts. In 1930

he was appointed *Privatdozent* at the University of Heidelberg.

His work during the 1920s outside of journalism was largely devoted to industry studies; at Kiel he directed a large study on export industries for the Reichstag. Among the papers with the greatest permanent interest were his first (1923), on the raging debate started by Ludwig von Mises on the possibility of a rationally planned socialist society; he argued that the market system could not only be used under socialism but was likely to work better there than under the monopolistic distortions of actual capitalism. It is interesting to see here the earliest manifestation of his later interests in organization theory. The other major paper of this period was a study of the "new middle class" (1926), the white-collar workers who, it was argued, were economically workers but sociologically middle class. It was a detailed empirical study, a pioneer in its field.

His empirical work led him into studies of demand. He began to participate in the growing econometric movement (not yet so-named) in which formal statistical methods and economic theory were used jointly to interpret empirical economic data. His careful work on the elasticity of demand (1931*b*) was, with the contemporary works of Wassily Leontief and Ragnar Frisch, a major contribution to the development of the field. He also started, with Walther Lederer, a major empirical study of capital formation financed by the Rockefeller Foundation; because of the political upheavals, it was not published until 1936 in England, in the original German. In response to the depression, he was among the younger German economists who advocated compensatory public works policy years before the publication of Keynes's *The General Theory of Employment, Interest and Money* (1936).

In 1933 he immediately perceived the consequences of the Nazi accession to power. He had by this time achieved sufficient international reputation to have been invited to write two articles in the *Encyclopaedia of the Social Sciences* (1931*a*; 1935). Through the good offices of Redvers Opie, he was appointed Chichele lecturer at All Souls College, Oxford, a position designed for refugees. In general, English economics was far in advance of German, but Marschak's quantitative skills brought an element that Oxford lacked. Two years later, the Oxford Institute of Statistics was created, again with Rockefeller Foundation support, for sys-

tematic empirical work. Through Opie and Roy F. Harrod, Marschak was made director. Though treated with great reserve by most of the Oxford economics faculty, the institute became a world-recognized center of empirical analysis in economics. Marschak himself, with Helen Makower and H. W. Robinson, carried out a study of the geographical mobility of labor that represented probably the first true fusion of theoretical reasoning with formal statistical analysis in this area (1938). As befits the period, differentials in unemployment rates among cities were found to be a chief determinant of mobility.

His work on demand and on capital formation deepened and developed into more theoretical and methodological studies. The earliest of his papers that are still influential today were two on money and the theory of assets, one with Makower, in which he introduced the portfolio approach to the demand for money as one among a set of assets (Marschak 1938; Marschak & Makower 1938). He therefore stressed the relationship of money holdings to wealth, rather than income, as had the Cambridge and Fisher approaches, and above all, derived the demand for different kinds of assets from the uncertainties connected with their holding. Although ideas of this kind had long been informally expressed, this was their first true modeling. Since the expected-utility theory of behavior under uncertainty was then in limbo because it appeared to conflict with the ordinal concept of utility, Marschak had an alternative criterion function, an indifference map in the space of the first two or three moments of the probability distribution of returns.

In 1939 some methodologically and practically important papers on demand analysis appeared in several journals. In particular, they studied the usefulness of budget studies in developing the consumption function, showed how to combine budget and time series studies, and studied the aggregation of individual demand functions into a national total.

Marschak spent the period from December 1938 to August 1939 in the United States as a Rockefeller Foundation travelling fellow. Anticipating the outbreak of World War II, he arranged for his family to be brought over. In 1940 he was appointed professor of economics in the graduate faculty of the New School for Social Research, a faculty created largely to accommodate the flood of German refugee scholars and make use of their talents. At the same time he organized, through the National Bureau

of Economic Research, a seminar in mathematical economics and econometrics that served as a clearing house for a flood of new ideas, primarily, though not exclusively, for the growing group of European scholars; after the outbreak of the war they came not only from Germany but also from such occupied countries as Norway and the Netherlands.

One of the great motivating forces in quantitative research in this period was Jan Tinbergen's massive study (1939) of business cycles, conducted for the League of Nations. Like many large-scale studies, its most important scientific effects occurred after it was completed, in the reflections by participants and critics on the ways in which it could be done better. Ragnar Frisch had perceived while the investigation was under way that statistical inference in the case of simultaneous relations posed new problems not dreamed of in the philosophy of regression analysis. His student Trygve Haavelmo, in the United States during the war, developed the principles of maximum likelihood estimation of simultaneous equations (1943); this was among the papers presented at Marschak's seminar. The importance of Haavelmo's work was immediately recognized within a small circle in New York. Marschak wrote a paper on its general philosophy as early as 1942; the theoretical statisticians Henry B. Mann and Abraham Wald (1943) proved a fundamental consistency property of the estimates; and Tjalling Koopmans, who had earlier worked on Tinbergen's study and had found the distribution of the serial correlation coefficient (a closely related statistical problem), was drawn into the field of simultaneous-equations estimation.

At this moment there occurred an unusual conjunction of opportunities. In 1943 Marschak was appointed director of the Cowles Commission for Research in Economics and professor of economics at the University of Chicago. The commission had been founded and supported by Alfred R. Cowles, 3rd, an investment manager who had also been treasurer of the Econometric Society in its early days and contributed to its support. The commission had pioneered in the use of econometric methods; in particular, its prewar summer conferences at Colorado Springs attracted the leaders in the then small sect of econometricians. Marschak had attended the 1937 summer conference and then been offered the directorship.

During Marschak's directorship (1943-1948), the central focus of the commission's work was

the development of economy-wide models for predictive and structural analysis. For this two elements were needed; the further development of the appropriate statistical methods, and the economic theory for the equations to be fitted. For the latter the impetus came from John Maynard Keynes and his American followers. Marschak's receptivity to arguments about purchasing power enabled him to accept the Keynesian theory with little of the difficulties that many others had; but by the same token he lacked the dogmatism of many of the newly converted English and American economists. Several papers in 1940 and 1941 expounded Keynesian policies and theories and related them to earlier European thought, especially Knut Wicksell's; many of the methodological confusions in the literature on identities, equilibrium, and stability were cleared up in an important paper of 1942 (Marschak 1942*b*). Marschak supervised Franco Modigliani's dissertation at the New School, for a long time the most influential exposition of Keynes. But the highly specific discipline of fitting a complete model meant that the Keynesian apparatus had to be specified in far more detail than had been needed for more general expositions and vague policy statements.

There was a small but increasing band of young economists who found mathematics and formal thinking important tools. Moreover, statistical theory was just beginning to be taught and to attract significant numbers of students. Neither of these groups was in great demand. The academic market during the war and the immediate postwar period was not strong; mathematical economics was little regarded, and theoretical statistics had yet to find a suitable academic home. Marschak seized the opportunity to attract a remarkable staff at bargain prices. He secured a tenure faculty appointment for Koopmans, but the rest were appointed on a purely research basis. Lawrence Klein developed the model and the data. Haavelmo was there for a few years, working on both model development and statistical methodology. The main developments in theoretical statistics were carried out by Koopmans and the theoretical statisticians Theodore W. Anderson, and Herman Rubin. Kenneth J. Arrow and Leonid Hurwicz worked on problems of economic theory and statistical inference suggested by the basic modeling effort, though sometimes going far afield. Among the University of Chicago graduate students drawn into the work, mention must be made of Don Patinkin, whose thesis under

Marschak's supervision was a milestone in the integration of Keynesian and monetary theory with neoclassical general equilibrium analysis; his subsequent appointment at the Hebrew University of Jerusalem, where he refashioned the economics department, stemmed from a recommendation by Marschak to Fritz Naphtali, who had headed the *Forschungsstelle für Wirtschaftspolitik* during Marschak's stay. Indeed, the commission in this period had scholars as well as scholarship as an output, and it is a close question which was the more valuable.

Marschak's role was to give vision and drive. He did not impose direction; rather, he released the energies of the research associates and saw that they flowed in their own most productive channels. He insisted on understanding the essence of the most technical developments, and, by requiring explanations, forced the staff to understand their own work and its significance better. His introductions to the volumes in which the statistical methodology was presented penetrated remarkably deeply, clearly, and tersely to the essentials of the issues.

He also carried out one major study of his own, with William H. Andrews (1944), the first application in fact of the simultaneous equations technique to the estimation of production functions. Difficulties of interpretation had been found in the pioneering work of Paul Douglas; these were shown to be resolvable by the new statistical approaches.

During the same period Marschak reverted once to his old field of industry studies, though in a new context. Through his personal relations with Leo Szilard and the other European scientists at Chicago who had participated in the Manhattan Project, he came to feel that the economist's perspective was important in assessing the future of atomic energy. He initiated such a study at the Cowles Commission and wrote a summary introduction. The bulk of the actual study was done by others, particularly Sam Schurr and Herbert Simon, but Marschak's judgment was amply confirmed. Straightforward economic analysis showed that the impact of atomic energy could be nothing like the total transformation envisaged by the technologists and by many intellectuals.

Upon turning the directorship of the Cowles Commission over to Koopmans in 1948, Marschak moved very rapidly into pure scholarship, indeed into more abstract and theoretical work than anything before. He was greatly excited by the revival by John von Neumann and Oskar

Morgenstern of the expected-utility theory in its axiomatic form; by Abraham Wald's exploration of the foundations of statistical decision theory; and by the axiomatic development of subjective probability theory by Leonard J. Savage and by Rubin and Herman Chernoff at the Cowles Commission. His most immediate application was to his old subject, the demand for money and other assets (1949).

But his interests found new focus. Attempts to clarify the concept of liquidity showed the importance to economic behavior of anticipating new information. He was led to a series of studies on the economics of information. The value or demand price of information in any context was governed by the additional benefit that could be obtained by its optimal use. The approach was initiated in a paper of 1954 and summarized in 1971. The subject broached by Marschak has become a major research area, his most noteworthy immediate influences being on his student Roy Radner and on his colleague at the University of California, Jack Hirshleifer.

The importance of communication and its limits in the transmission of information led to a new approach to the study of organization, a simplification of the theory of games known as the theory of teams. It appeared in the same 1954 paper as the evaluation of information (an earlier version had appeared in French the preceding year at an extraordinarily interesting conference on the theory of risk bearing). A team is an organization in which the members have the same preferences and prior beliefs, but have different information and choose different actions. The problem is the choice of optimal decision rules, prescribing for each member his action as a function of his information. This framework makes precise the meaning of informational decentralization, a concept of such central importance in the controversy on socialist planning which was the subject of his first paper. Characteristically, the analysis does not so much solve the problem as put it in an entirely different and much more varied setting. His work in the theory of teams, with Radner, was embodied in a book (1972).

The third major area of his research after 1948 was stochastic decision. It is recognized that individuals are not thoroughly consistent, and yet the theory of rational behavior has some foundation. Individuals are assumed to make choices randomly about a rational (transitive and connected) pattern. This thesis has been developed both theoretically and experimentally;

typical papers appeared in 1960 and, with G. Becker and M. DeGroot, in 1963. This work so far has had greater impact on psychologists than on economists, though it has recently become the basis of Daniel McFadden's work (1976) on choice among alternative transportation modes.

In 1955 Marschak left Chicago for Yale University along with the Cowles Commission, now renamed the Cowles Foundation for Research in Economics. He did not stay long; in 1960 he accepted the post of professor of economics and business administration at the University of California at Los Angeles, where he remained. Official retirement did not change his activities at all; he remained as active in teaching and research when emeritus as he had been before.

For most of his American career, Marschak had had a reputation as being rather esoteric; and his later broad interests did not change that view. But widespread recognition grew, especially as his former students and junior colleagues themselves became information disseminators. He was elected fellow of the American Academy of Arts and Sciences in 1962, distinguished fellow of the American Economic Association in 1967, and member of the National Academy of Sciences in 1972; he received honorary degrees from the University of Bonn in 1968, his old University of Heidelberg in 1972, and Northwestern University in 1977. In 1976 he was finally nominated and elected president-elect of the American Economic Association for the year 1977. According to the custom of the association, the president-elect prepares the annual meeting and becomes president the following year. Marschak had completed all arrangements for the December 1977 meeting when he died in the summer of that year.

KENNETH J. ARROW

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MARSHALL, T. H.

Thomas Humphrey Marshall, a British sociologist, was born in London on December 19, 1893. He was the son of a successful architect and grew up in a well-to-do professional environment. He attended a public school with children of a similar class background. His genuine and enduring interest in class inequality and social problems did not emerge until he attended Cambridge University to study history. There he "first became aware of meeting young men of working-class origin on equal terms" (1973, p. 88). During his undergraduate years, the two teachers who influenced him most decisively were the economic historian John Harold Clapham and the American medieval scholar Gaillard Lapsley. As a result of his close reading of the classics of medieval social structure, he received a thorough training in the use of sociological categories. Then, during his four years as a civilian prisoner of the Germans during World War I in the camp at Ruhleben, he began to take a serious interest in the analysis of the development of organizational bonds and social institutions. In his brief autobiography, he wrote that "Ruhleben breached the defences of the secluded world of the bourgeois intelligentsia in which I had been brought up, but the full effects of this were not realized until I had engaged in another adventure after my return to England" (*ibid.*, p. 90).

This second great adventure was political in nature. Although Lapsley and Clapham helped Marshall to secure a six-year fellowship at Trinity College, Cambridge, he was unable to retreat into the cloistered world of academic history. He was persuaded to enter into electoral politics and was an unsuccessful Labour candidate in an overwhelmingly Conservative constituency in Surrey in 1922. Although he did

surprisingly well, considering the odds against him, he decided not to pursue his political career any further.

Marshall returned to Trinity College and did a fair amount of writing in eighteenth century economic history (e.g., 1925; 1929). He would probably have continued in this field if he had not received an appointment at the London School of Economics (L.S.E.) in 1925, where William Henry Beveridge, the director of the L.S.E., wanted him primarily to tutor students in social work. He lectured on English economic and constitutional history, but was soon attracted to the comparative analysis so characteristic of the two great sociologists then at the L.S.E., L. T. Hobhouse and Morris Ginsberg. When Hobhouse died in 1929 and Ginsberg succeeded to his chair, Marshall was invited to enter the department of sociology and teach the discipline known as "comparative social institutions." "And that," he wrote, "is how I became a sociologist" (1973, p. 91). He was not at all trained in any of the fields of sociology as defined at that time, had not done any work in sociological theory, and his methodological skills were essentially those of the historian.

In his early work, he concentrated exclusively on developments in England. After 1949, his work shows the influence of several years of intensive analysis of conditions in Germany. His early experiences as a prisoner had stimulated his interest in German affairs, and when World War II broke out, he enrolled in an organization established to monitor the foreign press for the British Foreign Office. He became an expert on German society and politics, and after the war spent 18 months as an adviser on educational affairs with the British Control Commission in Germany.

In fact, he devoted the best part of the ten years from 1939 to 1949 to administrative tasks. At the L.S.E., for example, he was the head of the department responsible for training social workers. It was only after this long period of administrative work had ended that he found the leisure time to concentrate on what he gradually saw as his primary task: the systematization of a historical perspective in the analysis of social structures. His outstanding contribution came in 1949 in the form of a lecture at Cambridge University in honor of the economist Alfred Marshall. Entitled *Citizenship and Social Class* (1950), this lecture established his reputation internationally. But this period of creative scholarship was to be brief. In 1956 he

became the director of the Social Sciences Division at the United Nations Educational, Cultural and Scientific Organization. In the four years he served UNESCO, he was involved in the worldwide development of sociology as a scholarly discipline, and in 1959 succeeded Georges Friedmann as president of the International Sociological Association. In 1960 he completed his term at UNESCO, retired from the L.S.E., and returned to his academic home in Cambridge. He lectured part time in the economics faculty, was active in establishing the discipline of sociology within that university, and continued his writing. He published *Social Policy* (1965), which offered ample illustration of the importance of a comparative historical perspective in the analysis of key concepts of social inquiry.

Citizenship and social class. Thomas Marshall will primarily be remembered for his contributions to historical sociology, and *Citizenship and Social Class* will remain a classic example of lucid conceptual analysis within the framework of an elegant scheme of developmental explanation.

The scheme caught the imagination of a wide range of scholars because of the great simplicity of the historicist linkup of a Marxian analysis of class position with a Weberian emphasis on the universalization of territorial citizenship. The quarrels within international Marxism had alerted the scholarly world to the difficulties of reconciling the claims of loyalty to the class with the obligations of citizenship in the nation-state. Marshall concentrated his analysis on the class-state, class-nation polarity, and tried to analyze the history of the extension of citizenship rights as the result of a sequence of counterstrategies in the struggle to maintain territorial-national unity in the face of the increasing bitterness of class conflicts under capitalism. The ascendancy of the capitalist mode of production had increased the potential for open conflict; the extension and the equalization of citizenship rights represented a crucially important complex of counterstrategies.

As a result of his research, Marshall established a simple typology of citizenship rights and an equally simple historical sequence of change:

(1) the eighteenth century brought *civil citizenship*: equality before the law, liberty of the person, freedom of speech, thought, and faith, the right to own property and conclude contracts;

(2) the nineteenth century expanded *polit-*

ical citizenship: the right to take part in elections, the right to serve in bodies invested with political authority, whether legislatures or cabinets;

(3) the twentieth century gave us elements of *social citizenship*: the right to a modicum of economic and social welfare, the "right to share to the full in the social heritage and to live the life of a civilized being according to the standard prevailing in the society" (1963, p. 74).

This three-step sequence was primarily based on the history of institutional change in Great Britain. Marshall was well aware that the sequences of change differed from country to country, but he did not try to work out any systematic scheme of explanation for the variations observed in the sequencing of steps. Scholars following in Marshall's footsteps have developed this three-step analysis into a broader comparative perspective, but have not found any reason to challenge his basic intuition (e.g., Bendix & Rokkan 1964; Rokkan et al. 1970; Flora et al. 1977). Marshall linked his analysis of citizenship with the concept of social class. He asked: How is it that the growth of citizenship coincided with the rise of capitalism, which is a system of inequality? What made it possible for these two processes to be reconciled with one another and to become, for a time at least, allies instead of antagonists? Marshall found that class prejudices and an unequal distribution of wealth were barriers to the full enjoyment of rights. Lack of economic opportunity curtailed civil rights. He argued that the capitalist class system and citizenship were at war in the early twentieth century. The development of rights did not occur without conflicts. Gradually, a dilution of class monopoly in the legal profession as well as the increase in all classes of a sense of social equality dampened—if not eradicated—social conflicts, and made citizenship rights more real.

In his lecture in honor of Alfred Marshall, T. H. Marshall first presented his three-step scheme. He took as his starting point Alfred Marshall's lecture, "The Future of the Working Classes" (1873 in *Memorials . . .* 1925), and discussed his reasons for believing that the widening of educational opportunities and the expansion of political rights would gradually tie the workers more closely to the fate of their nation-state and, as a consequence, reduce the revolutionary potential of class polarity. Marshall developed Alfred Marshall's analysis in

further depth and showed how the extension of political and social rights had "nationalized" the working class.

Marshall pursued his analysis of class and status in a number of articles, most of them reprinted in *Sociology at the Crossroads* (1963). His discussion did not go much beyond Weber's famous "class-status-party" trichotomy, but he wrote lucidly and persuasively about the historical changes in the weight of these distinctions, particularly in light of developments in Britain. He also contributed one or two important analyses of the trends toward greater professionalization and their consequences for the stratification systems of contemporary societies.

Studies of welfare and social policy. Marshall had taken a keen interest in problems of social welfare ever since his first appointment at the L.S.E. He wrote a number of papers on the development of the welfare state in the 1940s and 1950s (reprinted in Marshall 1963). He pursued this line of research in greater detail after his retirement in *Social Policy*. The questions he asked were essentially those of a historian: How did the elites of the Western political systems arrive at the conviction that the State should interfere in the market mechanism? What were the social and ideological tensions at the time of the breakthrough of modern social policies at the turn of the century? Marshall concentrated on the evolution of social reforms in Britain, but put this development in a broader European perspective, from the initiation of social insurance programs in Germany in the 1880s to their temporary culmination after World War II. He concentrated on the principles and aims of social policy and discussed a pattern of shifting aims over time, from an emphasis on the elimination of poverty, through a phase focusing on the maximization of welfare, to a phase of greater concentration on the pursuit of equality.

Marshall's historical-institutional approach has inspired many scholars to follow up his research with systematic, and at times, quantitative comparative analyses of the development of the welfare state (Flora et al. 1977; Kuhnle 1978). His interpretations of the processes shaping social policy have had a significant impact on the works of contemporary scholars. Social policy is increasingly viewed as part of a broader process, and not as a separate area of political action.

STEIN KUHNLE AND STEIN ROKKAN

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MASLOW, ABRAHAM H.

Abraham H. Maslow (1908-1970) first distinguished himself in psychology with his Uni-

versity of Wisconsin doctoral dissertation on "The Role of Dominance in the Social and Sexual Behavior of Infra-human Primates" (1936). Its research was carried out under the supervision of Harry F. Harlow, who nearly forty years later wrote: "Abe's most significant contribution was the [surprising] finding that dominance is usually established by visual contact, without the need to fight. . . . Maslow's data on dominance in monkeys was the final definitive research in this area for approximately thirty years, and to say that he was ahead of his time is [a vast] understatement" (Maslow 1973, p. 1). Harlow added that when Maslow left the University of Wisconsin in 1935, "he indicated that he was going to work strictly with people" (*ibid.*). Harlow was not, however, surprised by this decision; after all, he "knew all along that Abe's interests surpassed the simians" (*ibid.*).

Maslow's interests did indeed surpass the simians, extending to what he described in later years as the "farther reaches of human nature." As an undergraduate he was convinced that there were "wonderful possibilities and inscrutable depths in mankind" and that the world much needed a "larger, more wonderful conception of the greatness of the human species and the wonderful vistas of progress just faintly glimpsed against the future" (Lowry 1973, p. 15). By the late 1930s Maslow, then a professional psychologist, had begun to see that such a conception had to begin with a reconstruction of the conventional psychological understanding of human motivation. It was toward this end that he published his landmark paper "A Theory of Human Motivation" (1943c).

The conventional psychology of motivation held that only motives conducive to the preservation of the individual or the propagation of the species could be regarded as "primary," "basic," or "instinctive." All other motives—love, knowledge, beauty, and the like—could be regarded only as "secondary" and "derivative." In part this view was an extrapolation from classical Darwinian evolutionary theory. But in part it also stemmed from the assumption that a motive could be basic or instinctive only if it appeared universally in the species. Inasmuch as only the "gut drives"—hunger, thirst, sexuality, and the like—do appear universally in the human species, only these, *ex hypothesi*, could be basic motives.

Maslow's argument in "A Theory of Human Motivation" was that the "higher motives" fail to appear universally in the human species not

because they are less basic, but because they are less "prepotent." Even among physiologically based needs that are clearly basic and instinctive, some take precedence over others. Thus, if a very hungry man is suddenly deprived of breathable air, his hunger motive will vanish as though it never existed, utterly eclipsed by his prepotent and now urgent need for oxygen. But hunger does not cease to exist, nor is it "secondary" or "derivative." It is merely masked, or inhibited, by an unsatisfied need that happens to be more prepotent in the need-motive hierarchy. What is more, once the more prepotent need has been satisfied, the formerly masked need will return in full force.

Maslow applied the same line of reasoning to several classes of human needs that, despite failure to appear universally, seemed nonetheless to be "instinctoid" in nature. These were (1) safety and security, (2) love and belongingness, and (3) self-esteem. The need for safety and security was "basic" to human nature, but it could make its appearance only after the more prepotent physiological drives had been fairly well satisfied. So, too, with the need for love and belongingness, save that it had to wait as well for a moderate satisfaction of the more prepotent safety-security need; the same held for self-esteem.

This hierarchical scheme of human motivation could have been extended to include other classes of "instinctoid" human motives, but Maslow did not do so in the 1943 paper, except by brief allusion to the possibility of basic cognitive and aesthetic motives and to something that he rather vaguely described as "self-actualization . . . man's desire . . . to become actualized in what he is potentially." By 1950, however, these further possibilities had become much clearer to him. This was the year in which he published his famous "Self-actualizing People: A Study of Psychological Health" (1950). Although Maslow's concept of "self-actualization" soared at times to some rather dizzying heights, it began its flight with a perfectly straightforward observation concerning the effects of motivation on perception, cognition, and behavior. Few things are certain in psychology. One thing that is certain, however, is that "needs, drives, and motives" can rule behavior and distort perception and cognition. Maslow pursued that observation to its logical conclusion. What would happen if all of a person's "deficiency needs"—physiological, safety-security, love-belongingness, and self-esteem—were well satisfied? To Maslow the

answer seemed plain: behavior would no longer be "driven," and perception-cognition would no longer be distorted. Such a person, no longer laboring under the constraints of deficiency motivation, would be free to actualize his or her higher potential—provided, of course, that such a "higher potential" existed. Maslow was profoundly convinced that this potential did exist, not only in a gifted few but in everyone.

He was also convinced that he had actually found persons in whom this higher potential had been actualized. The announced aim of his 1950 paper was to describe empirically the characteristics of these self-actualizing persons. Although this description may have been strongly colored by Maslow's extra-empirical vision of what human nature is really all about (see Lowry 1973), for present purposes, one may take the "empirical description" at face value.

As Maslow saw it, the central characteristic of self-actualizing persons was that they had a "more efficient perception of reality and more comfortable relations with it." The more efficient perception was first noticed "as an unusual ability to detect the spurious, the fake, and the dishonest in personality, and in general to judge people correctly and efficiently." As Maslow got more deeply into the study of these persons, however, "it slowly became apparent that this efficiency extended to many other areas of life—indeed *all* areas that were tested. In art and music, in things of the intellect, in scientific matters, in politics and public affairs, they seemed as a group to be able to see concealed or confused realities more swiftly and more correctly than others" (1954*b*, pp. 203–204). They saw reality more clearly because their perceptions of it were undistorted by deficiency motivation: "They are therefore far more apt to perceive what is there rather than their own wishes, hopes, fears, anxieties, their own theories and beliefs, or those of their cultural group" (*ibid.*, p. 205). For the same reason they could also relate to reality more comfortably and acceptingly. Placing no unrealistic "neurotic demands" upon reality, they were able to accept and deal with external reality, and the reality of human nature: "As the child looks out upon the world with wide, uncritical, innocent eyes, so does the self-actualizing person look upon human nature in himself and others. . . . All (natural things) are accepted without question as worth while, simply because these people are inclined to accept the work of nature rather than to argue with her for not having con-

structed things to different pattern" (*ibid.*, p. 207). This was not to say that self-actualizing persons never feel discontent. Indeed they could feel it quite often and intensely. The difference, however, was that their's was not a "neurotic" but a healthy discontent: "The general formula seems to be that healthy people will feel bad about discrepancies between what is and what might very well be or ought to be" (*ibid.*, p. 208).

Maslow also noted that his self-actualizing persons tended to have far more "peak-experiences" than the average person. Being more open to reality and less threatened by it, they have "the wonderful capacity to appreciate again and again, freshly and naively, the basic goods of life, with awe, pleasure, wonder, and even ecstasy, however stale these experiences may have become to others. Thus for such a person, any sunset may be as beautiful as the first one, any flower may be of breath-taking loveliness, even after he has seen a million flowers" (*ibid.*, pp. 214–215). This, of course, was only to repeat that self-actualizing persons saw reality more clearly, through a lens unclouded by wishes, prejudices, and preconceptions.

The other characteristics that Maslow attributed to these healthy, self-actualizing persons all revolved around this central trait of "more efficient perception of reality and more comfortable relations with it." Thus the self-actualizing person was "resistant to enculturation"; identified with all of humanity (*Gemeinschaftsgefühl*); had a sense of privacy and preferred a few close friendships to many superficial ones; had a "democratic character structure"; exhibited a philosophical, unhostile sense of humor; was spontaneous and creative; and was "problem-centered" rather than "means-centered" in dealing with reality.

Maslow knew that such descriptions might make his self-actualizing persons seem too good to be true. This was contrary to his intention, for he did not want them to be seen as "stuffed shirts or marionettes or unreal projections of unreal ideals" but rather as the "robust, healthy, lusty individuals they really are." He therefore made a special effort to point out that self-actualizing persons were still, after all, only human: "Our subjects show many of the lesser human failings. They too are equipped with silly, wasteful, or thoughtless habits. They can be boring, stubborn, irritating. They are by no means free from a rather superficial vanity, pride, partiality to their own productions, fam-

ily, friends, and children. Temper outbursts are not rare" (*ibid.*, p. 228). Maslow went on to mention another characteristic that might seem at first to be a major human failing—that these persons were “occasionally capable of an extraordinary and unexpected ruthlessness.” But it must be remembered, he added, “that they are very strong people. This makes it possible for them to display a surgical coldness when this is called for, beyond the power of the average man” (*ibid.*, pp. 228–229).

Although self-actualizing persons were by no means “typical,” “normal,” or “average,” Maslow nonetheless saw them as the truest representation of human nature and human potential. The self-actualizing person differed from the “average” only in that his/her instinctoid potential was no longer masked or suppressed by unsatisfied deficiency needs. Let these needs be satisfied in the average person and self-actualization would begin to appear there, too. Maslow had held this view for many years, at least as far back as the time of his doctoral studies of primate dominance, probably earlier: “People are all decent underneath. . . . People are good, if only their fundamental wishes are satisfied, their wishes for affection and security. . . . Give people affection and security, and they will give affection and be secure in their feelings and behavior” (Lowry 1973, pp. 17–18). Similar reflections can be found in Maslow’s unpublished writings even as far back as his high-school years. In a sense he had been working on his “self-actualization” concept since early adolescence, before the thought of becoming a psychologist crossed his mind.

Most of the remainder of Maslow’s psychological career was devoted to extending and refining the conceptions put forward in his 1950 paper on self-actualization. A brief, partial list of his post-1950 publications will show the many paths that radiated from this central point: “Love in Healthy People” (1953); “The Instinctoid Nature of Basic Needs” (1954a); “Deficiency Motivation and Growth Motivation” (1955); “Creativity in Self-actualizing People” (1959b); “Cognition of Being in the Peak Experiences” (1959a); “Eupsychia: The Good Society” (1961); *Toward a Psychology of Being* (1962); “The Need to Know and the Fear of Knowing” (1963b); *Religions, Values and Peak-experiences* (1964); *Eupsychian Management* (1965); *The Psychology of Science* (1966); “A Theory of Metamotivation: The

Biological Rooting of the Value-life” (1967); *The Farther Reaches of Human Nature* (1971).

In the last year of his life, Maslow began work on a book that was to summarize his view of human nature as it had ripened over twenty years, planning to call the book “Higher Ceilings for Human Nature.” Although he died too soon after he began to develop his ideas extensively, he did leave a brief statement of what he would have written, had he been able: “If I had to condense this whole book into a single sentence, I [would say] that it spells out the consequences of the discovery that man has a higher nature and that this is part of his essence; or more simply, that human beings can be wonderful out of their own human and biological nature” (Lowry 1973, p. 77).

RICHARD LOWRY

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MEAD, MARGARET

A few minutes ago I stood at the edge of the sea, looking out toward the reef where twenty canoes, black silhouettes against the pale dawn sky, are out fishing. Very soon, the canoes, with their crews of people of all ages, will begin to come in with a catch of fish for early-morning breakfast. Half a mile away, rising above the water of the salt lagoon, a cluster of trees marks the site where in the old days the people of Peri village lived in houses set high on stilts. . . . That was where I also lived—in a Manus house with a high, thatched roof and a slatted floor through which small objects fell into the water—when I first studied the Manus people. . . .

This is my third field trip to Manus. On my first trip I studied the children; and now, thirty-six years later, these children are middle-aged. In 1928 I went to study the Manus because they had preserved their old, savage ways and had not yet taken on the ideas and the religion of the West. Twenty-five years later, in 1953, I returned to find out how

much they had changed over a lifetime of a generation and what had become of them as they changed. And now, eleven years later, I have come back a third time to follow their racing course toward modernization. . . .

Here in Peri, talking and listening, where a whole people have moved without losing their sense of continuity, I realize again how important it is to recognize both our common human capacity for change and for creating continuity in change. . . .

Each day I go about the ordinary business of field work . . . one person, all alone, face-to-face with a whole community. . . . [W]hereas the child learns as a part of growing up and becomes what he learns, the anthropologist must learn culture without embodying it, in order to become its accurate chronicler. . . . (Mead & Metraux 1970, pp. 299-311)

Such "letters from the field" are part of the record that Margaret Mead kept and shared through the past fifty years with "kith and kin," with the "peoples" whom she studied, with her fellow anthropologists, and with a planet-wide public of children, parents, and grandparents. Out of the obligation she felt to be a "chronicler" of anthropology, as well as an observing participant in its history and development, out of her willingness to give as full an account of her own life as that of the peoples she described, out of her belief that "nothing one has ever seen or heard, smelled or tasted is meaningless or unconnected with other things" emerged Mead's extensive and interlocking chronicles of her "working life" and her "personal life" (1965, p. 134). She not only provided rich field materials on Samoans, Manus, Arapesh, Mundugumor, Tchambuli, Balinese, Iatmul, and Americans ("the eight peoples who have admitted me to their lives"), but also on the anthropologist and person who lived and worked among them. She has left an Odyssean account of how she changed "with each step of the journey, with each new image . . . and each day in the field" (1977b, p. 15), and of her growing awareness of her own culture—given to her by her parents and grandmother, clarified by anthropologist "ancestors" and teachers, and "sharpened by many years of work among South Sea peoples." She hoped, moreover, that this dual revelation would help Americans "understand themselves" (1972b, p. 1).

Mead's lifetime as an anthropologist was virtually coterminous with the emergence and evolution of American anthropology as a social science—or "human science," as she preferred

to call it. In her senior year at Barnard College (1921/1922), she took her first anthropology course with the man who subsequently became her mentor: Franz Boas, a founder of modern anthropology and the father of American anthropology. Boas' teaching assistant in that course was Ruth Benedict, then a graduate student, who later achieved renown in her own right as a pioneering anthropologist. She became one of Mead's closest colleagues and most intimate friends, and in 1948, upon Benedict's death, Mead became her biographer, and the custodian of her field notes and papers (1949*b*; 1959*a*; 1968*a*).

Mead's decision to enter anthropology, rather than to be a psychologist, sociologist, writer, or politician (each of which she considered during her college years), was made "in the excitement of a genuine vocational choice." The sense of vocation that anthropology aroused in her was quasireligious. She had been searching for the best way to "make a contribution" through the medium of science; to use fully whatever her "talents" and "special gifts" (in the Biblical sense of the term); to do "work that mattered," that "had to be done now," that would further "understanding of human behavior, and that would be effective in the world of human events" (1962). In these fundamental ways, anthropology "called" Mead, and in 1924, after completing an M.A. in psychology at Columbia University, she committed herself to the discipline. Her vision of anthropology was deeply influenced by her two major teachers, Boas and Benedict, both of whom "were able to convey a sense of urgency, of the need to record those unique primitive cultures which once gone could never be recovered. . . . When I was a graduate student I used to wake up saying to myself, 'The last man on Raratonga who knows anything about the past will probably die today. I must hurry'" (1972*a*, p. 320).

Mead took seriously the Boasian dictum that irrespective of what special phenomena an anthropologist may single out to study, he or she has both an intellectual and a moral responsibility to describe each precious, vanishing culture in enough detail so that it is "kept for all mankind, including that particular people" (1974*a*, p. 298). Several of her early monographs—"Social Organization of Manu'a" (1930*b*); "Kinship in the Admiralty Islands" (1934); *The Mountain Arapesh* (1938–1949)—were written in the comprehensive, whole culture way advocated by her mentor. In all of her work,

throughout her career as an anthropologist, Mead's ardor about collecting and recording cultural data was noteworthy.

Out of Mead's commitment to anthropology as a human science, an "unmapped country" (1962, pp. 120–121), and a "giant rescue operation" (1959*b*, p. 30), there not only came 44 published books (18 of them coauthored), more than 1,000 articles and monographs, and the manuscripts of countless lectures and conferences, but also carefully preserved and systematically catalogued field notes, notebooks, verbatim interviews, drawings, maps, genealogical charts, photographs, films, tape recordings, and artifacts. Mead prepared these archives and made them available to interested scholars so that those who came after her might "begin their explorations where [her own] mapmaking ceased" (1962, pp. 120–129). But in addition to being an exceptionally rich source of ethnographic data, these materials comprise a first-hand record of a seminal period in American anthropology. They are the "living stuff out of which anthropology . . . developed as a science" (1977*b*, p. 1); the body of assumptions, theories, methods, and facts on which Mead drew during the half-century that her work spanned, and to which she so significantly contributed.

These same archives also constitute a unique, three-generational cultural biography and history of the numerous peoples among whom Mead was a long-term, recurrent participant observer. Her dedication and endurance, her "capacity to perceive and abandon herself to raw data," her "miraculous memory" (Lee 1954, p. 1109), and her prolificacy enabled her to set down and transmit extraordinarily detailed, massive, and long-term accounts. But above all, it was her continuous field work that made these chronicles distinctive.

When Margaret Mead left Manus in 1928, at the end of her first visit, the slit drums sounded the tattoo for the dead, as her canoe pulled away from the shores of Pere village. The people mourned her as they mourned for their dead, because they sorrowfully believed that she would never return. And this would have been an accurate prediction to make about the majority of anthropologists, who come to know one, or at most several peoples, through a single, prolonged field experience. A few will make a subsequent, follow-up trip to the site of their original work; others will carry out restudies of places observed by previous field workers. But, in contradistinction to these general field pat-

terns, Mead returned "again and again . . . to the same place[s] and the same people[s]" (1972a, p. 12), making some 24 trips to do so, and visiting one site—Pere village, Manus Island, Papua, New Guinea—6 times, over the period 1928 to 1975, to study the remarkable cultural transformation the community underwent as it moved "from the end of the Stone Age to the Electronic Age" (1976b, p. 64) in a single generation.

Mead's major field expeditions, as listed in her curriculum vitae, were: Samoa (1925–1926); Manus, Admiralty Islands, New Guinea (1929); an American Indian tribe (1930); Mountain Arapesh, Mundugumor, and Tchambuli, New Guinea (1931–1933); Bali, Iatmul in Tambunan, New Guinea (1936–1938); lecture tour of wartime England (1943); lecture tour of Australia (1951); restudy of Manus, Admiralty Islands, New Guinea (1953); return to Iatmul, Sepik (May–July 1967); return to Admiralty Islands for a National Educational Television film (August–September 1967); return to Manus, Admiralty Islands, New Guinea (July–August 1971); return to Mundugumor, Iatmul, New Guinea (1971); return to Samoa (1971); restudy of Mountain Arapesh group resettled in New Britain (1973); and return to Manus, Admiralty Islands, New Guinea (1975).

The fact that Mead considered her lecture tours in England and Australia as field trips was consistent with her concept of field work as a continuous activity to be conducted wherever one went: "in the office . . . down the hall . . . in a small village in Europe" (1965, p. 134), lecturing, and through correspondence with her vast readership. She saw her audiences as informants whose questions and comments taught her how to "write a book which will make the life of a remote island people meaningful" to American readers, or conversely how to "explain Americans" to other peoples (1965, p. 126). Although she claimed to be equally at home in each "far away place" that she visited during her many years of field work, America was the place to which she always returned at the end of each journey, and Americans were the primary people for whom she lectured and wrote.

Mead's earliest works were published at a time when the anthropological concept of culture was relatively new, and the wide variability of culture patterns from one society to another was just being discovered and documented. In *Coming of Age in Samoa* (1928a), *Growing Up in New Guinea* (1930a), and *Sex and Temper-*

ament in Three Primitive Societies (1935), she attempted to show the important role that culture plays in shaping human attitudes and behaviors, both within and between societies, focusing descriptively and analytically on adolescence, child rearing, and sex roles.

Mead was the first anthropologist to deal with women in a cross-cultural perspective. She did so within the larger framework of her studies of how "different peoples can cast males and females now for one role, now for another" (1949a, pp. 31–32). In *Sex and Temperament in Three Primitive Societies*, she concluded that: "If temperamental attitudes which we have traditionally regarded as feminine . . . can so easily be masculine in one tribe and in another outlawed for . . . women as well as . . . men, we no longer have any basis for regarding such aspects of behavior as sex-linked" (1935, pp. 279–280). In a later work, *Male and Female* (1949a), she shifted her perspective, placing greater emphasis on biological aspects of the behavior of girls and boys, and women and men than she had in the past, and on the "essentials" and "regularities [in] maleness and femaleness with which every society must reckon" (p. 32).

Mead was one of the founders of the culture and personality school of anthropology, and the first to do psychologically oriented field work. Within the area of culture and personality, she created the subfield of socialization and enculturation. She was the first anthropologist to study child-rearing practices (1930a). Over the years, she systematized and consistently refined methodological procedures for the precise observation and fine recording of child behavior (1946b) and enculturation: "the actual process of learning as it takes place in a specific culture" (1963, p. 185). Among anthropologists, Mead is best known for her insistence on "the most minute correspondence between the over-all patterns of a culture and the patterns of child rearing in that culture" (*ibid.*).

Mead made important contributions to the study of national character as well (1946b; 1951d; 1954; 1961; 1963), primarily as a defender of the way that Benedict, Geoffrey Gorer, and others described and delineated the modal character structure of a culture, and its infant care practices. In response to criticism, Mead attempted to make clear that "the theory . . . does not trace the origins of the culture" (1954, p. 404), but only the process of enculturation, adding that it was not discrete acts such as swaddling or weaning that communicated cul-

ture to children, but rather the cultural context in which these acts were embedded (1963, p. 186).

From the outset of her research, and in every subsequent phase and facet of her work, Mead was consistently concerned with methodological issues. She devoted a great deal of attention to anthropological sampling procedure (1951*d*; 1953*a*; 1954; 1955; 1961; Mead & Metraux 1953), and to the philosophy and techniques of field work (1930*a*; 1939*a*; 1951*a*; 1952; 1960; 1970*a*). She was one of the first American anthropologists to rely more on participant observation than on interviews with informants. In New Guinea, she wrote, "we built our house without walls, so that we could always see what was going on" (1949*a*, p. 41). She recommended that the anthropologist learn relevant native languages, and she considered it useful to acquire key native skills, such as paddling a canoe or dancing (1939*a*). In these respects, her style of field work was highly participatory, involving as close to total immersion in the culture as possible. Mead's approach to field work accorded with her conviction that anthropology should be a data-based discipline, in which "loving preservation of the actual detail contrasts with the . . . abstractions and generalizations of the scientist" (1951*b*, p. 151). "Our data," she wrote, "are hypothesis forming, not theory validating," and the anthropologist should avoid "tight taxonomic systems . . . [which] cramp and distort the necessary uncommittedness of our approach" (1960, reprinted in 1964*a*, p. 6). In fact, she felt that learning a culture was like learning a language whose grammar had not been previously described: both required the ability to "recognize pattern" (1952).

Mead pioneered in the use of photography and film in the field. She regarded them as media that enriched and refined the perceptions of the participant observer, and enhanced their validity and reliability. Cues that were once imperceptible could now be isolated and studied on the screen. Furthermore, the existence of a photographic record permitted reinterpretation after a period of field work had been completed (Mead & MacGregor 1951). *Balinese Character* (1942), coauthored with Gregory Bateson, was Mead's pathbreaking contribution to the use of photography not simply as ethnographic illustration, but as a detailed and rigorous form of cultural analysis. The book included more than seven hundred candid photographs by Bateson

of behavioral sequences, highly organized and integrated to depict major features of Balinese life, and certain theoretical concerns of the authors. Mead and Bateson went on to make numerous films, and to create a huge library of "filmic description." Of these, "Bathing Babies in Three Cultures" became an anthropological classic. "In the history of the discipline," anthropologist Hildred Geertz has written, "it may be said that this film was the starting point of the whole field of kinesics" (Geertz 1976, p. 725).

Mead was equally crucial to the development of methods for studying cultures that are spatially or temporally inaccessible. She did this primarily through the Columbia University Research in Contemporary Cultures project, of which she became the director upon Benedict's death. This project involved the study of cultures "at a distance," by interdisciplinary teams that collected and analyzed interviews and literary and other documentary materials in order to prepare ethnographies where direct field work was not feasible. Out of this research came many volumes of verbatim interviews with French, Czechoslovakian, Polish, Russian, east European Jewish, and other immigrant informants that constitute a valuable source of data for contemporary ethnohistorians.

Mead was also ahead of her time in a number of applied areas of research that involved the forging of interdisciplinary links between anthropology and other fields. Among her most outstanding contributions in this regard was the work that she did on nutrition. In 1941, at Benedict's request, Mead took charge of the United States government's Committee on Food Habits. Although her research was policy-oriented, Mead's work on food anticipated certain recent, basic developments in the field of biomedical anthropology (Montgomery & Bennett, forthcoming). She defined food habits as a "culturally standardized set of behaviors" (1943*b*, p. 20), and presented a systematic and detailed methodology for their study (1945), emphasizing the concept of a cultural definition of food, and the psychodynamics and cultural context of food habits (1950).

Mead's interest in what today would be called biomedical anthropology extended beyond the area of nutrition to the cultural patterning of biological processes (1947*c*), maturation and aging, and growth and development (1950). She was the first scientist to discover that women who had never borne children could lactate

(1957, p. 375), and her discussion of the Gesell-Ilg approach (1947*d*) to the study of maturation—in which she hypothesized significant differences between those cultures that elaborate normal biological rhythms and those which disrupt them—was one of the first attempts to incorporate biology into culture and personality theory.

No other anthropologist has described child-birth, lactation, and infant care practices in as much detail as Mead. In *Male and Female*, she wrote that “the mother’s nurturing tie to her child is apparently so deeply rooted in . . . actual biological conditions that only fairly complicated social arrangements can break it down entirely. . . . Women may be said to be mothers, unless they are taught to deny their child bearing qualities” (1949*a*, pp. 191–192, 230–231). Later, Mead stressed the significance of ethological studies of maternal–infant behavior within the framework of human evolution. She described her “continuing attempt to treat mothers and infants as a single transactional system, biologically, psychologically, culturally, and situationally defined” (1974*a*, p. 316; Mead & Newton 1967, pp. 142–144). She condemned modern “methods of obstetrical and pediatric care . . . [which] were invading the neonatal period . . . separating . . . mother . . . from the newborn . . . and enforcing methods of feeding and care which did not take into account the delicate circular relationship between . . . mother and child” (1957, p. 370; 1949*a*, p. 192). And she even suggested that through cultural rules “enjoining upon the mother active or placid behavior, the process of learning may begin within the womb . . .” (1949*a*, p. 61).

It was Mead’s interest in maternal–infant interaction which led to her work on nonverbal, tactile, kinaesthetic, and symbolic modes of learning. In Bali (1936–1939), Mead had noticed that infants responded not to the “surface expressions” of their mothers, but to kinaesthetically experienced emotions and clues (1957, p. 375). “. . . The mother smiles and bows as she greets . . . the stranger . . . but the baby in her arms screams with terror. . . . It is reacting to a kinaesthetically experienced fear which the mother expresses neither verbally nor in her face or posture” (*ibid.*). Mead felt that she had discovered an entirely new kind of learning, a process akin to the ethological concept of imprinting (1958). Such learning, un verbalized and inarticulate, “can

only occur when the . . . model is present” (1958, p. 487), she affirmed, and is as different from adult learning as is “learning to speak and later learning a second language” (1954, pp. 402–404). Mead’s work on learning theory may well be her most significant contribution to the study of enculturation, and only in recent years have other anthropologists begun to focus on learning through imprinting-type processes (Wallace [1961] 1970, pp. 157–158).

Mead commented that her interest in the “perception of nonverbal cues was later extended from the original impetus [of kinaesthetic learning] . . . to the elaboration of the whole field of semiotics and the use of audiovisual recordings of behavior” (1974*a*, p. 314). It made her sympathetic to research on psychic phenomena—which she viewed as part of “a whole continuum of communication phenomena” (1977*a*, pp. 49–50)—as early as 1943 (1974*a*, p. 314), and she was instrumental in gaining scientific recognition for the American Society for Psychic Research (Long 1977). For Mead, enculturation itself was a process of communication, and experience might be transmitted through symbolic means:

. . . *simultaneity of impact* is carried not only by the behavior of each individual with whom the child comes in contact, but it is also mediated by ritual, drama, and the arts. The shape of a pot, the design on the temple door, the pattern of the courtyard . . . are . . . reinforcements . . . of the same pattern which the child is experiencing serially. (1947*e*, p. 634)

In . . . societies where change is very slow . . . each child has a chance to learn from different parts of his experience. Some children will learn from the shape of the roof, as they lie on their backs looking up at the thatch, others from the feel of the pot in their hands, still others from the cadence of voices, but. . . these things are all part of a whole, shaped and polished, ground down and tuned up through the generations. (1957, p. 376)

Mead was the first anthropologist to introduce the symbolic interpretation of culture later elaborated by anthropologists such as Victor Turner and Clifford Geertz. Ritual, art, and the body itself were metaphors for culture (Mead & Bateson 1942; Mead & Wolfenstein 1955; Mead & Metraux 1953). In Mead’s view, because of the correspondence between childhood experiences, interpersonal relationships, and ritual and symbolic forms (1947*a*; 1949*a*), analysis could start at any point, but symbols which embodied distilled and multiple meanings pro-

vided keys for understanding the pattern. Mead began her work with Benedict's (1932; 1934) concept of culture as pattern. Her interest in the individual led to a redefinition of culture as the "plot" which underlay human behavior (1935; 1939*b*). And her work in communication further refined her concept of culture until in 1974 (1974*a*) she wrote of culture as "intricate and . . . improbable . . . shaped by thousands of imaginations through the centuries . . . a work of art, multisensory and multidimensional in multimedia."

Throughout her work, regardless of its empirical focus or the theoretical and methodological issues it addressed, Mead was always "interested in applying the knowledge gained through comparative studies to effective responsible intervention in our own culture" (1962, p. 121). This was true even in her earliest studies: *Coming of Age in Samoa*, *Growing Up in New Guinea*, and *Sex and Temperament in Three Primitive Societies*. She was increasingly certain that because anthropologists are trained in a comparative perspective, they have a particular obligation to avoid the "arrogant," "trivializing," and "parochial" overspecialization that, in her opinion, had fragmented the social sciences, and made them "relatively impotent in a time when [their] insights . . . are grievously needed" (1964*b*, p. 6; Webb 1972, p. 108).

World War II reinforced her convictions in these regards, and greatly intensified and expanded her commitment to "applied anthropology": "The atomic bomb exploded over Hiroshima in the summer of 1945. At that point I tore up every page of a book that I had nearly finished. Every sentence was out of date" (1972*a*, p. 296).

In Mead's perspective, the war had precipitated extraordinarily rapid and pervasive culture change on a global scale: change fraught with great "hope" and "promise," and at the same time, with potential "chaos" and "disaster." It was these historic developments that sent her back to Pere, the Manus village in the Admiralty Islands, and from there, to other Oceanic villages, to make restudies. Now she went to observe and record the "incredibly" accelerated change that these people were undergoing, as they took on "new lives for old" (1956*a*), and to analyze the larger significance of their cultural transformation. It seemed to her that what was happening in Manus was both a microcosm and a paradigm of the world-wide change that was occurring:

. . . The Manus simply moved themselves from a Stone Age culture into the present. They looked their culture over. They inventoried it. They thought about it. They decided what they would keep and what they would discard. . . .

When anthropologists started looking at the way in which the Manus used American society as a model, we began to realize . . . *what it is like to build a society when you have no model*. . . . We have *no* models of how to build a planetary society. . . .

As we looked to people like the Manus, . . . I began to realize what we will be up against in the future and why we had the generation gap of the mid-1960s. . . . For the first time we saw young people who had *no* models—they would never be the people their parents and teachers were. For the first time in human history all the older people had to look at all the young ones and realize that they were not their successors as they had been in the past. There would never be young people just like them. (1976*a*)

The war and her return to Manus had altered significantly the assumptions about change that were inherent in her earlier work (1928*b*; 1932). The idea that change must be slow to be effective no longer seemed tenable, nor did the notion that it took at least two generations to occur. Furthermore, in contrast to Mead's insistence in *Growing Up in New Guinea* that change had to take place in adult society first if it was to have lasting effects on the lives of children, it now appeared that the opposite had been true in Manus: it was the children, not yet completely involved in the old adult roles, who were the chief transformative agents in the society, changing themselves and bringing about changes that they conveyed to the adults.

Galvanized by the Manus case, throughout the 1950s and into the 1960s, Mead moved back and forth, observationally and analytically as well as geographically, between Oceanic villages and mainland United States, with shorter trips to a number of other countries. She was impressed by the "terrible division between the elders . . . and the young" in virtually every society (1972*a*, p. 122). And it was the phenomenon of the "generation gap" that she examined in *Culture and Commitment* (1970*b*), a panoramic summing-up of her post-World War II insights about cultural development, evolution, and change. In this work, she further developed a triad of interrelated concepts she had first used in 1961: "prefigurative," "postfigurative," and "cofigurative" culture. At the present time, Mead wrote, many societies have under-

gone such fast and extensive change that they have a prefigurative culture, in which the experience of children and elders is so different that elders learn from the children, as much as the reverse. This sharply contrasts with the postfigurative culture of more stable and traditional societies, in which the behavior of elders is an exact model of what the young are expected to be and will learn to become. Falling somewhere between these two polar types is the cofigurative culture, characteristic of a society in fairly rapid change, in which both the children and adults learn from their peers, who are their contemporary role models.

It was not only Mead's empirical insights and theoretical formulations concerning cultural change that underwent modification during and after the war, but also her world view. What took place was not so much a discontinuity in her outlook as a magnification of it. At the end of her presidential address to the American Association for the Advancement of Science, Mead speculatively suggested that in the future, "we shall need still newer kinds of instrumentation—macroscopes that can simplify without distorting the complexity of our knowledge of the biosphere and the cosmos within which a recognition of all disciplined human endeavor must now take place" (1976c, p. 909). Mead seemed to be visualizing the consequences of the world-wide cultural change through such a macroscope. This imagined instrument enlarged her sense of scientific urgency and anthropological mission so that they became both more cosmic and more apocalyptic. One of the major themes that appeared in her publications and lectures throughout the 1960s and the 1970s is the notion of how interdependently vulnerable we have become on this "small," "precious," "fragile," and imperiled earth that we inhabit, where not only our collective "future," but our very "survival" is threatened. Mead's affirmation in the face of what she considered this ultimate but very real possibility was an amplified version of the scientific, religious, and social action convictions that had always been fundamental to her vocation as an anthropologist. But for Mead, the original mission with which she entered anthropology—Boas' "giant rescue operation"—had become planetary:

. . . Knowledge joined to action . . . can protect the future. . . [K]nowledge about mankind sought in reverence for life, can bring life. (1972a, p. 296)

Within the new possibilities of analysis and procedure provided by science, humanity can begin to face the overwhelming responsibilities of this period of human history, when the great religious truth of the unity of all mankind must be actualized, or the people perish—all of them everywhere in the world. . . . [E]ach man is indeed his brother's keeper, and the need to love our enemies must be given new concrete scientific and religious meaning. . . . [F]aith and the human sciences are needed to erect a social order in which the children of our enemies will be protected as surely as our own children, so that all will be safe. (1972b, pp. 2–3)

Children and young people were always at the center of the analyses of socialization, education, and enculturation, and of cultural continuity, evolution, and change with which Mead was continually concerned. In the 1970s phase of her work, they became even more symbolic for her, signifying our "trusteeship for the future of all peoples" and of our "endangered planet." The children are the *raison d'être* for "growing chaos . . . to be ordered again upon a greener earth." They are also the vehicles of survival, for there is the hope that in them and through them, what has been, what is, and what will be will not only be perpetuated, but also united. "Peace and Blessing for all our children's children" read the inscription on Margaret Mead's Christmas card for 1973, alongside of a photograph of her own granddaughter; on her 1975 Christmas card there was a photograph of her grandchild and herself together, with the message, "To Cherish and Protect the Life of the World." And in her "rap on race" with James Baldwin (1971), she made the following affirmation: "I can take any people in my arms. . . . If we start off with . . . commitment to the future and the unborn, and these are the people that we hold in our arms . . . then what we make of the past is what we give them as part of the future . . ." (pp. 202, 220, 237).

Because of her belief in the importance of making anthropological findings available to all who might use them, Mead devoted an exceptional amount of attention to presenting her field to nonanthropologists—to specialists in related fields, and to "the public." This commitment underlay her conscious decision to write in "ordinary English," eschewing the "technical jargon" and omitting the "many native terms" (1965, pp. 121–122) traditionally employed in professional monographs. It also motivated her to write and lecture widely on a vast range of topics and concerns, including her field experi-

ences, the birth and raising of children, birth control and abortion, adolescence, sex and marriage, the position of women in society, the future of the American family, women and children, education, race and ethnicity, mental and spiritual health, religion in the modern world, aging and death, science and technology, ekistics (the study of habitats and human settlements), the problems and ethics of overpopulation, food and hunger, ecology and environmental pollution, community planning, culture-building, transnational relations, and peace.

Mead participated, and assumed leadership, in countless scientific, civic, humanitarian, and service organizations. She lectured to all kinds of audiences—in schools, before scientists' groups, and in women's clubs, in as many parts of the country as possible (1965, p. 126), published numerous articles in newspapers and popular magazines, prepared a monthly column for *Redbook Magazine*, wrote and narrated anthropological films, and made herself widely available to the mass media, through her willingness to be interviewed, photographed, and to appear on television. Her books are sufficiently well-written, with "extraordinary vividness and semblance to life" (Kroeber 1931) for several of them to have become best sellers, notably: *Coming of Age in Samoa*, *Cultural Patterns and Technical Change* (1953b), and *Culture and Commitment*. In and through these roles, "for almost a generation," she not only "symbolized and represented an entire scientific discipline to the general public" (Webb 1972, p. 102), but also (to quote from the citation that accompanied the presidential medal of freedom that she was awarded posthumously), "a teacher from whom all may learn." In the words of Jacquelyn Anderson Mattfield, president of Barnard College: "Her classroom was the world and its citizens her students. . . . She was an educator for all seasons."

Margaret Mead may well have been the most famous of anthropologists, and she "achieved a degree of eminence rare in the history of science" (Webb 1972, p. 102). She was accorded 28 honorary degrees from various universities and colleges, both in the United States and abroad—many of them institutions where she had taught or lectured. In addition, she received more than forty awards. These included academic and scientific honors, such as the Viking medal in general anthropology of the Wenner-Gren Foundation, honorary fellow in the Royal Anthropological Institute of Great

Britain and Ireland, and the omega achievers award in the field of education; membership in the National Academy of Sciences, the American Philosophical Association, the American Academy of Arts and Sciences, and the World Academy of Arts and Sciences; and election to the presidencies of the American Anthropological Association, the American Association for the Advancement of Science, the Scientists' Institute for Public Information, the Society for General Systems Research, the World Federation of Mental Health, and the World Federation of Ekistics. She was also the recipient of numerous public and media awards, among them, those that cited her as "woman of the year in science and research," "woman of conscience," "outstanding woman of the twentieth century," "world citizen," and "mother of the world."

The fact that Mead chose to become a public and applied, as well as an academic anthropologist, that she had the intellectual and literary ability, motivation, energy, charisma, and know-how to do so on a national, international, and global scale, and that as a consequence, she became an almost legendary figure, made her the target of criticism and controversy, as well as the subject of praise and esteem. She was "lambasted and lampooned" by the media and the public, "lionized and mythologized, called an institution and a stormy petrel, and cartooned as a candidate for the presidency wearing a human skull around [her] neck" (1972a, p. 288).

Many of her fellow anthropologists responded ambivalently to her personality, her publications, and her pronouncements. This ambivalence was continually expressed, for example, in the reviews of Mead's books that appeared in *American Anthropologist* over the years:

. . . [A] piece of work need not be ethnographically unreliable because it is aesthetically effective. And an artist Margaret Mead surely is. . . . When she leaves her vein to intellectualize about American educational tenets of 1930, or to compromise between the amount of cultural fact that ethnologists want and the public will stand, her own peculiar quality rapidly evaporates. . . . (Kroeber 1931, pp. 248–250)

It is useful. . . for the insights it provides into one of the most productive and controversial, and certainly most widely known, members of our profession. One is the buoyant enthusiasm with which she urges everyone into the anthropological swim, holding forth promise of unlocking the secrets of man's behavior to anyone willing to spend time in

anthropological studies. . . . [S]he appears to make the assumption that anyone who has mastered anthropology can do all the things that she has done. . . . Few are likely to be deceived by this assumption, and meanwhile her enthusiasm is infectious and often charming. . . . (Gladwin 1956, pp. 764-767)

The way that Margaret Mead's contribution to basic and applied anthropology has been perceived by anthropologists was not only influenced and complicated by the intricate social and cultural role that she played, and by the ethos of the discipline that she pioneered, but also by her status as a singular woman in her profession. At the January 1979 memorial program for Mead in the American Museum of Natural History, Jacquelyn Mattfield pointed out that:

. . . in spite of publications of acclaim and wide readership, Margaret Mead's *academic* career looks on paper very like that of any other person in the small number of able women affiliated with major universities between 1930 and 1965. The out-of-main-stream titles "Lecturer," "Guest Professor," "Adjunct Professor," "Visiting Professor," which Margaret Mead, like those other academic women of her generation, carried for all but the last phase of her career, in no way reflect the distinction of her career as an educator or anthropologist.

One of the least understood aspects of Mead's commitment was the degree to which she was willing to offer up her work, her resources, her whole person, and all aspects of her life as "instruments" on behalf of the scientific-humanistic anthropological goals to which she was dedicated. In the prologue to her autobiography, *Blackberry Winter* (1972a), she addressed herself to "young people, young enough to be my own great-grandchildren," saying: "I can try to lay my life on the line, as you speak of laying your bodies on the line" (1972a, p. 5). She did so in many ways, with clear-sighted willingness, for example to accept whatever the consequences might be of appearing in public clad in her familiar red cape, carrying her famous black lacquered, shoulder-high forked staff or "thumb stick"; or of testifying before a U.S. Senate committee that, in her opinion, legalizing marijuana would do something toward correcting the dangerous amount of drug use.

This is not to portray her as the vigorous nun-become-saint that she thought she might like to be when she was ten years old. Nor is it to deny that she thoroughly enjoyed being Margaret

Mead, including the public figure and "personality" aspects of her role, and the ways in which she stage-managed them. But perhaps the best indicator of the extent to which she gave of her "talents," and of her "generosity" (one of her frequently articulated values) is the Institute for Intercultural Studies, a nonprofit organization that she established in 1944, and of which she was secretary (Dillon 1979). The main purposes of the institute as expressed in its certificate of incorporation are to "stimulate . . . research . . . dealing with the behavior, customs, psychology, and social organization of . . . various peoples and nations . . . [emphasizing research which is] most likely to affect intercultural and international relations"; and to offer fellowships and other aid, including publicity and publication, for such research. The major source of the institute's funds were Mead's own earnings, not only as curator of the American Museum of Natural History and adjunct professor at Columbia University, but also from her royalties, lectures, newspaper and magazine articles, and television appearances. The greater part of her income was donated to this institute and redistributed to persons and groups engaged in the types of activities for which it was founded. Mead was especially interested in giving small grants to young persons (particularly young women) and to other newcomers in the early stages of their behavioral science careers, and in promoting the kinds of research for which there are "few funds and few supporters" available. A good deal of her own research was also supported by the institute. Although in energy as well as in monies it amounted to far more than one-tenth of her resources, what Mead donated of herself in this way is perhaps best thought of as a tithe—a religious offering that grew out of her sense of calling, and out of the old American, Protestant Christian tradition in which she was raised.

Mead was a devout, practicing Episcopalian, who (in the words of the Reverend William F. Starr, Episcopal chaplain of Columbia University) "brought to her faith the same incisive, direct, probing intellect that animates her scientific work. She had the keenest possible sense of our present peril, and of humanness, not as a given, but as a task." She was not only an active laywoman in the Episcopal Church, but also in many ecumenical committees, conferences, and activities of both the National Council and the World Council of Churches.

Mead wrote repeatedly about the significant

and lifelong extent to which she felt her family influenced her attitudes and values, beliefs and ideas:

All my forebears came to this country before the Revolutionary War, and seven of the eight lines can be traced to ancestors who fought in the Revolution; the eighth were Tories from upper New York State who migrated to Canada. . . . I knew that we were of mixed English, Scotch, Irish, and Welsh stock, and was told that my ancestors had all suffered in the cause of religious liberty, as pilgrims, puritans, and covenanters who had had to hide to escape persecution. . . . (Mead 1974a, p. 299)

Mead's mother (Emily Fogg Mead) and father (Edward Sherwood Mead), as well as her paternal grandmother (Martha Adaline Ramsay Mead), who came to live with her parents when they were married, are especially well known to readers because she has written so fully and movingly about them. Mead was born December 17, 1901, in Philadelphia, the first of five children and, in her perception, a thoroughly wanted and loved child. During her childhood, the family lived in a series of places in and around Philadelphia, and in Holicong in the Buckingham valley of Bucks County, Pennsylvania. Her father, an economist, was a professor of finance at the Wharton School of the University of Pennsylvania. Her mother, a graduate of Wellesley College and a teacher, had met and married him when they were both graduate students at the University of Pennsylvania. The couple moved to Hammonton, New Jersey, so that Emily Mead could study Italian immigrants for her dissertation in sociology. She was a devoted wife and mother, an early feminist, a suffragist, and she was always actively engaged in numerous social causes and organized community efforts "to make this a better world." Because Mead's parents—"educational enthusiasts"—disapproved of schools that kept children indoors and chained to their desks all day, until her high school years she was taught, principally at home, by her grandmother, also a college graduate and a certified teacher.

The "pattern my family made for me," as Mead has described and analyzed it, "was related comparatively to the past, vigorously and ethically to the present, and hopefully to the future. . . ." From her father, mother, and grandmother, and the sense of heritage that they conveyed to her, came "enjoyment of the intellect"; an "enormous respect for facts"; "the academic ethos"; an interest in "real processes" and in "real children"; the association of obser-

vation with "an act of love"; trustworthiness and "strength of conscience"; egalitarian principles; a "sense of responsibility for society" and for justice; austerity; domesticity; generosity and compassion; affection and love. She also believed that she inherited her almost photographic memory from them. In all these ways, Mead felt that her work was significantly influenced by the three people who raised her.

In addition, an extensive web of family relations—including even mother's sisters and father's cousins—remained important to her throughout her life. She was close to her siblings: Richard, an economist, transportation specialist, and professor at the University of Southern California; Elizabeth, a professor of art at Lesley College and wife of William Steig, artist and cartoonist; and Priscilla, wife of Leo Rosten, political scientist and writer, who studied social work in the last years of her life. Another sister, Katherine, who died at the age of nine months, was always a living presence for Mead.

In adult life, kinship for Mead included three successive husbands, all of them anthropologists. Her first marriage, in 1923, was to Luther Cressman, who was studying for the ministry at General Theological Seminary at the time that Mead began her graduate work in anthropology. On her way home from her first field trip to Samoa, she met Reo Fortune. They were married in 1928, on shipboard, en route to the Admiralty Islands in New Guinea to study the Manus. Five years later, when she and Fortune were studying the Tchambuli in New Guinea, she met Gregory Bateson, then working among the Iatmul. They were married in 1935. Their daughter, Mead's only child, was born in 1939. Mead had always planned to have many children—six, she had thought—when she first married. But because of a tipped uterus, it was only possible for her to have one, Mary Catherine Bateson, named after "the most distinguished [of Bateson's] maternal aunts, a pioneer historian, who died young," and Mead's "lost little sister" (1972a, p. 257). For Mead, her daughter's birth—"after many years of experience as a student of child development and childbirth in remote villages"—represented the miraculous arrival of a "new person," and a "completely happy" and fulfilling experience as a woman, for which she had hoped all her life (1972a, pp. 249, 258–259).

Mead and Bateson's collaborative research in Bali (1936–1939) produced *Balinese Character*, one of Mead's most original and generative works, both in terms of its impact on anthropological

methods, and on Mead's own future theoretical perspectives. With her divorce from Bateson in 1945, her "years as a collaborating wife, trying to combine intensive field work with an intense personal life . . . came to an end" (1972a, p. 27).

Mary Catherine Bateson became a linguist and anthropologist, specializing in Middle Eastern languages and culture, and married an Armenian engineer, Barkev Kassarian. For Mead, the birth of her granddaughter Sevanne Margaret Kassarian in 1970 "rounded out [her] understanding of something for which [she had] pleaded all her life—that everyone needs to have access to both grandparents and grandchildren in order to be a full human being" (1972a, p. 282).

But it was not only relatives with whom Mead maintained close ties over time and space. She acknowledged that her entwined personal and professional life "depended to an enormous degree" on continuing contact with a wide range of persons, from many social backgrounds, age groups, and occupations, in different parts of the world, and also on "face-to-face close work with collaborators." At the 1976 meeting of the American Association for the Advancement of Science (of which Mead was president that year), Wilton S. Dillon, an anthropologist and director of symposia and seminars at the Smithsonian Institution, referred to the vast and varied Mead circle as a big "kula ring," through which, in an ever-widening, gift-exchange kind of way, human and intellectual services and ideas are continually given and received. Mead had a concept of "kinship by choice," and her "ring" included kin-like friend-colleagues, such as Benedict, Gorner, Jane Belo, Marie E. Eichelberger, and Edward Sapir; Lawrence K. and Mary Frank and their children, with whom Mead and her daughter shared a joint household during most of Mary Catherine Bateson's childhood; and Rhoda Metraux, her frequent collaborator and coauthor. The circle also encompassed persons from numerous other disciplines, such as writer James Baldwin, philosopher of science Jacob Bronowski, geneticist Theodosius Dobzhansky (Mead et al. 1968), inventor and philosopher R. Buckminster Fuller, and photographer Ken Heyman (Mead & Heyman 1965; 1975); several generations of younger anthropologists who were once her students, like Theodore Schwartz, Barbara Honeyman Roll, and Lenora Shargo Foerstal; former members of her staff of young assistants at the American Museum of Natural History; nonanthropologist social scientists in whose

work Mead took a long-term interest; informants and respondents from among the peoples she studied; and an "amazingly diverse group" of other persons to whom her professional activities and "her celebrity status" were relatively unknown (Dillon 1974, p. 492).

Although Mead stated that "moving and staying at home, traveling and arriving [were] all of a piece" for her (1972a, p. 9), there were places as well as people in her life that had special significance for her: childhood homes in Hammon-ton and Holicong; Barnard College, from which she graduated in 1923, where she not only found the kind of student life that "matched [her] dreams," but also her vocation as an anthropologist; Columbia University, where she received her graduate training in anthropology and became an adjunct professor in 1954; Samoa, Manus, and Bali. But it was her "attic office" in the American Museum of Natural History in New York City that was her professional home. Here she lived her work when she was not in the field, did most of her writing, and stored all of her field materials.

One day about midway in my field trip [to Samoa in 1925–1926], a cable came offering me the Museum job. I remember that there was no one to understand what it meant and no way to celebrate. I just walked along the beach, alone, thinking what it would be like. . . . (1965, p. 116)

Then, in 1926, after my return from Samoa, I came to the . . . Museum, . . . where I was given as my office an attic room under the eaves up in the west tower of the old Seventy-Seventh Street building. . . . Perhaps it was because I was given that little attic room with a view over the city roofs that I decided within a few months that I was going to stay at the Museum all my life. . . . (1972a, pp. 14–15)

In 1969, Mead became curator emeritus of ethnology at the museum. In 1971, her 45 years of work there culminated in the opening of the Hall of the Peoples of the Pacific, an exhibit that she largely planned and assembled. In 1976, on the occasion of her 50th year at the museum and her 75th birthday, a Margaret Mead Fund for the Advancement of Anthropology was established to endow a newly-created Margaret Mead chair in anthropology, to relocate, expand, and maintain the Hall of the Peoples of the Pacific, to provide for training younger anthropologists, and to preserve irreplaceable museum collections.

Two years later, on November 15, 1978, one

month before her 77th birthday, on the first day of the annual meeting of the American Anthropological Association, and on the day that the *World Almanac* named her one of the world's 25 most influential women, Margaret Mead died of cancer of the pancreas. Her illness had lasted a year. As she had requested, she had been told "the full truth about [her] physical condition without any protective evasions or optimisms whatsoever," so that she could "use . . . a limited and definite period before death . . . responsibly and constructively" (Modell 1974, pp. 907–908). On August 7, 1978, she had renewed and renotarized the "Living Will: On Medical Intervention" document that she had originally drafted in 1962. Characteristically, Mead's "statement of intent in case I am incapacitated from making such a decision at the time that a decision is called for," both antedated and anticipated the current "right to die" and "death with dignity" issues that have become so acute and pervasive in American society during the 1970s. Her statement not only included a "recital of choices" to guide the physicians who might care for her, but also some general, cultural insights into what she identified as the "crisis . . . we have reached . . . because medical advances have outdistanced our expected forms of ethical behavior" (Modell 1974, pp. 907–908). Several years earlier, when her health was still intact, Mead's Living Will had been published, "without identification," in the *New England Journal of Medicine* by Walter Modell (*ibid.*, pp. 907–908), to whom (in a letter dated January 30, 1974), she had given permission to do so. "I would be happy to have that piece published, with or without my name," she had written to him, "whichever you thought would have the most effect. I believe that death with dignity is a fairly pressing agenda, and anything that can be done to help it along should be done."

Until she entered the hospital, Mead continued to hope that she could return to Manus in December 1978, for a planned, seventh visit. Although this became impossible, she remained a field-working anthropologist to the end: a participant observer of her own final illness, and of the hospital where she spent the last six weeks of her life; a probingly interested interviewer of the nurses and physicians who cared for her; and the voluntary client and collaborator—subject of a faith healer, whom she regarded as a modern version of the many "shamans" that she

had known and studied in more primitive cultures.

Margaret Mead was buried privately and religiously in the graveyard of what she had described in *Blackberry Winter* as "the little Episcopal church" in Buckingham, Pennsylvania, where, as a young woman, "I had been baptized of my own choice with godmothers summoned by myself" (1972*a*, p. 116). But her death was experienced and reported as a major public, American, international, and symbolic event. It was covered by virtually every local and national radio and television station in the United States, and by numerous European ones as well, and it was announced on the morning news of Papua New Guinea National Broadcasting Company. The news of her death and the story of her life and work appeared on the front pages of American, British, and French papers, and feature essays and editorials about her were printed in popular magazines such as *Time* and *Newsweek*, and in scientific and professional journals like *Science* and *American Anthropologist*. A bulletin about her death arrived in Peking one day after it had occurred, where it was relayed to the American Association for the Advancement of Science's board of directors who were visiting the People's Republic of China.

Memorial services and programs were conducted for Mead at St. Paul's Chapel at Columbia University; the Washington (D.C.) Cathedral (with assistance from New Directions, the national citizens' lobby on global issues of which she was founding chairperson); at the University of Cincinnati College of Medicine (where she had been a visiting professor every year since 1957); in the "combined" anthropology department of Columbia University, Teacher's College, and Barnard College; at the annual meetings of the American Anthropological Association, the Society for Applied Anthropology, the American Association for the Advancement of Science, the American Orthopsychiatric Association; and at the American Museum of Natural History.

When the people of Pere village in Manus learned of the death of "Margarit Mit," they closed their school and remained in their houses for 24 hours. Then came five days of mourning ceremonies, reserved for the passing of a great chief. The official mourners for these ceremonies, 26 women who had known Mead well, went to the house that had been built for her in 1965, where they chanted dirges, some of

which were especially composed to recount the history of "Margarit's" visits to Pere and her career in the outside world. On November 22, Francis Paliau, the councilor (mayor) of the village sent the following cable to Theodore Schwartz and Barbara Roll (anthropologists) and Frederick Roll (photographer) who worked closely with Mead in Manus: "People sorry of Margaret Mead's death. With sympathy, respect. Rested seven days. Planted coconut tree memory of great friend" (Roll 1979).

A Margaret Mead memorial Community Center was established in Manus. At the Museum of Natural History in New York, the Hall of the Peoples of the Pacific was renamed the Margaret Mead Hall. Many letters arrived in Mead's office from various of the voluntary associations, scientific organizations, and schools for which she had spoken, requesting permission to arrange a Margaret Mead Day, create a Margaret Mead lecture series, establish a Margaret Mead award, start a Margaret Mead memorial fund, or to name their institution after her. On January 6, 1979, she posthumously received the Philip A. Hart award from the Urban Environment Conference, "for fostering alliances between labor, minority and environmental interests." And on January 20, 1979, Mead was posthumously awarded the presidential medal of freedom, the nation's highest civilian honor, which was presented to her daughter by Andrew Young, United States Ambassador to the United Nations, on behalf of President Jimmy Carter, in a ceremony at the museum.

Less than a month after her death, a volume that Mead had planned as a Christmas remembrance, and coauthored with Rhoda Mettraux, was published as a book. *An Interview With Santa Claus* ([1977] 1978), dedicated and addressed to Kate (her goddaughter and Mettraux's granddaughter), contains Mead's final, allegorical statement about some of the basic moral convictions and transcendent spiritual beliefs that animated her life and her work:

Why shouldn't there be a whole clan of gift-giving figures? Don't they all, in some way, convey a special message to children? . . . All of them have appeared on some day close to the shortest day of the year, when the very sharpness of a cold, barren winter gives promise of spring, so that long ago, in the midst of winter, human hearts rejoiced and were moved to generosity and gaiety, especially for children.

Santa Claus is but the newest of these gift-givers.

From him, as from the others, children can learn that giving as well as receiving is joyous, and that the gifts that seem to be freely given by wonderful, benign visitors are tokens of the happy care given by mothers and fathers.

Now it is enough for Kate, and all small children, to learn the legends of Santa Claus. Later, when legend and reality meet in a new way, she will begin to understand, we think, that giving is itself a kind of thank offering. (pp. 43-48)

RENÉE C. FOX

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MEADE, JAMES E.

James Meade shared the Nobel Prize in economic science in 1977 for his work on international trade and capital movements, but his contributions covered a wider field. As a professional economist he has written on macroeconomics, national income accounting, economic planning, welfare economics, public finance, growth theory, population policy, and the distribution of income and wealth.

Meade was born on June 23, 1907, went to school at Malvern College, and then attended Oriel College, Oxford (M.A. Oxon). After graduating, he spent a formative and creative year in Cambridge, one of the youngest of the "circus" around John Maynard Keynes who were thinking through the central issues of what was to become *The General Theory of Employment, Interest and Money* (Keynes 1936). His contributions in this crucial period are acknowledged in Richard Kahn's pioneering article on the multiplier (1931). Meade returned to Oxford as a fellow of Hertford College (1930-1937). During this period he synthesized with great clarity the new ideas in macroeconomics and in imperfect competition in *An Introduction to Economic Analysis and Policy* (1936), the first systematic textbook to embody the essential Keynesian framework.

The subsequent ten years spent outside of academic life at the League of Nations (1937-1940) and in the Economic Section of the Cabinet Office (where he was director in 1946/1947) had a profound influence on his writing. The experiences of international economic problems and of postwar reconstruction were probably an important stimulus to *The Theory of International Economic Policy* (1951-1955),

discussed below. Brought into the Cabinet Office on Keynes's recommendation, he worked with Richard Stone on the early national income accounts, an endeavor that led to their *National Income and Expenditure* (1944). During this period, Meade was concerned with planning and public enterprise policy, interests that were reflected in several articles and in *Planning and the Price Mechanism* (1948).

The single most important period in Meade's career was the decade spent as professor of commerce at the London School of Economics (1947-1957), where he made his most lasting contributions to international economics and welfare economics. The two volumes of *The Theory of International Economic Policy—The Balance of Payments* (1951) and *Trade and Welfare* (1955)—are landmarks in economic theory. This fruitful period also yielded a number of subsidiary books, of which the most influential has been *The Theory of Customs Unions* (1955b).

The two new features of *The Balance of Payments* were its integration of income and price effects in a general equilibrium framework, and its policy orientation. In both respects there was a departure from previous theory, which had tended to keep income and price mechanisms in watertight compartments and had dealt mainly with automatic processes of balance-of-payments adjustment. Keynesian income analysis had previously been separated from the analysis of exchange rate changes, with the latter based on the assumption of constant money incomes, and analyzed in terms of Marshallian partial equilibrium.

The core of *The Balance of Payments* is a policy model. The policy objectives are taken to be internal and external balance. The available types of policy are income adjustments, which operate through fiscal and monetary policies, and price adjustments through exchange rate variations or wage flexibility. If two objectives of policy are to be achieved simultaneously, two policy variables are required. Here Meade was applying the principle, developed independently by Jan Tinbergen, that the number of instruments must equal the number of targets. Meade showed that if only one policy variable was used, conflicts between objectives could arise, yielding what have later been called "dilemma cases."

Meade's model has become part of the luggage of every economist. It represents the first systematic exploration of the relationship be-

tween domestic and international equilibrium. There are a number of other significant contributions in this book. Balance-of-payments theory at the time had tended to ignore capital movements; Meade integrated these into his model. The book contains the seeds of what has come to be known as the "theory of optimum currency areas" and an original analysis of speculative capital movements. A generally neglected chapter introduces home-trade (non-traded) goods into the analysis of devaluation; it was the precursor of an extensive literature analyzing balance-of-payments policy in terms of traded and nontraded goods.

A key contribution of *The Balance of Payments* was to explain that, starting in internal balance, a devaluation would be inflationary unless it was accompanied by appropriate expenditure reduction. In considering various relative price policies—notably devaluation—designed to improve the balance of payments, Meade generally assumed that an internal balance policy was being simultaneously pursued by appropriate adjustments of financial policies. He did not discuss to what extent a devaluation would bring about automatic expenditure reduction or how the balance of payments would be affected if internal balance were not simultaneously maintained. This gap in his analysis was filled in the 1950s by the "absorption approach" to balance-of-payments theory. In addition, he did not succeed fully in integrating real and monetary theory. He assumed a flexible money supply policy designed to maintain a given rate of interest, monetary policy changes being expressed in terms of interest-rate changes. In the monetary approach to balance-of-payments theory subsequently developed, either the money supply or domestic credit creation is held constant, the interest rate being endogenous. Exchange-rate changes affect the balance of payments through the real balance effect. In this and other respects, balance-of-payments theory has advanced since *The Balance of Payments*. But subsequent contributors, notably Robert Mundell and Harry Johnson, have built upon Meade's work.

In *Trade and Welfare*, Meade presented a systematic analysis of arguments for trade and factor controls. The analysis of factor controls and factor movements is highly original. It includes a model of the effects of migration on the terms of trade and a contribution to optimum population theory. But the whole concept of embracing trade and factor controls in one methodology was novel.

Trade and Welfare has contributed to much more than international economics, representing an innovative development of welfare economics. In particular, the "theory of second best" shows that arguments for trade or factor controls can be made on the basis that there are various constraints, whether originating in government policies (for example, a tax) or in a feature of the private market economy (an entrenched monopoly) that must be taken as given. Optimizing trade policies subject to these constraints can yield a different policy result from that in a first-best world.

The theory of the second best was given a rather nihilistic interpretation, mostly under the influence of a celebrated article by Richard G. Lipsey and Kelvin Lancaster (1956) that derived from *Trade and Welfare*. It was suggested that nothing in general could be said about the nature of optimal policies because of the existence of "distortions." But in *Trade and Welfare* itself the emphasis is characteristically much more positive: the optimal second-best tax or tariff would usually partially offset some other given distortion, even while creating, at the margin, new distortions. Meade used the principle of the second best to analyze the effects of domestic divergences from the first best, initiating the "theory of domestic distortions." He applied the methodology to the welfare economics of customs unions, where he also made significant contributions to the positive theory, *The Theory of Customs Unions* being the first systematic analysis of this subject based on the pioneering work by Jacob Viner (1950).

Meade's constructive approach was also reflected in his rejection of the "new welfare economics." Even though he had drafted a greater part of *Trade and Welfare* in terms of potential welfare and the compensation principle, the very negative conclusions they yielded led him to revert to the older tradition, which allowed for interpersonal comparisons as expressed in explicit distributional weights. The attraction of this common-sense approach was that it provided a methodology sufficiently general to allow different weighting systems to be applied. In addition, Meade introduced a general method for measuring small changes in welfare, which was a variation on Marshallian consumers' surplus, with its attendant limitations.

These developments built on earlier literature; but the three aspects of his welfare methodology taken together—second best, distributional weights, and the measurement of small

changes—constitute fundamental contributions to a number of modern subjects. They include cost-benefit analysis, even though this form of applied welfare economics actually developed independently, Meade's methodology being re-discovered in more operational contexts. The same pattern applies to the economics of taxation. The "Mathematical Supplement" to *Trade and Welfare* contains valuable analyses of the efficiency effects of direct and indirect taxation and of the general equilibrium incidence of taxation. They foreshadow later literature on optimal taxation and tax incidence.

When Meade's two great volumes and the various associated books are examined as a whole, it can be said that the policy model of *The Balance of Payments* was very influential and that the work had a fairly rapid impact on policy-makers and key writers in the field. Similarly, the *Theory of Customs Unions* was widely noted, possibly because of the topicality of its subject. By contrast, the influence of *Trade and Welfare* was more delayed, and to a great extent many of its original ideas were rediscovered independently later. Its principal impact was probably through the theory of second best. Both books, but especially *Trade and Welfare*, are written in a taxonomic and rather heavy style, with no footnote references to the literature and a failure to highlight the author's original contributions. Although the books are immensely rewarding to serious students, their messages often reach a wider audience only through the intermediation of more succinct, if less original, writers.

Meade initially half-promised a "Theory of Domestic Economic Policy." This has not materialized as a full-length work, but much of his writing has been concerned with this subject, especially since 1957, when he went to Cambridge as professor of political economy. Two themes dominate much of his work. The first is the importance, in the allocation of resources, of the market mechanism, coupled with an active government ensuring effective competition. This theme was set out clearly in *Planning and the Price Mechanism* (1948), and Meade returned to it again nearly thirty years later in *The Intelligent Radical's Guide to Economic Policy* (1975). Both books have had wide influence in Britain, particularly on debates about economic planning. Meade's belief in market forces, however, is tempered with a concern for the redistribution of income—the second main theme of his writings on domestic policy. The

two books referred to contain extensive discussions of schemes to reduce inequality. At a more theoretical level, he has contributed significantly to the development of models of income distribution. *Efficiency, Equality and the Ownership of Property* (1964a) provides a very suggestive account of the forces underlying the accumulation of capital and the relationship between earned and unearned incomes. This essay stimulated much of the revival of interest in this subject, at least in the United Kingdom; and Meade has himself developed the model in *The Inheritance of Inequalities* (1974a) and *The Just Economy* (1965–1976, vol. 4).

Finally, although Meade never returned to full-time government service, he did not remain totally removed from issues of policy. He chaired an influential committee of enquiry into *The Economic and Social Structure of Mauritius* (Meade et al. 1961) and in 1975–1977 a committee on *The Structure and Reform of Direct Taxation* (Meade et al. 1978). Both reports, while the joint products of committees, bear the unmistakable stamp of his high intellectual standards and distinctive literary–arithmetical style.

Meade's attitude to economics is clearly stated in the preface to *The Balance of Payments*: "My interest in economics has always been in considering the contribution which pure economic analysis can make to the formation of economic policy. . . . This volume is the work neither of a tool-maker nor of a tool-user, but of a tool-setter" (p. vii). This approach stems from a deep-felt concern for social reform and from liberal-radical values close to those of the "reasonable citizen" for whom he wrote *The Intelligent Radical's Guide to Economic Policy*. This belief in the role of reason, or at least in the possibility that persons of power or influence can be influenced by cogent arguments, places him very much in the English tradition. It also explains his style of writing and why he embarked on the *Principles of Political Economy* (1965–1976), of which four volumes have appeared to date. He has seen his role as "the modern equivalent of the old Political Economist, namely the worker who, in the interests of those whose task it is to apply economic theory in policy decisions, specialises in generalisation" (preface to vol. 1, p. 8). He has written books to sort out systematically his own ideas on subject after subject, rather like his great Cambridge predecessor, Arthur C. Pigou.

Meade's independent approach has meant

that he effectively opted out of the mainstream of many of the relevant scholarly debates. The failure in his writing to show how his work fits into the development of the subject has reduced his influence, especially in North America. His literary style, while clear and rigorous, tends to conceal the great originality of his contributions. From his writings the reader, in fact, obtains a remarkably coherent vision of the principles underlying the economic system and of the ways in which, within the confines of a mixed economy, economic policy might be better directed. He has thought through problems from their basic principles in the context of abstract models without losing sight of their application to particular issues. His work is dominated by an open-minded search for truth and the highest standards of intellectual honesty.

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ternational Law and Organization*.

MEANS, GARDINER C.

Gardiner Coit Means, born in 1896, is responsible for introducing into social science literature two concepts that are critical for understanding the role of large corporations in the American and world economies. In his classic *The Modern Corporation and Private Property* (Berle & Means 1932), Means showed how the dispersion of stockholdings in the two hundred largest nonfinancial corporations had led to a separation of ownership and control. Two years later, in a memorandum written for the secretary of agriculture, he pointed to the widespread existence, within the industrial sector, of "inflexible administered prices" which rendered the market largely ineffective as the economy's self-correcting mechanism and destroyed the underlying basis for a laissez-faire policy. These two findings—the increasing separation of control from ownership and the pervasiveness of administered prices—have since served as the starting point for those who, mainly outside the major graduate departments of economics, have wished to understand the social consequences of what Means himself has termed "the corporate revolution." Relative to the influence which he has had, both on public policy and on the social sciences generally, Gardiner Means is probably America's least honored economist.

Means, the son of a Congregational minister, was born in Windham, Connecticut, in 1896 and grew up in Winchester, Massachusetts, and Madison, Maine, where he attended public school. After a year at Exeter Academy he entered Harvard University at the age of 18 and majored in chemistry. In his junior year, the United States declared war on Germany and, like many of his classmates, Means enlisted in the military. He was assigned to an infantry officers' training camp in Plattsburg, New York, where he first met Adolf Berle, Jr. After receiving his

commission, he transferred to the Air Corps and completed flight training, but not in time to serve overseas. Still seeking experience, he joined the Armenian relief program in Turkey, where he spent a year and a half working in that country's interior.

When he returned to the United States in 1920, Means studied at the Lowell (Massachusetts) Textile School and, two years later, started a textile business in that city. Then, while continuing to run his business, Means moved to Cambridge and entered the Harvard Graduate School, prompted by the desire to understand the workings of the United States economy in more general terms. He soon realized that the economics he was being taught was more relevant to a preindustrial country like Turkey than it was to the United States. Conditioned by his scientific training, he sought evidence to test the applicability of the theories he was being taught to the American economy.

The opportunity to do this arose in 1927 when, after receiving his master's degree in economics, Berle, then at the Columbia University Law School, asked him to join in an interdisciplinary research project, funded by the Social Science Research Council, to study the legal and economic implications of the growing separation between ownership and management in large corporations which Berle, as a corporation lawyer, had observed. During the next four years, Means gathered the empirical evidence necessary to demonstrate not only the increasing separation of ownership and management but also the key role played in the economy by the two hundred largest nonfinancial corporations, where this process of separation was most advanced. The results of this collaborative effort were first published under the title *The Modern Corporation and Private Property*.

The significance of the study for public policy was immediately recognized, as it helped lay the groundwork for the Securities and Exchange Act of 1934. A parallel study, *The Holding Company: Its Public Significance and Its Regulation* (Bonbright & Means 1932), played a similar role in advancing passage of the Holding Company Act of 1935. The implications for economic theory, however, were ignored or resisted in many academic circles.

Prior to the publication of *The Modern Corporation and Private Property*, Means had reported his findings in three articles published in scholarly journals. The Harvard economics department suggested that he combine these arti-

cles with an interpretation of their significance and submit them as his doctoral dissertation.

In the theoretical section, Means argued that the empirical evidence did not support four major postulates of received economic theory, namely, the trading character of the market, the determinacy of cost, the unity of the process of real saving, and the efficacy of the profit motive; and that these postulates needed to be replaced, insofar as corporate enterprise was concerned, with an alternative set of formulations. Although Means received his PH.D. from Harvard based on the three published articles, the theoretical section, and thus the dissertation itself, was rejected.

In the summer of 1933, soon after receiving his doctorate, Rexford G. Tugwell recruited Means to serve in the Roosevelt administration as an economic adviser on the relations between agriculture and other sectors of the economy to the secretary of agriculture, Henry A. Wallace. In this capacity, Means prepared a memorandum for the secretary pointing out that "inflexible administered prices" had "produced highly disrupting effects in the functioning of the economy" and were largely responsible for the failure of the economy to recover on its own from the depression. Means produced a set of statistical charts showing that "there are two essentially different types of markets in operation—the traditional market in which supply and demand are equated by a flexible price and the administered market in which production and demand are equated at an inflexible administered price." The charts revealed that, where flexible market prices prevailed, production fell only slightly in response to the decline in overall demand as that which had occurred between 1929 and 1933; but that in those industries characterized by inflexible administered prices, the primary response to such a decline in demand was a reduction in production and employment. Means argued that this lack of responsiveness of prices in the administered markets made a laissez-faire policy no longer tenable in the American economy.

To other economists brought to Washington by the New Deal, the memorandum written by Means and then distributed among high level administration officials seemed to offer not only an explanation for the continuing economic crisis but also evidence in support of that explanation. Senator William E. Borah, learning of the memorandum and erroneously suspecting that it was being suppressed, pushed through a

Senate resolution requiring that it be made public; it was published in 1935 as a Senate document, entitled "Industrial Prices and Their Relative Inflexibility." Borah and other proponents of a more vigorous antitrust policy seized upon Means's findings as support for their position, and the memorandum played a key role in the subsequent launching of the Temporary National Economic Committee (TNEC) hearings in 1938—the most comprehensive congressional inquiry into the concentration of economic power since the Industrial Commission's hearings at the turn of the century. The inquiry confirmed Means's factual evidence but interpreted it in terms of the traditional paradigm with its bias toward antitrust remedies.

For Means himself, however, antitrust was not the answer to the problems revealed by his findings. The productivity of modern industrial organization would be lost, he felt, by the effort to break up the largest corporations and restore classically competitive markets in the industrial sector. He therefore passed up the opportunity to serve as economic coordinator of the TNEC and remained instead in the position he had held since 1935 with the National Resources Planning Board (later renamed the National Resources Committee).

As director of the industrial section of that agency, and with the aid of an advisory committee of leading government economists, Means carried forward the research that would lay the foundation for what was then known as "democratic planning." In *Patterns of Resource Use* (1939b), he developed the first statistical techniques for projecting, at varying levels of total demand, the interrelated patterns of production and employment. In *The Structure of the American Economy* (1939a), he provided the most detailed statistical analysis of the American economy's organizational structure then available. However, World War II intervened before he could complete his projected companion study of the economy's operating characteristics.

Means then moved to the newly established fiscal division of the Bureau of the Budget as a chief fiscal analyst. There he persuaded the price control administration to delay freezing prices until flexible and administered prices reached a full-employment balance and price controls could be more easily maintained. He also used the techniques described in *Patterns of Resource Use* to estimate the capacity of the American economy to engage in total war. During the war, these same techniques were ex-

tensively employed by the War Production Board to estimate civilian demand and thus to determine what wartime restraints were needed to control nonmilitary consumption.

Wartime full employment and the problems of a war economy temporarily turned the attention of economists away from the questions of cyclical stability. However, as the prospect of peace emerged, a sharp disagreement arose between those who predicted a severe postwar depression and those who feared a postwar inflation. Many government economists, together with an increasing number of Keynesians, expected at least a slump in the economy, possibly a return to depression levels. Means, on the other hand, had earlier pointed to the importance of monetary policy as a stabilizing influence (1935), and he now argued that the doubling of the money stock during the war would lead to a major inflation when price controls were removed, a prediction subsequently confirmed by events.

As a result of this disagreement, Means left the Bureau of the Budget and became associate director of research for the Committee for Economic Development (CED), a business-sponsored, private research group originally concerned with government policies to assure a full-employment transition to a peacetime economy. While at the CED, Means began the collection of the statistical series on money flows now regularly published by the Federal Reserve Board in its *Flow of Funds Accounts*, an important supplement to the more widely used *National Income and Product Accounts*.

In 1957, Senator Estes Kefauver, chairman of the subcommittee on antitrust and monopoly, undertook a series of hearings on "administered price," which, after receiving testimony from Means and other economists, led to a detailed examination of the steel, auto, drug, and other industries. The hearings, under the direction of John Blair, a former student of Means (when Means taught at the American University's graduate school) provided an intimate portrait of industrial pricing policies. Together with studies carried out simultaneously for the Joint Economic Committee, they played a role in convincing a minority group of economists that inflation might be produced by the exercise of market power as well as by excess demand. This unorthodox viewpoint, which Means set forth in a pamphlet, *Administrative Inflation and Public Policy* (1959), became the basis for the presidential guideposts announced by President John F.

Kennedy in 1962 and later continued by President Lyndon B. Johnson. Under the guideposts, which worked successfully for three years before collapsing because of a flaw in the labor aspect of their design, large corporations were to hold their prices at existing levels and trade unions were to limit their demands for wage increases to the general productivity trend.

In 1962, two books by Means appeared. *Pricing Power and the Public Interest*, based in part on the Kefauver committee hearings, analyzed the role of the steel industry in the administrative inflation of the 1950s. *The Corporate Revolution in America*, meanwhile, brought together the most important of Means's scattered essays. And yet Means continued to be largely ignored by academic economists, both Keynesians and monetarists. The majority still held to the belief that inflation was caused by excess demand; and the pricing paradigm which was taught to students remained essentially the same as that to which Means himself had been exposed at Harvard in the 1920s, with the focus on classically competitive markets governed by supply and demand factors. The only exception to the general lack of recognition which Means's work has received from his fellow economists in the academic community has been the action of the Society for Evolutionary Economics in presenting him with its Veblen-Commons award in 1974 "in recognition of outstanding contributions in broadening and enriching the discipline of economics."

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MONTAGU, ASHLEY

Ashley Montagu, born in 1905, is one of those rare men of learning who have succeeded in making substantive scholarly contributions to their academic disciplines while at the same time maintaining contact with the educated layman, indeed contributing substantively to the latter's learning. In addition, he is a dedicated and articulate social critic, concerned with bringing to bear the findings of the social and biological sciences toward the betterment of man's lot, while subjecting some of those very findings to critical social scrutiny. His accomplishments in these three domains, the scientific, the public-educational, and the socioethical, will be treated as a unity in what follows, in accordance with what is evidently the spirit of the program that has guided his life's work.

Although Montagu's contributions span a variety of fields in the social and biological sciences—including work on problems as diverse as Australian aborigines' concepts of sexuality and reproduction, the measurement of internal anatomical landmarks on the heads of intact living human beings, adolescent infertility in girls, the role of cooperative behavior in evolution, and the biological and cultural factors in aggression and in sex roles—his principal legacy will indisputably consist of his critical analysis of the concept of race.

The problem of race preoccupied Montagu from the beginning of his intellectual career (Montagu 1925; 1926), more than a quarter century before the 1954 U.S. Supreme Court desegregation decision in *Brown v. Board of Education of Topeka* (347 U.S. 483), which heralded the civil rights activism that has since followed in America. Montagu's work played a role in that Supreme Court decision, as well as in shaping the social consciousness that ushered it in and has attended it ever since. If some of his ideas, as they are discussed below, appear to be relatively uncontroversial and a matter of common knowledge and assent, let it not be forgotten that that very knowledge and assent is in some measure due to the work and efforts

of Montagu, and that he was also forcefully expounding those ideas at an earlier time, when they were far from accepted, and indeed being brutally violated on a scale unparalleled in human history (Montagu 1939; 1941a).

Montagu's papers on race in the late 1930s, culminating in his book *Man's Most Dangerous Myth: The Fallacy of Race* (1942a) and followed by a series of works (including Montagu 1951; 1964; 1975), had the effect of upsetting the traditional concept of "race" accepted by most anthropologists in that it challenged the reality of anything corresponding to that notion. Montagu emphasized that gene-frequency analysis of traits would tell us more about the evolution of human populations, arguing that the "omelet" conception of "racial mixing" was totally artificial and did nothing to explain the origins and consequences of the differences between populations. Since men were all originally gatherer-hunters, wherever they were, the environmental challenges faced by different populations tended to be very similar; hence, one would not expect mental differences. This theory, as set forth in an article coauthored with the geneticist Theodosius Dobzhansky (1947), subsequently became generally accepted by anthropologists. Montagu was also asked to draw up the United Nations Educational, Scientific and Cultural Organization's *Statement on Race* (1951) in 1950.

In addition to his work on race, Montagu was also among the first to present a number of views, since widely accepted, on such familiar social and psychological themes as aggression and war (1946b; 1976), social factors in crime (Montagu & Merton 1940), women's rights (1953b), psychoanalysis and psychiatry in anthropology (1941b), love (1953a), home birth and prenatal care (1950; 1962), Afro-American studies (1944), sociobiology (1940), birth order (1948), privacy (1956a), and even smoking (1942b) and natural foods (1958). In these and other works Montagu was always a strong advocate of gene-environment interactionism (1926; 1940; 1956b; 1959; 1962), stressing that heredity is not biologically "given" in the genes, and that man's constitution is a dynamic process arising out of the interaction between his unique experiential history and the constraints and potential encoded in his genetic material.

This interactionist stance allowed Montagu to be an effective exponent of the often polarized realms of cultural and biological anthropology. He could adduce evidence on behalf of the bio-

social nature of man (1956*b*) while at the same time showing the virtually limitless capacity of education and culture to shape that very nature (1962). His interactionism attempted to reconcile these two poles, not only in terms of the history of the dual influences acting during one man's lifetime, but also those in mankind's evolutionary history. Montagu emphasized social cooperation and love (1953*a*; 1974), as critical selectional factors in evolution—ideas that considerably predated the sociobiological preoccupation with "altruism" (in the new inclusive fitness sense) in the late 1970s.

Other works by Montagu had fewer social repercussions, but still represented important contributions to anthropology. *Coming Into Being Among the Australian Aborigines* (1937) is one of the classic works on this subject and continues to be a useful source, treating such topics as awareness of the facts of maternity and paternity and the significance of ritual sexual mutilation. This was not only a pioneer study which served to stimulate many students and research workers, but its approach systematized a field which, aside from Bronislaw Malinowski's *Sexual Life of Savages* (1929), had been only vaguely and poorly understood previously. In addition, Montagu's work on the adolescent sterility period (1946*a*) solved a perplexing problem encountered by many anthropologists—most notably by Malinowski in his studies on the Trobrianders (1929)—that although adolescent girls engaged in extensive premarital sexual relations, they rarely became pregnant.

Montagu also worked on technical problems in anthropometry. He established certain craniometric reference points on the scalp and devised measuring instruments to determine homologous points on the underlying skull in living subjects (1960). His anatomical work on non-human primates and on fossils culminated in the publication of one of the earliest textbooks of physical anthropology (1945), which continued for a long time to be a widely-used and authoritative work on the subject. Montagu's other texts include reference works on heredity (1959) and anatomy and physiology (Montagu & Steen 1959), an excellent biography of Edward Tyson (1943), and a large variety of elegant and informative books written for the educated layman.

Montagu's doctorate in anthropology was conferred by Columbia University in 1937. His early academic and intellectual background had been as richly varied as his later contributions. After

a long-standing childhood interest in skulls, fossils, and medical matters, fostered by encouragement from the anatomist–anthropologist Arthur Keith, of the Royal College of Surgeons in London, Montagu enrolled at 17 years of age at University College London, for a diplomate in psychology, with a view to transferring to anthropology. Among his professors in psychology were C. E. Spearman and the father of modern statistics and biometrics, Karl Pearson; in anthropology he was taught by Elliot Smith and C. G. Seligman. At that time in Europe the new anthropology was just developing, with the functional school of Malinowski. The earlier sticks/stones/bones approach was being replaced by an analysis of the functional interrelations among the elements of culture. Montagu became Malinowski's first student, and surely bears the latter's imprint (along with an even stronger one, some feel, from his other great teacher, Franz Boas); but he soon diverged in favor of a strong biological orientation, particularly in matters pertaining to psychology. (Montagu was one of the first exponents of Sigmund Freud in anthropology, although he later became a critic of the psychoanalytic approach.)

C. Loring Brace, an anthropologist at the University of Michigan, feels that Montagu "has done more than anyone except Margaret Mead to bring the findings of anthropology to the attention of the public." Weston LaBarre of Duke University describes him as "the most prolific and effective popularizer of humanistic subjects since H. G. Wells." Not all anthropologists take such a favorable view of popularization, however; and this may have adversely affected Montagu's own intradisciplinary popularity; yet more than one of his colleagues have suggested that this negative attitude may well reflect "sour grapes."

Popularization has not been the only factor diminishing Montagu's professional popularity. According to Marcus Goldstein of Tel Aviv University:

The reason for this, in my opinion, has been his forthrightness, his fearless and blunt attack on works and issues that he felt were scientifically wrong, and perhaps more important, were or could be socially harmful. Two examples come to mind. At one of the early meetings of the American Association of Physical Anthropologists, he sharply criticized Prof. E. A. Hooton's work on a typology of U.S. criminals, a virtual return to Lombrosoism. One must remember in this connection that Hooton was the revered teacher of nearly all of the young

physical anthropologists of the day! In a subsequent paper co-authored with Robert Merton ("Crime and the anthropologist," 1940), Hooton's premises and methodology were systematically demonstrated to be invalid. At another meeting of the Association, Montagu proposed a motion to censure the German anthropologists who were patently misusing the discipline to conform with the vicious Nazi ideology. The motion was defeated, yet the following year the very man instrumental for its defeat proposed the same motion, which passed unanimously.

The final arbiter as to the value of popularization will of course have to be history. Whether in the mid to later part of the twentieth century, with its unprecedentedly well-educated and well-informed general population and its pervasive and powerful communications media it was still possible for scientists, particularly social scientists, to pursue their research, particularly on socially sensitive or otherwise significant topics, without simultaneously assuming an advocate's, or at least an exegete's role vis-à-vis the educated populace, is an empirical question that only the actual turn of events can answer. In any case, it is clear that Ashley Montagu cast his lot with the new dual role of the social scientist, and fulfilled both aspects of it admirably.

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MORENO, JACOB L.

Jacob Levy Moreno (1889-1974) had a revolutionary goal to change society by bringing together individuals who are capable of harmonious interpersonal relationships and so to create social groups that can function with maximum efficiency and with minimum disruptive processes (1934, p. xii).

The concept of "role" was central to Moreno's theory and he played many himself. He was a psychiatrist, dramatist, theologian, poet, philosopher, inventor, group psychologist, psycho-

dramatist, sociodramatist, sociometrist, sociatrist, and educator. Although he made significant contributions in all of these roles, the three areas in which he was most creative, and that have had a major impact on the theory and practice of social science and group psychotherapy, are psychodrama, sociodrama, and sociometry.

Background. Moreno was born in Bucharest, Romania, on May 20, 1889, the son of Nissim and Pauline (Wolf). He went to school in Vienna, where his family moved when he was five, until his early teens, when his parents migrated to Germany. Moreno was unable to adjust to German culture and schooling, and he moved back to Vienna, earning his keep as a tutor.

Early professional years in Vienna (1908–1925). During the first part of this period, Moreno was a student at the University of Vienna, first in philosophy and then in medicine, receiving his medical degree in 1917. From 1915 to 1917 Moreno held the posts of superintendent and medical officer at a refugee camp in Mitterndorf, where he made his first observations of the importance of interpersonal bonds for a viable community. Then from 1918 through 1925 he served as public health officer in Vosslau, Austria, and engaged in private psychiatric practice in Vosslau and Vienna.

All of his papers during this period were published in German. The major themes can be seen in *Einladung zu einer Begegnung* (1914) and *Das Stegreiftheater* (1923). As editor and publisher of the magazine *Daimon*, he brought to public attention many new writers, one of whom was Martin Buber. Moreno felt that he was creating a new positive religion that would take up the task of building a creative society where Karl Marx and Sigmund Freud had left off. He tried to do through sociometry what "religion without science" had failed to accomplish in the past and what "science without religion" had failed to accomplish in Soviet Russia.

Psychodrama. In 1911, while he was a medical student, Moreno began to develop his psychodramatic insights as he watched children play in the gardens of Vienna. He became a special kind of teacher or catalyst as a teller of tales. Children would gather around him to listen and would spontaneously act out the themes of the stories. As the interaction progressed, hostility decreased and creativity flowered.

In 1921, Moreno founded a "Theatre of Spontaneity" which reflected in part the contemporary interest in experimental theater in Vienna. He would appear on the stage and warm his audience up to a theme by discussing current items of news. First actors, then members of the audience would enact these events, and a postenactment discussion would follow. He discovered that unlike conventional theater, where the actors do not have their own personal catharsis through the action, these actors of real-life situations experienced a relief from personal pressures or pain during both the enactment and the ensuing discussion.

As Moreno went on to develop the "classical" form of psychodrama as a method of group psychotherapy, he identified five basic elements: (1) the *protagonist*, the person who portrays his or her own life situation on the stage; (2) the *director*, usually a trained psychodramatist who leads the session; (3) *auxiliary egos*, persons who play parts that are significant for the protagonist, as other persons, parts of the self, objects, or symbols; (4) the *audience*, those present who serve as a pool from which the auxiliaries are chosen and participate in the initial warm-up and the sharing at the end of the session; and (5) the *stage* or action area, which can be a three-tiered circular stage (invented by Moreno in 1922), where the protagonist can present his or her world, reality, or life space.

Although a number of techniques can be used, it is primarily through reversing roles with significant others that the protagonist achieves an emotional catharsis, and then, new insights in a catharsis of integration. In the final scenes of the drama an opportunity may be provided for role training, behavioral practice, or the protagonist to experience an ideal situation. For Moreno these techniques went beyond reality to create a "surplus reality."

Sociodrama. Moreno defined sociodrama as a deep action method dealing with intergroup relations and collective ideologies ([1934] 1953, p. 87). In contrast to psychodrama with its focus on the individual, the true subject of sociodrama is the group. For Moreno, the concept underlying this approach is that humans are role players and that every culture is characterized by a certain set of roles which are imposed with varying degrees of success upon its members. The sociodramatic approach deals with social problems arising from role conflicts and aims at social catharsis. In its methods and

over-all format sociodrama is similar to psychodrama.

Sociometry. The elements of sociometric, psychodramatic, and sociodramatic theory are essentially the same, since all three sets of concepts are part of one over-all theory. The differences are mainly a matter of emphasis, derived from the way in which they are used. For sociometry the goal is the creation of societies that make it possible for each individual to survive as a creative human being. The practice of bringing social health to whole societies Moreno sometimes called "sociatry."

Moreno saw individuals in groups as parts of "social atoms," in which the individual was surrounded by persons with whom she or he had positive or negative relationships. He used the term "tele" to refer to the two-way bond that combined elements of empathy and transference. Individuals were also seen as the focal point in a pattern of role relations that they share with others, their "cultural atom." If groups could be rearranged so that each individual was surrounded by persons with relationships of positive "tele" and with minimal conflict of roles, then the individuals could be more spontaneous. On occasion this spontaneity would generate creative responses to situations that might then be passed on to others in the form of "cultural conserves."

Developing sociometry in the United States. In 1925 Moreno left Vienna for the United States to promote an electromagnetic recorder he had invented. It was a radio film to record sound on discs for radio transmissions and reception. For the first two years in the United States (1925-1927), he continued working on his invention in Ohio. He then settled in New York where he became a licensed physician in 1927, began private psychiatric practice, and introduced psychodramatic work in several institutions. He also organized a new version of the spontaneity theater, the "living newspaper." Performances were given at Carnegie Hall (1929-1931), where the news events of the day were acted out. His ideas about spontaneity and creativity were published in the magazine *Impromptu* in 1931, and later reprinted in the first volume of *Psychodrama* (1946-1969). His work in sociometry at Sing Sing prison led to monographs on the group method for the classification of prisoners, collected in *The First Book on Group Psychotherapy* (1932).

During this period his wife, Zerka Moreno, notes that he was a problem for the psychiatric

fraternity: "His views of man, and his interpersonal and intergroup relations flew in the face of all that was being taught. He was just too controversial, too personally difficult to accept: a maverick, a loner, a narcissistic leader, charismatic but aloof, gregarious but selective, lovable but eccentric, unlovable and appealing" (1976, p. 132). Despite this he received support from Dr. William Alanson White, who made it possible for Moreno to construct a theater for psychodrama at St. Elizabeth's Hospital in Washington, where White was superintendent. White also encouraged Moreno's sociometric reorganization of the Hudson School for girls from 1932 to 1938. Some of the results of this study were published in 1936 as a report to the board of directors of the school in the *Sociometric Review*, but the major report, with a foreword by White, was published under the title *Who Shall Survive?* (1934). The revision and expansion of this book, in which he added discussions of all of the important concepts in psychodrama, sociodrama, and sociatry, was Moreno's *magnum opus*.

Promoting group psychotherapy. During the next 18 years (1936-1953) Moreno wrote largely on the major concepts and methods of sociometry and psychodrama. In 1936 he opened his sanatorium at Beacon, New York, and in 1940 he founded the Psychodramatic Institute in the same city. He was a special lecturer at the New School for Social Research in 1937/1938, and at Teachers College, Columbia University, in 1939/1940. From 1951 through 1966 he was an adjunct professor in the department of sociology in the Graduate School of Arts and Sciences at New York University.

Moreno was also active in promoting group psychotherapy. In 1945 he edited a special combined issue of *Sociometry* on the subject, published under the title: *Group Psychotherapy: A Symposium*. In 1951 he organized the International Committee on Group Psychotherapy, later enlarged and called the International Council of Group Psychotherapy, which became responsible for arranging and sponsoring a series of international congresses on group psychotherapy. The first one took place in 1954 in Toronto. Before his death he was able to transform the council into an incorporated International Association of Group Psychotherapy numbering almost eight hundred members from many countries. This was his last achievement; it took place during the fifth international congress in Zurich in August 1973.

Twenty-six years after he had departed from Europe, Moreno began a series of trips to bring group psychotherapy and other areas of his concern to the attention of his overseas colleagues. The Sorbonne honored him with the establishment of a Sociometric Institute in the sociology department, then under the guidance of Georges Gurvitch. In 1954, he was invited by the U.S. State Department to undertake a tour of various universities and America Houses in West Germany. Similar tours were later undertaken at regular intervals throughout Europe; several included invitations by the United Nations Educational, Scientific and Cultural Organization. The countries, some of which were visited repeatedly, were France, Germany, Norway, Switzerland, Austria, Italy, Spain, Czechoslovakia, Hungary, Russia, Greece, Yugoslavia, Turkey, and Israel.

The third psychiatric revolution. During the seven years from 1954 to 1960, Moreno saw the spread of his ideas in social science literature throughout the world. "The third psychiatric revolution," as he termed it, had now arrived. The group had become both doctor and patient. Moreno's articles on psychodrama were in wide demand for collections of readings and handbooks. From 1956 through 1960 he edited five volumes in *Progress in Psychotherapy*, first with Frieda Fromm-Reichmann as senior editor and then with Jules H. Masserman. He also published *Gruppenpsychotherapie und Psychodrama* (1959), which is similar to the third volume of *Psychodrama* (1946-1969), but also contains original material that was never translated into English.

Review and evaluation. The last period from 1961 until his death on May 14, 1974 was one in which Moreno reviewed the history of the movements he had initiated and evaluated their impact. Additional articles appeared in collections, handbooks, and foreign publications, as his work continued to spread. Moreno does not give much space in his writing to acknowledgments of the persons whose work influenced his own creativity. He was much more concerned about how his own ideas were being used by others, often he thought, without due credit. As a social reformer he saw himself in the tradition of Jean Jacques Rousseau, Johann H. Pestalozzi, and Friedrich Froebel, although he wished to keep the initiative for reform in the hands of the people.

The Medical Society of the State of New York, of which he was a life member, gave him a ci-

tation in 1967 in recognition of fifty years of medical practice. In 1968 he received an honorary doctorate from the medical faculty of the University of Barcelona (Spain), and in 1969 a golden doctor diploma from the University of Vienna. In the same year a plaque was placed on the house in Vosslau near Vienna to commemorate his work there as a public health officer from 1918 to 1925.

He was a fellow of the American Psychiatric Association, a life member of the American Medical Association, and a member of the American Society for Group Psychotherapy and Psychodrama and the American Sociometric Association, of which he was president in 1945.

Moreno's legacy. By 1976 thousands of social scientists had used sociometric methods in their research, many of them without any realization of the part Moreno had played in developing the method that had since become part of the public domain. However, a record had been kept of persons who trained as psychodrama directors, either directly with Moreno or in one of the institutes established by his pupils. There were approximately fifteen hundred practitioners of psychodrama, primarily in the United States. Some of these practitioners were active on the staffs of institutes involved in teaching and training in psychodrama, sociometry, and group processes. There were 23 institutes in the United States and 11 in other countries. Practitioners gave courses on psychodrama and sociometry in 14 universities in the United States and 12 in other countries.

During his professional career Moreno published more than three hundred books and articles. Many of his articles were republished in whole or in part in other journals in English, in foreign journals, handbooks, and other edited collections. He also founded and edited a number of journals: *Daimon*; *Impromptu*; *Sociometric Review*, which later became *Sociometry* and was turned over to the American Sociological Association in 1956; and *Sociatry*, which became *Group Psychotherapy* and later *Group Psychotherapy and Psychodrama*. In 1956 he founded the *International Journal of Sociometry and Sociatry* which became the *Handbook of International Sociometry* in 1971. Moreno was working on the manuscript of an autobiography when he died, and he also left a number of unpublished articles.

Thus it was likely that even after his death, the list of Moreno's publications would continue to grow. Given the number of creative contri-

butions he had made during his lifetime it was certain that his impact on social science and on various forms of group psychotherapy would continue to expand.

A. PAUL HARE

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MORGENSTERN, OSKAR

Oskar Morgenstern was born in Goerlitz in Silesia, Germany, on January 24, 1902. He was the son of a small businessman and an illegitimate daughter of Emperor Frederick III of Germany. He died on July 26, 1977, in his house in Princeton, New Jersey.

He was a man of great tenacity, fortitude, and optimism. The two intellectual centers of his life were Vienna and Princeton; not the universities alone or even primarily, but the many individuals and institutions, such as the Institute for Advanced Study at Princeton, that were active at these locations.

His family moved to Vienna when he was young, and he attended high school and university in Vienna, where he obtained his doctorate in 1925. He was greatly influenced by Karl Menger and Eugen von Böhm-Bawerk, whose work stimulated his lifetime interest in the problem of the division of product between cooperating agents and in the problem of exchange.

After receiving his doctorate he was able to obtain the Laura Spelman Rockefeller fellowship, which enabled him to study in London, Paris, Rome, and at Harvard and Columbia universities for a period of three years. During this period he had the opportunity to meet with Francis Ysidro Edgeworth; Antonio de Viti de Marco; Luigi Einaudi; Alfred North Whitehead; and several others. One of his earliest publications was an obituary of Edgeworth that appeared in 1927.

His first major work, *Wirtschaftsprognose* (1928), served as his *Habilitation* thesis, which enabled him to become a *Privatdozent* at the University of Vienna in 1929. In this book Morgenstern considered the difficulties and paradoxes inherent in economic prediction. He was particularly concerned with the influence of prediction upon the event predicted. He illustrated the difficulty with the example of the pursuit of Sherlock Holmes by Professor Moriarty, showing that an "I think, that he thinks that I think . . ." chain of reasoning does not yield a satisfactory solution.

He was promoted to professor at the University of Vienna in 1935. In that year he published "Vollkommene Voraussicht und wirtschaftliches Gleichgewicht," which dealt with the fundamental difficulties in the assumption of perfect foresight in the study of economic equilibrium. The mathematician Edward Čech heard Morgen-

stern lecture in Vienna on this topic and pointed out that the problems raised by Morgenstern were related to those treated by John von Neumann in "Zur Theorie der Gesellschaftsspiele" (1928).

During the years 1929–1938 his life was filled with both pure research and applied activities. He was editor of the *Zeitschrift für Nationalökonomie*, an adviser to the national bank of Austria, and from 1931 to 1938 held the position of director of the Austrian Institute for Business Cycle Research. He was a member of the League of Nations advisory group of statisticians until its dissolution in 1945. He was also active in the "Vienna Circle," an important intellectual group led by the philosopher Moritz Schlick. Karl Menger, Karl Popper, Kurt Gödel, and Rudolf Carnap were among his friends and acquaintances.

In January 1938, Morgenstern, invited by the Carnegie Endowment for International Peace, visited the United States to give a series of lectures. While he was in the United States the Nazis occupied Austria and he was dismissed from the University of Vienna as "politically unbearable." He was offered several posts at American universities and accepted the offer from Princeton, partly because of the presence of John von Neumann at the Institute for Advanced Study. He and von Neumann first remembered meeting each other at a meeting on February 1, 1939, where Morgenstern gave a talk on business cycles. They quickly became close friends and remained so until von Neumann's death on February 8, 1957.

The theory of games. The friendship and intense discussions between Morgenstern and von Neumann led the two to embark upon a joint work that would show economists the potential of an approach to the study of social behavior in general, and economic behavior in particular, by means of formal mathematical models of games. They soon realized that they had set themselves a major task that would require at least one book of considerable size. In 1944 they published the *Theory of Games and Economic Behavior*. This major work contained a basically important novel approach to utility theory. The approach used three different representations of a game of strategy—"the extensive form," "the strategic form," and "the cooperative form," of a game—and two solution concepts—the maxmin solution for two-person zero-sum games (von Neumann's way of resolving the problem that Morgenstern had posed in his discussion of

Holmes and Moriarty), and the stable-set solution. In many ways Morgenstern was a master modeler, with a sensitivity to relevant paradox; and von Neumann was a master analyst. They appreciated each other's strengths and weaknesses.

Morgenstern and von Neumann were aware of the limitations of their initial work in game theory. In the first chapter of their book, they stressed that they were beginning by trying to provide a sound basis for a static theory and that the history of science indicated that a satisfactory dynamics might look completely different. This concern was manifested in the modeling of the three different forms of a game.

The extensive form describes a game in complete detail. It provides a total anatomy in which subtle differences in information conditions and the details of moves can be identified. The now familiar "game tree" is closely related to the decision tree used in statistics and is of considerable influence in mathematical psychology.

The extensive form is of great use in laying out the details of process and exploring conditions on information, but the strategic form provides a natural and powerful way to illustrate the essential aspects of strategic control over the environment by different individuals. The latter can be deduced from the former, but not vice versa.

It is to the two-person game of pure opposition (that is, the winnings of one player are completely negatively correlated with the winnings of the other) that the maxmin solution theory of von Neumann was first applied. The brilliant work of John F. Nash, Jr. (1951) provided a solution theory for general n -person games (games with any number of players whose goals can contain any mixture of coinciding or conflicting interests). However, although Morgenstern recognized that Nash had forced a link between the early mathematical economics of Antoine Augustin Cournot (1838) and the theory of games, neither he nor von Neumann was particularly taken with the "non-cooperative equilibrium point" solution.

Their major interest was in cooperative solutions, and to that point the cooperative form of a game was proposed. The device used was the "characteristic function" of an n -person game. This describes the potential gains that any one of the 2^n coalitions that can be formed among n -persons can obtain.

The characteristic function can be deduced

from the strategic form of a game but not vice versa. The solution theory they proposed was that of the "stable set." Although the stable set has great mathematical sophistication and interesting interpretations, it has not proved to be as good a solution concept as had originally been expected. In particular, the conjecture that the stable-set solution would exist for all games was proved false by William Lucas.

In his years at Princeton, from 1938 until his retirement in 1970, Morgenstern's relationship with the department of economics was far from smooth. There was little interest in the theory of games. Students in economics who specialized in game theory were few. However, through a combination of the economics research project sponsored by the Office of Naval Research and the active interest in the mathematics department in combinatoric methods and game theory, a distinguished roster of younger scholars began to work on the theory of games.

Among the Princeton students with some interest in game theory in the 1950s and 1960s were Donald Gillies, John Milnor, John Nash, Lloyd Shapley, Martin Shubik, and Gerald Thompson. Later there were Robert Aumann, Ralph Gomory, William Lucas, Herbert Scarf, and many others. These individuals represent only a fraction of those who by the time of Oskar Morgenstern's death had contributed to the growth of more than six thousand books and articles on the theory of games.

In spite of the steady growth of literature on game theory, honors and recognition were relatively slow to come. Even at Morgenstern's death a large part of the economics profession was highly skeptical of, or indifferent to, the uses or potential uses of game theory in economics. Nevertheless, in 1971 the Musée de la Monnaie in France commissioned the artist Georgeo Mathieu to create a medal in honor of the theory of games, as part of a series of 17 medals to commemorate important stages in the development of Western thought. Morgenstern was made a corresponding member of the Institute of France; in 1976 he was made a distinguished fellow of the American Economic Association; he was a member of the American Academy of Arts and Sciences. In 1976 he was awarded the great golden cross of the Republic of Austria, and in 1977 was made an honorary member of the Hebrew University in Jerusalem. These honors, together with several honorary doctorates, did not cause him to change his belief that the theory of games was such a radical

departure from the accepted ways of economic thought that the speed of its acceptance was bound to be slow.

If anything, judged by the growth of the game theory literature in economics, political science, experimental gaming, and even law and biology, Morgenstern's predictions were probably too pessimistic. In particular, neither he nor von Neumann, apparently, saw the proliferation of solution concepts and their application that took place in the development of game theory. These include the "core" of Shapley, Gillies, Shubik, Scarf, and Debreu; the "value" of Harsanyi, Nash, and Shapley; the "noncooperative equilibrium" of Nash, and the "bargaining set" of Aumann and Maschler.

Other later works. The theory of games was undoubtedly Morgenstern's greatest contribution and collaboration. However, his interests as an economist were far more wide ranging. In particular, he maintained throughout his life a keen interest in the way in which economic information is gathered and in the use and misuse of economic statistics. In 1950 he published *On the Accuracy of Economic Observations*, and in 1970 he published jointly with C. W. J. Granger a searching study entitled the *Predictability of Stock Market Prices*.

He had a considerable interest in matters of national defense and felt strongly (as did von Neumann) that it was important for senior scientists to participate actively in helping to analyze and formulate policy. In 1959 he published *The Question of National Defense*, and in 1973, in collaboration with Klaus Peter Heiss and Klaus Knorr, *Long Term Projections of Power: Political, Economic, and Military Forecasting*.

Virtually to the end he regarded himself as an outrider in economic theory, and in 1972 he published in the *Journal of Economic Literature* a highly critical article entitled "Thirteen Critical Points in Contemporary Economic Theory."

He had a deep interest in the elegant work that von Neumann put forth as early as 1932 on the expansion of an economic system, and in the early 1950s he encouraged von Neumann to present his thoughts to a somewhat nonreceptive economics department at Princeton. That model became one of the keystones of modern growth theory.

Morgenstern remained fascinated with the ideas of von Neumann on growth, and he expanded them to include ideas on contracting and decomposable economies. Knowing for a

considerable time that he had incurable cancer, he was concerned to live long enough to finish a book written with Gerald Thompson entitled the *Mathematical Theory of Expanding and Contracting Economies* (1976). He remained intellectually vigorous, curious, and active to the last.

Other contributions. Morgenstern was an entrepreneur of talent, who was particularly concerned with attracting powerful mathematicians to the consideration of problems in the social sciences. He was deeply grateful to the Office of Naval Research for the support it gave to his research program over many years, and was an editor of the *Naval Logistics Research Quarterly*.

Because he believed that a good economist should be able to do applied work, he was one of the founders in 1959 of *Mathematica*, a highly successful and sophisticated consulting firm, remaining chairman of the board until his death. He was also instrumental in the founding of the Institute for Advanced Study in Vienna.

In 1948, Morgenstern married Dorothy Young and they had two children.

MARTIN SHUBIK

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MORGENTHAU, HANS J.

Hans J. Morgenthau was born in 1904 in Coburg, a small town in central Germany that is now part of northern Bavaria. The ruler of the duchy, a grandson of Queen Victoria, was known for his public display of German nationalism and, later, for his support of Hitler's cruel practice of anti-Semitism. As a schoolboy during World War I and its aftermath, Morgenthau witnessed the defeat of a powerful German army once confident of early victory, the flight of the leaders of the Imperial government and their

replacement by a Weimar régime that suffered the lack of a sense of power and a too-narrow political base and that drew its support primarily from the working class and the bourgeoisie.

Because the war had ended with no foreign troops on German soil, the enemies of Weimar promulgated the legend that Germany had been defeated because of a "stab in the back." First the former ruling class (overwhelmingly the most influential group in such a community as Coburg) and then the Nazis propagated the myth that Germany had not actually lost the war but had been undermined by traitors within, such as the trade unions, socialists, Jews, Catholics, liberals, and freemasons. The Nazi party founded in 1919 turned this legend into a powerful instrument of political propaganda. For the young Morgenthau, the Weimar era demonstrated the importance of political power and its interplay with swiftly flowing currents of irrationalism in politics. The German people needed a scapegoat in order to account for military defeat and rampant inflation, and they found it in the Jews. In such an environment, Morgenthau learned early that a government must have the capacity of *governing*, including the ability to maintain economic stability.

As an only child, Morgenthau suffered from profound loneliness and frequent illness caused or aggravated by family circumstances. His father was tyrannical and authoritarian; his mother warm and highly intelligent. His father's influence helped to create in him an inferiority complex, a fear of rejection, and an undisguised shyness that have endured throughout his life. Family relationships may help to explain the paradox later observed by many of his students: no professor was more actively involved in advancing their future, yet none appeared more reserved and detached.

Morgenthau's commitment to the creation of a Jewish homeland, and particularly the opinions in his later writings, strongly supporting Israel's quest for security, also go back to his formative years. He remembers having been spit upon while marching in the German equivalent of the American Boy Scouts, having been denied admission to an upper class fraternity in a classical Gymnasium, and being ridiculed and ostracized when he graduated first in his class and spoke at a founders' day ceremony celebrating the crowning of the duke of Coburg.

Morgenthau's early interest in literature, which his father discouraged, was a harbinger

of his lifelong preference for the concrete in history. His aspirations had been to become a writer, perhaps a professor, and possibly a poet. In September 1922, he wrote as a senior Gymnasium student: "My hopes for the future move in two directions. I hope for the lifting of the pressure to which I am exposed by the social environment, and I hope to find a direction and a purpose for my future activities. The latter cannot be realized before the former is fulfilled" (Thompson & Myers 1977, p. 1). In this essay he observed that his relationship to his environment was determined by three facts: he was a German, he was a Jew, and he had matured after World War I. He resolved not to play the role of suffering martyr but to oppose the pressures of anti-Semitism which he asserted shattered all foundations of morality: "The stronger the pressure from outside becomes, the more violent and one-sided will be my reaction to this movement. . . ." He saw himself as approaching a choice between two activities, the amassing of riches or the service of a higher cause. As a guide to his choice he quoted a passage from Goethe's autobiography, *Poetry and Truth*: "Our desires are presentiments of the abilities that lie within ourselves. . . . We feel a longing for what we already possess in silence. . . . If such a direction is decisively presented by our nature, every step in our development fulfills a part of the original desire. . . ."

Morgenthau began his university studies in 1923 at the University of Frankfurt but soon transferred to the University of Munich. His professors at both institutions included learned men but not internationally recognized scholars. He studied philosophy, which along with literature, had dominated his intellectual interests.

His early studies in philosophy, however, were disappointing, except for the writings of Benedetto Croce. The young student was repulsed by the pedantry of minute epistemological distinctions that went so far even as to dissect individual sentences in philosophical writings. So he moved to the University of Munich and the study of law, and came under the influence of two outstanding teachers, Heinrich Wölfflin, an art historian who had founded a school of aesthetics, and Hermann Oncken, a diplomatic historian who discussed the relationships between history and personalities and lectured on Bismarck and nineteenth-century foreign and military policy. Looking back, Morgenthau wrote in "Fragment of an Intellectual Autobiography: 1904-1932": "For the

first time, I felt the impact of a coherent system of thought, primarily a distillation of Bismarck's *Realpolitik*, that . . . [supported] my isolated and impressionistic judgments on contemporary issues of foreign policy" (Thompson & Myers 1977, p. 6). At Munich, he was also introduced by Karl Rothenbücher to Max Weber's political and social philosophy; he later wrote: "Weber was everything most of his colleagues pretended to be but were not . . . a passionate observer [as a citizen] of the political scene and a frustrated participant in it, as a scholar . . . [viewing] politics without passion . . . [or] political purpose beyond the intellectual one of understanding" (*ibid.*, p. 7). Weber became for Morgenthau the model of the political scientist.

Two other schools of thought had largely negative influences on Morgenthau: Marxism and psychoanalysis. Marxism had attracted many younger intellectuals as an instrument for hastening the disintegration of an unjust post-war society and rebuilding it on more equitable foundations. Its focal point in Germany was the Institut für Sozialforschung at the University of Frankfurt, whose more prominent members were avowed Marxists. Morgenthau, though acknowledging a limited indebtedness to Marxian sociology, was repelled, as the Nazi enemy stood at the gate, by Marxists who split hairs, pedantically picked at true meaning of phrases, clauses, and sentences. Listening to Karl Mannheim appealing for "free-floating intelligence" against the Nazis, Morgenthau understood Marx's declaration to his son-in-law: "Moi, je ne suis pas Marxiste." As for psychoanalysis, he had no doubt that Freud, like Marx, had opened new vistas of human understanding, and at one stage he tried to construct a theoretical system of politics based on Freudian concepts and insights. He abandoned the effort, however, without publishing the results, concluding: "What defeats a psychoanalytical theory of politics is what has defeated a Marxist theory of politics: the impossibility of accounting for complexities of political experience with the simplicities of a reductionist theory, economic or psychological" (Thompson & Myers 1977, p. 4).

Morgenthau pursued postgraduate work at the Graduate Institute of International Studies in Geneva, was admitted to the bar, and served as acting president of the Labor Law Court in Frankfurt. In 1932 he joined the University of Geneva to teach public law, and, because of Hitler's rise in 1933 remained in Geneva until 1935,

proceeding then to Madrid, where he taught until 1936. He moved to the United States the following year without friends or sponsors, but his persistence and intellectual vitality won him successive faculty appointments at Brooklyn College (1937–1939) and the University of Kansas City (1939–1943). He went on to appointments at the University of Chicago (1943–1971) and the City College of New York (1968–1975). In 1975, he began teaching at the New School for Social Research in New York.

Morgenthau's study of international politics is marked by a dual emphasis on philosophy and politics. His first major work, *Scientific Man vs. Power Politics* (1946), was an original and forceful criticism of the social, political, and moral philosophy of modern Western thought and its consequences for political life. Making reference to the rise and fall of the Roman Empire, he traced the crises of the first half of the twentieth century and the general decay in political thought, reflected in "the belief in the power of science to solve all problems and, more particularly, all political problems" (1946, p. vi). Although pessimistic about the future, especially the failures of liberalism, Morgenthau, influenced by Reinhold Niebuhr, called for a renewal of faith in "those intellectual and moral faculties of man to which alone the problems of the social world will yield" (*ibid.*). The study also challenged the scientific approach to politics prevailing in the United States, going back to *New Aspects of Politics* (1925) by Charles E. Merriam. When Leonard White, a supporter of Merriam who was Morgenthau's superior at the University of Chicago, read *Scientific Man*, he suggested the author teach a course in administrative law to put *him* on the right track. At Chicago, Morgenthau contended with an intellectual atmosphere in political science hostile at the time to philosophy (the dominant interest of the Chicago department was public administration and international law). His main encouragement came from the university's top leadership, particularly Robert M. Hutchins, a few of his younger colleagues, and most of all from his students, who entered his classes with skepticism and generally left with enduring respect for his approach. The empiricists and behaviorists saw in him a threat to their generously endowed research and, not by accident, the severest critics of President Hutchins (whose own neo-Thomism and world government crusade later caused a breach with Morgenthau) opposed his devotion to philosophy.

By 1948 with the publication of his epoch-making text, *Politics Among Nations*, criticism of his work was redirected at the second element in his approach—his definition and concept of politics. The subtitle described his purpose as an inquiry into *The Struggle for Power and Peace* and in it he wrote: “Whatever the ultimate aims of international politics, power is always the immediate aim” (1948, p. 13). Then he added: “the struggle for power is universal in time and space and is an undeniable fact of experience . . .” (1948, p. 17). It would be difficult today to imagine the sense of concern and alarm touched off by his formulation. Power politics at the time was a questionable and controversial phrase at Chicago. It was what world government and public administration were to eradicate and epitomized all that was evil and had to be uprooted if men were to live in a civilized world. American political theorists condemned Morgenthau’s “Germanic way of looking at things.” Practical politicians, whose popularity depended on their appearing to stand for less ignoble ends than power, were quick to dissociate themselves, publicly at least, from his definition of politics.

Ironically, critics overlooked Morgenthau’s early emphasis on the limitations and proper use of power, its integral relation to national purpose and the constraints of national interest. Also overlooked was his extended analysis of international morality and the role of ethics, mores, and laws. He wrote: “From the Bible to the ethics and constitutional arrangements of modern democracy, the main functions of these normative systems has been to keep aspirations for power within socially tolerable bounds” (1948, p. 169). The fundamental error to which he called attention in 1951 in his first comprehensive treatise on American foreign policy, *In Defense of the National Interest*, was to oppose moral principles and the national interest. He insisted that “the choice is not between moral principles and the national interest, devoid of moral dignity, but between one set of moral principles divorced from political reality, and another set of moral principles derived from political reality” (1951, p. 33). He called on Americans to relearn the principles of statecraft and political morality which had guided the Founding Fathers and had continued, oftentimes in moralistic disguise, through the first century of the republic’s existence.

Morgenthau continued thereafter in a vast outpouring of articles and books on American

foreign policy to examine, test and apply his central principles of power, interest, and morality. In *The Purpose of American Politics* (1960), he wrote: “In order to be worthy of our lasting sympathy, a nation must pursue its interests for the sake of a transcendent purpose that gives meaning to the day-to-day operations of its foreign policy” (1960, p. 8). Such moral principles must be applied in the international environment prudently, and with careful regard to their political consequences. Military and economic power were not to be employed to serve diverse universal humanitarian missions, but must be measured against the imperatives of the national interest. He warned against letting the fear of communism influence foreign policy too strongly and he maintained that indiscriminate anticommunism could not provide the basis for sound policy. In a steady stream of writings directed to specific foreign policy problems, including *Vietnam and the United States* (1965), *A New Foreign Policy for the United States* (1969), and *Truth and Power* (1970), he criticized a crusading foreign policy based on moral abstractions and the transfer of American policies successfully employed in Europe to Asia and the Third World. By the mid-1960s he had become America’s major critic of the Vietnam War, basing his criticism on a principle enunciated in *Politics Among Nations*: Never put yourself in a position from which you cannot retreat without a loss of face, and from which you cannot advance without undue risk. He engaged in public debates with such American officials as national security advisers, McGeorge Bundy and Zbigniew K. Brzezinski. He entered the public arena reluctantly, for at one stage he seems to have agreed with J. Robert Oppenheimer, who warned that if in politics one tried at the same time to be both actor and observer, one would fail in both respects. Whatever Morgenthau may himself have written about the limitations of the philosopher in politics, he defied in practice. A successful classroom teacher, he sought to make the Congress, successive administrations, and every available public an extended classroom. He traveled to every corner of the globe, unsparing of himself and unyielding in his criticism of what he considered false teachings and prevailing nostrums.

It is difficult to measure Morgenthau’s influence, and writers such as Raymond Aron have pointed to his tragic failure to reshape American foreign policy. His impact on foreign policy has been greatest in the realm of principles, not of

tactics or day-to-day decisions. By the 1970s, however, no responsible leader in American public life dared scorn the need to consider the national interest in the formulation of American foreign policy. No secretary of state could pretend that the world was rid of international rivalries or power politics. No liberal journalist could ignore the counterforces of nationalism and internationalism. Morgenthau was not alone in his teachings, but realist principles would not have been so powerfully communicated without his voice. In two personal respects, moreover, he seems to have realized his mission. First, he succeeded in discovering what he had sought as an 18-year-old: a cause that would survive him and that would justify his moral and intellectual journey. Second, he has held firm to a goal that he himself has best described:

Our aspirations, molding our expectations, take account of what we would like the empirical world to look like rather than what it actually is. Thus endlessly, empirical reality denies the validity of our aspirations and expectations . . . We expect the oracle to give us a clearcut answer. What we get is an enigma compounding the riddle. What remains is a searching mind, conscious of itself and of the world, seeing, hearing, feeling, thinking, and speaking—seeking ultimate reality beyond illusion.” (Thompson & Myers 1977, pp. 16–17)

KENNETH W. THOMPSON

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MOWRER, O. H.

O. Hobart Mowrer was born in 1907. He is research professor of psychology emeritus at the University of Illinois, and has been a creative and provocative force in the last forty years of dialogue in the behavioral sciences. His knack for arousing either immense acclaim or intense reproof from both scientific and lay audiences stems from two factors. In a day of single-minded specialization, he has been something of a Renaissance scholar, unwilling to remain “at home” with his specialty; moreover, whether “at home” or “afield,” he has had an instinct for the sacred matched only by a skill for incisive irreverence. Such a thinker is likely to appear either boldly refreshing and enlightening or heretical and intrusive.

Mowrer began his career as an undergraduate at the University of Missouri (1925–1929) under the tutelage of Max F. Meyer, a strict behaviorist antedating, though overshadowed by, John B. Watson. Meyer’s emphasis on the mechanics of behavior was reflected in Mowrer’s initial research on vestibular reflexes, first as a graduate student under Knight Dunlap at the Johns Hopkins University (PH.D., 1932), and later as a fellow at Princeton, Northwestern, and Yale universities. Paradoxically, the choice of this area of research also reflected a characteristic that became more apparent as Mowrer’s career advanced: his readiness to attend to his own subjective experiences. It was an inability to shake off his own confusion about directions while in Baltimore that helped spur his interest in vestibular and orientational phenomena.

In 1934 he went to Yale, ostensibly to study further the vestibular functions under Raymond Dodge. But Yale was a beehive of crossdisciplinary exchange because of its recently established (1929) Institute of Human Relations. Under this stimulus Mowrer’s interest in mechanistic psychology was soon to end, and he returned to questions *verboten* to the behaviorist that had haunted him as an undergraduate. Is not “preparatory set” something different from observable behavior (1938)? Do we not have to postulate a motivational state (fear) to explain anticipatory avoidance behavior, which, though functional, fails to replicate the “unconditional (escape) response” (Mowrer 1939; 1940; Mowrer & Lamoreaux 1942; 1946)?

Though now heralded as one of the first to apply “behavior modification” (Mowrer & Mowrer 1938), Mowrer has never been a be-

haviorist in the strict sense. He has been more interested in understanding and explaining than in predicting behavior. And while he has readily acknowledged that intervening variables are inventions of convenience, he has tried to render them as plausible hypothetical constructs. He has quoted in appreciation Gordon Allport's criticism of stimulus-response-behaviorism as the "psychology of the empty organism" and noted that the challenge for him has been to fill that emptiness with persuasive constructs about the nature of animals and man as learning, cognating, and deciding entities.

Even as Mowrer moved toward a "mentalist" rather than a "motoric" conception of preparatory set, he was entertaining another departure from orthodoxy. His association with Clark L. Hull was most cordial, and his initial notions were consonant with Hull's monistic theory of learning. In this theory, fear was viewed as a response with motivating properties, but one learned, like any other response, as a function of immediate reinforcement. Soon, however, Mowrer strongly questioned this theory. It asks us, he noted, to assume that rewarding a fear response (e.g., giving comfort to a fearful child) will strengthen the fear, while punishing the fear (e.g., scolding the fearful child) will weaken it. Surely the opposite is more nearly true.

He concluded that fear involves a principle of reinforcement different from motoric habits and expounded his now famous two-factor theory of learning (1947). Recognizing that a similar bifurcation had been proposed by a number of thinkers—C. S. Sherrington (1906), B. F. Skinner (1935), Jerzy Konorski and Stefan Miller (1937), and Harold Schlosberg (1937)—Mowrer nevertheless formulated a distinctive thesis resting on and reconciling a considerable body of prior research. He later attempted to demonstrate conclusively that fear did not observe reward reinforcement principles, but did observe conditioning (Pavlovian) principles (Mowrer 1950; Mowrer & Aiken 1954; Mowrer & Solomon 1954). In the judgment of many, he succeeded.

However, these supporting researches were no sooner public than Mowrer himself expressed critical concern. The impetus was again a personal experience, this time in the laboratory of Grant Fairbanks, a University of Illinois colleague and professor of speech who had invented an audio delayed feedback apparatus. Curious, Mowrer submitted to the apparatus and tried to speak and read. Hearing his own words delayed

about .25 seconds after uttering them had a characteristic, but to Mowrer, a shockingly unexpected result: his normally fluent speech was reduced to stuttering incapacity! How could a well-practiced series of motoric habits richly prompted by preceding cues be undone by a simple displacement of the audible sensory aftereffect? From that moment, Mowrer knew he could no longer support the Thorndike-Hull "bond" theory of motor learning that had been advanced in his own two-factor theory.

Shortly thereafter, he published a "revised two-factor theory" (1956) in which he swung to a Pavlovian rendering of all learning but retained the notion that there were two reinforcing conditions: drive onset (fear learning) and drive reduction (hope learning). Reminiscent of Edward C. Tolman, Mowrer now held that overt (motoric) behavior is *not* learned or fixed in hierarchical S-R categories. Instead, what is learned are a series of positive or negative emotional-attitudinal states associated with (cued by) the stimulus aftermath (kinesthetic, visual, auditory, etc.) of any action. Behavior as such is fluid, variable, "nonmechanical," and spontaneous. "Habits" emerge only because the sensory aftermath of "correct" behavior regularly elicits maximum hope and minimum fear reactions. To explain the initiation and efficient selection of behavior guided by such a servo-feedback concept, Mowrer expanded considerably upon a cognitive psychology in *Learning Theory and the Symbolic Processes* (1960b), the second of the two-volume set in which his work on learning culminated.

Since then Mowrer has been absorbed in the question of human behavioral disorder and recovery—a matter that has been of interest for a considerably longer period than his published work indicates. Mowrer's readiness to use and disclose his personal experience has been particularly evident in this arena. In autobiographical accounts (1966a; 1974) he has traced his struggles with depression and his initial search for relief through psychoanalytic therapy.

In 1945 a seminar with Harry Stack Sullivan illuminated a direction leading away from analytic psychology. Mowrer's break with Freudian tradition was made formal in the now classic paper (1948) proposing the "neurotic paradox." There and subsequently (1953) he rejected the "overlearning" (trauma) thesis embraced by Freud and others to explain self-defeating but self-perpetuating behavior (the "neurotic para-

dox"). He believed the "paradox" to be an artifact of conceptualization: Neurosis, he noted, is a form of "ignorance," of *underlearning* rather than *overlearning*. The neurotic has learned, in the guise of secretive, deceptive behavior, how *not* to learn (the full measure of appropriate concern and guilt). His actions are understandable, for he does what we are all tempted to do in the interest of immediate gratification. The mystery, if there is one, resides in the *normal* individual who manages by adulthood to develop the integrity to resist momentary gratifications in the interest of remote goals.

The frustrations of man's moral promptings, rather than of his biological urges, thus produce his inner disquiet. To drive home this point, Mowrer asserted that he preferred "sin" to "sick" as a term characterizing disordered persons (1960c). He could not have chosen a more sensitive nerve; the reaction was intense, even vitriolic. Though his point may have been lost on some respondents, the article has often been cited and is included in several edited works.

Then came a grand experiment. If human disorder was interpersonal and moral in essence, why not seek help from the huge existing capital, in both manpower and real estate, of the institutional church? Many colleagues viewed Mowrer's effort as a desertion of professionalism at best and a surrender to mysticism at worst. Meanwhile, many religious persons smarted under his biting analysis (1961) of the church's abrogation of ancient principles, preoccupation with theism at the cost of brotherhood, and abandonment of the "soul sick" to the secular healing of psychoanalysts.

The church was not unmindful of Mowrer's campaign, but he soon concluded that too much inertia was inherent in its structure. With characteristic willingness to abandon the impractical, he began to examine the domain of self-help groups (1966b). A review of research (1968) showed consistent support for his position that troubled individuals have customarily violated their community with significant others. His appreciation has steadily grown for the power of Alcoholics Anonymous and other nonprofessional enterprises to address such human issues effectively.

At the age of 69, Mowrer undertook another revision of his theory, again prompted by personal experience. Although he had been free, for the most part, of depression for 13 years, a returning episode in 1966 introduced him to antidepressant drugs and prompted him to

study the new research on genetic predisposition and to modify his strict social-learning view of disorder. He has concluded from both his own experience and research reviews that a "diathesis-stress" concept is more nearly correct (1976).

In the 1960s, Ivar Lovaas, a prominent psychologist, observed to the author: "You think you have a new idea—the use of punishment with children, learned helplessness, or whatever. You go to the literature, and Mowrer has been there. It is amazing the range of virgin territory he has explored." No doubt he will continue to explore and revise.

V. EDWIN BIXENSTINE

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MUMFORD, LEWIS

Lewis Mumford was born in Flushing, New York, in 1895. Although he attended the City College of New York, Columbia University, and the New School for Social Research, with Thorstein Veblen an influential professor, Mumford never received an undergraduate degree. He married Sophia Wittenberg in 1921, and they came to divide their residence between New York City and a farmhouse near Amenia, which after 1930 increasingly served as home for them and their children, and as his writing base.

As a writer, Mumford's career, spanning 65 years, has been characterized by an intensive and sustained intellectual exploration—a self-directed, continuing education that he shares with readers through his outpouring of writings. Most readers have been exposed selectively to those Mumford writings that relate to their interests. Thus, some have read his early books on nineteenth-century American literature, some his architectural criticism, some his history of technology, some his history of cities, some his accounts of man's search for his own humanness and of man's utopian images, and some his

call to arms against fascism or his warnings about atomic bombs.

Mumford's pervasive thrust has been historical criticism; seeking to learn from the lessons of the past as a basis for choosing directions into the future. His evaluative characterizations of the major periods of Western civilization search out the evolving meanings of humanness, the good life, and the best societal and environmental settings for such a life. His books may conveniently be grouped according to their several themes: (1) critical reviews of nineteenth-century American literature, arts, and architecture; (2) broad humanistic reviews of the evolution of Western man; (3) critical history of technology as it relates to human development; and (4) critical history of cities and city planning. The first grouping represents an early and relatively separate period, the second encompasses his central theme, while the third and fourth represent concurrent themes embellishing that central theme. In addition, Mumford has published numerous collections of articles, and in recent years, autobiographical notes. An autobiography covering his first eighty years is forthcoming (1978).

While still in his thirties, Mumford wrote four books—*Sticks and Stones* (1924), *The Golden Day* (1926), *Herman Melville* (1929), and *The Brown Decades* (1931)—dealing selectively with American literature, and with less emphasis, American architecture and painting, during the thirty years after the Civil War. Along with Van Wyck Brooks, Randolph Bourne, Waldo Frank, and other contemporaries, he "helped to place, not only himself, but an entire generation in possession of its usable past" (Dow 1977, pp. 37-38).

At the same time, Mumford began his appraisal of the condition of Western man. With *The Story of Utopias* (1922) providing a dominant motif—to the effect that people's ideas about improving their community settings are powerful guides for action—Mumford went on in *The Condition of Man* (1944), *The Conduct of Life* (1951), and *The Transformations of Man* (1956) "to show [man] what changes in his plan of life are necessary if he is to make the most of the vast powers he may now command" ([1944] 1963, p. v). Mumford is convinced that the pervasive inroads of the machine, the city, bureaucracy, centralized power, and competitiveness have led to excessive specialization, fragmented personalities, and a fundamental loss of humanness. Thus, in his words: "The central effort in

the renewal of life today must be to bring back the possibility of wholeness and balance, not indeed as goods in themselves, but as the condition for renewal and growth and self-transcendence. We must break down the segregation of functions and activities, both within the personality and within the community" ([1951] 1952, p. 187). Mumford seeks to give "emphasis precisely to those aspects of man's life that are usually neglected: his dreams, his purposes, his ideals, his utopias" (1944, p. 10). He judges the most important needs to be those that foster spiritual growth, and he repeatedly affirms the idea of organic growth. Skeptical of any system that promotes the great truth or the grand design, his organicism would encourage the harmonious and balanced coexistence of the fullest variety of approaches. Led off by *Technics and Civilization* (1934) and *The Culture of Cities* (1938), both to be discussed below, *The Condition of Man* and *The Conduct of Life* rounded out a tetralogy designated by Mumford as the renewal of life series. His prophetic message that by renewal of life we could recapture the humanness we have lost has brought him a world-wide following, not the least among younger readers.

In *Technics and Civilization*, perhaps his most original book, Mumford demonstrates that our technology, particularly the machine, is to be understood as an integral component and expression of the larger society and not as deterministic or external. He stresses those evolutionary innovations that were undramatically brought into service, such as physical containers (for grain or other food stuffs) and the organization of complex human collective effort (for the construction of public works). Fundamentally optimistic, Mumford concludes that "the next step toward re-orienting our technics consists in bringing it more completely into harmony with the new cultural and regional and societal and personal patterns we have coordinately begun to develop" (1934, p. 434). He expands his previous work on the history of technological development in his two-volume study, *The Myth of the Machine* (1967-1970). Again interpreting the emergence of technology within the broader context of man's continuing efforts to develop his human capabilities, he urges us to subordinate scientific discovery to the ends of fostering the kinds of human beings and the kind of society we want to become. Mumford urges us to displace "the mechanical world picture with an organic world picture, in the center of which stands man himself in person . . ."

(1967-1970, vol. 2, p. 393). Although less optimistic than in his 1934 book, he suggests that there are "already many indications, though scattered, faint, and often contradictory, that a fresh cultural transformation is in the making" (*ibid.*, p. 429).

To many social scientists, however, Mumford is best known for his writings on the city. His work on urban history and his views on urban development relate integrally both to his central concern that man renew his humanness and also to his special interest in the shaping of the urban environment as exemplified by his voluminous contributions to architectural criticism. Patrick Geddes, the Scottish sociologist-biologist-regionalist-planner, exercised a profound influence on Mumford, particularly regarding the concept of organic growth and various ideas about regionalism. *The Culture of Cities* as well as *The City in History* (1961) provide synoptic accounts of some five thousand years of city development, together with ideas for improving cities. Mumford strongly approves of Greek cities and medieval towns because he admires their association with the qualities of community life. He wants cities to be of a form and at a scale such that they provide a sense of order for, and never overpower, their residents. He is strongly drawn to the ideas of new towns (admiring Ebenezer Howard's "garden cities"), organic growth (the natural splicing in of the new so as to provide continuity from the old), and gridtype regional organization (smaller communities able to draw upon regional resources). He has had an extensive following among city planners and architects, although perhaps less in America (e.g., Friedmann 1962) than in Britain, where he provided philosophic support (e.g., Mumford 1943) and where he was awarded highest honors by the town planning and architectural professions, a university honorary degree, and a rare honorary knighthood. Some critics in both countries have accused him of overlooking inner-city problems in his advocacy of new towns, of failing to identify more fully the programs actually necessary to renew cities, and of furthering a long tradition of suspicion about large cities (e.g., Goist 1969; Starr 1976).

Seemingly self-confidently, Mumford has largely fashioned his own methodology. He eschews sharp contradictions between the "is," the "likely," and the "ought to be." He merges empirically-based reporting with intellectual exploration, while also drawing upon speculation

and intuitive understanding. In fashioning broad comparisons, involving distant time periods and divergent cultures, he is not above invoking poetic convenience. For example, he described his interpretation in *The Transformations of Man* as "the continued interplay between a mass of well-sifted specialized knowledge, a sprinkling of free but circumspect speculations, and the cumulative reports of personal observation and experience. . . . As a generalist I have taken advantage of a license too often self-denied by the specialized scholar: that of assembling data from widely different areas in order to bring out a larger pattern that otherwise escapes observation . . ." ([1956] 1972, pp. 1-2).

Not surprisingly, Mumford's work has evoked varied reactions from social scientists. Some question his disdain for empirical methodology, as did Richard C. Wade in his review of *The City in History* (Riesman, Wade, & Mandelker 1962, p. 297). Others, such as David Riesman, commend him for his generalism and his capacity for transcending specialties (*ibid.*, pp. 288, 293).

Lewis Mumford's original and enormous scholarly output will long be recognized as a remarkable interpretive exploration of our human evolution and a continuing search for the conditions conducive to the good life and the good community. His historical criticism stands in distinct contrast to the specialization and the empiricism characterizing so much of contemporary social science.

DONALD L. FOLEY

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MURDOCK, GEORGE P.

George Peter Murdock, American anthropologist, was born in 1897 on a prosperous farm near Meriden, Connecticut, the eldest of three children of George Bronson Murdock and Harriet Elizabeth Graves. His parents provided a background that was politically democratic, individualistic, and religiously agnostic, and that valued education and the cultivation of knowledge as the path to personal and social fulfillment. Murdock was educated accordingly. After attending Phillips Academy, Andover, he entered Yale University, where he earned the A.B. with honors in American history in 1919. His studies at Yale were interrupted by military service as a member of the National Guard in the Mexican border incident of 1916 and, again, in World War I, when he was commissioned a second lieutenant of field artillery. Following his graduation from Yale, Murdock studied law at Harvard University, from which he withdrew in his second year to travel in Asia and Europe. His travels, especially in Asia, rekindled a boyhood fascination with geography and led him to enroll for graduate study in a combined anthropology-sociology program under Albert G. Keller at Yale, where he received the Ph.D. in 1925. In the same year he married Carmen Emily Swanson.

After teaching sociology and anthropology at the University of Maryland for two years, Murdock returned to Yale in 1928 as an assistant professor in Keller's department of the science of society, which later became the department of sociology. Three years later, he received a joint appointment in the newly founded department of anthropology. He became fully affiliated with the latter department when he assumed its chairmanship in 1938. He was made professor of anthropology in the following year and remained at Yale for another 21 years, with time out for service as lieutenant commander (1943-1945) and commander (1945-1946) in the U.S. Naval Reserve during World War II. In 1960 he went to the University of Pittsburgh as Andrew Mellon professor of social anthropology, and he retired from this position in 1973 at the age of 75.

From Keller and Keller's predecessor, William Graham Sumner, Murdock derived an abiding interest in discovering ordered processes in social organization and in exploring social and cultural change. Out of his boyhood interest in

geography came a great love for world ethnography, a subject in which he achieved unequaled mastery. These interests were evident in his early works: an edited translation from the German of Julius Lippert's *The Evolution of Culture* (1931); *Our Primitive Contemporaries* (1934c), a book of ethnographic summaries widely used for many years in the teaching of anthropology; and an edited volume of essays in honor of Keller, entitled *Studies in the Science of Society* (1937c). Not having done field ethnography under Keller's tutelage, he set out after joining Yale's anthropology department to make up for this deficiency. Accordingly, he undertook a field study of the Haida in the summer of 1932 (1934a; 1936a) and of the Tenino in the summers of 1934 and 1935 (1938; 1958; 1965b). This experience convinced him that field study is essential in the training of anthropologists, a conviction he continued to hold throughout his career.

At the same time, he was preparing to pursue further his search for regularities in social and cultural phenomena. To do this required comparative study on a massive scale, and such study could not be undertaken until ethnographic data were first assembled and processed so that information could be retrieved readily for study purposes. The absence of such an archive of processed data had been a major obstacle to systematic comparative study in the past. In 1937, drawing on his experience in preparing *Our Primitive Contemporaries*, Murdock conceived and organized the massive cross-cultural survey (1940a; 1950a; 1950c; 1953) at Yale's Institute of Human Relations, of which he was a member. The psychologists, psychiatrists, sociologists, and anthropologists in the institute were committed to developing a general unified theory of behavior. They saw the creation of an organized body of data on human societies and cultures as essential and gave strong support to the cross-cultural survey. The first task for the survey was to prepare a comprehensive system for indexing cultural materials, which Murdock and his associates published as *Outline of Cultural Materials* (Murdock et al. 1938).

At the same time it was necessary to review the status of ethnographic information in order to develop a satisfactory working sample of cultures to be processed. Out of the accompanying bibliographic work came *Ethnographic Bibliography of North America* (1941). The latter

was republished in two revised editions and then, in collaboration with Timothy O'Leary, in a massively expanded edition (1975), that remains the definitive bibliography.

The problem of providing materials for comparative study continued to occupy Murdock's attention for the remainder of his active career. With C. S. Ford and J. W. M. Whiting, he applied the cross-cultural survey's indexing system to the processing of data for the U.S. Navy on all islands of the Pacific held by Japan prior to World War II. He and his colleagues then prepared twenty "Civil Affairs Handbooks" for the guidance of government administrators in these islands during and after the war. The indexing system was also used to organize information on a number of countries of strategic interest to the United States. Subsequently, Murdock was a prime mover with C. S. Ford in creating the Human Relations Area Files, Inc. (HRAF), established in 1949 as an interuniversity consortium to reorganize the cross-cultural survey and put it on a continuing basis (Ford 1970). Murdock served as its scientific consultant until 1961 and then as a member of its board of directors until 1972, when he was made an honorary director for life. He was board chairman in 1964.

To facilitate cross-cultural research further, Murdock set out to code a large sample of the world's cultures with respect to many items of interest to cultural and behavioral theorists. The results were published in preliminary form as "World Ethnographic Sample" (1957*b*). Then came the much fuller "Ethnographic Atlas" (1967) and for computer use a data bank on tape, on which Murdock continued to work until he retired. Other works arising from these data-processing activities were *Outline of South American Cultures* (1951*b*) and *Outline of World Cultures* (1954*a*), as well as papers on South American culture areas (1951*c*), sampling (1966; 1968; Murdock & White 1969), coding of particular subjects (Murdock & Morrow 1970; Murdock & Wilson 1972), the measurement of cultural complexity (Murdock & Provost 1973*b*), and the world distribution of theories of illness (Murdock, Wilson, & Frederick 1978).

Murdock's cross-cultural interests made him acutely conscious of the need for anthropologists to publish the results of their ethnographic field researches. To encourage such publication, he established in 1962 what soon

became the highly regarded international journal *Ethnology*, which he continued to edit with the help of his colleagues at the University of Pittsburgh until he retired.

His wartime researches and service as a military government officer in Okinawa led Murdock to take an active interest in promoting ethnographic research in the Pacific, especially in Micronesia. He played a leading role in organizing and overseeing the Coordinated Investigation of Micronesian Anthropology (CIMA), a program conducted under the Pacific Science Board of the National Research Council and funded by the Office of Naval Research (1948*a*). Under this program 33 anthropologists and linguists went to Micronesia; Murdock himself led a team of 5 researchers in 1947 to Truk, where he spent 5 months studying the social organization (Murdock & Goodenough 1947; Murdock 1948*b*; 1948*c*).

The CIMA program had several important sequels. One was that for almost a decade anthropologists were regularly included on the administrative staff of the Trust Territory of the Pacific. Another was that ethnographic and linguistic study continued to a point where Micronesia has become one of the best described areas of the Pacific. A third sequel was a program of ecological studies of atolls, including their human and cultural ecology, undertaken by the Pacific Science Board of the National Research Council. Murdock was an active member of this board and of the international Pacific Science Association until 1966. Called in as special adviser to the Bernice P. Bishop Museum in 1952, after the death of its director, Peter Buck, he made a number of important recommendations regarding administrative reorganization, exhibitions, developing financial support, and enlisting public interest (Bryan 1953, pp. 5-6). He later served as acting director of the museum in the spring of 1956. Representing Yale, he joined with Alexander Spoehr, the Bishop Museum's new director, and Leonard Mason, of the University of Hawaii, in organizing the Tri-institutional Pacific Program (TRIPP), which from 1953-1964 supported ethnographers, linguists, and archeologists in what were judged to be most critically needed and hitherto neglected areas of study. A paper on the ecosystems of high islands (1963) and another on developments in Oceanic linguistics (1964*a*) reflect Murdock's involvement in this program.

Murdock's concern to bring order to the world

ethnographic picture led him in the 1950s to turn his attention to Africa. There was a mass of ethnographic literature in French, German, English, Dutch, and Italian that had never been synthesized into a satisfactory overview. To deal with the problem, Murdock set himself a schedule of questions of general anthropological interest. He then went systematically through the literature on every known society to answer those questions. The results of this endeavor were published as *Africa: Its Peoples and Their Culture History* (1959a). Although the book was much criticized by Africanists, it put consideration of African ethnography on a new footing, and as a general synthesis it has not yet been superseded. A major contribution of the book and a companion paper (1960c) was the evidence he presented for the ancient development in west Africa of a distinctive food-producing tradition that was independent in origin from the one that originated in the ancient Near East.

In all the foregoing work, Murdock held social organization as a major interest, especially modes of family and kinship organization. He focused on social organization in his own field studies and made it a major concern in his comparative research. Early comparative studies dealt with descent and descent groups (1937b, 1940b), the division of labor by sex (1937a), and bifurcate merging kinship terminology (1947). These were followed by what was immediately recognized as a major work, *Social Structure* (1949c). Using a sample of 250 cultures, drawn in part from the cross-cultural survey, Murdock formulated and tested what remains the most carefully worked out theory of the determinants of kinship classification and descent reckoning. Building on the work of his predecessors, he produced a new typology of forms of social organization and proposed a theory of how these forms change one into another. This theory, according to Naroll, "explains much—though by no means all—of the variation in residence, descent, and kinship terminology among human societies. Its establishment is a major accomplishment of cross-cultural surveys" (1970, p. 1240). In the same book Murdock presented a new theory of the determinants of incest taboos.

Social Structure laid to rest a number of issues that had preoccupied students of family and kinship in the preceding decades. At the same time, the refinement of concepts and the greater degree of order Murdock brought to the

subject raised new problems. It stimulated yet further refinement of concepts and led to recognition of new forms of organization involving cognatic descent groups, in regard to which Murdock made a significant contribution of his own, including the volume *Social Structure in Southeast Asia* (1960b), which he edited and to which he contributed an important introductory paper (1960a).

His many other contributions to the study of social organization included analyses of the literature on the Inca (1934b), the Witoto (1936b), the Murngin (Murdock & Lawrence 1949), the peoples of Nigeria (1962), and the Natchez (Murdock, White, & Scaglione 1971). They also included cross-cultural and topical papers on the social regulation of sexual behavior (1949b), family stability (1950b), parental attitudes (Murdock & Whiting 1951), social organization in North America (1955b), parental kin terms (1959b), the distribution of kin term patterns (1970), cross-sex kin behavior (1971), and the division of labor by sex (1937a; Murdock & Provost 1973a). More general or theoretical papers dealt with changing emphases in the study of social organization (1955a), political moieties (1956b), the evolution of social organization (1959c), typology in social organization (1960d), and the kindred (1964b).

In all of his work, Murdock maintained a broad behavioral orientation. It is evident in the explanatory propositions he put forth in his cross-cultural studies. It is also a consistent theme in his theoretically oriented papers, dealing with culture as an object of scientific study (1932), the role of anthropology in the study of human relations (1941), the constants in otherwise diverse cultures (1945), how learning, society, culture, and personality must all be studied in the context of a unified science of behavior (1949a), the limitations of British social anthropology (1951a), intergroup antagonisms (1952), the interrelation of sociology and anthropology (1954b), culture change (1956a), anthropology as a comparative science (1957a), myths that have misguided anthropological theory (1972), and the scientifically appropriate approach to comparative research (1977).

Murdock's breadth of interest is also indicated in the range of his organizational activity. In addition to the activities already mentioned, he was president of the Society for Applied Anthropology, a society he helped found, in 1947, of the American Ethnological Society in 1952/1953, and of the American Anthropological As-

sociation in 1955. He played a leading role in establishing the Society for Cross-Cultural Research in 1972. He also was influential in bringing linguists and social scientists into the National Academy of Sciences, to which he was elected in 1964, and he was chairman of the division of behavioral sciences of the National Research Council in 1964–1966. Recognition of his many accomplishments came with the Viking fund medal in 1949, the Herbert E. Gregory medal in 1966, the Thomas H. Huxley medal in 1971, and election to the American Academy of Arts and Sciences and the National Academy of Sciences.

Murdock did not found a personal school of anthropology in the name of a particular theory or approach. His achievements and influence were of a different kind. He saw himself as sharing with his colleagues and students in a vast cooperative undertaking to develop a unified science of society, culture, and human behavior. In this endeavor, his role was to apply himself to what seemed to him to be the most urgent immediate tasks for anthropologists.

He often said that he learned as much from his students as he taught them. He encouraged them to follow their own individual interests, the fruits of which were evident in the volume of their papers dedicated to him as *Explorations in Cultural Anthropology* (Goodenough 1964). He reached out among his contemporaries for new perspectives and approaches: in linguistics to Edward Sapir, in psychology to John Dollard, Clark L. Hull, Neal E. Miller, and Earl Zinn, and in anthropology to Fred Eggan, John Gillin, A. Irving Hallowell, Clyde Kluckhohn, and Ralph Linton for meaningful syntheses of cultural, historical, psychological, and sociological approaches. He kept pace with his students and younger colleagues as well, modifying his thinking in the light of their work, and following and contributing to new developments in the study of social organization and to new and more rigorous methods of cross-cultural research, as is evident in a collection of his essays, *Culture and Society* (1965a).

His own continuing intellectual odyssey was dramatically demonstrated in 1971, when, on giving the Huxley memorial lecture in England, he announced a major rethinking of his views about the relationship of the individual, society, and culture (1972). Like many American anthropologists, he had considered the behavior of individuals in society to be explained in significant part by the society's culture. On

this occasion, however, he rejected culture as an explanatory concept and aligned himself with those social and behavioral scientists who see both culture and social structure as explained by processes taking place within individuals as they pursue their interests in interaction with one another.

This renunciation of a particular, long-held theoretical position illustrates his conception of the obligations of science. It does not diminish his singular and enduring contribution, which came from his recognition of the need to organize the vast body of ethnographic data for efficient scientific use. He undertook the enormous task of organizing it, and he demonstrated in his own work the possibilities it afforded for social and behavioral science.

WARD H. GOODENOUGH

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MURPHY, GARDNER

A few scientists achieve fame as theoreticians, such as Isaac Newton; others achieve fame as inventors, such as Thomas Edison; but a rare scientist achieves eminence essentially as a teacher. Gardner Murphy was such a scientist. He taught several generations of psychologists through his brilliant formal lectures and invited addresses; he taught many whom he never met through his books which spanned virtually the whole domain of psychology; and he taught how to do careful, systematic research by doing such research in parapsychology, social psychology, personality dynamics, and cognitive-perceptual functioning.

Here was a man whom colleagues, students, and friends instinctively trusted as he led them on expeditions in the search of knowledge. It did not matter if the knowledge was from Odysseus in the original Greek, Shakespeare, Sigmund Freud, Jean Piaget, William S. Gilbert and Arthur Sullivan, Mohandas K. Gandhi, the comic strips, or research publications. His teaching at various times embraced all of these and more. His students felt his encouragement to explore everywhere and they often found themselves going further than they ever dreamed they would go.

The official facts about the career of Murphy are really only markers or points of achievement or change but do not tell us the most interesting and important "in-betweens." However, they do

provide landmarks which help locate the roots or foundations of Murphy's ideas and ways of relating to others which influenced the development of other psychologists.

Gardner Murphy (1895-1979) was born in Chillicothe, Ohio. He received his A.B. from Yale University in 1916, his A.M. from Harvard University in 1917, and his Ph.D. from Columbia University in 1923. He was a Hodgson fellow at Harvard from 1922 to 1923, and taught at Columbia University from 1921 to 1940. From 1940 to 1952 he was professor and chairman of the psychology department at the City College of New York. He became director of the research department at the Menninger Foundation in Topeka, Kansas in 1952, where he remained until 1967, when he was named visiting professor emeritus at George Washington University in Washington.

Throughout his life, Murphy was an eclectic, a liberal borrower from a variety of sources. Even his birth involved a union of diversity. His father was an Episcopalian minister from Texas and a liberal who founded the National Child Labor Committee. His mother was a New England Yankee and a schoolteacher. He was raised partly in the South but mostly in Concord, Massachusetts, with the maternal side of the family. Murphy was fond of telling stories about his problems of identity. He told about the Boston and Maine Railway that separated the blue collar, Irish, stay-at-home Catholics from the white collar, Anglo-Saxon, commuting Protestants. With a name like "Murphy" he ought to have been on one side of the track, but being Episcopalian he was on the other side. As a consequence, he often claimed that he suffered from an identity problem. But in reality he suffered from a diversity problem. He had a maternal grandfather who often quoted Shakespeare, poetry, or the Bible; he had a grandmother who was warm, gentle, and stoic. The rich profusion of ideas and feelings that surrounded him required a complex integration. It is small wonder that in all the things he did Murphy was an integrator. He could seize upon and integrate everything he encountered, no matter how diverse, to form new connections and unions between would-be islands of knowledge.

The major areas in which Murphy has made contributions in either writing, research, or teaching were the history of psychology, social psychology, personality theory, parapsychology, and education. Although his fields of endeavor

were diverse, he always referred to himself as primarily a social psychologist. He always began with the premise that to understand man one must always perceive man as a social creature, in a social world, with a social history.

Parapsychology. The earliest field of psychology to attract Murphy was parapsychology. His father had planned before his early death to write a book on problems in this area as they related to Christianity, and his maternal grandfather, George A. King, had been the attorney for the psychic medium, Mrs. Piper, whom William James had studied. These family interests stirred the mind of the young Murphy and directed him to a path he would always follow. However, outside of his own readings, he was left unguided until his graduate work at Harvard, where L. T. Troland asked him to be his assistant in research on parapsychology financed by the Richard Hodgson fund. This gave him a chance to immerse himself thoroughly in readings on telepathy. It was natural, then, for him to join the Society for Psychical Research in London while in France with the American Expeditionary Forces during World War I. It was there that he met the great French parapsychologist René Warcollier with whom he maintained a warm and lasting friendship, following his work and visiting him again in 1929. Returning from the war, Murphy went to Columbia University where he remained until 1940. While working on his doctorate at Columbia, Murphy traveled back and forth to Harvard, where William McDougall had arranged for him to have a Richard Hodgson fellowship so that he could continue his research on parapsychology, particularly in the area of long-distance telepathy. His collaborators in this research were Harry Helson and George Estabrooks. This double life, this back and forth travel between the scientifically credible and the outer limits or frontiers of science, continued all his life. Although his own personal research over the years with such people as J. G. Pratt, Ernest Taves, J. L. Woodruff, and especially Gertrude Schmeidler did not yield the consistently significant outcome that he expected, Murphy came more and more firmly to believe that our usual conceptions of space, time, and personal identity have been challenged by parapsychological research, that there are transspatial, transtemporal, and transpersonal relationships that go beyond the usually accepted concepts of space, time, and self. As president of the American

Society for Psychical Research, he initiated and supported a wide range of rigorous research, always demanding that both the spontaneous cases of paranormal experience and the controlled outcome of experiments be examined. He also added to the historical background of the field by editing with Robert O. Ballou (1960) the writings of William James on this controversial area.

Other major areas of long-term interest. Murphy's contributions to psychology followed a crooked path, but all were bound together by certain general assumptions that he summarized in his autobiography (1967, p. 261). These are:

1. "Things are best understood through the study of their origins and evolution."

2. "Psychology is only separable from the biological sciences on the one hand, and the social sciences on the other, through some sort of arbitrary compartmentalization which is likely to do much more harm than good."

3. "If psychology is seriously the study of the whole organism, the whole individual, it is necessarily a study of experience, attitude, immediacy, as well as a study of what is observed from outside."

4. "Behavioral studies are good, and behavioristic beliefs are bad for science."

5. "Inclusiveness, and an accent on the positive, necessitates encouraging many primitive, groping efforts which might sometimes become science, though it will be a long way to get there."

A sixth general assumption that was implicit in Murphy's approach to research and teaching is:

6. "Always treat others with dignity and respect as human beings. You may not agree with another person but listen to what he or she has to say and do not deprecate another's ideas or dreams."

These assumptions are more of a credo for the understanding of psychology than just a set of implicit assumptions. In a remarkable sense they provide an overview of Murphy's contributions. For example, to understand the ways things are we must study their origins and evolution. This idea probably dates back at least to the anthropology course taught by Albert G. Keller, whom Murphy admired, at Yale University. It was fitting that the first book by Murphy was his *Historical Introduction to Modern Psychology* (1929), prepared first as a set of lecture notes for a class in the history of psy-

chology that he began to teach at Columbia in 1923. His eyes failed him badly about this time, and a handful of faithful students and his wife read the manuscript and the revisions to him. In spite of these physical difficulties, Murphy traced the history of psychology from the early Greek and Indian philosophers to the most immediate date possible. Later, he included Edward B. Titchener, George T. Ladd, Robert S. Woodworth, William James, and John B. Watson. In 1972 he added the remarkable advances of Russian psychology as well as up-to-the-minute developments in such broad areas as the psychology of learning; sensory, perceptual, and cognitive functions; comparative, ethological, and physiological psychology; life-span psychology; personality; and social psychology; he even took a tentative look at where the whole area was going in the future. The magnificent sweep of history as the science of psychology unfolds as well as the continuity from early Greek and Indian thought to the most modern developments really sustained Murphy's first credo. One cannot find a finer and more sensitive perspective of where we have been, where we are, and where we are going. In his historical writings he provided exceptional insights with the sequential development of psychological ideas, and all too often he showed how little we have learned from our history.

Of course, origins and evolution also apply to the evolution of the species of man, including the subtle and pervasive factors of social structure and the development of each individual child. This includes a keen awareness of how our cognitive and affective functions resemble those of less complex animal species. Murphy, however, did not hesitate to assert that the human species has capacities and potentialities which are distinctive, species specific, and not to be reduced to the simpler phenomena of apes, rats, and pigeons. This refusal to see things on a reduced time-space scale was natural to Murphy, but it was heavily reinforced and extended by his wife. He met Lois in 1924 through a student of his, Ruth Munroe, and they were married in 1926. She was interested in personality development in children, psychoanalytic theory, education, and comparative religion. Together they discovered and explored the writings of Gandhi, Freud, and others. She sustained those interests Murphy had to begin with, but went on to shape his views on personality functioning by adding a clinical and

developmental dimension to the multifaceted view he had already evolved.

Lois Murphy was, of course, a coauthor of Gardner Murphy's second book—*Experimental Social Psychology* (1931)—which was based largely upon his lecture notes for his class in social psychology at Columbia. It was the first modern, experimentally oriented textbook in this vital area. True to his second rule, that the social and the biological can be separated only arbitrarily and often with great harm, this book managed to incorporate behavioral studies without being behavioristic. It took the lead already pushed by Floyd Allport much earlier. Indeed, Murphy knew Allport from their student days at Harvard, as also in France, and indeed he had substituted as a teacher for Allport's course in social psychology at Syracuse University during the summer of 1927. However, Murphy de-emphasized the behavioral point of view and built up the importance of the individual, his cultural matrix, and his role as a social initiator and responder, ideas later developed more fully by Muzafer Sherif. The institutional notions of Robert S. and Helen M. Lynd's *Middletown* (1929) were incorporated into a fuller view of social psychology, along with personality and cultural differences, often with encouragement from his wife. This book won the Butler medal at Columbia in 1932.

Through his students and his associates Murphy contributed greatly to social psychology. His student, Rensis Likert, developed the Likert method of scaling attitudes, and the resulting research appeared in 1938 as *Public Opinion and the Individual*. Murphy was active in the group (David Krech, Ross Stagner et al.) that developed the Society for the Psychological Study of Social Issues (SPSSI), now division 7 of the American Psychological Association. Murphy was chosen to edit a volume on peace initiatives and problems published as *Human Nature and Enduring Peace* in 1945.

Murphy went to India in 1949 for the United Nations Educational, Scientific, and Cultural Organization to examine the conflict between the Hindus and the Moslems. As usual, this led to a deeper study of Indian religions, as well as the establishment of educational conferences and research groups throughout India. All of this was consolidated in 1953 in the book *In the Minds of Men*. They were the ones who encouraged Erik H. Erikson to go to India, from which emerged Erikson's study of Gandhi. At

the time of his death Murphy was still consulting with people interested in problems of international peace, still hoping to make a small dent in the problem of how to achieve a lasting peace.

Murphy's broad vision was best illustrated by his theory of personality, which began with lectures at Columbia University on abnormal psychology, later published as *An Outline of Abnormal Psychology* (1929). Later this developed into *Approaches to Personality* (Murphy & Jensen 1932). Encouraged by his wife, he explored Freud, Jung, Eugen Bleuler, Charlotte Bühler, and William James, among others, during the 1930s. Developmental notions increasingly impressed him, and he began fusing and extending psychoanalytic notions. This whole line of development eventuated in his *Personality* (1947), and later he included field theory notions and larger possibilities in *Human Potentialities* (1957a) and *Outgrowing Self-deception* (Murphy & Leeds 1975).

Murphy had early rejected what he considered to be naive behavioristic conceptions in favor of a phenomenological approach to personality and social phenomena. As such he was one of the founders of the American school of perceptual-cognitive psychology. In particular he believed that perception was always need-relevant and usually need-directed. This formed the basis for a concept of *autistic* perception, in which contacts with reality were often blunted and confused, and it intertwined cognitive development in children through the canalization of motives via biofeedback systems. Indeed, the importance of feedback, both muscular and visceral, for the direction and stabilization of perception was being stressed by Murphy during the early 1950s, almost a decade before these notions really became fashionable. The early work on autistic perception was carried out by senior honors students at City College. Students such as Robert LeVine, Harold Proshansky, Roy Schafer, Jerome Levine, and Leo Postman did much of the data gathering, and (as usual), Gardner gave them most of the credit. Julian Hochberg (Murphy & Hochberg 1951) helped delimit the early theory of autistic perception and postdoctoral students, such as Douglas N. Jackson and Samuel Messick, at the Menninger Foundation, helped to refine the research and theory which was consolidated in *Development of the Perceptual World* (Solley & Murphy 1960), a major attempt to deal system-

atically with perceptual learning and development.

The lecture skills of Gardner Murphy were legendary. He credited much of that skill to his sophomore English teacher and debate coach at Yale, John Chester Adams. Year after year the graduating seniors at City College of New York voted Murphy the most outstanding teacher in their experience. Most of his contributions to writing and research began as a part of his teaching, sometimes as formal lectures but most frequently with bits and pieces of casual conversation that inspired and directed the listener. This capacity to excite, to direct, and to make the listener feel important has been amply described by Eugene Hartley in the *Festschrift* volume prepared for Murphy in 1960 (Peatman & Hartley 1960). Indeed, in a survey of psychologists by Kenneth E. Clark (1957), Murphy was ranked second only to Sigmund Freud in terms of the number of persons whom they inspired to go into psychology. The number of his students who went on to receive a PH.D. and who achieved eminence in psychology is remarkable. His classes were means of exploring, questioning, expanding, trimming, and solidifying through his students. However, Murphy would say that he was always the one who learned through such encounters. He continually insisted that his students, his colleagues, and his friends were his teachers.

He was honored by being elected president of the American Psychological Association from 1943 to 1944. He was also given the gold medal by that association in 1973 for outstanding contributions to the development of theory and research in psychology.

CHARLES M. SOLLEY

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MURPHY, LOIS B.

Lois Barclay Murphy was born in 1902 to well-educated parents who provided an intellectually and culturally stimulating home environment for their five children and who expected high levels of achievement from them. Early visits with her minister-father to slum areas in Chicago created in her an awareness that not all children were as fortunate as she and her siblings, and helped generate in her a lifelong concern for the welfare of underprivileged children. Similarly, the early assumption of respon-

sibility for the younger children in the family due to progressive illness in her mother helped Murphy acquire skills in child care that would prove invaluable in her later scientific and professional work.

The stimulation she found at home, supplemented by contact with a number of stimulating secondary teachers, caused her at first to find college life at Vassar disappointing. She found stultifying some of her early contact with women professors who seemed to model the attitude that cognitive achievement necessitated abandonment of all warmth and intuition and sensitivity to people. The ideas of John B. Watson dominated the thinking of the psychology faculty, and Murphy, with a background that involved dinner-table discussions of William James and John Dewey, found this approach sterile and uninspiring. Her interests were still broad at this time, as they have remained throughout her life, and she majored in economics and only minored in psychology at Vassar. A course in comparative religion at Vassar provided the most powerful stimulus for thought and involvement and motivated her to do graduate work in this area at Union Theological Seminary, where she specialized in the religions of India. This study undoubtedly provided the kind of philosophical background that later enabled her to coauthor with Gardner Murphy a book on the psychology of the East.

In reviewing Murphy's undergraduate career, the impression is of a student who provided maximum challenge to professors; in the balance, it might be difficult to determine whether she gained as much from the experience as she contributed to it. It was a time for the maturation and ripening of her own intellectual powers and one of strengthening the convictions about life and human development which had developed during her earlier formative years and which were to influence countless students and colleagues in later years.

Upon her graduation from Vassar in 1923, Murphy was "avid about getting experience" and briefly did some volunteer work in the child guidance service of the Board of Education of Cincinnati which quickly turned into a full-time job. However, in the fall of 1924 she returned to New York and enrolled as a student of comparative religion at Union Theological Seminary, at the same time taking courses in psychology at Columbia Teachers College and planning to get an M.A. along with her degree from Union Sem-

inary. However, as her prior level of sophistication again made some of the courses seem naive, she abandoned the psychology project. One of her associates whose own courses were far from naive, was the brilliant and compassionate Gardner Murphy, who was to become her husband in 1926. In a masterpiece of understatement, she described their 54-year union by saying: "It was an extraordinarily congenial marriage." From the time of the birth of their son in 1930 until the Murphys joined the staff of the Menninger Foundation in 1952, Lois Murphy showed how important her family was to her by working only part time. This fact makes her productivity over the years even more impressive.

The real launching of Lois Murphy's career began in 1928 when she became affiliated with the fledgling Sarah Lawrence College. She began her work there by teaching comparative religion but gradually spent more and more time training the undergraduate students in how to study young children and how to understand their development. In 1937 she helped launch the Nursery School at Sarah Lawrence and continued her involvement with it until 1952. At that time Lois and Gardner Murphy moved to the Menninger Clinic in Topeka, Kansas, where they worked until 1970. Since that time Lois has continued analyzing data collected at Menninger's and has served as consultant to a host of national and international projects concerned with young children.

Throughout her career, Lois Murphy's interests have not wandered far afield from the early study of sympathy which she began at Sarah Lawrence during the early 1930s. That is, she has remained close to the socio-affective domain and has been concerned with positive rather than with negative behavior—how children develop and utilize their own strengths in adapting to challenge and conflict, how they interact with significant others in their intimate and distal interpersonal environments, how they reveal their adaptive vulnerability and their resilience, how they react to different patterns of early child care, how they can be helped to regain competency when something goes wrong, and so on. To describe this process, Lois introduced a down-to-earth, nonjargonistic word to the vocabulary of psychology: "coping." The term "coping" was not used as an index term in *Psychological Abstracts* at the time Murphy began using it. But it was an excellent choice, for she was interested

in the *process*, not in some outcome such as adaptation or maladjustment. Use of the term coping increases "awareness of the individuality, spontaneity, even creativity characterizing the new patternings of response we see, as well as the gallant persistence and repetitive efforts which are often necessary in the struggle toward mastery" (Murphy et al. 1962, p. 7).

At least three scientific strictures are mandated by Murphy's approach: (1) longitudinal study, (2) observational methodology, and (3) concern with individuality. How can one observe effective coping if only a narrow slice of a child's life is taken? If only a short-term response to any sort of crisis is assessed, how can correct judgments of resilience and invulnerability be made. According to Murphy, we take the assumption of continuity of effect too much for granted and do not allow for the mobilization of internal strengths which can ensure adequate coping with experience to become manifest. Only sustained follow-up of children into subsequent developmental periods will allow an adequate assessment of potential coping strength to be made. Murphy remarked that in both our research and in our daily lives we see both children and adults whose resilience makes some of our predictions of doom appear ridiculous.

Her commitment to observation in relevant life settings has also been consistent throughout her professional life, and it appears that she seldom gets the credit due her on this issue. In the early book on *Experimental Social Psychology* ([1931] 1937), that she coauthored with Gardner Murphy and Theodore Newcomb, there is a long section on observational methods that can be used to assess the behavior of young children. The same theme is carried through her lengthy two-volume series, *Personality in Young Children* (1956). If Murphy has any intolerance which has found its way into her writings, it is of scientists who rely on narrow experimental procedures without adequately observing the domain in which their experiments belong. Thus, although she never used the term to my knowledge, Lois Murphy was advocating "ecological relevance" for psychological experiments long before that term became fashionable.

A third overriding concern that appears throughout Murphy's work involves appreciation of and respect for individuality. Recognizing that there are not only individual differences in internal coping capacities and styles but also

in terms of the patterns of environmental demands likely to be encountered during the process of development, she consistently stressed the need for adults to be aware of and to respect these differences. When child care outside the home became an important political issue in the United States, Murphy had some concerns that large-scale efforts to develop programs would of necessity overlook this need for individualization of care. In a paper prepared for people who were attempting to design infant day care programs, she urged programmatic caution based on this need for an idiographic approach:

Whether we are talking about very depriving group care for babies or about some of the better examples, we still find that there is a need for a far more systematic and sensitive assessment of the needs of individual babies as a basis for planning the care, the environment, the materials, and the opportunities which would meet the needs of the specific baby and make possible a more adequate realization of the potentialities of each one. For every baby there is a level of both quality and quantity of stimulation which evokes a response that leads to interaction with adults or other children and thus to integrative development, communication, and active experiencing and mastery of the environment. The level must be ascertained for each baby if we are to protect him from personality disturbances. (Murphy 1968, pp.122-123)

To these three scientific strictures—longitudinal work, observational methodology, individualization—one would have to add a fourth major theme of her work—the celebration of the positive characteristics within the individual. Her psychology has always been one of searching for and paying tribute to strengths rather than always being blinded by evidence of psychological weakness and experiential trauma. And yet it is not a Panglossian psychology which ignores signs of vulnerability within the organism or of potential devastation by the environment. Rather the coping process is one through which compromises are achieved—but always according to the pattern which characterizes each individual. The scientist who is comfortable only when human beings can be reduced to a set of figures, whose only way to work is to test hypotheses derived from other hypotheses, and who moves relentlessly toward the derivation of nomothetic laws of behavior will not be comfortable within the framework created by her approach to psychology. On the other hand, the scientist who has time to take time as a variable,

who is committed to the necessity of obtaining knowledge in real-life environmental settings, and for whom developmental successes communicate as much information about laws of behavior as do dysfunctions, will consider her a theorist of major proportion.

As an epilogue, it should be noted that Murphy has moved for fifty years in a circle of intellectually exciting individuals. She and her husband related equally well to experimental psychologists, psychoanalysts, nursery educators, students, and persons from the full array of the humanities. And through all the intellectual and social excitement, they appeared to relate so well to each other and to learn so much from one another. Murphy thus appears to be one of the fortunate persons for whom personal and professional satisfaction came together through a lifelong experience of being cared about and caring for others in such a way that a keen intelligence developed within a context of strong social concern. Her life and her work seem to comprise an integrated package, undoubtedly because Lois Murphy is a truly integrated person.

BETTYE M. CALDWELL

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MURRAY, HENRY A.

Henry A. Murray is a major twentieth-century contributor to the theory and empirical study of human personality. His varied background in medicine, biological science, literature, psychoanalysis, and academic psychology has provided an appropriate base for his contributions to the broad and encompassing field of personality psychology. Murray is clearly associated with a general approach to the study of personality, as well as with the development of specific instruments for measuring aspects of personality, particularly the Thematic Apperception Test. His theoretical conceptions have changed substantially over a period of more than thirty years, but they have generally reflected a conviction that the individual personality should be studied and represented in its full complexity. He introduced the term "personology" to refer to the work of those who focus on the study of the individual case and also to minimize the tendency to develop a cult or movement associated with a particular individual, as in the case of Freudianism. He has also specified a wide variety of variables or dimensions for describing the individual and his or her significant environment.

Born in New York City in 1893, Murray was educated at the Groton School and at Harvard College. Subsequently, he received medical training at Columbia University, worked briefly as an instructor in physiology at Harvard University, and then served a two-year surgical internship at Columbia Presbyterian Hospital in New York City. He next joined the staff of the Rockefeller Institute for Medical Research in New York City, where he conducted embryological research for two years. Then came a period of study at Cambridge University where he received his PH.D. in biochemistry in 1927. During this interval of European study, his interests first turned definitely toward psychology.

Even in his medical experience and biological research, Murray showed an unusual interest in the details of human experience, in the personality characteristics of patients, and in the complex interaction between the physical-biological and the psychosocial. Murray read with fascination Carl Jung's *Psychological Types* (1921) and found himself drawn to the mysteries of the unconscious. He became convinced that Jung's work provided a powerful means of gaining insight into the basic determinants of human behavior. While studying at Cambridge

he visited Zurich for what turned out to be a fateful encounter with Jung—this experience was the turning point in Murray's transition from biomedicine to the behavioral sciences.

Murray returned to the United States committed to psychology, but spent another year at the Rockefeller Institute before taking the step that would determine the rest of his career. In spite of his unorthodox background, Murray had influential admirers at Harvard who were convinced that regardless of his training, he would succeed at whatever he set his mind to. He thus came to Harvard University as an instructor in psychology, joining a distinguished traditional department at just the time that Morton Prince had founded the Harvard Psychological Clinic. The clinic was endowed for the purpose of studying and teaching abnormal and dynamic psychology, and Prince, searching for a young and promising scholar to guide the future of the clinic, clearly saw Murray as filling these qualifications. In 1928 Murray was appointed director of the Psychological Clinic, and in 1937 he was promoted to associate professor. He was one of the charter members of the Boston Psychoanalytic Society and by 1935 had completed his training in psychoanalysis under Franz Alexander and Hans Sachs. A detailed and revealing account of his training analysis and his attitudes toward psychoanalysis is contained in a symposium concerning psychologists and psychoanalysis (Murray 1940).

During the roughly 15 years before World War II interrupted, the Harvard Psychological Clinic, under Murray's personal and intellectual leadership, was the scene of an exciting theoretical and empirical enterprise. Murray gathered about him a group of able scholars, young and not so young, whose joint efforts to formulate and investigate the human personality had enormous impact upon the future of the field. *Explorations in Personality* (Murray et al. 1938) contains an incomplete record of the creativity of this era, but the most important effects were carried away in the form of convictions, questions, conceptions, and plans by individual participants. For the first time psychoanalytic theory was given a serious academic audience, and earnest efforts were made to find ways of translating the brilliant clinical insights of Freud, Jung, and other psychoanalysts into experimental operations that would permit some degree of empirical confirmation or rejection. Not only did Murray create a sense of excitement and imminent discovery among his own

students, but the clinic opened its doors to mature scholars from a variety of fields, such as Erik H. Erikson, Cora DuBois, Walter Dyk, and H. Scudder McKeel, so that the enterprise had a marked interdisciplinary flavor.

In 1943 this period ended as Murray left Harvard to join the Army Medical Corps. There, as a major and subsequently, a lieutenant colonel, he established and directed an assessment service for the Office of Strategic Services. He and his colleagues were assigned the difficult task of screening candidates for secret and dangerous missions by evaluating their emotional stability, capacity to withstand stress, and interpersonal skills. The activities of this group have been summarized in *Assessment of Men* (Office . . . 1948). For his work with the army, he was awarded the legion of merit.

In 1947 he returned to Harvard on a part-time basis as a lecturer on clinical psychology in the newly formed department of social relations. There is no doubt that his intellectual and personal ties with distinguished Harvard anthropologists and sociologists played a significant role in legitimizing this interdisciplinary venture. In 1950, he was appointed professor of clinical psychology. He declined to resume direction of the Psychological Clinic, and in 1949 established the Psychological Clinic Annex at Harvard University, where he and several colleagues and graduate students conducted further studies of personality until his retirement in 1962. Since then, Murray has continued to lead an active scholarly life. Among many other awards, Murray has received the distinguished scientific contribution award of the American Psychological Association and the gold medal award of the American Psychological Foundation for a lifetime of contribution to the discipline.

The number and diversity of theoretical models to which Murray has been exposed are so great that one can do no more than select a few of the most prominent. Clearly psychoanalysis, in the broadest sense of the term, has had great influence on his intellectual development. In a direct and personal sense, Jung, Alexander, and Sachs have all influenced him, and Freud, primarily through his writing, had great impact. The depth of the influence of psychoanalysis upon Murray's view of behavior is shown clearly not only in his own work but in that of many students and collaborators, such as R. Nevitt Sanford, Saul Rosenzweig, Robert W. White, Silvan S. Tomkins, Erik H. Erikson,

Robert R. Holt, and Gardner Lindzey, all of whom, together with their students, have conducted empirical studies relevant to psychoanalysis.

Murray's medical and biological research and training have contributed to the deep respect he has consistently shown for the importance of the physical and biological substrate. His experience in medical diagnosis has resulted in his belief that personality should ideally be assessed by a team of specialists and that in this assessment the subject's statements about himself should be given serious audience. His interest in problems of taxonomy or classification, as well as his conviction that the careful study of individual cases is essential to future psychological progress, is also highly congruent with his medical background.

His thorough knowledge of contemporary and classical literature, and particularly his expert knowledge of Herman Melville and his works, has provided a rich source of ideas about man and his potential for good and evil. In Alfred North Whitehead, he found a model of logical and synthetic thought, while the truculent, but brilliant, Lawrence J. Henderson served as a model of rigor and critical orientation. His thinking has absorbed much from academic psychologists, including Kurt Lewin and William McDougall, from his long-time friend and colleague, Gordon W. Allport, also a major contributor to the field of personality who served for many decades as a source of stimulation and friendly disagreement, and from Clyde Kluckhohn in cultural anthropology. From such a complex lineage it is no wonder that the evolved product is luxuriant, elaborate, and ever changing. His debt to the scholars identified above and to numerous others, including several generations of students, is amply acknowledged in three very personal documents (1940; 1959; 1967) and in a volume of essays written in honor of Murray (White 1963).

It is clear to all who have known him that Murray's talent and devotion to the study of the human personality are only partially revealed in his published works. His casual remarks and free-ranging speculations on an endless variety of topics have provided fruitful research ideas for his students and colleagues. Unfortunately not all these messages have fallen on fertile ground, and one regrets that the spoken word has not been preserved to enrich the written record. Murray's tendency to publish only occasional fruits of his intellect is demonstrated

in the few publications that have stemmed from his years of intensive study of Melville. These years of dedicated scholarship have earned him an unparalleled reputation among students of Melville, yet he has thus far published but two papers dealing with this engrossing topic: a brilliant analysis of the psychological meaning of *Moby Dick* (Murray 1951a) and an introduction to and penetrating analysis of *Pierre* (Murray 1949), one of Melville's most baffling novels.

Murray's analysis of *Moby Dick* rests upon his sophistication in psychological theory, particularly psychoanalysis, but also derives from his detailed knowledge of Melville's life and major novel. It is difficult to summarize a complex psychological analysis of an intricate literary product, particularly when much of the persuasiveness of Murray's piece is associated with his rich and powerful writing style.

In starkly oversimplified terms, Murray examines, then rejects, the thesis that there may be no psychological meaning to *Moby Dick*. Much of his paper presents evidence to support the hypothesis that Captain Ahab represents the devil and his forces of evil—hence in psychological terms, the primitive and uncontrolled forces of the id. In contrast, the white whale, Moby Dick, represents the superego—the moral and restraining forces within the individual and also the social conventions and sanctions that are a part of Melville's society.

Granted the inadequacy of the written record, Murray's psychological theorizing and research are best represented in *Explorations in Personality* (Murray et al. 1938), which summarizes the thought and research of the Psychological Clinic staff at the end of its first decade of existence. A partial record of Murray's subsequent research is contained in *A Clinical Study of Sentiments* (1945), which Murray wrote with his long-time collaborator Christiana Morgan. The major changes in his theoretical convictions undergone in subsequent years are best represented in a chapter written jointly with Clyde Kluckhohn (1948), a chapter published in *Towards a General Theory of Action* (1951c), an article published in *Dialectica* (1951b), a chapter written for *Psychology: A Study of a Science* (1959), and an article written for the *International Encyclopedia of the Social Sciences* (1968). The *Thematic Apperception Test Manual* (1943) serves as the best introduction to this personality instrument, devised jointly with Morgan (Morgan & Murray 1935), which has become one of the most important

and widely used empirical tools of the clinician and personality investigator. The great sensitivity and ingenuity which Murray has shown in developing means of appraising and analyzing man's capacities and directional tendencies are vividly revealed in *Assessment of Men* (Office . . . 1948).

Methods of personality assessment. Murray has been actively involved in the creation of many different instruments for measuring or assessing personality. A number of publications, particularly *Explorations in Personality*, provide partial or detailed accounts of a wide variety of methods for studying personality under more or less standardized conditions.

Undoubtedly the best known of these devices is the *Thematic Apperception Test* (TAT), which together with the Rorschach technique dominates the field of projective testing. This instrument, when used conventionally, involves presenting to the subject twenty pictures, each of which is open to many interpretations, with the request that the subject incorporate the picture into a story of his or her own creation. The stories produced by the subject can then be interpreted in a variety of ways, although traditionally the emphasis has been placed upon the potential of the stories for revealing aspects of the individual of which he or she is unaware. One may view the test, when used to reveal unconscious or covert aspects of personality, as providing a substitute for or supplement to dreams used in traditional psychoanalysis. Murray was fully aware of the fact that the TAT also revealed overt or known aspects of the individual, so that the conscious and unconscious were intricately blended.

Many other psychologists have devised scoring systems for arriving at quantitative scores from TAT stories for particular variables, such as achievement, affiliation, dominance, and sexuality (Atkinson 1958). Indeed, a vast literature on achievement motivation, its origins, correlates, and consequences, is based upon Murray's initial conceptions and methods.

The test has been used in hundreds of empirical studies of human personality as well as in clinical settings for arriving at a psychodiagnosis or an understanding of the individual personality. The instrument is used not only in situations focusing on the diagnosis and treatment of a subject with impaired capacity, but also in assessment settings where the goal is to predict the capacity of the individual to function satisfactorily in industrial or military roles.

Human motivation. At the heart of Murray's psychology lies a deep interest in conceptualizing and measuring motives. The many personality instruments he devised should be seen primarily as an attempt to provide a means of assessing the motivational variables he considers essential determinants of human behavior. Though not alone in his interest in motivation, and deeply indebted to Freud and Jung for his emphasis on covert as well as overt motivation, he is distinctive in the detailed identification of what he views as key motives and in his attempts to provide careful definitions, methods of measuring concepts and relating them to one another as well as to the environmental context in which they operate.

Although his theoretical conceptions have undergone many changes, the concept of "need" is generally regarded as his primary motivational concept, and his careful enumerations and definitions in *Explorations in Personality* are probably the most influential of any of his writings. In this work he identifies twenty principal needs, several of which are listed here with abbreviated definitions: *abasement* (To submit passively to external force. To accept injury, blame, criticism, punishment. To surrender. To become resigned to fate. To admit inferiority, error, wrongdoing, or defeat. To confess and atone. To blame, belittle, or mutilate the self. To seek and enjoy pain, punishment, illness, and misfortune.); *achievement* (To accomplish something difficult. To master, manipulate, or organize physical objects, human beings, or ideas. To do this as rapidly and as independently as possible. To overcome obstacles and attain a high standard. To excel oneself. To rival and surpass others. To increase self-regard by the successful exercise of talent.); *affiliation* (To draw near and enjoyably cooperate or reciprocate with an allied other [an other who resembles the subject or who likes the subject]. To please and win affection of a cathected object. To adhere and remain loyal to a friend.); *aggression* (To overcome opposition forcefully. To fight. To revenge an injury. To attack, injure, or kill another. To oppose forcefully or punish another.); *autonomy* (To get free, shake off restraint, break out of confinement. To resist coercion and restriction. To avoid or quit activities prescribed by domineering authorities. To be independent and free to act according to impulse. To be unattached, irresponsible. To defy convention.); *dominance* (To control one's human environment. To influence or direct the

behavior of others by suggestion, seduction, persuasion, or command. To dissuade, restrain, or prohibit.); *nurturance* (To give sympathy and gratify the needs of a helpless object: an infant or any object that is weak, disabled, tired, inexperienced, infirm, defeated, humiliated, lonely, dejected, sick, mentally confused. To assist an object in danger. To feed, help, support, console, protect, comfort, nurse, heal.); *succorance* (To have one's needs gratified by the sympathetic aid of an allied object. To be nursed, supported, sustained, surrounded, protected, loved, advised, guided, indulged, forgiven, consoled. To remain close to a devoted protector. To always have a supporter.).

In addition to identifying these major needs, Murray also subdivided them in many ways, distinguishing covert and overt needs, viscerogenic as against psychogenic needs, proactive and reactive needs, and focal and diffuse needs.

Although Murray's concept of "need" has many parallels in the writings of other psychologists, including such familiar terms as trait, instinct, motive, and disposition, his complementary concept of "press" lacks comparable similarity to the concepts of other psychologists. Need is a dispositional concept that represents the motivational origins of behavior, whereas press refers to the significant aspects of the environment as they influence or determine behavior. Thus, the two concepts in combination provide a systematic representation of both the psychological-biological and the environmental determinants of behavior. Press concepts represent persons or objects with real or potential implications for the satisfaction of needs. In Murray's words, "the press of an object is what it can *do to the subject* or *for the subject*—the power that it has to affect the well-being of the subject in one way or another" (Murray et al. 1938, p. 121).

Although many psychologists have stressed the importance of recognizing and representing the perceived environment within which humans function, few have gone beyond exhortation or naturalistic description. Murray's emphasis and concepts are important forerunners of what is sometimes referred to as environmental psychology or psychological ecology. Illustrative of Murray's press concepts are: family discord; capricious discipline; parental separation; absence of either parent; parental illness; inferiority of either parent; dissimilar parent; poverty; unsettled home; physical in-support, height; water; aloneness, darkness; in-

clement weather, lightning; fire; accident; lack or loss of nourishment, possessions, companionship, or variety; nurturance, indulgence; succorance, demands for tenderness; deference, praise, recognition; and affiliation, friendships.

Discussions of the relative importance of traits or other enduring dispositions, as opposed to the importance of situational determinants of behavior, are leading to a general acceptance of an "interactionist" position. This position implies that the significance of a trait or disposition must always be assessed in conjunction with the particular set of situational or environmental factors that operate. As the above discussion implies, Murray was precise in specifying the interaction of needs and press and thus of "interactionism," suggesting more than three decades ago, a position that is currently fashionable.

Study of the individual. Murray is a staunch defender of an approach to the study of human behavior that is based on the detailed study of a small number of individual cases rather than on a casual or focused approach to large numbers of subjects. He believes that more is to be learned through intensive, long-term examination of individual lives from multiple perspectives than through delimited and objective study of many subjects.

His concern for the careful, longitudinal study of individual cases is congruent with Allport's emphasis upon the life history. His distrust of findings that describe group results but do not necessarily describe accurately the behavior of any single individual is consonant with B. F. Skinner's emphasis upon reporting results for individual subjects rather than group findings.

Not only did Murray believe in studying individuals, but he also thought more could be learned from studying normal as opposed to abnormal behavior. Maintaining that the individual should be studied by more than one observer, he has used the term "diagnostic council" to refer to a procedure in which a number of skilled observers study a single individual with different methods and perspectives, ultimately synthesizing and integrating their diverse findings into an over-all picture of the person. As a consequence, Murray's research has involved small numbers of subjects, who are studied intensively for years, with the use of multiple observers. The results of these inquiries often appear in the form of detailed life

histories. Some of the best examples of this approach are "American Icarus" (1955) and a series of life histories published with Christiana Morgan under the title, "A Clinical Study of Sentiments" (1945).

Murray's importance to the field of psychology seems assured so long as interest continues in human motives and their individual differences. Four decades after its publication, *Explorations in Personality* is still one of the most influential volumes in the field of personality and there is no reason to expect this to change in the next four decades.

GARDNER LINDZEY

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MYRDAL, GUNNAR

Gunnar Myrdal was born in 1898 in the village Solvarbo, in the parish of Gustafs, in the Swedish province of Dalarna. He attributes his faith in the Puritan work ethic and his egalitarianism to his sturdy farming background.

His early interests were in the natural sciences. However, at Stockholm University he started to read law in the belief that he would learn about how society functioned, but was disappointed and soon changed to economics, the method of which was more like that of the natural sciences. He was a student of the giant figures Knut Wicksell, David Davidson, Eli F. Heckscher, Gösta Bagge, and above all (Karl) Gustav Cassel. His personal friendship was warmest with Cassel, to whose chair in political economy and financial sciences at Stockholm University he succeeded.

At first a pure theorist, Myrdal's year in the United States as a Rockefeller fellow, following the crash of 1929, turned his interest to political issues. On his return from America he, together with his wife Alva, a pioneering emancipator of women and in many ventures a partner of her husband, became active in politics. Labor came to power in Sweden in 1932. He was involved in the work of a number of Royal commissions and public committees, and in 1935, became a member of Parliament. Together with his wife he pioneered modern population policy. His work in Sweden between 1931 and 1938 turned him from a "theoretical" economist into a political economist and what he himself described as an institutionalist. In 1938 the Carnegie Corporation selected him for a major investigation of the black problem in America, a project that resulted in *An American Dilemma*

(1944a). He returned to Sweden in 1942 and for five years was involved in political activities. He headed the committee that drafted the Social Democratic postwar program. He returned to Parliament, and became a member of the board of directors of the Swedish Bank, chairman of the Swedish Planning Commission, and Minister for Trade and Commerce (1945–1947). As minister he arranged for a highly controversial treaty with the Soviet Union and was also involved in controversy over the dismantling of wartime controls. In 1947 he became executive secretary of the United Nations Economic Commission for Europe, to which he recruited an outstandingly able team of colleagues and where he inspired young people to give their best. After ten years with the commission in Geneva he embarked on a ten-year study of development in Asia, the result of which was the monumental *Asian Drama* (1968).

Methodological questions occupied Myrdal's thoughts throughout his life. They were already present in the young Myrdal's iconoclastic *The Political Element in the Development of Economic Theory* (1930). It was under the influence of the remarkable Uppsala University philosopher Axel Hägerström that he had begun to question the economic establishment.

Myrdal's doctoral dissertation on price formation and economic change, *Prisbildningsproblemet och föränderligheten* ("Price Formation and Economic Change"; 1927), systematically introduced expectations into the analysis of prices, profits, and changes in capital values. The microeconomic analysis focused on planning by the firm. Many of these ideas were used in his later microeconomic work, such as *Monetary Equilibrium* (1931).

Much confusion had been caused by the lack of distinction between anticipations and results. The concepts *ex ante* and *ex post* greatly clarified the discussion of savings, investment, and income, and their effects on prices. In anticipation, intention, and planning, savings can diverge from investment; after the event they must be identical, because the community can save only by accumulating real assets. It is the process by which anticipations *ex ante* are adjusted so as to bring about the bookkeeping identify *ex post* that explains unexpected gains and losses as well as fluctuations in prices. Only in equilibrium are *ex ante* savings equal to *ex ante* investment, so that there is no tendency for prices to change. Myrdal saw the chief con-

tribution of this book as the formulation of this distinction.

By introducing expectations into the analysis of economic processes, Myrdal made a major contribution to liberalizing economics from a static theory in which the future is like the past or "other things remain equal," and to paving the way for dynamics, in which time, uncertainty, and expectations enter in an essential way.

What is common to his subsequent three important books, *The Political Element*, *An American Dilemma*, and *Asian Drama* is the emphasis on realistic and relevant research, whether on economic problems, race relations, or world poverty, and with it, the effort to purge economic thinking of systematic biases.

The work for *An American Dilemma* was done for the Carnegie Corporation of New York, at the invitation of its trustees. They had turned to Myrdal as a student from "a non-imperialist country with no background of discrimination of one race against another." They requested that he produce "a comprehensive study of the Negro in the United States to be undertaken in a wholly objective way as a social phenomenon."

Starting on the study almost completely ignorant, he soon discovered that, in order to understand the black people in the United States, he had to study "the American civilization in its entirety, though viewed in its implications for the most disadvantaged population group" (introduction to section 4 of 1944). The way to reach objectivity was to state explicitly the value premises of the study. These premises were not chosen arbitrarily, but were what Myrdal called the "American Creed" of justice, liberty, and equality of opportunity. But while these value premises were chosen for their relevance to American society, they corresponded to Myrdal's own valuations. As a result, he became closely identified with America's ideals and the study turned into a deep personal commitment. Indeed, he came to regard it as his war service. And the war gave additional importance to race relations as a source of national concern. The book, published before the end of the war, attempts to present a comprehensive, well-documented account, and an intensive scientific analysis of the facts and the causal relations between facts at the end of the 1930s and early 1940s, with the discernible trend of future changes. The major contribution of the book is the analysis of the more than six decades after Reconstruction as "a temporary interregnum,"

not a "stable equilibrium," and of the incipient changes on which the prediction of the black revolt in the South was based.

Myrdal has never been easy to typecast. On many issues, he fires at both sides of the conventional barricade and likes to emphasize the false shared premises of the combatants. Thus in the discussion of the role of the purely economic factors in development, to the exclusion of cultural, social, political, and psychological, he criticizes liberal and conservative economics for assuming the noneconomic factors to be fully adapted to economic progress, and therefore bundled away under *ceteris paribus* clauses, and the Marxists for believing that these factors are responsive and automatically adaptable, as a result of changes in what Marxists call the economic substructure, and therefore also beyond analysis and policy. From diametrically opposed premises, liberals—conservatives and Marxists—revolutionaries therefore arrive at the same conclusion: there is no need for direct action on noneconomic variables (administration, educational systems, labor markets), for in the one case they are fully suited for the required change and in the other they will inevitably and automatically be shaped by the underlying economic change. Liberals and revolutionaries share common ground, which prevents them from seeing the need for the conscious planning of institutions.

While highly critical of the results of a free market system, Myrdal is not among those who dismiss economic progress as irrelevant or detrimental to human life. In his view, economic advance is a necessary condition for achieving social ends and he disagrees with those conservationists who believe that a better quality of life is possible only by abandoning economic growth.

The options before us, in Myrdal's view, are not confined to the models of capitalist or Soviet development. Social objectives can be pursued by a system of decentralized decision making in which planning is combined with freedom. But here again, the added option is not one of the apolitical possibilities of the futurologists or science fiction writers, but is anchored in political feasibility. He stands nearest to the so-called "utopian" socialists, whom Marx and Engels contemptuously dismissed as "unscientific," but who paid careful attention to shaping social institutions and even human attitudes for a better society. According to the Marxists such planning was impossible or unnecessary: impossible

before the revolution, because they formed part of the superstructure determined by economic conditions; unnecessary after the revolution, when human attitudes and social institutions would be automatically adapted to the socialist society.

Apart from his work on racial problems, Myrdal is best known for his critique of conventional economic theory applied to underdeveloped countries. He calls for a reconstruction of such theory. First, we must free ourselves from the limitation imposed on our thinking by Eurocentricity. Many of our concepts, models, theories, paradigms are "Western" (and "Western" for this purpose includes Marxist and Soviet) in the sense that they fit, more or less, the reality of advanced industrial societies, but are quite "inadequate to the reality" of underdeveloped societies. (It is interesting to note that Myrdal's critique of the application of "Western" concepts to poor countries has caused a reappraisal of the applicability of these concepts to Western societies themselves. In development studies, as in history, there are bonuses.) Both the concepts singled out as strategic variables, like "capital," and the issues ignored, like "corruption," reject the experience of "Western" societies and are opportunistically motivated schemas.

Myrdal's appeal for realism, or as he calls it, "adequacy to reality," is not primarily a critique of abstraction or selection or simplification. As a most sophisticated social scientist, he is, of course, not only aware but also insists that all theorizing must abstract and select. His criticism is that the abstractions follow the wrong lines, that the irrelevant features are selected. It is pouring out the baby *instead of* the bath water. He subjects the commonly used concepts of employment, unemployment, income, consumption, savings, investment, capital, output, capital/output ratio, etc., to close scrutiny and finds that in large measure they dissolve when applied to underdeveloped societies.

Having dissolved them, the question arises whether they can be reassembled: whether capital, for example, cannot be given a new and wider meaning, including investment in forms conventionally accounted as consumption, or investment in a program of family planning or in a land reform; and whether employment cannot be replaced by a richer and more realistic concept of "labor utilization." This would allow for the different attitudes of different castes toward work; it would allow for the effects of the components of the level of living, like nutrition

and health, on intensity of effort; and it would allow for the different types of labor markets. Myrdal has been less successful in this reformulation and reconstruction than in his critique of existing concepts. His critique is often not accompanied by the presentation of useful alternatives. He has often remarked that "facts kick" against the hard crust of established models or paradigms. But the powerful hold of these paradigms, and the need to demolish established paradigms by providing alternative ones rather than simply by pointing to facts inconsistent with them, may explain why Myrdal's critique has not been more widely accepted in the profession.

A second line of Myrdal's criticism has been directed at the narrow definition of development as economic growth. He replaces it by the concept of the modernization ideals. The emphasis here is again on actual needs and valuations (much more than narrow interests) of real people and groups of people. The ideas must be relevant to the actual valuations of men and women and not the created abstractions of philosophers, statisticians, and economists. The United Nations accepted this approach in 1969 as the integrated or unified strategy of development, and the United Nations Research Institute for Social Development, on the governing council of which Myrdal sat, was entrusted with further research on it. The social indicator movement has also derived strength from it.

His third criticism is directed at the narrow definitions and limits of disciplines. The essence of the institutional approach, advocated by Myrdal, is to bring to bear all relevant knowledge and techniques on the analysis of a problem. According to this institutional approach "history and politics, theories and ideologies, economic structures and levels, social stratification, agriculture and industry, population developments, health and education and so on must be studied not in isolation but in their mutual relationships" (1968, vol. 1, p. x). In an interdependent social system, there are no economic problems, political problems, or social problems, there are only *problems*.

His fourth line of criticism is directed at phony objectivity, which under the pretense of scientific analysis conceals political valuations and interest. Myrdal argues that this pseudoscience should be replaced by explicit valuations, in the light of which analysis can be conducted and policies advocated. He is not so naive as to believe that simple specification of these value

premises is easy or even possible at all, and has shown how complicated and complex the nexus between valuations and facts can be. But he has constantly fought the inheritance of natural law and utilitarianism, according to which we can derive certain recommendations from pure, theoretical analysis. "The greatest good for the greatest number" or the "maximization of social welfare" are targets for his critique.

A fifth line of criticism throughout his writings is directed against biases and twisted terminology. He examines not only economic concepts such as unemployment, but also such expressions as "United Nations," "international," "values," "developing countries," "bilateral aid," and "the free world," and lays bare the opportunistic interests underlying such use of language.

The features against which these lines of criticism are advanced are combined in the technocrat. He isolates economic (or other technical) relations from their social context; he neglects social and political variables and thereby, unconsciously, ministers to the vested interest that might otherwise be violated; he pretends to scientific objectivity and is socially and culturally insensitive. Certain types of planners and so-called experts who try to impose their technical models on a living society fit the picture.

But scholars may ask: Is this not simply a question of method? Can the narrow technocrat not be replaced by one who introduces social variables openly into his formal models? Jan Tinbergen, Hollis B. Chenery, and Irma Adelman have tried to do precisely this. Cannot the "Western" approach be saved in this way?

Myrdal's answer is yes and no. In certain areas, a widening or redefinition of concepts can be allowed for. The productive effects of better nutrition can, in principle, be studied and the line between investment and consumption be redrawn for poor societies. The influence of climate (much neglected by most economists), of attitudes, and of institutions can be introduced either as constraints or as variables. An agricultural production function can be postulated in which health, education, distance from town, etc., figure as "inputs." "Capital" can be redefined so as to cover everything on which the expenditure of resources now raises the flow of output later, so that it is greater than it would otherwise have been.

But there are limits to such revisionism. These limits apply both to the analysis of facts

and to the recommendations of policies. On the factual side, the reformulation runs into difficulties if the connection between expenditure of resources and "yield" is only tenuous, as in the initiation of a birth control program or a land reform.

In the analysis of values, the construction of a social welfare function in Myrdal's view is not a logical task. The unity of the social program of a party or a movement is not like that of a computer program or a logical system, but more like that of a personality. It is discovered not only by deductive reasoning and the application of syllogisms, but by empathy, imagination, and even artistic and intuitive understanding. Just as we may ask "what would a person like this do or want if the situation were different in specified ways from what it is," so we may ask similar questions about classes, parties, groups, or even whole societies. Means and ends, targets and instruments, are very misleading ways of grasping this type of question, for the unity from which we infer recommendations is not logical but psychological.

It is important here to return to Myrdal's call for expressing our valuations explicitly before embarking on social analysis, precisely in order to make research more objective. What are these valuations in the modernization ideals? They constitute a complex system that includes rationality, planning the future, raising productivity, raising levels of living, social and economic equalization, improved institutions and attitudes, national consolidation, national independence, political democracy, and social discipline. All of these value premises and the valuations derived from them are subsumed under the quest for rationality. As we examine them, we cannot fail to become aware that these are the valuations of the Swedish welfare state, writ large. High material standards of living must be combined with welfare care for the ill, the poor, and the victims of the competitive struggle. They are the liberal values of a mixed economy, part public, part private. Myrdal, the great critic of the transfer of inappropriate "Western" concepts and values, has been accused of assessing, if only unconsciously, the experience of the underdeveloped countries against that of the modern welfare state, and of a certain lack of empathy for the possibility of doing things differently, by providing alternative roads to development. The Indian anthropologist T. N. Madan charged him with failure to practice what he proclaims, when Myrdal

complains that "large numbers of South Asians have only one set of clothing which is seldom washed, except in bathing. Typically, the same clothes are worn day and night since pajamas and even underwear are luxuries a great many people can ill afford. The hygienic consequences are easy to imagine" (1968, vol. 1, p. 552; Madan 1969, pp. 289–290). In particular, he has no place for what some of his critics regard as benign forms of corruption and nepotism. Traditional valuations are acceptable only if they do not conflict with the modernization ideals, otherwise they represent "obstacles and inhibitions."

No doubt, there is some justification for this accusation. As a proud, somewhat un-Swedish Swede (he is admired and revered by some and detested by other Swedes, and the award of the Nobel Prize in 1973, in its fifth year, is regarded by some as too late, by others as too early), he finds it easier to identify with liberal Americans than with the English or French, and easier with Englishmen than with the Indian masses. It is partly for this reason that *An American Dilemma* is an optimistic book, and *Asian Drama* a pessimistic one. He once said how kindred American aspirations and ideals, and the "American creed," were to his own beliefs, and how he could identify with these ideals when writing the book on the black problem; and how, in contrast, when he visited an Indian textile factory, the thin, half-naked brown bodies struck him as utterly alien. But more profound than this difference in personal allegiance is Myrdal's view that the American creed was not only the set of value premises chosen for *An American Dilemma*, it also represented the historical trend; whereas the modernization ideals, though almost a state religion, do not necessarily reflect the trend of the future.

Optimism and pessimism, Myrdal would be the first to emphasize, should have no place in an objective analysis because they are wishful thinking or biased positions, convenient to those who hold them, and reflect "opportunistic" beliefs in what they select and "opportunistic" ignorance in what they omit. The scholar should be concerned with realism. In particular Myrdal brought out clearly the origins of and the interests behind the swings from colonial pessimism about the "idle natives" to the postindependence optimism of the 1950s and early 1960s, and back to the pessimism of the 1970s. He was an early critic of the euphoria of the "Green Revolution" (the use of high-yielding varieties of grain in agriculture), a euphoria

which ran against the stark critique of *Asian Drama*.

Asian Drama is an odd book. And it is odder still that no reviewer pointed out this oddity. A recurrent theme in the main body of the book is the reasons for biases in economic theorizing about south Asia. Some issues concerning substantive economic analysis of Asian development, on the other hand, are relegated to appendixes. Myrdal once said it is like a stocking turned inside out. For Myrdal, the important task is to purge, to cleanse of biases before analyzing and reconstructing. He regards the existing structure as deeply contaminated by bias, behind which stand vested interest.

How then are biases related to interests? According to the colonial ideology, a hot climate, a backward social structure, and ethnic inferiority prevent economic advance. Economic progress is the privilege of a few races. The pessimism about the possibility of development was opportunistic for it lifted responsibility for promoting development from the colonial administration.

Independence saw the rapid growth of the "development industry." A massive body of research and communications grew up in the 1950s and 1960s, and the mood changed to one of optimism. The new independence, the desire of the ruling elites to emulate the West, and the rivalries of the cold war fed this optimism. The false analogy of European reconstruction under the Marshall Plan was used as the paradigm for development. Since the communists had blamed the colonial powers for lack of development, the response was to drop the colonial doctrine. The existing body of economic analysis came in very handy. It neglected climate, it ignored attitudes and institutions as strategic variables, it regarded consumption as not productive, and it treated the state as exogenous. The conclusion: pour capital into the sausage machine, turn the growth handle, and out comes evergrowing output. The optimism is reflected in the changing terminology: from backward regions (not yet countries!) to underdeveloped countries to developing countries. "Diplomacy by terminology," Myrdal calls it. There is assumed to be a "trade-off" between equality and growth, and therefore equality, which hinders growth, has to be sacrificed or postponed, and with it goes any deep analysis of land reform, education, corruption, social discipline, and the interest and efficiency of the state.

In the late 1960s and 1970s, there occurred a return to a pessimistic mood, rationalized by the

ineffectiveness of aid, by "wrong" domestic economic policies, and by reduced need for aid. At the same time, the undeveloped countries called for a "New International Economic Order." Myrdal had stressed throughout his work the need for reforms inside the underdeveloped countries themselves, though the developed countries and international reform can make contributions to overcoming internal difficulties. In the prescient *An International Economy* (1956b) he had developed the idea that national integration led to international disintegration.

An important idea in Myrdal's arsenal is that of circular or cumulative causation (or the vicious- or virtuous-circle), first fully developed in *An American Dilemma*. Traditional theory explains inequality between individuals, regions, and countries as the results of differential resource "endowments." But resources are the result, not the cause of income and wealth. Unimproved land, which is an endowment, is important for resource-based industries, but not for processing and manufacturing. It is the resource poor countries, like Israel, Hong Kong, Singapore, Taiwan, Japan, and South Korea, that present the success stories of development. Capital, an important factor of production, is also much more the result than the cause of economic growth. The principle of cumulative causation postulates increasing returns through specialization and economies of scale, and shows how small advantages are magnified.

The principle goes back to Wicksell, who, in *Interest and Prices* (1898), had analyzed divergences between the natural and the market rates of interest in terms of upward or downward cumulative price movements, until the divergence was eliminated. Wicksell pointed out that if banks keep their loan rate of interest below the real rate of return on capital, they will encourage expansion of production and investment in plant and equipment. As a result, prices will rise and will continue to rise as long as the lending rate is kept below the real rate.

The principle of cumulative causation can be used to show movements away from an equilibrium position as a result of the interaction of several variables. But not every form of circular or mutual causation or interaction is cumulative and hence disequilibrating, for a series of mutually caused events can, after a disturbance, rapidly converge either on the initial or on some other point of stable equilibrium. In order to create instability, the numerical values of the

coefficients of interdependence have to be above a critical minimum size. For example, an increase in consumption will raise incomes which in turn will raise consumption, and so on. But the infinite series will rapidly converge on a finite value. Only if the whole of the extra income or more were spent on consumption would the process be cumulative and disequilibrating.

The notion was applied by Myrdal most illuminatingly to price expectations in *Monetary Equilibrium* (1931) and to the relations between regions in *Economic Theory and Underdeveloped Regions* (1957). He showed how the advantages of growth poles can become cumulative, so that "unto those who have shall be given," while the backward region may be relatively or even absolutely impoverished.

Myrdal applied the notion also to sociological variables and their interaction with economic ones, like discrimination against blacks, their incomes, and their level of performance (low skills, low morals, crime, disease, etc.). In the analysis of development the relation between better nutrition, better health, better education, and higher productivity, and hence the ability to further improve health, education, and nutrition, shows that the inclusion of noneconomic variables in the analysis opens up the possibility of numerous cumulative processes to which conventional economic analysis is blind. It also guards against uncausal explanations and universal remedies or panaceas. The revolutionary character of the concept of cumulative causation is brought out by the fact that interaction takes place not only within a social system in which the various elements interact, but also in time, so that memory and expectations are of crucial importance. The responses to any given variable, say a price, are different according to what the history of this variable has been. It is this dynamic feature of analysis and its implications for policy that distinguish Myrdal's approach from that of economists who think in terms of general equilibrium.

In *Economic Theory and Underdeveloped Regions* (delivered as the Cairo lectures), and later in *Asian Drama*, he used the concepts "backwash" and "spread" effects to analyze the movement of regions or whole countries at different stages of development and the effects of unification. It is a highly suggestive, realistic, and fruitful alternative explanation to that of stable equilibrium analysis, which is usually based on competitive conditions and diminishing returns, and concludes that gains are widely

and evenly distributed. Some might consider these concepts as one of Myrdal's most important contributions.

Like the Marxists, Myrdal emphasizes the unequal distribution of power and property as an obstacle not only to equity but also to growth. But his conclusion is not Marxist. He regards a direct planning of institutions and shaping of attitudes (what Marx regarded as part of the superstructure) as necessary, though very difficult, partly because the policies which aim at reforming attitudes and institutions are themselves part of the social system, part of the power and property structure.

This brings us to Myrdal's critique of the kind of government he calls the "soft state." This critique has sometimes been misunderstood. It is plain that "softness" in Myrdal's sense is quite compatible with a high degree of coercion, violence, and cruelty. The Tamils in Sri Lanka, the Indians in Burma, the Chinese in Indonesia, the Hindus in Pakistan, the Moslems in India, the Biharis in Bangladesh—to take six states he calls "soft"—would not complain about excessively soft treatment. "Soft states" also use military violence, both internal and external. Their "softness" lies in their unwillingness to coerce in order to implement declared policy goals. It is not the results of gentleness or weakness, but reflects the power structure and a gap between real intentions and professions.

Myrdal has also applied his method to the analysis of inflation combined with widespread unemployment in the developed countries of the West in the 1970s. He attributes "stagflation" to the organization of producers as pressure groups and the dispersion and comparative weakness of consumers, to the tax system which encourages speculative expenditures, to the structure of markets, and to the methods of oligopoly administrative pricing, and he condemns inflation as a socially highly divisive force.

The approach favored by Myrdal is one of neither Soviet authority and force nor of capitalist laissez-faire, but of a third way: that of using prices for planning purposes and of attacking attitudes and institutions directly to make them the instruments of reform. The difficulty is that any instrument, even if used with the intention to reform, within a given power structure may serve the powerful and reestablish the old equilibrium. Even well-intentioned allocations, rationing, and controls may reinforce monopoly and big business. What looks like socialism in

the first round feeds monopoly capitalism in the second or third. How does one break out of this lock? Myrdal does not draw revolutionary conclusions but relies on the admittedly difficult possibility of self-reform that arises, in both the American creed and in the modernization ideals, from the tensions between proclaimed beliefs and actions.

On the one hand, he thus stands more firmly in the neoclassical tradition than he might be prepared to admit and attributes considerable importance to avoiding "distortions" of interest rates and prices. On the other hand, there is an inconsistency in his advocacy of central planning and his contempt for most politicians and bureaucrats, which reveals an anarchistic streak.

Both *An American Dilemma* and *Asian Drama* are books about the interaction and the conflict between ideals and reality, and about how, when the two conflict, one of them must give way. Much of conventional economic theory is a rationalization whose purpose it is to conceal that conflict. But it is bound to reassert itself sooner or later. When this happens, either the ideals will be scaled down to conform to the reality or the reality will be shaped by the ideals. Even if the chances of success are only one in a hundred, Myrdal, never afraid to express unconventional and unpopular views in plain language, will have been a leader, in thought and action, toward a reality shaped by enlightened ideals.

PAUL STREETEN

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NAGEL, ERNEST

Ernest Nagel is one of the most distinguished philosophers of science of this century. He is also widely recognized as a leading spokesman for a critical naturalism and a refurbished empiricism, and as a defender of some leading themes in the pragmatic approach to knowledge and science. But he is also critical of some pragmatic theses, e.g., that it is the function of scientific theories to predict and not explain, and that scientific theories and laws should not and cannot coherently be construed as statements—true or false—but must be described only as useful or useless rules of inference.

Many of the themes and interests just indicated are conceptually interconnected for Nagel. He buttresses his defense of naturalism by reference to the outcome of scientific inquiry, and believes that his version of empiricism is supported by reference to scientific procedures and claims. Thus he does not think that scientific practice and theories lend credence to some distinctive rationalistic theses, among them, the thesis that scientific laws express necessities or necessary truths, or that science supposes the truth of determinism. He further accepts and develops the Peircean claim that scientific procedures are self-corrective, that science is distinguished by its method, and that there are no premises that are certain and indubitable and that serve as the foundation for knowledge. He is therefore critical of some traditional versions of empiricism, hence the previously used term “refurbished empiricism.” He has no sympathy with the view that there is an indubitable ob-

servational language that supports scientific theories or by appeal to which scientific theories are tested. It is equally obvious that he is critical of some recent approaches to the philosophy of science developed by Paul Feyerabend and others, which challenge, in John Dewey’s phrase, the thesis of the supremacy of method. And as against Thomas Kuhn, he takes it as given that despite revolutionary changes, scientific advances and the growth of knowledge are cumulative, at least in many areas of research, and certainly in the physical sciences.

Nagel has written extensively on issues in the philosophy of science, theory of knowledge, philosophy of law, and moral theory, and has written papers that fall squarely into the area of the history of science. His writings include *The Logic of Measurement* (1930); *An Introduction to Logic and Scientific Method* (Cohen & Nagel 1934); *Principles of the Theory of Probability* (1939); *Sovereign Reason* (1954); *Logic Without Metaphysics* (1956); and *The Structure of Science* (1961). He has also coedited a number of books, among them *Induction: Some Current Issues* (Nagel & Kyburg 1963) and *Meaning and Knowledge: Systematic Readings in Epistemology* (Nagel & Brandt 1965). Another book, *Teleology Revisited and Other Essays in the History and Philosophy of Science*, is now in press. His writings exhibit the influence of the work of various scientists on his thinking: Jules H. Poincaré, Pierre Duhem, Norman R. Campbell, James C. Maxwell, Paul F. Lazarsfeld. Morris Cohen and John Dewey were the teachers who influenced him the most. He often expresses indebtedness to the writings of Philipp Frank and

Rudolf Carnap. Nagel has been very influential both as writer and teacher, and countless students have expressed indebtedness to him.

He was born in Czechoslovakia in 1901, and received his B.S. in social science at the City College of New York in 1923, his M.A. from Columbia University in 1925, and his Ph.D. in 1930. He was an instructor in philosophy at City College in 1930/1931, and taught at Columbia University until his retirement in 1970. In 1965, he was named John Dewey professor of philosophy. He became university professor in 1967, and after his retirement was a special lecturer from 1970 to 1974. He has been widely honored, and has served as president of the Association of Symbolic Logic (1941–1943), the American Philosophical Association (1954), and the Philosophy of Science Association (1961–1963).

It is distinctive of Nagel's approach that he formulates his theses with extreme care, and defends them only after suitable qualifications have been made. What follows is therefore a sketch of some aspects of Nagel's views, particularly as they relate to issues in the philosophy of the social sciences.

Nagel's naturalism is in part a thesis that defines what there is—it denies that there are disembodied entities or states. It also includes a correlative thesis about explanation, namely that all explanations exist by reference to the behavior of organized bodies. Nagel further claims that the results of modern science lend credence to a version of contextual naturalism that he thinks is the most distinctive contribution of American philosophical thought of this century. As suggested, Nagel views naturalism not as a body of a priori truths, but as leading principles which are in accord with scientific theories and procedures. It therefore follows that for Nagel nonnaturalistic, even theological, theses are not cognitively meaningless.

Notice that contextual naturalism as construed by Nagel does not deny the reality of mental events or occurrences or states. Again, Nagel's naturalism does not entail the thesis that mental states—e.g., pains—are to be identified with brain states, or with dispositions to behave; or, to use the formal mode, that statements about mental states are synonymous with statements about behavior. Nagel's naturalism is therefore to be distinguished from most versions of materialism, and from some recent versions of extreme behaviorism (see "Natural-

ism Revisited" in *Logic Without Metaphysics*, and "The Perspective of Science and the Prospects of Men" in *Sovereign Reason*).

Some epistemological views on these issues deserve review. Nagel's naturalism and empiricism allow him to admit that agents may have privileged knowledge of or about their mental states, but not exclusively so. That is to say, he does claim that some mental terms or predicates—e.g., terms denoting attitudes—may be viewed as behavioral dispositional terms, which denote dispositions to behave, and hence, when people talk about their attitudes, they may not be in a privileged position to know about them. Granting that agents may be in a privileged position about some mental states—e.g., pains—he adds that others, however, may also be in a position to confirm or disconfirm statements about them as well. Statements about pain may be directly or indirectly confirmable by reference to behavior, verbal or otherwise, of agents (1956, chapter 2; 1961, pp. 447–454).

Social scientific theories and terms or subject matter may be subjective in a number of senses. Explanations frequently refer to intentions of agents; social scientific theories are frequently about inner states. Again, agents may be privileged in their knowledge of the truth about some sentences describing their psychological states; and many psychological statements are not synonymous with statements about brain states or equivalent in meaning with statements about behavior, but are, nevertheless, partially reducible to them. In general, mental terms applied by **A** to **B** may be, as far as **B** is concerned, partially reduced to statements about **A**'s behavior, or may function as theoretical terms to **B**. It is therefore readily apparent that Nagel does not want to continue as significant for the purposes of understanding—not merely in social scientific discourse, but in scientific discourse in general—some distinction between observational and theoretical terms. More generally, Nagel has little patience with philosophical theses that claim that theoretical terms are synonymous with conjunctions or disjunctions of observational terms. Although Nagel is sympathetic to the verification theory of meaning, he does not subscribe to any specific formulation of that theory. When he does dismiss some specific claims as pointless, or cognitively meaningless, he tries to give specific reasons for his dismissal. More often than not, as in his well-known critique of some versions of Freudian theory, he argues that it is difficult to specify

clearly the empirical significance of the claim in question and to know what evidence counts for or against it, as distinguished from the evidence for or against the contrary theory. Needless to say, he often adds that it is otiose to ask for the testing of scientific hypotheses in isolation, and admits that scientific terms get their meaning in part from the lawlike sentences in which they are embedded. Critiques for him are therefore critiques of systems of thought (1961, chapters 6, 13; 1954, chapters 4, 5, 6).

The distinction just alluded to between observational and theoretical is for Nagel neither a precise nor an absolute one. As already suggested, both terms get their meaning, or, perhaps better, their significance, from the laws and generalizations in which they appear. Observational terms do not describe the momentarily given, and therefore, in a sense, are theoretical, and thus not certain. Still, for Nagel a functional distinction exists between observational and theoretical terms. By and large, the former are terms about which there is widespread agreement as to their applicability to the experience to be encountered, and they have remained of stable use despite changes in scientific beliefs and theories. And since he believes that there is wide agreement about the direct applicability of some terms to collectives and groups, he does not agree with those versions of methodological individualism that claim that all statements in the social sciences must be tested by reference to the behavior of individuals or be reduced to statements about the behavior of individuals. Although he is sympathetic to the thesis that terms about collectives are often clarified when reduced to terms about individuals, he does not claim that they must be. A term *S* may be reduced to *T* even if *S* is not explicitly definable by reference to *T*, but is only partially so (1956, pp. 361–369; 1961, pp. 473–485, 535–546).

According to Nagel, methodological individualism must be distinguished from psychologism, or the thesis that all theories in the social sciences are reducible to psychology—a thesis Nagel is hesitant to accept. On all accounts, he has very little sympathy with some classic versions of psychologism—e.g., that of John Stuart Mill, which has been proven untenable by Dewey, Thorstein Veblen, and Franz Boas, and others. Psychologism, in turn, must be distinguished from social scientific reductionism, or the thesis that ultimately the basic laws or theories of the social sciences, whatever they

may be, are reducible to biology or perhaps ultimately to the physical sciences. Nagel does not believe that social scientific reductionism is hopeless, even if it may be a speculative thesis. At any rate, he has argued at length for the reducibility in principle of biological theories to theories in the physical sciences. It should be noted that Nagel does not think it is helpful to discuss the reducibility of one type of phenomenon to another, or even the reducibility of one discipline to another. He thinks it is essential for the purposes of clarity to discuss the reducibility of one theory to another, and has stipulated various conditions that must be achieved before one theory may be said to be reduced to another. For him, theory *A* is reduced to theory *B* if (but not only if) theory *B* contains terms which theory *A* does not, the terms of theory *A* are suitably related to the terms of theory *B* and the laws of theory *B* are deduced from the laws of theory *A*. In some writings, he allows that theory *B* may be reduced to theory *A* even if the laws are not deduced from theory *A*, but, let us say, made probable by them. It is of some moment to remember that the terms of theory *B* are not considered by him synonymous with terms in theory *A*, the terms in theory *B* still apply to phenomena directly. Should the laws or theories about headaches, for example, be reduced to the laws about brain states, it does not follow that there are no headaches but only brain states. Nagel has stipulated the semantic conditions that obtain between the terms of theory *A* and *B*, and how these conditions figure in the deduction of the laws of theory *B* from theory *A* (1961, chapter 11).

Nagel's naturalism therefore supports some theses about the social sciences which stress their continuity with the natural. Nagel especially argues for the thesis that the criteria for the acceptability of claims as warranted, or as worthy-of-belief, are the same in the natural and the social sciences. This does not require Nagel to deny that there is no obvious simple way to avoid bias or "relativity" in the social sciences, and he does not deny that values frequently play a role in the genesis of observational reports. These admissions do not diminish the force of Nagel's contentions about method, for it is not the genesis of the reports that matter, but their subsequent history. Reports once made can be publicly checked and criticized, bias possibly can be checked by the self-corrective method of the sciences. He also tries to show that some extreme claims about

the sociology of knowledge made by Karl Mannheim and others are incoherent and self-defeating (1961, pp. 485–503).

There is another invariance between the natural and social sciences stressed by Nagel. All the sciences aim at presenting comprehensive explanations and aim to formulate and test lawlike statements and theories. These aims are interconnected for Nagel since he accepts the nomological model for explanation. Let us use the term “explanandum,” for the sentences or set of sentences describing that which we want to explain; a member of that set therefore might describe an event or, if lawlike, a regularity. Again, let us use the term “explanans,” for the set of sentences which afford us the explanation. Nagel defends the thesis that the explanans must contain a law. Notice, therefore, that Nagel does not defend what has occasionally been called the covering-law model. That model claims that we have a satisfactory explanation if, and only if, the explanans contains a law, and the explanandum is deduced from the explanans. One might add here that a sentence is a law if it is lawlike and true, a sentence is lawlike if it is general and satisfies other requirements about the conditions which Nagel develops in *The Structure of Science*. We shall use the word “theory” to apply to a set of laws which may be used to systematize a discipline, and a theory generally contains theoretical terms. Nagel tries to specify in many of his writings a way of understanding the structure of most theories, especially in the natural sciences. He has written at length about three distinguishable elements that play a role in the formation and interpretation of scientific theories: the implicit formalism, the coordinating definitions, and the model for the theory. It is only by understanding all three together that we can see how theories will afford us explanations of laws or, if one wishes, the regularities which laws express (1961, chapters 1, 13, pp. 459–466).

The aims suggested for the sciences, social and otherwise, are long-term ones. It does not follow that it is the immediate aim of any social scientific discipline to try to test and confirm comprehensive theories at once. The immediate aims of a discipline for Nagel can only be specified fruitfully by reference to the history of the discipline, and an awareness of the problems faced by the discipline. But if one accepts Nagel’s long-range aim, one need not accept any version of determinism. And certainly one

need not accept determinism as a metaphysical thesis to the effect that every event must have a cause; or that every event—no matter how fine a description we may have of that event—is explainable in a way that satisfies the covering-law model of explanation, namely, that the sentence describing the event is deducible from a set of laws and the relevant initial conditions. Two points may be added. Nagel thinks that determinism is best formulated not as a statement true or false, but as a leading principle or directive to inquiry which may turn out to be useful or useless. He does not, however, agree with the claim that has frequently been made that our claims about human actions, our assignments of guilt and responsibility, commit us to indeterminism, a position he finds dubious. So although Nagel does not think that his approach to explanation presupposes acceptance of determinism as a truth about nature or man, he does not think that any argument has been shown that coherent discourse about human action requires commitment to indeterminism or libertarianism. It should be added that although Nagel thinks that the social sciences will most likely at best be able to confirm and test statistical laws and theories, he does not think it helpful to say on that account that human action or behavior is “inherently statistical.” He claims that a deterministic account may be available only if we allow ourselves to use physiological terms and theories, but adds that if we do so, we may be using terms of no interest to us as social scientists (1961, pp. 316–324, 592–606; 1954, chapter 15; 1956, chapter 4, pp. 95–103).

Nagel distinguishes between four types of explanation: deductive, probabilistic, genetic, and functional. In a deductive explanation, the explanandum is deduced from the explanans; in a probabilistic one, it is made probable. In a genetic explanation, an event or an institution is explained, in part or as a whole, by tracing its genesis or history. In a functional explanation, an item within a system, such as an organ or institution, is explained by showing the role it plays within the system of which it is a part. Nagel then tries to show how these four types of explanations appear in the sciences, social and natural, and the problems each one of these types encounters.

As already noted, all these explanations may be of the nomological kind, and Nagel tries to show that indeed they are. Given this, a corollary follows: Nagel’s approach does not deny

that many but not all explanations are of a causal account; that we explain an event *e* by showing that it was caused by event *f*. The link between causal and nomological explanations is provided by David Hume's thesis which Nagel develops. Hume's view was that a statement to the effect that event *e* is caused by event *f*, is short for a body of statements of the following type: Event *e* manifests property *P*, event *f* property *Q*, all events that manifest property *P* are succeeded by events that manifest property *Q* (1961, chapters 2, 3, pp. 73–78; 1956, chapter 11).

It has on occasion been claimed that the approach suggested by Nagel, and others sympathetic to the views he develops, forget that it is the aim of the social sciences to enable us to understand human action. But there is no incompatibility between the quest for understanding and the adoption of the types of naturalism espoused by Nagel. Compatibility is at hand if we adopt the thesis that *S*'s action is made understandable by a certain type of explanation, one in which the explanans, or the set of sentences explaining *S*'s actions, contains sentences about *S*'s beliefs, intentions, and attitudes, and lawlike sentences. There is incompatibility if one argues that *A* can know about *B*'s intentions or beliefs only by a special mode of confirmation. Nagel shows that some who have argued against his view are committed to the unacceptable thesis that *A* can understand *B*, only if *A* attributes "experiences" to *B* which *A* has had as well. This thesis would lead to the result that only a schizophrenic can "understand" a schizophrenic or, more germane, can explain a schizophrenic's behavior (1961, pp. 535–546).

Nagel, as indicated, tries to show the applicability of his approach to the work of the social scientist, and especially to show how various statistical explanations in the social science are in accord with the deductive model. He also applies his analyses in an attempt to reconstruct historical explanations, many of which are only of the inductive kind (1961, pp. 508–520, 551–576).

Functional statements and functional explanations play a role both in biology and the social sciences. Nagel first tries to show that functional statements are eliminable. That statements to the effect that the function of *x* is *y*, can be rephrased to read that *x* is necessary in order for the organism of which it is a part to be in a certain state. A functional explanation then tries to show that *x* is necessary for the

accomplishment of a goal of the organism of which it is a part. We have functional explanations when we can clearly indicate the conditions under which an organism or entity can exist when it has clear goals, and when certain conditions are necessary for the accomplishment of these goals. And Nagel tries to show that diverse proposed functional approaches in the social sciences, especially those offered by British functionalists—e.g., Bronislaw Malinowski and others—fail to satisfy these requirements (1956, chapter 10).

The term "goal," or rather, the longer term "goal-directed" behavior, requires elucidation. At least two cases have to be distinguished. The one where an agent acts with a conscious end in view, and tries to behave in a way which would at least make probable the accomplishment of his or her end. The other, where no conscious intention is attributable to the entity. In such cases, we may say that an organism or entity *O*, behaves teleologically toward goal *G*, if at least four conditions are satisfied (1) *O* can be in a *G*-state under some but not all conditions; (2) *O* exhibits a certain persistence, it is or can be in a *G*-state even if there are variations in the environment in which *O* finds itself; notoriously, there are ranges within which this may occur; (3) *O* exhibits plasticity, its internal states change mainly, but not only, in response to changes in environmental changes to enable it to be in a *G*-state; (4) There is nomic-independence in the relative state variables. Assume, for the sake of argument, that *O* can be in a *G*-state only if it is in a suitable *Z*-state; assume further, that a *Z*-state is one in which *O* has two characteristics which admit of degree, or that there are two state variables *P*, *Q*. (*O* is in a *Z*-state if *P*₁ and *Q*₁, or *P*₂ and *Q*₂, etc.). The fourth condition then maintains that there is no general law which requires an organism to be in a specific *P*₁ condition at *t*, if it is in a specific *Q*₁ condition at *t*. These are the four conditions defended by Nagel in his approach to functional explanation, and in some of his articles he tries to show how various attempts at functional explanations and functional theories come close to satisfying these conditions, but frequently fail. His analyses of the logic of functional explanations, as given by Robert Merton, has played an important role in the development of the logic of the social sciences (1961, pp. 401–427).

In addition to his work in the philosophy of the social sciences Nagel's philosophy is replete with analyses of many problems in the areas of

science in general. In *The Structure of Science*, he deals with the notion of convention, with the role of conventions in physical theory, the nature of physical theory, the rise and decline of mechanical explanation, the status of geometry, and kindred topics which have received a great deal of attention. His monograph on *Principles of the Theory of Probability* is a defense of the frequency view of probability—or at least the applicability of the frequency view in many contexts of scientific investigation. Nagel adds, however, that the frequency view cannot elucidate the various contexts in which we speak of one theory being more probable than another, or one theory being more probable as evidence for it accumulates. His early text, written with Cohen, has remained a classic text in the philosophy of science, and in the logic of the scientific method.

SIDNEY MORGENBESSER

WORKS BY NAGEL

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NEWCOMB, THEODORE M.

When Theodore M. Newcomb received his doctoral degree at Columbia University, the field of social psychology was beginning to be identifiable as a subdiscipline of the fields of psychology and sociology. The development of that field since that time has been importantly influenced by his contributions as a researcher and theorist and also by his role as a mentor of graduate students who became noted behavioral scientists. Newcomb began his graduate studies at Union Theological Seminary where he experienced the open mindedness, intellectuality, and social concern of his professors (particularly Harrison S. Elliot and Harry Ward). How-

ever, the courses he took "across the street" at Columbia's Teachers College shifted his interests to psychology. He transferred to Columbia where he received his PH.D. in 1929. There, he was strongly impressed by the measurement and methodological approaches of Robert S. Woodworth and Edward Lee Thorndike. His grounding as a psychologist can be attributed in large measure to his graduate work with Gardner Murphy with whom he developed a lifelong personal friendship and intellectual relationship. Newcomb's orientation and perspectives in psychology were also formed by the teaching and guidance of Goodwin Watson, who became a close friend as well.

Born in 1903, the son of a Congregational minister, Newcomb's early years were spent in a rural Ohio setting. He attended Oberlin College, earning his B.A. in 1924. After teaching French and English for one year, he entered Union Theological Seminary with the intention of becoming a college teacher of religion. His transfer to Columbia University Teachers College took place during his second year at Union Seminary.

After receiving his doctorate, Newcomb's first academic appointment was as assistant professor in psychology at Lehigh University. After a year at Lehigh he joined the psychology faculty at the Cleveland campus of Western Reserve University where he spent the next four years (1930–1934). While at Western Reserve he carried out a research study with one of his graduate students. This study (Newcomb & Svehla 1937), which in its method and content presaged the character of much of his later work, was concerned with similarities and differences of attitudes among dyads (in this case parent-child comparisons), used measurement techniques that were advanced for its time, and related two-person interpersonal influences to the effects of larger social groupings. Specifically, the study showed that the contexts of ethnic and religious memberships were vehicles for the transmission of attitudes from parent to child. Another quality of this study, which became somewhat typical of Newcomb, was the detailed and painstaking statistical analysis of data leading to unpredicted and unexpected findings.

During his last year in Cleveland, Newcomb coauthored, with Gardner Murphy and Lois B. Murphy, a revision of their book, *Experimental Social Psychology* (1937). This revision was probably influential in setting the future aspect

of American social psychology as an experimental and quasiexperimental field.

Newcomb moved to Bennington College in 1934, only two years after the college admitted its first freshman class. This was hardly a typical place of higher education. It was new, small, isolated in the Green Mountains of Vermont, innovative, and college-community oriented. In addition, its student body was entirely female and disproportionately from upper income families. Yet it was here that Newcomb collected data on the student population for the "Bennington Study" that became a landmark research study of theoretical significance and generalizability. It appeared as a publication entitled *Personality and Social Change* (1943). The research involved a longitudinal study of political-social attitude change. The findings showed a prevailing shift toward declining conservatism among students during the college years largely attributable to peer group associations and community norms. The exceptions tended to be students who were more passive and dependent on family and outside community norms, and/or who belonged to small campus friendship groups that were relatively isolated from or negativistic toward the larger campus community.

In 1941 Newcomb was appointed an associate professor in the sociology department of the University of Michigan. He remained at Michigan until his retirement in 1973, with the exception of four years he spent in Washington in various war-related positions. When he returned to Ann Arbor in 1945, he received the rank of professor in both the psychology and sociology departments. In 1955, during his tenure at Michigan, he was elected president of the American Psychological Association.

The Doctoral Program in Social Psychology was created at Michigan in 1946, and in 1947 Newcomb became its director. Until its dissolution in 1963, which he deeply regretted, the program gained the reputation of turning out top quality PH.D.s in social psychology, many of whom have had outstanding careers. In addition to his role as a graduate program administrator and mentor of doctoral candidates, Newcomb was highly esteemed as an undergraduate lecturer.

Two of Newcomb's books, closely related to his teaching perspectives, had considerable impact in shaping the content of social psychology, both in its influence on colleagues and on the education of students. These were *Readings in*

Social Psychology (1947), edited with Eugene L. Hartley, and *Social Psychology* (1950). The *Readings in Social Psychology* presented to the student a selection of research articles and conceptual essays reflecting the state of advancement of social psychology. An appendix was included to acquaint the student with the rudiments of hypothesis testing inferential statistics.

Social Psychology, while serving as a textbook, presented a comprehensive theory of social psychology which drew together and integrated several conceptual frameworks, including the social self theories of Charles Horton Cooley and George Herbert Mead; frame of reference theory largely based on the work of Muzafer Sherif; and the structure-function definitional system as outlined by Ralph Linton. Linton's concepts of role and status especially gave Newcomb's book the quality of an interface between social structure and behavior at the individual psychological level.

In the early 1950s Newcomb's interests focused on theoretical formulations of the nature of interaction between individuals' attitudes toward each other and their attitudes toward external objects (other people or ideas). The prototype of his formulations along these lines appeared in an article in the *Psychological Review* (1953). The following year he began research on students who were housed together for one academic year with their understanding that they would serve as subjects of study. Two groups were studied, one group in each of two successive years. Newcomb's research aims were to test empirically his theoretical propositions and to refine them in terms of the results. This research was reported in his book, *The Acquaintance Process* (1961). He found that while there was no appreciable change in attitudes toward the external objects, a balancing effect did take place whereby the students' attitudes toward each other and accompanying interpersonal grouping did change as the acquaintance process proceeded. These results were in line with Newcomb's disposition toward Fritz Heider's notions of "balance theory." This study led to a concern for understanding the nature of stability in individuals and in groups, which Newcomb discussed in two subsequent papers (1963; 1965).

Nearly 25 years after the original Bennington study, Newcomb began follow-up research on the former students, comparing them to the Bennington students of the middle 1960s. This research was published as *Persistence and*

Change: Bennington College and Its Students After Twenty-five Years (Newcomb et al. 1967). Perhaps surprisingly, it turned out that the alumnae tended to persist in their liberalism rather than "regress" toward conservatism. His data on the students at Bennington in the 1960s showed that an important difference from the students of the 1930s was that even before entering college, the students of the early 1960s held markedly liberal views. The Bennington experience, therefore, had no appreciable effect in shifting their attitudes. Newcomb interpreted the effects, rather, in terms of reinforcing already existing attitudes. However, sociopolitical attitudes were not found to be the salient dimension of concern in the early 1960s Bennington environment. Instead, the more personal attitudes of individualism, unconventionality, and intellectualism were relevant to the college community norms.

The general phenomena of college student peer groups was taken up by Newcomb in two essays which appeared in two collections, one edited by Nevitt Sanford, *The American College* (1962) and a related volume edited by Newcomb and Everett K. Wilson, *College Peer Groups* (1966). In these articles Newcomb theorized about the variables which determine peer formation and more particularly dealt with the disjuncture between peer group interests and values on the one hand and the academic intellectual functions of the college on the other. Newcomb acknowledged that this concern is certainly not a new one for the colleges, but he ascribed an exacerbation of this condition in more recent times to the largeness of colleges and the growing breakdown of community qualities:

I believe that college faculty members, by and large are nowadays no less capable of offering intellectual excitement than they used to be. But for the most part they now operate in social systems such that whatever excitement they offer tends not to be caught up, re-enforced, and multiplied by virtue of being shared outside the classroom. Time was when colleges were typically small, their student bodies relatively homogenous, and their general atmosphere community-like. Most of the changes of the past few decades have tended to deprive large numbers of colleges of these characteristics. The result has been that peer-group influences are as potent as ever, but increasingly divorced from intellectual concerns. (Newcomb & Wilson 1966, p. 484)

In collaboration with Kenneth A. Feldman, Newcomb wrote an extensive review and syn-

thesis of the research on the effects of attending college, *The Impact of College on Students* (1969). In this work the authors point up a process which they call "accentuation"—the fixation and intensification of values and motivational orientations which already exist in the individual. Accentuation is particularly affected by college environments when the size of the college unit or the subunit is small enough to allow for a community atmosphere. The intellectual goals of the college can be enhanced when the size and organization of the college unit makes for informal faculty-student social contact. Belief in this kind of proposition led to the establishment in 1967 of the Residential College at the University of Michigan, with an initial class of 220 freshmen. Newcomb, who participated in the advocacy and planning of the college, became its associate director for assessment and research.

Upon his academic retirement to emeritus status, Newcomb's research activity remained undiminished. He continued in the analysis of data emanating from the Residential College research and undertook the analysis of data obtained from a sample of two thousand inmates drawn from all the juvenile correctional institutions in the United States. The purpose was to try to discern why "corrections" took place for some of the children and not others. This shift of interest from the most to the least favored youth in our society is testimony both to the general applicability of Newcomb's theoretical formulations and his lifelong concern with social issues.

LLOYD BARENBLATT

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NEYMAN, JERZY

Jerzy Neyman, director of the Statistical Laboratory, University of California, Berkeley, is a leading figure in the field of statistics. He is known for his fundamental contributions to theoretical statistics and for his extensive involvement in applications to astronomy, biology, health, and meteorology.

Neyman was born on April 16, 1894, to a Polish family then living in Bendery, Russia. In his early years, he became fluent in Polish, Ukrainian, Russian, German, French, and classical Latin. After attending school in Simferopol (1904–1906) and Kharkov (1906–1912), he studied physics and mathematics at the University of Kharkov. As a result of his studies in physics, he volunteered to prospect for radioactive minerals in an expedition to Outer Mongolia.

Neyman's mathematical development was influenced by the Russian probabilist Serge Bernstein and especially by Henri Lebesgue's book *Leçons sur l'intégration et la recherche des fonctions primitives* (1904). His candidate thesis at the University of Kharkov (1916) was on the integral of Lebesgue. His first paper (1923a) deals with a covering problem for the Lebesgue measure. Other influences of a more philosophical nature include Karl Pearson's *Grammar of Science* (1892), which Neyman discussed at length in Kharkov with Otto Struve, then a budding astronomer. In 1917, he returned to the university for postgraduate study, and for one year he was a *Docent* at the Institute of Technology, Kharkov.

The Russian–Polish war eventually led to his incarceration as an enemy alien. He was sent to Poland as part of an exchange of prisoners of war. In Warsaw, Neyman worked for a time on mathematical problems, under the guidance of Wacław Sierpinski. However, in postwar Poland other problems were pressing and from 1921 to 1923 Neyman worked as a statistician at the Agricultural Institute of Bydgoszcz. His doctoral

thesis (1923a) concerns applications of probability to the solution of questions of agricultural experimentation.

From 1923 to 1925 he was a lecturer at the Central College of Agriculture Warsaw. In 1924 Neyman went to Karl Pearson's Biometric Laboratory, University College London (as holder of a postdoctoral fellowship) and in 1928 he became head of the Biometric Laboratory at the Nencki Institute in Warsaw. At University College, he made the acquaintance of E. S. Pearson, son of Karl Pearson. Their scientific collaboration started only in 1925. Except for occasional meetings in 1925–1926, when Neyman studied in Paris under Lebesgue and Jacques Hadamard, the collaboration was carried out by mail until Neyman's return to University College in 1934.

Neyman and Pearson succeeded in placing the theory of testing statistical hypotheses on a firm logical and mathematical basis. A crucial idea was that statistical hypotheses are formulated mathematically by elaborating probabilistic models of the phenomenon under study. The qualities of a statistical test of validity of one model cannot be judged unless the possible alternatives are considered. This idea was not accepted easily by the authorities in the field. R. A. Fisher opposed it bitterly to the end of his life.

The 1928 paper which proposes this fundamental idea investigates in this light the performance of several known tests and of a class of likelihood ratio tests. Subsequent papers (Neyman & Pearson 1933a; 1933b; Neyman 1934) approach the task of finding tests which are optimal in that they control the probability of rejection of a correct hypothesis and maximize the "power" of the test, that is, the probability of rejection when the hypothesis is false. The optimization problem has unique solutions in certain simple cases. In other cases further restrictions must be imposed to obtain an answer. Several articles on this subject appeared in the *Statistical Research Memoirs* (Neyman & Pearson 1936).

The impact of the Neyman–Pearson ideas was felt throughout the field of statistics. After their appearance it became inconceivable that one could design an experiment, be it a clinical trial or a weather modification experiment, without regard to considerations of power. They determine the length or size of the experiment.

The framework provided by Neyman and Pearson's description of statistical problems has

become the basis for the practical totality of theoretical work in statistics. For instance, the widely acclaimed "theory of statistical decision functions" due to Abraham Wald (1950) is entirely dependent on this framework.

In the early 1930s, Neyman also investigated estimation problems and proposed his theory of confidence intervals. In the case of a one-dimension parameter, Θ , confidence intervals are a pair $(\underline{\Theta}, \bar{\Theta})$ of functions of the observations so selected that the probability that the random pair $(\underline{\Theta}, \bar{\Theta})$ covers the (nonrandom) true point Θ has a preassigned value $1 - \alpha$, with α small.

The theory is expounded in detail in two papers published in 1937. At first there was a suspicion that Neyman's confidence intervals were a variation of what Fisher called "fiducial intervals." This suspicion was quickly put to rest by Fisher himself. The logical arguments underlying the two approaches were incompatible. Eventually numerical differences between the two were noted (1941). A clear exposition of all these ideas can be found in Neyman's *Lectures and Conferences* (1938c).

From the start of his career as a statistician in Bydgoszcz, throughout the period of collaboration with Pearson, and continuing after his creation in 1938 of the Statistical Laboratory at Berkeley, Neyman was involved with the study of practical problems. Among those which led to publications one may mention the problems of counting the number of viruses or bacteria needed to cause disease (Neyman & Iwazkiewicz 1931), the accuracy of the dilution method (Neyman et al. 1935), sickness due to industrial exposure (Neyman & Iwazkiewicz 1934), and health insurance. In some of the problems Neyman became, as he says, "emotionally involved."

One of the deepest involvements, in astronomy, resulted in more than twenty years of collaboration with Elizabeth L. Scott, C. D. Shane, and many astronomers on problems arising from the clustered appearance of photographs of extragalactic bodies. The problems of interest involved, among other things, the possibility of using statistical procedures to determine (1) whether the observed red-shift was in fact due to recession velocities; (2) whether clusters of galaxies were expanding or recessing as rigid bodies; and (3) whether the evolution of galaxies proceeds from elliptical to spiral, to irregulars.

The statistical tools needed led to an extensive theory of the process of clustering (1955;

Neyman & Scott 1972). Neyman had already encountered clustering earlier in his description of abundance of larvae in the field, leading him to introduce the distributions called contagious distributions (1939).

In another domain, weather modification, Neyman's involvement started in 1951. Methods of evaluation of the effects of cloud seeding proposed by commercial operators could not be considered scientific. After a few years and a few experiments nothing remarkable could be asserted. However, a review of the Swiss hail suppression experiments and a detailed examination of other properly randomized experiments led to the conclusion that cloud seeding may have positive or negative effects often extending downwind for a hundred miles or more (Neyman et al. 1971; Neyman & Osborn 1971).

Since 1960, Neyman has devoted a substantial part of his time to the study of cancer and problems of carcinogenesis. The initial effort was to find out whether one could devise an experiment and statistical procedures which could differentiate between the one-step and two-step theories of carcinogenesis (1961). The study branched out to experiments on the mechanism by which urethan induces lung tumors in mice (Neyman & Scott 1967a; Neyman 1974b) as well as to elaborate models of the action of radiation on cells.

In a related domain Neyman has been deeply involved in statistical studies of the effects of pollution (Neyman et al. 1972) and the possibility of sorting out the effects of individual pollutants and of their synergistic combinations (1977b). Part of the necessary statistical framework was provided earlier by Neyman's theory of competing risks (1950; Neyman & Fix 1951). It is a domain where much caution is necessary. A volume of the sixth Berkeley symposium devoted to the effects of pollution on health indicates how little is known and how necessary is the large-scale effort recommended by Neyman. Along the way, Neyman met many other problems. A series of papers (Neyman & Bates 1952) deals with the possibility of distinguishing between variability of proneness to disease and contagion. Some deal with models of epidemics (Neyman & Scott 1964). Another series of papers prompted in part by T. Park's observations on flour beetles, deals with population dynamics (Neyman et al. 1956; Neyman & Scott 1959). Two different species (*T. confusum* and *T. castaneum*) survive happily and indefinitely if raised separately. On the contrary, when

raised together, one of the two species disappears rapidly the probability of survival of either being a function of the temperature, humidity, and other environmental conditions.

The studies of a practical nature would not have been able to proceed without adequate statistical methods. One large class of procedures introduced by Neyman around 1945 is given by his theory of best asymptotically normal estimates (1949). One may regard this theory as an asymptotic equivalent of the theory of least squares. It provides computable answers in many complex situations. The mathematical justification involves using so-called "nearby alternatives" that move toward the given hypothesis at the rate $(1/\sqrt{n})$ when the number n of observations increases. The procedure, introduced by Neyman (1937*b*), has invaded much of asymptotic statistical theory.

The best asymptotically normal estimates can be used for estimation purposes and also for testing purposes. In the latter case, Neyman has also provided another technique by his theory of $C(\alpha)$ -tests (1954), which are applicable to situations which involve nuisance parameters and yield test functions which are often readily obtainable from logarithmic derivatives of the probability densities.

Neyman is also known for his organization of the Berkeley symposia on mathematical statistics and probability. These occurred every five years from 1945 to 1970. Other works of a different nature include the publication of the volume *Heritage of Copernicus: Theories "More Pleasing to the Mind"* (1974*a*).

Neyman is a member of the National Academy of Sciences, the American Academy of Arts and Sciences, the Royal Swedish Academy, and the National Academy of Poland. He is a fellow of the Royal Society, London, and is honorary president of the International Statistical Institute.

LUCIEN LE CAM

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NIEBUHR, H. RICHARD

Helmut Richard Niebuhr was born in Wright City, Missouri, in 1894. At the time of his sudden and unexpected death in 1962 he was within one year of retirement as Sterling professor of theology and Christian ethics at Yale Divinity School. After an academic career spanning more than forty years, during which he and his older brother Reinhold were leaders in reshaping the religious consciousness of America, Niebuhr had looked forward to having time to write his *magnum opus* on Christian ethics. Despite this unfulfilled dream, Niebuhr, through his gifted teaching and a written legacy of seven books and numerous articles, has exercised increasing influence on the theological and ethical thinking of the American church and beyond.

A son of German immigrants who had settled in the Midwest, Niebuhr followed his father and elder brother into the ministry of the German Evangelical Church. He prepared for the ministry at two schools of his denomination, Elmhurst College (1908-1912) and Eden Theological Seminary (1912-1915). After ordination in 1916 he served as a pastor in St. Louis while completing a master's degree in history at Washington University. In 1919 Niebuhr accepted an invitation to teach theology and ethics at Eden, where he remained for the next three years. During this period he married and also spent a valuable summer of study at the University of Chicago, where he was introduced to the social philosophy and psychology of George Herbert Mead. Mead's conception of the self as arising from a social matrix had a lasting influence on the young theologian.

In 1922 Niebuhr resumed full-time theological studies at Yale Divinity School, earning both a B.D. and a PH.D. by 1924. He then served as president of Elmhurst College for three years but in 1927 returned to theological education at Eden. In 1930 he had sabbatical leave in Germany, with a side trip to Russia, and upon return to the United States received an offer to teach at Yale Divinity School; he began one year later.

Like others of his generation, Niebuhr experienced the trauma of war, revolution, and social upheaval that shook the foundations of Western liberalism in the second and third decades of the twentieth century. Although reared in the atmosphere of liberal theology with its anthropocentric focus on the basic goodness of humanity and its belief in the progress of the kingdom of God as a social entity on earth, Niebuhr's sensitive spirit was soon attracted to those advocates of a more realistic view of Christianity and the world: the empirically oriented religious realists of America, above all his chief mentor at Yale, Douglas Clyde Macintosh, and the Biblically oriented crisis theologians of Europe, especially Karl Barth and Paul Tillich. However, Niebuhr never lost the social concern characteristic of liberalism or succumbed to the pessimistic otherworldliness he detected in some of the realists. His career might be described as a bold attempt to forge a middle way that blended the ethical passion of the liberal tradition with the theocentric perspective of what came to be called neoorthodoxy.

Niebuhr once referred to Ernst Troeltsch and Barth as theologians who had been his teachers through their writings. Troeltsch's philosophy of religion was the subject of Niebuhr's doctoral dissertation at Yale, and it was from Troeltsch that he inherited the problem of historical relativism, along with the importance of sociological analysis. The latter is evidence in Niebuhr's first book, *The Social Sources of Denominationalism* (1929), in which he demonstrated that the divisions represented by American denominations are due primarily to socio-historical rather than doctrinal differences—that is, to such factors as national background, race, class, and economic status.

Niebuhr utilized Troeltsch's and Max Weber's distinction between the church and the sect to show how important the differences in the sociological structures of religious groups are in determining their doctrine. Whereas churches are inclusive, socially established, culture ac-

commodating institutions into which members are born, sects are exclusive, discipline demanding, often persecuted associations of minorities that require joining. Niebuhr pointed out that in Protestant history it has been the sects, made up of the poor and the disenfranchized, whose revolts have brought about religious change, but that by their very nature sectarian types of organization are valid for only one generation. As children are born and disciplined asceticism increases wealth, compromise with the world becomes inevitable. So the sect becomes a church. Illustrations of the phenomenon, Niebuhr indicated, are rampant in American denominational history.

Unlike Troeltsch, who remained a not uncritical liberal, Barth was the Swiss theologian who during the 1920s led an all-out assault on the presuppositions of liberal theology, stressing that the God of the Bible is wholly other than humanity and is known only through his self-revelation in the history of Israel and definitively through Jesus Christ, a revelation received through the eyes of faith alone. Although Niebuhr appreciated Barth's prophetic protest against the anthropocentricity of liberalism and his powerful recovery of the Reformation themes of human sin and divine grace, he was more attracted to the "belief-ful realism" of Paul Tillich, who while emphasizing the transcendence of God related him to historical experience in a way the early Barth did not. Niebuhr helped to introduce Tillich's theology to America by translating his *The Religious Situation* (1932). Later Niebuhr launched his own attack on liberalism in a book entitled *The Church Against the World* (1935a; 1935b). In it he called for the church to cast off its bondage to the cultural gods of capitalism, nationalism, and humanism, and to return to faith in God alone.

If in his first book Niebuhr disclosed the social factors underlying the formation of Protestant denominations, in *The Kingdom of God in America* (1937) he sought to understand the theological dynamic that propelled the Protestant movement itself. This dynamic he discovered to be the idea of the kingdom of God, understood in the sweep of American history as the sovereignty of God (early Puritan period), the reign of Christ (the Great Awakening and revivals of the eighteenth century), and the kingdom on earth (nineteenth century social gospel). Helped by his study of American Protestantism as well as by the great theological tradition of

Augustine and Aquinas, Luther and Calvin, Pascal and Edwards, Niebuhr himself became convinced of the absolute sovereignty of God, and, concomitantly, of the universal sinfulness of humanity and the mystery of divine grace. Moreover, he came to the conviction that Christianity must be understood not as an institution or a series of institutions but as a dynamic movement involving both worship and work, a continual dialectical movement toward God and toward the world that demands of the church a constant *metanoia* or revolution.

Many consider Niebuhr's finest theological treatise to be *The Meaning of Revelation* (1941), in which he dealt with the problem of how the absolute God can be revealed within the relativities of history. Following Luther's dictum that God and faith belong together, Niebuhr argued that revelation is not an event of external history that any disinterested observer can see, but an event of inner history that illumines and makes intelligible the entire history of the self. That is, revelation involves faith, but Niebuhr pointed out that although the perspective of faith is historically and culturally relative, the objectivity of the God perceived is corroborated by the experience of others in the community. For the Christian community the God who is revealed in Jesus Christ is the infinite person who is its knower, its author, its judge, and its only savior. God, insisted Niebuhr, is the solely absolute One to whom all else is intrinsically related, the One whose power is shown in the weakness of Jesus and whose goodness is the simple everyday goodness of love.

Ten tumultuous years of war and reconstruction passed before Niebuhr published *Christ and Culture* (1951), his best-known work. In it he analyzed the differing interpretations given by Christians to the interplay between revelation and cultural forces, between the claims of Christ and the demands of the world. Niebuhr expanded the Troeltsch-Weber sociological typology of the church and the sect into a five-fold pattern, ranging from those Christians who set Christ against culture to those who accommodate Christ to culture. Between these extremes of radical separation and cultural accommodation, Niebuhr proposed three mediating positions that represent the majority of Christians: the synthesist position, which affirms both Christ and culture but makes clear Christ's exalted position above culture; the dualist, which holds Christ and culture in the tension of a paradoxical relationship; and the conversion-

ist, which believes that Christ transforms culture. In setting forth the strengths and weaknesses of each position, Niebuhr made clear his own preference for the conversionist type of Christianity.

In 1953 Niebuhr, in collaboration with Daniel D. Williams and James M. Gustafson, directed a major study of theological education in the United States and Canada. Of the three volumes and numerous bulletins that resulted, Niebuhr himself authored *The Purpose of the Church and Its Ministry* (1956), in which he defined the purpose of the church as the increase among humans of the love of God and the love of neighbor, and indicated an emerging conception of the minister as pastoral director, whose task was both to edify the congregation and to enable it to carry out its ministry in the world.

The theocentricity of Niebuhr's theology is best depicted in *Radical Monotheism and Western Culture* (1960), in which he showed that monotheism in Western history is in constant battle with the polytheistic and henotheistic cultural faiths. Niebuhr's final work, published posthumously from sets of lectures, was *The Responsible Self* (1963). In this essay in Christian moral philosophy he centered his understanding of human life around the symbol of responsibility and advocated an ethics in terms of the fitting as a response to the action of another, rather than of the good as a goal or the right as a duty.

In summary, the distinguishing elements of Niebuhr's thought are radical monotheism, historical relativism, and ethical transformationism. His confessional theology always involves God, the self, and the self's companions. It is based upon the revelation of God in Christ and calls for the response of a radically theocentric faith that transforms the world's idolatrous faiths and evokes loyalty to the divine cause of reconciliation.

JOHN D. GODSEY

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NIEBUHR, REINHOLD

Reinhold Niebuhr (1892–1971) was a dominant Protestant social philosopher of North America in the twentieth century. His many works stressed the relationship between religious sources of wisdom and practical social action. George F. Kennan, referring to a group of political thinkers identified as “realists,” called Niebuhr “the Father of us all,” and Hans J. Morgenthau, leading theorist of international relations, said Niebuhr was “the greatest living political philosopher of America.”

Niebuhr was born in Wright City, Missouri, on June 21, 1892, and christened Karl Paul Reinhold Niebuhr. His father, Gustav Niebuhr, who had emigrated from Germany, was a pastor in the Evangelical synod of North America. His mother, Lydia Niebuhr, was the daughter of Edward Hosto, also a pastor of the Evangelical

synod. In 1931, Niebuhr married Ursula M. Keppel-Compton. His sister Hulda became a professor at McCormick Theological Seminary, and his younger brother H. Richard Niebuhr became a professor of ethics at Yale Divinity School and to this day remains his only rival for influence in the field of Protestant ethics in the United States.

Reinhold Niebuhr graduated from Elmhurst College and Eden Theological Seminary, then unaccredited schools of the Evangelical synod. In 1914 he received his bachelor of divinity degree from Yale University and in 1915 received the Yale M.A. with a thesis entitled “The Contribution of Christianity to the Doctrine of Immortality.”

From 1915 to 1928 he served as pastor of Bethel Church in Detroit where, after the Detroit race riots of 1925, he became involved in the struggle for social justice, serving as the chairman of the mayor’s race committee. His experience with the plight of industrial workers led to the publication of several articles critical of the Ford Motor Company in the liberal *Christian Century*. His first book, *Does Civilization Need Religion?* (1927), criticized the liberal and social gospel movement thought that he had learned at Yale as inadequate to the social crisis of industrial civilization. His diary of his years in the parish, *Leaves From the Notebook of a Tamed Cynic* (1929), shows his great love for pastoral work and his early wrestling with issues of social concern. In World War I, he identified with Woodrow Wilson’s politics and wartime aims and served as chairman of the Evangelical synod’s war welfare commission, which involved him deeply in support of the war.

He joined the faculty of Union Theological Seminary in 1928 and remained until his retirement in 1960. His social thought took on depth and power during the late 1920s and 1930s. In the depression of the 1930s, he used many Marxist categories to express his pessimism about capitalist society. His most important book, *Moral Man and Immoral Society* (1932b), is a critique of liberal–capitalist culture and an appeal for a realist ethic adequate to inspire social action. The work stresses sharply the need to distinguish between the contribution of religion to a personal ethic and to social ethics. He was a friend of Norman Thomas and vice chairman of the Socialist party.

Reflections on the End of an Era (1934) is his most Marxist-influenced work. His affirma-

tion of key conceptions of Marxist social philosophy was always balanced by a religious and ethical critique of the weakness of Marxism.

In 1940 he turned from socialism to support the election of Franklin D. Roosevelt. From that time on he labored for liberal-radical causes within the Democratic party. He was a founder and chairman of Americans for Democratic Action and vice chairman of the Liberal party in New York state.

His most important theological work, *The Nature and Destiny of Man*, was published in two volumes in 1941 and 1943. The work stresses the grandeur and misery of the human condition and criticizes the one-sided optimism of liberal culture. These volumes of Christian anthropology earned him the reputation of chief American spokesman for a theology called Christian realism. Using an Augustinian-Reformation perspective, he tried to ground social ethics and political action on a new foundation. He continued this emphasis in volumes on the philosophy of history, *Faith and History* (1949) and *The Self and the Dramas of History* (1955), and in a volume on democratic political philosophy, *The Children of Light and the Children of Darkness* (1944).

In addition to teaching, lecturing, and preaching almost every Sunday, Niebuhr founded the Fellowship of Socialist Christians, which evolved into Christian Action as his political philosophy changed. In 1935 he founded the quarterly *Radical Religion*, which became *Christianity and Society*, lasting until 1956. He also founded *Christianity and Crisis*, a biweekly journal that dealt with cultural and political interpretation; it continues to be published with some of his former students serving on the editorial board.

Niebuhr suffered a stroke in 1952 that left him partially paralyzed and slowed down his active involvement in social movements, political activities, and church affairs. He continued to write at least two articles or editorials each week. As the cold war developed he wrote such interpretations of American history as *The Irony of American History* (1952), *Pious and Secular America* (1958a), *A Nation So Conceived* (Niebuhr & Heimert 1963), and *The Democratic Experience: Past and Prospects* (Niebuhr & Sigmund 1969). He became a polemical opponent of communism particularly in *Christian Realism and Political Problems* (1953). While a fellow of the Institute for Advanced Study in Princeton he wrote his major work on the theory of imperial politics, *The Structure of*

Nations and Empires (1959b). In all these works he attacked the tendency of liberalism to trust too easily in historical progress. He regarded the defense of democracy as a worthy cause and one that required sober judgments about the illusions of the self and one's own nation. As he had served briefly as an adviser to the Foreign Policy Planning Staff Committee in 1947, he was deeply committed to the necessity of countering Soviet power. He was enthusiastic about John F. Kennedy's foreign policy, both in its policies of strength and in its search to eliminate nuclear weapons. An early critic of the war in Vietnam, he spoke out against it during the Lyndon Johnson administration. He criticized his friend Hubert H. Humphrey sharply for going along with it and the vice president left Niebuhr's New York apartment stung by the criticism. In his later years, he spoke at Union Theological Seminary; though he needed assistance in walking, his scathing humor and irony inspired the students to acts of resistance to the war. He opposed the war on the grounds of natural law, including just war theory, political prudence, and moral failure.

The theological foundations of Niebuhr's perspective are most obvious in the published collections of his sermons, *Beyond Tragedy* (1937), *Discerning the Signs of the Times* (1946), and *Justice and Mercy* (1974). He characteristically applied insights from the prophets and the New Testament to illumine the sin, tragedy, and irony of the human situation. He defended the essential truth of the Biblical perspectives on humanity while using the insights of secular human wisdom. A pragmatist in epistemology, he was not as concerned about the source of a perspective as he was about its verifiability in the light of broad historical evidence. He continually stressed the distortion of human actions by human anxiety. The ultimate answer to human anxiety was the acceptance of self as finite and limited but loved by God. Anxiety produced pride, and the antidote to prideful self-assertion was basic trust, which could be restored by the acceptance of grace.

In the struggles of history, however, the structures of human nature, nation-states, and international relations were rooted in anxiety and insecurity. Justice, or a fair distribution of the opportunities of human relations, could best be secured by a balance of power that prevented the inordinate aggrandizement of anyone or any group. Niebuhr did not expect peace within history or justice within any nation. Peace and

justice were goals to be sought, but they could be completely fulfilled only in the Kingdom of God. In history, peace and justice would only be realized fragmentarily. His life was a testimony to the style of ethics he taught—that one acts for justice because one is obligated to because it is part of true human nature so to strive. A bit more justice may be realized, but one acts for justice because it is right to do so, not because one expects to achieve it. The realism with which his name is associated is the recognition that the very structure of human nature resists the realization of justice while promoting the search for justice.

In his last years he published *Man's Nature and His Communities* (1965) as a summary and review of the major themes of his work. The book is less explicitly theological than his *magnum opus*, *The Nature and Destiny of Man*, but it presents the same view of human nature in terms close to those of the ego psychology of Erik H. Erikson. Social theory is the interaction between the ideal possibilities that humanity projects and the struggling communities in which it lives. He applied this analysis in 1969 in "The King's Chapel and the King's Court," an essay attacking the injustice and religious pretensions of the Nixon administration, which drew more critical response from his political opponents than anything else he had written. Niebuhr died on June 1, 1971, in his home at Stockbridge, Massachusetts.

RONALD STONE

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NOTESTEIN, FRANK W.

Frank W. Notestein was born on August 16, 1902, and retired from his last full-time position in 1968. During these 66 years, demography developed from a subject of occasional notice

by statisticians and economists to a flourishing discipline with its own journals, research organizations, and national and international professional societies. This coincidence in timing between Notestein's career and the growth of demography is not wholly accidental. The ideas he contributed are at the core of modern thinking about population; he was the first to introduce a program of research and graduate training at an American university, and he instilled a scientific attitude and professional competence in a long list of advanced students from many countries. He also created, or helped to create, several of the institutions that provide leadership in scholarship, the formation of policy, and technical assistance in matters relating to population.

One can never judge what would have happened if a particularly creative person had not existed; it is always possible that his accomplishments would have emerged from other hands. Thus there are two interpretations of what Notestein has contributed to the better understanding of human populations: either he has been an indispensable catalyst, who greatly accelerated the development of the field, or he was fortunate enough repeatedly to be the right man at the right place at the right time.

Although he was born and raised in Alma, Michigan, Notestein's family background is too intellectual by far to be considered typical of a small midwestern town. His father, Frank Newton Notestein, was a professor of mathematics and dean of the faculty of Alma College; and his grandfather, Isaac Notestein, taught at an academy that his great-grandfather, Jonas Notestein, had founded. His mother, Mary Elizabeth Wallace, was the daughter of a generous founding contributor to Wooster College.

Frank Notestein spent his freshman year at Alma College, but he transferred to Wooster College to study economics. Upon his graduation in 1923, he entered Cornell University. In his last year at Wooster, he became engaged to his classmate, Daphne Limbach. They were married four years later when Notestein completed his thesis, for which she drew the charts.

The dominant figure in economics at Cornell in the 1920s was Herbert J. Davenport, an outstanding exponent of the neoclassical utilitarian theory of value and price, which Notestein found uncongenial because he thought it tautological and futile. He respected the rigor of Davenport's thinking and the skill of his teaching, and ap-

parently Davenport supported Notestein with enthusiasm in spite of their differences. Notestein found the more institutional and empirical approach of Morris Copeland (then an assistant professor) more congenial. The professor at Cornell who influenced Notestein the most was Walter F. Willcox, who taught statistics and sociology. Willcox, who had developed an interest in statistics after his own formal graduate training was completed, did not know mathematics or modern statistics. For training in statistics, Notestein had to take work with Frank A. Pearson at the College of Agriculture. Willcox did appreciate the value of simplicity, and the importance of knowing one's basic data, attitudes that Notestein later implanted in his own graduate students and that his own work always reflected.

Willcox was an adviser to the Milbank Memorial Fund, which was claiming success in dropping the death rate from tuberculosis in Catteraugus County, New York, an area the fund had selected for health demonstrations. Willcox asked Notestein (while the latter was still a graduate student) to review these claims. To the annoyance of the head of the fund, Notestein found that death rates from tuberculosis were dropping about as fast in control counties, with no special program, as in Catteraugus. A short time later Edgar Sydenstricker joined the fund, having just completed pioneering statistical work in the U.S. Public Health Service and the League of Nations. Sydenstricker offered Notestein a summer job compiling historical vital statistics for Catteraugus and Syracuse counties. These data formed the basis of Notestein's PH.D. thesis.

With the thesis completed the Notesteins sailed to Europe where he had a one-year Social Science Research Council fellowship to study occupational mortality in London, Frankfurt, and Geneva. Just before the fellowship was to end, Notestein was offered a job at the Milbank Memorial Fund. In the fall of 1928, as a research associate, he began a study of differential fertility by occupational class.

In his years at the Milbank Fund (1928-1936) Notestein began to formulate some of his most influential contributions to demographic thought. These contributions yielded a better understanding of the large reduction in birth and death rates that had occurred in the nineteenth and early twentieth centuries in the economically and technologically more advanced

countries. Notestein foresaw, before almost anyone else, that a similar transformation would occur if and when the less advanced areas experienced economic and technological progress.

In the 1920s and 1930s there was still disagreement between biologists and biometricians (such as Raymond Pearl and Corrado Gini) who thought that population trends responded mostly to biological influences, especially population density, and social scientists who looked for social explanations for the decline in fertility that was evident throughout Europe, North America, Australia, New Zealand, and Japan. The social scientists in turn were divided between those like William Fielding Ogburn and Henry Pratt Fairchild, who attributed the decline in fertility and class differences in fertility to the diffusion of effective contraceptive technology throughout society, beginning within the more educated and better informed urban upper classes and gradually spreading to less educated, lower socioeconomic categories, and those like René Dumont, LeRoy Beaulieu, and Alfred Landry, who emphasized the changing attitude toward procreation in societies less dominated by tradition, more open to social advancement, providing more opportunities for women outside the home, and with newly established traditions (and laws) of universal education.

Notestein's particular contribution was to use direct evidence to show the importance of attitude rather than biology or contraceptive technique. He analyzed the records he gathered in a follow-up study of patients from the Margaret Sanger Clinic in New York (Stix & Notestein 1940). This study showed that after allowance for gross pathology, there was no significant group differences in conception rates in the absence of contraception, and that in the absence of contraception twentieth-century American women would have had birth rates as high as any on record. Most significantly, they found that *coitus interruptus* was widely used in pre- and postclinic experience, and was quite effective. Another piece of empirical information was derived from an experiment conducted by Gilbert Beebe in an Appalachian coal mining area with the support of the Milbank Fund. Beebe made a simple contraceptive method available and found some accepters, but there was no detectable residual effect in a follow-up three years later.

From these observations Notestein concluded that it had been a change in attitude, not tech-

nological invention alone, that had led to lower rates of childbearing. Enhanced motivation to restrict fertility could just as well be called the cause of modern methods as modern methods could be called the cause of the restriction. Clearly both were important. These inferences from the analysis of the records of birth control clinics led Notestein to a clearer and more soundly based statement of "the demographic transition" than the earlier formulations by Dumont, Beaulieu, Landry, Charles P. Blacker, and Warren S. Thompson. Notestein's first statement appeared in chapter 15 of *Controlled Fertility* (Stix & Notestein 1940); his most fully developed version is contained in "Economic Problems of Population Change" (1953).

Notestein perceived at an early date that the relation between social change and population trends (the demographic transition) that he and others found in the history of the economically more advanced countries created a potential for very rapid growth in the less developed countries. His graduate students before World War II learned that the expected effect of economic progress in areas such as India or Indonesia was a reduction in the death rate long before there was any fall in the birth rate, and a resultant period of very rapid increase in population. Because some of the areas in which rapid growth was a prospect were very poor and already very densely settled, he feared that these demographic tendencies would themselves be an impediment to progress (Notestein 1944; 1945). These prophetic ideas were published long before most of the world had any notion that an explosive growth of numbers in the less developed countries was about to begin. A special instance of his foresight was his study of population problems in Palestine (Notestein & Jurkat 1945). Notestein and his colleague Jurkat noted that although the current rates of increase were about the same for Jews and Arabs, the potential for growth was much greater for the Arab populations. The Jews were rapidly increasing in number because new migrants had brought a concentration of young adults of childbearing age. Their lifetime reproductive behavior, however, was European, not Asian. The death rates of the Arabs were falling rapidly as a result of the new prosperity and health programs of the British Mandate established by the League of Nations, but their birth rates were high because of the continuation of traditional attitudes and practices. Notestein was asked to

testify before the Anglo-American Commission on the Future of Palestine. Years later the attorney for the Zionists told him that his testimony had helped the Jewish leaders decide in favor of the partition of Palestine.

The most distinctive feature of Notestein's career is his quiet success in building institutions of exceptional quality and influence, most notably the Office of Population Research at Princeton University, the Population Division of the United Nations, and the Population Council.

The Office of Population Research was founded in 1936 when Notestein came to Princeton from the Milbank Memorial Fund to become the first director of the office. It was the first university-based unit for research and graduate training in demography, and began with its director, a secretary, a research assistant, and a graduate student who held a newly established Milbank Memorial Fund fellowship. Although it never had a very large staff (a maximum of 10 or 12 professional demographers; at most times fewer), the office established a solid reputation for innovative work on diverse aspects of demography: the causes and consequences of population trends, particular studies of the population of many countries and regions, contributions to formal and mathematical demography, methods of measurement and projection, and analytical and empirical research on fertility, mortality, nuptiality, and age structure. The staff that Notestein recruited over the years at Princeton—among them Irene Taeuber, Frank Lorimer, Dudley Kirk, Kingsley Davis, John Hajnal, Robert Potter, and Charles Westoff—and the students he trained—among them John Durand, Norman Ryder, Harvey Leibenstein, Alvaro Lopez, and Paul Demeny—would find a place in any history of the development of the modern study of population. Both colleagues and students were in some degree infected by his skepticism, respect for evidence, and insistence on rigor and technical competence.

In 1946 Notestein, on half-time leave of absence from Princeton for two years, helped to establish, as its first director, the Population Division of the United Nations. He somehow succeeded in having fact-gathering and scientific analysis accepted as major functions of the division, in spite of the usual predominance of procedural and political matters within the large bureaucracy of the United Nations Secretariat. He fostered within the division the standards of preciseness and objectivity that he tried to maintain at the Office of Population Research.

Partly as the result of the impetus he gave to the division, some of the world's leading demographers have served with it and they have made discoveries and enriched the literature with some of the most important contributions of the period.

The Population Council cannot be counted as Notestein's creation. It was founded in 1952 by John D. Rockefeller, 3d, who assumed the presidency of this small organization limited at first to modest support for demographic research, biomedical research on human reproduction, and providing fellowships for advanced training both in demography and the biomedical field, primarily to candidates from less developed countries. The next president of the council, Frederick Osborn, shares with Notestein an outstanding record of successful promotion and statesmanship in demography. When Osborn retired in 1959, Notestein succeeded him as president of the council.

Notestein came to the Population Council at an opportune moment. In its early years most of its funds were personal contributions by John Rockefeller, but by 1959 it was receiving support from the Ford and Rockefeller foundations.

In 1959, and for the first few years of Notestein's presidency of the council, the United States government and the United Nations were still barred from technical assistance in family planning, and the Population Council was the only organization in the world from which countries could ask for help.

Under these conditions, and with Notestein's leadership, the Population Council became a critical component in the expansion of technical assistance, in the extension of training both in demography and the biomedical aspects of reproduction, and in the development of these two scientific fields. It was of great assistance in launching family planning programs in South Korea, Taiwan, and many other countries. It set up systems of continuing evaluation of these programs that were effective and have been useful models for others. The biomedical research program at the council provided intellectual leadership in an area that in 1959 had few resources in money or personnel. Now that financial support is on a much larger scale, and many laboratories are working in the field, the council program is still influential. An instance of the practical impact of this program is its funding (about 95 per cent) of the development of the intrauterine contraceptive device. In 20 years the cumulative number of Population Council fel-

lows reached approximately 1400, of whom 1150 were from Asia, Africa, and Latin America. A substantial fraction of the professional demographers in the less developed countries are Population Council fellows. Other, larger programs financed by governments are building on the foundations established by the Population Council.

ANSLEY J. COALE

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O

OAKESHOTT, MICHAEL

Michael Oakeshott is a political theorist who does not fit into any of the usual categories; he is a traditionalist with few traditional beliefs, an "idealist" who is more skeptical than many positivists, a lover of liberty who repudiates liberalism, an individualist who prefers Hegel to Locke, a philosopher who disapproves of *philosophisme*, a romantic perhaps, and a marvelous stylist. He was born in England in 1901, the son of a Fabian-socialist writer, Joseph Oakeshott. He was educated at St. Christopher's, a coeducational school, and at Gonville and Caius College, Cambridge. He took a first class honors degree in history (1923) and became a fellow and history tutor at this college (1924-1939). After army service in World War II, he taught at Oxford University, becoming a professor of political science at the London School of Economics (1952-1969). It was during private visits to Germany during the interwar years that he developed his interest in Hegelian philosophy.

The argument of his earliest important work, *Experience and Its Modes* (1933), undoubtedly owes a certain amount to Hegel and to the theory of truth as coherence. Oakeshott depicts the philosopher's task as the resolution of the inconsistency in any set of concrete images so as to make it more intelligible. Ordinary or common-sense views may be used as a point of departure, but they are adopted only to be examined and criticized, with the aim of moving beyond them to a more comprehensive perspective. Philosophy, in this view, neither seeks nor desires a stopping place. It is not the con-

struction of a complete system of knowledge, but rather a method or way of thinking. Its objective is nothing more ambitious than understanding. Philosophy, for Oakeshott, is "explanation."

Oakeshott notes that men have an incurable tendency to look at the world through limited perspectives known as "modes" of experience. Each "mode" constructs a specific and homogeneous picture that is not a part of the real world, but of the whole of experience as it is seen from a given point of interest. Oakeshott sees no theoretical limit to the number of such abstract worlds, but he distinguishes four as being particularly familiar and fully formed: practice, science, history, and poetry. Each of these "modes," Oakeshott suggests, has its own validity, but all are in different ways limited, and none is able, in its own terms, to understand its limitations. Only the philosophical perspective is able to transcend the shortcomings of these several "modes," and reinterpret them from the standpoint of experience as a totality. The philosopher seeks to bring out the logical form of each particular "mode" with a view to perceiving the degree and limitations of the coherence achieved. But, Oakeshott adds, philosophy is "useless to men of business and troublesome to men of pleasure. . . . Philosophy is not the enhancement of life" (1933, p. 355).

In his later writings, notably *Rationalism in Politics* (1962) and *On Human Conduct* (1975), Oakeshott applies his philosophical method to the study of politics, so that his first question becomes: What is the principle of coherence to be sought in the interpretation of political ac-

tivity? Oakeshott considers, and rejects, two well-known ways of characterizing political behavior. First, he repudiates the notion of politics as an *ad hoc* activity, for this, he thinks, is to represent politics as something entirely capricious, which it is not. Secondly, he rejects the more exalted and very fashionable belief that politics is an activity which may be guided by an independently premeditated plan or set of principles. Some of the most telling, and often quoted, passages in Oakeshott's writings are directed against this vision of politics, which he likes to call "rationalist."

Since the word "rationalism" has several meanings, it is hardly surprising that these arguments of Oakeshott have been misunderstood. He is certainly not attacking reason, but rather a kind of intellectualism or what he has sometimes called *philosophisme*. The kind of rationalist Oakeshott criticizes is the man who thinks he can apply intellectual blueprints to the world of politics, who imagines he can solve concrete problems by the light of abstract generalizations, and who seeks, in effect, to introduce into politics the method of the *polytechnicien* or engineer.

Against such belief in the sovereignty of technique, Oakeshott insists on the importance of practical knowledge, which, he claims, is largely traditional knowledge. Political understanding comes as a result of being apprenticed to and participating in an activity, and so "using the resources of a traditional manner of behaviour in order to make a friend of every hostile occasion" (1962, p. 127).

Oakeshott speaks of politics as "the activity of attending to the general arrangements of a set of people whom chance or choice has brought together" (1956, p. 2). Any group may have its politics, but the word "politics" is used primarily in connection with those associations known as states. Like all procedures among men, the rules and institutions of states are most useful when they are familiar and are not altered excessively. Not that he sees anything sacrosanct in such rules and institutions; all are "susceptible of change and improvement." But such possible improvements are prompted neither by caprice nor by abstract principles, but derive from the recognition of specific and ascertainable defects in what already obtains. Thus political activity is a matter of amending existing arrangements to make them more coherent.

An example Oakeshott takes from contemporary history is the enfranchisement of women

in England. He suggests that this was not enacted because Parliament recognized any natural or human right to equality of suffrage, but because Englishwomen had already, as a result of the Married Women's Property Act and of various wartime changes, achieved by 1918 equality with Englishmen in so many other fields that it was anomalous, or inconsistent, for them to remain excluded from the voting register. Their enfranchisement was a result of Parliament's recognition of "an incoherence in the arrangements of the society which pressed convincingly for remedy" (1956, p. 13). He insists that "there is of course no place in civil association for so-called 'distributive' justice. . . . *Civil rulers* have nothing to distribute" (1975, p. 153).

This emphasis of Oakeshott on the "politics of repair" and on statesmanship as "choosing the least evil" has prompted some readers to see him as another Edmund Burke. But this is a mistake. Burke, like most conservative political theorists, is a champion of the Christian order, natural law, and the right to property. Oakeshott, who carries the skepticism of his philosophy into his politics, has no belief in such metaphysical abstractions. His kindred spirit is not Burke, but David Hume. Like Hume, Oakeshott is conservative as a result of his doubt. Hume relied on tradition, habit, and custom precisely because he could see nothing else to rely on: no God, no natural law, no rights. But Hume did not make the mistake of elevating custom and tradition into sacred substitutes for God and natural law. His skeptical conservatism was open, undogmatic, and splendidly tolerant.

This is equally true of Oakeshott's conservatism. Opposed to all ideology, he cannot, and does not, share the ideological conservatism of Burke and his successors. This attitude, which is required by Oakeshott's theory, is clearly also part of his natural disposition. No one can read his writings without being struck by his manifest devotion to freedom. Whatever his debt to Hegel in other fields, Oakeshott owes nothing to Hegel in his understanding of freedom. Oakeshott's notion of freedom is the plain man's, or rather the plain Englishman's notion. Freedom is something to be defended against the régime and against any other great concentration of power. Although Oakeshott has sometimes criticized forms of theoretical individualism, his freedom is the freedom of the individual. "The 'masses' as they appear in modern European history," Oakeshott writes, "are not composed of

individuals, they are composed of 'anti-individuals' united in revulsion from individuality" (1970, p. 113).

He argues that freedom emerged in medieval times with the sense of individuality. Individuality demanded a government strong enough to enable the individual to escape from communal or other established pressures; a government that could maintain order and create new rights and duties appropriate to the interests of individuality, but which at the same time was not so powerful that it would itself constitute a new threat to those interests. Legislative bodies arose to make laws favorable to the individual and to establish spheres of private activities (or liberties) in which the individual could act without interference. Government acted like an umpire, administering the rules of the game without taking part, intervening only to settle collisions of interest among the players. Such is the chief characteristic of what Oakeshott speaks of as "parliamentary government."

The rise of the "anti-individual" goes together with what Oakeshott calls "popular" (as opposed to "parliamentary") government. The popular system looks to the establishment of universal adult suffrage to confirm the authority of mere numbers or the mass man. The parliamentary representative is seen, not as an individual, but as an instructed delegate whose function is to assist the creation of a society appropriate to his masters; mass parties grow up composed of "anti-individuals" and are dominated by their leaders. But in all this the mass man does not make his own choice; he does not really give a mandate to his leaders. The so-called representative draws up his own mandate and "by a familiar trick of ventriloquism" put it into the mouths of his electors (1970, p. 105). Similarly, the favorite device of "popular" government, the plebiscite, is not a method by which the mass man finally achieves release from the burden of individuality; he is told emphatically what to choose. Oakeshott adds that the style of general political discourse most suited to "popular" (as opposed to "parliamentary") government tends naturally to be the idiom of ideology or, as he calls it, rationalism.

Oakeshott's belief in the superiority of the parliamentary form of government goes together with his special feeling for England and the English tradition of parliamentary government. But he is not, as is often said, the successor of T. H. Green, Bernard Bosanquet, and F. H. Bradley in the English idealist school. Green, in par-

ticular, is an example of the high-minded ideology that Oakeshott most mistrusts. Oakeshott's "Hegelianism" comes straight from Hegel without any mediation from Victorian Oxford. Oakeshott's affinities are altogether closer to several continental theorists, to Wilhelm Dilthey, for example, and Benedetto Croce. And just as Croce, who always reckoned himself a liberal, proved when it came to the test surprisingly conservative, so Oakeshott, the professed conservative, turns out to be remarkably liberal (or "libertarian" as he would rather have it), in the sense of one who loves liberty for the sake of liberty.

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OHLIN, BERTIL

Bertil Gotthard Ohlin (1899-1979) grew up in Klippan, Sweden, where his father was a lawyer and a district police superintendent. At the University of Lund, to which he was admitted at the age of 16, he studied economics, statistics, and mathematics. In 1917, he transferred to the Stockholm School of Economics and Business Administration, where Eli F. Heckscher was professor of economics. One year later, he became a member of the National-ekonomiska Klubben, an exclusive club of

economists, whose most prominent members were Heckscher, David Davidson, Knut Wicksell, and Gustav Cassel. After passing his examination in 1919, Ohlin wrote reports for a government committee on the development of three Swedish industries, with special reference to the effects of tariff policy.

In the early 1920s, Ohlin completed his military service and continued his studies under Cassel at the University of Stockholm. In 1922, he presented a brief outline of the theory of international trade, which Cassel approved as a qualification for the licentiate degree. Ohlin subsequently attended Cambridge University for a few months, and Harvard University for the academic year 1922/1923. At Harvard, he met and was influenced by Frank W. Taussig and John Williams. Having returned to the University of Stockholm, he defended his doctoral dissertation in 1924. In this thesis (1924), he extended the Walras-Cassel mutual interdependence price system so as to hold for a system of two or more regions trading with each other.

From 1925 to 1930, Ohlin held a chair as professor of economics at the University of Copenhagen. In 1930, he returned to Sweden to succeed Heckscher at the Stockholm School of Economics and Business Administration; he held this chair until 1965. During 1930 and 1931, Ohlin completed his magnum opus, *Interregional and International Trade* (1933a), and wrote a study on the world economic depression for the League of Nations (1931a). A government committee on unemployment also commissioned him to prepare a theoretical monograph on monetary policy, public works, subsidies, and tariffs as means against unemployment (1934). This report shows the influence on Ohlin of the works of Wicksell, Erik Lindahl, Gunnar Myrdal, and John Maynard Keynes. Central traits of the theory in Ohlin's study were similar to Keynes's analysis in *The General Theory of Employment, Interest and Money* (1936).

In Sweden, Ohlin is probably better known as a politician than as an economist. He was chairman of the Folkpartiets Ungdomsförbund (Liberal Youth Federation), 1934-1939; a member of the Swedish parliament, 1938-1970; leader of the Folkpartiet (Liberal party), 1944-1967; and minister of trade, 1944-1945. From 1946 to 1967, except for one brief period, the Folkpartiet was the leading opposition party. As a politician, Ohlin characterizes himself as a social liberal.

Ohlin's intense preoccupation with politics

since the middle of the 1930s has not prevented him from making new contributions to economics. In 1947, he lectured at Oxford University and at Columbia University on the theory of economic stabilization (1949). He contributed to the discussions concerning the possibility of harmonizing social legislation and the system of taxation within economic unions (1956; 1958; 1965), and demonstrated facts versus popular views on income distribution between labor and capital.

In 1977, Bertil Ohlin and James E. Meade were awarded the Alfred Nobel memorial prize in economic science "for their pathbreaking contributions to the theory of international trade and international capital movements." *Interregional and International Trade* was mentioned as Ohlin's principal contribution.

Interregional and international trade. The theoretical model given by Ohlin in *Interregional and International Trade* is based on the simplified Walrasian equilibrium model for a closed economy presented by Cassel in his *Theoretische Sozialökonomie* (1918). In the early 1920s, Ohlin set out to extend Cassel's mathematical model by analyzing the effects of trade on two regions or countries. The first result was a model that is published in Ohlin's dissertation (1924). In this model, commodities can move without cost between regions, but the factors of production of one region cannot move to the other region; each region has its factor endowment. Differences between the regions with regard to "the relative scarcity" of productive factors cause differences in relative prices when the regions are isolated from each other. These price differences, in turn, make interregional trade profitable, unless impediments to commodity movements prevent it. The trade causes an equalization between regions not only of commodity prices but, to some extent, also of factor prices.

In the preface to his dissertation, Ohlin pointed out that in several essential respects, these results coincide with those in an article by Heckscher (Heckscher 1919). He remarked that although his point of departure (the combination of two-price systems of mutual interdependence using Cassel's one-market equilibrium model) was different from Heckscher's comparative cost analysis, he had been much influenced by Heckscher's article.

On the basis of his mathematical model, Ohlin developed a more comprehensive theory of trade in his dissertation. He discussed the sup-

ply reactions of the factors of production as well as the economies of scale; he studied the effects on trade of import duties and the cost of transportation between regions; he dropped the assumption that factors of production cannot move from one region to another; and in particular, he analyzed the causes and effects of capital movements under various conditions, including fixed and flexible exchange rates. Finally, he indicated what he viewed as the defects in the classical theory of international trade and criticized the classical value theory as a basis for a theory of trade.

In 1929, Keynes wrote an article on the German transfer problem in which he concluded that the Germans had to reduce the prices of their exports if they were to increase the sale of their products, and thus be able to pay larger indemnities (Keynes 1929). To this, Ohlin (1929) replied that Keynes had ignored the importance of the transfer of "buying power." Germany had borrowed much more than it had paid in reparation payments. This meant a considerable net addition to its buying power. Thus, one of the results of Germany's borrowing was to reduce its exports and increase its imports. In the absence of any further borrowing, Germany's reparation payments would lead to a transfer of buying power in the opposite direction, which would have an effect on trade.

In *Interregional and International Trade*, the basic mathematical model, which has since been widely referred to as "the Heckscher-Ohlin model," is developed in the first two chapters and in a mathematical appendix. Essentially, it is identical with the mathematical model in Ohlin's dissertation. In the remaining 23 chapters, the theory is modified and extended in various respects, and tested against empirical material. For example, the assumption of complete divisibility is dropped and large-scale economies are shown to be a cause of trade; exceptions to the rule that trade tends to equalize factor prices are given; the influence of trade on demand for goods and on supply of productive factors is studied; costs of transfer of commodities and interregional factor movements are introduced; theoretical aspects of location within a region are given; effects of dumping and import duties are analyzed; and the mechanism of international trade variations and capital movements is studied. As regards capital movements, the role of changes in buying power is stressed. Ohlin also discussed earlier

theories of international trade in two appendices.

Not long after its publication, *Interregional and International Trade* was recognized as a major contribution to international economics. For example, the Heckscher-Ohlin model was already an essential element in the first edition of Paul Ellsworth's well-known textbook (1938). Then, in an article in 1941, Wolfgang F. Stolper and Paul A. Samuelson discussed the effects of tariffs on real wages. As a point of departure they used the following proposition: "A country will export those commodities which are produced with its relatively abundant factors of production, and will import those in the production of which its relatively scarce factors are important. . . . And as a result of the shift towards increased production of those goods in which the abundant factors predominate, there will be a tendency—necessarily incomplete—towards an equalization of factor prices between the two or more trading countries" (pp. 58–59). Later, Samuelson (1948; 1949) analyzed this theorem more closely and found that the reservation "necessarily incomplete" is not strictly correct; under certain specified conditions, trade will lead to complete factor price equalization. However, among these conditions is at least one that is obviously unrealistic: that all commodities move perfectly freely in international trade, without encountering tariffs or transport costs.

The first proposition of what Stolper and Samuelson (1941) referred to as the Heckscher-Ohlin Theorem was subjected to an empirical test by Wassily Leontief in the early 1950s. Since it could hardly be disputed that the United States possessed more productive capital per worker than any other country, the theorem suggested that American relations with other countries should be based mainly on the export of "capital intensive" goods in exchange for "labor intensive" products. But Leontief's analysis of statistical material suggested the opposite. America's participation in the international division of labor was based on its specialization in labor intensive, rather than capital intensive, lines of production. Several alternative explanations of this so-called Leontief paradox have been given. Some of the most interesting have been accounted for by Bo Södersten (1970), who concludes that the discussion has provided a good deal of insight into the foreign trade position of the United States, but that it has hardly helped to establish or refute the Heckscher-Ohlin model of international trade.

Ohlin's *Interregional and International Trade* has influenced not only the development of international economics but also that of location theory and regional economics. Walter Isard (1956) referred to it as the first serious attempt to integrate trade and location theories. However, in Isard's opinion, Ohlin started at the wrong end. He should have developed a general localization theory first. Then, by focusing upon certain forms of immobilities of factors and goods, he could have developed his theory of interregional and international trade as a special case.

Theory of money, employment, and economic fluctuations. In his analysis of international trade variations and capital movements, Ohlin pointed out the role of changes in the buying power of the trading countries. He used the notion of buying power—or total monetary demand—also in his analysis of employment and price fluctuations within a country. It appeared in a pamphlet on stimulating production (1927*b*), where he also anticipated a part of R. F. Kahn's multiplier theory (Kahn 1931). In a paper on monetary theory (1933*b*), Ohlin also connected it with the then widespread idea that a discrepancy between savings and investment causes changes in the general price level. He referred to a situation where "the planned savings falls short of the planned investment," and made the important statement that a more straightforward way of describing this situation is to say that the sum of planned investment and planned consumption (i.e., total demand) exceeds total planned supply. In his application of the concept of total demand, which he found was due to the influence of Wicksell, Ohlin anticipated one of the aspects of Keynes's theory of 1936 (c.f., Ohlin 1974; Steiger 1976; Keynes 1936).

For Ohlin the comparisons between total demand and total supply were only the point of departure for a much more detailed analysis, which he presented in his 1934 report on employment policy. He realized that an extreme aggregation can be misleading, and made further comments in an article some years later (1941*b*).

Ohlin stressed the importance of process analyses combining what he, following Myrdal, called *ex post* and *ex ante* analysis. The main features of such a process analysis can be described as follows: Actual events during one period affect expectations for the future. The expectations more or less govern actions during the next period. These actions may be such that the expectations for this second period do not

all come true. The surprises affect expectations, which, in turn, influence actions during the third period, and so on (Ohlin, Robertson, & Hawtry 1937).

Critics of Ohlin's contributions to dynamic theory have pointed out that process analysis must be casuistic. In Ohlin's opinion, this is a true, but not a relevant, objection. He declared that a principal task of monetary theory is to describe processes (1933*b*), and some years later he wrote (1941*b*) that if we want to attain insight into economic reality, it is necessary to construct and study a set of typical cases where the economic development can be described and predicted on the basis of assumptions about the speed of reactions and the strength of tendencies.

Ohlin also contributed to macroeconomic theory by sketching alternative theories of the rate of interest (Ohlin, Robertson, & Hawtry 1937); anticipating Keynes's idea of a "liquidity trap" (1931*a*); contributing to the theory of public works as a means against unemployment (1934); and discussing the effects of overemployment (1949). Unfortunately, most of these books are only available in Scandinavian languages. Therefore, in contrast to *Interregional and International Trade*, they have not attracted the attention that they deserve.

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P

PARSONS, TALCOTT

The leading theorist in American sociology since World War II, Talcott Parsons (1902–1979) pioneered a new framework called action theory for thinking about modern man, culture, society, and their evolution. Its conceptual scheme connects Max Weber's and Émile Durkheim's contrasting sociological approaches to large-scale, modern society with learning theory, anthropological functionalism, and Sigmund Freud's psychodynamics. This unprecedented theoretical synthesis is the generative core for the many-sided development of action theory. Later in his career, Parsons also linked action theory with cybernetics, structural linguistics, input–output economics, and microgenetics, and he hoped that action theory would help strengthen the relationship between sociology and the other social–behavioral sciences in spite of growing specialization.

Parsons' sociology focused on the common and developmental features of the governing institutions of society; his conceptual notion of society was based on his more generic model of social systems, which sought to specify the self-maintaining elements of any organized form of human interaction. Any such interactive organization requires personal commitments by the prevailing participants to shared cultural norms that they spontaneously act to enforce and reinforce.

In the 1940s, Parsons devised a structural–functional strategy (a label he later eschewed) to simplify the modeling of the organizational complexities of societies. This approach is cen-

tral to all of his later work, and has been widely applied in sociology since the mid-1950s, when it spawned a vigorous school that made “Parsonian” a standard term in academic dictionaries. It has also been selectively used in anthropology, history, political science, and psychology, some areas of the humanities, and in applied fields ranging from psychiatry and administrative science to city planning, social work, and policy science.

Yet his “functionalism” is the most controversial part of Parsons' work, and it has generated a large body of critical literature. Opposition has been steadily voiced from every rival theoretical camp, varyingly criticizing its alleged untestability, overly analytical character, ahistoricism, oversocialized and conformist view of man, psychological reductionism, idealistic view of starker political realities, unreadability, static emphasis, status-quo bias, and anti-Marxism. But Parsons continued to develop his theories within the storm center, and his influence grew until the Vietnam War and the rise of the New Left in the late 1960s. As protests shook academia and debates became angry confrontations, criticism became politicized and polarized. Many of the growing number of Marxian social scientists (especially in sociology) in the West singled out Parsons' “functionalism” for “demonization,” as the leading theoretical “enemy of Marxism.” His works were condemned in wholesale fashion as capitalist apologetics, and his school was vilified as a hegemonic agency of American imperialism. Most criticisms written in this vein are best forgotten.

This view persisted in fervent “Marxist” cen-

ters in the 1970s, and many non-Marxian social scientists who came of age at that time remain closed to anything associated with Parsons' theory. Since this included the theories of Weber and Durkheim from which his program crucially devolved, special efforts have been made in the 1970s to dissociate these theories from those of Parsons. But since the "New Left" subsided in the mid-1970s, a revived sociological interest in Parsons developed. This partly reflects the work of some of his younger students, who modified action theory to better fit the temperament of their peers, and that of Parsons himself, who gave more attention to the more neglected aspects of his theories. There has also been a notable growing interest in Parsonian theory among eminent social scientists in Poland, Yugoslavia, and other East European communist countries, and more selectively, among such leading neo-Marxists as Louis Althusser, Ernest Mandel, and Jürgen Habermas in the West. The past debate continues, but there have been important rapprochements.

Background and early career

Talcott Parsons was born in 1902, in Colorado Springs, the youngest of five children. His father, Edward S. Parsons, who was descended from a line of Yankee merchants, broke with family tradition by attending Yale Divinity School, where he was ordained a Congregational minister. Shortly before the outbreak of World War I, the family moved to New York City, where Talcott Parsons attended Horace Mann, the experimental boys' high school of Columbia University. He described his home environment as liberal for that era. His mother, Mary A. Ingersol Parsons, was a suffragist who also supported other progressive causes, and his father was a "social gospel" Protestant of broad academic interests, who accepted the theories of Charles Darwin and viewed science as supplemental to religion. In the fall of 1920, Parsons entered Amherst College, which his father and two older brothers had attended.

It is important for his later work that both his college and postgraduate studies were unusually broad and diverse for a sociologist of his generation. Parsons was exposed to the major trends in the natural and social sciences, and studied with many remarkable teachers. Although he majored in biology at Amherst, he became interested in the social sciences during his junior year as a result of a course he took

with Walton Hamilton, an institutional economist. In graduate school, Parsons thus decided to clarify the relationship between economics and sociology, the problem that launched his theoretical program.

Parsons began his graduate studies as a non-degree student at the London School of Economics (L.S.E.) in 1924/1925. He was initially attracted to the L.S.E. because of two of its leading social democrats: the political economist Harold Laski and the economic historian R. H. Tawney. He also attended the lectures of L. T. Hobhouse, the noted evolutionary sociologist who had modified Herbert Spencer's *laissez-faire* doctrine; Hobhouse's younger, more empirical protégé, Morris Ginsberg; and the conservative monetary theorist, Edwin Cannan (1977c, chapter 2). But his most important experience was a seminar with the great functional anthropologist Bronislaw Malinowski, in which the anthropologist previewed his 1925 essay, "Magic, Science and Religion" (Parsons 1977c, p. 83). Parsons became interested in Malinowski's attack on evolutionary positivism (found in both classical economics and sociology) for its conceit in minimizing the rationality of "savages," his stress on the ultimate limits of science in the face of unalterables such as death, and the correlative universal need for religion. Malinowski's functional view of cultures as systems of interdependent parts and his emphasis on the universal biopsychological and social needs underlying cultural differences also made lasting impressions on Parsons, who retained them (in much altered form) in his own structural functionalism.

Before he completed his first year at the L.S.E., Parsons received an exchange fellowship to study at the University of Heidelberg in Germany. This was where Max Weber had spent most of his academic years, and though Weber had died in 1920, his influence was widespread. In contrast with the L.S.E., the University of Heidelberg was deeply divided on the value of science in general, and social science in particular. The prevailing view, with roots in German social thought going back to Hegel, has been called historical idealism. This view was "idealist" in seeing human actions as culturally determined, and "historicist" in seeing each culture as uniquely reflecting a ruling "spirit" (*Geist*), which was not susceptible to lawful scientific analysis. The rival view was a form of Marxism that accepted the uniqueness of historical systems, but saw their ruling ideals as sequentially

determined by the evolution of material forces. Max Weber's approach was significant as a neo-Kantian "third force," in which Parsons became interested soon after his arrival (Parsons 1937, chapter 13; Martel 1977, pp. 4-7).

Parsons enrolled in the philosophy faculty and studied sociological and economic theory. In sociology, his main teacher and examiner was Alfred Weber, Max Weber's younger brother, an eminent scholar in his own right who was somewhat more historicist than his more famous sibling. But he also took a seminar on Max Weber with Karl Mannheim and discussed Weber's work with Alexander von Schelting, a young sociologist who did much to clarify Weber's methodology, and with Edgar Salin, an iconoclastic economist close to both Weber brothers, who became Parsons' dissertation adviser. In addition, he studied Marxian theory with Émil Lederer, a noted socialist economist, and he personally delved into *Das Kapital* and those of Marx's related theoretical works that were then available.

Parsons' other important teacher at Heidelberg was Karl Jaspers, with whom he took an intensive minor on Immanuel Kant's philosophy. Jaspers, a non-Freudian psychiatrist who became a philosopher under Max Weber's influence, is usually thought of as a phenomenologist and a founder of "existentialism." But to Parsons, he stood for a sensitive, qualified defense of "objective" social science and a "*Verstehen*" approach that helped Parsons clarify his own developing methodology. Jaspers also spoke appreciatively of Durkheim, who had been dismissed by Ginsberg in London for allegedly having a mystical "group-mind" concept, and by Malinowski for more ambivalent reasons (Parsons 1979).

For Parsons, his encounter with Max Weber's works was his most important experience at Heidelberg. He decided to write his dissertation on the issues raised by Marx's approach to capitalism. At Salin's suggestion, he confined it to recent German works and the final work was a comparison of the approaches of Marx and Weber with that of Werner Sombart, a leading sociological historicist, whose three-volume tome, *Der moderne Kapitalismus* (1902) and other writings were prominent in the debates. Parsons completed his thesis, "The Concept of Capitalism in the Recent German Literature," and received his DR. PHIL. in 1929.

Before returning to the United States in 1926 to teach economics at Harvard University, the

university with which he has been identified ever since, he married Helen B. Walker, a New Englander who had also studied economics at the L.S.E.

In 1928-1929, Parsons published two articles in English, based on his thesis, which indicated several of his future positions. First, he favored the position of Marx, Weber, and Sombart that Western capitalism is a historically distinctive system, whose market laws and salient motives (e.g., profit seeking) reflect cultural conditioning, and he agreed with their broad rejection of more universal claims by classical economists. Second, he rejected both Sombart's idealistic view of capitalism's uniqueness and indeterminate origins and Marx's concept of its materialist, unilinear evolution. Instead, he supported Weber's attempt to reconcile the two theories in *The Protestant Ethic and the Spirit of Capitalism* (1904-1905), which Parsons translated into English in 1930. He also endorsed Weber's effort to combine historical and comparative analysis (rejecting the historicist dogma of their exclusiveness), as well as his multilineal evolutionism, viewing history as a "branching tree" (1928-1929, vol. 37, p. 45). But his support of Weber was not unqualified, and he criticized Weber's failure consistently to distinguish between historical and analytical concepts, his *ad hoc* use of "ideal types," and his lack of a system model of societies. On the other hand, he praised Marx's systemic view of capitalism and the notion that "abuses only could be remedied by changes in the fundamental basis of class interests within the system" as "a great advance over utopian social (ism)" (1928-1929, vol. 36, p. 659). He concluded that the "analysis of the moving forces in social life . . . in German sociology . . . has its starting point to a very large extent in Marx. Here is a set of problems which sociology cannot afford to neglect" (*ibid.*, vol. 37, p. 50).

Although Parsons was exposed to economic, sociological, and philosophical thinking during his studies abroad, his background was more limited in the technical economic theories developing in England and the United States when he joined the Harvard economics department. The department included leaders in the classical tradition, such as F. W. Taussig, T. N. Carver, and W. Z. Ripley, the economic historian, Edwin Gay, who had studied in Germany, and Joseph Schumpeter, who had just arrived from Austria. He audited Taussig's graduate course on economic theory, which emphasized the works of

the neo-classicist Alfred Marshall. These studies led Parsons to a more technical consideration of the links between economic and sociological theory. As he later wrote, the idea dawned on him that both "should be conceived as standing within some sort of theoretical matrix" (1970, p. 828). This was the beginning of his general theory of action.

But Parsons still was working in the undefined area between the two fields. In 1931, when the sociology department was founded at Harvard (with Pitirim Sorokin as chairman), Parsons became a charter member. Among his graduate students during these early years were Robert K. Merton, Kingsley Davis, Robin M. Williams, Jr., and Wilbert E. Moore. They were attracted by his rare combination of analytical brilliance, scholarly breadth, and personal warmth. The effects were interactive, and student support proved essential to the growth of his theoretical program.

First major work. With his shift into the newly formed sociology department, Parsons' interests in the noneconomic assumptions of economic theory broadened, and led him into a much wider investigation of social theories. This study resulted in his first major work, *The Structure of Social Action* (1937), which was hailed as a work of rare genre and is now regarded as a classic, even though some of its findings have become extremely controversial.

In this book, Parsons extended his study of German approaches to capitalism to include British, French, and other European writers. Detailed attention is given to a comparison of Marshall, Durkheim, Weber, and Vilfredo Pareto. It was the first book to introduce Durkheim, Weber, and Pareto for serious and detailed consideration to English-speaking sociologists and, in the absence of any other English work comparable in quality, it became a standard source. This was not Parsons' aim. In his introduction he explained that while the aspects of their work that he discusses are "of great, sometimes central importance to their work as a whole," his approach was selective (p. 15).

The volume has four central and related aims. First, it is an empirical study in the sociology of science, concerned with changing views of individualism and rationality in the leading social and economic theories of the Western world since the emergence of modern economies and social science in the eighteenth century. Writing during the depression of the 1930s, when socialist, fascist, and Keynesian remedies were

being tried to revive fallen capitalist economies, Parsons sounded his opening theme:

A basic revolution in empirical interpretation of some of the most important social problems has been going on. Linear evolution has been slipping [and] various kinds of individualism have been under increasingly heavy fire. In their place have been appearing socialistic, collectivistic, organic theories of all sorts. The role of reason and the status of scientific knowledge as an element of action have been attacked again and again. We have been overwhelmed by a flood of anti-intellectualistic theories of human nature and behavior. (p. 5)

Parsons further presumed that "an equally radical change in the structure of theoretical systems . . . of society" (p. 11) has occurred over several centuries, which is not only "an ideological reflection of certain basic social changes" (p. 5), but partly reflects genuine scientific progress. The second aim, then, is a critical as well as descriptive examination and appraisal of trends in the leading theories. Third, the book seeks to arrive at the most defensible theoretical framework and methodological basis for future theories of social action. Finally, it uses both its sampled theories, along with their supporting data and logic, to justify its analysis of future theoretical directions.

Parsons' much debated conclusion was that as the research and thinking of Marshall, Pareto, Durkheim, and Weber evolved, a broad convergence is shown in their works toward a common theoretical and methodological position that Parsons called the "voluntaristic theory of action." It combined important features of both classical economics and historical idealism. Parsons' claim was dramatic, especially since scholarly opinion then placed each of the four theorists in a different corner of the theoretical map. They also came from four different countries and had contrasting social backgrounds, ideologies, and temperaments. As Parsons wrote: "More violent contrasts are scarcely imaginable than between Marshall, the strongly moralistic middle-class Englishman; Durkheim, the Alsatian Jewish, radical, anticlerical French professor; Pareto, the aloof, sophisticated Italian nobleman; and, finally, Weber, a son of the most highly cultured German upper middle class . . ." (p. 13). They reached their positions independently, with little awareness of each other's paths.

But what counted most to Parsons was their contrasting methodological starting points. Parsons analyzed the four theorists against the de-

velopmental backdrop of idealism and positivism, the two dominant Western outlooks on rationality and individualism in the study of man. Each outlook is defined as an ideal type, and then the two approaches are traced up to the early twentieth century in a series of brilliant sketches of their outstanding exponents. Part of the unusual power, even excitement, of *The Structure of Social Action* comes from the vivid, incisive way that Parsons pointed up critical theoretical problems in each of the two dominant traditions in his brief sketches of Thomas Hobbes and Thomas R. Malthus on the positive side and Georg Hegel and Wilhelm Dilthey on the idealist side. On one level, Parsons traced the modern idealist breach with positivism from Kant's dualistic attempt to defend Newtonian science and yet preserve a concept of human moral responsibility. As he wrote:

Kant's answer to Hume . . . reestablished the validity of physical science by reducing physical bodies and events to . . . 'phenomena'. . . . Man, to be sure, participated in (this physical) world, not only as a knowing subject but also as an object, as a physical body. But this did not exhaust man (who also was) an active, purposeful being, an actor In this sphere man was not subject to law in the physical sense but was free. (pp. 474-475)

Parsons goes on to say: "The tendency of Kantian thought was thus toward a radical dualism . . . in relation to man (favoring) a reduction of all (physical) aspects of man, especially the biological, to a 'materialist' basis. (It) produced a radical hiatus between . . . the natural sciences and the sciences of culture or mind (*Geist*) in Germany" (p. 474).

Positivism in Western Europe sought an approach to social life closely modeled on natural science. Acting units were restricted to individuals, since no physical group links existed between them after birth. All subjective properties—definable by their meanings to various actors or culture groups—were virtually excluded. In the extreme (radical positivism), this was a biosocial view of men as organisms adapting to environments like any animals. This view was epitomized by Darwin, behaviorist psychology, and instinct theories. By comparison, the more tempered positivism of utilitarians Hobbes and John Locke, who set the framework for classical economics with their ends-means scheme, was preferable. Here men were at least seen as conscious beings, who had the ability to choose from various means available the ones better

suited to their ends; their choices had some causal weight. Objectivity was upheld by assuming that men had common ends in the public sphere (e.g., seeking the lowest market prices) and that means could be judged by the scientist's own rational norms. Thus, the actors' meanings were residualized and ignored, and discrepancies were the result of their ignorance or errors. Hobbes initially saw a problem of "order" in any such individualistic model of social behavior, but Locke more optimistically added a "prop" of a natural identity of interests.

By contrast, idealist approaches (especially those developed in Germany) stressed precisely the subjective, symbolic qualities of men that set them apart from other species. Theoretically, the distinctive contribution of historicism is its concept of cultural organization as a system of interdependent meanings. In contrast to the physical systems treated by positivist science, the historicists called attention to the "ideal reality" of cultures (1937, pp. 482-483), whose comprehension required a method of *Verstehen* (interpretive "understanding" of a unique system of symbolic connections) rather than explanation by causal laws. As Parsons emphasized, such symbolic links contrast with causal relations in being timeless (in the sense that by the norms of a system of logic, a "proof" once achieved retains its acceptability). Historicism in its relativism also challenged the uncritical rationalism of positivism, and argued for the unique integrity of each subcultural mode (e.g., arts, politics, science, etc.) as well as for the integrity of each total cultural system. But the difficulty was that cultural meanings, and individual or collective actions relating to them, were set totally apart from the physical realm of action.

The "convergence" that Parsons attributed to Marshall, Pareto, Durkheim, and Weber depends as much on their links to these traditions as to trends in their individual writings (pp. 697-719). Thus Marshall, who was deeply committed to British utilitarian economics, found difficulties in its treatment of ends and was led to take account of the concrete wants of men in their daily lives. Pareto, who had come into economics from mathematics and physics, arrived at similar criticisms, but sought to answer them with a broad analytical theory. He argued that the problem with classical market models was not that they were abstract, but that they were too narrowly and crudely abstract. Thus, in portraying "workers seeking highest wages"

or “merchants seeking top profits,” they ignored other aspects of motivation that powerfully affect economic systems and are important in their own right. He strongly attacked positivistic economics for uniformly imposing common ends and rational norms on actors whether they fitted or not. In later work, he developed a sociological scheme that gave a central place to nonlogical sentiments, which could affect the varying emphases given to economic self-interest (e.g., respect for tradition or the desire for group approval). Parsons convincingly showed that the sentiments Pareto discussed are more closely linked to norms and values than many readers had thought.

The sociologist Durkheim, starting from the more organicist positivism of Auguste Comte, moved toward a similar position that he developed much further. This is first seen in his early book, *The Division of Labor* (1893), where he discussed Spencer’s utilitarian vision of economic relations in future industrial societies as increasingly based on voluntary contracts. To Durkheim, who was trained in jurisprudence, this naively ignored the problems of making and enforcing contracts without a system of legal codes and common law guidelines. His argument concerning the essentiality of shared norms for social order was strengthened in his later notion of “collective conscience” as the regulative mechanism for stable groups. This was extended in his book on religion to include sacred moral values.

Complementing these trends from the idealist side is Weber, who began his studies in economic and legal history. He never abandoned the historicist doctrine that each culture had to be understood in its own terms by a method of *Verstehen*, but he argued that such understanding was only descriptive, and that the explanation of cultural formation, persistence, and change required a scientific approach with the same logical kind of comparative variables and causal propositions as the explanations of physics. In his view, the difference was that in cultural explanations at least some of the comparative variables had to have categories that contrasted the different values and beliefs of actors in different culture groups (e.g., the “otherworldliness” of early Christians versus the aim of “making a Kingdom of Heaven on earth” in the Puritan ethic). This meant that *Verstehen* was essential in formulating categories and hypotheses, but was not a method of

explanation or proof. More basically, he argued that science and history were distinct but interdependent ways of studying any phenomenon (e.g., astronomers who study the history of solar systems), and their relative importance depends on the interests of researchers and not on the subject matter. Thus, strongly concerned with the issues raised by Biblical criticism, he maintained that evidence of historical errors in a sacred gospel might be irrelevant to a believer, but of great interest to a religious historian—an argument that in some ways parallels Pareto’s critique of dogmatic economic rationality and Durkheim’s stress on the power of contrasting forms of “collective conscience” in different groups.

Outline of the action framework. The final chapter of *The Structure of Social Action* maps an approach for future work in line with the common directions that Parsons identified in his sources. This approach derives most directly from Weber’s “action framework,” which sought a grounding for the comparative analysis of large-scale institutions, conceptually anchored in individually motivated acts. In Parsons’ version, like Weber’s, the “end-means” scheme is retained, but modified to emphasize cultural norms and beliefs. An act is defined by its end, which is assumed to reflect cultural ideals that an investigator must know in order to understand the situation. The contrast between means (manipulable aspects) and conditions (unalterables) in an actor’s situation is similarly important in reflecting cultural values and beliefs. Thus, one culture group may know how to convert gold ore into coinage and use it as money; a second may know the process but restrict it to religious use; a third (not knowing metallurgy) may simply react to it like any other part of their environmental conditions.

Parsons then tentatively suggested a more rational method of distinguishing between and uniting economics, political science, and sociology using the basic action scheme. He postulated that each is concerned with a partly distinct, emergent system of aggregational action, definable by a distinctive set of “effect variables” (not causes). Thus, analytically speaking, economics is concerned with the rationality of means in relation to scarce, valued ends; political analysis with Hobbesian power struggles and their resolutions; and sociology with the integration of social action through shared values or norms defining over-all goal priorities (ultimate

ends) and institutionally acceptable means in various situations. Concretely, all three systems are completely intertwined, which also implies that the abstracted systems have complex causal links. Thus, all social institutions are partly affected by economic and political processes and vice versa. This implies that a theory limited to any one system will be limited in its explanatory power. Parsons made this argument across the board as a case against "single factor" theories and for theoretical unity.

In terms of later controversies, two points should be noted concerning Parsons' stress on the normative aspects of action. One is that values and norms are emphasized because they unite people without physical links or social instincts into cohesive groups, and identify some of the key patterns that make one human group different from another. This process is never without important strains, and Parsons asserted: "Action must always be thought of as involving a state of tension between two different orders of elements, the normative and the conditional" (p. 732). A second point is that the groups are not limited to societies, and the normative thesis is applied to subcultural groups. This is seen, for example, in Parsons' discussion of Marx, whose views receive a more ambivalent treatment than those of any other major theorist. Commenting on the idea of class formation, Parsons suggested that the conversion of a dormant class into an active, class-conscious group involves "what looks very much like a common value element," and wonders why this "does not play a part in Marx's general view of history" (p. 494).

Analytical realism. Equally important, in the final chapter of *The Structure of Social Action*, Parsons outlined an "analytical-realist" methodology that attempts to synthesize positivist and idealist concerns; it is the most consistent feature of Parsons' subsequent work. For example, it underlay his more specific "functional" approach a decade later, and is one reason why his functionalism differs from that of Durkheim and Malinowski. Most crucially, he linked Weber's critique of historicism and Pareto's critique of economic positivism with Alfred N. Whitehead's critical analysis (1925) of materialism in nineteenth-century physics. He argued that the "facts" of science or history are never simply "observations," but rather are classifications of objects using language concepts that various trained observers can share. The "ob-

jectivity" of science, then, critically depends on building conceptual schemes that many researchers can reliably use, and not on the direct observability of the properties of interest. Whitehead had shown, in fact, that as physics developed it gave much less emphasis to directly observable properties, and more to properties involving complex inferences from empirical classifications. This clarification bolstered Durkheim's argument that cultural meanings expressed in symbolic form (collective representations) could be studied in as scientific a manner as any other phenomena.

As did Weber and Whitehead, Parsons stressed that conceptual schemes, at best, provide selective portrayals of objects. Whitehead had labeled the failure to understand this selectivity "the fallacy of misplaced concreteness," which is mistaking a partial description of objects for their "essence" or entirety (racism and sexism are important social examples). In effect, Parsons showed that Weber's critique of historicism and Pareto's of economic positivism illustrated the same point. Like Whitehead and Pareto, he then drew the "emergentist" conclusion that multiple frameworks and theories can be applied to the same objects without contradiction. In British positivism the aim traditionally had been to unify all scientific variables into a single, closed system, but Parsons rejected this view. Instead, for human action at least, he argued in favor of multiple theoretical systems that vary in their degree of independence. This converts the concept of system from an attribute to a variable (degrees of systemness), which established the groundwork for Parsons' much more rigorous development of action systems in later works (Martel 1971, pp. 178-185).

In one sense, Parsons' analytical realism is an argument for a broader positivism that gives social science a central place in the defense of rationality against anti-intellectual forces. But his key postulate on the rationalism he defended bears quoting: "The central fact—a fact beyond all question—is that in certain aspects and to certain degrees, under certain conditions, human action is rational" (1937, p. 19). This highly qualified defense by Parsons, as heir to the Enlightenment, typified his defense of other values as his work unfolded, such as individual freedom and moral integrity. Starting with a minimum state of realization of a given value, his strategy partly was to ask which other socially essential values limit its further realization

under various social conditions. Axiologically, his was a quest to specify a total system of basic human values, and to clarify how practically they might be optimized in varied human circumstances.

Note on critical reactions. For years, Parsons' *The Structure of Social Action* was his most widely respected book. Disputes were never lacking (see Parsons 1978c), but they were discriminate. With the rising tide against Parsons in the late 1960s, a special attack was launched against the application of the convergence thesis to Durkheim and Weber, with criticisms, begun in Britain, of Parsons' selective reading or unholy marriage of the French and German theorists (see Parsons 1978b). Much of this line of criticism ignores the aims, context, logic, and qualifications of Parsons' study and reacts more to the impact of the book than its contents. Before its publication, few sociologists saw *any* important affinities between Durkheim and Weber. It is a mark of Parsons' success that we now have a literature asserting their partial differences. The more important criticisms often concern his later development, which retains important but selective and increasingly divergent ties with his starting sources.

Later career and work (1937–1973)

In his first book, Parsons presented a broad unifying program for action theory that included analytical–realist guidelines. It remains his best source on his relations to precursors and basic methodology. But his action framework, which began to take detailed form in the 1940s, was blueprinted in two major works in the early 1950s, and has been steadily expanded and refined ever since. The expansion of the scheme is closely tied to the development of a distinctive modernization theory that is also rooted in Durkheim and Weber. The theory focused on the rationalization of modern societies in Weber's sense, with the growth of market exchange, specialized occupations, and a secular outlook on life. Since the mid-1960s, this concern has broadened into a full-scale neo-evolutionary quest, with closer attention to ancient societies, and a search for sequential stages to help order historical variations.

After World War II, Parsons' writing was marked by his increased attention to anthropology and psychology. This grew out of his associations with anthropologists Clyde Kluckhohn, Elton Mayo, and W. Lloyd Warner, and psychol-

ogists Gordon Allport and Henry Murray. They interested him in pursuing an intensive study of Freud's theories, as well as anthropological culture and personality approaches. A study of the medical profession, which Parsons began before the war, put him in touch with Stanley Cobb, founder of the Psychoanalytical Institute in Boston. In 1946, despite his lack of a medical degree, Parsons applied to the Institute for Psychoanalytic Training and was accepted. From then on, Freudian theory was central to his work.

Another important event in 1946 was the formation of the department of social relations at Harvard, which brought together a noted group of sociologists, psychologists, and anthropologists for more than two decades. Parsons was a prime mover in this merger and was chairman for its first decade. During this postwar period, when the United States reached its apex in world affairs, its universities and academic fields had their greatest expansion. Top students from all over came to Harvard's unique experimental program. Parsons' fame and influence grew in this expansionist, multidisciplinary climate, which provided the structural support, stimulus, and team diversity necessary to accommodate a major theorist with his talent and breadth. By the later 1950s, although his theories remained controversial, he became the most celebrated sociologist in academic life. In 1967 he was the first social scientist elected to the presidency of the American Academy of Arts and Sciences. His postwar writings in many ways reflect the ascendancy of the United States and its universities, as well as his own rising star.

In tracing the complex growth of Parsons' postwar theory and conceptual scheme, two main points of crystallization in his work will be stressed: first, his functionalism of the early 1950s, and second, his functional–evolutionary model of the later 1960s. This approach is taken because his theory grew like a rapidly widening, oscillating spiral. His universe of problems expanded, and each problem was periodically reexamined on a broader basis. Partly this also is because much of Parsons' writing was in essay form, the more important of which now fill seven volumes. In these essays one finds a whole series of middle-range theories reflecting the many facets of his modernization argument—an argument that grew by installments as his scheme developed. They include major arguments about modern professions, economic or-

ganization, education, stratification, families, age-sex roles, socialization of children, sick roles and mental health, countercultures, deviance, democratic processes and the law, bureaucracy, ethnicity, religious secularization, and science. All are partly linked to his broader theory. But it is his smaller arguments, presented more accessibly in his essays, that have most interested specialists and influenced (sometimes altering) many fields. Often the specialists concerned have not understood the larger contexts of Parsons' theses, and many fields of sociology have lost much time refuting their interpretation of Parsons' theories of sick roles, changing functions of families, stratification functions, or conforming behavior. The most frequent error has been to test Parsons' arguments concerning the institutions of societies on smaller groups in local communities for which convenient data could be secured.

Only a few of these middle-range theories are explicitly noted below, and no work exists that begins to distill their riches. Parsons' own exceptional summary (1970) and the books by Rocher (1975) and Bourricaud (1977) give some idea of their scope; Mitchell (1967) is excellent on his political writings; and three recent *Festschriften* (Barber & Inkeles, 1971; Loubser et al. 1976; Hallen et al. 1977) summarize and extend his main theories. Of these, the Loubser volumes give the most complete summary and critique for the more technically interested reader. The present article mainly seeks to trace the growth of the basic theory and the framework that ties the many essays together.

The three-system model. Parsons' more developed framework first appeared in his second major book, *The Social System* (1951) and a companion essay with Edward Shils (1951), whom Parsons brought to Harvard from the University of Chicago. All ingredients of the earlier prospectus (1937) were retained, but with different accents. The focus was now on social, cultural, and personality systems as the three main interpenetrative types of action organization (with political and economic systems viewed as special kinds of social systems). While all the systems are viewed as emergent organizations of basic action elements, there is a shift from the individual actor making choices to the interdependence of choices of socialized actors. On the whole, the new version better expresses Parsons' earlier aims, by taking advantage of his newer anthropological and psychological resources.

The generic action unit is no longer the elementary act, but rather the more complex notion of an "actor-orienting-to-a-situation." The actor may be either an individual or a collectivity, with each viewed as equally real. In some ways, individuals as carriers of energy are still the basic action units, but to be an actor, an individual must be a socially enculturated person, who in many ways is a product of his/her upbringing (whether conformist or not), so the relation is complex. Whether individual or collective, action is defined as "behavior oriented to the attainment of ends in situations, by means of normatively regulated expenditures of energy" (Parsons & Shils 1951, p. 53). But behavior includes the decision-making process leading to an action choice as well as the overt activity, and should not be confused with behavior in stimulus-response psychology. In contrast with the latter, action again is purposeful—seeking out and defining its environmental stimuli in terms of motives and values, rather than merely responding to them. It includes the alternatives an actor rejects or defers for a later time and the imagined responses by others to his various action alternatives. This becomes more important since the Freudian concepts of repression and taboo are added to the action model. The familiar fact that a psychoanalyst listening to a patient pays close attention to subjects *not* discussed, as signs of important motivational conflicts, is a case in point.

In Parsons' view, each of the three main types of action systems—culture, personality, and social systems—has a distinctive coordinative role in the action process, and therefore has some degree of causal autonomy. Thus, personalities organize the total set of learned needs, demands, and action choices of individual actors, no two of whom are alike although they may speak the same language and share many cultural values and beliefs. Social systems have an interpersonal focus, and organize the divergent and often conflicting action tendencies of individuals into coordinative forms of relations. In more complex cases, they organize social groups into larger organizations through hierarchies or representatives. Culture, by contrast, is defined more systematically and narrowly by Parsons than by most anthropologists. To Parsons, it consists of norms, values, beliefs, and other ideas relating to action, which have been objectified in symbolic codes and can be transmitted from one individual, group, or generation to another. In a strict sense, while it originates in

interaction and provides symbolic resources through which action can be structured, it is a system of symbolic patterns rather than a system of action, and it serves to organize such patterns into configurations which have symbolic as well as behavioral coherence, as in the idealist notions discussed above (Parsons 1937).

Most importantly, culture provides the scripts (systems of rules or norms) for organized action, like dramatic plays which can be performed over and over again once they have been memorized or written down. It also provides the broader value standards for justifying why certain scripts should (or should not) be performed, their priorities, and guidelines for proper staging and performance. In this conception, culture is an information bank that becomes operative in action when individuals internalize its patterns or groups make commitments to them.

The scheme's further basic novelty is found in the idea that the three action systems are "interpenetrating," and their elements importantly overlap. Some key elements in one system also have necessary counterparts in the others. The idea is used to clarify systematically the important concepts of social roles and institutions as well as many others. Thus, social systems are conceived as being composed of social roles consisting of the norms or script an actor is expected to follow in a particular situation. These norms are derived from the culture(s) to which the actors in the social system relate. But Parsons also argued that a role for a given person (termed ego) should only be counted as socially operative when it is internalized by the definers of ego's role (called alters) as operative motives in their individual personalities. This means in effect that alters have a need for ego's conformity to their role definitions, and that ego's conformity rewards this need and significant deviations violate it. Adding a frustration-aggression hypothesis from Freud, Parsons further assumed that if ego rewards alters by his conformity, they are likely to reward him in return, and the reverse if ego deviates. Thus, extending the argument to a pair relation where each party is both ego and alter, it is assumed that where compatible role definitions exist, the sheer continuation of the relationship within acceptable bounds is interreinforcing, and no other special sanctions or rewards are required. And under these conditions, the *system* of role relations becomes self-regulating. Thus, the concept of social role takes on a much more sys-

tematic meaning in Parsons' model than before, as the linkage concept between culture and personality in interaction.

In more complex social systems, every party does not have to be strongly committed to every role, but some prevailing set of alters must exhibit such a commitment. This means not only that separate alters are likely to sanction ego negatively if he or she steps out of line, but that alters will sanction one another as sanctioners to ego. Thus, if the norm is upheld in a church that parents must keep children quiet during services, when a child (ego) acts up and his parent (alter) fails to quiet him, other people close by (the parent's alters) are likely to show disapproval to the parent. This is because their needs include not only ego's behavior but alter's proper response to such behavior. Where a prevailing group commitment is made to a set of roles, with some awareness that these commitments are widely shared, Parsons referred to the role norms as being institutionalized in that group, and to the organized systems of such roles as an institution. An institution in this sense represents the internalization of cultural norms in the personalities of alters (the role definers), but it is also an interactional emergent referring to interalter relations. In societies and other highly complex social systems, part of the job of defining and sanctioning social roles may be assigned to specialized officials or agencies (legislatures, courts, police, etc.). It was Parsons' assumption, however, that such officials could only function effectively where they are generally backed by a public consensus of their alters. He was quite aware that in large groups anything like perfect consensus is impossible, and that some disputes must be resolved through force or the threat of its use. But he argued that widespread and continued reliance on force is very costly, and over time has great limits as a basis for stable organization. The use of force in larger groups, moreover, depends on some institutional commitments, at least within the power group, and his model makes terrorist dictatorships seem relatively vulnerable.

The pattern variables. Conceptually, Parsons' second main innovation is the pattern-variable scheme, an idea that has been widely accepted and used. It was his insight that several main contrasts between traditional and modern societies found in earlier theories (from Comte to Ferdinand Tönnies, Weber, and Durkheim) could be redefined in more universal and basic

action terms. This led to the formulation of five key variable properties of action patterns, later reduced to the four listed below. Each is presented as a binary choice that arises in every social relationship and which must be resolved by a clear priority selection before the relation can be stabilized. The first two choices pertain to ego's orientation to others, while the last two choices concern the way alters (individuals or status groups) are defined:

(1) affectivity *vs.* neutrality. Whether ego seeks immediate gratification in the situation (e.g., expresses feelings freely), or exercises restraint toward alter.

(2) diffuseness *vs.* specificity. Whether ego seeks a broad range of gratification from alter, or maintains a narrower orientation.

(3) particularism *vs.* universalism. Whether ego defines alter in terms of a special relation they have (friends, kin), or in terms of alter's membership in a broader status class (fellow citizen, male or female, human). The contrast is relative to a system of reference, and within a family a parent may be particularistic toward a favored or unfavored child.

(4) ascription *vs.* achievement (later generalized to "qualities" *vs.* "performance"). Whether alter is defined by criteria independent of his actions in a situation, or is defined on the basis of his actions and how well he performs them. (Age, sex, and race illustrate the former, and "merit norms" the latter.)

Each pair of choices refers to aspects of an actor's definition of a role or relation—the reference is highly abstract in order to permit broad comparisons. In empirical applications they require a content analysis of meanings, but theoretically they make it possible to build up ideal types of action patterns using the alternative choices as conceptual molecules. For example, the first terms in the four variables define widespread elements of the norms of friendship relations which often are held to be affective, diffuse, particularistic, and ascriptive (despite many variations in their detailed meanings). By contrast, the second terms define widespread elements of professional and official roles toward clients. Thus, a teacher often is expected to be neutral/specific/universalistic/performance-oriented to students in classroom situations, although the student side is more variable and often more affectivity is encouraged. As Parsons' theories developed, each of these four pairs of choices blended into a scale and he used them less dichotomously.

Part of the power of this scheme is that its categories can apply to cultural values and personal needs as well as to role norms, and they can also be used to portray aspects of behavioral conformity or deviation from social roles. Using this scheme, Parsons introduced a very general hypothesis about social systems: that every social relationship of any complexity must include *all* value combinations of the paired choices. Thus, every friendship relation must give some attention to certain tasks where universal achievement standards (of abilities) are recognized, and every professional or work relationship must include some moments when affective particularistic meanings are expressed. In part, this generalizes a finding in several different sociological fields. Industrial sociologists had called attention to the importance of human relations (personal relations) even in the most formal work groups, and researchers on friendship and love were pointing up utilitarian aspects of even the most intimate relations. Parsons' argument was that all stabilized relations are made up of all the important social values, and differ in the priorities each receives. The thesis is applied most importantly to societies.

Types of societies and value priorities. In *The Social System* the pattern variables are most basically used in an experimental typology of societies (chapter 5) that gives a first sketch of Parsons' modernization theory. In very simple terms, he uses the object variables (particularism—universalism and ascription—achievement) to contrast broadly four major types of societies, identified by their dominant values (e.g., type of *Geist*). Only the two extremes are mentioned here. At one extreme, traditional societies are identified as "particularistic—ascriptive," where the emphasis is on kinship relations, and most important activities (work, education, leisure pursuits, religious practices, etc.) are carried out by family members or by actors in related families. At their core, the relations between kinsmen reflect the dominant particularistic—ascriptive values of the society and also are likely to be affective and diffuse. But Parsons stressed that where kinship pervades all of social life, there must be some differentiated role relations between kinsmen where the other value priorities are stressed. This is especially likely in economic production where some minimum standards of competence must often be observed, but it is also important in child raising, recreation, and religious observance. Where kinship attachments and related values prevail,

contrasting values are likely to be constrained by the dominant commitments (e.g., you do not fire kinfolk). This sets great limits on the possibilities for economic, political and other forms of sociocultural development, which must be overcome before modernization can begin to develop.

By contrast, in the more modernized societies, there is a tendency for universalistic-achievement standards to become dominant as the rationalization of social life develops, and in many ways the value priorities reverse. Within the public sphere of political and economic life universal merit standards are stressed, and nepotistic preferences in assigning jobs, tax rates, licenses, or judicial decisions are tabooed. The norm in one type of society becomes serious deviance in the other, and such taboos tend to be extended against favoritism to friends, fellow members of ethnic groups, and other particularistic ties. Weber had feared that rationalization could pervade the entire modern social fabric and threaten all spontaneous human relations with destruction. Instead, Parsons argued that rationality as a principle has limits just as kinship values have, and he posited that kinship and personal relations still will be maintained, cherished, and protected in the private sphere.

Four main points about this approach should be mentioned which, with many added complexities, remain central to Parsons' subsequent development of his theory. First, despite their many differences, it is argued that all stable societies have certain basic values in common, hinted at by the pattern-variables. They must also have some specialized structures and occasions in which each value pattern can be expressed. In extreme contrast to cultural relativism, Parsons' believed in the cultural unity of man, and the pattern variables (in combinations) gave a starting point for identifying important universals. Second, the major differences between cultures are seen as a matter of degree, in the priorities and nuances given to the same basic concerns. While differences of degree are by no means unimportant, he extended his earlier argument that doctrines of unique cultural spirits mislead by only identifying the dominant values and beliefs of a culture, ignoring its lower priority values and over-particularizing their meanings.

Third, the dominant values of a society set the priorities for the organization, resources, and locations given to its various role activities, which concretely limit their respective develop-

ment. Thus, where kinship is stressed, work and other activities are located, whenever possible, near the home to facilitate (and minimize disruption) of family relations. This greatly limits economic, educational, and even recreational development. By contrast, in modern societies where economic development and public life rate highest, factories and other economic organizations receive choice locations for their needs (resources, labor, transportation, etc.), prime time and energies are given to the work sphere, and family as well as other role systems are contracted and adapted to fit these priorities. In both cases, however, each essential differentiated sphere will receive at least minimum attention.

Fourth, all societies (indeed, all social systems) are intrinsically beset by contradictions, strains, and conflicts of all kinds, at the most basic level of values and meanings. But to Parsons this did not support any dialectic view of history (Hegelian or Marxian), if taken to mean that such basic conflicts can be essentially resolved. This is perhaps his most basic and consistent contrast with Marx—the idea that important contradictions are adventitious only to certain types of societies, and that one can hope realistically for their virtual elimination. Any hope of this kind is utopian in Parsons' view, and he further contended that there are strong forces in all social systems making for commitment to such utopian patterns (1951, p. 166). This relates to a theory of countercultures included in the discussion of the four types of societies. His key argument was that utopian countercultures are generated by the strains of the universal values suppressed in a given society. Thus, men in kin-dominated societies may dream of utopias in which merit standards and individual freedom are more fully appreciated, while modernists may yearn for communal utopias in which kinsmen and personal identities are reunited again. Such strains also may lead to active and successful attempts to change the system, but then another compromise will be established.

Parsons retained the basic action scheme presented in *The Social System* and its companion essay in all his later work, although it was quickly augmented. By the 1960s he had developed a vastly superior model of action systems and societies. As his scheme expanded, his theory improved and the analysis in *The Social System* now seems less sophisticated by comparison. However, the core ideas noted above

are included in his later developments, and the book retains special importance for two reasons. First, it shows Parsons at the height of his search for an organizational framework, before several advances began to narrow his exploration. Second, the book considers a wider range of possible forms of societies than perhaps any work in the literature since Herbert Spencer's *Principles of Sociology* (1876–1896).

Development of Parsons' structural functionalism. In methodological terms, Parsons' approach to action systems in *The Social System* relied on a structural–functional strategy that was greatly extended in the next few years. This approach, with antecedents going back to Comte and Durkheim, is concerned with the organizational properties of emergent systems which: (1) are dependent on environmental resources for survival, as a human organism is for oxygen and food, and (2) also depend for survival on complex internal coordination of specialized parts or subsystems. Questions about *functions* for such organized systems concern the effectiveness with which the system adapts to its environment, and the related question of how its internal coordination is effected. The *structural* aspect of the approach is modeled after anatomy in biology, and seeks to simplify complex questions of processual relations by using typological concepts stressing a few main qualitative contrasts (e.g., vertebrates *vs.* nonvertebrates in older biology) rather than continuous variables. The value of such a structural approach depends on identifying fairly stable features of a system that correlate with many other important properties, as the vertebrate category did for its time. In Parsons' work, dominant values have been stressed in defining structural types, as shown in the types of societies categorized by pattern variables in the last section.

Parsons' structural functionalism and his argument for its sociological use was first presented in several essays in the 1940s (1949, chapters 1, 2), and was extensively illustrated in *The Social System* (1951, pp. 19–22). There he maintained that societies can be taken as the focal systems in human action because they are the human survival groups. That is, they have the cultural and institutional resources to perpetuate themselves across generations. At the same time, it is made clear that any other action system can also be taken as a reference for functional analysis in its own right, as long as the system reference is clear. But in the main, Parsons' work (like that of Durkheim and

Weber) begins with the analysis of societies, and other social systems are positioned by the societies in which their members are implicated.

The functional analysis of a system depends on some systematic listing of its organizational problems and needs, and at the time of *The Social System*, Parsons was groping for such a list. Previous functional analysts had usually settled for fairly concrete and spontaneous classifications, but Parsons was seeking a more systematic and theoretically integrative approach. In the next few years he began to provide for this need with his famous four-function scheme, which has had a commanding place in his work ever since. It was introduced in three collaborative papers with Robert F. Bales in *Working Papers in the Theory of Action* (Parsons, Bales, & Shils 1953), where its explicit use was limited to small task groups. But it then was rapidly extended to societies and other social systems in two other collaborative works: *Family, Socialization and Interaction Process* (Parsons et al. 1955) and *Economy and Society* (Parsons & Smelser 1956).

The four-function paradigm. The four-function paradigm posits that every social system must continually confront and solve the four sets of organizational problems indicated below. The first two concern its external relations with its environment, including its physical habitat, the bodily needs of its members, and other social systems with which it must contend. The second pair of problems concern its internal organization as a human group of socialized persons with cultural commitments in interaction. The four organizational problems are depicted roughly as follows:

(1) *adaptation.* The external organizational problems of relating effectively to the physical and action environments. Most crucial here are procurement of resources needed for its activities, protection against physical and social threats, and developing information relating to these.

(2) *goal attainment.* The external organizational problem of effectively coordinating in any collective tasks directed outside the system itself.

(3) *integration.* The internal problem of maintaining gratifying relations between the members in their interaction, and avoiding disruptive conflicts. For small groups this concerns interpersonal relations, but for larger organizations it concerns intergroup relations.

(4) *latent pattern maintenance*. The internal organizational problem of legitimating the membership and activity patterns of the system, and also of adjusting the role demands on members so they are compatible with their other role commitments.

The paradigm has become known as the "AGIL" scheme by the acronym of the first letters of the names for these four functions. The importance of the scheme derives from an argument Parsons and Bales first made for small groups (Parsons, Bales, & Shils 1953): that the motivational and perceptual requirements for effectively dealing with each set of problems are inherently incompatible. A further suggestion was that the contradictory orientations needed could be partly indicated by the pattern variables. To simplify a complex thesis, it was argued that affectivity was facilitative for integration and goal attainment, but disruptive for pattern maintenance and adaptation, where neutrality (and detachment) were needed (cf., Parsons, Bales, & Shils 1953, especially p. 182). Parsons qualified and greatly extended this hypothesis in later works, but the core rationale was maintained. Because of the contradictory orientations needed for coping effectively with these problems, Parsons argued that small groups tend to differentiate into four somewhat distinct subsystems of role relations, each of which links the members in a somewhat different manner. For example, Bales had found in experimental task groups that different members tended to serve as leaders when the group focus was on a task (goal attainment) rather than personal relations (integration). In more complex organizations, specialized groups are likely to develop for dealing with each problem.

In *Economy and Society*, Parsons and Smelser boldly extended this idea to the organization of societies, arguing that an economy analytically could be thought of as the adaptive subsystem of an organized society, and the political system (termed polity) as the goal-attainment subsystem. They also began to develop a more fully evolutionary approach to societal variations, which centered in the functional differentiation hypothesis that: "total societies tend to differentiate into subsystems (social structures) which are specialized in each of the four primary functions" (1956, p. 47). At that time, the structural subsystems corresponding to the other two main functions only were discussed in relation to the economy as a focal concern. In the 1960s, however, Parsons identified these internal systems of

society more clearly, and they were called the societal community (integrative) and fiduciary (pattern-maintenance) systems (1971, p. 297).

One other central idea that crystallized in *Economy and Society* is that certain basic economic analogies can be applied to the other three sectors, and that each is a production system. Thus, Parsons contended that the polity subsystem dealt with the production of effective political decisions, while the integrative system was concerned with producing social solidarity. Most importantly, he viewed the four production systems interdependently, with each of their products seen as a necessary factor for the productive goals of the other systems.

Levels of society. While Parsons formally extended the four-function scheme from small groups in *Economy and Society*, the idea gained clarity and precision soon after when he more clearly identified four main organizational levels in developed societies. These levels were seemingly derived by extension generalizing the features of a work group in a factory (an organization of work groups) within an institutionally organized economy, to the other functional sectors. But there are also ties to Weber's analysis of bureaucratic organization in the modernization process. In Parsons' model, three main levels were distinguished in each functional sector: primary-technical or direct interactional; managerial or formal organizational; and institutional or supramanagerial level. Society itself then was viewed as the system at the highest level which served to link the various institutions together (1958; Hills, in Loubser et al. 1976, chapter 33).

The result is a levels-and-sectors model of developed societies, where differentiation must be seen in both its horizontal and vertical aspects. At each level, any social system is multifunctional, and has to develop adequate subsystems for all basic functional problems. But, as societies become more complexly organized and specialized, the social systems within them become more variegated in ways that the four-function system can partly portray. For example, in a small firm all members deal directly with each other, even though some begin to specialize more in procurement (part of adaptation) and others informally in morale problems (integrative). As the firm grows, it may form a personnel division of full-time specialists, some of whom deal with hiring (part of procurement) and some with morale problems. If it becomes sufficiently large, it may even develop a fiduciary

division of its own, with a board of trustees. From society's standpoint, the firm as an organization has adaptive (economic) functions; but taken as a reference point, some of the firm's divisions may give primacy to each of the other functions. In a comparable way, a small church (with pattern-maintenance functions for society) may grow to need full-time specialists for teaching, fundraising, bookkeeping, and custodial tasks.

Further extensions of the four-function scheme. Shortly after he sketched the outlines of the levels of social organization, Parsons extended the functional differentiation thesis to personality (1959a) and to culture (Parsons et al. 1961, pp. 963–996; Parsons 1967, chapter 5). For personality theory, the most important result was the systematic introduction of role learning into a Freudian-based socialization theory. Thus, as societies become more complexly differentiated, children must acquire the basis for taking part in roles increasingly different from those first presented in their family experience. While specialized organizations, such as schools, can provide for much of this, the prototypes must be set within the family if a drastic transition is to be avoided (Johnson, in Hallen et al. 1977, pp. 372–383). On the cultural side, more generalized, discriminate, and flexible systems of cognitive, expressive, as well as moral concepts and values are needed to guide individual and interactive choices. Individuals also require more complex interpretations of human existence (grounds for meaning) as social and psychological contingencies grow. In more traditional societies, a more limited set of morally sanctioned alternatives for proper role behavior in family, work, and other important relations can be delimited, but modernization is constrained unless more varied and flexible forms of relationships are made legitimate.

A fourth system of action. In the late 1950s, Parsons added a fourth major system of action to his roster, initially called the behavioral organism, to bridge the gap between personality and its biological base. Recently, however, two of his associates convinced him that this system could be better formulated in more cognitive terms, drawing on Jean Piaget, and it has been relabeled the behavioral system (1978a, pp. 352–353; Lidz & Lidz, in Loubser et al. 1976, pp. 203–205). The newer terms and concept will be used in the remaining discussion.

General action systems and cybernetic control. By the 1960s, with the addition of the fourth

behavior system, Parsons extended his four-function scheme still further, generalizing it to a general-action level of analysis. The result was a broader, more integrative concept of a general-action system that was concerned with the systemic links between the four major action systems (1960, reprinted in 1967, chapter 7). With this broader organizational sphere as a reference, social systems (and especially societies) can be seen as performing the chief integrative function for all action systems, while culture provides pattern maintenance, and personality and behavioral systems respectively provide goal attainment and adaptive functions. There are clear affinities between this functional argument and Parsons' differentiation thesis for societies that become especially important a few years later in his explicit evolutionary theory.

Along with this, Parsons moved toward a clearer model of action systems—their subsystems and intersystemic relations—which paved the way for a more flexible and expanded concept of functional analysis. This gave even greater emphasis to action *systems* as shown in his contributions to *Theories of Society* (Parsons et al. 1961, pp. 30–41) and were further refined a decade later (1970, reprinted in 1977c, chapter 10). As this model matured, the pattern variables and other structural classificatory variables became subordinate to the four functions and other system properties, and action processes and functions were viewed more explicitly as part of the continuum of life, with functions seen as problems of all living systems. While his analysis of action still maintained the distinctive feature of action theory—in viewing action from both the observer's and the actor's point of view—the latter came to occupy a somewhat smaller place in Parsons' total scheme.

At the same time, Parsons moved in the later 1950s toward a clarified model of relations between action systems on different levels. A major step was the introduction of cybernetic concepts into his scheme to clarify the causal relation between various types of action systems (1956; Parsons et al. 1961, pp. 36–41, 72). This led to the idea of a cybernetic hierarchy of conditions and controls as a vertical principle of intersystemic links to be paired with the horizontal principle of functional differentiation.

Cybernetic theory was stimulated in the 1940s by the invention of computers, which posed serious anomalies for theoretical physics. Here was a clear-cut case in which the behavior of a machine in some way was clearly influenced by

an idea system, and could not be fully explained without taking this into account. But, existing concepts in physics could not even depict the organizational pattern of a program. The cybernetic solution was to add the concept of information and to view the process as one of informational control. In this view, a system high in information but low in energy (like culture) could, under some conditions control a system with the reverse feature (like a social system).

To Parsons this was a move from a positivist heartland (physics) toward confronting idealist problems, since an information system was accepted as an emergent organization not reducible to properties of energy or matter (1978a, pp. 374–380). Some cyberneticists had begun to draw analogies with the rational behavior of individuals, but Parsons was one of the first to extend cybernetic concepts to social and cultural systems.

The evolution of societies. By the mid-1960s, Parsons had developed an explicit evolutionary approach to the comparative analysis of societies that integrated and expanded most of his previous arguments about societal modernization. In his earlier discussions, he occasionally suggested contrasts between modern societies and their earlier forms, but now he tried to portray the major stages of sociocultural development in a systematic and encompassing manner. In part, this represented his attempt to rework the classic nineteenth-century theories of Spencer and Comte as well as Weber and Durkheim. But Parsons drew on a much improved data base from anthropology, history, and other fields, also bringing a vastly more sophisticated framework to the task.

His model of social evolution was most fully presented in two small paperbacks on pre-modern societies (1966) and modern societies (1971), which are combined with a new introduction in *The Evolution of Societies* (1977a). Written for the general college student, the latter may be the best place for a reader interested in Parsons' developed theory to delve into his work. While the main focus is on the development of societies, his general-action system (linking societies to culture and personality) is used to avoid numerous traps that plagued earlier social evolutionists.

Parsons distinguished three main growth stages of societies—primitive, intermediate, and modern—but these are by no means thought to portray all or most known societies. Rather, they

are seen as major growth phases of the “trunk of a branching tree,” revising a metaphor Parsons applied in his dissertation to Weber's evolutionism (1928–1929, vol. 37, p. 43). Some diverging and transitional types are also noted. In contrast to some of the nineteenth-century unilinear evolutionists, Parsons did not assume that any modern society had gone through the earlier “pre-adult” growth phases before reaching maturity. Instead, he saw the evolutionary process as a *general* evolutionary process that applies to the entire cultural development of mankind. This process is related first to human general-action systems, and second to specific societies. Thus, Parsons traced Western societies culturally from ancient Greece, Israel, and Rome, the first two of which were destroyed as political entities for a long period, but are still seen as cultural seedbeds for later developments. And, the old debates about evolution versus diffusion are simply put to rest, along with those about evolution versus functionalism. In each case, the alternatives are deftly and sensibly combined.

Broadly, Parsons defined evolution for any living system as growth in its generalized adaptive capacity. Specialized parts are progressively differentiated in a way that permits their more flexible mobilization for more varied purposes. Parsons' approach, then, was one of evolutionary functionalism, and his four-function model is applied to human action systems at all historical phases. Primitive societies are portrayed above all by their limited differentiation, and also by the predominance of kinship in their social organization and of religion in all phases of their culture. In this view, such primitive systems begin to evolve (to what he terms an advanced primitive stage) when they developed political and economic relations partly free from kinship and tribal control.

In Parsons' three-stage model, social evolution involves two basic, complex transformations, each of which centers on a cultural innovation. The conversion from primitive to intermediate society is linked to the introduction of writing, the importance of which is routinely assumed in the common anthropological reference to simple societies as nonliterate. Using his four-system model, however, Parsons gave this idea a more systematic grounding. The onset of the intermediate stage requires the differentiation of culture from the specific social systems and personalities with which it is associated. In an action system limited to an oral tradition, this

separation is difficult, but with writing, cultural values and beliefs are more easily disassociated from their adherents, and objectified (e.g., instead of "they believe," one can say, "it is written"). This eases cultural diffusion to larger groups, whether initiated by conquest, proselytization, or absorption, and greatly aids the integration of larger populations with more divergent roles into the same social system. Writing also helps to standardize the basis for cumulative growth in every aspect of culture, and creates a new basis for individual socialization and learning.

The main cases of intermediate societies that Parsons analyzed were the empires of ancient China, India, Islam, and Rome. While literacy in all these cases was limited to certain elites (especially the clergy and some political officials), they contributed to the growth of more universal laws, and in time, to scientific and technological developments as well. Parsons did not ignore that each of these empires was initially formed in part through military conquest. But expanding an earlier theme, he argued that conquest alone is a limited basis for maintaining a complex division-of-labor over centuries and for fostering cultural growth.

Against the comparative background of the four empires mentioned, Parsons took up Weber's question of why modern industrialization first developed in the West rather than in China, which, until the fourteenth century, technologically surpassed the West. While many factors are cited, Parsons argued that this was crucially linked to the more universal character of Roman law that introduced the concept of modern citizenship on which nation-states were later built. The Christian concept of membership in a universal church and the separation of the spiritual and temporal spheres, which partly preserved and augmented the Roman legal concept after its empire dissolved, were also important. The fall of Rome as a political system was seen by Parsons as a regression from an evolutionary standpoint as the Western world broke up into smaller units for centuries. But culturally, Roman Catholicism maintained and expanded the larger membership boundaries in the West, and important features of Roman law survived in the church and in various common law traditions. Again, the argument echoes Comte, Weber, Toynbee, and others, but the difference lies in the much greater conceptual discrimination and unity maintained.

This sets the stage for the second transforma-

tion, which is historically sketched for certain parts of the Western world (and should be read with Parsons' article on "Christianity" [1968a]). Modern societies first emerged in the northwest corner of Europe after the Renaissance and Reformation, especially in England, Holland, and France. Of many factors complexly intertwined, Parsons singled out the growth of universal legal systems in the emergence of these three nations. However, the development is attributed to the European cultural system rather than to specific societies, with different sectors and nations alternatively taking the lead at different points. But the main initiative, modifying Weber's thesis, is attributed to the Protestant nations and to France.

Where the transformation to ancient civilizations centered in the differentiation of culture from social systems, the modern transformation involves the differentiation of the four main sectors of society defined in four-function terms. This occurs in a complex series of changes extending over centuries. An early step is when the restrictive controls of kinship and church over the societal community are reduced so that people of varied ethnic and religious backgrounds can more flexibly join in larger and more diverse divisions of labor and other enterprises. The separation of church and state and the emergence of modern concepts of citizenship are assumed to aid this process. As extended kinship obligations of the past contract in importance, it is important that the smaller nuclear family emerges as the main operative kinship unit. This relates to the decline in kin groups as the organizing units of economic production, and the emergence of factories and business firms more flexibly combining workers on the basis of skills rather than family ties. The reduction in importance of more distant kinship ties increases mobility and aids the development of a more fluid labor force. Colonial resettlement in America by Europeans, which involved the break-up of larger families in Europe, is a main example of Parsons' point. Culturally, another important aspect of the process is a secularization trend, in which science and technology are differentiated from religious constraints, as symbolized by the Renaissance.

On another level, Parsons argued that analyses of modernization have given undue importance to the industrial revolution, particularly to the economic side of the process. Instead, he claimed that three revolutions of comparable importance were involved: the democratic, edu-

cational, and industrial. He further maintained that each is dependent on the other two at each successive phase of development. The educational revolution ties to the secular trend, but especially involves the separation of occupational training from family and church. This first occurred with the guild apprenticeship system and the development of training centers for professional careers in law and medicine. As industrial and political developments proceeded, a more differentiated, specialized, and flexible educational system was needed, and universities arose to meet this need. These provided a broader scientific basis for the increasingly diversified and changing occupations that modern organizations require. But they also aided the more complex process of role socialization and moral orientation entailed by growing social complexity (Parsons & Platt 1973).

Especially in its later twentieth-century developments in the most advanced societies, Parsons concluded that modernization required a new kind of man and an unprecedented system of values or morality. The new man envisioned is not the organizational man conceived by many critics of modern society who must fit into gigantic bureaucratic machines, nor is it the other-directed person who shifts with every riptide of fashion in a rootless manner. To Parsons, the former conception applied to earlier stages of modernization where regimentation was often needed to begin the process. At more advanced stages, however, the occupational system becomes increasingly professionalized, and larger numbers can find creative challenges in their work if they have the proper training. In contrast to critics of mass culture, Parsons saw industrialization as providing greater leisure time, and he saw mass cultural production as making the fine arts and other cultural benefits (previously confined to the privileged elites) available to larger numbers. Thus, a substantial upgrading in consumption as well as production standards occurs, at least in potentiality. But education is needed to provide the appreciative frameworks needed for increasing numbers to take advantage of this potentiality.

If this brief synopsis makes Parsons' view of modernization sound idyllic, it misleads. Parsons believed that compared to societies of the past, modern societies—and especially democratic, post-Keynesian, capitalist societies—have an unprecedented material standard and more potential freedoms for their members. But he was acutely aware that modernization is highly aliena-

tive for many, creating new anxieties and frustrations. In part this is due to the institutionalization of greater individual freedom itself, a point Durkheim had centrally raised in his studies. Many are bewildered by the enormous menu of choice proffered, and seek refuge in more traditional, simpler alternatives. Parsons interpreted various twentieth-century movements, including aspects of German Nazism in the 1930s and the New Left in the 1960s, partly as fundamentalist reactions against the new demands of modern freedoms (1977a, pp. 193–195).

Parsons argued that the new freedoms of modernization place an unprecedented moral burden on individuals. In past and traditional societies, people often live their lives surrounded by long-standing acquaintances and guided by fairly detailed standards of proper behavior. Their choices are largely restricted to a small number of alternatives. By contrast, advanced modernization opens up vast ranges of alternatives, and presents individuals with frequent changes that require flexible moral codes and complex moral judgments. In his view, the educational revolution above all is needed in this later phase to prepare individuals for living adequately with such complexities.

Parsons' evolutionary model has come in for various kinds of criticism. Many claim that he uncritically accepted the standards of his own society in his own time as a standard of human development, a point of which he was well aware. More specifically, some argue that he was narrow in his view of primitive societies and tended to residualize them. A second criticism is that he assumed democratic societies inherently have greater productive capacities than socialist alternatives, which may not continue to be true in the future (Bialyszewski, in Hallen et al. 1977, pp. 395–405). Even if he is right on the economic side, another argument is that he may have underestimated the social and personal costs of technocratic societies, and overestimated the flexibility of socialization (Lasch 1977). This is one specific aspect of the recurrent criticism that Parsons holds an "over-socialized view" of man. It becomes more pertinent when applied to his recent evolutionary arguments than to mistaken interpretations of his "static functionalism" in the past. Thus, as Guy Rocher—a former Parsons student and one of his best expositors—suggests: "Parsons' functionalism in itself is a dynamic approach. But it is thwarted by [his] evolutionist interpretation

of [capitalist] industrial society as the final peak after a long and difficult climb" (1975, p. 158). This writer does not believe that Parsons saw any existing society as anything like a "final peak" of human evolution. Analytically, his ideological-historical preferences (or those of any other theorist) can be separated from his theoretical model and empirical claims. His developed action scheme and his basic modernization analysis do not uniquely lead to diagnostic conclusions about the merits of various societies, and his theory might best be further developed by opening it again to a wider range of empirical possibilities. By his own basic criteria, this would seem to be the more modern way.

Parsons in retirement

After his retirement from Harvard in 1973, Parsons continued his writing and other activities. He taught at the University of Pennsylvania, Brown University, Rutgers University, the University of California at Berkeley, and Kwansai Gakuin University in Japan. In addition, he gave numerous special lectures that included every continent except South America.

His continuous flow of essays is partly exhibited in his most recent collection (1978a), the majority of which first appeared after 1973. At the time of his death, he was working on a major book, tentatively entitled "The Integration of Modern Societies," in which he sought to integrate his many-sided analysis of modernization over the past three decades.

He also continued to expand his theoretical universe, extending his "media of interchange models" to a much broader realm. This model first was developed in the mid-1960s to characterize the key symbolic standards needed to maintain effective links between economic, political, community, and fiduciary sectors of highly differentiated societies. His well-known argument (still poorly understood) is that the role of *money* in economic markets has counterparts in standard symbolic codes for *power*, *influence*, and *value commitments* in the other three sectors (1967; 1969, part 4). A key hypothesis is that all four symbolic standards must be *combined* for any of the four sectors to retain its functional distinctiveness. Thus, economic transactions require that adequate investments of power, influence, and value commitments as well as money must be made. This means for example that without a sufficient basis of political, social, and moral support, an economic venture like the opening of a privately owned

nuclear energy plant cannot succeed no matter how much money and materiel is put into it. It also means that in complex societies, a government or church sponsored program needs monetary resources as well as support in terms of the other media. A related hypothesis, moreover, is that organizations in each of the four sectors depend most on a distinctive mix of the four symbolic media corresponding to their respective functional priorities.

By the early 1970s, Parsons began to extend his concept of symbolic media from societies to general action systems. This involved the argument that before standardized symbols, like those of money, power, or influence, could gain acceptance in a society, more basic symbolic codes must be instilled in the collective and individual consciousness of its members. Thus, the acceptance of money (a set of monetary codes) as a measure of the value standards for innumerable goods and services, is hypothesized to depend on prior acceptance of more generalized symbols of "intelligence" or rationality. Similarly, the credibility of symbols of power or influence is thought to depend on the acceptance of more basic standards of "performance" and of "affect" (Parsons & Platt 1973, appendix). In 1975, Parsons made another leap and broadened the media concept to include relations between human action in its totality (i.e., historical experience) to the physical and organic worlds beneath it, and to a "telic" realm of human potentialities above it. The later side removes the very fine membrane separating the most ambitious scientific efforts from the philosopher's quest. His extended meta-metatheoretical framework here was called a paradigm of the human condition, and is the subject of three provocative essays in his last collection (1978a), including its culminating title essay. It opens up whole new possibilities for the exploration of the dynamic links between biology and human action, with more ultimate implications for the kind of "moral science" that Hobbes, Locke, Comte, and other founders of the social sciences once envisaged.

Parsons' theoretical work clearly always has reflected strong ideological or moral concerns. In his writings, however, he mainly was negative in his explicit value positions. That is, while he publicly opposed certain ideological positions which deal too narrowly with human social realities as utopian (e.g., overly stressing equality or social unity or individual freedom, while ignoring many other values and conditions on

which these depend), he stopped short of explicit moralizing himself. Having started by stressing the selectivity of theoretical concepts in science (especially social science), he strove for a more comprehensive "sampling" in social theories and research. As he put it a decade ago, "[any] regularity of relationship can be [better] understood if the whole complex of interdependencies of which it forms a part is taken into account" (1968*f*, p. 458).

Concluding note. Parsons' conceptual and theoretical effort to restore the connection between the increasingly differentiated social-behavioral sciences is unparalleled by any contemporary. He created a framework for the analysis of social processes in the broadest sense that places more different organizational processes (psychological as well as cultural and social) on the same theoretical map than any other existing scheme. He has also broken through numerous philosophical and disciplinary barriers, and has succeeded remarkably in forging a bridge between science and history as well as between positivism and idealism.

His developed scheme succeeds better by positivistic standards than is generally recognized, not as a precise operational framework, but as a generative corpus for social inquiry. This is best shown by the thousands of studies and dozens of middle-range theories it has inspired, and by the innovative ways they often trespass established boundaries (Barber & Inkeles 1971; Loubser et al. 1976). But like other pioneering theorists of the past, Parsons has developed one special version of his theory with many important restrictions, and it is likely to undergo significant modifications in the future.

In 1979, the University of Heidelberg sponsored a special program to celebrate the fiftieth anniversary of Parsons' receiving his Ph.D. Both Parsons and some of Germany's leading sociologists gave papers. It was a rare tribute to a foreign alumnus. On May 8, 1979, the day after the program, Parsons died in Munich.

MARTIN U. MARTEL

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PEARSON, E. S.

Egon Sharpe Pearson was born in London in 1895 and is now living in retirement in Sussex. As the son of Karl Pearson and as one of the most eminent statistical figures of this century, Pearson has enjoyed a unique vantage point from which to view the development of statistics. Educated at Winchester and at Trinity College, Cambridge, he joined Karl Pearson's department of applied statistics at University College London in 1921 and attended all of Karl Pearson's lecture courses. The father influenced the son deeply, but mainly by force of example and by providing him with many of the tools for future work. E. S. Pearson's statistical philosophy was shaped to a much greater extent by W. S. Gosset ("Student"), particularly by Gosset's pioneering work in small-sample theory which was later put on a rigorous footing

and much extended by R. A. Fisher. Pearson was excited by these developments with which his father never felt wholly comfortable. A few years later Pearson began the extraordinary ten-year collaboration with Jerzy Neyman for which both men are best known despite their numerous important individual contributions.

When Karl Pearson retired in 1933 his department was split in two; Fisher was appointed Galton professor of eugenics, Pearson reader and in 1935 professor of statistics. This arrangement inevitably resulted in jurisdictional problems, but Pearson never allowed such matters to interfere with his high regard for most of Fisher's work. With his father's death in 1936, Pearson took over the editorship of *Biometrika*, retaining it until his retirement in 1966; he continued until 1975 as editor of *Biometrika* auxiliary publications.

Endowed with a sense of history and an exceptionally lucid style, Pearson has contributed much to an understanding of the issues in which he or his father were principals. His biographical accounts in the 1930s of Karl Pearson and Gosset are masterly. In 1955 he instituted in *Biometrika*, with the help of Maurice Kendall, a still ongoing series of articles, by different authors, on the history of probability and statistics, a first collection appearing in Pearson and Kendall (1970). In 1978 he completed the editing of lectures delivered by Karl Pearson from 1921 to 1933, on the history of statistics in the seventeenth and eighteenth centuries.

Robustness, experimental sampling, and Pearson curves. These three rather different topics, all of which proved to be long-term interests of Pearson, were first united in a short paper entitled "The Distribution of Frequency Constants in Small Samples from Symmetrical Populations" (1928). Using L. H. C. Tippett's random numbers to generate 1000 samples of size 2 and 500 samples of 5, 10, and 20 from symmetrical Pearson-type distributions with various coefficients of kurtosis $\beta_2 (= \mu_4/\sigma^4)$, Pearson (assisted by N. K. Adyanthāya) obtained estimates of mean, standard error (s.e.), $\beta_1 (= \mu_3/\sigma^3)$, and β_2 of the range and also of the s.e. of midpoint and median. Supplementing these experimental sampling values with exact results for rectangular and normal populations, he was able to conclude *inter alia*: "where it is known that the population is leptokurtic [$\beta_2 > 3$], the use of the median rather than the mean as a central estimate certainly appears to be worth consideration . . . it is not a satisfactory estimate in

the case of platykurtic [$\beta_2 < 3$] distributions" (1928, p. 358).

It is clear that Pearson is concerned here with what would today be called the robustness of estimation, more specifically with the effects of nonnormality. His approach is a direct precursor of the current Monte Carlo studies of robustness of estimators of location and scale. The article, labelled "Preliminary Notice," was soon followed up by extensive similar studies of the effects of skewness as well as kurtosis (a) on Student's t —the statistic and the test—and (b) more generally in the analysis of variance. In 1929 Pearson put two of the key questions with typical directness: "How sensitive are 'normal theory' tests to changes in the population form? May I use some with less hesitation than others?" (1929, p. 259). Such questions were never far from his mind when a new test was developed and some answers were usually provided. Later, mainly through Monte Carlo methods, more detailed results could be presented in a readily appreciated form (Pearson & Please 1975).

The use of Karl Pearson's system of curves to represent underlying populations was characteristic of time and place. E. S. Pearson was also frequently to employ the system as a means of approximating theoretical distributions too complex to be handled exactly.

Random numbers were developed by Tippett at Karl Pearson's suggestion as an adjunct to his well-known 1925 paper on the range in order to "illustrate and confirm" the theoretical results he had obtained. The use of these numbers in robustness studies appears to have been the result of Pearson's influence, with some encouragement from Gosset. Both had become concerned with the too ready assumption of normality which followed Fisher's great success in developing an elegant small-sample distribution theory under population normality.

Collaboration with Neyman. From 1928 to 1938 ten papers by Neyman and Pearson appeared, the essence of which has become an integral part of every statistician's education and vocabulary. One can discern in them the evolution of many familiar concepts and methods, beginning with the notion of the two types of error. Employing Fisher's representation of a sample (x_1, x_2, \dots, x_n) as a point in n -dimensional space, and building on his use of the likelihood function in the theory of estimation (Fisher 1922), Neyman and Pearson developed the likelihood ratio criterion as a unifying method of test construc-

tion. Basic to this was the consideration of hypotheses H_1, H_2, \dots as alternatives to the hypothesis H_0 under test. An error of type I occurs when H_0 is rejected although true, an error of type II when H_0 is accepted although one of H_1, H_2, \dots holds. Balancing these errors depends on the seriousness of their consequences and is an important but not a statistical problem. The procedure advocated is first to control the type I error—in the sense that repeated applications of the test used lead to false rejection of H_0 in a specified proportion α of cases; and, second, to choose a test with good power properties—i.e., one that can effectively keep down the probability of a type II error.

The likelihood ratio criterion, put forward without any claim of optimality, gained in strength by its ability to reproduce many of the existing tests as well as by providing appealing new tests. A major breakthrough occurred in 1933 with the search for optimal procedures. It was shown that in testing a simple (i.e., fully specified) hypothesis against a simple alternative H_1 , the likelihood ratio test is optimal, among all tests with the same α , in the sense of minimizing the probability of accepting H_0 when H_1 is true. This result, arrived at by a calculus of variations argument, has become known as the Neyman–Pearson fundamental lemma. Matters were shown to be more complicated when either or both of H_0 and H_1 are composite (i.e., when one or more parameters are unspecified). Under favorable circumstances the best critical region (the best region for rejection of H_0) may be common to all members in the specified class of alternatives. In such cases the resultant test is said to be uniformly most powerful (UMP). In the absence of an UMP test, other criteria, such as unbiasedness, were proposed to help one choose from possible tests.

Both technical and personal aspects of the Neyman–Pearson collaboration, much of which was carried out by mail, have been well described by Pearson. He was the initiator of the partnership, having arrived through the study of special cases at some of the main early ideas including the likelihood ratio criterion and what was later termed the power function. It required Neyman, however, to provide mathematical refinement and generality, and to take the lead in the pursuit of optimal approaches.

Biometrika tables and other activities. In the 1930s Pearson became interested in the use of statistical methods in industry. One upshot was

a major publication (1935) issued by the British Standards Institution, for which he has long been a consultant. During World War II, Pearson, together with most of his University College staff, was attached to the British Ordnance Board.

The year 1954 saw the publication, joint with H. O. Hartley, of *Biometrika Tables for Statisticians (BTS)*, volume 1, a project for which Pearson had been preparing since the late 1930s. This collection of what were deemed the more commonly used tables was supplemented in 1972 by a second volume providing more specialized tables as well as more extensive tabulations of the main distributions. The influence of Karl Pearson is unmistakable, and not only because the tables were conceived to replace his largely obsolete *Tables for Statisticians and Biometricians*. Nearly all the tables in volume 1 of *BTS* had first appeared in *Biometrika*, many suggested or coauthored by Pearson. Most notable among the latter are the Clopper-Pearson charts providing confidence limits for the binomial parameter; percentage points of Pearson curves; and a number of tables related to the range, a perennial favorite of Pearson's. The well-known Pearson-Hartley charts for determining the power of the *t* and *F* test in a fixed effects model are reproduced in volume 2 which contains tables from many sources besides *Biometrika*.

Biometrika Tables have been widely recognized as models of their kind. Throughout, meticulous attention is paid to ensuring an attractive layout and a form of tabulation permitting easy interpolation. The volumes gain immeasurably from invaluable extensive introductory material in which the use of each table is clearly explained and richly illustrated.

Pearson continues to be active in retirement and is at present putting together some ninety letters he exchanged with Gosset from 1926 until Gosset's death in 1937.

HERBERT A. DAVID

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1975 PEARSON, E. S.; and PLEASE, N. W. Relation Between the Shape of Population Distribution and the Robustness of Four Simple Test Statistics. *Biometrika* 62:223-241.
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PHILLIPS, A. WILLIAM

Alban William Housego Phillips (1914-1975), known by the second and simplest of his given names, received no university education until he was past thirty (and then the subject was sociology rather than economics); had a short professional life of less than twenty years before suffering a crippling stroke; and produced fewer papers than many American assistant professors have needed to gain tenure. Yet his name became a household word among economists, students of economics, and readers of the financial sections of newspapers through his depiction of the "Phillips curve"—the relationship between the rate of inflation and the level of unemployment.

Phillips was born to a New Zealand farming family and was educated at New Zealand state schools until the age of 16. He became an

electrical engineer by the practical route and embarked on a wandering career that included an interlude of crocodile hunting in an Australian mining camp, a journey across the Soviet Union on the trans-Siberian railroad, and a period as a prisoner of war. This last episode occurred after he had arrived in London, worked for the London Electricity Board, joined the Royal Air Force as a technical officer (1940), and been posted to the Far East. For his activities while a prisoner of war he was made a member of the Order of the British Empire (1946).

His academic career began when, after World War II, he enrolled as a student at the London School of Economics (L.S.E.). Like many others after the war, he felt the need to understand more about "society," and thus he chose to study sociology. It was the required course in economics that caught his imagination, however, and in particular the stock-flow relationships depicted in Kenneth E. Boulding's text *Economic Analysis* (1941). He felt that important aspects of the working of the economic system could be demonstrated, at least for teaching purposes, by a machine in which flows and stocks of economic variables were represented by visible flows and stocks of liquids, and he devoted his time and considerable ingenuity to the design and construction of such a machine. The prototype, built in collaboration with W. T. Newlyn of the University of Leeds, was installed in the University of Leeds. Its design, together with a general analysis of the potential use of mechanical and other analog models in economics, formed the subject matter of Phillips' first published paper (1950).

Phillips had shown the prototype to James E. Meade of the L.S.E., who was considerably impressed and arranged for Phillips to demonstrate his machine before the prestigious research seminar conducted by Lionel Robbins at the school. The tale is told of how this ordinary and slightly impoverished looking man, already ten years older than most of the graduate students and bright young teachers who filled the seminar, an unknown figure with no apparent professional credentials, appeared with his machine and also with his proud landlady, and of how he stood up under the ferocious attack and cross-examination that was one of the seminar's traditions. Within a short time Phillips was offered a teaching post at the school, which also financed the construction of a machine for its own use. This second version was more sophisticated than that

at Leeds and, at Meade's suggestion, represented the working of two separate economies linked through a balance-of-payments mechanism.

His 17 years at the L.S.E. constituted the whole of his effective professional career. In the first four he obtained his doctorate and rose from assistant lecturer to lecturer to reader. Within four more years he became the occupant of the Tooke chair of economic science and statistics (a chair, last held by Arthur Lyon Bowley, that was revived specifically for him). In this period, too, he married and became the father of two daughters.

All of Phillips' work at the L.S.E. was devoted to a single objective, understanding the economy as a dynamic system and learning how best to maintain it on an even course. He took a macroeconomic perspective and had little interest in relative prices or other microeconomic concerns. His thinking was always influenced by his engineering background, which led him to take a somewhat mechanistic approach to economic modeling but gave him a much healthier respect for the importance of lags and delays in response than was usual in the 1950s. His second published paper (1954) was devoted to the problem of stabilization policy in an economy with response lags. In this paper he made the important distinction between "proportional" policy, in which corrections were made in response to current errors in economic variables; "integral" policy, in which the response was related to the cumulated deviations from the desired state; and "derivative" policy, in which response was related to the rate of change in the target variables. He showed that the optimal policy would depend on the response lag properties of the system, and that it would consist of a mix of proportional, integral, and derivative components.

As he further probed the dynamic properties of economic systems, often by simulation studies on the analog computers at the National Physical Laboratory and elsewhere (the digital computer revolution was just around the corner, but had not yet arrived in Britain), Phillips became convinced that there was little to be said about the specifics of policy unless the lag structures in the economy could be determined. He became more interested in the statistical problems of estimation, as was apparent in his 1956 and 1957 papers.

The famous "Phillips curve" paper of 1958 emerged from this increasing empirical interest.

It began as a simple study of the relationship between wage rate changes and unemployment, designed to provide one set of dynamic parameters for use in his studies and chosen because of the availability of the data that had been assembled by his colleague, E. H. Phelps Brown. After plotting the data, Phillips noted that a curve fitted better than the straight line he had expected. The curve showed that zero wage change occurred at a positive level of unemployment; that wages increased at an increasing rate as unemployment fell below this level; and that the rate of increase became very large as unemployment approached zero. However, unemployment above the zero wage change level caused little downward change in wages, and the rate of fall in wages was relatively small even as unemployment became very large. The published paper formalized the results obtained from the freehand curve by estimating the parameters of a specific nonlinear relationship, using ingenious but entirely *ad hoc* methods devised by Phillips, discussing the fit of the data (those for the United Kingdom from 1861 to 1957) in detail, and providing some underlying theory. Phillips noted, but did not stress, the aspect of the curve that was to become famous—the portion showing the tradeoff that must apparently occur between reducing inflation and reducing unemployment.

The impact of the paper undoubtedly gained strength from the rapid follow-up by R. G. Lipsey (1960), one of Phillips' colleagues at the L.S.E., who reestimated the relationship by less eccentric methods and provided a firmer theoretical foundation. Within a few years, "Phillips curves" had been estimated for many countries, had been incorporated into the eighth (1970) edition of Samuelson's widely used basic text, and had appeared in the President's economic report to Congress in 1969. The first 20 years of the Phillips curve literature, surveyed by Santomero and Seater (1978), shows no less than 228 references. The reader is directed to this survey for a detailed account of the Phillips curve and its significance.

Phillips published only three more papers, in 1961, 1962, and 1968. They reiterate his constant theme—the problems of formulating economic policy in a dynamic context. He became increasingly aware of the difficulties of estimating the relationships he considered necessary for policy design and of the fact that the necessary techniques were beyond his grasp. His intellectual integrity was such that he felt he

could not continue to "profess" an area in which he felt he had no further contribution to make, and he believed that he could contribute more by taking up the study of the economy of China, a country of which he had firsthand knowledge from his early wandering days and in which interest was growing rapidly. In 1967 he left the L.S.E. to accept a chair at the Australian National University, hoping to develop his new interest in Canberra. He had barely started in his new field when, in 1969, he suffered a severe stroke from which he never fully recovered. He retired from his Australian chair in 1970 and returned to his native New Zealand. There he taught part time, conducting a seminar on the economic development of China until his death in 1975 at the age of 60.

KELVIN LANCASTER

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PIAGET, JEAN

Jean Piaget was born on August 9, 1896, in Neuchâtel, Switzerland, where his father taught medieval literature at the local university. Although Piaget received his doctorate from the

University of Neuchâtel in biology (1918)—an interest he has continued in his experimental and theoretical work—he was also a student of philosophy and the history of science, as well as a participant in a practicum of experimental psychology and in a didactic psychoanalysis. In 1920, Piaget became an assistant at the Alfred Binet Institute in Paris, where he collaborated on the standardization of an intelligence test for children. As he analyzed the answers of children to the probing questions used in this project, he realized that a judicious use of the clinical interview could become the key to exploring the thinking of the child. While his supervisors in the test-and-measurement tradition focused on the statistical distribution of correct responses, Piaget was fascinated by the explicit or implicit reasoning behind the children's responses, whether they were right or wrong. A faithful description of the formal characteristics of children's thinking, of how it changed progressively from childhood to adulthood, would, Piaget thought, go a long way toward answering questions about the development of the intellect that had engaged philosophical thinking for centuries. By that time Piaget had already decided what his life's work would be: he would attempt to turn epistemology—the heretofore philosophical theorizing on the nature of knowledge and scientific truth—into an empirical investigation with controlled observation and appropriate logical consistency.

From the start it was clear to him that this would be an interdisciplinary task, involving philosophy, biology, the history of science, and psychology. Philosophy is indispensable because it poses the decisive questions and provides criteria of logical analysis, while biology, standing at the opposite pole from philosophy, anchors the epistemological task in the behavior of a living subject. The history of science and its relation to common sense knowledge are clearly relevant; unfortunately historical records are often incomplete, particularly in those formative periods when new ideas are being discovered and elaborated. As a consequence, Piaget saw psychology, or more specifically, the study of the child's development of thinking, as the most direct way of approaching the epistemological problem empirically, and that is where the bulk of his research has been conducted. It is not surprising, therefore, that Piaget is generally considered to be a psychologist, even though he prefers to see himself as the originator of a new empirical discipline: genetic epistemology.

A brief overview of past epistemological trends will place Piaget's work in historical perspective. René Descartes had set the scene by splitting sense knowledge from the rational intellect. Having thus severed the intellect from its biological base, its origin and capacity for engendering objective truth about the world became an acute problem. Philosophers of rationalist tendencies, such as Leibniz, ascribed the universal and necessary characteristics of logical thinking to the God-given nature of the intellect and explained its conformity to the objective world by the happy coincidence of a divine providence. The British empiricists objected to these views as altogether too speculative and void of any observable account. Instead, they postulated sense experience as the sole base of intellectual knowledge. The dilemma created by these opposing epistemologies was brought to a head by Kant with his discovery of the epistemic subject. Kant argued that scientific knowledge is not something absolute or thinglike, but the relation between a subject and an object. Thus "objective" knowledge—its name notwithstanding—has two components, an objective component neglected by the rationalists and a subjective component denied by the empiricist. Kant referred to the objective component as a posteriori and to the subjective component as a priori. The source of the a posteriori component is experience and it provides the specific content of knowledge, while the a priori component is illustrated in logical categories, such as space, class, and causality, and is the conceptually prior condition of knowledge.

Kant's epistemological revolution was seen as the culmination of a philosophical inquiry into the nature of knowledge at a time when philosophy and science were still largely undifferentiated. Piaget (1965) points out that all philosophers of the past had been scientists, acquainted with the business of patient empirical discovery and controlled observation. After Kant, as more and more sciences split off from their philosophical matrix, philosophy itself became one among other academic disciplines. The bridge Kant had built was widely acclaimed as solving the epistemological dilemma. However, it proved to be quite precarious as philosophers turned to other problems and reenacted the previous extremes of rationalism and empiricism in a different version. The young Piaget grew up in a world of ideas where science flourished and expanded in unparalleled progress. Darwin had established the evolutionary origin of biological species; Hegel

and Marx had pointed out the historical origin of particular societies; and Freud had described the individual origin of personal mental disturbances. A philosophical school, almost slavishly subject to a particular view on science, arose and proclaimed positive "facts" as the only legitimate objects worthy of knowledge. Adherents of this positivist approach quickly came to realize that in addition to "facts" a language is needed to articulate the facts and make them communicable. In the perspective of this logical positivism, and its more recent form of analytic philosophy, knowledge is an internal representation of an objective reality and logical thinking is reduced to, and for all practical purposes identified with, the proper use of a conventional language. As a reaction against this empiricist approach in philosophy, and also against academic psychology which had developed in a blatantly positive framework, vitalistic and phenomenistic philosophers stressed the subjective side of human experience. They went so far as to suggest that the "objective" knowledge of science and the "subjective" knowledge of personal experience are radically different and perhaps even irreconcilable.

When Piaget began his own research, he was responsive to all of these epistemological trends, but his stance is in stark contrast to all of them. By using the clinical interview technique as a tool to probe into the origin of Kant's a priori categories, Piaget opened up Kant's system of philosophical theorizing to empirical verification. When Piaget asked, how does knowledge come about?, rather than, what is the nature of knowledge?, he had effectively turned a philosophical quest into an empirical investigation. In doing this he merely followed Bacon's notion that to understand something we have to know its method of construction.

In 1921, the renowned psychologist Édouard Claparède, who had read one of Piaget's first articles in psychology, offered him the post of director of studies at the Jean Jacques Rousseau Institute. Piaget at once embarked upon a career of research, teaching, and publishing, which over the years has resulted in more than sixty books and many more monographs and articles. In 1925, Piaget became professor of psychology, sociology, and philosophy of sciences at Neuchâtel, and four years later he returned to Geneva where he still resides. After teaching the history of science at the University of Geneva for ten years, the university established the chair of experimental psychology, which Piaget occupied from

1940 until his retirement in 1971. During the entire period from 1921 onward, he continued his research activities at the institute, and eventually became its director. In addition, he taught at the University of Lausanne (1938–1951) and at the Sorbonne (1952–1963). In 1956 he founded an interdisciplinary Center for Genetic Epistemology in Geneva, which organizes yearly symposia and issues a series of monographs.

When his three children were born between 1925 and 1931, he searched meticulously for behavioral signs of the growing intellect in the action of the infants, and extended the radical constructivism of his first five books on child thought, published between 1924 and 1932. He noted that such notions as object, space, time, causality were not present at birth, but constructed in a gradual fashion during the first two years of life. In three books of this period he described six developmental substages of what he came to call the sensorimotor period, from stage I, which is almost entirely reflexive (or innately programmed), to stage VI, which precedes the period of operatory activity. Reflex activity relates to the logic of physiology and the nervous system; sensorimotor activity is the logic of action, and the "usual" logic of adult reflective thinking is the logic of operations. These represent the three basic developmental stages, and it is important to grasp both what all these stages have in common and how they differ. Sensorimotor is a means-end intelligence that benefits from past experience and is a coordination of external actions to reach present goals. This is contrasted with reflex activity that is preprogrammed (by evolution) in its goal and external action and operatory activity that deals with concepts, representations, and mental (rather than merely external) action. The bridge Piaget wanted to build between biology and logic (operations), he discovered in the sensorimotor logic of action. There is logic in action because all action implies an assimilation of external realities to what Piaget calls schemes. Development is precisely the gradual construction of these schemes of assimilation.

Consider the notion of "objects"; for instance, this book you are reading. This notion does not refer to your knowledge of *what* a book is (the "concept" of book), but rather the more primitive notion *that* the book is an object of knowledge, with a permanent existence, separate from your sensorimotor actions on the book. Piaget demonstrated that infants at first do not at all search for a vanishing object. Later on,

when they begin to search, they look for it in a place where they had previously found it, even though they perceive that it is being hidden in a new place. Apparently for these infants, people and things in their environment are not separate entities, but extensions of their own activity. Only toward the end of the sensorimotor period is there systematic search for a hidden object, from this Piaget infers that infants have now acquired the notion of a permanent object together with a framework of space, causality, and time; but all that is as yet limited to sensorimotor action knowledge.

Conceptual knowledge, along with the ability to represent absent events, begins at this point. With the help of an increasing number of students and collaborators, such as Bärbel Inhelder, Piaget devoted a whole series of researches and publications, beginning around 1940, to the detailed description of the development of various general concepts, such as number, quantity, time, movement and speed, space, geometry, chance, experimental operations, classes, and relations. In addition, Piaget's research on perception was integrated in one book (1961), which was followed by works on mental images (Piaget & Inhelder 1966b) and memory (Piaget & Inhelder 1968). In these works Piaget demonstrated how the action knowledge of sensorimotor intelligence is slowly transformed into conceptual knowledge (without losing its action character). As an illustration, two-year-old children have a fairly stable framework of space (as shown in their movements and perceptions), but it takes almost ten years for them to reconstruct this framework on a conceptual level. He refers to the frameworks of stable conceptual knowledge as "operations." They imply a logically self-consistent system within which a person can mentally move from any point of the system to another and return. In psychological terms, thinking based on operations is "reversible" and therefore stable. For example, when a ball of clay is transformed into a thin stick, we *perceive* changes but we *know* that the quantity of clay remains the same. This illustrates Piaget's best-known experiment: conservation of quantity. A first stage of operations is achieved between ages seven and ten; they are called concrete operations because they focus on real, or imaginable, events. The more complete stage is "formal" operations through which a person can deal with logical reality as such (in the form of propositions) and consider the concretely real as one among many theo-

retical possibilities. Even though the operations are individual constructions, they are not individualistic. On the contrary, for Piaget logic expresses what all human minds have in common; it makes true cooperation between socialized adults possible. Note that the stages Piaget describes (reflex, sensorimotor, preoperations, and concrete and formal operations) apply to action coordinations and concepts rather than to the child. The notions are "in" stages, not the people. Within a person all of these stages intermingle and people will operate at their most advanced level only if internal dispositions and social conditions favor it.

Alongside these "psychological" investigations Piaget continued his more specialized preoccupations with epistemological problems of logic, history of science, and biology. In his *Biology and Knowledge* (1967a) he attempted a synthesis between lower and higher forms of knowledge and between evolutionary and developmental processes. *Logique et connaissance scientifique* (1967b) presents Piaget's perspective on epistemological problems underlying the main branches of contemporary scientific thinking and includes contributions from 18 specialists in these fields.

Piaget's later works can all be said to revolve around the concept of equilibration which has been central to this thinking from the very beginning of his career. That it took him almost fifty years before he published a book (1975) on it that he considered halfway satisfactory (in which he criticized the shortcomings of his earlier descriptions) is a witness both to Piaget's single-minded scientific persistency and to the centrality of this concept. What gravity is to Newton's system, equilibration is to Piaget's system. From one viewpoint, knowledge is a relation. On a biological level, it is a relation between organism and environment; on the human plane, a relation between person and society (the primary human environment); and on the conceptual plane, a relation between subject and object. From another viewpoint, knowledge is a structured totality in which the parts and the whole of a person's intellect are reciprocally interrelated. Both viewpoints are amply confirmed by Piaget's multitude of psychological observations on children's thinking. The relational approach is illustrated by the concepts of assimilation and accommodation as two complementary functions in all knowing. Assimilation is the process by which knowledge structures take in information; accommodation is the adjustment

of these structures to the particular information. The structural approach is apparent in his description of developmental stages. However, both viewpoints become distorted if one fails to realize that in knowing, the relations are primary and the two poles of the relations secondary and derivative. In other words, Piaget's is not a theory that postulates merely the interaction between a pre-existing subject and object. It is much more radical and asserts that what we call subject and object are the products of the relations. If we can talk of an object of knowledge (e.g., a historical fact, a mathematical proof) as if it had an absolute existence, this is an illusion created by our mental capacity for separating subject and object. In Piaget's perspective a historical fact or mathematical proof exists only in relation to a mind that can produce and understand it. Similarly this "mind" is the product of years of individual development and at the same time the common possession of socialized adults (relative to a given historical society).

Imagine subject and object as the two poles that knowledge constructs and relates. We can then see that the two poles have to be in balance toward each other. Equilibration is the process, intrinsic to a person's intellect, that coordinates these relations, and makes them productive in the construction of a (relatively) rational person and of a (relatively) coherent society and world of knowledge. As a living process the balance of knowledge is not static (as is a physical equilibrium) but dynamic and expanding. By its own functioning knowledge tends to construct ever new perspectives. These new constructs create new internal conflicts which in turn are compensated for by new constructs. In this manner development proceeds in an ever expanding dialectical spiral of subject and object, of empirical observation and theoretical construction. Equilibration explains how physiological conditions (maturation, heredity) and environmental experiences (society) contribute to development even though they are not a primary cause of that development. In fact equilibration does away with the need to postulate external causes of intellectual development, just as gravity did away with the need to postulate an external cause of planetary motion.

Piaget's perspective does not easily fit into the predominant paradigms of current academic psychology, such as stimulus-response models, behavioral control, information processing, measurement of individual differences, and social and verbal learning. Although Piaget's ideas are now

frequently quoted, at times to demonstrate their apparent inadequacy and at other times to support novel developmental approaches, attempts to fuse Piaget's and other paradigms are usually more confusing than clarifying. The major criticisms of Piaget are that his theories are not supported by precise measurements and that his concepts lack clarity. On the first point, it is noteworthy that Piaget's results have been among the most replicable and stable observations of human behavior. There are now literally hundreds of replication studies to be found in the psychological literature. Moreover, the practical relevance of Piaget's ideas is most apparent in the applied field of education. His revolutionary perspective on knowledge, and its development provides a powerful base to many modern approaches to the teaching of mathematics and science and to an overall atmosphere of intellectual health in school. Most misunderstandings are clearly related to a clash of paradigms, and for that reason alone it is imperative to stress the radical differences rather than the superficial similarities among the prevailing paradigms.

It would help to single out five specific differences: (1) *Intelligence*. Intelligence tests measure individual differences against a given norm, and theoretically determine to what extent certain conditions, such as a specific environment, contribute to a person's measured intelligence. Piaget's investigations focus on the characteristics of general intelligence that are common to all people and he describes both structural changes, as revealed in developmental stages, and common functions at all stages; he postulates equilibration as the main cause of intellectual development while heredity and environment are occasions but not explanatory causes of that development. (2) *Development*. Piaget rejects the traditional dichotomy of development into maturation (heredity) and learning (environment); he separates the physiological process of maturation on the one hand, and the learning and storing of information on the other hand, from the internal process of development. (3) *Memory*. For Piaget memory is not a process separate from the intellect; rather he distinguishes at least three forms of memory, depending on its relation to intellect: one, the stability of the framework of understanding (which is not really memory since this is our stable intelligence); two, sensorimotor recognition, which is limited to present action; and three, operatory recall which is an internal reconstruction. (4) *Knowledge*. For Piaget the intellect and its

resulting knowledge are coextensive with human life: all behavior partakes of knowledge; there is no radical split between action and knowledge, between perception and action, between the conscious or the unconscious, between personal and social actions. There is a knowledge component in all these forms of behavior. (5) *Language*. In Piaget's system social relating and communicating is essential in providing the environmental occasions for development. Moreover, serious (in contrast to playful or submissive) conceptual thinking by its own functioning takes on a social character of an inner dialogue. For Piaget this "inner" language is a result of overcoming an egocentric attitude and taking account of other reasonable viewpoints; the actual "language" in which it is carried out is quite secondary and can be wholly fragmentary or idiosyncratic. Hence, society's language, to the extent that it serves the communicative function, facilitates development. However, by itself, it is at best an occasion for learning information and is in no way supportive of structural understanding. In fact, up to and inclusive of the stage of concrete operations, children's most advanced knowing is beyond what they can handle verbally. Even in adult thinking society's language is frequently inadequate so that a formal language (e.g., algebra) has to be invented.

In conclusion, Piaget's work spans a period of more than sixty years, during which various trends in the thinking of the biological and social sciences as well as in philosophy came into prominence or went into decline. Because of the innovative character of Piaget's work, which did not move along traditional academic lines, his research continued steadily and expanded organically, but largely apart from other academic research. The response to, and recognition of, its scientific value was very much a function of these various trends: thus, after an initial enthusiastic acceptance, Piaget's ideas were generally neglected in American psychology until the revival of interest in developmental issues in the early 1960s. Yet, as was pointed out all along, to regard Piaget primarily as a psychologist is to misjudge his most basic work, which is both more and less than psychology, since it includes other disciplines related to his research, such as philosophy, biology, the history of ideas, sociology, education, and psychiatry. While Piaget either did not want, or was not able, to found a school in the sense of Freud or other innovators who sent their followers across the academic landscape, the ultimate test of the

soundness of his approach is probably not measured in terms of its acceptance by academia as an autonomous discipline. What counts is the continuous openness and relevance of Piaget's ideas to the more advanced trends and issues in these various disciplines which constitute precisely Piaget's original concept of an interdisciplinary and scientific *genetic epistemology*.

HANS FURTH

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POLANYI, MICHAEL

Michael Polanyi was born in Budapest, Hungary, on March 11, 1891. One of his older brothers was Karl Polanyi. His early interests centered on chemistry, but, because his family was Jewish and he feared that he might not be able to obtain a university post, he studied medicine at the University of Budapest and became a physician in 1913. The pull of chemistry remained strong, however, and he very shortly thereafter entered the Technische Hochschule at Karlsruhe in Germany to study chemistry. He then began a correspondence with Albert Einstein that continued throughout World War I.

During the war Polanyi served in the Austro-Hungarian army as a medical officer; but he continued his studies in physical chemistry and wrote several papers during this time. One paper, developing his own theory of adsorption,

became his doctoral dissertation, accepted by the University of Budapest in 1919.

He then returned to teach at Karlsruhe. There he met another chemist, Magda Kemeny, whom he married in 1921 in Berlin, where he had joined the Kaiser Wilhelm Institute für Faserstoffchemie. They had two sons: George, an economist, and John, a chemist. At the institute, Polanyi made many contributions in the X ray analysis of fibrous structure and crystals, especially metallic crystals.

In 1923, at the invitation of Fritz Haber, he became head of one of the departments in Haber's Institute für Physikalische Chemie und Electrochemie. There he did much significant work on crystals and in reaction kinetics. In 1926 he received a professorship at the University of Berlin and a life membership in the Kaiser Wilhelm Society (which had become the Max Planck Gesellschaft after World War I).

In 1933, threatened with Nazi interference in academic matters, Polanyi resigned from the institute and accepted the chair in physical chemistry at the University of Manchester. He continued his research in reaction kinetics until 1948 when he was transferred to a chair in social studies. He was elected a fellow of the Royal Society in 1944.

The transition from physical chemistry to social science and philosophy had been a gradual one. For 15 years he had published papers in both physical chemistry and in economics, as well as on social questions generally and in philosophy. His last chemistry paper was published in 1949, his first publication in economics appeared in 1935.

This transition was a natural one for him. His interests had always been broad and his thoughts reflexive. From the days of his youth in Hungary, he had been critical of the socialistic ideas of his peers and of the prevailing positivism of the time. At the Kaiser Wilhelm Institute, he and his collaborators in chemical research had regularly discussed political theory and economics as well as chemistry.

In England he found that Marxist concepts of science as a class-based ideology were rather common, as were complementary utilitarian notions that science should be organized to serve social purposes. Polanyi realized, from his own understanding of how science worked, that planned science was an absurdity. But he also saw that such ideas were dangerous to the very existence of science. It became clear to him, too, that those scientists who wished to resist these

ideas had no philosophic position from which to argue against planning. Accordingly, he became active in several societies working to defend or restore freedom in the pursuit of science.

But he came to see that two lines of intellectual attack also were essential: one economic and one philosophical. Since the strongest support for the idea of a science subservient to social needs was Marxism, he felt that he had to show first that Marxist economics erred, specifically when it assumed that a modern industrial economy could be managed without markets, pricing, and profits—i.e., through centrally planned production and distribution. He was therefore concerned to show how a modern industrial economy—with or without *private* capital—had to use a decentralized system of markets and profits, because it had to solve what was essentially a polycentric problem, the continual readjustment of resources to productive and consumptive needs and vice versa. There had to be many centers simultaneously making mutual readjustments. The *effective* span of deliberate corporate control, he showed, would always be too narrow to permit the myriad adjustments required in an entire economy in the time required for such adjustments.

He also had to show, he felt, that there were means for controlling business cycles and thus eliminating periodic large-scale unemployment—the major source of criticism of a free economy. He was the first economist to propose that Keynesian economic theories provided the means, through deficit spending, for maintaining full employment.

Accordingly, he attempted (1) to reverse the tendency among many economists to propose socialistic remedies for the West's economic problems, and (2) to educate the public generally on the way a free (unplanned) economic system worked—and could be made to work better. From the 1930s to the 1950s, he wrote a number of articles and books on these matters and produced a popular cartoon film showing how a free economy works.

Polanyi broadened this general position to demonstrate that a free society depended upon the working of many polycentric systems (including that of science) generating spontaneous orders of mutual adjustment, and that the continued existence of such a pluralistic, libertarian society depended upon general popular commitment to the values of truth, beauty, and justice, along with trust that the communities

of scientists, artists, and jurists were developing and defining these ideals through their own systems of free and mutual adjustment.

He realized that this tenet flew in the face of the generally accepted philosophic stance that true knowledge was utterly detached and objective, that it rested upon the absence of belief, and that social science was *qua* science, essentially value free, since no sets of values could be positively established as objectively true. Polanyi thus moved easily and necessarily to his second task: the development of a new philosophical epistemology that he called “personal knowledge.” Knowledge (even scientific knowledge), he held, proceeded from nonexplicable, tacit commitments and convictions (in which the knowers “dwelt”) to the synthesizing (by a personal act of the knower's mind) of focally grasped, integrated wholes made up of particulars of which we could only be *subsidiarily* aware. Such constructed wholes or meanings were, however, constructed with “universal intent” and so were not whimsical, arbitrary, or subjective, even though they could never be given wholly explicit or objective expression or defense.

Belief in such a view of man's knowledge capacities would, he held, restore to us belief in the ideals essential to mutual trust in a free society. Individuals would once again have a basis for respecting each other's ideas and efforts and thus for allowing the various communities of mutual adjustment to operate freely, rather than supposing that men could and should be controlled in a totalitarian manner by those with objective, “scientific” knowledge of the needs of society and the “causes” of men's ideas and actions, who could thus program or plan a whole society. The sciences of man, he held, could only be understood by fully committed participation in men's activities and thoughts, not through observational aloofness from this subject matter in the service of a false ideal of total objectivity.

The chair of social studies at Manchester required no teaching. This freedom, plus a grant from the Rockefeller Foundation, enabled Polanyi to give full expression to these social and philosophical views in his *magnus opus*, *Personal Knowledge* (1958). In this same year Polanyi was appointed a senior research fellow at Merton College, Oxford. From this base he continued to write and lecture at many of the world's universities until his last illness and death on February 22, 1976.

Polanyi's last few years were spent in showing how man could understand himself and achieve a rich meaning in his life through participatory knowledge in poetry, the arts, rites, and religion, as well as in science. He also attempted to demonstrate that recent achievements in biology, such as the discovery of DNA (deoxyribonucleic acid), indicated that living organisms are not reducible to physical and chemical events, but that they must be understood as hierarchically structured. Specifically, he found that living things have, at bottom, two levels of being: that of interaction between their atomic and molecular parts, and that of boundary principles structuring or harnessing these parts in the service of achievements impossible without them. Neither level is reducible to the other; thus, neither reductionism in biology nor behaviorism in psychology is possible, since hierarchical structuring continues into and through the higher levels of man's behavior.

Man, Polanyi maintained, finds his meaning in the service of these higher levels, which create obligations and lead to a richer existence than is possible on any lower level of being. A free society, which is essential to the aims of these higher levels, can be created and maintained only by mutual authority, mutual trust, and mutual dedication. Conversely, such authority, trust, and dedication will be permitted an existence only in such a free society.

HARRY PROSCH

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POPPER, KARL R.

Karl Raimund Popper, philosopher and methodologist of natural and social science, was born on July 28, 1902, in Vienna, the son of a lawyer of Jewish descent. Although his family was impoverished at the end of World War I, Popper managed to continue his studies at the University of Vienna, where he was exposed to the various forms of Marxism then current. His teachers included the psychologist Karl Bühler, who had a lasting influence on Popper's ideas about language and on Popper's decision to write his doctoral dissertation, completed in 1928, on the psychology of thinking. After receiving his doctorate, Popper taught mathematics and physics in a secondary school.

During the 1920s, Popper was in contact with several members of the Vienna Circle of logical positivists, notably Rudolf Carnap and Herbert Feigl. Although the circle sponsored Popper's early publications—an article in its periodical *Erkenntnis*, and in 1935, a book entitled *Logik der Forschung*—he was not a member of the circle and his main philosophical convictions explicitly rejected the principles of logical positivism. In 1937, with the approach of Hitler's

Anschluss, Popper accepted a post teaching philosophy at Canterbury University College in New Zealand.

Theory of scientific knowledge. Popper dissented in three main ways from the account of the relation between experience and scientific knowledge given by the Vienna positivists. First, while they contended that the statements of a language are meaningful or scientific only if verifiable in experience, Popper argued that the relevant property is falsifiability, and that this quality serves to demarcate science from pseudo-science. Empirically unfalsifiable statements, which are either tautologies or metaphysical statements, are not meaningless and should not be shunned absolutely, but they must be distinguished from empirically scientific statements of fact. Metaphysics, Popper admitted, often anticipates science, and is only to be deplored when it pretends to be science, as in astrology, alchemy, and in what Popper saw as their contemporary equivalents, Marxism and psychoanalysis.

Second, Popper disagreed with the positivists' belief that science moves in fact from observation to theory in an inductive manner. What is more, he contended that science cannot be *justified* inductively because theories cannot be supported by observation, which, by its nature, is selective and guided by antecedent theoretical presumptions. There is no such thing as pure, theory-free observation. As for justification, Popper endorsed Hume's argument that no general assertion is conclusively verified by an accumulation of particular instances of the regularity it asserts; in fact, he argues such an accumulation does not even confer any probability on the assertion.

According to Popper, scientific knowledge develops by a process of conjecture and refutation. First, a conjecture, as substantial as possible, is put forward. Observation is then called upon by way of a resolute search for states of affairs that would falsify the conjecture. If it survives these attempts at falsification it is so far corroborated. If it does not, a revised conjecture, compatible with what is known so far, is put forward and the attempt to falsify begins anew. By such refinement of successive conjectures through the elimination of errors, Popper contends, advance is made. At first, he took the advance to be simply the acceptance of the most substantial theories not yet refuted by experience. No claim about their truth was involved. But later, he came to interpret the advance as a

continuous approximation to truth, an increase in verisimilitude.

Popper argued that the measure of the extent to which a conjectured hypothesis is substantial is its falsifiability. Of any two hypotheses, the one that is more falsifiable—the one that rules out the greater number of logically possible states of affairs—has the greater substance or empirical content. Thus, a tautology, which does not rule out any possible states of affairs, is empirically vacuous. Popper ingeniously identifies degree of falsifiability, and therefore of empirical content, first with simplicity (often proposed as a criterion for choice between competing hypotheses consistent with what is known), and secondly, with improbability. The paradox involved in his rule “choose the least probable hypothesis” is only apparent. It does not mean that the least corroborated hypothesis should be accepted and acted on, but that the hypothesis which is antecedently least probable is the one to choose for testing.

The third respect in which Popper's views differed from those of the Vienna positivists concerns their respective notions of the empirical foundations of science. Most of the positivists, under the influence of Moritz Schlick, and more remotely, Ernst Mach, took that basis to be reports of immediate, subjective experience. Popper, on the other hand, argued that basic empirical statements report facts about the existence and properties of straightforwardly observable (“middle-sized”) material bodies in circumscribed regions of space and time. The decision to adopt those statements is motivated, but not authorized, by perceptual experience, and can be revised at any time. Basic statements are only the relative limits to the process of observational testing. They can themselves be tested by the further basic statements which can be derived from them with the aid of accepted theories. Carnap's conversion to this point of view was brought about partly by Popper's arguments.

In general, Popper's ideas about scientific knowledge add up to a major redirection of traditional empiricism, one which was not content to recast the principles of Locke, Hume, and Mill in a more up-to-date and logically sophisticated form. Although his ideas evoked interest, they did not seriously catch the attention of philosophers for a long time. Today, however, the conception of scientific theory as hypothetico-deductive, anticipated by William Whewell and Charles Sanders Peirce and directly opposed to

the orthodoxy of Bacon and Mill, is widely accepted. Popper's more extreme inferences from this doctrine, in particular of the conclusion that there is no such thing as inductive confirmation, remain controversial, but his work has been widely endorsed by scientists such as the biologists Peter B. Medawar and John Eccles, the cosmologist Hermann Bondi, and people working in other fields, such as the distinguished art historian E. H. Gombrich.

Antihistoricism. During the years Popper spent in New Zealand before and during World War II, he was preoccupied with the application of his doctrines to history and to social and political theory. His aim was the refutation of what he idiosyncratically describes as "historicism": the view that there is a discoverable law of the over-all development of history. In undertaking this research, Popper was motivated by his belief that totalitarian movements typically justify themselves with historicist theories. He waged his campaign in two main works, *The Poverty of Historicism* (1957) and *The Open Society and Its Enemies* (1945).

In *The Poverty of Historicism*, Popper maintains that historicism is supported both by those who believe that the social sciences use the same methods as the natural sciences and by those who believe that the social sciences have distinct and characteristic methods of their own. The former, the pronaturalists, endorse historicism because they misunderstand the true character of natural science and believe that induction is the essential scientific method. An important application of this view sees the evolutionary conception of the development of the animal species as a major achievement of inductive thinking. Popper contends that the evolutionary history of animal life is a trend, not a law, and that in general, the extrapolation of trends, however inductive it may be, is not scientific and is altogether different from the kind of prediction, which is always conditional, that is made on the basis of scientific laws. The anti-naturalists, on the other hand, though correct in recognizing that history is not an inductive science, wrongly imagine that it can arrive at predictions by intuitive or holistic methods of its own.

History, as Popper sees it, is a descriptive and explanatory, but not a generalizing, discipline. It does not propound laws, but tries to give a rationally coherent narrative in which events are explained and inferred with the assistance of laws supplied largely by the social sciences.

Many of the laws used in history are so trivial and obvious as hardly to be recognized as laws at all, but such truistic pieces of common knowledge are not a part of history; rather, they are drawn on by it. As an afterthought, Popper adds an a priori argument against the possibility of large-scale, unconditional predictions about the course of history in general. What happens in history is strongly influenced by the state of knowledge of historical agents at the time, but future knowledge cannot be predicted in relevant detail now. If it could be, it would be present knowledge. Therefore, the future course of history is unpredictable.

In *The Open Society*, a book that is remarkable for its scope, variety, and eloquence, Popper surveys three major bodies of historicist theory: those of Plato, Hegel, and Marx. The critique of Plato, a hero to so much of modern European culture, is an impressive scholarly tour de force. The pessimistically destructive character of Plato's conservatism is traced through all of its manifestations: from the theory of forms, seen by Popper as the metaphysical counterpart of political hostility to change, by way of Plato's theory of the necessarily degenerating sequence of political systems in time, to the part he played in the political life of his own epoch. The attack on Hegel is weakened by insufficiently controlled antipathy, but the critique of Marx, to whom in many ways Popper is sympathetic, is of memorable power. The examination of the exemplary historicists is accompanied by illuminating digressions into philosophy, and is illustrated, particularly in the discussion of Marx, by Popper's recollections of the experiences of the Austrian social democratic movement in the years between World War I and World War II.

Popper draws a number of directly political implications from his examination of historicism. The first is that in view of the very limited and conditional predictability of the historical future, planning should always be piecemeal, gradualistic, and ready to be revised in the face of unexpected developments. Another is the political counterpart of falsificationism. The fundamental problem of politics, Popper maintains, is not that of who should rule, but that of how can we design institutions so that bad rulers can do as little harm as possible and be replaced as easily as possible. Thirdly, Popper draws on studies of self-refutation as a problem in formal logic to argue that various sorts of politically absolutist doctrines are implicitly self-refuting. Against the absolute principle that every opinion

should be tolerated, Popper urges the qualified variant: tolerate what does not seek to destroy tolerance.

Later developments. At the end of World War II, Popper accepted a position as head of the department of philosophy, logic, and scientific method at the London School of Economics, and stayed there until his retirement in 1970. In this postwar period, he returned to the field of his earlier work. He has written, but not yet published, a large "postscript" to his *Logik der Forschung*. Two particular lines of development are significant. He has continued his critique of induction, which represented the mind as creatively active in the acquisition of knowledge, and developed that line of thought into first, an antideterminist metaphysics, and more specifically, in *The Self and Its Brain* (Popper & Eccles 1978), into an account of the mind on Cartesian lines as quite different in nature from the body, but as in causal interaction with it.

Secondly, as a result of the view of scientific progress as a matter of trial and error (the outcome of a kind of controlled struggle for existence between hypotheses), Popper has elaborated the conception of knowledge as an evolutionary product. He conceives of it as something produced by the minds of men, but once produced, as existing independently of them (1972).

ANTHONY QUINTON

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POTTER, DAVID M.

David M. Potter (1910–1971), one of the most influential American historians of his time, was born and grew up in Augusta, Georgia. He attended Emory University in Atlanta and was a member of its debate team along with C. Vann Woodward, another future historian of distinction. In Atlanta, new voices were by then challenging some of the old Southern myths, and Potter later made special mention of Glenn Rainey, the young debate coach "whose probing questions led a good many Southern youths to reflect for the first time that segregation was perhaps not a necessary part of the order of nature, like sunrise and sunset" (Potter 1973, p. 137). Potter entered Yale University as a graduate student in 1932, working first under the guidance of the noted Southern scholar, Ulrich B. Phillips, and then, after Phillips' death, under the intellectual historian Ralph H. Gabriel. His Ph.D. dissertation, completed in 1940 and published two years later with the title *Lincoln and His Party in the Secession Crisis*, won much critical acclaim. Potter was promptly appointed to the Yale history faculty and by 1950 had risen from an assistant professorship to an endowed chair.

His second major book, *People of Plenty: Economic Abundance and the American Character* (1954), in which he used the insights of social psychology and cultural anthropology to interpret the whole of American history, established Potter's reputation as a scholar of broad vision and analytic power. In 1961 he accepted appointment to a chair in history at Stanford University, where his principal publications were articles and essays exploring a variety of historical subjects. Most of these shorter pieces were eventually gathered together in two volumes, *The South and the Sectional Conflict* (1968) and *History and American Society* (1973). Meanwhile, he also continued work on his longest book, a general study of the coming of the Civil War. Begun in 1954 and still unfinished when he died, it appeared in 1976 as *The Impending Crisis, 1848–1861*, and was awarded a Pulitzer Prize.

Potter was also a prolific reviewer, a highly successful teacher, and a leader in university affairs. Enjoying the give and take of committees and conferences, he played an active part in the work of a number of professional organizations. At the time of his death he was president of the American Historical Association and

the Organization of American Historians. To many persons who knew him, Potter seemed the very model of a true scholar: urbane, reflective, critical, open-minded, eloquent, and wise. His professional reputation, unaccompanied by popular fame, probably resulted as much from the personal force of his character as from the quality and significance of his publications.

The predominant and often interconnected themes in Potter's writing were the sectional conflict over slavery, the American national character, the "enigma" of the South, and the nature of the historian's craft. As a Southerner, he was naturally drawn first to the study of the Civil War era, but in choosing to write his dissertation on Lincoln and the Republican party, he broke with the Southern perspective and assumed the role of a national historian. Later in his career, he turned more to Southern history, having come to regard himself as "no longer a Southerner" and thus able to view the South "with detachment, though not without fondness" (Potter 1968, p. vi). The experience of outgrowing his Southern heritage, while retaining a clear memory of what it meant to be a Southerner, may help account for his rare ability to combine sympathy with tough-mindedness and for his subtle perception of the complexity of the past.

The writing of *People of Plenty* was also, in a sense, an act of self-liberation—this time from the bounds of conventional history. Yet the book did not originate in a deliberate decision to become an interdisciplinary scholar. Potter's interest in the problem of national character was sparked during the 1940s when he was deeply involved in teaching the introductory course in American history at Yale, and when world events were inspiring much new reflection on the nature of American democracy. The people of the United States, he thought, were much too sure of the superiority of their national ideals and should be made aware that nations usually have "the kind of ideals which they can afford." Freedom and democracy were expensive ideals that Americans had been able to afford because of the favorable set of "conditions" in which they lived (Garraty 1970, vol. 2, pp. 315–316). Thus, in what amounted to a modification and expansion of Frederick Jackson Turner's frontier hypothesis, Potter developed his thesis that economic abundance, deriving not only from the physical riches of the continent but also from the cultural factor of technology, "has exercised

a pervasive influence in the shaping of the American character" (Potter 1954, p. 208). As he wrestled with the problem of defining national character, Potter found himself relying primarily upon the work of various behavioral scientists, including Karen Horney, Abram Kardiner, Ralph Linton, T. W. Adorno, Margaret Mead, and David Riesman. Gradually, this preliminary task assumed greater importance than the theme of abundance, and his essay on "The Behavioral Scientists and National Character" grew to be the longest and perhaps most significant in the volume.

With its materialistic assumptions and over-argued thesis, *People of Plenty* was not Potter's most representative book, but it revealed the exciting quality of his mind and earned him recognition as an authority on the disciplinary connections between history and the social sciences. Actually, he had little interest in mastering the methodology and literature of other disciplines but was satisfied merely to use their insights, concepts, and conclusions in his own enterprises. He remained a conventional historian in his research methods and disapproved of efforts to force history into the mold of social science. Historians, he said, were fundamentally different from social scientists in having to work largely with heterogeneous instead of homogeneous factors. The historian's generalizations were therefore not conclusions arrived at scientifically but rather more like the judgmental decisions of statesmen.

Appointed in 1956 to the Committee on Historical Analysis of the Social Science Research Council, Potter contributed an essay to the committee's report, *Generalization in the Writing of History* (1963). In it he argued that historians must recognize and systematically examine the implicit and often subconscious theoretical assumptions that shape their ostensibly factual statements about the past. Committee discussion extending over six years stimulated him to write also "The Historian's Use of Nationalism and Vice Versa" (Potter 1968, pp. 34–83). In this, his best-known essay, he distinguished between the "psychological" and the "institutional" forms of nationalism—that is, between the subjective, collective sense of being one people and the organized, independent exercise of power over the population of a defined area. Historians, he said, are disposed to define the phenomenon in psychological terms, treating it descriptively as one kind of group loyalty. But the concrete and

overwhelming presence of institutionalized nationalism "does violence to the historian's theory, for it pulls him in the direction of treating nationality as objective rather than subjective, absolute rather than relative, and total rather than partial" (*ibid.*, p. 41). As a consequence, nationalism is set off against other forms of group loyalty, and the attribution of nationality becomes evaluative instead of descriptive. "Indeed, where the concept of nationality is involved, the virtue or the evil of a man's act may not be determined by the character of the act itself, nor even by the motives for which it was executed, but entirely by the status of the group in whose behalf it is undertaken. . . . The attribution of nationality therefore involves a sanction—a sanction for the exercise of autonomy or self-determination" (*ibid.*, p. 45). In the second half of his essay, he then proceeded to demonstrate how evaluative elements in the concept of nationalism have affected and often "warped" historical writing about the South and the sectional conflict.

"The Historian's Use of Nationalism" thus embraced all of Potter's major thematic interests, and it was in many ways typical of his scholarship. The essay combined broad conceptualization with detailed illustrative analysis of a particular historical problem. It was professionally self-conscious. Its tone tended to be cautious in the reading of evidence and cautionary in addressing other historians. Furthermore, it reflected the author's fascination with the polarities, paradoxes, and dilemmas of history and historiography. To be faced with an imperative and difficult choice was, in Potter's view, a characteristic predicament of man and of the working historian. In his receptiveness to conflicting interpretations and his preference for uncategorical conclusions, some critics saw evidence of indecisiveness and even moral diffidence. But Potter regarded the integrity of the past as an ethical matter in itself. There was

"public virtue," he insisted, in extending the range of one's historical understanding beyond the limits of one's approval; and there was "moral value" in helping others realize "how circumstances enhance or limit human potentialities" (Potter 1968, p. 28; Garraty 1970, vol. 2, p. 330).

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RADZINOWICZ, LEON

Leon Radzinowicz was born into a well-to-do family in Lodz, Poland, on August 15, 1906. His father, a man of property, was also a distinguished doctor, the head of a hospital, who led a cultivated social life. Radzinowicz attended school in Lodz and from an early age exhibited an interest in history and languages, and a lack of skill in mathematics. Immediately upon leaving school he studied at the faculty of law at the Sorbonne, then at the height of its reputation with Paul Girard in Roman law, René Demogue in civil law, and Donnedieu de Vabres in criminal law. During his 12 months in Paris, Radzinowicz read widely in sociology, political theory, and history, and took a keen interest in political affairs. He then moved to the University of Geneva's highly regarded faculty of law and economic sciences, whose professor of criminal law, Paul Logoz, later became judge of the Supreme Court of Switzerland. There he developed an interest in criminology, which was taught by a rather obscure Yugoslavian *Docent*. Radzinowicz read widely in German and French—Gabriel Tarde, Alexandre Lacassagne, and Franz von Liszt. Even at that age he refused to specialize, insisting upon the interdependence of the social, individual, and legal conceptions of crime, and taking a catholic interest in all departments of the subject. He taught himself Italian and, after graduating in 1927 with an M.A., he decided to study in Rome under Enrico Ferri, leader of the most advanced and controversial criminological school in the world.

Radzinowicz studied in the faculty of law and

at the famous criminological institute, which embraced all approaches to the subject: biological, psychiatric, sociological, police, penological, and comparative law. In one year (and in Italian) at the age of 22, he received his doctorate, cum maxima laude. It is not surprising that he fell under Ferri's spell. Ferri combined a remarkable personality, presence, and eloquence with a burning belief in his conception of crime. His new positivist school was at the time regarded—uncritically—as a revelation. It married the sociological perspective on crime as a product of economic inequality (a view fundamental to Ferri's early and strong socialist beliefs) with individualistic views of the endogenous nature of persistent criminality. Radzinowicz embraced the positivist perspective and policy on the etiology, prevention, and control of crime, favoring its advocacy of indeterminate sentences, its concept of the state of danger, and other elements of the doctrine. His Italian studies were rounded off with a survey of Italian criminal law and criminology from Cesare Beccaria to Cesare Lombroso.

Ferri supported Radzinowicz' return to Geneva, where in 1929 he was made a *Docent*, lecturing for two years on positivist criminology. From there he went to the faculty of law at the University of Cracow to obtain a doctorate on problems of penal policy under Vladislav Wolters.

In 1930 Radzinowicz traveled to Belgium to study the new experiments in the penitentiaries—absolute solitary confinement; the introduction of a biological service to study the personality and to classify prisoners; Merxplas, a spe-

cial institution for young adults; and the new law of social defense for habitual criminals and psychopaths. His resulting report was widely acclaimed as a bold and perceptive account of these changes. Naturally, for someone so young and so committed to the positivist cause, enthusiasm prevailed over criticism. The work was presented by Count Carton de Wiart to the Belgium Academy and Radzinowicz was made Chevalier de l'Ordre Léopold. He followed this with a study of *crimes passionel*, in which he challenged the positivist doctrine that these crimes should not be punished.

Radzinowicz' travels, which took him throughout Europe, gave him firsthand insight into the relationship between penal ideas and policy and the wider political and social environment. He experienced the France of Léon Blum, the developing fascism in Italy, and the Weimar Republic in Germany. Returning to Poland in 1932, he was appointed assistant professor at the Free University of Warsaw. The senior professor of criminology, Adam Ettinger, had, before Willem Bongers, attempted to replace positivism with a Marxist interpretation of crime. Radzinowicz' approach now became less doctrinaire, and although still positivistic in its assumptions and approaches, concentrated on methodology rather than on ideological tenets.

Radzinowicz' work now became empirical, historical, and comparative. He published, in Polish, works on the principles of penitentiary science and on the state of crime. When the government stopped the publication of criminal statistics, he managed to obtain them and produced important articles on the "Variability of the Sex Ratio of Criminality" (1937) and on "The Influence of Economic Conditions on Crime" (1941a), the latter being regarded as a classic of its kind. His several critical articles on the crisis of the Polish penal system did not always endear him to the régime. He continued to travel, giving public lectures in universities, to the Société Générale des Prisons, to the Criminological Institute of Paris, and to other audiences.

Radzinowicz' connection with England began when, as a young student in Geneva, he had visited London for three months. When, in 1937, he returned to England to prepare a report on English penal policy for the Polish government, he encountered a climate of great optimism. The crime rate was low and serious crime rare. The reformatory theory of punishment was in the ascendancy and seemed to be vindicated

by the remarkable success of the enlightened borstal system, by the successful experiments with open prisons, by the progress made under the Children and Young Persons Act, and by the expansion of probation and aftercare. Hopes ran high that the bulk of recidivism could be cut off at the roots and that a transformed prison system could effectively treat and train those who persisted in crime. The criminal justice bill introduced in 1938 embodied the idealism of the times. Radzinowicz spelled out the guiding principles: "the avoidance, whenever possible, of deprivation of liberty, particularly for short terms. When, however, it is considered useless or dangerous to release an offender, even under certain conditions, the detention should be prolonged and of such a nature as to provide means for his re-education and to ensure the better protection of society" (1945b, p. 32). In a series of perceptive lectures and papers, later brought together as his contribution to *The Modern Approach to Criminal Law* (1940-1978, vol. 4), he described and analyzed the trends in sentencing and in penal thought and practice. He made plain his view on the scope, purpose, and value of what he called "criminal science." As in all his later works, he blended historical with contemporary analysis, the abstract principles with the problems to be faced by political reality, the quest for a scientific approach with a fervent adherence to humanity, and liberalism. In an essay, written with his great friend J. W. C. Turner, on the meaning and scope of criminal science, he expounded his belief in the necessary connection between criminology and criminal policy. For him, the main purpose of all criminal science was "to ascertain how best to fight against crime" (Radzinowicz & Turner 1945a, p. 21). At that time he held that criminological knowledge about the causes of crime was the only basis for a rational policy: "It is the function of criminal policy to adopt the classification of criminals formulated by criminology and to decide on the appropriate treatment for them" (*ibid.*, p. 23). This necessitated not only inquiry into the social conditions and personality types conducive to crime, but the study of the nature and efficacy of the methods of punishment and their effects on offenders. His essays on criminal statistics were eloquent pleas for a program of research. But throughout all these essays ran another theme—that crime, criminal law, and punishment were all inter-related, and that they could not be studied separately since they owed their origins to the same

social conditions. To him, the study of criminology, shorn of the study of law and policy, was both arid and dangerous, and particularly dangerous when it ignored the wider questions of social and political values. Characteristically, in a lecture in Paris in 1940, he vigorously attacked the criminal policy of Nazi Germany, not only for its disdain for legality, but also for being "strikingly anti-humanitarian" (*ibid.*, pp. 34-35). Despite his unabashed enthusiasm for what he saw as the English ability to combine liberal ideals with social welfare in a "humane and rational" system, he warned that the tendency toward indeterminate sentences would, if allowed to go too far, "become an instrument of social aggression and weaken the basic principle of individual liberty" (*ibid.*, p. 167).

He has always insisted that matters of penal policy, "the systematic study of all the measures to be taken in the spheres of prevention (direct and indirect), of legislation, of the enforcement of the criminal law, of punishments and other methods of treatment, constitute an indisputable and integral part of criminology" and that "to rob it of this practical function is to divorce criminology from reality and render it sterile" (1961a, p. 168).

This deeply held belief underlies Radzinowicz' distinctive contribution to the development of criminological studies in England. It is apparent everywhere in his work: in his monumental historical studies, in the impetus he gave to criminological teaching and research, in his efforts to bring practitioners into the academic setting, in his contributions to government as a powerful figure on committees of inquiry, in his international work at the United Nations and the Council of Europe.

When World War II broke out, Radzinowicz was still in England, and he remained to make it his home. He settled in Cambridge and by 1940 had edited, with J. W. C. Turner, volume one of *English Studies in Criminal Science* (1940-1978), published under the auspices of a committee set up by the law faculty, under the chairmanship of Percy Winfield, to consider the promotion of research and teaching in criminal science.

His many-faceted career from this point on may, for purposes of convenience, be divided into his scholarship, his promotion and direction of teaching and research, and his role as adviser and stimulant to national and international committees and organizations.

From the beginning Leon Radzinowicz was

fascinated by the peculiar English blend of principle and pragmatism in penal matters. He soon discovered the fascinating sources of the "Blue-books," the reports of commissions, committees of inquiries, inspectors, governmental departments, accounts and papers, returns, statistics, and the rest, all of which had lain almost untouched by historians of criminal law. He embarked on a colossal task—one on which he is still engaged—to trace the developments in English criminal and penal policy since the middle of the eighteenth century. It was an extraordinary enterprise for one brought up with neither a close knowledge of English history and institutions, nor with English as a natural tongue, although with his gift for languages and his feel for language and idiom this was soon more than mastered. Volume one of *A History of English Criminal Law and Its Administration from 1750* (1948-1968), *The Movement for Reform*, was hailed as a work of outstanding scholarship and originality. It ensured him a fellowship at Trinity College, Cambridge, and won him the James Barr Ames prize of the Harvard Law School. Its main theme was the place of capital punishment in the enforcement of criminal law. In tracing the complex movement for the amelioration of the severity of punishment, it described the application of the death penalty to a vast range of offenses in the eighteenth century; the place of the death penalty in the law of larceny; its use under the notorious Waltham Black Act; the attitudes of courts and juries; and the exercise of the Royal prerogative of mercy. It vividly portrayed the statutes in operation and the nature, forms, and customs of execution. The work is also a history of ideas and currents of opinion. It analyzes the conflict between the doctrine of suspended terror or maximum severity and the doctrines of the liberal enlightenment as expounded by Montesquieu and Beccaria and taken up in various ways by the English reformers, William Eden and Samuel Romilly, and in his own inimitable way, by Jeremy Bentham. It is also a history of the social and political movement for reform beginning with the efforts of Henry Fielding and culminating in the arduous campaign of Romilly, the Committee of 1819, and Robert Peel's reforms. It is this combination of precise history of legislative change, history of ideas, history of social movements, history of public and political opinion, all informed by a critical analysis of the complex relationship between penal theory, penal practice, and social and political change, which

also marks the following three volumes. These volumes deal with the emergence of public police out of a system of private initiative founded on the doctrine of laissez-faire and fear of state control. In the fourth volume, both the campaigns for reform of capital laws and the establishment of regular and efficient police are followed to their culminations in the 1860s.

Radzinowicz brought this historical and comparative perspective to bear in all his other writings. Outstanding among them is his essay *Ideology and Crime* (1966), which surveys the movement of criminological ideas from the classical liberal perspective through the "deterministic" position of the positivist school to what he called a "pragmatic position" inherent in much contemporary sociological thought and penal practice. His latest book, *The Growth of Crime* (Radzinowicz & King 1977), surveys, on an international scale, the main currents of crime, criminal law, criminological thought, and dilemmas in the administration of criminal justice and the penal system. None of the optimism of Radzinowicz' views in the 1940s remains; the work is a somber assessment of a penal pessimism that in the intervening years, perhaps paradoxically, had been revealed and generated partly by criminological thought and research.

As early as 1961 Radzinowicz had sounded a more cautious note to those who looked to criminological research as a means of discovering effective and efficient penal tools. His survey of European and American criminology, published as *In Search of Criminology* (1961a), persuaded him that no false hopes should be engendered. He was by then skeptical that any useful progress could be made in the search for "causes" of crime, preferring instead "descriptive analytic accounts of the state of crime, of various classes of offenders, of the enforcement of the criminal law, of the effectiveness of various measures of treatment, of the working of the penal system—in a word, of all the matters which come within the orbit of penal policy and penology. Such applied research, if well conducted, will not only increase the social utility of criminology, but bring a refinement of method and a more exact perception of the things that matter" (1961a, pp. 177–178). He warned that criminology could not solve the problem of crime, since crime was an integral part of society, and cautioned that "the connection between criminological research and penal reform should not be too dogmatically insisted upon." Most important, he warned criminologists of the dangers of placing reliance on

narrow expertise: "There are deep-rooted and far-reaching issues of public morality, of social expediency, of the subtle but vital balance between the rights of the individual and the protection of the community, which underlie decisions of penal policy, and must often override the conclusions of the experts." The role for criminology was to give "a new vitality, a better purpose and an increased effectiveness to the law and its specific function, which is that of dispensing criminal justice" (1961a, pp. 178–179).

As Lord Butler pointed out in his contribution to *Crime, Criminology and Public Policy: Essays in Honour of Sir Leon Radzinowicz* (Hood 1974), Radzinowicz was the obvious choice to be the first director of the Institute of Criminology established in Cambridge in 1959 on the initiative of the government. The other two leading figures in British criminology, Hermann Mannheim at the London School of Economics and Max Grünhut at Oxford University, were at the end of their academic careers. The University of London was barely lukewarm about establishing such an institute, while Cambridge, where Radzinowicz was already director of a small department of criminal science, was enthusiastic and determined in its negotiations. With the establishment of the institute, the Isaac Wolfson Foundation endowed the first chair of criminology in England, and Radzinowicz was appointed the first Wolfson professor. His energy, determination, persuasiveness, foresight, and insistence on the highest standards ensured the international reputation of the institute and the establishment of criminology as an academic discipline not only in Cambridge but in many universities in England and overseas.

The institute became a center for teaching and research. In its early years the program of research followed the lines of inquiry that Radzinowicz favored, and was concerned mainly with charting the nature of crime and assessing the effects of penal sanctions. Under his guidance the department of criminal science had already published a survey of sexual crime carried out by F. H. McClintock and F. J. Odgers. At the institute, McClintock conducted further investigations into crimes of violence and robbery and surveyed the state of crime in the country as a whole. Other studies in the vigorous research program carried out by the assistant directors of research included J. P. Martin's inquiries into the social consequences of conviction and the cost of crime; R. F. Spark's sur-

vey and analysis of local prisons and of victims of crime; A. E. Bottoms and F. H. McClintock's evaluation of the régime of a borstal institution; R. G. Hood's studies of borstal and the sentencing of motoring offenders; D. J. West's psychiatrically oriented inquiries into murder and suicide, and his comprehensive "cohort" study of the relationship between delinquent behavior and the personal and social backgrounds of young Londoners. Some criticized the institute for its close connections with the Home Office; its concentration on empirical research; its overreliance in many studies on official records as a source of data; and, more generally, its scant attention to the new sociology of deviance that appeared in the late 1960s—the so-called labeling, interactionist, and radical perspectives. Indeed, a rival "deviancy symposium" was set up to redress what were considered to be the conservative tendencies in the Cambridge "criminological establishment." But in reality, the institute remained independent of the Home Office, its permanent university posts protecting it from interference from any grant-giving agency. Yet its program did remain faithful to Radzinowicz' view that criminology, especially empirical research, should never divorce itself from the concerns of public policy.

On his world-wide travels Radzinowicz had been struck above all by the lack of formal teaching in criminology at both undergraduate and graduate levels, particularly in law schools, and tried to remedy this oversight. The subject was introduced into the Cambridge Law Tripos and soon became one of the most popular options. Perhaps more important, he established a post-graduate course leading to the Diploma in Criminology in order "to give a chance to young people in this country to start to specialise in criminology." In fact, it also attracted many good students from overseas, including those brought from Columbia Law School under a highly successful exchange scheme. Over the years it has provided a substantial proportion of scholars teaching or carrying out research in British universities as well as in several overseas. But teaching and the exchange of information were not confined to students planning academic careers. A senior course in criminology was established as a biennial seminar for senior practitioners in various branches of the criminal justice and penal systems; a biennial national conference acted as a focus for discussions of both teaching and research; fellowships were endowed to enable practitioners to record and interpret their

experiences. Priority was given to the establishment of a first class library—now known as the Radzinowicz Library of the Institute.

And Leon Radzinowicz, himself, branched out to encourage an interest in criminology in the law schools of the United States. In 1962 he had been Walter Meyer research professor at Yale University and a few years later, when many others would have been contemplating a quiet retirement, he vigorously launched a new career at Columbia Law School, where he was adjunct professor from 1968 to 1976. In addition, he lectured at the law schools of the University of Virginia and Rutgers University, and at the Center for Criminology of the University of Pennsylvania, where his close collaboration with Marvin E. Wolfgang produced the three volume collection *Crime and Justice* (1971). In recent years he has been distinguished visiting professor at the University of Minnesota, the Benjamin Cardozo Law School, Yeshiva University, and the John Jay College of Criminal Justice of the City University of New York. His gift of exposition and his unusual personal magnetism ensured the establishment of criminology where many feared it could never flourish.

Leon Radzinowicz' conception of the scope and potential value of criminology naturally led him to take an active part in public life both in England and overseas. He was a member of the Royal Commission on Capital Punishment; of the Advisory Council on the Treatment of Offenders; of the Royal Commission on the Penal System, where his resignation was a decisive factor in causing this ineffective inquiry to be dissolved; of the Advisory Council on the Penal System, being particularly influential as chairman of the subcommittee that rejected Lord Mountbatten's plans for a British "Alcatraz" in favor of a more humane environment for long-term dangerous prisoners. In 1945 he was in charge of the United Nations section for social defense which produced important reports on probation and on international collaboration in criminal science. He was an important influence in making such collaboration a reality during his long tenure as the first chairman of the scientific council of the division of crime problems established by the Council of Europe. In 1970, as the first general rapporteur of a United Nations congress on crime from outside governmental circles, he fearlessly delivered a scathing indictment of the appalling standards of criminal justice and penal administration still endemic among the member nations. He inspired collaboration between

Scandinavian and English criminologists. In New York he was called upon by the city Bar Association and the Ford Foundation to present a blueprint for an independent institute of criminology, later published as *The Need for Criminology* (1965*b*). He traveled not merely to attend conferences, but as an adviser to governments and institutes, in an endeavor to promote criminological studies. He aided the establishment of an independent institute of criminology at the University of Cape Town; he was adviser to President Lyndon B. Johnson's Commission on Violence; he visited South America, the Far East, Yugoslavia, Australia, and his native Poland.

When Leon Radzinowicz retired from the Wolfson chair in 1973, a collection of essays written by colleagues and students under the title *Crime, Criminology and Public Policy* (Hood 1974) was published in his honor. He was knighted in 1970 and in 1973 was elected a fellow of the British Academy and honorary foreign member of the American Academy of Arts and Sciences in acknowledgment of a career that has embraced the best of scholarship, academic enterprise, and public service.

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RHINE, J. B.

J(oseph) B(anks) Rhine, born in 1895, is the man who transformed psychical research into modern parapsychology, and made ESP (extra-sensory perception) a household term. His career spans more than fifty years of research, writing, and administration, directed toward establishing parapsychology as an experimental science.

Rhine and his wife, Louisa E. Rhine, whose work paralleled and furthered his own, were botanists who received their doctorates in biology from the University of Chicago. Rhine taught plant physiology both before and after he received his PH.D. in 1925, but his thinking turned increasingly toward broader issues of the nature of life, especially the questions of whether mental activity transcended physical laws and whether the mind could survive death. The heritage of the Darwinian revolution made him hope that scientific inquiry could resolve these questions, but investigating them would require him to leave biology for psychical research. Although the Rhines were idealistically willing to sacrifice security for work they thought important, they first consulted three eminent men involved with psychical research: one vitriolic critic of it, Joseph Jastrow, and two proponents, Gardner Murphy and William McDougall.

McDougall's advice determined Rhine's career, though it was given only in a hurried interview as McDougall began a one-year leave from chairing the department of psychology at Harvard University. Rhine, in 1926, studied psychology and philosophy at Harvard and worked with Walter Franklin Prince, former research officer of the American Society for Psychical Research, primarily in helping to uncover medium-

istic fraud. In 1927 McDougall (who now was chairman of the psychology department at Duke University) accepted Rhine as an honorary graduate fellow there. In 1928 Rhine became an instructor in psychology and philosophy at Duke, but his work centered increasingly in parapsychology. After a few years he withdrew from official ties with philosophy, and by 1950 gave up his professorship in psychology to assume the full-time directorship of the Parapsychology Laboratory. He retired as professor emeritus in 1965 and transferred the laboratory's functions to the Foundation for Research on the Nature of Man, whose chief publication for research was the *Journal of Parapsychology*, founded in 1937 with McDougall and Rhine as editors.

Rhine's first major contribution was to standardize an experimental method of investigating "telepathy." Anecdotal evidence for telepathy was challengingly persuasive but methodologically weak, since even well-authenticated cases might be only coincidental. Rhine demanded an objective baseline to assess coincidence, as in earlier experiments in which subjects guessed playing cards or colors. But earlier methods had two difficulties. One was that subjects grew discouraged when odds against chance were long, as with 1:52 for cards, while with short odds, subjects grew impatient at the many guesses needed for evaluation. Rhine decided on odds of 1:5 as an appropriate compromise. The second difficulty was that subjects' preferences for certain targets or sequences might distort their guesses. Rhine developed targets that elicited minimum response bias. Modifying the suggestion of a colleague, Karl Zener, he introduced what became known as ESP cards: circle, square, star, cross, and waves.

His initial procedure was to shuffle a deck of 25 of these symbols, look at the first, ask a subject to guess it, and continue playfully but challengingly. Several subjects had extraordinarily high average scores over thousands of guesses, and also showed meaningful declines with boredom or lessened motivation, but upswings with a new challenge. Rhine and his students soon introduced variations, both to tighten the controls and to study the limits of success. They found no limits then or later. Scores stayed high even with cards 250 miles from the subject or when no one looked at the cards. This latter was not telepathy, but clairvoyant response to the concealed symbols, and they realized that clairvoyance could ac-

count for the apparent telepathy data. Rhine coined the term ESP as a noncommittal designation for his findings.

After the publication of his first book, *Extrasensory Perception* (1934), he was immediately embroiled in controversy. Initially, as in 1938, when a meeting of the American Psychological Association discussed his work, arguments centered on possible methodological flaws. Rhine took them as constructive criticisms and changed his methods to eliminate the flaws. With double blind techniques, a pre-stated number of items per series, careful target randomization, etc., research from his laboratory became so rigorous that intelligent criticism ceased. Even then, however, some who would not accept his conclusions continued to attack him. They suggested that data contradicting conventional theories were necessarily wrong; that statistical analyses were useless; that replications were insufficient. Later, when independent replications grew impressive, they disregarded the body of careful laboratory research and denigrated Rhine's findings because stage or amateur magicians used fraud.

Others responded favorably to the careful technical reports or to Rhine's stream of persuasive popular writings. His frequent lectures conveyed his care in controlling conditions and his caution in interpreting preliminary findings. These, plus the revolutionary impact of his replicated findings, encouraged young scholars to work with him, and other laboratories began to do similar work.

In 1943 the Rhines described a second major methodological innovation: an objective way of assessing PK (psychokinesis), the influence of mental activity on physical processes. The method, as it later became refined, was to release dice mechanically while subjects hoped for a particular face. To control for dice bias, subjects hoped equal numbers of times (ordinarily 24) for each face. Results showed overall success beyond chance expectation and also an even stronger internal pattern: successes clustered significantly at the beginning of each new page of 24 calls for a single face, then dwindled progressively. This decline in score with boring repetition, and upswing with novelty, was like the declines repeatedly observed in ESP tests and of course in more conventional tasks. It supported the thesis that PK represented mental activity.

After the Rhines introduced their procedure, others invented variants—e.g., the placement

technique whereby subjects hope that objects released at a medial line will fall left or right, and the method most in use in the 1970s, whereby subjects hope that radioactive emissions will slow down or speed up, and a machine registers output. PK effects were repeatedly confirmed.

Further research from the Duke laboratory examined precognition (extrachance effects when targets are randomly selected in the future). Results were as high as for clairvoyance. Other experiments attempted to study "pure telepathy": an experimenter mentally translated randomized digits into arbitrarily chosen ESP symbols but never wrote or spoke those symbols. Although subjects succeeded in guessing the symbols, Rhine considered the method unsatisfactory because it could not eliminate clairvoyance of the experimenter's brain processes.

Gradually research emphasis shifted, in Rhine's laboratory and others, from demonstrating extrachance effects to examining the conditions that influence success. Many were found: attitude toward the task, mood, target preference, drug intake, etc.—and especially the subject-experimenter relationship, which so often is a modulating variable. Independent replications with varied techniques have supported the earlier findings and begun to flesh out the information.

Rhine's mature conclusions (1977a; 1977b) are, briefly, that ESP and PK are mental abilities, distinctively different from all known physical processes because they are uninfluenced by physical screening, spatial distance, or time. They cannot be consciously controlled. Their effectiveness, like that of other unconscious processes, depends largely on motivation, and especially on the subject-experimenter relationship. Rhine has relinquished his hopes for studying telepathy because no technique distinguishes it from clairvoyance. Similarly, he thinks research on survival and on mind-body separation should be shelved, because evidence for either can be reduced to ESP or PK. Since ESP and PK vary with psychological processes, psychologists are welcome to study them; since they vary with physiological factors like drug intake, biologists are welcome; since, eventually, general laws to include these phenomena may be discovered, physicists are welcome. But because ESP and PK are distinctive they constitute a separate science, parapsychology.

To further parapsychology, Rhine in 1957 organized the Parapsychological Association, an

international professional society that in 1969 became affiliated with the American Association for the Advancement of Science. Its annual conventions include dozens of research reports, yet after twenty years its full members number barely more than one hundred. Funding is meager; estimates of full-time parapsychology positions range around twenty; and few who enter the field are able to stay in it. Although more than seventy colleges offer parapsychology courses, most are taught by nonspecialists.

Rhine's work nevertheless has had substantial impact. Psychic research has become an experimental science and variants of his methods its norms. His detailed conclusions are controversial, but his basic contentions—that ESP and PK do occur—are well supported. Popular acceptance of ESP is widespread, though, unfortunately, occult claims are sometimes confounded with laboratory findings. Within the scientific community, the dualistic implications of Rhine's thesis arouse emotional resistance, and outdated criticisms from the 1930s are still cited to justify disregard of them. But the few scientists who examine contemporary research tend to treat parapsychology with increasing respect, as a topic that deserves further investigation, or as a field that is already well established.

GERTRUDE R. SCHMEIDLER

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RICHARDS, AUDREY I.

Audrey I. Richards, an English social anthropologist who was born in 1899 and spent her early childhood in India, brought a vivid sense of the ethnographic present to the understanding of hierarchic, authoritarian societies in south and east Africa. Her career was not narrowly academic. Indeed, it might be said that it was her wider, personal experience of the higher echelons of public administration, both government and academic, which gave such a ring of authenticity to her descriptions of the political culture of palace politics, elitist councils, and colonialism among the Bemba of Zambia (1960; 1969; 1971) and the Ganda of Uganda (1964; 1969). Trained initially in biology, she maintained a lifetime interest in interdisciplinary work. She deliberately addressed her own work to administrators and the general public as well as to sociologists, and she led teams of fieldworkers in interdisciplinary and applied research, first as director of the Institute of Social Research in East Africa from 1950 to 1956, and later as founder of the African Studies Centre in Cambridge, where she was Smuts reader from 1962 until her retirement in 1967. Much of her early research was done, she later explained, in "The Colonial Office and the Organization of Social Research" (1977), at a time when "the Colonial Office and the colonial governments made little use of anthropologists, viewed them with some suspicion and starved them of finance" (p. 187). During the early part of World War II, she worked to change this attitude and to get finance for field research by social scientists, when she became a principal in the British Colonial Office and secretary of the Colonial Social Science Research Council. Even after her retirement, she never lost her zest for field work, and continued to study an English village.

Richards was one of Bronislaw Malinowski's first students at the London School of Economics, along with E. E. Evans-Pritchard, Raymond Firth, Isaac Schapera, and Hortense Powdermaker. With them and some of their immediate successors, Meyer Fortes, S. F. Nadel, Godfrey Wilson, and later Phyllis Kaberry, she carried forward the debates that transformed functionalism as a method and approach in anthropology. After Malinowski's death, she recalled that he had tended to regard his students, at least until they broke openly with him—which Richards never did—"rather as a team engaged on a joint

battle than as a number of individuals with different interests and needs. . . . Students worked at any problem in which he was at the moment intensely absorbed" (Richards 1943, p. 3). His influence was naturally most immediate and evident in her first work, but it remained fundamental throughout, and she was always a Malinowskian anthropologist. Thus, his *Sex and Repression in Savage Society* (1927; see also Malinowski 1929) led to her *Hunger and Work in a Savage Tribe* (1932, based on her Ph.D. thesis [1931]). The shift in focus was from sex to food, from reproduction to production, but their functional methods were parallel: teacher and student alike stressed "primary social wants" and the satisfaction of biologically based "cultural needs." Again, following his *Coral Gardens and Their Magic* (1935), she wrote, about the Bemba, *Land, Labour and Diet in Northern Rhodesia* (1939). In this monumental work, which she continued in the brief monographs on marriage, fertility, and female initiation ritual, *Bemba Marriage and Modern Economic Conditions* (1940) and *Chisungu* (1956), she adhered to the view of tribal life as an integrated whole: major change and the threat to integration came from outside. Her functional system, if more changeable than Malinowski's initial one, was still a total system and had to be described comprehensively. This meant including agricultural activities along with as many correlated aspects of culture as possible. The result was the first major anthropological study in Africa of reciprocal food sharing and underproduction in a redistributive economy undergoing rapid change as a result largely of sharply increased labor migration from the countryside to towns.

For a period after the early 1940s her approach became somewhat unfashionable in British social anthropology. There was a reaction against description that left institutions, as a leading critic put it, "overloaded with reality" (Gluckman [1947] 1963, p. 226). The call was for greater abstraction from the total cultural context, for the dissection of institutions into smaller units more susceptible to comparative analysis. In "A Problem of Anthropological Approach" (1941), Richards anticipated the reaction and raised objections to it that were to be rediscovered, as if for the first time, in a counterreaction decades later. The initial breakthrough was made by Evans-Pritchard's *The Nuer* (1940), and, fittingly enough, Richards put her objections in a deeply considered review article about it. Doubting the validity of a "rigid re-

striction of generalisation to the problem of social structure alone," she insisted on the relevance of various kinds of cultural data to the very problems Evans-Pritchard posed (1941, p. 49). She rejected the sharp distinction between domestic and political systems which, until the counterreaction, became an axiom in much social anthropology: "The two systems seem to me to grow one out of the other, and in the dynamics of a social situation constantly to overlap" (*ibid.*, p. 51). She criticized the utility of Evans-Pritchard's basic model, with its "segment simile" that conveyed "a more equal and logical division into sub-groups than actually occurs" (*ibid.*, p. 47). She based her objections on her experience of societies with great inequalities in status and variability in organization, according to a "number of different principles of grouping such as age, local attachment, rank etc., each with its own ideology culturally defined. These principles may reinforce each other, exist side by side, or even come in conflict with each other" (*ibid.*, p. 51). Her contention was that the segmentary model was static and could not cope with history and with change in the relationship between these principles over time.

Richards became a regional specialist par excellence. Among anthropologists the most significant others she considered, repeatedly, besides her fellow students of Malinowski, were the Africanists, especially Monica Wilson, Max Gluckman, and Hilda Kuper. An apparent exception was Gregory Bateson, of whose decoding of the symbolic functions of transvestism in New Guinea (1936) she wrote "rather sardonically" (Firth 1973, p. 183). Hers was, in effect, a backwards translation into Bateson's views from those of an Africanist. She equated the quite different, if historically linked, views of Bateson and Gluckman with an admittedly "crude" version of unconscious compensation as an explanation of role reversal in ritual (1956, pp. 119, 154). As a consequence, Richards' own explication in *Chisungu* of the sequential patterns in the ritualization of sex roles suffered a lack of fully systematic decoding.

Her method demanded the highest standard of field work. This she sustained among the Bemba, though less so among the Ganda. In her Bemba studies, above all, she counterbalanced, quite rigorously, informants' accounts of what was and ought to be, according to traditional dogma and values, with close and long-term observation of what recognizable individuals actually did. Among the Ganda, she became too

involved in large-scale statistical surveys that were no match for the quality of her own first-hand observation, and the administrative duties of her institute left her little time for anything else. Hence, the books she edited or wrote about Uganda, *Economic Development and Tribal Change* (1954), *The Changing Structure of a Ganda Village* (1966), *Subsistence to Commercial Farming in Present-day Buganda* (Richards et al. 1973), had less of her earlier rigor, though they did give much detailed and insightful information on changes in labor migration and land tenure with the introduction of individual freehold.

The wider breadth of her influence is reflected in the two books of essays in her honor. The first (Schneider & Gough 1961) took up the comparison of matrilineal kinship structures that she had pioneered in her taxonomy of the central Bantu (1950)—an approach which had evoked Edmund Leach's backhanded compliment and sharp condemnation that it was Radcliffe-Brownian and tautological (1961, p. 4). The second (La Fontaine 1972) was a tribute to her argument, most explicit in *Chisungu* and much indebted to Malinowski's "Baloma: The Spirits of the Dead in the Trobriand Islands" (1916), that symbolic behavior, by its very nature, is multiple in significance (Malinowski's "orthodox," "popular," and "individual" interpretations [1916, p. 429]), and must be the subject of multiple explanations, with a view to variation in sentiments, emotions, and values.

RICHARD P. WERBNER

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Lionel Charles Robbins, born in 1898 and created a baron in 1959, entered the London School of Economics (L.S.E.) in 1920 after service in World War I. Apart from short interludes in Oxford (1924-1925 and 1927-1929) and the years of World War II, he has been associated with that institution as, successively, undergraduate (1920-1923), research assistant (1923/1924), lecturer (1925-1927), professor of economics (1929-1961), honorary lecturer (1961-), and chairman of the Board of Governors (1968-1974).

When Robbins first studied economics after World War I, the subject, in Britain at least, was in a slump. Although important work was going on, the 1920s may be seen as a lull before the storm of the great depression and the ensuing upheavals. At the L.S.E., Robbins' teacher was Edwin Cannan, who represented a healthy common-sense approach, deepened by a critical, scholarly study of the history of economic thought, particularly the English classical economists. But in Britain generally the subject was dominated by the somewhat blinkered orthodoxy of the school of Alfred Marshall, and though Cannan was critical of Marshall he hardly managed or sought to establish alternative theoretical ideas.

From an early stage Robbins sought to break

out from these narrow intellectual horizons. Beginning with Cassel's *Theory of Social Economy* (1918) he went on to explore the leading recent and contemporary American and European writers. Important influences were those of Philip H. Wicksteed, a kind of London predecessor, and of the Austrian school, especially Ludwig von Mises. Robbins' early articles are critical of Marshall and introduced such Austrian ideas as opportunity cost—as against Marshallian “real” cost. Much of Robbins' intellectual development at this time is reflected in his introductions to new editions of Wicksteed's *The Common Sense of Political Economy* ([1910] 1950), and of Knut Wicksell's *Lectures on Political Economy* ([1901–1906] 1935–1951). Although his choice of influences may be criticized, Robbins helped powerfully in dispelling a certain parochialism that had descended on the subject in England, opening up, in the 1930s, alternative intellectual vistas. If, as has been suggested, a “neoclassical synthesis” emerged in orthodox economics in the second or third quarter of the twentieth century, then, so far as England is concerned, the early work of Robbins played a significant part in the process.

The intellectual influences outlined above were apparent in Robbins' first major work *An Essay on the Nature and Significance of Economic Science* (1932). The essay opens with a discussion of the definition of the subject matter of “Economic Science.” Robbins argues against definitions like those maintained by Cannan and Marshall, in terms of material wealth and “the ordinary business of life,” proposing, rather, *scarcity* as the central, organizing concept of economics in his much-quoted definition: “Economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses” ([1932] 1935, p. 16). A second theme of the *Essay*, which follows Max Weber's defense of *Wertfreiheit*, is a sharp distinction between ends and means, and between ethical or normative propositions regarding what ought to be and positive propositions about what is or what follows what. Robbins criticized especially the ambiguous concept of economic welfare as involving invalid interpersonal comparisons of utility. He was later misrepresented as having attempted to exclude or limit the discussion and advocacy of policies by economists. A third feature of Robbins' *Essay* was his analysis of the nature of economic generalizations (chapters 4;5). Here the influences of von Mises and

other Austrians, together with that of later English classical economists, such as J. E. Cairnes, are very apparent. Robbins seemed to assert that significant, nonempty economic theories could be based on postulates that were beyond dispute and testing: “These are not postulates the existence of whose counterpart in reality admits of extensive dispute once their nature is fully realized” (p. 79). Moreover, such economic postulates provided a sounder base than that available for the natural sciences: with regard to economic generalizations, “there is less reason to doubt their real bearing than that of the generalisations of the natural sciences” (p. 105). Robbins' *Essay on the Nature and Significance of Economic Science* has remained much his best-known and most-quoted book, in spite, or because of, its controversial features. Notwithstanding his insistence on distinguishing between the normative and positive elements in economic advice, there was much in Robbins' *Essay* that economists would have found appealing—for example, his claims that “Economic Science” could contain “a body of generalisations whose substantial accuracy and importance are open to question only by the ignorant or the perverse” (p. 1), while its methods “may be regarded as settled as between reasonable people” (p. 72).

After the second edition of his *Essay* (1935), Robbins did not return at length to the controversial questions it had raised, and after World War II his writings were concerned largely with economic policy and the history of economic thought. On economic policy one of his earliest and most controversial contributions came in 1930, with the onset of the great depression, when he was invited by John Maynard Keynes to join a five-man committee of the Economic Advisory Council set up to propose remedies for unemployment, which was approaching twenty per cent in Britain. Robbins, influenced by the Austrian theory of the business cycle, found himself in a minority of one in opposing the expansionist policies advocated by Keynes, Arthur C. Pigou, and the others. However, he subsequently came to regard his views at this time as mistaken, and in the later 1930s and during World War II, moved in favor of moderate Keynesian policies. As director of the Economic Section of the Offices of the War Cabinet, he contributed to the White Paper of 1944, which is usually taken to mark a turning point in Britain in laying down the duty for governments of maintaining “a high and stable level

of employment." In the postwar world he stood for the multilateral principles of the Bretton Woods agreements, which he, with Keynes, had assisted in negotiating. But he was critical of many of the trends and underlying ideas of British economic policies in the 1950s and 1960s, such as the abandonment of monetary policy. He also warned against the risks involved in trying to push employment and growth up to too high levels. Robbins contributed notably to the discussion of the broader principles of economic policy in *The Economic Problem in Peace and War* (1947), and in his lecture "Freedom and Order" (1955), which presents a balanced survey of the economic functions of government in a free society as including "the maintenance of aggregate demand," but excluding redistribution beyond "mitigations of inequality in the interests of greater freedom."

Much of Robbins' later writings have been devoted to the history of economic thought. Although his earlier essays in this field, in the 1930s, had been mainly concerned with the neo-classical economists, he later came to concentrate on the English classical economists. Probably his best-known work on the history of thought is *The Theory of Economic Policy in English Classical Political Economy* (1952), the main theme of which is a defense of the classical economists (defined to include Hume and Bentham) against the charge that they advocated laissez-faire. Robbins' argument was illustrated by many telling quotations but was perhaps too comprehensive in seeking to exonerate all the classical economists from reproach. In 1958 came a scholarly monograph, *Robert Torrens and the Evolution of Classical Economics*, which included a learned bibliographical appendix. Since Torrens had contributed to many of the main controversies on theory and policy in the first half of the nineteenth century, Robbins' review covered most of the important themes in the history of the subject from the Ricardian period through to the later classical writers. Generally Robbins' work on the history of economic thought set an example of scholarship and erudition at a time when such qualities were passing out of fashion. He deplored the decline of interest in the history of economics as compared with the generation of Marshall and Cannan, and wrote:

I do not think that we can hope to understand the problems and policies of our own day if we do not know the problems and policies out of which they

grew. I suspect that damage has been done, not merely to historical and speculative culture, but also to our practical insight, by this indifference to our intellectual past—this provincialism in time—which has become so characteristic of our particular branch of social studies. (1952, p. 1)

From the late 1950s on, though continuing to lecture on the history of economic thought at the L.S.E., Robbins devoted himself increasingly to extra-academic interests, as chairman of the *Financial Times* newspaper; as a director of British Petroleum and of the Royal Opera House, Covent Garden; and as a trustee of the National Gallery and the Tate Gallery. Notable was his chairmanship of the Committee on Higher Education (1961–1963) which recommended a massive expansion in Britain. His later works on the history of economic thought include *The Theory of Economic Development in the History of Economic Thought* (1968); *The Evolution of Modern Economic Theory* (1970); and *Political Economy: Past and Present* (1976); as well as his fascinating *Autobiography of an Economist* (1971).

TERENCE W. HUTCHISON

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ROBINSON, JOAN

Joan Violet Robinson is the rebel with a cause *par excellence*. She has been at the forefront of most major developments, some of them revolutionary, in modern economic theory since the late 1920s. She has always believed in economics as a force for enlightenment and has coupled that belief with an equally passionate hatred of social injustice and oppression. She has thrown in her lot with the wretched of the earth, whether they be the unemployed of the capitalist world in the 1930s, or the poverty stricken and militarily oppressed of the Third World in the postwar era, or students cheated of the living fire by their professors in the 1970s.

Robinson was born on October 31, 1903, into an upper middle-class English family characterized by vigorous dissent and independence of mind. Her great grandfather was F. D. Maurice, the Christian Socialist; her father was Major-General Sir Frederick Maurice, victim of the infamous Maurice debate in 1918, who later became principal of what is now Queen Mary College in the University of London. Her mother was Helen Margaret Marsh, the daughter of Frederick Howard Marsh, professor of surgery and master of Downing at Cambridge University. An uncle was Edward Marsh, civil servant, patron of the arts, and scholar. Robinson was educated at St. Paul's Girls' School and Girton College, Cambridge, where she was Gilchrist scholar. She read for the Economics Tripos, 1922–1925, graduating in 1925 with second-class honors (“a great disappointment”). The next year, she married Austin Robinson, the Cambridge economist. After a period in India, the Robinsons returned to Cambridge in 1929. Joan Robinson joined the Cambridge faculty as a faculty assistant lecturer in economics in 1931; she became a university lecturer in 1937, reader in 1949, and professor in 1965. She was elected to a professorial fellowship at Newnham and

made an honorary fellow of Girton in 1965, and she became an honorary fellow of Newnham in 1971. She “retired” from her chair in 1971, remaining as active as ever. Although Cambridge has always been her geographical as well as her intellectual home, she is an intrepid and enthusiastic traveler, regularly visiting places as disparate as China and Canada.

Robinson has an incisive mind which allows her to cut to the heart of the matter and to detect the logical fallacy of an intricate theoretical argument or the political realities of a complex situation. She can distill the essence of a thesis into a few sharp crystal clear sentences and can translate technical literature, despite—or, as she says, because of—her innocence of mathematical training. These qualities explain why she is an outstanding theoretician. They also explain why her political analyses and judgments are sometimes simplistic and distorted, by-products of the good theoretician's ability to abstract and simplify. She is also one of the toughest people in the trade; she neither avoids nor minds confrontation. As the late Harry G. Johnson described a visit to Chicago (not recommended as a place for the timid): “Once she came to Chicago to talk to my students there; they looked at her and decided, ‘Well, we'll certainly show this old grandmother where she gets off.’ . . . [T]hey picked their heads up off the floor, having been ticked off with a few well-chosen blunt squelches . . .” (Johnson 1974, p. 30). Her barbs are spiced with a robust and civilized sense of humor, combined, it must be said, with what John Vaizey calls “bleak Cambridge rudeness.” For example, she has written: “They [the professors of M.I.T.] now admit . . . that there is no logical reason why the pseudo-production function should be [well behaved]. They just assume that it is so. After putting the rabbit into the hat in full view of the audience it does not seem necessary to make so much fuss about drawing it out again” (Robinson 1966a, p. 308). “This model was described as a parable. A parable, in the usual sense, is a story drawn from everyday life intended to explain a mystery; in this case it is the mystery which is expected to explain everyday life” (Robinson 1977c, p. 10). As we shall see, she has the ability to cast off and start anew; she is no respecter of vested interests, certainly not her own, though at any moment of time she will argue fiercely in defense and in favor of her current position.

Among her mentors and sources of inspira-

tion, four close associates hold pride of place: John Maynard Keynes, Piero Sraffa, Michał Kalecki, and, over many years, Richard Kahn, whose "remorseless logic [has been] an ideal complement to her innovative enthusiasm" (Eatwell 1977, p. 64). In the foreword to her first major book, *The Economics of Imperfect Competition* (1933a, p. v), she wrote: "Of not all the new ideas, however, can I definitely say that 'this is my own invention.' In particular I have had the constant assistance of Mr. R. F. Kahn. The whole technical apparatus was built up with his aid, and many of the major problems . . . were solved as much by him as by me." In the preface to her *magnum opus*, *The Accumulation of Capital* (1956, p. vi), she again affirmed Kahn's importance. Piero Sraffa was the inspiration for at least two of her major contributions: *The Economics of Imperfect Competition* and her contributions to the theory of value, distribution, capital, and growth. Her initial "theory of imperfect competition [was] inspired by Sraffa's [1926] article" (Kregel 1973, p. x), and for her "generalisation of the *General Theory*," especially her analysis of the meaning of the rate of profits, "Piero Sraffa's interpretation of Ricardo provided the most important clue and the long-delayed publication of his book, the *Production of Commodities by Means of Commodities* [1960], put into a sharp form the ideas that I had been groping for" (Robinson 1951-1973, vol. 4, p. 125). Her debt to Keynes is documented in many places; Kalecki's influence is discussed below. Other contemporary influences include Ester Boserup, Roy Harrod, Nicholas Kaldor, Gunnar Myrdal, A. C. Pigou, and Gerald Shove, whose "teaching in Cambridge for many years past . . . influenced . . . the whole approach to many problems of economic analysis" (Robinson 1933a, p. vi).

Of the older thinkers, the most important to her development have been David Ricardo, Karl Marx, Alfred Marshall, and Knut Wicksell. She sees herself today as in tune more with the former two than with the latter, and her lasting contribution may be her attempt to form a unified system of political economy that is classical-cum-Keynesian-Kaleckian in inspiration, directly applicable to the analysis of policy problems in the modern world. She admires Wicksell, not so much for his contributions or approach, as for his candor and honesty, which she contrasts with Marshall's attitudes (Robinson 1951-1973, vol. 4, pp. 125, 259).

Her first major work was *The Economics of*

Imperfect Competition. In writing the book, she was inspired by Sraffa's 1926 article and his "sacrilege in pointing out inconsistencies in Marshall . . . [who] *was* economics" when she came up to Cambridge in 1922. As Robinson has come to see it, the inconsistencies related to a deep-seated conflict in Marshall's *Principles* (1890) between the analysis, which is purely static, and the conclusions drawn from it, which apply to an economy developing through time with accumulation taking place. Sraffa, and presumably Robinson, saw at the time that the inconsistencies related to the internal logic of static partial equilibrium analysis, especially the dilemma of reconciling the simultaneous existence of falling supply price and competition. Looking back forty years later, Robinson states that her "aim was to attack the internal logic of the theory of static equilibrium and to refute, by means of its own arguments, the doctrine that wages are determined by the marginal productivity of labour" (Kregel 1973, p. x).

Although the latter part of the statement may reflect Robinson's hindsight and present attitudes, it is not sustained by either her stated objectives at the time or by her work in these areas up to the 1940s, especially her papers on "Euler's Theorem" (1934a), "What is Perfect Competition?" (1934b), and "Rising Supply Price" (1941)—that "excellent article . . . which has not attracted the attention . . . it eminently deserves" (Viner 1953, p. 227). Probably Keynes, in his report on the book to Macmillan in November 1932, came closest to the correct assessment. He referred to "a very considerable development of the theory of value in the last five years," developments that were found in journals and in "oral discussion at Cambridge and Oxford," and to the fact that there was "no convenient place" in which could be found "a clear statement of the nature of modern technique, or a summary of the recent work on the subject. Mrs. Robinson aims at filling this gap . . . has done it very well. . . . [T]he book will be for a little while to come an essential one for any serious student of the modern theory of value."

In *The Economics of Imperfect Competition*, Robinson explored systematically the implications, for firms in a competitive environment, of facing downward-sloping demand curves for their products, so that the profit-maximizing prices and quantities are determined by the intersections of their marginal cost and marginal revenue curves. This analysis illuminated the

real-world facts (alluded to by Sraffa) that businessmen felt it was demand conditions rather than rising costs that limited their sales, and that firms could still make profits with plants running well below capacity—facts that were incomprehensible within the framework of the Marshallian–Pigovian theories that preceded it.

In the preface to the second edition of the book, Robinson rejected much of the analysis: “to apply the analysis to the so-called theory of the firm, I had to make a number of limitations and simplifications which led the argument astray” (Robinson [1933a] 1969, p. vi). Her main dissatisfaction was with the static method and its inability to handle time. She regarded as a “shameless fudge” the notion that businessmen could find the “correct” price by a process of trial and error, because it assumed that the equilibrium position toward which a firm is tending at any point in time is independent of the path it is *actually* taking. Thus, she subjected her own analysis to what she sees as the most fundamental criticism of the general method of analysis by comparison of static equilibrium positions, a critique that she had developed in other areas in the ensuing years. She still approves of the section on price discrimination, but is distressed that certain critical aspects of the book, especially the critique of the marginal productivity theory of wages within its own theoretical framework, have been ignored while its weaknesses have been frozen into orthodox teaching.

In the early 1930s, Robinson was also playing a significant part in the formation and propagation of what has come to be known as the Keynesian revolution. Keynes was attempting theoretically to explain why the capitalist world had fallen into a deep and sustained slump, in the process (as it turns out) mounting “a powerful attack on equilibrium theory.” As is now clear from volumes 13 and 14 of *The Collected Writings of John Maynard Keynes* (1973), the people who were most influential in persuading Keynes both to modify and to expand his original analysis (Keynes 1930) and helping to develop his ideas with criticisms and contributions, included Harrod, Kahn, James Meade, Sraffa, and Austin and Joan Robinson. The last five constituted the “circus,” whose members argued out Keynes’s *Treatise* and helped formulate what was to become the *General Theory* (Keynes 1936). The fascinating exchanges that took place as Keynes moved toward the final draft of the *General Theory* indicate clearly his respect

for Joan Robinson’s contributions and judgment. She was important for both her critical grasp and her expository powers in making the new theory widely accessible to students and others. Her little book *Introduction to the Theory of Employment* (1937b) is still one of the most lucid accounts of the essentials of Keynes’s theory, as are her “Essays 1935” (Robinson 1951–1973, vol. 4, part 2). Furthermore, she was one of the first to extend Keynes’s analysis to an open economy.

In her essay, “Kalecki and Keynes” (1951–1973, vol. 3, pp. 92–93), Robinson described how in the early years of the depression Keynes, who was groping for a theory of employment (which he ultimately found in his theory of effective demand, the possibility of sustained underemployment equilibria or rest states), set Kahn to work out properly the impact of a rise in investment on employment and saving in order to back up Keynes’s argument supporting Lloyd George’s scheme for public works. Kahn’s famous article on the multiplier came out in 1931. The *Treatise* went to the printers for the last time in September 1930. It contained no theory of employment, being concerned mainly with fluctuations in the general level of prices, though it did have the “highly significant conception” of a relationship between investment and saving via profits. A three-year argument that churned over these ideas followed. Austin Robinson (1977, p. 35) recalled that by the end of 1931, “the questions [which] the *General Theory* set out to answer” had begun to be asked by Keynes and his junior colleagues. “In 1933 [Joan Robinson] published an interim report which clears the ground for the new theory but does not supply it.” That, of course, had to wait until the publication of the *General Theory* itself in early 1936. Even then, there were “moments when [there was] some trouble in getting Maynard to see what the point of his revolution really was. . . . [However,] when he came to sum it up [Keynes 1937] after the book was published he got it into focus” (Robinson 1973, p. 3). For Robinson, the central themes of the *General Theory* were the theory of effective demand in which is integrated a theory of money and the interest rate; a theory of the general price level; and an analysis of the impact of an uncertain future on the present, which occurs through investment expenditure, thus locking Keynes’s analysis securely into actual historical time.

Important as her contribution to Keynesian

(of the *General Theory*) analysis has been, the most significant step in her thought occurred when she decided to graft Marx onto Keynes, partly through the influence of Kalecki, a Polish Marxist contemporary who independently discovered the main propositions of the *General Theory*. She dates this effort at 1940, but with Sraffa and Maurice Dobb as colleagues, and given her interest in generalizing the *General Theory*, her interest was probably aroused sooner. Similarly, she claimed that "in 1940, as a distraction from the news, I began to read Marx. . . . For me, the main message of Marx was the need to think in terms of history, not of equilibrium" (Kregel 1973, p. x). Again this view benefits from hindsight; it was Harrod's work, *Towards a Dynamic Economics* (1948), which she reviewed in 1949, that really brought this message home. She thus found in Marx what she also found in Keynes. As she has said elsewhere of the Keynesian revolution: "[O]n the plane of theory, the revolution lay in the change from the conception of equilibrium to the conception of history; from the principles of rational choice to the problems of decisions based on guess-work or on convention" and "once we admit that an economy exists in time, that history goes one way, from the irrevocable past into the unknown future, the conception of equilibrium based on the mechanical analogy of a pendulum swinging to and fro in space becomes untenable" (1973, pp. 3, 5). Thinking in terms of history also involves always asking what sort of society (and its accompanying institutions) is being examined and what its ruling social relationships are. It involves, moreover, distinguishing between theories that deal with logical time and those which deal with historical time: "Logical time can be traced from left to right on the surface of a blackboard. Historical time moves from the dark past behind it into the unknown future in front" (1977a, p. 57). Analyses in logical time are at best the flexing of intellectual muscles, sometimes in a framework in which to sort out doctrinal puzzles, usually as a preliminary to the real thing, the analysis of processes occurring in historical time. This approach also implies that economics is very much a "horses for courses" discipline, rather than a general theory into which particular situations may be fitted as special cases.

Her book on Marx (1942) is still one of the best introductory pieces to be found, despite its idiosyncrasies and even though, or perhaps

because, it contains a few heresies that infuriate the faithful. Especially is this true of her attitude to the labor theory of value, which has hardened over the years: "[w]e are told that it is impossible to account for exploitation except in terms of *value*, but why do we need *value* to show that profits can be made in industry by selling commodities for more than they cost to produce, or to explain the power of those who command finance to push around those who do not?" (1977a, p. 51). To learn from Marx's ideas we do not have to remain "stuck in the groove that led him to them." Nevertheless, the book was not written "as a criticism of Marx [but] to alert my bourgeois colleagues to the existence of penetrating and important ideas in *Capital* that they ought not to continue to neglect" (*ibid.*, p. 50). It abounds in insights and produces a lucid sketch of the skeleton that sustains Marx's system, a skeleton too often obscured by the flesh of Hegel, by polemic, and by the lack of time and health to polish and rewrite that characterizes much of Marx's own writing. It is a constructive and sympathetic critique of Marx's work. The same may be said of her subsequent writings on Marx, attitudes that contrast with the impatience she sometimes shows toward Marxists themselves.

This book led Robinson into her two main preoccupations of the postwar period: on the positive side, the attempt to provide a "generalisation of the *General Theory*, that is, an extension of Keynes's short-period analysis to long-run development" (1956, p. vi), found largely in *The Accumulation of Capital* and interpretative books and articles that have grown up around and from it: *Exercises in Economic Analysis* (1960), *Essays in the Theory of Economic Growth* (1962b), *Economic Heresies* (1971). A further influence on the way may well have been Rosa Luxemburg's book (1913) of the same title, to the English edition of which Robinson contributed the introduction (Robinson 1951-1973, vol. 2, pp. 59-73). Robinson's own work provides us with a Keynesian-Marxist framework (derived in structure from Kalecki's adaptation of the Marxian schemes of reproduction) with which to interpret the process of growth in capitalist economies and to tackle the grand problems of classical political economy: the possibilities of growth in output per head, and the course of the distribution of the national product between broad classes in capitalist society as capital goods are accumulated over time, as these processes are influenced by the nature of the

“animal spirits” of the businessmen, as well as by population growth and technical advances. In this area, Robinson shares with Harrod, and possibly, with Kaldor and Pasinetti, the credit for the most influential contributions of the Keynesian school to the modern theory of economic growth and distribution.

The Accumulation of Capital has sometimes been misunderstood. Robinson starts the analysis with an examination of the conditions necessary for steady growth, a search for the characteristics of what she calls “golden ages.” Too often this has been taken for descriptive analysis rather than the careful setting out of logical conditions and relationships, one of the principal purposes of which, as is hinted at by the very name, is to show how unlikely it is that they will ever be realized in fact. “I used the phrase ‘a golden age’ to describe smooth, steady growth with full employment (intending thereby to indicate its mythical nature)” (1962*b*, p. 52). In the subsequent clarifications and expansions of her findings, she has emphasized more the lessons of the later chapters on the short period and the interconnections of short periods over time. She also has reiterated what she stated in the original work, that the sections on the choice of techniques of production at the level of the economy as a whole occupy more space than their importance (as opposed to their difficulty) warrants. Moreover, they relate largely to the realm of doctrinal debate associated with the vast literature on the aggregate production function rather than to that of positive analysis.

Her second postwar concern has been a sustained attack on the currently received paradigm of economics, the neoclassical theory of value, production, and distribution. Its focus has been centered in the last 25 years in the theory of capital, mainly because of her celebrated article “The Production Function and the Theory of Capital” (1953–1954), which started what have become known as the “Cambridge controversies” in the theory of capital. The main protagonists in the controversies have all been associated, either temporarily or permanently, with the two Cambridges—Cambridge, England, and Cambridge, Massachusetts, where M.I.T. and Paul Samuelson and Robert Solow, are situated. On the surface, the argument is over whether or not it is possible to measure “capital” as a factor of production; what units to use; whether there is a unit that is independent of distribution and prices; and whether any sense can be made of

the proposition that the marginal product of capital equals the rate of profits. But, as Robinson has stressed continually, the argument really has nothing to do with the problem of measuring and valuing “capital,” as opposed to the *meaning* of “capital,” but involves the attempt of those she dubs “bastard Keynesians” to reconstruct “pre-Keynesian theory after Keynes”: “[I]t has nothing to do either with measurement or with capital; it has to do with abolishing time. For a world that is always in equilibrium there is no difference between the future and the past, there is no history and there is no need for Keynes” (1973, p. 6). Nor has it anything to do with Marx: “The controversies over so-called capital theory arose out of the search for a model appropriate to a modern western economy, which would allow for an analysis of accumulation and of the distribution of the net product of industry between wages and profits. . . . Long-run accumulation became the centre of interest, [so making] it necessary to come to grips with concepts of the quantity of capital and the rate of profit in the economy as a whole” (1977*c*, pp. 5, 6–7). Robinson sees the response to her criticisms as the outcome of an ideological tide that reacts continually against the damaging criticisms of Marx, Keynes, and Sraffa; and that attempts to create an economic theory supporting, by implication at least, the status quo—in particular, democratic capitalist free market institutions and, in at least some influential quarters, a doctrine of *laissez-faire*.

Here Robinson may have aimed at too many targets at once. The groups most favorably disposed to *laissez-faire*, Milton Friedman and the Chicago School, and their burgeoning offshoots elsewhere, have been vigorously attacking what *they* take to be the exposed flanks of the American or “bastard Keynesians,” who are led from M.I.T. and Yale University, while Robinson has been attacking what she takes to be other vulnerable areas. The attacked themselves could, with justice, claim not only that they are staunch advocates and defenders of middle-of-the-road to leftist Keynesian policies, but also that they have provided much of the ammunition that has been used over the years to destroy the more grandiose claims for a free-rein market economy as an efficient allocator of resources and maximizer of community welfare. As Tobin (1973, p. 106, note 1) remarks, “[Samuelson’s] work on the theory of public goods . . . is only an outstanding example of the attention modern theorists, in America and overseas, have paid to the

allocative failures of *laissez-faire*." Thus, Robinson's simplicity of vision, though faulted in details, is nevertheless, in its general argument, largely valid; hence the irritation and anger she arouses, especially in conservative academic and political circles.

In the Cambridge debates, Robinson has tenaciously and consistently returned to the theme that orthodox equilibrium analysis cannot handle the essential facts of a capitalist economy—the fact that it exists in real historical time; that investment decisions by capitalist businessmen (and not the saving decisions of households) are the dynamic driving force of the economy; that uncertainty and unrealized expectations about the future are inescapable facts of life that *must* find a place in any theory of the development of a capitalist economy over time; that "interest [is] the price that a businessman pays for the use of finance to be committed to an investment [while] profit [is] the return that he hopes to get on it, [and that] wage rates are settled in terms of money [while] the level of real wages depends upon the operation of the economy as a whole" (1977c, p. 5). An index of her success in these endeavors is that both Samuelson (1975) and F. H. Hahn, in a number of places, including his inaugural lecture (1973), have either explicitly or implicitly conceded the validity of many of her claims. Tobin, in an otherwise rather pained review of her *Economic Heresies* and the Cambridge debates, nevertheless praises her repeated stress on the treatment of expectations and her objection that "Walrasian general equilibrium, even when enlarged to postulate markets in all commodities in all contingencies at all future dates, is no real solution" (1973, p. 109). John Hicks, having partly repudiated the versions of Keynesian theory that are peculiarly associated with him through his 1937 paper "Mr. Keynes and the 'Classics'" now takes approaches that closely parallel Robinson's (see, for example, Hicks 1976; 1977). In addition, Robinson, along with others, especially Sraffa, has exposed the logical inconsistencies in those versions of neo-classical theory that attempt to provide a theory of distribution to take the place of classical, especially Ricardian, theory and also, of course, of Marxian theory. These particular criticisms came to a head in the reswitching and capital-reversing debates of the mid-1960s. The debates themselves were the culmination of earlier discussions of whether certain results that were rigorously true of simple one-commodity neo-

classical models would continue to be so in more complex heterogeneous capital-good models. Robinson now regards these particular criticisms and results as "unimportant" (1975). She prefers to rest her critique on her more general methodological arguments, and her insistence that one must always postulate the social relationships and institutions of the economy being modeled and specify the stage in its history at which the analytical story is taken up. Finally, in her Richard T. Ely lecture to the American Economic Association Meeting in 1971 (a personal triumph in which the main room overflowed into subsidiary rooms and for which she received a standing ovation), Robinson identified a second crisis in economic theory (the first being its inability to handle the interwar slump): the lack of a suitable framework with which to tackle the terrible problems of modern economic life—poverty, racism, urban puzzles and pollution, excessive population growth, and war.

Her latest work on these questions is her comprehensive paper "What Are the Questions?" (1977d). She starts by arguing that ideology and economic analysis are indissolubly mixed, and that the dominant ideology exerts disproportionate power in the discipline at any moment of time; she quotes Benjamin Ward (1972, pp. 29–30) in support. She then blasts Lionel Robbins' definition of economics when it is set in the context of a capitalist economy: "The question of scarce means with alternative uses becomes self contradictory when it is set in historical time, where today is an ever-moving break between the irrevocable past and the unknown future. At any moment, certainly, resources are scarce, but they have hardly any range of alternative uses" (1977d, p. 1322). She deplores the major distinction in modern orthodox economics between micro and macro. One cannot exist without the other, for "[m]icro questions . . . cannot be discussed in the air without any reference to the structure of the economy in which they exist [or] to the processes of cyclical and secular change. Equally, macro theories of accumulation and effective demand are generalizations about micro behaviour. . . . If there is no micro theory, there cannot be any macro theory either" (p. 1320). Moreover, the macro setting for orthodox micro theory is a kind of vague Say's Law world which, until very recently anyway, is *not* the macro world that is analyzed in *its own separate compartment*.

Robinson has also made several contributions to the theory of international trade. She was

among the first systematically to apply the Keynesian mode of thought to the problems of an open economy; she wrote a seminal article on the theory of the foreign exchanges (1937a), and in her inaugural lecture, *The New Mercantilism* (1966c), and lectures at Manchester University (reprinted in Robinson 1951–1973, vol. 4), she applied her general critiques of orthodox theory to the special area of international trade and suggested alternative avenues of approach. In a “half-way house” paper (1946–1947), she critically expounded the classical theory of international trade as it came down from Marshall, “to try to see what basis it offers for the belief in a natural tendency towards equilibrium” (p. 98).

She returned to this theme again in “What Are the Questions?,” pointing out that Ricardo (“[i]n the famous story which begins with England and Portugal both producing both cloth and wine”) was the first to commit the cardinal sin (in her eyes) of analyzing a process going on through time by the comparison of two equilibrium positions—an invalid procedure that is, as we have seen, the center piece of her critique of orthodoxy (though Ricardo, as a pioneer, is absolved). It must be said that Samuelson, whom she has repeatedly criticized along with Solow for doing this, courteously but firmly denies it, producing chapter and verse in support (Samuelson 1975). Furthermore, Pierangelo Garegnani, an influential ally of Robinson and Sraffa in their attack on neoclassical theory, also takes issue with her on this point. He argues that comparisons of long-run positions (not, note, equilibrium ones, for equilibrium is a notion intimately related to supply and demand) are fundamental to economic methodology. However, he maintains, the neoclassicals err when they try to incorporate the method with *their* overriding emphasis on the forces of supply and demand. *Their* theories, he argues, then run into insuperable logical difficulties, especially in the depiction of the demand curve for “capital” and the consequent existence and stability of long-run equilibrium positions (see Garegnani 1958; 1970; 1976). That is to say, Garegnani wishes to preserve the tradition that began with the classicals of relating key concepts—for example, natural prices—to sustained and fundamental forces. He feels that Robinson’s attack on orthodoxy threatens this tradition too. Robinson also wishes to retain the key classical concepts but scrap the method.

There is a puzzle that often emerges in discussions of Robinson’s contributions—namely,

her lack of empirical work of at least the conventional kind. The answer probably lies in two areas: first, she has been concerned mainly with such fundamental theoretical questions as the necessary setting out of definitions, concepts, and logical relationships; the provision of a framework that must precede good empirical work. Secondly, her close associates over the years, Kahn and, of course, Keynes and Kalecki, were applied economists in the old-fashioned sense *par excellence*. They made it their business to know intimately the institutions, the historical sequences, and the orders of magnitude of particular situations, and they had a feel for the limits of particular policy recommendations. Robinson’s work, therefore, was often complementary to theirs, as theirs was to hers. Moreover, much of her theoretical work is based on Marshallian-type empirical generalizations—that is to say, broad qualitative statements that constitute either the basis for the development of a logical argument or the puzzles that are to be explained by theoretical reasoning.

Robinson’s admiration for, and extensive writings on the Chinese experiment are well known, probably to an audience wider than that for any of her other works. She is always stimulating, full of insights, putting a complicated and changing scene into a manageable framework. Her writing in this area contains a leaven of advocacy, a conscious effort to try to offset what she believes to have been unsympathetic critiques of Chinese policies emanating from orthodox circles. In addition, she has written extensively on the theoretical and practical aspects of planning in socialist societies, on the basis of her experience with, and criticisms of, the Russian and eastern European experiments.

Her championship of Kalecki’s independent discovery of the main propositions of the *General Theory* is well known from a number of delightfully written and absorbing articles (especially Robinson 1976; 1977b; the latter is also a fine introduction to, and exposition of, Kalecki’s analysis of capitalism). Moreover, time is confirming her judgment that “[i]n several respects Kalecki’s version is more robust than Keynes’” (Robinson 1977b, p. 10). Nor has she neglected the involved intellectual’s task of communicating to a wider circle than those in their discipline. She contributed a charming and influential book, *Economic Philosophy* (1962a), to the New Thinkers Library although it is, perhaps, too Popperian for most Marxists’ taste. For a wide audience she has written *Economics*:

An Awkward Corner (1966b), which diagnosed Britain's economic ills, and *Freedom and Necessity* (1970), which is a model for a challenging introductory course in the social sciences.

Last, but certainly not least, her concern for students and what they are taught has been expressed in a number of areas. Generations of Cambridge undergraduates and research students have paid tribute to her as a demanding but devoted supervisor. She has lectured to students across the world, often at their request. In 1973, she wrote, with John Eatwell, *An Introduction to Modern Economics*, a new type of textbook that she fervently hoped would herald a new dawn in the teaching of economics. Although splendid in conception, it is rough in execution, and too ambitious. Robinson tried to distill her lifelong ponderings into one work for first-year students. Even the cream of the British intelligentsia whom she and Eatwell are accustomed to teach find it more than hard going. Nevertheless, it is a noble experiment that should not be ignored, and that may serve to produce the "generation well educated, resistant to fudging, imbued with the humility and the pride of genuine scientists [making] contributions both to knowledge and to the conduct of affairs that no one need be ashamed of" (Robinson 1957-1973, vol. 3, p. 6). But whatever the outcome of this particular venture, Joan Robinson herself has much more than fulfilled her own modest aim of doing "a little good here and there to set in the scales against all the harm" (*ibid.*).

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ROGERS, CARL R.

Carl R. Rogers is best known as the founder of client-centered psychotherapy, which was first identified as nondirective counseling or therapy. The movement originated by Rogers is currently labeled the "person-centered approach." As such, it applies to interpersonal behavior and relationships in many fields, not only psychotherapy, but education, marriage and other types of partnership, leadership, organizational development, conflict resolution, and the facilitation of a large group process in which individuals

come to realize their own power and use it in the interest of the community. The latter is seen by Rogers and his associates as having revolutionary implications for resolving some of the life-and-death issues of our times. Indeed, because of the everwidening scope of his influence, as well as the nature of his original impact on the field of psychotherapy, Rogers has become known increasingly as a "quiet revolutionary," and a father of the "third force," or humanistic psychology, as distinguished from the psychoanalytic and behavioral approaches.

Rogers' influence stems from his profound belief in and his success in communicating certain basic values. The most central of these is the hypothesis that all living organisms contain within themselves powerful intrinsic forces for orderly growth. These may be inhibited under certain conditions or, on the other hand, facilitated by the provision of other conditions. In the human being, a central organizing tendency is related to the person's concept of self, and the drive for self-actualization. Rogers, individually and with many associates, has developed and tested extensively the theory that, with the creation of therapeutic conditions such as empathy, congruence (or genuineness), and unconditional positive regard (or nonpossessive caring), the actualizing forces within the individual or group will be released. These activities have been carried out under live, natural conditions and with increasingly large numbers of people.

With origins in the Midwest and identified at first as a logical positivist in the American scientific tradition, Rogers' outlook and practice have broadened to include much of Eastern philosophy as well as European existential thought. At the same time, his activities and recognition have become more and more international in scope, including the European, Asian, South American, and Australian continents.

In his own country, Rogers' position has been paradoxical. On the one hand, he has been the recipient of the highest honors of the academic and professional psychology and psychotherapy establishment. He has been president of the American Psychological Association (1947); first president of the American Academy of Psychotherapists (1956-1958); one of the first group of three recipients (with Wolfgang Köhler and Kenneth W. Spence) of the American Psychological Association award for distinguished scientific contribution, the first recipient of the APA award for distinguished professional contribution. He has been professor of psychology

at Ohio State University and the University of Chicago as well as professor of psychology and psychiatry at the University of Wisconsin. He has been awarded a doctor of science from Northwestern University and many other honorary degrees.

At the same time, Rogers is regarded as a rebel, a revolutionary, and an iconoclast, and has voluntarily removed himself from the academic and professional establishment; instead of being a university professor or institutional president, he has chosen to be a "resident fellow" of a forward-looking group that is marked by flexible organization and close personal relationships. While institutions continue to turn to him, he has consistently marched to his own tune, forming fresh associations and generating new experiences and ideas concerning the facilitation of personal power and community actualization.

Carl Ransom Rogers was born on January 8, 1902, the son of Julia Cushing and Walter Rogers, a contractor and engineer. He was the middle son among five brothers, and had one sister, in a family characterized by devotion and religious fundamentalism.

Rogers attended public schools and entered the Agricultural College of the University of Wisconsin in 1919. During his first two years, he attended some religious conferences, became very involved, and decided to become a minister. To prepare himself better for this, he became a history major and in his junior year, he was selected as one of a dozen American students to attend the World Student Christian Federation Conference in China. He graduated in 1924, and on August 28th of that year he and Helen Elliott were married; their children are David and Natalie.

Graduate training. After graduation, Rogers entered Union Theological Seminary in New York City. Particularly influenced by experiences in a student organized seminar and in a course on "Working with Young People" taught by progressive educators and the psychologist Goodwin Watson, he transferred "across the street" to Columbia University's Teachers College, where he studied clinical psychology and received his PH.D. in 1931. For his doctoral dissertation, Rogers developed a test, *Measuring Personality Adjustment in Children Nine to Thirteen Years of Age* (1931), which was marketed by the Psychological Corporation and is still a highly successful and distinctive clinical instrument. It was an early demonstration of Rogers' prefer-

ence for understanding people through a phenomenological approach, his appreciation of the importance of self-concept, his clinical sensitivity, and his talent for devising objective methods for the measurement of subjective data. At the same time, Rogers' education at Columbia and his internship at the psychoanalytically oriented Institute for Child Guidance in New York City contributed to a professional stance characterized by objectivity and personal distance, noteworthy because of the contrasting ideology that he developed later.

Rochester. From 1928 to 1940, Rogers worked in Rochester, New York, starting as a psychologist in the child study department of the Society for the Prevention of Cruelty to Children. In 1930, he became the director of this department and, in 1939, of its replacement, the Rochester Guidance Center.

In this setting, Rogers continued to perfect his skills by practicing traditional child guidance based on the clinician's assessment of the situation. In the latter half of the 1930s, this approach took an extremely significant new turn—the recognition of the client's capacity for insight and innovation—which Rogers has related to several factors.

One of these was that the new Rochester Guidance Center had more self-referrals, so that the helping process was more dependent on the development of a relationship between clinician and client. Another influence was Rogers' experience that listening and following the client's lead often obtained better results than did the clinician's guidance. Rogers has cited one particularly significant case of a problem child who had not responded to the traditional diagnostic-prescriptive approach. The mother in question asked to be seen, and in the ensuing interviews, she became stronger as an individual. This was associated with improvement in her marital relationship and her son's behavior. Rogers described this experience as a crucial one in changing his clinical approach, which was furthered by a two-day seminar with Otto Rank and his relationship with a social worker on his staff who had trained with Rank at the Philadelphia School of Social Work. This period was capped by the publication of *The Clinical Treatment of the Problem Child* (1939), a comprehensive book covering both traditional approaches to the child guidance field and the newer "relationship therapy."

Ohio State University. Rogers accepted a position as professor of psychology at Ohio State

University in 1940. Here he taught courses in counseling practice and theory, began systematically to develop and write about counseling, psychotherapy, and personality theory, and started to build rewarding relationships with graduate students. Virginia Axline, Arthur Combs, Bernard Covner, Thomas Gordon, Donald Grummon, Nicholas Hobbs, E. H. Porter, Jr., Victor Raimy, William U. Snyder, and Bernard Steinzor exemplify students who worked with Rogers at Ohio State and then became outstanding authors, teachers, therapists, and professional leaders. As Rogers moved on to other universities and institutions, this process of mutual stimulation and growth multiplied and grew into a legion, not of followers, but of distinctive contributors to personality theory, clinical and educational practice, parent-child relationships, concepts of leadership, the development of professional standards, and other areas.

It became clear to Rogers and others, soon after he came to Ohio State, that he was developing a new way of practicing counseling and psychotherapy, and his book entitled *Counseling and Psychotherapy* was published in 1942. This included the verbatim typescript of an electrically recorded eight-interview case, a precedent breaking contribution that initiated the objective study of the therapy process. Rogers and his students rapidly developed methods of classifying client statements and counselor responses and of measuring self-regarding attitudes. The significance of the Ohio State period, then, in Rogers' career and in the history of the person-centered approach, includes the emergence of the self-concept as a central construct of personality organization, the refinement of recognition and clarification of feeling as a basic therapeutic technique, and the enormous advances made in the objective study of personality change in therapy and of therapist and client behavior in psychotherapy. In a more general way, it was dawning on Rogers and his students that the nondirective approach had far-reaching implications for all types of interpersonal relationships.

The University of Chicago. During World War II, Rogers was on leave from Ohio State as director of counseling services for the United Service Organization. In 1945, he accepted a position as professor of psychology and head of the counseling center at the University of Chicago. Rogers remained at Chicago for 12 years, a period of tremendous growth in client-centered theory, philosophy, practice, research, applica-

tions, and implications. Central to this expansion in scope and depth was the organization of the counseling center. As an extension of the trust that was working so successfully with his clients, Rogers deliberately set out to empower the staff of the center to assume full responsibility for its functioning. The result was not a smooth, conflict free operation, but a highly successful and gratifying one, regarded by many of its constituents as the best institutional experience of their careers. This staff group phenomenon was paralleled by training workshops that included the elements of the basic encounter movement. Student-centered learning became the basic educational process in the teaching of personality theory and psychotherapy; in this approach, the student was given the opportunity to decide what and how he wished to learn and how he should be graded. Congruence and unconditional positive regard were added to empathy as "necessary and sufficient conditions for therapeutic personality change" in a classic article published by Rogers in 1957. The therapist was recognized clearly as a person in the psychotherapeutic process, rather than as a narrowly skilled, professionally involved practitioner. Great interest was shown in Martin Buber, Søren Kierkegaard, and other existentialist thinkers and, at the same time, Rogers contrasted his free choice philosophy with the deterministic values of B. F. Skinner and behavior modification. The client-centered approach was applied broadly to interpersonal communication and relationships. Sophisticated research methodology was designed and carried out in large-scale research projects on personality change in psychotherapy. Among Rogers' students and collaborators at Chicago, in addition to many who had also been with him at Ohio State, were Godfrey Barrett-Lennard, Oliver Bown, John M. Butler, Rosalind Dymond Cartwright, Eugene Gendlin, Gerard Haigh, Jules Seeman, John Shlien, Stanley Standal, Eugene Streich, Ferdinand van der Veen, and Fred Zimring.

The University of Wisconsin. Rogers was professor of psychology and psychiatry at Wisconsin from 1957 to 1963. Again with many collaborators, he designed and executed a massive research project to test the basic hypothesis that the provision of and perception by the client of the therapeutic conditions of congruence or genuineness, acceptance or unconditional positive regard, and empathic understanding would lead to significant personality change. In this case,

the clients were state hospital patients diagnosed as schizophrenic, so a significant factor was that the client-centered hypothesis was being tested on both a highly disturbed and often unmotivated population. The central construct employed in the study of the personality change of these individuals was termed "experiencing," and scales were developed by Rogers and Rablen, and by Gendlin and Tomlinson, to see if therapy resulted in an approach to experience less rigid, fixed, and remote, and more fluid, changing, responsive, and "owned" by the individual. A vast array of both existing and newly devised tests and rating scales were employed to measure both the conditions and effects and the results were generally positive, but also very complex because of the multiplicity of the measures. The tremendous research achievement, represented by the volume, *The Therapeutic Relationship and Its Impact: A Study of Psychotherapy With Schizophrenics* (1967), was accompanied by subjective advances in learning to relate to extremely withdrawn individuals. They were approached no less as persons than people functioning in the community, and both Rogers and Gendlin described intense relationships that included sitting with patients during many hours of silence as well as initiating thoughts and feelings to try to understand what patients were experiencing and to express their own caring. Such therapist activity sometimes resulted in dramatic responses and exchanges.

La Jolla. In La Jolla, California, at the Western Behavioral Sciences Institute, beginning in 1964, and at the Center for the Study of Persons, since 1968, Rogers has generalized and extended the principles of client-centered psychotherapy to "the person-centered approach" on a large scale. Several arenas of this application can be cited: entire school systems, the institution of marriage, community process in large groups. Rogers has written major books on the "freedom to learn," "becoming partners," and "personal power." In addition, he has given talks and made films and audiotapes that have had great impact on these and related areas, such as intercultural conflict resolution. He has shown deep concern for the position of individuals disadvantaged by reason of economics, race, sex, and status. He has traveled on a world-wide scale to work with people in small and large groups in Asia, Europe, South America, and Australia. In all these enterprises, he has espoused the basic principles that have

made his reputation, and which may be paraphrased in this way:

I understand that you may see me as an authority but, like you, I am a person who can feel doubtful and fearful as well as strong and confident. I am more comfortable seeing myself as a facilitator rather than therapist or teacher or other kind of expert. I think I can help you most by being real myself, by prizing you and your individuality and by trying to understand the way you look at and feel about yourself and others. I hope I can help you to be yourself as much as possible and to realize all your possibilities. I am willing to be an understanding and empathic partner as you struggle to do this.

Perhaps the most revolutionary aspect of this philosophy is that it is the one with which Rogers enters into any new relationship. The person may be a stranger, an executive or laborer, an old person or a child, a completely unthreatening member of the majority or a possibly hostile minority group member, an individual who is fluent or one who is halting, a person with a "perfect record" or a convicted murderer.

Rogers implements his philosophy by making himself part of a sharing support group, which in turn will facilitate the development of a community in a group of perhaps hundreds. A few of the people with whom Rogers has been associated in this way include Maria Villas-Boas Bowen, Maureen Miller, Natalie Rogers, and John Wood. Richard Farson, who was instrumental in bringing Rogers to California, is a colleague who has most effectively articulated his revolutionary impact.

NATHANIEL J. RASKIN

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ROPER, ELMO

The 1936 presidential election marked the first national application of modern sampling methods to preelection polling. Instead of relying upon the weight of large numbers to provide accuracy, which was the method used by straw polls, such as the up to then prestigious *Literary Digest* poll, Elmo Roper, George Gallup, and Archibald Crossley based their polls on relatively small samples selected in a manner intended to yield representative cross-sections of the electorate. All 3 correctly forecast Franklin D. Roosevelt's landslide victory over Alfred Landon (Roper being within 1 percentage point of the actual vote), while the *Literary Digest* predicted a Landon victory margin of 19 percentage points. This demonstration of the accuracy of small representative samples was the start of the now flourishing political polling industry, which has become an integral part of the "managerial" style of political campaigning characteristic of the 1970s.

Little in Roper's prior career presaged his eminence in election and public opinion polling. A former jewelry salesman who had entered market research, Roper had no specific background in public opinion. Born July 31, 1900, in Hebron, Nebraska, he had studied at the University of Minnesota and the University of Edinburgh without completing his degree. After leaving Edinburgh, he opened a jewelry store in Creston, Iowa, where he discovered that he could increase sales by talking to customers to determine their styling preferences. In early 1929, he closed his store and went to work as a route salesman first for the Thomas Clock Com-

pany, later the New Haven Clock Company, and then the Traub Manufacturing Company. As he advanced into sales management, Roper applied his earlier experience in talking to retail customers to sales forecasting by seeking retailer reactions to the various product lines under consideration. He discovered that this method yielded far more accurate sales forecasts than did the "informed" judgments of his associates. By 1933 he was in New York, where he soon teamed up with Paul Cherrington, of the Harvard Business School, and Richardson Wood, an advertising writer from the J. Walter Thompson Company, to form a market consulting firm. (Wood left the firm in 1936 and Cherrington in 1938. In 1952, Carol Crusius and Elmo Roper's son, Burns, became partners in the firm. Elmo Roper retired from the firm in 1967, four years before his death on April 30, 1971.)

Roper's primary responsibility with the new firm was to conduct market studies in which he applied the techniques that he had used in developing jewelry sales forecasts. These techniques became a hallmark of his approach to both marketing and public opinion research—namely, reliance upon a small, highly trained interviewing staff, most of whom worked only for him, and upon a relatively qualitative probing of attitudes. Initially, Roper did all the field work himself, but heavy work loads soon made it necessary to hire a small staff to supplement his own field investigations. To be sure that the staff covered all the points that he would, Roper prepared lists of questions for them to ask. To control the selection of the subjects whom the field investigators would interview, he developed guidelines from a careful examination of census data. Out of this process emerged the survey method of using formal questionnaires with quota samples.

A combination of commercial utility and Roper's long-standing interest in politics led to the founding of the *Fortune* poll and Roper's successful entry into preelection polling. In 1935 Henry Luce engaged Roper's firm to conduct the first of a series of public opinion surveys, which became a regular feature in *Fortune* magazine through September 1950. As the 1936 election approached, Roper felt that these surveys provided him with an excellent opportunity to demonstrate the accuracy of the methods he had been using in his market studies.

A particular method Roper employed in the

1936 polls, and one that he continued to use in subsequent election years, was a four-point verbal attitude scale, which he preferred to a direct preference question. Roper's use of verbal attitude scales contrasted with the characteristic Gallup method, which relied upon dichotomous questions designed to measure the way public opinion splits during an election or on an issue. Roper believed that in the tense political climate of 1936 many working-class Roosevelt adherents would be reluctant to admit a voting preference for Roosevelt. In fact, the method did produce a low undecided vote, apparently because many were willing to rate Roosevelt moderately favorably on a scale, even though they were hesitant to say they would vote for him.

Roper's use of quota sampling methods was common to public opinion polls throughout the 1930s and 1940s and was the target of intense criticism from researchers such as Rensis Likert, who were pioneering the application of area probability sampling to surveys of human populations. These criticisms, however, had little impact as long as polls based on quota samples produced results that were close estimates of actual voting behavior. In 1940 Roper's final preelection poll was one-half of one per cent off the mark, and in 1944, two-tenths of one per cent. In September 1948 Roper announced that in light of Thomas E. Dewey's then sizable lead over Harry S. Truman, there was no purpose in conducting another poll of voter preference: Roper's decision was based on his experience in the 1936, 1940, and 1944 elections, which had indicated that most voters had decided early in the campaign whether they would vote for Roosevelt or for his Republican opponent. (In October, Gallup and Crossley also decided not to conduct a final survey.) However, postelection analyses indicated that Truman's victory was the result of many voters either changing their minds or deciding late, so that it was not until the final week of the campaign that Truman took the lead.

The fact that all the major preelection polls had forecast a Dewey victory created a storm of public criticism and placed the entire survey profession in question. The Social Science Research Council established a special committee to investigate the polls and their methods. Roper, along with Gallup and Crossley, cooperated fully in this investigation. The pollsters themselves became most concerned with ways of measuring last-minute trends, identifying

likely voters, and allocating undecided voters, while outside critics, like Likert, focused much of their criticism on the use of quota sampling methods. The lasting results of the 1948 experience included the eventual acceptance of the superiority of probability sampling over quota sampling, though various versions of "modified probability" sampling continued to be used by some. More generally, the 1948 experience led to a more sober appreciation of the methodological pitfalls of election polling—especially the need to measure changes in voter preferences during the course of a campaign, rather than to treat poll results as forecasts.

Roper's role in public opinion research. It was probably through his network of personal relationships that Roper made his greatest impact on the developing field of public opinion research. This is manifest in the roster of people who worked with and for him over the years, including Raymond Franzen, Solomon Dutka, Louis Harris, John Kraft, Alfred Politz, Oliver Quayle, Robert Pratt, Robert Petty, Julian L. Woodward, and Elmo Wilson. He wrote little for the academic world, preferring rather to direct his writing to national business and opinion leaders and to the general public in such publications as *Fortune*, the *New York Herald Tribune*, and the *Saturday Review of Literature*. Although Roper's formal relationship with academia was limited to a short period in the 1940s, when he served as an assistant professor, giving occasional lectures, at the Columbia University Graduate School of Journalism, he always maintained close personal ties with the academic world, most notably with Paul F. Lazarsfeld, who served as a consultant to Roper during the late 1940s and early 1950s. These contacts had a dual impact. On the one hand, they provided a channel whereby Roper was kept up to date on theoretical developments; on the other hand, the academic world was enriched by access to Roper data. A major contribution to academic research was his founding of the Roper Research Center at Williams College, a data archive that has made commercial research data available for secondary analysis.

During the years immediately preceding World War II and during the war itself, policy makers first made effective use of public opinion polls. An early instance was Roosevelt's decision in 1941 to sell destroyers to Great Britain for its campaign against the German U-boats. In the intense conflict between interventionists and isolationists, there was considerable doubt

as to congressional acceptance of this sale. Public opinion polls were conducted by Roper and Gallup, and on the basis of these polls Roosevelt decided that it was politically safe to complete the sale.

The early war years were also marked by Roper's association at high policy levels with such government agencies as the Office of Production Management and the Office of Facts and Figures. He served as deputy director for the Office of Coordinator of Information and, later, as deputy director for the Office of Strategic Services. He was a "dollar-a-year" man for the Office of War Information and was instrumental in setting up its survey operations. An important contact made during this period was Henry Ford II whom he served briefly as an unpaid consultant. A strong internationalist, Roper joined the Atlantic Union after the war and campaigned vigorously in favor of the Senate resolution that sent a lay group to the 1951 North Atlantic Treaty Organization conference.

In all of his associations with those in decision-making positions, Roper endorsed the application of survey research in the search for solutions to problems of public and business policy. Thus his association with Ford led to his membership on the Fund for the Republic's board of directors, where he was instrumental in obtaining funding for Samuel A. Stouffer's monumental study of the McCarthy era, *Communism, Conformity, and Civil Liberties* (1955). Similarly, his interest in population problems led to his active promotion of survey research in this area.

Roper's personality was perhaps the greatest source of his influence. His dedication to principle persuaded others of his integrity whether or not they agreed with him. Friendly, gregarious, an effective "soft-sell" salesman, he could also be an aggressive critic of those who he felt had in some way violated professional standards of performance and ethics—in marketing as well as public opinion research. This concern engaged him in bitter controversy and on occasion disrupted amicable and even profitable associations. While a director of the Fund for the Republic, Roper voted with the other members of the board to retain Robert M. Hutchins as president of the fund, in direct opposition to Henry Ford II. This move led to a personal break with Ford and the eventual loss of what had been a lucrative business relationship between Roper's research firm and the Ford Motor Company. Another instance was his criticism of Louis

Harris' role in the 1960 presidential primaries, which culminated in a heated exchange at the 1960 annual conference of the American Association for Public Opinion Research (AAPOR). In the commercial field, Roper presented a dramatic criticism of what he felt were the excesses of motivation research, this time at the 1956 annual conference of AAPOR.

Roper did not believe that the role of the researcher was that of uninvolved observer and analyst. He felt that his role as researcher would be most effectively implemented if he was involved in the policy-making process itself. This posture did not reflect a desire to bias or influence results but stemmed rather from his feeling that the effective use of research was contingent upon the researcher's involvement in the policy-making process. He sought out people who were involved and interested in the important issues facing society and business. He took people seriously and felt a responsibility for solving client problems not by conducting "interesting" research, but by providing them with straightforward answers and explanations. Not a methodologist or theoretician himself, he was eager for the guidance of sophisticated professionals. Most significantly, Roper was a highly effective salesman for the application of survey research in decision making at the highest policy levels.

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ROSE, ARNOLD M.

Arnold Marshall Rose (1917-1968), president elect of the American Sociological Association and professor of sociology at the University of Minnesota at the time of his death, was born in Chicago on July 2, 1917, to Frank A. and Ruth (Wilamsky) Rose, pharmacists. At the University of Chicago, he earned his A.B. in sociology (1938), his A.B. in economics (1939), and his M.A. (1940) and Ph.D. (1946) in sociology.

As a graduate student assistant to Samuel A. Stouffer, Rose was employed on the Carnegie-Myrdal study of the American Negro as well as on the War Department study of the American soldier. Thus, even before he earned his Ph.D. Rose had been one of the coauthors of an outstanding study of America's foremost social problem, and had gained experience on the most famous research project of the immediate post-war period. In December 1942 he married Caroline Baer, a Chicago graduate student, who had also been employed on the Myrdal project.

The shaping influences on Arnold Rose were Herbert Blumer, under whom he prepared his master's thesis and who supplied his symbolic interactionist perspective; Samuel Stouffer, who provided his basic research experience in the use of field data gathered by questionnaire; Gunnar Myrdal, who instilled in him the habit of exploring social issues by the survey of major authorities and the collection and collation of published facts; and Louis Wirth, his doctoral sponsor, who inspired an enduring interest in power and politics.

Rose established his lifetime pattern of research, writing, and teaching early in his career. After teaching at Bennington College for one year, he accepted an appointment as associate professor of sociology at Washington University in St. Louis. With a grant from the American Jewish Committee, he researched *Studies in Reduction of Prejudice* (1947). The following year

he published *The Negro in America* (1948), a popular condensation of *An American Dilemma* (Myrdal 1944), and with Caroline Rose, *America Divided* (Rose & Rose 1948), in which they applied the skills they had acquired on the Myrdal project to the problems of all minorities. After joining the faculty of the University of Minnesota in 1949, he continued his research into prejudice and discrimination, publishing *The Negro's Morale* (1949), *The Roots of Prejudice* (1951b), and a book of readings, *Race, Prejudice and Discrimination* (1951a). He spent 1951 in France on a Fulbright research scholarship.

Arnold Rose was catapulted to prominence by *An American Dilemma*, the single most important book of his career. Nine of his other books developed directly from it, either by continuing exploration of its themes or by applying its method to closely related problems. In time, he applied the methods he learned as a member of Myrdal's team to other social issues.

By 1957, at the age of 40, Rose had reached the fullness of his powers, and though he continued to publish at the same rate as previously, he decided to enter active politics. Rose's role in the Myrdal study was a major factor in his successful election to the Minnesota legislature for a two-year term beginning in 1962. As a result of attacks upon him by an extremist group for his role in *An American Dilemma*, he filed a libel suit in 1964, which he won in district court but lost on appeal to the Minnesota supreme court (Rose 1968a). He died of cancer on January 2, 1968.

As Myrdal understood the assignment from the Carnegie Corporation, it was to provide a status report on the black minority in the United States. He corresponded with and interviewed hundreds of authorities on all aspects of black life, commissioned dozens of special monographs, and with his assistants, including Rose, undertook the integration of all published and especially collected material. Myrdal then confronted the American liberal ethos, a combination of moralism and rationalism that was largely a cultural product of its Protestant heritage, with the social, economic, political, and educational realities of American Negro life. The American liberal had either to face up to the public policy implications or remain publicly convicted of hypocrisy.

An American Dilemma was a challenge to liberals and opened a new era in American minority relations. Hubert H. Humphrey launched

a career in national politics in 1949 on a platform supporting civil rights for blacks, partly in response to the challenge of the book. In 1954 the Warren Court cited the book in a footnote to its landmark decision in *Brown v. Board of Education of Topeka* (347 U.S. 483). It is hardly surprising that Rose's association with the book opened all doors to him.

The character and quality of Rose's personal contribution to the understanding of race, minorities, and prejudice may be seen in his conclusions to *The Negro's Morale* and *The Roots of Prejudice*. He claimed that a growing group identification with accompanying self-consciousness among blacks had increased their self-confidence and aided in their development of effective protest organizations. They had become aware of events that affected them both at home and abroad, had learned to make their vote count, and had stated their intention to demand their full rights in a democracy (1949, pp. 141-145). Prejudice, Rose maintained, harms its supporters financially and psychologically. Its influence may be reduced by the dissemination of accurate information, by the elimination of racial stereotypes, by legislation to prohibit discrimination, by education, by the solution to major social problems, by the demonstration that fears of minorities are groundless, and by the cultivation of a healthy personality (1951*b*, pp. 39-41).

In the 1950s Rose branched out and applied similar methods—summarizing current knowledge and presenting it for consideration to the liberal conscience—to the problems of the mentally ill and the elderly. As in the case of the blacks, he was one of the first to bring these issues to the attention of researchers and policy makers.

In the 1960s, he studied power and politics, areas of interest that dated back to his work under Louis Wirth. In *The Power Structure* (1967) Rose challenged the contention by C. Wright Mills (1956) and Floyd Hunter (1953; 1959) that power in America is elite dominated, with a view similar to that of political scientists Robert A. Dahl and Nelson Polsby that power in the United States is diffuse, multi-dimensional, and more political than economic in character. Again Rose had adopted the central liberal position.

While Rose did not contribute to or transform a single major idea in contemporary sociology, he became the most eminent sociologist at the University of Minnesota, president of the So-

ciety for Social Problems (1956), president of the Midwest Sociological Society (1962), and president elect of the American Sociological Association (1968). He was interested in problems and issues, not in theories or methodologies. He was a liberal with faith in principles rather than in people. Although he was preoccupied with the new, he studiously avoided the avant-garde. In social issues, always his primary concern, Rose quickly found the conventional liberal center. He saw in legislation and education the source of orderly progress, was an unwavering apostle of the welfare state, and looked to the law for a resolution of all conflicts. He was the paradigm of the liberal social scientist who found his primary mission in the organization of known facts on social issues for policy consideration and the identification of spheres where further research may be needed.

During his academic life Arnold Rose advised or coadvised 16 people through the doctorate. His bibliography comprises more than 20 books and 150 articles. The ultimate idealist, he placed public service even above family and gave the bulk of his estate for the establishment of the Arnold and Caroline Rose Fund for the Publication of Scholarly Monographs.

DON MARTINDALE

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ROSENSTEIN-RODAN, PAUL N.

Paul N. Rosenstein-Rodan, who was born in 1902, helped found and became one of the leaders of the field of development economics that emerged at the end of World War II. He began his professional career in 1925, working on the frontiers of the Austrian theory of consumer demand. However, starting in the early 1940s, Rosenstein-Rodan concentrated on economic development issues. The change also

marked a shift from pure economic theory to applied economics. The series of hypotheses that he later offered to characterize growth problems in developing countries contrasted sharply with the stylized economic facts conventionally accepted, and led development analysis in new directions. He became one of the leading advisers on economic policy toward and within developing countries, and in that role dealt increasingly with financial and international development assistance issues. Yet in many of his contributions to the development literature, it is possible to trace the working out of several fertile concepts that were among his first pre-occupations in economics: complementarity in consumption and production, the time sequence of economic adjustments, economies of scale in production, and economic planning.

Rosenstein-Rodan began his studies in physical chemistry at the University of Lausanne, but returned to the University of Vienna to study economics. His early training in a physical science gave him a familiarity with mathematics and an analytic approach that was then rare among economists. While earning his doctorate in economics at the University of Vienna, he became one of the protégés of Hans Mayer, the resident heir to Friedrich von Wieser as a leader of the Austrian school. As the practical director of the project that produced the massive four-volume survey of the status of current economic analysis, *Wirtschaftstheorie der Gegenwart*, Rosenstein-Rodan acquired the wide knowledge of the history of economic thought that became a hallmark of his career.

Understandably following the interests of his teachers, Rosenstein-Rodan's attention turned first to the theory of consumer demand and price determination. His first article, "Marginal Utility" (1927), was a comprehensive, analytical review of consumer utility and demand theory. The article devoted particular attention to complementary relationships among goods demanded by consumers and was further elaborated in "La Complementarietà" (1933). Such goods are "cooperating" in the sense that a decrease in the price of one good will shift the entire demand schedules of its complements, tending to increase quantities demanded at any price.

Rosenstein-Rodan's early work also showed the influence of Philip H. Wicksteed on the Austrian school, especially Wicksteed's stress on understanding economic phenomena as processes occurring over time rather than as static

configurations. Following ideas raised by this line of thought, Rosenstein-Rodan in 1929 published "Das Zeitmoment in der Mathematischen Theorie des Wirtschaftlichen Gleichgewichtes." This was elaborated in "The Role of Time in Economic Theory," after Rosenstein-Rodan moved to the University College of the University of London in 1934. Although the argument is somewhat sketchy and the conditions for alternative possible patterns of behavior are not worked out, the article lends support to the conjecture by Lionel Robbins that Rosenstein-Rodan was the first to develop the theory of cobweb price-quantity adjustments over time. In this theory, prices in one period determine the quantity supplied in the subsequent period, and those quantities, with demand, determine prices at a level that will clear the amount supplied in the market. Rosenstein-Rodan himself credits a number of predecessors for independently formulating the theory and describing the conditions for stable or unstable price-quantity behavior.

On his arrival in England in 1931, after a period as a Rockefeller fellow in Italy, Rosenstein-Rodan was exposed to the intense discussion of the nature and significance of economies of scale and external effects. These concepts, which were subsequently to become central elements in the intellectual structure with which Rosenstein-Rodan approached development economic issues, had been the central focus of a famous speech and article by Allyn Young shortly after he took a chair in 1928 at the London School of Economics. The first concept associates reductions in unit costs with increasing levels of production. The latter related changes in revenues or costs in a particular firm with activities outside the firm whose influences are not fully transmitted through conventional market channels.

A further element, one that appeared in Rosenstein-Rodan's first article, was submerged for some time and then reappeared often in the context of development policy. It is contained in his early sentence, "An economic plan aims at the selection of the most suitable allocation of goods. Since the allocations of all goods are closely linked by the latter's psychological and technical complementarity the process of selection must be unitary and systematic" ([1927] 1960, p. 76). Although this sentence was written with respect to the formulation of consumption decisions by an individual, it has a natural extension to planning on a national scale; read in that way, it could have appeared in any of

Rosenstein-Rodan's writings on development policy after 1940. The reasoning is much like that expressed by Irving Fisher in *The Rate of Interest* (1907), a classic well known to the Austrian school.

The concepts of complementarity, economies of scale, externalities, and planning were brought together in the seminal article on economic development, "Problems of Industrialization of Eastern and South-eastern Europe" (1943), written during World War II, when Rosenstein-Rodan was a member of the Economic Group of the Committee on Reconstruction of the Royal Institute of Economic Affairs. He argued that development requires a program that, by encompassing the entire economy, would satisfy the requirements of complementarity in consumption and production; the broad range of outputs generated would guarantee that the production of any single type of commodity would be met by consumption and/or production demands created in other sectors of the economy. According to Rosenstein-Rodan, the economy-wide growth of output would also generate positive externalities through the reduction of the risk and uncertainty of individual producers. However, Rosenstein-Rodan argued that development programs, rather than proceeding in small, though widespread, incremental steps, should be on a scale large enough to benefit from the lower costs that accompany higher rates of production. Finally, it was deemed necessary to have a minimum amount of over-all development planning for coordination of investment programs in order to achieve the benefits of complementarities, external economies, and economies of scale.

In 1947 Rosenstein-Rodan moved from his position as head of the department of political economy at the University College of the University of London, to the International Bank for Reconstruction and Development (IBRD), where he became the assistant director of the economics department and head of the economic advisory staff. While at the bank, he concentrated first on the economic issues in the reconstruction of the war-damaged European economies and then, increasingly, on the problems of developing countries. Again, his emphasis on the complementarities in consumption and production was evident in his advocacy within the bank of "program" loans, rather than restriction of bank lending to "project" loans. The former support over-all development plans and projects that are conceived within such plans, while the latter

rest only on the viability of the individual undertakings.

Rosenstein-Rodan's move in 1952 to the Massachusetts Institute of Technology (M.I.T.) gave him the opportunity to consider development issues from an even broader viewpoint. He formulated more explicitly, in the theory of the "Big Push," the argument that development success required an effort large enough to achieve the advantages of both economies of scale and the requirements of complementarity. He also further elaborated the concept of "disguised unemployment," the absorption of labor in activities with low, even negligible productivity that, however, would hide or disguise the lack of opportunities for productive employment.

Rosenstein-Rodan's research, even after leaving the IBRD, was tied closely to development policy. In his role as director of the Italy Project of the M.I.T. Center for International Studies, he contributed to the analysis that formed the basis for the Vanoni Plan, prepared in 1955 to guide Italy's development for the next ten years. As director of projects in India and Chile, he similarly concentrated on the basic macroeconomic forecasting and accounting of the aggregate demand for and supply of investible resources. In these activities Rosenstein-Rodan played the role of informal adviser as well as independent research director. His article "Alternative Numerical Models of the Third Five Year Plan of India" (1964) reflects this dual position well. It formulated the essential macroeconomic balance equations that formed the rationale for the Third Plan and drew on his extensive experience to evaluate the plan's feasibility. The analysis in the article helped to provide the basis for the intensive debate waged over the Third Plan.

In 1961 Rosenstein-Rodan developed a new estimate of the amount of foreign economic assistance that could be used with reasonable levels of productivity in developing countries. He reviewed the position and potential performance of each of the developing countries, taking into account their "absorptive capacity"—that is, their ability to use capital effectively—as well as the marginal productivity of capital and the growth of domestic savings with income. The new estimate was within the range of the assistance that might plausibly be forthcoming and, as a result, helped transform foreign aid policy from a somewhat hopeless humanitarian gesture to a realistic means of ameliorating poverty in developing countries.

Rosenstein-Rodan also contributed more directly to economic policy formation in developing countries by serving on a number of national and international bodies concerned with these issues, such as the panel of experts, the "Nine Wise Men," of the Alliance for Progress from 1961 to 1966. After retiring from M.I.T. he moved to the University of Texas in 1968 and then in 1972 to Boston University, where he founded the Center for Latin American Development Studies and continued his work in development policy.

RICHARD S. ECKAUS

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RUSSELL, BERTRAND

Bertrand Arthur William Russell (1872–1970), British philosopher, mathematical logician, social theorist, and social activist, has exerted an enormous influence on philosophers of the twentieth century. From 1890 to 1894 Russell studied mathematics and philosophy at Trinity College, Cambridge. Henry Sidgwick, James Ward, and G. F. Stout were his philosophy teachers. Russell was a fellow of Trinity College from 1895 to 1901 and lecturer in philosophy from 1910 to 1916. Because of his opposition to World War I, he was dismissed from Trinity College and later served a term in jail. During the last years of his life, Russell was an energetic opponent of United States involvement in Vietnam. From 1916 to 1938, when Russell held no academic post, he wrote and lectured extensively.

Influenced by J. M. E. McTaggart, who for a time was a close friend of Russell, and F. H. Bradley, an Oxford University philosopher whose work he came to know through Stout, Russell started out more or less as a disciple of the British version of Hegelian idealism. Under Ward's guidance Russell learned to respect Kant as well, but he was soon led away from absolute idealism and Kant by his fellow student, G. E. Moore. For a time, Russell shared Moore's Platonism with respect to universals and sympathized with Moore's realism and reliance on common sense regarding the existence and properties of the external world. Russell's little classic, *The Problems of Philosophy* (1912), and the much longer, *Principles of Mathematics* (1903), were written during Russell's Platonistic period.

Russell's own distinctive philosophical style may be called "logical constructionism." Logical constructionism has an epistemological and a metaphysical application. The fundamental principle of Russell's logical constructionism is: "Whenever possible, substitute constructions out of known entities for inferences to unknown entities." Logical constructions are Russellian philosophical analyses. The epistemological application of the principle constitutes Russell's philosophy of science; its metaphysical application yields Russell's version of logical atomism, which differs in important ways from the logical atomism in Ludwig Wittgenstein's *Tractatus logico-philosophicus* (1921).

According to Russell, epistemological problems arise whenever we find we do not have adequate justification for believing what we

believe with respect to a certain domain of entities. We may be unclear about the nature of the entities involved, and we may not be confident that they exist. The aim of the epistemologist is to construct this body of knowledge in terms of relations among entities that are simpler, less questionable, and less puzzling. The body of putative knowledge can thereby be better understood and justified.

In order to see Russell's epistemological application of the principle of logical constructionism in its proper setting, it is necessary to note, first, that for Russell, as for a number of his illustrious predecessors, such as Descartes, Locke, Berkeley, Hume, and Kant, the fundamental problems of epistemology are to be located in what either science or common sense suppose to be true. For philosophers of this persuasion, epistemology is the critique of science and common sense. As to science, and this is the second point to keep in mind, there is, on the one hand, science as a body of putative knowledge, and, on the other hand, science as scientific method, i.e., a congeries of procedures, presuppositions, assumptions, reasonings upon which science as a body of putative knowledge rests. Until 1948 Russell's epistemology almost exclusively addressed itself to logically reconstructing science as a body of knowledge. In 1948 Russell published *Human Knowledge: Its Scope and Limits*. The principal task of that book is to examine the method of science, and most importantly to look into the problem of the nature and justification of nondemonstrative inference, e.g., induction.

The first application of the technique of logical constructionism is in *Principia Mathematica* (1910–1913) which Russell coauthored with Whitehead. This landmark of mathematical logic attempts to show how all of the concepts of pure mathematics are "reducible to," "analyzable into," "constructible from," a very few logical concepts. Logic, as conceived in *Principia Mathematica*, is that realm of discourse the basic vocabulary of which can be reduced to three locutions: "all," "neither–nor," and "is a member of." These three expressions are sufficient for defining every expression that counts as a logical expression and also for defining every expression that belongs to pure mathematics. Assuming that the basic concepts of logic are clearer, less puzzling, and less problematic than those of pure mathematics, the construction of mathematics from logic is an instance of what Russell would view as a philo-

sophical step in the right direction. Thus, number is defined as a class of classes. The number one is the class of all classes each of which is such that if x and y are members of it, then x and y are identical. Zero is the class that has no members. There may be philosophical progress here, even if we are not entirely comfortable with classes as entities. Russell himself was somewhat uneasy about them. In *Principia Mathematica* classes in turn are constructed from propositional functions, and these seem to be themselves just as abstract as classes, and perhaps just as puzzling. Nonetheless, progress has been made provided that misgivings about abstract entities, be they classes or propositional functions, are less strong than confusions about such things as numbers.

Next Russell applied the technique of logical constructions to scientific and common sense notions about physical objects. He attempted to construct such theoretical scientific entities as protons, quanta, mass, energy, and such common sense entities as sticks and stones, from sense data of which we are supposed to be immediately aware, so that there should be no question about their existence and properties. Russell attempted a similar reduction to sense data of mental entities.

The metaphysical application of the principle of logical constructionism resulted in logical atomism. As a metaphysical thesis this is essentially the claim that all of the ultimate factual constituents of reality are expressible in atomic sentences and their truth-functional compounds. An atomic sentence is exemplified by: 'This is red.' A truth-functional compound of two atomic sentences would be: 'This is red and that is blue.' Russell's methodological commitment to logical atomism did not prevent him from seeing its shortcomings very early on.

When he came to the problem of understanding the nature and justifiability of nondemonstrative inference characteristic of the natural sciences, Russell declared the inadequacy of pure empiricism. His contribution consists of a list of postulates that he does not try to defend on empirical grounds alone. These postulates entail a restricted form of the principle of induction. The principle of induction itself may be stated as follows: On the evidence that a number of instances of **A**s have been **B**s, it is probable that the next instance of **A** will be a **B** or that all **A**s are **B**s. Russell's postulates are meant to provide limitations on being **A** and being **B** so as to make the principle true.

Russell's work in metaphysics and epistemol-

ogy is controversial. His great influence on many of the most important philosophers of the twentieth century is the result of the profoundly stimulating way in which he conceived of philosophical problems, the ingenuity and technical virtuosity that he exercised, and the clarity with which he spoke and wrote.

Russell was also seriously interested in pedagogy and social theory. In 1926, he published *On Education, Especially in Early Childhood*, and in 1927, with the help of his second wife, Dora, he established a school for twenty children, including two of his own. Its purpose was to strike a balance between undue restraints on the freedom of the children and complete absence of discipline, to provide instruction in substantive subject matter, and to avoid religious instruction and prudery. Russell was disappointed with this experiment.

Russell's social theory is in the liberal tradition of John Stuart Mill, but with differences. Russell believes that economic and political reform are sufficient for improving human relationships. He does not agree with Marx that economic arrangements determine the political order. Russell's liberalism is a theory about means and ends. The end is to achieve "some new system of society by which life may become richer, more full of joy and less full of preventable evils than it is at present." This quotation from *Proposed Roads to Freedom* ([1918] 1919, p. viii) is a representative statement. The means is a social organization that performs these functions efficiently. Such a society must be socialist in its economy, for only thus can the possessive (bad) impulses of man be curbed and the creative (good) ones released. Here Russell differs from Mill, who considered socialism with care but did not adopt it. Russell agrees with Mill that the politics of the good society must be democratic.

In *The Practice and Theory of Bolshevism* (1920), Russell predicted in detail the repressive and reactionary features of the Soviet system that developed under Stalin, and this at a time when hardly anyone in the West had even heard of Stalin. In 1929 Russell published *Marriage and Morals*, in which he defended all of the freedoms about sexual customs that liberals take for granted nowadays. For his outspoken good sense in these matters Russell was vilified publicly and denied appointment at the City College of New York. He was elected a member of the Royal Society in 1908, and in 1950 he was awarded the Nobel prize, nominally in literature, actually in well-deserved recognition of

his philosophical contributions. They are part of the permanent record of achievement in Western philosophy.

GEORGE NAKHNIKIAN

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RYLE, GILBERT

Gilbert Ryle (1900–1976) was Waynflete professor of metaphysical philosophy in Oxford from 1945 to 1968 and editor of the British

philosophical periodical *Mind* from 1948 to 1971. He was probably the most conspicuous, fertile, and influential British philosopher during a period in which the status and quality of philosophy in Britain, and particularly in Oxford, were extremely high; and in earlier years he had played a crucially important part in the reanimation, particularly in Oxford, of the philosophical scene.

Ryle was born in Brighton, the son of a doctor and the grandson of a bishop, and educated at Brighton College. In 1919 he went to Queen's College, Oxford, where he read successively "greats" and "modern greats"; in 1924 he became a lecturer in philosophy at Christ Church and remained at that college (with a period of absence on military service during World War II) until he moved to Magdalen on election to the Waynflete professorship.

In his early years, as Ryle put it himself (Ryle 1971) "the philosophic kettle in Oxford was barely lukewarm." Though philosophers there were numerous, they did not very actively dispute even with one another and were scarcely at all interested in their contemporaries elsewhere. There were idealists in the tradition of F. H. Bradley, realists in the tradition of J. Cook Wilson, and some good classical scholars, but there was little sense of living research or controversy. Ryle felt that the problem was parochialism; and this he set himself, then and always, deliberately to overcome. As a student he took seriously, as his teachers did not, the work of Bertrand Russell; and from there, teaching himself German on the way, he moved to the study of Alexius Meinong, Franz Brentano, Bernard Bolzano, and Gottlob Frege—and also Ludwig Wittgenstein, whom he met for the first time in 1929. By that date he was preoccupied chiefly with the question of what philosophy itself is. If it was, as he felt and found it to be, a living subject, and not merely (as his teachers had made it appear) the respectful, scholarly study of other people's texts, what were its problems? What was it about? What would be appropriate, distinctively philosophical methods?

His first thought was that philosophy investigates the meanings of expressions. This definition seemed close to the idea made current at that time, principally by the work of G. E. Moore and afterwards by the logical positivists, that the proper business of philosophy was "analysis." But further questions arose. If philosophy's concern is with the meanings of expressions, is it just lexicography? In any case, as Moore himself insisted, when philosophical problems are

stated, are not the meanings of the expressions often, even usually, perfectly well known? What told a philosopher *which* expressions are in need of his investigations, and why? Quite soon Ryle came to the conclusion, which substantially he never abandoned, that the business of a philosopher was not directly with meanings but rather with a certain kind of meaninglessness; not with what expressions in general mean, but with why certain expressions make no sense. Thus the earliest paper that is fully characteristic of his work is "Systematically Misleading Expressions" (1932, in Ryle 1971, vol. 2, pp. 39–62). The thesis of that paper is that certain forms of expression, often quite ordinary ones, are "improper" to the states of affairs they actually record; that they thereby invite misassimilation to other expressions from which they in fact radically differ; and that such misassimilation tends to generate perplexity, or even flat nonsense, from which it is the business of philosophical argument to rescue us.

Shortly thereafter, notably in his paper "Categories" (1938, in Ryle 1971, vol. 2, pp. 170–184), Ryle abandoned the rather obscure notion of an expression's being "improper" to a state of affairs. His reformulated thesis was that expressions can be grouped into "types" or "categories," and that philosophical perplexity arises from handling, or attempting to handle, an expression of one category as if it belonged to another. In this view the characteristic genesis of philosophical puzzlement is a "category-mistake"; and the curative business of philosophy is to exhibit and correct categorial misassignments, it being the distinctive mark of such misassignment that it results in a "certain kind" of meaninglessness—or, as Ryle often also put it, of "absurdity." This reintroduction of the ancient notion of categories gave Ryle a good deal of trouble—he never really produced a satisfactory general account of just what a category, or derivatively a category-mistake, was supposed to be. He was always ready, however, to illustrate his general doctrine with particular examples, and indeed explicitly defended his talk of categories as late as 1971 (Magee 1971, pp. 108–109).

Ryle's most famous and most impressive work is unquestionably *The Concept of Mind* (1949). It is interesting that this immensely influential book had its origin, not in a particular interest in its topic, but in the general theory of philosophy that it was meant to illustrate. Ryle said that having debated for a decade or so what a

philosophical problem was and what philosophical argument should be, he felt it was time to put his views to actual work in the large-scale handling of some large philosophical problem. It was time to exhibit in practice a good, big "category-mistake" that actually issued in recognizable, preferably traditional and familiar, philosophical quandaries. He first thought that the problem of free will would best answer his purpose, and it was only after he had found that unsatisfactory that he hit upon mind-and-body problems instead. His readers and critics have for the most part reversed this emphasis. His general theory of mind, and even more his accounts of particular mental concepts, have proved both fascinating and fertile, while his general theory of philosophy, never very clear and perhaps not particularly important, has been not much regarded, and has often been regarded with disfavor.

The Concept of Mind, though certainly impressive, is not a wholly coherent book. In it at least two distinct theses are sporadically at war. The milder thesis is that the very many ways in which we speak, in broad terms, about "the mind" are potentially misleading; that philosophers, notably those Ryle calls "the Cartesians," have constantly been misled; and that they have been misled in particular into representing the mind, mental states, and happenings on the model of the body, physical states, and happenings—as if, the body being a physical thing existing and acting in the physical world, the mind were a nonphysical thing performing nonphysical acts, but in some mysterious way "in" a physical object, the body. There constantly obtrudes, however, a cruder, more extreme, apparently ontological thesis—that, contrary to what our ordinary ways of speaking would suggest, there *really are* only physical objects and physical happenings, and that all talk seemingly "about" minds and mental happenings is really no more than a certain way of talking about bodies. Ryle often denied, and his critics often asserted, that his book preached "behaviorism"; the fact is that it both did and did not, in different places.

Ryle's concern with the question of what philosophy is, and indeed his answers to that question, have a clear kinship with the work of his slightly older contemporary Wittgenstein, by whom he must have been influenced, even though it was only after Wittgenstein's death in 1951 that most of his work became accessible to the public. But between the two men there

were vast and vital differences. Ryle, as an effective and stimulating teacher, liked students and an audience (an early student was A. J. Ayer); but he did not want disciples, and particularly not a tiny circle of overreverent disciples. He was by temperament and on principle a philosophical expansionist and was chiefly responsible—through his writings, his travels, his editorship of *Mind*, his energetic activity as professor, and his immense friendliness as a person—for making Oxford in his time both the largest and the liveliest philosophical center in the world. Further, though he had made philosophy come alive in Oxford, he never had, or gave, the impression that it had never been alive anywhere else; he wrote a book on Plato (1966), and more than twenty papers from time to time on other philosophers, whom he saw no reason to disregard or undervalue simply because they had antedated his own appearance. He was occasionally regarded, perhaps not without a touch of jealousy, as a man of one book, *The Concept of Mind*; but that dismissive suggestion could not well survive the publication, in 1971, of his *Collected Papers*, 57 articles (to which he

afterwards added a few more) over a period of 50 years, which leave few areas of philosophy untouched and unenlivened.

G. J. WARNOCK

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SAUVY, ALFRED

Alfred Sauvy was born on October 31, 1898, in Villeneuve-de-la-Rabo, a village in the extreme south of France. He was the second eldest of a family of seven children, whose parents were wine producers. After leaving secondary school in Paris, Sauvy was called up to serve in the army during World War I. In the last year of the war, he was wounded at the battlefield. In 1932, he married Marthe Lambert.

In 1920, Sauvy was admitted to the École Polytechnique, and in 1922, he started upon a career as a statistician at the Central Statistical Office of France, known as La Statistique Générale. At that time, the national statistical offices were not as important as they have since become. Their activities were confined to census taking and to the tabulation of vital events. Data collection on production, foreign trade, prices, employment, and so on, was almost nonexistent, and the international statistical yearbooks were not yet begun. In 1929, the resignation of a statistician working on short-term forecasts provided a vacant post for Sauvy—one that he soon turned into a remarkable source of information.

In this new position, Sauvy was asked for information on price indexes by an assistant to a prominent French political figure, Paul Reynaud. Sauvy was amazed that a leader in French politics should be so poorly informed on such a basic subject as price indexes. "From that day," Sauvy later wrote, "the objective of all my life was settled: enlightening action" (1972, p. 12). This encounter prompted Reynaud's interest in Sauvy,

and their meeting marked the beginning of a long collaboration.

In March 1937, the Ministry of National Economy was created as a separate agency from the Ministry of Finance, and Sauvy was drafted to work for it part time. The creation of the new ministry was itself an important event because it marked the first recognition that a national economy could be run by people other than financiers and accountants. But the new ministry was, in fact, established without any real power, and Sauvy began his work without any precise assignment. He decided to translate into everyday language the tables published weekly and monthly by La Statistique Générale, and issued a small bulletin that had an immediate and spectacular success. "For me," wrote Sauvy, "the astonishment was great. I understood that it was necessary to speak to the people in their own words" (*ibid.*, p. 47). This concern with popular enlightenment is characteristic of Sauvy and is his original contribution to demography as a social science.

The new ministry lasted only a few months, and disappeared, along with most of its workers. Sauvy remained at the head of a small group of people who were in charge of monitoring the French economy. This Service d'Observation Économique was the first step toward the creation of a real institute, the Institut de Conjoncture.

In November, 1938, just after the Munich agreement, Paul Reynaud was nominated Minister of Finance in a government that had full power, for only a short period of time, from the

parliamentary assemblies to enact new decrees. Reynaud instructed Sauvy to prepare, in one week, a series of decrees aimed at transforming the economy of France. The main objectives were to reduce unemployment and increase production. Sauvy was convinced that the only way to accomplish these aims was to increase the work week, which was fixed by law at forty hours. Such a measure seemed to contradict common sense, but the decree passed in spite of strong protests from people who predicted disaster for the French economy. The following months, however, confirmed Sauvy's views. From then on, he concluded that a population increase could only be beneficial, and he tried to convey that idea to the rest of the world. Among Sauvy's other decrees was one that would be of major importance for the future of France: the decree that institutionalized family allowances and thus helped spur the baby boom that followed World War II. Another important decree created the Institut de Conjoncture, with Sauvy as director.

The work Sauvy had originally envisaged for the institute was interrupted by World War II. As director of the newly created institute, Sauvy worked in 1939/1940 with Jean Monnet, the chief of the commission in charge of economic cooperation between the United Kingdom and France. Then came the defeat and occupation of France by the Germans. During that period, Sauvy took the initiative in carrying out a survey to estimate the cost of the German occupation. He succeeded in organizing a network of information with foreign countries, and rapidly became one of the best informed people in France on the trend of the war. He started writing a bulletin that evaded French and German censure and that was sent confidentially to important figures in France. Perhaps even the Germans found news in the bulletin that official papers did not provide. At the same time, Sauvy continued to develop his theory on population problems and wrote two important works for population specialists (1943; 1945). For the first time, the concept of an optimum population was clearly defined, and the interrelationships between population trends and economic and social development were clarified. That field, familiar today, was almost totally unexplored at that time.

In April 1945, after the war, General Charles de Gaulle appointed Sauvy as Secretary General for Family and Population. Sauvy, however, soon preferred to return to his research, and in December 1945 he created the Institut National

d'Études Démographiques (INED), a multidisciplinary agency that rapidly became, under his direction, a leading research institution in France and abroad. The new institute issued a quarterly review, *Population*, for which Sauvy wrote an editorial on world population trends. The subject was treated in simple terms from many perspectives, and *Population* became, in fact, a platform for discussions of economic, social, and cultural development and even of environmental issues. From 1945 to 1962, Sauvy was director of INED, and he continued to be editor in chief of *Population* until 1975.

With all his research activity, Sauvy continued his interest in government action. In 1945 he became a member of the Haut Comité de la Population, chaired by de Gaulle. In 1947 he was nominated a member of the French Economic and Social Council, the government advisory agency. He was, until 1974, a member of this assembly and was for many years chairman of the Committee for Planning and Conjunctural Studies. In 1957 he created the Institut de Démographie de l'Université de Paris (IDUP), a teaching institute preparing postgraduate students for careers in demography. His research had convinced him that the increasing average age of the population was the most insidious trend in the developed world, and he came to feel that it was only by involving the younger generation in national leadership that the difficulties created by the aging process could be solved. The creation of IDUP was the first action taken along these lines; it was followed two years later by the publication of Sauvy's book, *La montée des jeunes* (1959).

In 1959 he was also coopted by his peers to become a professor at the Collège de France, where until 1969 he occupied the chair of social demography. In this capacity, he was named professor emeritus.

In the international scene, Sauvy became a member in 1946 of the newly created Nuclear Commission of Statistics of the United Nations. A year later he was chosen by the French government to represent France at the UN Population Commission, of which he was chairman from 1951 to 1953; he still represents France on that body. Through his contributions to the debates of the commission, he became a well-known expert in international circles. In 1952 he created the expression the "Third World" to designate developing countries.

In addition to his active public life, Sauvy found time for other activities, including the

restoration of a medieval village and old churches in his native region. Before World War II, he was a film critic and even a playwright, with Jacques Tati.

Throughout his life, Sauvy has been particularly interested in multidisciplinary studies and has always given preference to observation and experiment over theoretical considerations. He has made great efforts to disseminate demographic and economic knowledge among laymen. His emphasis on observation and experience has kept him from becoming involved in the creation of ideological or political schemes. In his writing, he has shed new light on his subjects without formulating imperative conclusions. He uses technical formula only rarely, and when absolutely necessary, to specify ideas more precisely. In such cases, he provides commentaries that attenuate a possibly forbidding mathematical approach.

Sauvy feels that democracy does not yet exist, or more precisely, that it exists only in the political framework, since the so-called sovereign masses know so little about their own affairs. However, he believes that any sociopolitical problem has a chance of solution as long as facts are widely disseminated.

JEAN BOURGEOIS-PICHAT

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SAVAGE, LEONARD JIMMIE

Leonard Jimmie Savage (1917–1971) was, at the time of his death just before his 54th birthday, a leader of the personalistic school of probability, a school that regards probability as the means by which a person expresses his uncertainty about the world as he sees it. He was particularly interested in the use of the personalistic view in both the theory and application of statistics.

His congenitally poor eyesight affected his early education, and it was not until he reached college that his high mathematical abilities were recognized. From then on all of his work was in mathematics, if we interpret that word in its broadest sense; for Savage was interested in everything and contributed to more fields than his published work suggests. He had the habit of taking a statement that most would regard as intelligible and straightforward and dissecting it to clarify the problems it hid or determine its precise meaning. This habit infuriated some, but most welcomed it as an enormous aid to their understanding. Moreover, his dissections did not lead to heterogeneous complexity but pushed forward to produce a synthesis that was often so overwhelmingly simple and obvious that one wondered how it could have been overlooked. These comments apply not only to his little paper on glottochronology (Dobson et al. 1972), but to his major interest, personal probability, which is essentially very simple but embraces a world of thought.

His statistical life began when he joined the Statistical Research Group at Columbia University in 1943 under the direction of W. Allen Wallis. There he met many people who subsequently became famous. Indeed it was unusual for a member of the SRG not to become well-known. In Savage's own words, it was "one of the greatest hot-beds statistics has ever had." Among his colleagues were Milton Friedman

and Abraham Wald. Friedman was influential in Savage's development, acting as a stimulus, forever questioning and encouraging new thoughts, discussing probability and utility theory with Savage. Friedman also had a beneficial effect on Savage's writing style; Savage's later writings have a lucidity unusual in contemporary science. They also coauthored a seminal paper on utility theory (1948) that has often been reprinted. Another important influence was John von Neumann, with whom Savage had worked before coming to the SRG. Von Neumann had just completed his work on the axiomatization of utility, and it was Savage's extension of this theory that led to his most famous work.

On leaving the SRG he went with Wallis to found the excellent statistics group at the University of Chicago. Initially he wrote papers that were immediately influential on point estimation, using the concepts of unbiased and minimum variance that he would later abandon. He also wrote a beautiful (the adjective is carefully chosen) paper on sufficiency with Paul R. Halmos (1949). But his major work undertaken there was *The Foundations of Statistics* (1954), which was original, important, and influential.

Von Neumann's notion of the axiomatization of utility, mentioned above, takes probability as a basic concept. Indeed, to say that something has utility (u , $0 \leq u \leq 1$) means that the assessor is indifferent between that something and a gamble that has probability u of obtaining a reference quantity of utility 1, and complementary probability $1 - u$ of another reference having 0 utility. Savage conceived the idea of axiomatizing the notion of a person faced with making decisions under uncertainty in a formulation that would incorporate the qualities of the consequences through utility, and the uncertainty attached to obtaining those consequences through probability. He was extremely successful and proved that, subject to mild and apparently reasonable restrictions on behavior expressed through axioms, a person conforming to those axioms acts as if he had a probability for an uncertain event, a utility for a consequence, and selects that decision of maximum expected utility. Savage called such a person "rational"; today the term "coherent" is more often used. He used the term "Thou" to represent such a coherent man; the archaism has been replaced by "You." Note the capital letter.

The result is extremely important not just in statistics (as will be seen below), but wherever

it is necessary to select one from among a number of courses of action—for example, in determining energy policies over the next decade; in decisions in a court of law; in warfare; and in applied science generally. The result may be as important in its sphere as was Newton's discovery of the basic laws of classical mechanics. For a *single* decision-maker, the result is compelling. It fails only where it was never intended to succeed—in dealing with *two* or more decision-makers. A second Savage is needed to study this problem. The results at the present stage of theorizing are either negative (Kenneth J. Arrow's theorem) or very restricted (zero-sum two-person games). Despite the compelling nature of Savage's result and the lack of convincing counterarguments, much work is still being done in statistics and elsewhere that is demonstrably "incoherent." The demonstration proving this contention would take the form of a Dutch book (that is, a series of gambles that, whatever the situation, are certain to lose the decision-maker money) constructed against the person.

During the period in which he was writing the *Foundations*, Savage was not greatly interested in these wider applications, but he was concerned with the use of his ideas in statistics. The first seven chapters of the book develop the formulation of decision making under uncertainty; the rest is devoted to statistical concepts. Savage had been enormously impressed by the success of modern statistics based on ideas of R. A. Fisher, Jerzy Neyman, E. S. Pearson, and others, but he had recognized that these ideas lacked a convincing framework and took the form of a series of loosely connected techniques, like maximum likelihood estimation and confidence intervals. He saw his ideas as providing the needed framework, and in the latter part of the book sought to justify the common statistical techniques by a coherent analysis. The result was a failure. His preface to the second edition (1972) admits this, and earlier (1962) he said: "As I wrote, I became increasingly deaf to my own leitmotif." For ironically, his demonstration achieved the opposite of his intent; as it turned out almost all the techniques he had sought to justify with his coherent analysis failed to meet his test. The techniques themselves were incoherent. The revolution implied by this result is only just beginning to be felt, but it is enormous.

The social sciences depend on data that must often be handled more delicately than experi-

mental data in the natural sciences, which are obtained under controlled conditions. For this reason, statistics plays an important role in the social sciences, as is evidenced by the frequent use of statistical tests in the psychological literature. Yet such tests are incoherent. For example: Let x denote the data and θ the unknown parameter. When the data are at hand, the only uncertain quantity is θ and therefore, by Savage's result, it is described probabilistically, conditional on the data: we have $p(\theta|x)$, the probability of θ given x . But a *hypothesis* test uses $p(x|\theta)$, the probability of x given θ , and calculates a tail area of the form $\int_R p(x|\theta)dx$, where R is a rejection region of x -values. The two probabilities, $p(\theta|x)$ and $p(x|\theta)$, are connected by Bayes' theorem, $p(\theta|x) \propto p(x|\theta)p(\theta)$, which clearly shows that in the coherent view no integration, no tail area, is involved. This is the famous likelihood principle that says that once the data are available, their only contribution is through the likelihood $p(x|\theta)$, and that any further use, say through integration, is irrelevant. This irrelevance typically introduces incoherence, and Dutch books can usually be constructed against significance testers.

Savage did not at first recognize this implication, but over the years he began to appreciate, more quickly than others, the revolutionary nature of his ideas. For example, although G. A. Barnard had, as early as 1947, formulated the likelihood principle, it was not until Allan Birnbaum's paper of 1962 that he began to take the idea seriously. In the disappointingly few years of life that remained to him, he lectured and wrote about the ideas. To follow these papers in sequence is to experience a great scientist discovering for himself simple but important ideas; basic ideas that have important consequences.

In his other major book, with Lester Dubins (1965), Savage treated several formidable mathematical ideas. The problem here is how to play in a casino where the odds are against you to maximize your chances of reaching a target sum. The answer is by bold play: stake everything you have. The main importance of the book may be the recognition that many probability problems can be expressed as gambling problems, and the discovery and application of powerful and simple methods for solving them.

Other important papers of Savage's that use powerful mathematical tools spring from the work of Bruno de Finetti. As Savage worked on *Foundations*, he investigated the work of earlier

writers—particularly F. P. Ramsey's brief paper (1926) in which the ideas had been presented earlier but not understood, and, more important, the rich writings of de Finetti. In the 1930s de Finetti had introduced the concept of a sequence $X_1, X_2, \dots, X_n, \dots$ of quantities that were *exchangeable* in the sense that the joint distribution of any finite set of them was invariant under any permutation of their suffixes. Where the X 's were all either zero or one, he had shown that such a sequence must be a mixture of Bernoulli sequences. Savage, with Edwin Hewitt (1955), had extended this result from 0–1 to more general sequences. This entire field is important because it provides an essential link between personalistic ideas and the frequency ideas that dominate most statistical thinking: exchangeability is the personalistic equivalent of frequency stability, and most statistical techniques today deal with exchangeable, or partially exchangeable, sequences. For many years Savage worked closely with de Finetti and helped the English-speaking world appreciate the enormous contributions of this original and difficult writer.

A man who unintentionally, and even at first ignorant that he is doing so, knocks down an established view rarely has an easy time. Savage was always patient with discussants, but eventually, for personal as well as academic reasons, he left Chicago to go first to the University of Michigan in Ann Arbor and then to Yale University, where he was Eugene Higgins professor of statistics at the time of his death. He continued to give advice to colleagues on statistical matters, and he wrote at least two important papers. One of the problems in the personalistic approach is how "You" are entitled to determine "Your" probabilities. De Finetti had earlier introduced the idea of a scoring rule that would score "Your" performance and improve "Your" probability assessment. The two worked together on this problem and Savage wrote the seminal paper in the field (1971). He also gave a quite remarkable Fisher memorial lecture (1976). Although one could have expected Savage and Fisher to be completely opposed to each other, Savage expressed a truly scholarly appreciation of the work of this statistical giant. He clarified Fisher's achievements and showed how valuable most of his ideas are within the coherent view.

Savage, at the very least, made statisticians aware of problems in the frequency viewpoint. In a broader sense, however, he may well have created a new paradigm for statistics: the co-

herent, or Bayesian, view. If that paradigm is ultimately accepted, it will revolutionize the handling and interpretation of data in the social sciences.

DENNIS V. LINDLEY

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SCHAPER, ISAAC

Isaac Schaper is one of a small band of South African social anthropologists whose work was directly influenced by the physical, politi-

cal, and social conditions in the country of their birth. His intellectual curiosity was directed less to the remote and exotic than to describing and interpreting the social and cultural lives of peoples who appeared familiar from childhood.

He was born in 1905 in Garies, an isolated dorp in Little Namaqualand south of the Orange River in the Northwestern Cape. An arid region, it was sparsely populated by people classified according to the racial stereotyping of South Africa as "whites," who were almost entirely Boers (descendants of early Dutch settlers), and Coloureds, most of whom were products of miscegenation between Boers and Nama (earlier inhabitants of the country, commonly labeled Hottentots). Remnants of the so-called Bushmen lived in the adjacent area of Little Bushmanland and of the pure Nama on mission stations and farms. There were no English-speaking people and no Bantu-speaking Africans in the vicinity.

Schaper's parents moved from Garies to Cape Town when he was seven years old. In Cape Town, he was sent to an English medium school, but retained his fluency in Afrikaans and his contact with people in Garies. On a holiday visit in the early 1920s he was befriended by the district surgeon, an amateur folklorist and archeologist, who owned a fine anthropological library that opened new vistas to Schaper and stimulated his interest in ethnography.

When he enrolled at the University of Cape Town, he intended to study law, but after attending a course of lectures by A. R. Radcliffe-Brown, who had recently returned from Australia and was the first professor of social anthropology in South Africa, switched to anthropology and began his professional apprenticeship. Radcliffe-Brown inducted him into the classics of anthropology and sociology, and Radcliffe-Brown's systematic and empirical Durkheimian structuralism left a permanent imprint.

Having completed his master's degree in 1925, Schaper was accepted as a doctoral candidate at the London School of Economics (L.S.E.) where Bronislaw Malinowski was at the peak of his creative teaching and functionalism was his formula. Schaper attended Malinowski's seminars and served for a time as his research assistant, but his supervisor was C. G. Seligman, a scholar in the more traditional school of ethnography with a record of survey work in Ceylon and Africa.

With a PH.D. from L.S.E. (1929), Schapera returned to South Africa to teach and do research. For a year he lectured at the Witwatersrand University in Johannesburg, and then, at the age of thirty, was appointed to the post formerly held by Radcliffe-Brown. The University of Cape Town remained his base until 1950, when he accepted a chair of anthropology at the L.S.E. As a teacher he did not found any particular school, but gave his students a broad and thorough training in structuralism and functionalism, with an emphasis on ethnographic description. He retired from teaching in 1969 and remained in England working mainly on material collected in Africa.

It was not random choice or chance that his earliest articles were on the Bushmen (1925*a*; 1925*b*); that his master's thesis was a "Preliminary Consideration of the Relationship of Bushmen and Hottentots," and that his first book, based on his doctoral dissertation, was titled *The Khoisan Peoples* (1930), the nomenclature he established in anthropological literature to replace the more pejorative labels for Hottentots (who called themselves Khoi) and Bushmen (whom the Khoi termed San).

His major field work was in Bechuanaland (now Botswana), a British protectorate until it became independent in 1966. The vast majority of the population were Tswana (Bantu-speaking Africans); the minority included San and whites. Most anthropologists stay in the field for a single stretch of one or two years, some return after a long absence to study changes, but in part because Bechuanaland was easily accessible—it is bounded on the east by the Transvaal and on the south by the Cape Province—and in part because of his particular interests and commitments, Schapera made numerous field trips, some of several months, some of a few weeks, spanning more than twenty years (1929–1950). In this way he was able to observe the process of change in all its complexity.

His data on the Tswana are unsurpassed in range and detail. In addition to the material of a keen observer, they include a rich corpus of texts (he mastered the language and recorded much in the vernacular), and numerous case studies culled from unpublished court records and old documents. Unlike many fieldworkers, his notes are meticulous and kept in such a way that they may be used as references by future scholars. His bibliography is extensive, each piece carefully researched and written with an

eye for precision and lucidity. His major contributions are in the field of politics, law, and kinship, approached as systems.

Schapeřa was always a historian as well as a social anthropologist. *The Khoisan Peoples* was based on library research, and though the material was presented under somewhat conventional ethnographic chapter headings, the presentation already indicated a historical perspective at a time when there was a strong trend among British structural and functional anthropologists to think and write only in the ethnographic present. He was one of the first anthropologists in Africa to collect and publish oral history data and to appreciate and edit the journals and letters of early missionaries (1951; 1959; 1960; 1961; 1963; 1974). He has been less concerned with the continuing debate on whether social anthropology is more an art, like history, or a science, like mathematics, than in using both to increase knowledge and understanding of changing customs and social relations, an approach he articulated most cogently in his presidential address to the Royal Anthropological Institute, "Should Anthropologists Be Historians?" (1962).

Schapera's view of anthropology, particularly in the pre-World War II period, was influenced by his sensitivity to the racial problems of South Africa and by a current assumption that anthropology could be effective in their resolution. Courses especially designed for administrators and others engaged in practical work were part of the curriculum at the University of Cape Town as well as at the L.S.E. Addressing the South African Association for the Advancement of Science in 1939, Schapera described anthropology as "an instrument for the promotion of human welfare" (1939, p. 89), and recommended increasing the use of professional anthropologists and the training of others for "applied anthropology."

In Bechuanaland where he worked closely with British authorities as well as with Tswana chiefs and subject people, three books, *A Handbook of Tswana Law and Custom* (1938), *Native Land Tenure in the Bechuanaland Protectorate* (1943), and *Migrant Labor and Tribal Life* (1947*a*), were written with their full cooperation and dealt specifically with controversial policies. Although he believed that an anthropologist's opinion of public policies is a personal and not a professional problem, he felt that as an anthropologist, he was "certainly justified in discussing the methods" (1939,

p. 93). He assumed that "facts" could be "objective," and that knowledge was the first essential for dealing with all difficulties. The information he provided, particularly in his studies of migration and land tenure, revealed the often unforeseen and unpremeditated effects of government decisions.

Schaper was the first British anthropologist to document the view, initially suggested by Radcliffe-Brown, that to understand the process of social change required an inclusive analysis not of items or traits but of the interaction between persons as members of groups within "the whole structure" (1928). District commissioner and chief, missionary and magician, doctor and diviner interacted in a single social system contained within an overarching political control that did not eliminate conflict and cultural differences. Over the years this approach, which was contrary to both the interpretation of acculturation prevalent in America and to the ahistorical model of culture change devised by Malinowski, has been widely accepted and theoretically elaborated.

Schaper's "theories" are usually embedded in his data. As an empiricist aiming at objectivity and believing in rationality, he avoided abstractions and conjecture. His contribution to the development of anthropology as a discipline lies largely in the way he focused his facts on specific topics within the context of different systems of social life. Thus in politics (a persistent and major interest) he analyzed the structural tensions within the political constitution of a southern African nation (1938); the influence of kinship ties on dynastic disputes (1947a; 1949b); the historiography surrounding chiefs (1965); and, in one of his most original works, the development of new legislation, introduced since 1795, by traditional leaders, who were responding through their own cultural values and interests to outside ideologies and technological pressures (1970). In the field of kinship, his focus included genealogies, kinship terminology, types of marriage, and marriage strategies. The effect of his rationalist orientation is most obvious in his work on religion which deals with ritual as action and avoids speculation into the meaning and value of symbols (1955a; 1955b; 1958).

Meticulous in his own scholarship, Schaper is suspicious of generalizations, and skeptical of the value of large-scale, cross-cultural comparison (1953a, p. 353). For him, as for Radcliffe-Brown, valid comparison begins in a defined

local region and is built on limited variations on a few major themes. A comprehensive typological survey (such as Schaper made for the peoples of South Africa and Botswana) is a necessary preliminary. To select examples from any part of the world invites distortion. His most adventurous comparative study, *Government and Politics in Tribal Societies* (1956), uses material from the Khoi, the San, and the Bantu-speaking peoples of southern Africa to examine the nature of government in the light of interpretations by others. Paradoxically, while Schaper is pessimistic about cross-cultural comparison, the solid worth of his work is being recognized by scholars increasingly using his data for comparative purposes.

HILDA KUPER

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SCHATTSCHNEIDER, E. E.

Elmer Eric Schattschneider (1892-1971) was born in Bethany, Minnesota, August 11, 1892, and spent his early years in Wisconsin. If his political consciousness was nurtured in that seat of Progressivism, his adult life was spent opposing many of its central tenets. Progressives and Schattschneider shared a distaste of machine politics and corruption, and both approached democratic politics as a moral crusade. From there they parted company. Schattschneider spent his professional life in the anal-

ysis and advocacy of political parties, precisely the institutions so successfully attacked by such Progressive legislation as nonpartisan elections, city manager government, at-large districts, and the direct primary. In the minds of United States legislators and citizens alike, the Progressives carried the day. Most political scientists disagree. The antiparty spirit that animated Progressive legislation is responsible, many believe, for the decline of American political parties in the twentieth century.

Schattschneider believed that the mass of citizens could participate effectively in government only through party competition. This theme is developed in each of Schattschneider's books spanning 34 years: *Politics, Pressures and the Tariff* (1935), *Party Government* (1942), *The Semisovereign People* (1960), and *Two Hundred Million Americans in Search of a Government* (1969). His political theories are of an intellectual piece. That consistency of outlook put him in and out of phase with the central concerns of political scientists during the four decades of his professional life.

One reason for the cohesiveness and consistency of his views is perhaps that Schattschneider was a mature man when he published his first book. Between his University of Wisconsin B.A. in 1915 and his Columbia University PH.D. twenty years later, Schattschneider spent a year in the navy, two years working with the Young Men's Christian Association, and eight years as a high school teacher. He was 43 when he completed *Politics, Pressures and the Tariff*. Since the principal arguments of his political theory had evolved by the time his first book was published, his work can be discussed conceptually rather than chronologically.

Nature of man. Schattschneider was not a Hobbesian, not even a Madisonian. He thought people were, in their fundamental aspects, benign. If Madison believed that all men, given the opportunity, would tyrannize over other men (Dahl 1956), Schattschneider could reply without embarrassment that "democracy is about the love of people" (1969, p. 43). According to Schattschneider, "democracy is both a moral system and a form of government." That moral ideal was equality: "Democracy begins as an act of imagination about people. . . . Democracy does not turn its back on anybody. It takes a lot of indiscriminate affection for people as people to run a democracy" (p. 46). A realist, Schattschneider knew that many people are hard to love. The second law of politics? "It is impos-

sible to get all of the S.O.B.'s into one party" (p. 53).

Two Hundred Million Americans contains an ingenious demonstration of the progress toward an egalitarian political culture in the United States. Working on the assumption that language reflects a value system, Schattschneider used nineteenth-century dictionaries to show the degree to which our language has been democratized. In those early dictionaries common people were often characterized with the pejoratives vulgar, ordinary, mean, low. Now, "the vocabulary of indignities has been cleaned up to make the language fit for use in a democratic society" (p. 51).

As an egalitarian, Schattschneider was an unapologetic majoritarian, and he labeled Madison's *Federalist No. 10* a philosophical curiosity, embodying serious inconsistency. Madison believed that social pluralism—a large, diverse republic—would render it unlikely for any popular majority to unite to deny political liberties to a minority. Why, then, Schattschneider wondered, did Madison defend the necessity of a constitutional separation of powers as well? (1942, p. 18). Madison could have replied that divided constitutional authority was intended less to solve the problem of a tyrannical majority among the citizenry than it was to solve another problem, the abuse of constitutional authority by government officials themselves. Still, Schattschneider's pique at Madison's defense of separation of powers is understandable. Madison had helped establish a division of constitutional authority that was incompatible with Schattschneider's ideal of party government.

If Schattschneider's assumptions about the nature of political man begin with an act of democratic imagination, he does not imagine that political man is an informed civic activist. His views on the political knowledge of citizens are quite consistent with those of such political economists as Downs (1957) and Schumpeter (1942). Extensive knowledge is a scarce resource, expensive for the expert to obtain and not to be expected of citizens. Nor is it necessary either: "Economists, trying to explain the operation of the economy, use a political expression when they speak of the 'sovereignty of the consumer,' precisely because they realize that it is not necessary to know how to *make* a television set in order to buy one intelligently. Democracy is like nearly everything else we do; it is a form of collaboration of ignorant people and experts" (1960, p. 137).

Socialization of conflict. In Schattschneider's world, conflict is endemic in political life. All politics revolves around the exploitation of energy that is based on conflict. People turn to government when they want to ensure that the balance of power among competing private interests does not prevail. When the loser in a battle among private interests seeks reinforcements among the previously uninvolved, the competition for popular support enhances people's knowledge of what they have at stake in the contest: "To a great extent, the whole discussion of the role of government in modern society is at root a question of the scale [of] conflict. Democratic government is the greatest single instrument for the socialization of conflict in the American community" (*ibid.*, p. 13).

When Schattschneider wrote these words, most political theorists viewed conflict as a danger to civil liberties and to democratic government. Against the historical backdrop of the world-wide authoritarian mass movements of the 1930s and 1940s, McCarthyism in the 1950s, and a growing research literature that seemed to discover public ignorance of the substance and procedure of democratic government, pluralist theories emphasized the importance of protecting consensus and limiting conflict.

The importance attached to nonvoting is an example of the intellectual distance between Schattschneider and many pluralists of the time. Some pluralists followed Berelson's position (1952) that apathy could reflect people's satisfaction with the political system, and that sudden increases in turnout were often associated with an antilibertarian movement destructive of a tolerant political system. Schattschneider's concern was starkly different. He worried that "nonvoting is a characteristic of the poorest, least well-established, least educated stratum of the community," and he lamented the failure of the parties to compete on issues that would reveal to this nonvoting population what interests it had in political conflicts (1960, p. 105).

As a "conflict theory" published during the vogue of pluralist consensus theories, *The Semi-sovereign People* was not well received at the time of its publication. As conflict theories have gained in respectability, so has the impact of Schattschneider's work on contemporary theory. But it must be remembered that, for Schattschneider, democratic conflict requires a moral consensus. Without its "moral basis" Schattschneider emphasized, without a belief in equality and tolerance, "democracy as a form of gov-

ernment may be a dangerous instrument for generating destructive conflict" (1969, p. 45).

Private versus public interests. The distinction between public and private interests is one of the most important of Schattschneider's concepts. It is also one of his most problematic. He defines the public interest as those "general or common interests shared by all or by substantially all members of the community" (1960, p. 23). If, however, he means by common interests beliefs and goals actually shared in common, then the concept is inconsistent with the rest of the theory, which is based on competition and conflict of values.

Schattschneider's distinction between public and private interests must be understood in terms of some broader theory. For Schattschneider that theory would be liberal majoritarianism. A sensible interpretation of his argument is that the public interest is what an ideal democracy produces. When conflict is high, when parties compete for votes in terms of alternative programs, when a high proportion of the electorate becomes sufficiently aroused by the contest to consider the arguments and to vote its preferences, we are then willing to assume that people reasonably know their interests—at least as much as these interests can be known in a world in which information about the present is limited and the future cannot be predicted. In short, party government produces the public interest.

We can now begin to understand the basis of Schattschneider's faith in human nature. A democratic society encourages the sustenance of democratic citizens—people who know and act on their interests but who also come to appreciate that people of opposing interests should be treated with respect and tolerance.

Party politics versus pressure politics. Schattschneider (1948a) saw parties and pressure groups as antithetical. Parties exist to control government, and formulating policies and nominating candidates are means to that end. But groups are interested only in particular policies. Supporting candidates, lobbying legislators and bureaucrats, financing litigation—these are, in Schattschneider's view, simply means for groups to obtain policy goals.

Pressure groups also differ from parties in the proportion of the public each includes. As electoral organizations, parties engage mass followings. In contrast, "the law of the imperfect political mobilization of interests" (1942, p. 50) underlines the fact that the vast majority

of people are not members of any interest group. Hence Schattschneider's famous statement: "The flaw in the pluralist heaven is that the heavenly chorus sings with a strong upper class accent" (1960, p. 35).

Schattschneider's belief that the bias of the pressure system is probusiness and upper class was undoubtedly influenced by his choice of his first research project. An analysis of the Smoot-Hawley Tariff Act of 1930, *Politics, Pressures and the Tariff* is a notable demonstration of the activity of producer groups, and the passivity of consumer groups (1935, p. 285). Smoot-Hawley reflected an age of protectionism in which tariff rates for thousands of products were written by Congress directly into legislation. In 1934 the Roosevelt administration enacted the Reciprocal Trade Act, which authorized the president to negotiate mutual tariff reductions with other countries. By the 1950s, the ideology of protectionism had waned. The responsibility for tariff rates had passed to the executive branch, and as Bauer, Pool, and Dexter (1963) report in their landmark study of the Reciprocal Trade Extensions of 1954, 1955, and 1962, pressure groups ceased to dominate tariff rates. Nevertheless, they gave high praise to the enduring importance of Schattschneider's *Politics, Pressures and the Tariff*: "Schattschneider's book set the tone for a whole generation of political writing on pressure groups. The present volume is in some ways a dissent from Schattschneider's position. We find that what happened in 1929 is not a general model of the legislative process. We do not deny the facts of the case as Schattschneider presents them. He has reported an episode in the legislative drama during which selfish interests treated the halls of Congress as their own" (p. 25).

Party government. Schattschneider concluded his study of the tariff by stating: "To manage pressures is to govern; to let pressures run wild is to abdicate" (1935, p. 293). If it is the responsibility of government to control pressures, he believed, then only responsible parties are sufficient to the task. The doctrine of responsible parties long predates (Ranney 1954) Schattschneider's defense in *Party Government*. Many observers had previously argued that a political culture of individualism and a constitutional separation of powers made a hostile environment for cohesive parties. If responsible parties seem an impossible achievement, *Party Government* has remained important nonetheless. Many scholars have elaborated on its ideas of

the consequences of electoral systems for parties. To Schattschneider, the electoral system is a major cause of the two-party system (1942; cf., Duverger 1950). It exaggerates the representation of the winning party (cf., the "cube law," March 1957-1958) and produces moderate parties that move toward the "political center of gravity" (p. 86; cf., the convergence theorem in Downs 1957). What maintains competition between the parties? "It would be unprofitable to accumulate an excessive majority . . . a landslide is a political extravagance" (p. 95; cf., the theory of a minimum winning coalition in Riker 1962).

In 1946 the American Political Science Association created the Committee on Political Parties, with Schattschneider as its chairman. Its report, *Toward a More Responsible Two-party System* (1950), was widely praised and fiercely criticized. On the significant point of internal party democracy, the report departs strikingly from Schattschneider's own views. In contrast to the report's defense of internal party democracy, Schattschneider believed that "democracy is not to be found *in* the parties but *between* the parties" (1942, p. 60). A party is not a mass association of voters; it is "a political enterprise conducted by a group of working politicians supported by partisan voters" (*ibid.*, p. 59). Ranney (1975, p. 144) argued that one's definition of a party member determines one's position on nearly all issues of party reform and internal party democracy. So it was with Schattschneider. He consistently opposed the direct primary, which the *Report* defended, because he believed primaries to be destructive of party organizations and irrelevant to the broader question of democracy.

Democracy. What then is democracy? It is "a competitive political system in which competing leaders and organizations define the alternatives of public policy in such a way that the public can participate in the decision-making process" (1960, p. 141). Government officials take the initiative for policy proposals and accept responsibility for the conduct of public business. The opposition party stands as a critic of the government and as an alternative government. Between the two the public makes its sovereign judgment, "a sovereign whose vocabulary is limited to two words, 'Yes' and 'No'" (1942, p. 52). It is a theory of democracy that places consent rather than participation at its core. Americans, who value participation almost as much as they mistrust political parties, have

remained hostile to democracy defined as party government. Schattschneider's works failed then as public advocacy. Since he wrote *The Semisovereign People*, changes in party rules and election laws have increased internal party democracy at the same time that party government has withered even further. Progressivism has thus far had the last word.

Civic and professional life. One aspect of Progressivism that Schattschneider never rejected was a commitment to public advocacy and civic participation. He was elected to his city council and served on a number of town and state commissions. He won a Freedom Foundation award for a television broadcast with Julian Hart of Yale University on democracy as a moral system, the topic that became a major portion of *Two Hundred Million Americans*. He was also active in the American Political Science Association, serving as president in 1956/1957. Schattschneider taught at Columbia University from 1927-1930 while working on his doctorate, at the New Jersey College for Women in 1929/1930, and at Wesleyan University from 1930 until his retirement in 1960. He continued to teach and publish through his retirement years until his death at 78 in Old Saybrook, Connecticut, March 4, 1971.

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SCHEFFÉ, HENRY

The life and career of Henry Scheffé (1907-1977) cover the period when the study and development of statistical methods emerged as the recognized discipline of statistics or mathematical statistics (Owen 1976). He was one of the first and finest pioneers of the new species, "mathematical statistician." Until World War II, the subject was developed in the United States and abroad by those whose research required it. Thus, even if they were mathematically trained, the developers of the discipline usually had some other profession (e.g., biology, economics, etc.) than statistics.

Scheffé was born to German parents on April 11, 1907, in New York City. He initially studied engineering at the Cooper Union Free Night School, the Brooklyn Polytechnic Institute, and Bell Telephone Laboratories, where he also worked for a short time. In 1928, he entered the University of Wisconsin, taking a B.A. in math-

ematics in 1931. He remained there as a doctoral student of R. E. Langer, receiving his degree in 1935 for work on differential equations.

The great developments in statistics, by Karl and E. S. Pearson and R. A. Fisher in the United Kingdom and Jerzy Neyman in Poland, the United Kingdom, and then at Berkeley, had been picked up by Harold Hotelling at Columbia University and S. S. Wilks at Princeton University. In addition, Abraham Wald arrived at Columbia in 1938. When Scheffé decided in the late 1930s to leave mathematical analysis for statistics, he naturally went first to Princeton and Columbia, and inevitably Berkeley, which became the world center of mathematical statistics. He was at Princeton (1941-1944), Columbia (1946-1963), and the University of California at Berkeley from 1953 until his retirement in 1974. In 1954 he was president of the Institute of the Mathematical Statistics and from 1954 to 1956, vice president of the American Statistical Association. He died of injuries sustained in a bicycle accident in Berkeley on July 5, 1977.

By the early 1940s, Wilks's enthusiasm and the pioneering possibilities of the new subject had gathered many young people to Princeton. The outbreak of World War II and the need to apply mathematics and statistics to urgent national problems brought a further concentration of statisticians to Columbia and Princeton. Scheffé, like many others who subsequently had distinguished careers in statistics, worked during this period for the Office of Scientific Research and Development in the New York area. His mathematical talents were the more useful for his engineering background and growing knowledge of statistics.

In the 12-year period, from 1941 to 1953, Scheffé was almost literally in close contact with everyone and with every idea that has subsequently affected the course of statistics in the United States. These influences were diverse and conflicting—statistics as a guide to the elucidation of immediate practical problems; statistics as something to be studied in an orderly and elegant mathematical manner; the decision-theoretic formulation versus the more inferential mode; the polar interests of Wilks in nonparametric methods and multivariate (normal) analysis. One area of statistics that seems to have been underrepresented in this group and perhaps overrepresented elsewhere was the design and analysis of comparative experiments.

Scheffé was quiet and modest and very much

his own man. He was content that others should see his point of view by reading and reflecting on his words and writings, not by powerful advocacy. What, then, was his contribution to, and how was he affected by, the statistical revolution of the 1940s? In retrospect, he seems to have chosen to pursue what his colleagues did not—the design and analysis of comparative experiments. His other work, is, however, so often quoted that future scholars may make a different assessment. At present, the user of statistical methods most likely associates Scheffé with the multiple comparison problem; in the future, his treatment of the calibration problem may be better known. But Scheffé wrote for the professional statistician. The serious student of statistics may well learn the analysis of variance from his famous text. Graduate students of mathematical statistics will learn his name in connection with the concept of “completeness” and the convergence of a sequence of probability densities. Anyone reading the literature on the Behrens–Fisher problem will find that it was a lifelong interest of Scheffé’s.

Scheffé published 37 very carefully considered and written works, which are discussed in serial order by Daniel and Lehmann (1979). The latter was Scheffé’s colleague in Berkeley and coauthor of the great *Sankhyā* (1950; 1955) papers on “Completeness, Similar Regions, and Unbiased Estimation.” There, they introduced the notion of a complete sufficient statistic to clarify the problems of finding unbiased estimators with uniformly minimum variance and of finding tests whose size does not vary with nuisance parameters. Cuthbert Daniel, a well-known applied statistician, discussed, over a period of many years, problems in applying statistical methods to industrial problems, particularly those related to the analysis of variance. Being a consultant to a consultant suited Scheffé’s personality and directly or indirectly inspired much of his later work on the analysis of variance, multiple comparisons, mixture designs, and calibration. The fact that he was a statistician’s statistician makes it difficult to discuss his best work, because it is necessarily technical. For example, he foresaw clearly, in a 1943 review, the future and the theoretical requirements of nonparametric statistics. He had been on the west coast for two years before E. J. G. Pitman’s Columbia lectures in 1948 began to fill in some of these gaps.

The applied statistician constantly analyzes and isolates sources of variability or variance

in data. The technique is the analysis of variance. This logic has led to experimental designs which enable more sensitive treatment comparisons to be made; the analysis of variance eliminates irrelevant sources of variation. Scheffé’s book (1959) summarized, for the first time fifty years of research in this basic area from the viewpoint of mathematical statistics. The traditional intuitive–numerical example style is abandoned in an effort to get the logic and assumptions in full view. It is a difficult book to read, the result of Scheffé’s efforts to reconcile what was practically important with his exacting mathematical standards. Chapter ten of his book is still a good—and for many years has been almost the only—reference on the robustness of the methods of analysis of variance.

The classical statistical methods of hypothesis testing and confidence intervals assume that the data collection has been planned to test hypotheses or to estimate specified quantities regardless of what the data, once collected, may suggest. In reality, however, though “set-piece” experiments may occasionally be run, data are often examined after collection for interesting possibilities. It is clearly improper to use a classical test to check the reality of an effect found in this manner, since if we keep looking at even purely random data, a significant result is guaranteed to appear. This problem had been recognized for many years (and even solved in some cases [e.g., Fisher 1929]), but in the early 1950s, both J. W. Tukey and Scheffé (1953) derived important results by restricting the problem. They initially considered a set of k independent means \bar{x}_i each of r observations, all with the same variance, s^2/r the typical situation in analysis of variance. $\sum c_i \bar{x}_i$ is called a “comparison” if $\sum c_i = 0$. For example, $\bar{x}_1 - \bar{x}_2$ is a comparison because $c_1 = 1$, $c_2 = -1$, $c_i = 0$, $i \neq 1, 2$. Let \bar{x} be the mean of all the \bar{x}_i and observe that $\sum c_i \bar{x}_i = \sum c_i (\bar{x}_i - \bar{x})$. Now Cauchy’s inequality says that always

$$[\sum c_i (\bar{x}_i - \bar{x})]^2 \leq \sum c_i^2 \sum (\bar{x}_i - \bar{x})^2$$

so that

$$\frac{[\sum c_i \bar{x}_i]^2}{\sum c_i^2 s^2/r} \leq \frac{\sum (\bar{x}_i - \bar{x})^2}{s^2/r}$$

The lefthand side is the square of the t -test statistic that would be used to test whether $\sum c_i \bar{x}_i$ is significantly different from zero. To make this test safely, we have only to see whether the righthand side is significantly

large; but this is the F -test of the associated analysis of variance. If the class of comparisons is smaller—say only pair-wise differences of means, too much is given away by the Cauchy inequality and a better procedure may be found. A full account is given by Miller (1966) with an update (1977).

In 1951 Scheffé considered some special analysis problems that arise in sensory preference tests where only paired comparisons arise; the results have been widely adopted. In 1958 he opened up a new field by considering the design of chemical experiments where the outcome depended only upon the proportion x_i of the constituents, $\sum x_i = 1$. These designs are very symmetric. The resulting literature has provided many alternatives similar to his original suggestions.

In his last major paper, Scheffé (1973) attempted to clarify and refine the ancient calibration method. According to this method, (1) experiments with various values of x are conducted in order to estimate a relation $y = f(x)$; (2) then, the easy measurement y is made and the difficult x determined by reading the graph of $y = f(x)$ backwards. Even if we assume correctly that $f(x) = a + \beta x$ and that $y = a + \beta x + u$, where the u 's are normally and independently distributed, there is a problem. Clearly, we can estimate a , β , σ^2 and give a distribution, depending on x , to any possible y . To give an interval estimate for the unknown x evidently requires that we invert this knowledge—roughly, it will contain every x that does not make the observed y too unlikely. An interval may or may not be “correct”—that is, it may or may not cover the true x . But the same calibration line is to be used again and again for different y 's. Scheffé provides a method ensuring a probability of at least $1 - \delta$ that indefinitely frequent usage of the method, for any (unknown) sequence of x -values will lead to a fraction of correct statements in excess of $1 - \alpha$. Work continues to make this method more available to users.

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SCHLOSBERG, HAROLD

Harold Schlosberg (1904-1964) was an experimental psychologist who made contributions to the study of conditioning, perception, emotion, and psychological theory.

Biography

Schlosberg was born in Brooklyn, New York, the son of an electrical contractor. As a youth he served an informal apprenticeship to his father, helping to install electrical circuits. As a result of this early experience, many of his experimental studies were carried out with the aid of electrical equipment that he designed himself. He was also widely known as an inventor of psychological gadgets of the "rubber band and paper clip" variety. Some of his publications were apparatus notes (1937).

Schlosberg received his A.B. (1925), M.A. (1926), and Ph.D. (1928) from Princeton University. His doctoral dissertation on the conditioned knee jerk in man was the topic of some

of his further early research. In 1928, Schlosberg became an instructor of psychology at Brown University, where he remained until his death.

Partly as a result of Schlosberg's experimental work, but perhaps more importantly as a result of his sage advice, Brown University was soon to move into the ranks of the top departments of psychology in the United States. In 1936, Walter S. Hunter came from Clark University to be chairman of the department, and he soon attracted a strong group of scholars. During the next two decades, in addition to Hunter and Schlosberg, the Brown psychology department included such people as Leonard Carmichael, Clarence Graham, Donald B. Lindsley, Lorrin A. Riggs, Carl Pfaffmann, J. McVicker Hunt, David Zeaman, and G. Robert Grice. In 1954 Schlosberg succeeded Hunter as chairman.

During his career, Schlosberg contributed to the science and profession of psychology and received many honors in return. He served on several committees of the American Psychological Association, including the Policy and Planning Board. In addition, he was a member of a panel of consultants on psychophysiology for the Surgeon General for ten years, a member of the editorial board of the *Annual Review of Psychology*, a consulting editor for *Psychological Review*, and, as chairman at Brown, Schlosberg was an active member (and chairman for one year) of the National Council of Chairmen of Graduate Departments of Psychology. During the summer of 1960, he toured laboratories in Russia and Poland under a grant from the Society for the Investigation of Human Ecology, accompanied by Neal E. Miller and Carl Pfaffmann. Schlosberg was among the first to assist in bringing the psychologies of America and eastern Europe closer together (Miller, Pfaffmann, & Schlosberg 1962).

Schlosberg received many honors. At different times, he was president of the Divisions of Experimental and Physiological Psychology of the American Psychological Association, chairman of the section on psychology of the American Association for the Advancement of Science, president of the Eastern Psychological Association (1953/1954), a member of the Society of Experimental Psychologists, and a fellow of the American Academy of Arts and Sciences.

From a fairly early age (in his thirties), Schlosberg suffered from arthritis in the neck and back. Because of this painful condition, which severely limited his physical activity,

Schlosberg spent little time in the laboratory and many of his contributions were of a theoretical nature.

Scientific contributions

At Princeton, Schlosberg was strongly influenced by the behavioristic psychologist and philosopher, Edwin B. Holt. This influence persisted throughout Schlosberg's career, which was devoted to the experimental study and theoretical interpretation of objectively observable behavior. Although he was an objectivist, Schlosberg resisted, and objected to, the most restrictive versions of operationalism. For the last decade or so of his life, he refused to submit articles to the journals that were most heavily dominated by this theme.

Studies of conditioning. In keeping with this liberalized objectivism, Schlosberg's earliest work (1932) was on conditioning in human beings. In 1902 Edwin B. Twitmyer had discovered that the patellar reflex (knee jerk) is conditionable. Although Twitmyer's dissertation was not published for more than seventy years (Twitmyer 1974), there was a report of the work at a meeting of the American Psychological Association in 1905. The general facts of such conditioning thus became common knowledge and stimulated further attempts to study the phenomenon.

By and large the results were disappointing. Schlosberg, for example, found that the patellar reflex was difficult to condition in anyone and that there were enormous individual differences in the degree to which people could be conditioned. Attempts to uncover the bases for these individual differences were unsuccessful. No correlation could be discovered between these differences, and measures of suggestibility (which have since yielded more positive results) were too crude to be useful. All of this led Schlosberg, in his later work, to turn to the laboratory rat as a subject for conditioning.

These early studies did produce one line of interpretation that was to become significant in Schlosberg's important paper (1937) on the distinction between classical and instrumental conditioning. The behavior of the subjects in the experiment supported the view that the conditioned reflex belonged in a special category of learning, "split off from the rest of the subject's activity, like an automatism." The evidence for such a statement was that the conditioned reflex appeared to develop apart from voluntary control. Some subjects seemed to be unable to con-

trol the knee jerk voluntarily. Others showed that they could, but that they were also able to assign the reflex to a separate psychological compartment and let it function according to its own rules. Interesting as such observations are today, they would not have been much appreciated in a psychology dominated by Watsonian behaviorism, which had ruled out of the field everything even faintly tainted with mentalism. Probably this is what led Schlosberg to turn to lower organisms for his next studies.

These studies (Schlosberg 1936; Kappauf & Schlosberg 1937) represent Schlosberg's major contribution to the factual literature in the area of conditioning. The general procedure in these studies was to pair an unconditioned stimulus, such as a shock to the tail or leg, with a conditioned stimulus, such as a light or buzzer at an interval of approximately 0.35 to 7.00 seconds. Three types of responses were measured: changes in breathing, tail or leg movements, and squeals. Some of the important findings in these studies were the following:

(1) The different responses were conditioned in a particular order. Changes in breathing appeared first, followed by tail or leg movements, and finally squeals. In extinction (presenting the conditioned stimulus without the unconditioned stimulus) they dropped out in the reverse order. This led Schlosberg to develop a concept of depth of conditioning, a notion that has never received the attention it deserves.

(2) Variations in the interval separating conditioned and unconditioned stimuli sometimes produced a gradient of effectiveness, with the best conditioning occurring at intervals of 2/3 or 1.0 seconds. At long intervals, little or no conditioning occurred. The most favorable intervals were much shorter than those reported by Ivan Petrovich Pavlov as effective, a fact that was confirmed in numerous later experiments by many others. At the same time, however, Schlosberg also observed that very strong shocks obliterated the gradient and longer intervals were effective. This came about by the conditioning of diffuse excitement which delayed the waning effectiveness of the conditioned stimulus.

(3) Crowding practice trials close together had an effect. Fifteen or twenty trials per day led to better conditioning than two hundred trials per day.

(4) There is a small loss in the strength of the conditioned response between daily sessions.

With a two month delay the conditioned response practically disappears.

(5) If more than a few unreinforced "test trials" are used in a daily session, the conditioned response is seriously weakened through extinction.

Conditioning theory. This last result, which Pavlov had also obtained, had an important implication. It called into question Edward L. Thorndike's most important principle, the so-called "law of effect," which holds that responses that result in "satisfaction" (Schlosberg substituted the word "success") are "stamped in"; those that result in "annoyance" or "failure" are "stamped out." In more ordinary terms, successful responses are learned; unsuccessful responses are lost.

Now, from the rat's point of view, the administration of a test trial should constitute an experience of "success," because making a response seems to avoid the shock. Aware of this point, Schlosberg ran a number of tests under conditions where a response would always prevent or terminate the shock. He found, contrary to the "law of effect," that the successful response was not learned quickly, but rather, that it needed reconditioning at established intervals. The response weakened with avoidance (success) to the point where it failed to avoid the shock; this led to shock again and the re-establishment of the response. Such observations led Schlosberg to the view that the "law of effect" does not apply to conditioned avoidance and to the suspicion that the classical conditioned reaction is only one type of learning and not the basis of all learning experiences. He developed these ideas more completely in his important 1937 paper, "The Relationship Between Success and the Laws of Conditioning."

Largely as a result of Pavlov's tendency to describe all learned behavior in terms of conditioned responses, the notion that there might be more than one form of learning was revolutionary in 1937. Nevertheless, the data had begun to indicate to a number of theorists that this might be the case. Probably the first of these were Stefan Miller and Jerzy Konorski (1928). B. F. Skinner proposed the same idea in 1935. Thorndike, in various places, suggested that classical conditioning might be a special form of learning, governed by a law of associative shifting. Schlosberg's statement, however, probably had the greatest impact.

The common theme that runs through all of these writings is that two experimental arrange-

ments employed in the laboratory involve different relationships between a to-be-conditioned response and its consequences. This meant that the different procedures may possibly have produced different kinds of learning. In one experiment, commonly called instrumental (or operant) conditioning, the response delivers some positive stimulus, e.g., food, or avoids a negative one, e.g., shock. In the maze, for example, the rat receives food only if it performs the correct series of responses. In the second laboratory setting, often called classical conditioning, the response has no effect upon the positive or negative stimulus. It appears no matter what the learner does. In Pavlov's experiments, for example, food is delivered whether or not the dog salivates.

Schlosberg referred to these two forms of learning, respectively, as "trial-and-error learning" or "problem solving" and "conditioning." Conditioning, he said, applies to diffuse preparatory responses which may correspond to what Edward C. Tolman (1933) was calling expectancies. They require only relatively simple neural processes and can be established by the mere pairing of conditioned and unconditioned stimuli. The "law of effect," or reinforcement, is specifically not involved. "Trial-and-error learning," by contrast, obeys the law of effect. It leads to the acquisition of very specific skeletal responses and requires more elaborate nervous mechanisms.

Other contributions. The detailed formulation of this distinction was Schlosberg's most significant contribution. With the outbreak of World War II, Schlosberg served as acting chairman of the department of psychology at Brown University while Hunter, involved in the war effort, was on leave. After the war Schlosberg did additional work on conditioning, but gave more of his attention to other projects. He set up a bank of aquariums in his office to study the instinctive behavior of tropical fish, but not much ever came of this work.

He published a simple mathematical model (1948), which described grossly the probability of seeing from two to seven dots exposed tachistoscopically. The probability of seeing N dots, according to his model, was the N th power of the probability of seeing one dot under the same conditions. Although this was one of Schlosberg's favorite papers, he did no further work on the topic.

The results of these two projects were meager because of Schlosberg's decision, in about 1949,

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to devote most of his time to collaborating with Robert S. Woodworth of Columbia University on the revision of Woodworth's *Experimental Psychology*. This massive work appeared in 1954. In the same year Schlosberg published his last important scientific paper. It was a dimensional analysis of the facial expression, which Schlosberg saw as a key problem in the psychology of emotion. In this paper he showed that people could locate facial expressions in a three-dimensional cone defined by three continua: sleep-tension, acceptance-rejection, pleasant-unpleasant.

As this description of Schlosberg's work may suggest, his contribution was largely one of bringing good sense and clear thinking to bear on a variety of topics. He created no school of psychology and gathered no cadre of disciples around him, although he was an influential teacher and administrator. He was more of a synthesizer than a creator and will be remembered in the history of psychology for that.

GREGORY A. KIMBLE

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Theodore W. Schultz has been a major contributor to the field of agricultural economics. Born in 1902, he grew up on a South Dakota farm in a community of German settlers. He received his bachelor's degree in 1926 from South Dakota State College and his doctorate in agricultural economics in 1930 from the University of Wisconsin. During his professional academic career at Iowa State College and the University of Chicago, he served extended periods as chairman of each department and had a major role in the development of both.

One of Schultz's major tenets is his emphasis that the field of agricultural economics was an integral part of economics, and that price and value theory could be fruitfully applied to problems of interest to agricultural economists. Much work in agricultural economics, despite some important exceptions, fell into such specific fields as farm management, marketing, land tenure, and credit, with little interrelationship among them and without much benefit of the insights and methods derived from economic theory. Schultz's effort to make agricultural economics a part of general economics was expressed not only in his own writing and research but in the way he molded the Iowa State College department of economics and sociology, which in the 1930s and early 1940s included general economics, agricultural economics, and rural sociology. He brought to the department some of the brightest young economic theorists available, including George J. Stigler, Albert G. Hart and Kenneth E. Boulding. Empirical research was also greatly strengthened by close association between economic theorists and statisticians.

In the late 1930s and the 1940s Schultz turned his attention to the analysis of agricultural policies. The many New Deal agricultural programs provided numerous research opportunities, and one important interest was to determine the effects of particular programs, such as acreage limitations, upon the actual output of farm products. His interest in agricultural policy resulted in the publication of four important and influential books from 1943 to 1953: *Redirecting Farm Policy* (1943), *Agriculture in an Unstable Economy* (1945a), *Production and Welfare of Agriculture* (1949), and *The Economic Organization of Agriculture* (1953). One of his major points was that governmental policies that increased output prices or distributed subsidies that were highly corre-

lated with inputs or outputs would do little or nothing to improve the economic position of low-income groups in agriculture. He argued that the primary function of prices was to direct the use of resources, and that their use to increase, or modify the distribution of, income resulted in a waste of resources and had little or no effect on income.

His later interest and research in the analysis of human capital followed from these earlier works. His prescriptions for improving the welfare of farm people included measures that would reduce the supply of labor to agriculture and increase public investment in human agents.

It was primarily his emphasis upon the objective analysis of agricultural policies that led to his move to Chicago in 1943. At the beginning of World War II Schultz and his colleagues at Iowa State College organized a series of studies, the *Wartime Farm and Food Policy Series*, designed to indicate how agricultural policies could be modified or designed to maximize agricultural contributions to the war effort. One of the studies, O. H. Brownlee's *Putting Dairying on a War Footing* (1943), aroused so much controversy within the state and was so mishandled by the Iowa State College administration that the bulletin was withdrawn from circulation. The conclusion considered so offensive was that margarine, a substitute for butter derived primarily from vegetable fats, was nutritionally equivalent to butter, and that taxes and other restrictions—such as prohibitions on the sale of margarine colored to resemble butter—should be removed. This thesis threatened the interests of the numerous dairy producers in Iowa who depended for most of their income on the sale of butter, and strong political pressure was placed on Iowa State College to repudiate the pamphlet (see Hardin 1955). After this violation of academic freedom, Schultz left Iowa for the University of Chicago.

A second major thread in Schultz's interests was economic growth—how to understand it and promote it. In *Food for the World* (1945b), a volume of papers presented at a conference that he had organized, he systematically developed the interrelationships of the factors affecting demand (population, income, and elasticity of demand) and supply (labor force, technology, improvements in the skill of farm people, and the rate of investment). His most significant work on this topic was *Transforming Traditional Agriculture* (1964), whose main points were

that traditional farmers were efficient in the use of resources; that the marginal productivity of farm labor was not zero; and that poor farmers would quickly adopt new methods of production if they were superior to the old. A primary, though not the only, approach to increasing food production and rural incomes in the low-income countries was to develop new technologies that were superior to those derived from centuries of practical experience. Schultz argued strongly against policies in developing countries that emphasized industry at the expense of agriculture and urban consumers at the expense of farm people and expanded food production. The rapid adoption by low income countries in the mid-1960s of the new high yielding varieties of rice and wheat was fully consistent with his conclusions.

Schultz had an uncanny knack for asking relevant questions that often inaugurated new lines of research for his students and colleagues. For example, one question involved the unexplained residual in the growth of output—i.e., the increase in output not explained by the increase in inputs as conventionally measured. In effect, he asked if the size of the residual was not more of an indication of our ignorance than it was a measure of productivity change. This question generated an enormous amount of research, including research on improved measures of various inputs and on the measurement of the returns to investment in research.

Although his article "Investment in Man: An Economist's View" (1959) did not originate the study of human capital, it did influence the direction of much of the work in this area during the 1960s and 1970s. His influence stemmed from his contributions to discussions with economists in the United States and abroad at the workshops in economics that flourished at the University of Chicago from the late 1940s on, at professional meetings, and at colloquia and symposia, some of which he organized. He has the capacity to learn and teach others simultaneously.

Schultz's interest in human capital and his belief that the social sciences had been ineffective in understanding the factors influencing the rate of population growth led him to turn to the economic analysis of fertility. To some degree this interest tied in with his earlier work on agriculture and world food and his succeeding efforts to analyze the potential for increasing income in low-income or developing countries.

While contributing to the analysis of human

capital and fertility, Schultz continued to make important contributions to the literature of agricultural economics. One of his continuing concerns has been the adverse impact of food and agricultural policies in the developing countries upon the growth of agricultural production. *Distortions of Agricultural Incentives* (1978), the paper presented at a 1977 conference, was one important result of this interest. He has also continued to emphasize the role of agricultural research in expanding production, and the contribution of investment in people in rural areas to the improvement of the welfare of those people.

Schultz has received many honors. He was one of the first fellows of the American Farm Economic Association (1957), president of the American Economic Association (1961), distinguished fellow of the American Economic Association (1965) and recipient of its Francis A. Walker medal (1972), and a member of the National Academy of Sciences and the American Academy of Arts and Sciences.

D. GALE JOHNSON

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SEARS, ROBERT R.

Robert R. Sears, a leader in the revival of developmental psychology in the mid-twentieth century, was born in Palo Alto, California, in 1908, of parents recently arrived from the American Midwest. He grew up in Palo Alto, studied literature and drama at Stanford University, found his interest in psychology piqued by the enthusiasm of his fiancée (Pauline Snedden, soon to be his wife and lifelong research colleague) and by the teaching of Paul Farnsworth. In 1929 Sears left Palo Alto for the 24 years—at Yale University, the University of Illinois, the University of Iowa, and Harvard University—that would be the most innovative and productive times of his research career. He returned to Stanford in 1953, first as head of psychology, then as dean of the School of Humanities and Science, and, in the years before his retirement in 1975, as David Starr Jordan professor of psychology.

Like most American psychologists, Sears was prepared for his career by a curious amalgam of accident, *Zeitgeist*, and personal predilection. The defining accidents were his appointment in 1932 as instructor at Illinois, where he was suddenly required to work up a course in personality psychology, and his later appointment in 1942 as director of the Iowa Child Welfare Research Station, where he became "a child psychologist by fiat." The *Zeitgeist* was best represented by the heady and intellectual club at Yale's confident Institute of Human Relations during Sears's years there (1929-1932 and 1936-1942). Sears studied hypnosis with Clark L. Hull, served as a subject in Ernest R. Hilgard's research on classical conditioning, prepared a dissertation on learning in the decorticate goldfish, worked with Leonard W. Doob,

John Dollard, O. Hobart Mowrer, and Neal E. Miller in the presentation of the frustration-aggression argument, and learned the varieties of psychological thinking in his colloquies with Carl I. Hovland and Kenneth W. Spence.

Chance and times came together in Sears's personal devotion to several principles that guided his life work. Never free of his groundings in literature and drama, always open to Pauline Sears's experience as clinical and educational psychologist, Sears for forty years practiced and taught with a rare conjunction of rigor and relevance in psychological research. He entered unexplored territory in developmental psychology with a strong theoretical predisposition (usually drawn from psychoanalysis or the work of Kurt Lewin), and, perhaps most consequentially for his students and his discipline, he insisted on the importance of close direct observation of human action in research settings as natural as he could arrange.

A Fourier analysis of Sears's professional life would reveal three principal components of his work. Two of them—public service in his profession and his original scholarship—show clear peaks. The third, his participation in the creation of settings for research and teaching, has a steadier and more persistent form. George Stoddard roughly defined the Iowa Child Welfare Station; Sears invested it with a commitment to research that joined intricate puzzles—the stability of personality, the variations of early parenting—with tough-minded clean research procedures. In his short stay at Harvard, Sears established a laboratory for the study of human development that, in several transformations, remained a setting for the study of children and families for many years after his departure. It was at Stanford, however, that Sears best worked his skills in the creation of settings. He brought the department of psychology to premier eminence in the world; as dean, he supervised the elevation of Stanford's faculty to renown; he was a moving force in the establishment of the Center for Advanced Study in the Behavioral Sciences; and, in his last years at Stanford, he joined in the design of the university's Boys Town Center for Youth Development.

The curve of Sears's public service to developmental psychology shows two peaks: first, his leadership in the revival and redefinition of the field after World War II, a cycle that was marked by his election in 1951 to the presidency of the American Psychological Association;

then his return to national service in the 1970s, with his leadership of the Society for Research in Child Development and his service as editor of the society's monograph series.

Sears's scholarly work over the years had a surface diversity that masks an underlying simplicity and wholeness. Aside from his student flirtation with Yale's biopsychology (he called his work with Hull on hypnosis and pain "muscle-twitch psychology with a vengeance," and his career as neuropsychologist ended when a dog he was conditioning at Illinois died), Sears's research and theorizing have represented an attempt to maintain the tension of content between biography and technical psychology, the tension of method between clinical methods and precise experimentation.

Sears was the first of the Yale group to stake out the possibilities of translating the principles of psychoanalysis (especially the psychoanalysis of defense) into the principles of contemporary learning theory. His summary of the potential relation between repression and Hull's anticipatory goal response (Sears 1936) was the first of a long series of papers that grew from the Wednesday night seminar that Hull later led at Yale on the connections between learning theory and Freud. Publications on aggression, on conflict, on repression, and, perhaps most seminally, on social learning flowed from the source that Sears had dug; in 1943, he published his *Survey of Objective Studies of Psychoanalytic Concepts*, a review that joined *Frustration and Aggression* (Dollard et al., 1939) and *Social Learning and Imitation* (Miller & Dollard 1941) in the statement of ". . . a remarkably influential set of concepts that were to dominate theoretical formulations in child psychology over the next two or three decades" (Cairns & Ornstein 1979).

With his move to Iowa City and his resultant translation into child psychologist, Sears continued in a new empirical domain his program of hardheaded examination of propositions about crucial events in everyday life. With George Bach, he initiated the use of systematically observed doll play as a window on the thinking of young children. Although the results of the Iowa studies were not as clear-cut as Sears had hoped, they represented his early concern with aggression and dependency in children and, long before it was fashionable, with the early social definition of what it meant to be male and female in the United States. Near the end of his stay at Iowa, Sears gathered

a group of talented young psychologists to address, through interviews with mothers and observations of their children, questions about the social development of human personality. Published in 1953 (Sears et al.), his report of the research is marked, once again, by his mixture of openness and precision, of simplicity of formulation and complexity of problem. The work on social learning in children and on the exchanges between mother and child in early development determined a major line of research for child psychology at mid-century. Reviewing the period between 1946 and 1979, Cairns and Ornstein observed that "the beginning [of the period] is easy [to summarize] because Robert R. Sears can readily be recognized as the person whose influence dominated the applications to child psychology of the original psychoanalytic-learning synthesis for the first half of the period" (1979). And so it was to be at Harvard. With many of the same colleagues, Sears began a much more ambitious study of early rearing patterns and personality in kindergartners that produced *Patterns of Child Rearing* in 1957 and *Identification and Child Rearing* in 1965.

Administrative service at Stanford slowed Sears's scholarship. When he left the deanship, however, he continued in systematic form the studies of Mark Twain (Sears 1976) that well represent the pattern of his life work. The Twain studies had their origins in his undergraduate interest in literature, in his reading of Dollard's *Criteria for the Life History* (1935) in the 1930s, and in his singular conviction that the intricacies of art, like the intricacies of human pathology and of human development, can be clarified, dissected, made to be countable, and brought under the control of simplifying propositions by a careful application of the rules of positive psychological science.

WILLIAM KESSEN

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SELLIN, THORSTEN

Thorsten Sellin was born on October 26, 1896, in the small northern Swedish town of Örn-sköldsvik, to Jonas and Martha Sellin. During his elementary and secondary schooling in Sweden, he became an avid reader and developed an early and abiding interest in history, literature, and modern languages.

At age 17, Sellin emigrated to Fort William, Ontario, with his mother and younger sister, to join his father, who had emigrated to Canada several years earlier. Urged by his parents to pursue his studies, he enrolled in Augustana College, a small Swedish-American institution of higher learning in Rock Island, Illinois, in 1914. Because of his advanced academic standing, he received his B.A. in just three semesters. In 1915, Sellin became a graduate sociology student at the University of Pennsylvania, where he received his M.A. in 1916.

Over the next several years, he continued his graduate studies on a part-time basis at the University of Minnesota while teaching full-time in the secondary schools of Minneapolis. On

June 10, 1920, he married Amy J. Anderson, his Augustana College classmate; they had three sons.

In the fall of 1920, after returning to Philadelphia, Sellin received a Harrison fellowship to resume his full-time graduate studies in sociology at the University of Pennsylvania. He was awarded his PH.D. in 1922; his doctoral dissertation was entitled "Marriage and Divorce Legislation in Sweden." This marked the beginning of his 46-year tenure at the university; he rose from instructor to professor, and served as chairman of the sociology department from 1945 to 1959. Since 1967, he has been professor emeritus.

As scholar and teacher, Sellin is best remembered as a man of extraordinary erudition and scientific rigor, with an exceptional memory and a special talent for languages: Swedish, English, German, and French, with working knowledge of all Romance and Germanic tongues. One of his colleagues remarked that Sellin is "to criminology what encyclopaedists or philosophers were, in their time, to the sciences in general." His scholarly excellence has been given formal recognition in several honorary degrees (University of Leiden, 1960; University of Copenhagen, 1968; University of Pennsylvania, 1968; and University of Brussels, 1979) and in awards that he has received from the Swedish and French governments and from any number of national and international organizations.

Although teaching and research in criminology at the University of Pennsylvania engaged most of Sellin's time and energy, he was, as an activist by inclination and an internationalist by orientation, drawn to other activities at home and abroad. From 1929 to 1968, for example, he served as editor of the *Annals of the American Academy of Political and Social Science*. In 1949 he was elected to the distinguished and exclusive American Philosophical Society where he remains active on its publications committee. He was also an active member of the Osborne Association and served as its president until 1971; a member of the American Society of Criminology; member of the executive board of directors and legislative committee of the American League to Abolish Capital Punishment; chairman of the Pennsylvania Committee on Penal Affairs; member of the Board of Inspectors of the Philadelphia County Prisons from 1939 to 1950; and chairman of the Philadelphia Police Advisory Board (1958-1963).

The importance that he attaches to an internationalist, comparative approach to criminology

is also apparent in his willingness to lecture at foreign universities (e.g., Paris, Brussels, Cambridge, Lund, Stockholm, Uppsala); his participation in various colloquia or special courses for international specialist and student groups (e.g., Paris, 1952; Fuchu, 1978); and his active participation in foreign learned and professional societies (e.g., the Society of Legal History in Paris; the Royal Society of Humanistic Knowledge in Lund, Sweden; and the International Society of Criminology, of which he was president from 1956 to 1965). Special mention should also be made of his long years of service to such intergovernmental organizations as the International Penal and Penitentiary Commission (which he served as secretary-general), the International Penal and Penitentiary Foundation (of which he was president from 1965 to 1971), and the United Nations Social Defense Section which, for example, he served as rapporteur, member, or chairman of the first five ad hoc international committees of experts in the prevention of crime and treatment of offenders.

Concerned that Americans knew little of European research literature in criminology, and vice versa, and convinced of each side's ignorance of the contributions made by the other to penal reform, Sellin spent much time in both Europe and America browsing among the libraries and archives, attending courses and colloquia, giving lectures, meeting other criminologists, and visiting penal institutions. His interest in the history of punishment and the evolution of penal practices became particularly acute, however, toward the end of the 1930s with the publication of a German work by Gustav Radbruch (1938) and a book on *Punishment and Social Structure* by George Rusche and Otto Kirchheimer (1939).

Sellin's works over the years on the history of punishment included the biographies of such reformers as Filippo Franco, Dom Jean Mabilion, Charles Goring, and Zebulon Reed Brockway; and such detailed descriptions of early penal institutions as the Hospice of St. Michael's in Rome, the Walnut Street Jail, and the early Amsterdam houses of correction in his now famous book, *Pioneering in Penology—The Amsterdam Houses of Correction in the Sixteenth and Seventeenth Centuries* (1944). Sellin's fascination with the development of penal practices, however, culminated in a comprehensive book on *Slavery and the Penal System* (1976), but his interest extended far beyond the chronicling of political, social, economic, and institu-

tional forces that molded and determined the character and form of punishment. He is particularly concerned with the nature and extent of social progression in the area of penal policies and with assessing their effectiveness in light of the objectives they are meant to achieve. He has written "Trends in Penal Treatment: Have We Overdone Punishment?" (1948), "A Quarter Century's Progress in Penal Institutions for Adults in the United States" (1933), "Prison Tendencies in Europe" (1931*b*), and "Correction in Historical Perspective" (1958*a*).

In the area of the efficacy of punishment, Sellin's best-known works, and those that have probably had the most direct effect on public policy, are his writings and testimony concerning the ineffectiveness of capital punishment. Particularly noteworthy is his memorandum on "The Death Penalty and the Problem of Deterrence" submitted to the Royal Commission on Capital Punishment (Great Britain) in 1951, his 1954 testimony before the joint committee of the Canadian Parliament on capital and corporal punishment and lotteries, his report on *The Death Penalty* prepared for the American Law Institute in 1959, and the various articles on capital punishment he has written since that time. In each of these works he has maintained his conviction, supported by statistical and experiential evidence, that capital punishment, as it is presently applied, is neither an effective deterrent to murder nor even an effective instrument of retributive justice. Sellin's most recent work on the subject was a lecture on general deterrence which he delivered in January 1979 to the Institut des Hautes Études in Brussels.

During his career, Sellin has devoted considerable time and energy to an area that is of great importance in the advancement of the science of criminology: criminal statistics. Since 1931, when he first became consultant and special agent to the U.S. Census Bureau, then responsible for the national criminal statistics program, Sellin has made numerous and significant contributions to the development and improvement of criminal statistics on a local, state, national, and international level. His belief in the importance and utility of criminal statistics and their necessity for measuring changes over time in the volume, trend, and character of crime are evident in his articles on "The Basis of a Crime Index" (1931*a*), "The Importance of Criminal Statistics" (1936), and "Criminal Statistics—An Urgently Needed Resource" (1967).

His efforts to assist in the development and

maintenance of a comprehensive national criminal statistics program in the United States are demonstrated in the Uniform Criminal Statistics Act which he drafted in 1944 at the invitation of the National Conference of Commissioners on Uniform State Laws (ultimately approved by the conference and the American Bar Association in 1946 and endorsed for adoption in all states by the Law Enforcement Assistance Administration of the U.S. Department of Justice in 1969). His continuing efforts to accelerate progress in this area and to demonstrate the deficiencies of the current statistical programs at the state and national levels are best illustrated in such articles as "The Status and Prospects of Criminal Statistics in the United States" (1958*b*) and in "Problems and Prospects of Criminal Statistics in the United States" (1954).

For almost half a century, Sellin has actively supported the creation of effective state bureaus of criminal statistics and the establishment of a single federal agency to assume responsibility for the collection, collation, and reporting of national comparative criminal statistics. Toward this end, he has served on a number of state and national committees and commissions and has provided expert testimony on the subject in 1968 to a subcommittee of the House of Representatives Committee of the Judiciary. In 1977, Sellin was again asked to assist the government by reviewing its draft plans for the establishment of a Bureau of Justice Statistics and is currently prepared to testify once more on the subject before a committee of the Congress.

It is in the area of the measurement of crime, however, that Sellin has made what is popularly regarded as one of his principal contributions to the advancement of the social sciences. The Sellin-Wolfgang index represents the culmination of years of thought regarding the difficulties in measuring the nature and extent of criminal behavior (Sellin & Wolfgang 1964). Some of these difficulties and the inherent problems encountered in the development and use of a crime index were carefully illustrated and examined by Sellin in "The Measurement of Criminality in Geographic Areas" (1953), "The Basis of a Crime Index" (1931*a*), and "The Significance of Records of Crime" (1951*b*). The Sellin-Wolfgang index, and its application in their later book, *Delinquency in a Birth Cohort* (Wolfgang, Figlio, & Sellin 1972), constituted a significant departure from earlier measurement techniques and a major contribution to the development of new and sophisticated research

techniques for measuring the volume and character of crime, identifying the characteristics of offenders, and evaluating the effectiveness of efforts to prevent and control crime and delinquency.

Another of Sellin's significant contributions was in social theory, particularly in his monograph on *Culture Conflict and Crime* (1938).

Between 1933 and 1936, Sellin served as a director of the Social Science Research Council, an organization interested in investigating the etiology of crime from a number of different perspectives, including the effects of such social crises as war (Sellin 1942) and economic depression. The council was particularly interested in the interrelationship between an individual's personality, as reflected in his behavior, and his cultural environment, and in 1935 Sellin and his colleague, Edwin H. Sutherland, were invited to form a subcommittee on delinquency of the committee on personality and culture to investigate the question. After considering a number of alternatives, the subcommittee focused on the problem of culture conflict and its role in the causation of crime.

As it turned out, however, the monograph, *Culture Conflict and Crime*, was much more than an elaboration of the culture conflict thesis then already known and widely discussed among sociologists. It was, perhaps more importantly, a treatise on the need for and requirements of a science of criminology and a scientific basis for the study of crime.

The importance of Sellin's work on culture conflict and crime has been widely recognized and acclaimed on an international scale by most prominent contemporary authorities. It has served as the basis for much of the sociological research and conceptualization, particularly in regard to criminal and delinquent subcultures, that has emerged in the forty years since its publication, and has totally changed the face of traditional criminology by expanding the field to embrace what is now commonly known as the sociology of deviance. It has also provided the path for pursuit of a science of criminology.

Sellin is in semiretirement at his home in Gilmanton, New Hampshire, where, with the aid of his enormous (more than ten thousand volumes) private library he continues to engage in research and to prepare books and articles for publication.

MARVIN E. WOLFGANG AND
LENORE KUPPERSTEIN

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SHELDON, WILLIAM H.

William Herbert Sheldon was born in 1898 in Warwick, Rhode Island, and died in 1977 at his Biological Humanics Center for Research in Human Constitutional Variation in Cambridge, Massachusetts. While known professionally as a constitutional psychologist, he was every bit as much a philosopher of the pragmatic school, who was destined to be in conflict with the main stream of American psychology.

Sheldon's first book, *Psychology and the Promethean Will* (1936), was a philosophical as much as a psychological treatment of the sources of conflict and the role of religion in modern life. This book was presented with the optimistic proposition that psychology would in the course of time prove useful (Sheldon et al. 1940). The book justified this optimism, exemplifying one of the principal tenets of pragmatism, that the truth of a proposition is measured by its practical outcome, as well as supporting the view of the great pragmatist William James that in vital matters of faith the criterion for acceptance is the will to believe. This book, written while Sheldon was in psychiatric study abroad on a fellowship from the National Council on Religion in Higher Education, provided the philosophical foundation for Alcoholics Anonymous (Bill Wilson, cofounder of AA, personal communication, 1954).

Sheldon's philosophies and the unique intellectual path he followed throughout his life clearly built upon early influences. His first teacher was his father, to whom he was very close and about whom we know little other than that he was a naturalist, hunting guide, professional judge of hunting dogs, and apparently a close friend of William James. How else at the age of 56 would William James have become Sheldon's godfather? While James died when Sheldon was only 12 years old, he has to be viewed as Sheldon's second teacher. In many respects, Sheldon's life interests were modeled on those of James and his protégé Martin Peck, whom James sent to study with Sigmund Freud. As an undergraduate at Brown University, Sheldon traveled to Harvard Medical School to attend a seminar by Peck: "He considered Freud the foremost emancipator of mankind but emphasized that the job was still only half done; that somebody now must bring descriptive order to comprehending the constitutional patterns underlying the psychiatric patterns" (Sheldon, Lewis, & Tenney 1968, p. 848). In essence,

Sheldon received his mission in life from Peck. How he was to pursue this mission had already been determined by his earlier orientation and training.

Sheldon's cousin, George Arnold, was a well-known Providence numismatist and coin dealer. By the age of 12, Sheldon was a recognized numismatist, traveling as far as Philadelphia on the senior circuit of numismatics. He was fascinated with the classification of coins, and in the years ahead collected and then sold outstanding coin collections for the purpose of advancing his education and carrying out his research in constitutional psychology. He also wrote two of the most important books in numismatics ever written, *Early American Cents* (1948) and *Penny Whimsey* (1958). Much of his precision in description and classification of the human body must have had its beginnings here. What he wrote about coin collecting also reveals another important side to this complex man. He was a great romantic: "We always live in a valley lying between the nostalgic past and unknown future. To own a family of the early cents is in some measure to command a causeway between the past and the future" (Sheldon 1948). He was also an authority on and had one of the most extensive collections of literature on the Knights of the Round Table and the Age of Chivalry.

Most important was the orientation he received as a child from his father as a naturalist and as a judge and classifier of living things. He was an authority on the classification and natural history of moths. This bent and its importance to his classification of human physique and temperament in works published in 1940 and 1942 respectively is seen in his explanation of the somatotype concept:

... the somatotype offers an early and rather crude tool fashioned to reflect a basic structural orderliness which can be perceived in human life. Despite the confusion now rampant in the social interrelationships of the species, underlying the social chaos is a matrix of organic order, which is to say, of beauty and truth. We are still being born into a world of such beauty as staggers imagination and beggars speech, and the physical reality that a man carries the stamp of truth even when the beauty is obscure to perception. In such a world it seems reasonable to suppose that there should be a way of so truthfully reflecting a man's structural self that the reflection will blend with the continuum of order like faint music. The somatotype is therefore a groping for a reflection in man

of the orderly continuum of nature. (Sheldon et al. 1954, p. 3)

We see the naturalist, the romantic, and the philosopher whose first book was *Psychology and the Promethean Will* (1936) and last was *Prometheus Revisited* (1974).

Sheldon received an A.B. from Brown in 1918, an A.M. from the University of Colorado in 1923, and from the University of Chicago a PH.D. in 1925 and an M.D. degree in 1933, thus acquiring what he referred to as the tools of the trade and union cards. Between 1925 and 1933, he practiced his trade of psychology by teaching at the University of Texas, the University of Wisconsin, and the University of Chicago. It was after this period that he went on his traveling fellowship from 1934–1936, bringing his individualism into full bloom, and received great stimulation from Carl Jung and Ernst Kretschmer. It was while in Darlington, England, living with Leonard Elmhirst that he and his close friend Gerald Heard met Bill Wilson and influenced the philosophical basis upon which AA was founded.

He returned to the position of professor of psychology at the Chicago Theological Seminary. By 1938, Sheldon became a full-time researcher, first at Harvard University, where his contacts with Ernest Hooton and S. S. Stevens were important to the technological development of his classifications of physique and temperament (Sheldon et al. 1940; 1942). At this time he also established two of his most important friendships and intellectual relationships that were to last for the rest of his life. These were with Nolan Lewis and Emil Hartl. His tireless, almost relentless, research and data collecting saw him in research positions at Columbia's College of Physicians and Surgeons, the University of California at Berkeley, the University of Oregon, with his friend Nolan Lewis, and Rockland State Hospital in New York, with one of his strongest and most tolerant supporters in American psychiatry, Nathan Kline. One of Sheldon's most fortunate associations was with Eugene McDermott, a founder of Texas Instruments. McDermott not only provided financial support for Sheldon's research after 1948, but also contributed importantly to the objectifying of the somatotyping technique and its final modification by about 1966 (Sheldon, Lewis, & Tenney 1968).

Throughout his life, Sheldon was an individualist and perhaps too frequently wore it like a

badge. He rejected publication in refereed scientific journals for he saw them as the organs of prosaic minds and structured to protect the "establishment" from the threat of original thought. He was intolerant of those he viewed as slow-witted or pompous. He called everyone "Doctor," using as his definition the old-fashioned term for a turkey-feather duster. In his later years he managed in this way to isolate himself from all but his closest and most dedicated friends and alienated many of those actually necessary to the promotion and practical application of his ideas.

His terms endomorphy, mesomorphy, and ectomorphy for the three components of physique have become household words, but the evidence is not yet in on his formulation of the unity of his morphologic and psychiatric components. Did he succeed in the mission he set for himself in Martin Peck's seminar: putting descriptive order to Freudian theory? In time we will know. Those who remained close to him (Emil Hartl, Dorothy Pascal, Edward Monnelly, and Roland Elderkin) are putting this evidence together and will share it with the scientific community as it had become impossible for Sheldon to do. But this much is certain. *Psychology and the Promethean Will* went as far as anything written in the mid-twentieth century to make psychology useful. More than half a million members of AA in the United States alone have been helped to cope more successfully with the stress of modern life because of the work of this man.

RICHARD H. OSBORNE

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SHERIF, MUZAFER

Muzafer Sherif was born in Turkey in 1906. He received an M.A. (1929) from the University of Istanbul, and continued his graduate studies at Harvard University (M.A., 1932) and Columbia University (Ph.D., 1935). Following publication of his first treatise on *The Psychology of Social Norms* (1936), he accepted teaching positions in Turkey. In 1945 he returned to the United States, and completed the research that resulted in a systematization of social psychology. For this enterprise, Sherif attracted several talented coworkers, not the least of whom was Carolyn Wood, whom he married in 1945.

Sherif's lifelong motivation was to help establish a science in the social sciences. According to Sherif, social psychology, the scientific study of experience and behavior in sociocultural settings, had a critical contribution to make in establishing a science of man. Individuals, with their processing capabilities, are not only affected by the sociocultural properties of the groups into which they are born, in which they are raised and function, and to which they relate through time and space, but are also the makers of culture and historical events—war, peace, and revolution. Thus, to understand human events, it is necessary to understand the properties and processes by which *Homo sapiens* reacts and responds to his entire environment, but especially to his sociocultural environment.

Influenced by gestalt psychology in perception, and by those aspects of psychophysics concerned with scaling and judgment, Sherif went on to “apply” (sometimes making his own contributions) the principles of cognition to understanding individuals in societies. A conception of the properties of societies and cultures was derived from the naturalistic work (of the 1930s and 1940s) in anthropology and sociology as well as from older works in the social sciences (e.g., the work of Bronislaw Malinowski), which attempted to define problems carefully while

tying generalizations to supportive observations and other data.

Sherif derived a view of the program of science from such naturalistic works in the philosophy of science as Einstein and Infeld's *The Evolution of Physics* (1942). According to Sherif, scientific methods and generalizations were to be formulated in the process of knowing phenomena. To avoid ethnocentric bias, the scientist had to gather findings from the study of a variety of cultures and societies and had to be concerned with how (rather than what) individuals perceive, remember, judge, and think, and how groups evolve and change. From such directed study—seeking to isolate and understand process in a variety of situations—it was possible to generalize and attempt to formulate invariant lawful relations which, like those in physics, would constitute a set of timeless hypotheses.

His earliest work, *The Psychology of Social Norms* (1936), concentrated on making limited generalizations about times of social transition where “the transition is not simply from the orderliness of one set of norms to chaos, but from one set of norms to a new set of norms, perhaps through a stage of uncertainty . . .” (p. 86). It dealt with universal phenomena (all known groups have norms which regulate social life) with the aim of formulating a verifiable scientific hypothesis concerning the conditions which require that new norms emerge by psychologically deterministic processes (*ibid.*). It was well received in part because Sherif, in a laboratory experiment that has been replicated in one form or another around the world, was able to show that under certain conditions individuals in interaction with one another do formulate a norm that regulates their behavior when they are apart from each other. Fundamentally, Sherif hypothesized that because there is a psychological tendency to reduce uncertainty, individuals, if allowed to interact, are bound to influence one another and formulate a norm (that will not be merely the “average” of their separate judgments from previous experience with the same situation), which subsequently will influence their own separate behavior. The very idea of using the laboratory to verify a hypothesis derived from the study of large-scale social phenomena created much excitement among those researchers in sociology and psychology who had confidence in the liberalizing force of science.

Sherif proceeded in a series of experiments and books to make contributions to every im-

portant aspect of social psychology. Repeatedly he followed the paradigm first used in the study of norm formation. Generalization concerning sociological structures was always based upon empirical work and historical reports. Generalizations concerning culture were made from the reports of field studies and from the work of linguists and other scientists. The generalizations were always subject to verification, sometimes by constituting abstract experiments such as the one employed to verify the conditions for norm formation, sometimes by formulating more "real-life" experimental situations, and sometimes by employing observational methods in field experiments which had all the complexity associated with sustained human interactions.

It was in the field studies that Sherif applied his methodological paradigm to "real-life" interactions. Until these studies were undertaken the essential abstracted features of sociological conditions were simulated in the laboratory. For example, a phase of a sociocultural process, such as "a period of uncertainty," was represented by an unstructured task, and differential status was manipulated by representing the views of high to low prestige sources. However, in the Robbers Cave study (Sherif et al. 1961) on the formation of groups and the establishment and resolution of intergroup conflict, Sherif used laboratory findings to verify the existence and change of sociological conditions. After a state of established intergroup conflict (for intergroup conflict is also governed by norms or conventions) was made to exist through the introduction of competitive events, laboratory results were used to verify that in- versus out-group relationships affected cognition. Thus, laboratory findings, like a series of snapshots, were used to confirm that sociocultural processes were in motion and/or to confirm that some steady state, such as established conflict, had been achieved.

In all his works, Sherif never sacrificed significance for experimental rigor in a mechanical sense. His experiments were always important, and the hallmark of his designs involved setting up situations where there were a great variety of ways to disconfirm but only one way to confirm a hypothesis. Although Sherif was influenced by probabilistic thinking, he only occasionally found the opportunity to apply statistical tests to his data. Usually his experimental data, even with a small number of subjects, were overwhelmingly conclusive even without statistical evaluation.

Although Sherif devoted a great deal of effort

to the verification of hypotheses in the social sciences, he contributed very few sociological generalizations. However, one generalization derived from his study of intergroup conflict has had a lasting impact upon the social sciences. He hypothesized that a series of superordinate goals—objectives desired by all but beyond the resources and capabilities of any one group—is required to reduce intergroup conflict, since one superordinate goal by itself is not sufficient to bring enough individuals belonging to separate groups into a set of novel interactions. It is in novel situations, for which there are no established intergroup mores to govern behavior, that new (and presumably favorable) attitudes toward individuals in various groups can be developed. In turn, these new attitudes eventually make older ones inapplicable, since there is a tendency for humans to resolve conflicts between attitudes in order to be consistent in response to others.

Sherif viewed the ego or self as a developmental subsystem of interrelated attitudes in the individual's make-up. The self-construct was used to account for consistency in behavior, and because ego attitudes are not necessarily integrated into a coherent system, it is possible to account for a variety of conflicts including, for example, role conflicts and conflicts with oneself over matters of personal consistency.

Apart from aiding in the formulation of a verifiable theory of self, Sherif made several contributions to the theory of attitude formation and change. Borrowing heavily from a body of findings in psychophysics concerning scaling and judgment, Sherif hypothesized the means by which individuals organize attitudes or reference scales. In a series of experiments he verified the structure of such scales by predicting responses to communications in accord with and at variance from an individual's own point of view. In addition, processes of attitude change were verified.

In all his work on attitude formation and change, Sherif employed psychological constructs concerning the processes of perception and judgment which he hoped would continue to be shown to apply universally. Rather than relying upon particular mechanisms learned in given cultures for resolving or managing conflict, Sherif attempted to formulate a theory that would allow for such learning but which also allowed individuals alone and in interaction to invent an infinite number of alternative ways to resolve conflict. Thus, his theory of attitudes and self reflects his persistent belief that it is

possible to formulate deterministic laws concerning the *processes* by which individuals learn and form percepts, judgments, norms, and self-concepts, but not the *nature* of the norms, values, or products of cognition.

Gardner Murphy ([1929] 1972) noted in his *Historical Introduction to Modern Psychology* that Sherif's definition of social psychology in cognitive terms had profound implications. Not only were the long-standing methods of experimental psychology brought successfully to bear upon understanding the behavior of individuals in social situations, but the psychological laboratory was also made significant, for it became the major (experimental) means of verifying how man is affected by and transforms culture.

BERTRAM L. KOSLIN

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SHRYOCK, RICHARD H.

Richard Harrison Shryock (1893–1972) was in the front rank of American historians in the use of social science concepts and methods. His scholarship and research, expository and interpretive in character, illuminated relationships between time, environment, and culture on a broad historical canvas. He delineated the historical development of medicine by showing how it interacted over time with psychological, social, and economic factors. By actively participating in committees of professional organizations and advisory agencies, he made his specialized knowledge available when technical and social questions were under consideration.

Personal circumstances explained Shryock's varied interests in historical scholarship, academic statesmanship, and, especially, the history of medicine. Financial problems thwarted his hope of becoming a physician. With a degree in education from the University of Pennsylvania he taught in Philadelphia public schools. This early experience contributed to his constructive and enduring interest in education on every level. As a private at the Army Medical School in World War I he began his self-education in medicine, and deepened his conviction concerning the importance of health and illness for both individuals and society. As a candidate for the Ph.D. in history at the University of Pennsylvania he proposed writing a dissertation on the development of public health in the United States. Though encouraged by the department's European and English historians, William E. Linglebach and Edward P. Cheyney, Shryock was told by the American constitutional historian, Herman V. Ames, that the topic was unsuitable. Choosing instead to investigate Georgia's relations with the federal government in 1850, he received the Ph.D. in 1924 and two years later published his dissertation (1926). In this research his initial interest in public health was whetted by using the manuscripts of Dr. Richard D. Arnold, the first secretary of the American Medical Association and mayor of Savannah. Later, while teaching history at Duke University (1925–1938), he learned a good deal from his colleagues in the medical school.

Shryock's interest in the history of medicine developed at an opportune time. He was encouraged by Dr. William H. Welch, founder of the Institute of the History of Medicine at the Johns Hopkins University, and by the director who succeeded him, Dr. Henry E. Sigerist. In contrast with traditional work in the field, largely

written by practitioners in biographical and technical or "internal" terms, Welch and Sigerist took cognizance of environmental, social, and humanistic influences on medical developments.

Shryock's edition of the *Letters of Richard D. Arnold, M.D.* (1929a), together with carefully researched articles on the origins of the public health movement (1929b), on medicine in the Old South (1930a), and on the course of public relations of the profession in Britain and America (1930b), established his credentials for lecturing on the history of medicine at Duke. His preparation continued with the study of the sociology of knowledge and with the history of medicine during a research leave in Germany in 1932/1933. Two years before leaving Duke for a professorship of history at the University of Pennsylvania, his pathbreaking synthesis, *The Development of Modern Medicine* (1936a) was published. Its subtitle, *An Interpretation of the Social and Scientific Factors Involved* indicated its distinctive character. The book won praise from prestigious physicians and distinguished historians, though the historical profession as a whole recognized its importance only after its publication in England and in Germany.

In Shryock's view, the history of medicine involved everything that affected the maintenance and improvement of individual and public health. Thus *The Development of Modern Medicine* and monographic contributions that followed took account of the various factors involved in the "logic" or "internal" aspects of medicine and the developing sciences related to it. In both the book and the articles Shryock also probed social factors affecting the transition from guild to profession; medical education and practice, and the psychological and economic interactions between doctor and patient; rivalries and cults; and the challenge of popular health nostrums and fads to the prestige and profits of the profession. In subsequent works he explored the role of tradition, authority, and the innovations affecting distinctive achievements in Britain, Germany, France, and Italy. Giving due credit in still other articles to American innovators (1936b; 1945; 1946a; 1953), he offered explanations for the relative paucity of basic research in nineteenth-century America (1948). Shryock further assessed the influence of such movements of thought as democracy, humanitarianism, laissez-faire, philanthropy, social control, Marxism, the body-mind issue, vitalism, Freudianism, and especially, rationalism and empiricism on technical developments and con-

troversies in medicine (1969). Above all, he documented interactions between medicine and public health with such social factors as race (notably slavery), class structure, sex roles, industrialism, wealth and poverty, urbanism, technological contributions to the measuring instruments in clinical practice, improvements in communication, and the development of medical libraries and journals. He also emphasized the role of such social institutions as teaching hospitals, research centers, sanitary commissions, licensing boards, and nonofficial agencies for spreading the practice of immunization, improved health care, and sanitation (1929b; 1957; 1967). His interest in the "state of health" led to critical assessments of available vital statistics (1946b). Shryock's broad frame of reference, well-documented investigations, disciplined insight, precision in the use of terms, and regard for chronology resulted in important generalizations. For example, he concluded that "by the end of the 1800s, medical success was *about* to reduce mortality and to increase life expectancy on a mass scale. For the first time in history, medicine was destined—for better or worse—to exercise a major influence upon society" (1959b, p. 146).

In 1949 Shryock resigned his professorship of history at the University of Pennsylvania to become director of the Institute of the History of Medicine at the Johns Hopkins University. In broadening the institute's program, by emphasizing the history of the sciences and the relation of the history of medicine to social science, its influence was extended both in the United States and abroad. Meantime, he also prepared papers and monographs on women in American medicine (1950), the role of Cotton Mather as "the first significant figure in American medicine" (1953), the National Tuberculosis Association (1957), and the history of nursing in its social aspects (1959a). His interest in Benjamin Rush continued until the publication of his last notable paper on Rush's changing reputation during his life and long after his death (1971).

Having reached the age of retirement at the institute in 1958 he returned to the University of Pennsylvania where he taught social and cultural history, promoted interdisciplinary programs in American civilization, trained able and devoted students, lectured on the history of medicine at several institutions, and continued to publish, largely in the American field. Always concerned with the discovery and accessibility of primary sources, as librarian of the American

Philosophical Society for several years, he expanded its holdings in the history of science and medicine.

Convinced that social scientists should contribute their specialized knowledge to public use, Shryock actively participated in committees and organizations. His persuasiveness and judiciousness increased his influence in convincing scholars that the problems of sickness and health needed to be brought into the mainstream of historical knowledge. As a member of the New York Academy of Medicine's Committee on Medicine and the Changing Order, he took a leading part in preparing the report describing the committee's findings (New York Academy of Medicine 1947). Both here and in his work on committees of the National Science Foundation, the Russell Sage Foundation, and the National Institutes of Health, Shryock showed how knowledge of medical history, if taken seriously, could help avoid mistakes. His retrospective view made plain the need of the cooperation of all interests in policy formation and of avoiding efforts to find quick and easy solutions to complex questions. In terms of American contexts and experience he indicated in what ways European efforts to provide health care insurance were relevant or irrelevant. He was sure that there were neither simple causes nor simple solutions for complex problems.

Shryock's interest in the problems of education in general and of academic life in particular was no less marked. His intensive study of the current problems of the University of Pennsylvania was notable for its acumen, fertile suggestions, and statesmanship (1959c). As president of the American Association of University Professors in 1951/1952 he vigorously upheld academic freedom at a critical time and evaluated the strengths and weaknesses of the academic profession on both sides of the Atlantic (1952; 1961b). His presidency of the International Association of University Professors and of the International Association of the History of Medicine testified to the widespread appreciation of his scholarship and understanding of academic issues.

The problems of historiographical theory in relation to philosophy and social science also engaged Shryock's active interest. As a member of the Social Science Research Council committee on historiography, he drew on his broad and detailed knowledge of American historical writing (1943a) in helping resolve controversies over "causation," "presentism," and "relativism,"

and in suggesting ways of bringing history into more fruitful relations with the social sciences (1946b). Having always used statistics in his own work, he organized a conference on quantification in 1960. His report assessed the contributions of quantification to the history of science and medicine and urged that its limitations in history and the social sciences should not discourage efforts to extend the boundaries of its use (1961a).

Among American historians Shryock was a pioneer in the use of the culture concept and in bringing precision to the comparative method. He proposed the contextual analyses of slavery in the South, Brazil, and the West Indies that other scholars undertook. He showed the value of microscopic studies of local history (1939b), and in broad comparative approaches he revised accepted views about the reputations of the major regions of the United States for achievements in literature, science, and medicine (1940; 1943b). His studies of the Pennsylvania-Germans as a subculture clarified perplexing problems involving "self-identity" and intercultural perceptions (1939b; 1942). Even more telling was his comparison of the agricultural practices and achievements of Pennsylvania-Germans and their English-speaking neighbors: Since the soil in contiguous areas might be regarded as a constant, success or failure seemed to result from different cultural values and customs. The Pennsylvania-Germans rotated crops, nurtured the soil, displayed high motivation, frugality, and willingness to forego immediate returns for long-range satisfactions, whereas English-speaking tobacco farmers, with little agricultural experience, exhausted the soil in quick-paying one-crop planting and in careless cultivation. Precise figures on yields per acre, degree of soil exhaustion, and costs of slave and of free labor, indicated the feasibility of manipulating factors in a way roughly comparable to methods in a laboratory experiment (1939a).

Shryock modestly claimed no originality for his scholarly innovations. He offered fresh and provocative hypotheses. His lucid, urbane prose was sprinkled with wit. He was a major figure in showing that medicine and public health, viewed historically, were social functions and in bringing the history of medicine, broadly viewed, into the main current of the social and cultural aspects of developing civilization. In doing this and in examining other aspects of American culture and historiography, he promoted by precept and example the growing rapport of his-

torical theory and practice with the social sciences.

MERLE CURTI

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SKINNER, B. F.

B(urrrhus) F(rederic) Skinner, born March 20, 1904, in Susquehanna, Pennsylvania, and since

1974 professor emeritus in the department of psychology and social relations at Harvard University, is one of the most influential, controversial, and publicly visible of living American psychologists ("Skinner's Utopia . . ." 1971, p. 47; Goodell 1977, pp. 4ff., 106-119). A biophysicist and a pharmacologist have each called him "great," the former stating that "Skinner may have had the worst press of any great scientist since Darwin" (Platt, as quoted in Goodell 1977, p. 107); and the latter that he was "one of the great men of our times. . . . Skinner's discoveries in the field of the transactions of a higher organism with its environment will have a greater and more enduring effect on man's view of himself than the views of Freud" (Dews 1970, pp. ix-x).

Skinner's many publications (Epstein 1977) pertain to four areas of major concern to the behavioral and social sciences: (a) philosophical and theoretical analyses of behavior and behaviorism; (b) experimental analyses of behavior in controlled environments; (c) technology of behavior analysis and modification based on procedures and laws abstracted from his philosophical, theoretical, and experimental analyses; and (d) behavioral analyses of cultural practices.

A life goal and a method for its fulfillment. The son of a lawyer, Skinner described his life from 1904 to 1928, when he entered graduate school, chronologically in *Particulars of My Life* (1976a) and topically in a brief autobiography (1967). During high school and college, Skinner repeatedly strengthened his resolve to make his life goal the furthering of the understanding of human behavior. By 1922, when he graduated from high school, he had read widely and written many creative works under the guidance and inspiration of Mary I. Graves, "my teacher in many fields for many years" ([1967] 1970, p. 2). Skinner credited Graves for his decision to major in English in college, to try to be a writer, and to dabble in art (*ibid.*, p. 3).

From 1922 to 1926 at Hamilton College in Clinton, New York, Skinner studied literature and philosophy in order better to understand human behavior. There he was briefly exposed to psychology, the discipline he later chose as his specialty. A biology teacher taught him advanced biology, dissection, and introduced him to Jacques Loeb's *Physiology of the Brain and Comparative Psychology* and *The Organism as a Whole* (Skinner 1976a, p. 295); from a philosophy teacher he learned the two-point

limen and some Wundtian psychology (*ibid.*, p. 292).

After receiving his A.B. (1926), Skinner tried writing as a career for approximately 18 months. After reading an H. G. Wells article contrasting the works of Ivan P. Pavlov and George Bernard Shaw in the November 1927 *New York Times Magazine*, he decided "to abandon literature and turn to psychology" (*ibid.*, p. 300). He then applied to Harvard University and was accepted as a graduate student in the department of psychology in the fall of 1928.

Skinner had acquired the skill of self-observation by reading the literary work of Marcel Proust (*ibid.*, p. 296). He had also developed a new orientation to philosophy and animal behavior from reading popular articles as well as Bertrand Russell's *Philosophy* (1927) (1976a, p. 298ff.). Skinner noted that Russell: (a) repudiated the philosopher Immanuel Kant; (b) termed John B. Watson's behaviorism a philosophy and a desirable method; (c) arrived at facts rapidly; (d) like Watson, assigned importance to Pavlov's principle of the conditioned reflex by the presentation of two approximately simultaneous stimuli; and (e) again like Watson tried to interpret Edward L. Thorndike's law of effect as an example of stimulus substitution. Years later (Skinner 1938) he corrected what he then termed Russell's and Watson's error in interpreting Thorndike's work and abandoned "the unproductive path of stimulus-response psychology" (Skinner 1976a, p. 299). In the meantime, Russell's work inspired him to read Watson's *Behaviorism* (1925) and Pavlov's *Conditioned Reflexes* (1927).

Pre- and postdoctoral training. At Harvard University, Skinner immediately placed himself on a strict daily work schedule with severely curtailed activities. He adopted an independent approach to graduate study (Skinner 1967; Keller 1970), but did make some important social contacts. Two fellow graduate students, the behavioristically sophisticated Fred S. Keller (Keller & Schoenfeld 1950; Keller & Sherman 1974) and the writer turned psychologist, Charles K. Trueblood, rather than faculty members, strengthened what Skinner called his "hodge-podge of interests" ([1967] 1970, p. 9). During his first two years of graduate study, he also met the philosophers William Ernest Hocking and Alfred North Whitehead, learned of Percy Williams Bridgman's *Logic of Modern Physics* (1927), read Ernst Mach, Henri Poincaré, and immediately relevant works in psy-

chology and physiology (Skinner [1967] 1970, p. 10), coauthored one article, and authored two articles (Epstein 1977, p. 103).

As a doctoral dissertation proposal, Skinner combined a Machian–Russellian analysis of the reflex concept (Skinner 1931) with analyses of drive, reflex strength, and data on eating behavior (Skinner [1967] 1970, p. 10). Although Edwin G. Boring rejected the proposal, a committee without Boring approved it. Skinner passed his orals and received his PH.D. from Harvard in 1931.

He stayed on at Harvard for five postdoctoral fellowship years, gathering data largely on rat behavior and writing data and theoretical papers. During his first postdoctoral year he worked in part on a study of the central nervous system with Alexander Forbes and Hallowell Davis and in part on animal behavior using his own equipment in space provided by W. J. Crozier. In 1936 he moved as an instructor to the psychology department of the University of Minnesota. There he first learned and taught college level introductory psychology. Classes were small but Skinner was proud that five per cent of his students went on to obtain the PH.D. in psychology, among them William K. Estes and Norman Guttman (*ibid.*, p. 11).

Parallels in Skinner and Mach. Skinner's approaches to the science of organismic behavior and to the philosophy guiding it parallel in many ways Ernst Mach's (1883) approaches to the science of physical mechanics. To understand the full import of Skinner's major systematic works and his place in Western science, therefore, it is essential to indicate these parallels:

(1) *Method of historical analysis.* In his first major theoretical publication, a paper on the concept of reflex, Skinner (1931) deliberately adopted Mach's method of historical analysis of fundamental scientific concepts. Skinner started with Descartes' original definition of the reflex, indicated the role of accident in its discovery, then documented its falls and rises in favor, its accretions and losses, and ultimately redefined it in terms of its one continuing positive aspect—"the observed correlation of two events, a stimulus and a response" ([1931] 1972, p. 457). Experimental analysis then could proceed along two lines: the study of correlational characteristics such as latency, and the study of effect of third variables such as drive (food deprivation) level. Thus, Skinner's historical analysis of a behavioral concept, like

Mach's historical analysis of Newton's force, generated a new way of looking at an old concept. Menger (1960) notes that even contemporary mathematics and physics could profit from presentation "*à la Mach*," thereby indicating the power and contemporaneity of Mach's method of historical analysis.

(2) *Method of critical analysis.* In his paper on the reflex concept, Skinner (1931) ultimately retained only observable properties, rejected unobservable ones, and then assigned reality status to correlations completely on a par with the reality status others had assigned to structures presumed to intervene between stimulus and response (cf., Russell 1927, pp. 114ff.; Item 4 below).

Mach's (1883; Menger 1960) own use of the method of critical analysis led to antimetaphysicalism and positivism in philosophy, to Albert Einstein's relativity theory, and to Bridgman's (1927) operationalism in physics, and through Bridgman to the Stevens–Boring operationalism in psychology (cf., Mackenzie 1977, p. 187).

Skinner (1945*b*), speaking and writing with the authority of one who had drunk from the original Machian fountain of knowledge, asserted that operationism was not "a new theory or mode of definition" ([1945*b*] 1972, p. 370), but was simply "the practice of talking about (1) one's observations, (2) the manipulative and calculational procedures involved in making them, (3) the logical and mathematical steps that intervene between earlier and later statements, and (4) '*nothing else*'" (*ibid.*).

(3) *Rule-governed behavior compared with contingency-controlled behavior.* Experimental analysis of contingencies of operant reinforcement, according to Skinner (1966, p. 13), may be viewed as an objectification of purpose, and the resultant behavior purpose-governed in an objective sense. Natural contingencies, however, may be formulated into rules including scientific laws. These "rules [and laws] can usually be learned more quickly than the behavior shaped by contingencies they describe" (Skinner 1974, p. 125).

Mach's version of a similar idea, "the economy of science," is generalized as the aim of science. He states that "it is the object of science to replace, or *save*, experiences, by the reproduction and anticipation of facts in thought. Memory is handier than experience, and often answers the same purpose" ([1883] 1960, p. 577).

(4) *Acausal functional analysis*. Skinner's *magnum opus*, *The Behavior of Organisms* (1938), is, like Newton's *Principia . . .* (1686–1729), a difficult book for many to read and understand because Skinner, like Newton, takes a functional or relational, as opposed to a simple causal, approach to his subject matter. Newton's functional analysis was communicated textually, graphically, and mathematically, while Skinner's was communicated only textually and graphically.

The eminent psychologist, Clark L. Hull (1943), deliberately modeled his theoretical efforts, in part, on Newton's *Principia* (Stanley 1965; personal communication to author, 1943). However, he erroneously assumed Newton's method was bold theoretical postulation. Skinner (1944), on the other hand, preferred an inductive functional approach which is much more comparable to Newton's method than is Hull's theorizing.

(5) *Phenomenal reality*. Skinner (1953) and his students and colleagues see all behavioral qualities (processes) as equally real. Thus, for example, learning and conditioning are as real to them as performance is to positivistically oriented theorists who use intervening or hypothetical variables.

Analogously, Mach ([1886] 1959, p. 6) regarded phenomena as real and Kantian noumena (things-in-themselves) as unreal (i.e., nonexistents). Moreover, Mach saw all sensory qualities as equally real, and argued that bodies, for example, become increasingly familiar as reals, while colors, sounds, and odors are erroneously assigned only secondary status because of their relative transience; hence their unfamiliarity as reals ([1883] 1960, p. 611). For Mach, however, no sensory quality was truly secondary; all existed on a par with so-called primary qualities.

(6) *Dangers of overspecialization in science*. Skinner has repeatedly exhorted his colleagues not to waste time on such specialized scientific techniques as statistics and mathematics, which although useful under special conditions are dangerous in that they may carry one away from the direct study of observable behavior itself. He also, probably more than any other behaviorist or psychologist, felt that we should be applying an already existent technology of behavior. Compare this with Mach ([1883] 1960, p. 609), who wrote, "science has its origin in the needs of life" and stressed the value of specialized concepts, while simultane-

ously warning of their danger if erroneously overextended.

(7) *Relating special to general knowledge*. Skinner believed that a science of behavior that analyzes the environment and finds controlling functions in it has the advantage of working with *accessible* controlling variables. Mentalistic and physiological approaches, in contrast, assign a determining role to inaccessible variables and thus retard the study of that which can be directly studied. Mach argued that every investigator needs a philosophy relating his branch of science to knowledge in general in order to preclude absurd questions and pseudoproblems wherein the "complicated and remote" of one narrow field are used to try to explain the "simple and immediate" in another field.

(8) *Need for independent development of each science*. Skinner (1938, p. 432) has argued for the need to study organismic behavior independently and in its own right before seeking integration with other disciplines and quotes Mach on the same points approvingly.

A scientific magnum opus. From 1930 to 1937 Skinner published some 19 papers, which provided him with material for *The Behavior of Organisms: An Experimental Analysis* (1938). Looking back years later ([1967] 1970, p. 10) he stated: "Russell and Watson had given me no glimpse of experimental method, but Pavlov had: control the environment and you will see order in behavior." Pavlov (1927) controlled the environment of his subjects by repeatedly presenting a response-eliciting stimulus contingent on prior presentation of another stimulus, and studied the formation (conditioning) of a new reflex consisting of the first stimulus and the response originally elicited only by the second or reinforcing stimulus. Pavlov also studied the extinction of conditioned reflexes resulting from omission of the reinforcing stimulus.

Behavior as studied by Pavlov can be divided into two classes, conditioned and unconditioned reflexes. Skinner (1938) accepted this classification, but then defined a still broader classification, that between respondent and operant behavior. The former is stimulus-*elicited* behavior, while the latter is *emitted* behavior where no eliciting stimuli can be observed when the behavior occurs. Implicit in this schema, but not made explicit in 1938, is a fourfold functional classification of behavior. The four functional classes are conditioned and unconditioned (i.e., phylogenically selected) respondents and conditioned and unconditioned (i.e., phylogenically

selected) operants. Although no one has made this fourfold classification explicit, it is clearly implied by Skinner (1938; [1975] 1978*b*, pp. 169ff.).

Conditioned operants are formed when a contingency of a reinforcing stimulus exists on the emission of an operant. Thus operant behavior is conditioned "when it acts upon the environment in such a way that a reinforcing stimulus is produced" (Skinner 1938, p. 22), while respondent behavior is conditioned by something done to the animal, not by how the animal acts upon the environment.

Eliciting, reinforcing, discriminative, and other functions of stimuli are not postulated hypothetico-deductively, but determined by observational and experimental analysis. A reinforcing stimulus is such by its demonstrated production of behavioral change, e.g., increase in frequency of an operant. Analysis has established two classes of reinforcing stimuli: *positive* because their presentation acts as a reinforcement operation, and *negative* because their cessation acts as a reinforcement operation.

In *The Behavior of Organisms* Skinner concentrated on conditioned operant reflexes and their frequency (rate) of occurrence over time under various conditions of environmental control. Conditioned respondent reflexes had already been well studied by Pavlov, but no one had studied operant reflexes with equal intensity. Skinner later derogated as "dogged" his continued use of the term *reflex* for both operant and respondent behavior, presumably because many readers restricted its meaning to respondents only. But, *reflex* is an excellent term if one thinks of it as meaning reflection of, rather than push-pull causation by, stimulus functions. Thus, unconditioned respondents and operants reflect a history of phylogenetic contingencies of survival, while conditioned respondents and operants reflect a history of ontogenic contingencies of reinforcement (cf., Skinner 1975; 1977*a*; Herrnstein 1977).

From rats to pigeons and the planning of a community. Skinner remained on the staff of the department of psychology of the University of Minnesota until 1945, when he moved for three years to the department of psychology of the University of Indiana. There he was not only chairman but also a departmental colleague of J. R. Kantor, whose many articles and books on interbehavioristic philosophy and psychology are excellent for the exorcism of "spooks" from

one's thinking, to paraphrase Skinner ([1967] 1970, p. 19).

While still at Minnesota, Skinner (1960) bought some pigeons with the idea that surface-to-air missiles might be guided by appropriately conditioned animals, and he laid the basis for an immense future data output from his laboratory. He also invented (1945*a*) the air crib to aid in the care of his second daughter, Deborah, and was well along on two book-length manuscripts, *Walden Two* (1948) and *Verbal Behavior* (1957*b*), all of which he carried with him to Indiana.

At Indiana, he did experimental work with pigeons, administered the department, and also devoted time to professional organizational chores. Students and colleagues at other universities, notably Fred S. Keller and William N. Schoenfeld at Columbia, were now doing research and teaching based on his *Behavior of Organisms*. There were meetings that culminated in the formation of division 25 of the American Psychological Association, which represented their scientific interests, and then in the establishment in 1958 of the *Journal of the Experimental Analysis of Behavior*, which provided an archival place of publication for articles by members of the group and kindred investigators.

A planned community and the problem of verbal behavior. Skinner delivered the William James lectures at Harvard in 1947 based on his material on verbal behavior. He moved in 1948 to Harvard's department of psychology and published a utopian novel *Walden Two*. The novel described a planned community where positive rather than negative reinforcers served directly to maintain appropriate behavior. The novel stimulated some founding of experimental communities ("Skinner's Utopia. . . ." 1971) and also second thoughts by Skinner (1976*b*).

When *Verbal Behavior* was published, it was vigorously attacked by a young linguist Noam Chomsky (1959). Although popular with critics, the review is a scientific overkill in that the critique is more relevant to simpler causal stimulus-response psychologies than to functional and operant analyses of behavior.

The unique contribution of the analysis in *Verbal Behavior* is (a) to define verbal behavior first as behavior controlled by the listener or audience as a source of reinforcement and then as behavior controlled by the behavior of the listener or audience specifically conditioned to

act that way; (b) to equate the "languages" of the linguist to the reinforcing practices of verbal communities (1957b, p. 461); and (c) to concentrate on the first point while largely ignoring the second. Small wonder the book is a puzzle to linguists.

Teaching and basic experimental analysis. At Harvard in the 1950s Skinner developed an introductory course on human behavior and wrote an appropriate text, *Science and Human Behavior* (1953). Here the terms and laws that first emerged from his laboratory analyses of rat and pigeon operant behavior were extended to human behavior and cultural practices. The text provided an introductory treatment complementary to that contained in the Keller and Schoenfeld book (1950).

Skinner worked closely with postdoctoral fellow Charles B. Ferster and with others comprising the pigeon group. The work with Ferster culminated in a widely used and cited book, *Schedules of Reinforcement* (1957). Here the pigeon is star performer as reflector in its behavior of simple and complex contingencies of reinforcement. A favorite, however, is the glossary of the book, where most basic terms are defined by an implicit *instance-class-operation-process* logic adumbrated in Skinner's early articles in the 1930s (1931; 1935).

From science to technology and cultural practices. Skinner's impact on colleagues, both basic and applied, has been great. This impact is documented, in part, in a *Festschrift* (Dews 1970) and in three editions (1959, 1961, 1972) of Skinner's *Cumulative Record*. Areas of impact or of new interest include problems in methodology (Sidman 1960); experimental analysis in speciality areas of psychology and related disciplines (Skinner 1978a; Honig 1966; Honig & Staddon 1977; Ferster 1978); education (Holland & Skinner 1961; Skinner 1968a; Keller & Sherman 1974); clarification of *Contingencies of Reinforcement* (Skinner 1969) and *About Behaviorism* (Skinner 1974), and the writing of the first volume of his autobiography (Skinner 1976a; Ferster 1977).

Beyond Freedom and Dignity (1971) made Skinner's name a household word. The title is slightly reminiscent of Nietzsche's *Beyond Good and Evil* (1885). Its thesis is that only a technology of behavior can save Western democracy from continually failing to solve the many individual and social problems that plague it. In effect, it is the knowledge of self- and social-

control of human behavior through appropriate manipulation of controlling environments *by the people* that Skinner (1977b) recommends as superior to the many cultural practices currently involving freedom of choice of individual action and individual dignity resulting from socially assigned credit for apparent personal achievement.

Critical summary. Skinner is an articulate and controversial scientist who has not only made original technical and theoretical contributions to psychology and to the behavioral and social sciences (e.g., Hilgard & Bower 1948), but has extended scientific analyses to many facets of individual and social life. He has done so without apparent inhibition or reservation concerning belief in the ultimate victory of a radical behavioristic methodology over other methods for study and understanding of human behavior.

Perhaps it is the very intensity with which he has extended his analyses to society at large that makes him unique and a target for many, especially the conservative and/or the timid. He certainly adheres to the general goals of Western democracy. But he would have us reach those goals sooner and more efficiently than many seem to desire at this time.

Skinner's behavioristic system of psychology is scientifically a system of psychology wherein spontaneity is seen as an observable property of the emission of behavior. The strength of such operant behavior is measured empirically by its frequency of occurrence and theoretically by a probability value.

In actual research, a previously established lawful relationship is examined as to its generality or specificity (Sidman 1960) or a new lawful relationship is discovered and reported with suggestions as to its generality or specificity. The fundamental procedure of functional analysis is to let one's subject matter, in this case behavior and its processes, be the measure of all the operations one performs. Having done this in a highly simplified and contrived situation under experimental conditions, one is then more educated as to what to look for when one looks at naturally occurring behavior (cf., Mackenzie 1977, pp. 165ff.). What one finds there may be similar, vastly different, or identical to what one sees in other places. One then extends or restricts previously known laws or reports the new laws one discovers. A functional system is simply not to be confused

with a hypothetico-deductive one and clearly cannot be evaluated as such. Rather, a functional system must always be evaluated on how well its laws (rules) work in real situations, both contrived and natural.

WALTER C. STANLEY

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SPENCE, KENNETH W.

Kenneth W. Spence was born in Chicago on May 6, 1907, and died in Austin, Texas, on January 12, 1967, at the age of 59. He was then professor of psychology at the University of Texas. Recognized as the logical successor to Clark L. Hull, who had died in 1952, Spence had been deeply involved for practically all his professional life, including his graduate student years, in the development of an empirically based behavior theory within the general framework of Hull's neobehaviorism.

Spence has been so closely identified with Hull's systematic behavior theory that treatments of both their points of view have often

been combined and referred to as the Hull–Spence approach (Logan 1959). Spence was first exposed to Hull's views when he attended the latter's 1930/1931 seminar as a graduate student at Yale University. At the time he was assistant to Robert M. Yerkes with whom he did his doctoral dissertation research, a study of visual acuity in the chimpanzee. After obtaining his PH.D. degree in 1933 (his other degrees were received at McGill University), Spence spent several years at the Yale Laboratories of Primate Biology in Orange Park, Florida. He then served for a year on the faculty of the University of Virginia and joined the University of Iowa as associate professor in the fall of 1938. In 1942 he became professor and chairman of the Iowa department of psychology and remained in that capacity until 1964, when he went to Texas.

When Spence arrived in Iowa City in 1938, the stage was already set for one of psychology's more significant and creative periods during which the major efforts of individuals like Hull, Kurt Lewin, Edward C. Tolman, Edwin R. Guthrie, and B. F. Skinner were directed toward the development of broad systematic theories or conceptions of behavior. Tolman's *Purposive Behavior in Animals and Men* had appeared in 1932. Skinner's *The Behavior of Organisms* became available in 1938. Hull, since his arrival at Yale in 1929, had been intensively engaged in the development of his behavior theory. Kurt Lewin, a faculty member of the Iowa Child Welfare Research Station, was pursuing the development of his field theory or topological psychology. Herbert Feigl, a philosopher of science identified with logical positivism or empiricism, was also at Iowa, but it was Gustav Bergmann, philosopher of similar persuasion, who had followed Feigl to Iowa, who had the greater influence on Spence.

Bergmann and Spence developed a strong and mutually satisfying intellectual relationship and coauthored a number of articles explicating the philosophical–logical underpinnings of Hull's theoretical efforts, as well as more general issues and problems involving methodology in psychology. In a paper concerned with operationalism and theory construction in psychology, Bergmann and Spence (1941) analyzed the character of Hull's early theorizing and showed that Hull was actually following Tolman's intervening variable approach to theory construction, a conclusion accepted by Hull. Spence maintained a continuing interest in such mat-

ters, and his methodological contributions were made explicit in a number of independent publications (1944; 1948; 1952; 1957).

Spence's early promise as a brilliant experimental and theoretical psychologist had been shown in a number of significant papers that had appeared before his arrival at the University of Iowa. They included an influential paper describing his theory of discrimination learning in animals (1936) and a related theoretical analysis of transposition phenomena (1937). Using simple conditioning principles of reinforcement and what he then called inhibition or frustration associated with nonreinforcement (extinction), and making quantitative assumptions about the ways in which reinforcement and frustration affect the growth of independent excitatory and inhibitory processes respectively, Spence was able to explain or predict a variety of discrimination-learning phenomena, including the development of systematic position responses and the seemingly sudden and insightful character of learning that followed the disappearance of lengthy and consistent position habits. Further, postulating generalization gradients of excitation and inhibition of a particular form, Spence was able to deduce the basic transposition phenomenon as well as additional related implications. With this theory, Spence replied to Gestalt psychologists who argued that transposition represented a response to the relationship between two stimuli—e.g., larger than—and that it could not be accounted for by absolute stimulus-response theory, which emphasized the acquisition of specific associations to stimuli.

These papers are interesting not only for their substantive contributions and their early demonstration of Spence's systematic orientation but also in relation to Spence's later thinking. One example is his subsequent, more systematic, treatment of inhibitory phenomena associated with the effects of nonreinforcement for previously rewarded responses and the role of emotional factors (frustration) in such processes (see 1960).

On a quite different level, these papers were referred to by Spence in his later writings to point up the importance of what he called the boundary conditions of a theory. As Spence tried to make clear to his critics, his theories of discrimination learning and transposition held only under specified boundary conditions—i.e., for nonarticulate organisms alone. He chided those who attempted to refute these theories on the

basis of experiments using college students as subjects.

Despite a continuing interest in discrimination learning, Spence, in accordance with Hull's broad strategy for the development of a systematic behavior theory, soon turned to other experimental situations. Spence agreed with Hull that the discovery of a set of basic or primary laws of molar behavior, expressed in quantitative form, could best be attained through the study of simple (inarticulate?) organisms in simple situations. Simple situations include classical and instrumental conditioning, with the former probably the simpler of the two. It was assumed that the basic laws derived from simple situations would hold for more complex ones—although, conceivably, new variables would be operating in the latter. Interaction laws, as Spence (1959) called them, would be required to specify the relationship of the basic variables to the new variables operating in the more complex contexts.

In a relevant entry, dated September 16, 1950, in his "Idea Books," Hull (1962, p. 881) referred to a conversation with his colleague Carl I. Hovland. Hovland had asked how one might proceed to discover new laws applicable to social psychological situations. Hull had doubted that there were any new primary laws, implying that these had already been isolated: "The main task is to verify the working out of the recognized laws of individual behavior in *purely social situations*."

This comment exemplifies a difference in theoretical "style" between Hull and Spence. Spence was much more conservative about the degree of progress made in the discovery of basic laws, and he was guided by and remained close to the data. Hull was more speculative about more complex situations in the sense that he was willing to address them, even though the basic (primary) laws of behavior had not been adequately specified, or perhaps because he assumed—prematurely and in the absence of adequate data—that they indeed had already been isolated. Not until some 25 years after the publication of his 1936 discrimination learning theory did Spence (1960) feel that enough progress had been made in the development of a theory of simple conditioning to justify further efforts to extend the theory to the more complex discrimination or selective learning situation.

On occasion, Spence (1956; 1959) took issue with those who stressed the application of psy-

chology to the solution of practical problems, and who criticized the concepts, experimental arrangements, and subjects used by psychologists like Hull and himself. Such criticism, in his judgment, reflected a misunderstanding of, and served to retard, the scientific enterprise. Although he did not preclude the possibility that significant research could be done in the more complex, applied, real-life setting—for example, the school, hospital, military, or industrial setting—he insisted that a more fruitful strategy was to search for basic behavior laws in simple situations, rather than attend prematurely to practical or applied matters. Moreover, he repeatedly emphasized that the pursuit of primary laws in simple situations represented only a first phase in an over-all strategy for developing a broad theory of behavior that was applicable to a wide range of complex situations.

Spence interpreted Hull's position with exceptional clarity, at the same time modifying and elaborating it with his own unique contributions. The most systematic over-all presentation of Spence's position is given in *Behavior Theory and Conditioning* (1956), a volume based on his Silliman lectures at Yale in 1955. The work begins with a historical—methodological introduction, after which Spence presented an experimental and theoretical analysis of conditioning, ending with a consideration of the extension of conditioning theory to complex learning. Although in the Hullian tradition, the volume is interesting in pointing up a number of methodological and substantive differences between Hull and Spence. For example, in contrast to Hull's monistic reinforcement position, Spence tentatively proposed a version of a two-factor theory in which reinforcement was necessary for classical conditioning but not for instrumental reward conditioning. This view was in direct opposition to earlier two-factor proposals—for example, Mowrer's (1947), which held that reinforcement was necessary for instrumental learning but not for classical conditioning. Other differences between Hull and Spence related to motivation and incentive variables and their interaction. Technical and space considerations preclude their discussion here. Significant, however, is that the formulations of both men were expressed in quantitative form and could be tested experimentally.

Spence spent 26 highly productive years at the University of Iowa as a researcher and teacher. At Iowa, 73 students completed their

doctoral dissertations under his supervision, and his students remember his strong commitment to teaching and his success in that role. He was a teacher both of students and of his peers and produced significant chapters analyzing the theorists of his day (1951a; 1951b).

Spence was honored during his lifetime and posthumously. He was elected to the National Academy of Sciences and was the only psychologist invited to present the Yale Silliman lectures. He was also among the first group of three psychologists to receive the distinguished scientific contribution award from the American Psychological Association in 1956. In connection with the award, he prepared a paper (1958) in which he elaborated Hull's multiplicative drive theory to include as an additional source of drive a hypothetical emotional response associated with aversive stimulation. After his death Howard H. Kendler and Janet T. Spence (1971) edited a memorial volume which included contributions by many of his students. On November 5, 1969, the University of Iowa dedicated and accepted in his memory the Spence Laboratories of Psychology, a new six-story structure in close proximity to the building in which he had spent so much of his professional life.

GEORGE J. WISCHNER

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SPROUT, HAROLD AND MARGARET

The academic study of international relations during the 1920s and 1930s in the United States had its roots in diplomatic history and international law. The subject matter and the perspectives brought to it were largely dominated by the legacy of Woodrow Wilson, the Paris Peace Conference, and the establishment of the League of Nations. With remarkably few exceptions, American teachers and writers on international subjects anticipated a new era of international cooperation, the abolition of secret diplomacy, and a stable peace into the indefinite future. These were key presuppositions that prevailed widely in academia when Harold Sprout, born in 1901, and Margaret Sprout, born in 1903, were pursuing their formal educations in the 1920s.

Not every teacher accepted this optimistic scenario. One who did not was Karl Frederick Geiser of Oberlin College. His inquiring mind and amiable skepticism left a deep imprint on many students who went on to become academicians, public officials, and business and civic leaders. The Sprouts were among those students profoundly influenced by Geiser, and always regarded themselves as his "protégés."

After their marriage in 1924, the Sprouts studied law first at the University of Wisconsin

and then at Western Reserve University. Unhappy, however, with the sanguine formalisms of the legalistic approach to international relations which Geiser had taught them to question, they soon shifted into graduate study in political science at the University of Wisconsin. In 1929 that institution awarded Harold Sprout a doctorate in political science and law, and conferred on Margaret Sprout a master of arts degree in political science and geography.

They spent the 1926/1927 academic year at Miami University of Ohio, where Harold Sprout obtained a fill-in faculty appointment, and 1929-1931 at Stanford University, where he obtained another similar position. Margaret Sprout never seriously considered continuing her formal education beyond the master's degree, knowing that a doctorate would not increase her professional opportunities, given the general unavailability of joint appointments for husband-and-wife teams—or, for that matter, faculty appointments for single women in most disciplines—at any major college or university in the country.

In 1931, when the great depression virtually wiped out the academic job market, Harold Sprout had the good fortune to receive an instructorship at Princeton University which—although it seemed a most unlikely prospect at the time—was to remain the base for the rest of their professional careers stretching out for approximately half a century. Throughout the desperate early and middle 1930s, Harold Sprout clung precariously to the lower rungs on the faculty ladder in Princeton's department of politics. Thanks largely to Tyler Dennet (then a senior faculty colleague, earlier a public official, and later president of Williams College), he escaped the professional unemployment that destroyed the careers of so many of his contemporaries. He finally reached the rank of full professor, served as department chairman (1949-1952), and was then appointed to the endowed Bryant chair in geography and international relations in the department of politics. In 1969 that position was translated to professor emeritus when he reached the mandatory retirement age of 68.

Along the way, Harold Sprout filled visiting appointments at the University of Pennsylvania (1938), Columbia University (1948-1950), the University of Denver (1950), Nuffield College, Oxford (1955), and Rutgers University (1970). In the mid-1940s he was briefly a member of the Princeton Institute for Advanced Study.

He conducted most of the negotiations that successfully resulted in the movement of an organizational structure and a number of professional scholars from Yale University in the context of the establishment of the Center of International Studies at Princeton University (1951). His own membership in the center was continued by appointment to a research position coincident with his retirement from the teaching faculty (1969). During 1955, 1957, and 1962, the Sprouts spent several semesters abroad in connection with research on which they were then engaged.

Margaret Sprout's career followed a different but related course. She demonstrated superior teaching and research abilities at the University of Wisconsin nearly two generations before the thrust of events and federal policies forced American colleges and universities to modify their traditional discrimination against female teachers and scholars. During the 1930s, she was a member of Edward M. Earle's faculty seminar at the Institute for Advanced Study. Though denied official university status, she was explicitly named (with her husband) as corecipient of the Shreve fellowship in history administered by Princeton University. Again, without any university status, she taught one of her husband's courses during his absence for governmental service in Washington during World War II. Along the way, she was a key participant in several professional conferences. She wrote the essay on "Mahan" for Earle's still widely-read symposium volume, *Makers of Modern Strategy* (1943). Prior and subsequent to that, she coauthored with her spouse eight books and numerous journal articles in the fields that they helped to create and to develop in political science. Finally, in 1969, she acquired formal university recognition when, through the courtesy and efforts of Cyril E. Black (then the newly installed director of the Princeton Center of International Studies and professor of history) she was appointed research associate, a position that she then held for virtually the remainder of her academic career.

In addition to her scholarly work as a political scientist, Margaret Sprout early became a civic leader with a predominant commitment to improving the delivery of public services (particularly services related to human health) at the level of individual homes and families. She served on the executive board of the New Jersey League of Women Voters (1936-1946), and was president of the league's Princeton area

chapter (1942-1946). She was cofounder and twice president of the Princeton Council of Community Services, and was founder and first president of the Princeton Homemaker Service. In 1971, she accepted an official public position as chairman of the Princeton Township Board of Health. From that point of departure, she became cofounder of the Princeton Regional Health Commission, serving in top executive positions in that agency in the late 1970s.

The Sprouts over the course of their long professional careers in academic life were again and again (metaphorically speaking) "seers ahead of their time." In 1931 Harold Sprout published a plea in the *American Political Science Review* for recognition of political geography as a field of political science—a suggestion that went unheeded until after World War II. That experience was symptomatic of other things to come. Repeatedly they took positions that went virtually unnoticed for years and then suddenly became "ideas whose time has come." For example, they persistently emphasized the political relevance of concepts and insights evoked by wide reading in the literature of geography, historiography, ecology, and sociology, explicitly stressing the importance of interdisciplinary contact and cooperation. In this way they were some two decades ahead of the interdisciplinary movement that began to be evident across many of the social sciences (particularly among those fields focusing on international behavior) in the middle to late 1950s. For another example, they gave careful attention from the beginning of their careers to an effort to understand and apply theories of knowledge—especially distinctions in the implications and requirements for testing various types of scientific statements such as description, explanation, and prediction—to the discipline of political science. In this way they were similarly some two decades ahead of a parallel movement in the middle to late 1950s to introduce a stronger sense of self-conscious rigor into the study of international relations. But at the same time they always understood that the full meaning of "being scientific" was not exhausted by the use of statistical methods with quantified data, and this understanding was not substantially shared until two more decades had elapsed as of the mid-1970s.

Finally, as environmental pollution became a major new public concern in the 1970s, the Sprouts were "rediscovered"—as it were—in their role as pioneer researchers and teachers on the

crucial interplay among the following sets of variables in any nation-state (but with special reference to the modern industrial state): (1) the nature of the social system in terms of an appetite and effective demand for consumer goods and services; (2) the nature of the economic system in the creation and delivery of those goods and services; (3) the nature of the political system in applying public authority to the demand for and supply of consumer goods and services while at the same time expressing the demand and attempting to supply collective goods and services such as are exemplified in the expectation of national security and military defense; and (4) finally—what could be regarded as the chief dependent variable in this set of four—the progressive deterioration of the human habitat.

These generalizations about the numbers of important ways in which Harold and Margaret Sprout were pathfinders over forty to fifty years can be illustrated most effectively in a review of their major publications. Those books and related studies reveal yet additional ways in which they were innovative trailblazers.

The Sprouts' first major work was *The Rise of American Naval Power* (1939). Carefully researched during the 1930s when American naval power was a highly controversial political issue in many dimensions, but avoiding those debates in order to stress scholarship, the book focused on the domestic and external milieu—or context—that affected naval design, legislation, administration, and strategy over the period from the 1780s to the end of World War I. Charles A. Beard characterized it as the first significant “parliamentary history of the Navy.” A sequel, *Toward a New Order of Sea Power* (1940), reconsidered the Washington Naval Conference of 1921–1922 from this perspective, and in addition provided a concise explanation of the rise and decline of British naval primacy in the context of Mahan's ideas on seapower. These two books, still in print and enjoying steady if small annual sales into the last years of the 1970s, stood the test of four decades. Indeed, as the Soviet–American naval rivalry spurred younger scholars to a new interest in research on the origins of modern navies, the Sprouts' books re-emerged more securely than ever as the foundation signposts for this kind of inquiry. They themselves had much earlier prepared the beginnings of a third volume on naval policy when World War II diverted their efforts into other channels.

While Margaret Sprout wrote the previously mentioned essay on Mahan for the Earle book, Harold Sprout wrote the text of *A War Atlas for Americans* (1944). He also performed a variety of wartime assignments for the War Department, the Navy Department, and the State Department. Both of the Sprouts played a major role in a project sponsored by then newly designated Secretary of the Navy James V. Forrestal, designed to achieve a more broadly focused classroom course on international relations for use in the postwar curriculum of the Naval Reserve Officers' Training Corps (ROTC) program. That project culminated in a book of original text materials and selected readings, coauthored and coedited by the Sprouts, entitled *Foundations of National Power* (1945). It was adopted as the basic textbook in many college and university courses having no connection with the naval ROTC, and in retrospect seems to have exerted considerable influence in turning the teaching of international relations toward a much broader and richer mixture of conditions and events than were previously considered appropriate in such courses. This in turn fueled the movement toward the interdisciplinary perspectives that had been part of the Sprouts' intellectual orientations all along.

An almost entirely new version of the original *Foundations of National Power* was published by the Sprouts some 17 years later (1962) under the title *Foundations of International Politics*. But over that period a theme that had run through all of the Sprouts' works had become for them a dominant new emphasis, and this was the focus on broadly environmental as well as more narrowly political and economic concerns in the study of national behavior in the context of international statecraft. In 1954 the Rockefeller Foundation joined with Princeton University to provide more substantial continuing support for their further studies along these lines that were to preoccupy them for the remainder of their careers.

The first publication resulting from this dominant new thrust was a set of two theoretical essays in *Man–Milieu Relationship Hypotheses in the Context of International Politics* (1956). Subsequent refinements and developments of these ideas tumbled forth in a rich profusion of publications over the years, including “Environmental Factors in the Study of International Politics” (1957), *The Ecological Perspective on Human Affairs* (1965), *An Ecological Paradigm for the Study of International Politics* (1968b),

"The Dilemma of Rising Demands and Insufficient Resources" (1968a), *Toward a Politics of the Planet Earth* (1971b), "The Environmental Crisis in the Context of American Politics" (1971a), and a chapter in *Environmental Politics* (1973).

In 1978 the Sprouts published *The Context of Environmental Politics*, a book that they regarded as perhaps their culminating masterwork embracing and synthesizing the dominant and evolving themes in their research and writing for more than two decades since the middle 1950s. Some of those themes can be briefly summarized in the following statements excerpted almost verbatim from their works:

(1) Political scientists should be severely reprimanded for sitting on their hands, leaving so many political aspects of environmental deterioration to be dealt with by insensitive nuclear physicists, aerospace engineers, and arrogant technocrats.

(2) Any program for spending large sums of public money for cleaner air, cleaner water, and other environmental improvements is likely to run into grave political trouble if these values are put ahead of comparable outlays for the equally commendable and necessary purposes of eradicating poverty and the conditions and consequences of underprivileged status in a modern society.

(3) A salient feature of the present situation is the simultaneous peaking of demands for social and environmental reform. Politically expressed demands of these magnitudes would present formidable difficulties under any circumstances, but they appear even more formidable when viewed in the light of the over-all crisis of priorities represented by the clash between foreign threats perceived as military security dangers and domestic needs perceived as absolutely necessary requirements for tolerable lives at home. Military defense issues in most modern industrial societies are likely never again to be considered without dramatically more simultaneous regard for the impact of defense spending on conditions within those societies.

(4) Business executives, necessarily driven by profit motives, tend to exploit the natural environment and to put domestic social circumstances into a low-priority category, such that only governments can assert a broader public need and demand for environmental maintenance and restoration.

(5) But governments, particularly the gov-

ernment of the United States, will find it exceedingly difficult if not impossible to offset the pressures for environmental exploitation from the business community, because many other interest groups and the public at large retain the hallowed American dream of a hedonistic utopia. Yet, it is a dream that will turn into a nightmarish mirage unless an effective public and political coalition can emerge which will put the maintenance of a sustainable human habitat at the top of social, economic, and political priorities.

If these themes from the work of the Sprouts now sound commonplace, in view of the growing numbers of scientists and scholars and other publicans who emerged stressing the same or similar messages in the 1970s, it remains true that the Sprouts were virtually alone for several earlier decades in reaching these conclusions from their interdisciplinary social science research, extensively and repeatedly supported by the Rockefeller Foundation and other benefactors. Their work heavily influenced several generations of students many of whom subsequently achieved great academic distinction in the social sciences and/or in public service. Their teaching was often in the context of the Woodrow Wilson School of Public and International Affairs, alongside their research in the Center of International Studies at Princeton University, where faculty colleagues as well as students were influenced not only substantively but in terms of the Sprouts' strong commitment to interdisciplinary research designs and methodologies. Indeed, it is probably not too much to say that the Sprouts were the steady beacons that largely guided in informal but powerful ways both the Woodrow Wilson School and the center, especially when these paired institutions and their journal *World Politics* served as the "intellectual capital" for the interdisciplinary field of international studies throughout the 1950s and the early 1960s. The field itself fell into some disarray in the later 1960s largely as a consequence of the inflamed emotions and arguments engendered by the U.S. involvement in southeast Asia. Yet, through those painful years, the Sprouts remained steady on their long-run course, and thus were ready to supply the scholarly foundations when environmental issues largely displaced defense issues in many scholarly circles in the 1970s and beyond. As long as social scientists address themselves to the question of how political processes can be used to save the planetary biosphere as a viable

and aesthetically appealing human habitat, the work of the Sprouts will provide guidance and inspiration.

VINCENT DAVIS

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SRAFFA, PIERO

Piero Sraffa is one of the most controversial theoretical economists of the twentieth century. A friend of Antonio Gramsci, the founder of the Italian Communist party, and a protégé of John Maynard Keynes, the great English economist, Sraffa spent almost his entire active life, as an unmarried don, in the quiet atmosphere of the Cambridge colleges, rarely seen at academic congresses or conferences around the world. His writings, nevertheless, have twice been at the center of major breakthroughs in economic theory in this century.

Piero Sraffa was born in Turin (northern Italy) on August 5, 1898, the only child of Angelo Sraffa, a well-known Italian university professor of commercial law, and of Irma Tivoli, who came from a rather matriarchal Jewish family. In Turin, Sraffa attended the lycée and the university. Among his teachers was Umberto Cosmo, a complex person with socialist tendencies, whose lectures on Italian literature had been attended by Gramsci. At the university, Sraffa wrote his dissertation on "Inflation in Italy During and After the War," supervised by Luigi Einaudi, the Italian economist and public finance expert, later to become the second president of the Italian Republic. Sraffa met Keynes on an occasional visit to Cambridge in 1921, and seems to have made a strong impression on him. Keynes commissioned him to write an article on the Italian financial markets and banking crisis. The article was published in two versions: in the *Economic Journal* (1922a), and in a section on the "Reconstruction of Europe"—edited by Keynes—in the weekly *Manchester Guardian Commercial* (1922b). This event turned out to be important in Sraffa's life. Mussolini, who had just seized power in Italy, was enraged by the article. For Sraffa this incident was the beginning of a series of troubles with the fascist régime, but, at the same time, of increasing contact with Keynes.

Sraffa became a professor of political economy at the University of Perugia in 1924 and at the University of Cagliari (Sardinia) in 1926. But the fascist régime was increasing in strength, and when in 1927 Keynes was able to offer him a lectureship at the University of Cambridge, Sraffa accepted. He emigrated to England and remained there.

By the time Sraffa arrived in Cambridge, he had already established himself, at 29, as a theoretical economist of international standing. In an article written in Italian (1925), he had carried out a meticulous criticism, centered on the question of returns to scale, of Alfred Marshall's theory of the firm, arguing that the only logically consistent assumption to make in that theory is that of constant returns to scale. Almost simultaneously, in a remarkably concise article (1926), Sraffa had exposed the glaring weaknesses of the perfect competition model that traditional economics until then had generally used. By stressing the importance of monopolistic market situations and the consistency of increasing returns to scale with limited production, when the single firm demand curve was negatively sloped, he had laid the foundations of an entire branch of new theories of value and pricing. That branch developed a few years later, especially with the works of Joan Robinson (1933) in England and Edward Chamberlin (1933) in the United States, though the latter started from quite an independent line of thought.

In Cambridge, Sraffa was soon a major protagonist, with Gerald Shove and Dennis Robertson, in a famous symposium on increasing returns (1930). Shortly afterwards, he launched a devastating critique (1932) of the traditional theory of money and capital as proposed by Friedrich A. von Hayek (1931). But he also devoted time to more leisurely activities. He loved collecting first editions of rare books, especially of eighteenth-century economists and philosophers. He did this, sometimes in competition with, and sometimes jointly with, Keynes. A remarkable example was their finding and jointly attributing and editing *An Abstract of a Treatise of Human Nature* (1938), a previously unknown pamphlet by David Hume. Yet this was no more than a hobby. A much bigger task was ahead. Cambridge University made it possible for Sraffa to realize a great ambition: the collection and editing of the *Works and Correspondence of David Ricardo* (1951–1973), the classical economist whom Sraffa admired so

much. This was a lifelong undertaking. Sraffa began it in the early 1930s and carried it through World War II. The accuracy of the collation of the texts, the richness of the editorial notes, and, most important, the analyses contained in the prefaces and introductions have made this work an almost unique masterpiece in economics publications. Sraffa provided a new reinterpretation of classical economics and at the same time set a high standard of excellence in editorial scholarship.

Few people expected anything more from Sraffa after the publication of Ricardo's works. But in 1960, at age 62, Sraffa published an admirably concise 99-page book, on which he had been working for more than 30 years. The title was baffling—*Production of Commodities by Means of Commodities: Prelude to a Critique of Economic Theory*—the content was even more so. In it, Sraffa abandoned the “marginal method” of analysis, which had characterized established economic theory for more than a century, and went back to the older approach of such classical economists as Adam Smith and David Ricardo.

The problem that Sraffa explored in this investigation concerns the properties of an economic system in which there is no change in the scale of production or in the proportions among the “factors” of production. Like François Quesnay in his *Tableau oeconomique* (1758), Sraffa considers the net national product as a “surplus” over and above what is required to replace the means of production, and he regards production as a “circular process” in which the same commodities appear both as means of production and as final products. This approach may look very similar to Leontief's input–output analysis. But Sraffa goes deeper, by resuming a more classical line of thought, about the theories of value and income distribution—which began with Ricardo and can be traced through Karl Marx, Ladislaus von Bortkiewicz, and Vladimir Karpovich Dmitriev. The classical theories had been abandoned at a certain stage because a few basic concepts on which they were built seemed to contain deficiencies and ambiguities. Sraffa's contribution consists precisely in dispelling those deficiencies and ambiguities. Three examples may suffice to illustrate the point:

(1) All classical economists, from Adam Smith onwards, had made a sharp distinction between necessary (or wage) goods and luxury goods. The distinction was important because it led to radically different conclusions for the

two kinds of goods; but it had later to be abandoned because it seemed arbitrary. Sraffa shifts the ground for the distinction back to technology, drawing the line between commodities that are technologically required for the production of all commodities—"basic" commodities—and commodities that are not so required—"non basic" commodities. In these terms, a mathematical notation can be used, and concepts become unambiguous.

(2) Ricardo had considered the distribution of income as "the principal problem of political economy" but he had encountered insuperable difficulties when he tried to investigate income distribution independently of prices. He realized that he could overcome these difficulties only if he found an "invariable standard of value," defined as a commodity whose value does not change when income distribution changes. But he never succeeded in finding such a commodity. Sraffa demonstrates that an "invariable standard" can in fact be constructed analytically in the form of a composite commodity—the "standard commodity"—that characterizes any given technique. Interestingly, this "standard commodity" coincides with the composition of production in von Neumann's growth model (1945).

(3) Finally, economists for years debated whether Marx's problem of the "transformation of values into prices of production" did or did not make any sense. Sraffa's analysis solves all the analytical difficulties. A relationship between quantities of embodied labor (i.e., Marxian "values") and prices is indeed shown to exist, but is much more complex than Marx imagined. It is, in any case, such as to give no analytical ground for the claim that Marxian "values" have any logical priority over competitive prices, or, for that matter, that competitive prices have any logical priority over Marxian "values."

A puzzling, and perhaps unsatisfactory, feature of Sraffa's book is that the author explicitly presents his propositions as preparatory to a critique of the marginal theory of value and income distribution; yet he does not himself carry out such a critique. This has led many economists, especially in the United States, to regard Sraffa's analysis as a merely abstract exercise in pure theory. More recent developments, however, point in quite different directions. In the late 1960s, a debate on capital theory, originating from a few propositions of Sraffa's (see Pasinetti et al. 1966), led to the discarding of

concepts—for example the aggregate neoclassical production function—that had for almost a century formed the basis of the marginal theory of capital and income distribution. Another debate among Marxists, also originating from Sraffa's book, has led many of them to the conclusion that Marx's pure labor theory of value must be abandoned (see Morishima 1973; Steedman 1977).

It is curious that Sraffa's analysis should emerge as the basis for a critique and discarding of well-established concepts both in marginal economic theory and in Marxian economic theory. Yet it is clearly in relation to developments of this kind that the significance of Sraffa's book will eventually have to be assessed.

LUIGI L. PASINETTI

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STAHL, HENRI H.

Henri H. Stahl was born in Bucharest, Romania, in 1901. He turned in 1926 to the study of social science at the University of Bucharest, after a legal education and a brief career as a lawyer. The 1920s and most of the 1930s were a golden, free period in Romanian intellectual history, a period of continual, lively debate on the merits of the ideologies contending for power in Europe. The best of these debates combined ideology with social theory in an effort to re-evaluate the nature of Romanian history and to study the causes of Romania's relatively backward condition. Stahl turned to socialism and to a Marxist analysis of Romanian social history long before it became politically expedient to do so.

At the University of Bucharest Stahl's intellectual mentors were the conservative nationalist historian Nicolae Iorga and the moderate reformist, liberal sociologist Dimitrie Gusti. By working with both of them, Stahl gained a unique perspective on Romanian agrarian history. He combined extensive field research in villages that were still among the least modernized in Europe with detailed knowledge of documentary sources in Romanian history. He also joined in contemporary ideological debates and in the Gusti group's formulation of reform programs to help the peasantry.

Stahl began to teach at the University of Bucharest in 1928, and became a full professor in 1939, the year he received his doctorate in sociology. His most important work that led to his degree was the direction of research teams in the mountains of the Vrancea, one of the last remaining areas of Europe with communal villages (1940). He also conducted research in villages whose populations had been subjected to serfdom and in villages which had been free, but

had adopted institutions of private, rather than communal property. The comparison of these different types of rural communities—communities differing in field shapes, social organization, and ecological circumstances—led Stahl to a theoretical reconstruction of Romanian agrarian history. Buttressing his thesis with documentary evidence, he concluded that Romania had never passed through a feudal stage, but had moved from something akin to egalitarian communalism directly to capitalist exploitation. Capitalism, however, had turned Romania's peasants into serfs and had created a backward, stagnant rural economy.

Independently, Stahl developed a method of historical research similar to the one that Marc Bloch pioneered in France during the interwar years. It involved "reading history backward," starting from the physical, cultural, and social evidence of the present. This was the first time that such a perspective had been used in Romania, and it cast into doubt all that had previously been written about Romania's supposedly "feudal" past. In 1947 Stahl summarized some of these findings in "The Sociology of the Romanian Communal Village."

In 1948 the Romanian Communist party outlawed sociology as a "bourgeois science." Stahl lost his position as well as the right to publish. He was, however, allowed to continue his work on documentary sources. In 1958 permission was granted for publication of the first volume of his great work, "Contributions to the Study of Romanian Communal Villages." The second volume was published in 1959, but its ideas—especially the theory that there had been no "slave" or even "feudal" modes of production in Romania prior to capitalism—were considered unorthodox because they challenged the orderly, dogmatic interpretation of history that was practiced in the Communist world. The third volume was not published until 1965.

These works, together with dozens of accompanying articles and conference papers, constitute one of Europe's foremost empirical studies on prefeudal and prestate modes of production. They also explain in great detail the changes that occur when such modes come into contact with capitalism. In demonstrating the limitations of the standard interpretation of feudalism, Stahl also proposed conclusions that may be useful in analyzing other eastern European and many Third World societies. If these conclusions occasionally caused him trouble with the authorities, they also enabled him to make a lasting,

original contribution to Marxist historiography (for an example of the importance of these topics, see especially Hobsbawm's introduction to the 1965 translation of part of Marx 1857-1858).

In 1969 Stahl expanded his arguments in "Controversies of Romanian History." In a series of essays he discussed the agrarian history of "Old Romania" (Wallachia & Moldavia) and compared it with that of Transylvania, which, under Hungarian rule, followed a more Western type of evolution. In 1972 he published "Studies in Historical Sociology" which confronted the problem of the Asiatic mode of production. He concluded that the concept was useful, but had to be modified for the Romanian case. Trade and tribute, not irrigation and despotism, were its main ingredients, and underneath the superficial state structures of thirteenth- and fourteenth-century Romania, villages remained largely pastoral, free, and communal.

After 1948 it proved impossible for Stahl to continue his social activism in villages, but with the ideological relaxation which began in the early 1960s, sociology was rehabilitated and Stahl's professorial chair was restored to him in 1966. He now set out to train a whole new generation of practically oriented sociologists, showing his students how to pose worthwhile questions, introducing them to classical sociological theory, and teaching some of them how to write. The two most important works produced by these efforts were volumes that he coauthored with his students to train them in field work techniques. "Two Villages" (1970) written with Mihail Cernea and Gh. Chepeș, compared a collective farm that worked well with one that did not. While the authors were unable to explain why this was the case, the book provided a great deal of information and data about collective farms. The second work, edited with Miron Constantinescu, detailed the spectacular growth of a typical new industrial city in Romania (1970) and analyzed some of the special problems and benefits of this kind of mushrooming growth. In order to help his teaching, Stahl then wrote a text on methods of social research (1974-1975).

In his long career Stahl taught and deeply influenced several generations of social science students. Some, like Miron Constantinescu, became high Communist party officials. Others, like Mattei Dogan, fled to the West and became distinguished sociologists. In Romania itself, the best post-1948 work in sociology (Cernea 1974)

acknowledges a great debt to Stahl. Even abroad, in France and the United States (e.g., Chirot 1976) Stahl's work and teaching have influenced social historians. Some of his students who survive in Romania continue to write, study, and maintain the tradition of distinguished sociological and historical research that he created. Unfortunately, the cosmopolitan, free intellectual environment of which Stahl was a product has ceased to exist in Romania. To its own loss Romania no longer tolerates Stahl's kind of open inquiry, which was so fruitful in the domain of social history, and which might have proved equally useful in solving pressing contemporary problems.

DANIEL CHIROT

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STEPHEN, JAMES F.

James Fitzjames Stephen (1829–1894) was an unusual man who came from an unusual family. His father, James Stephen, was an earnest evangelical. He belonged to that circle of influential men and women, known as the Clapham Sect, who rallied behind William Wilberforce, Zachary Macaulay, and Henry Thornton, and generated that remarkable movement for the moral reform of the nation based on religious revival. Evangelism indeed permeated the whole family background. As under-secretary of state for the colonies, when he “literally ruled the colonial empire” for many years, James Stephen earned the nicknames of “King Stephen” and “Mr. Over-Secretary Stephen.” He had joined the department, however, chiefly in a zealous effort to hasten the final abolition of slavery. His grandfather, James Stephen, was master in chancery and an influential member of Parliament.

But, all-pervading and consistent as these domestic influences undoubtedly were, they did not operate on a submissive or an impressionable nature. James Fitzjames Stephen came of a temerarious Aberdonian stock. His great-great-grandfather, James Stephen, had dealt a good deal in contraband importations. And his great-grandfather had found the King’s Bench prison, where he was detained as a bankrupt, a rather weak position from which to denounce the system of imprisonment for debt. When all his efforts to gain justice failed, he excited such a riot in the prison that the army had to be called in to restore order. From these forbears, James Fitzjames Stephen inherited a sturdy independence, a resolute energy, an urge to go his own way regardless of obstacles, a disposition to form opinions according to his tested standards of right and wrong, and an individualistic and searching mind; there was something of the expert wrestler in his intellectual tenacity. His father had married a daughter of John Venn, rector of Clapham, who could trace his descent through a long line of clerical ancestors to the time of Elizabeth I and was one of the original founders of the Church Missionary Society, a

characteristic product of the evangelical revival. Leslie Stephen, the inventor of the *Dictionary of National Biography*, was his brother and Virginia Woolf was his niece. He belonged to what Noel G. Annan so aptly described as the “intellectual aristocracy.”

Not unexpectedly, he was sent to Eton, but he was far from being a typical Etonian. “Public schools,” Courtenay Ilbert has remarked, “are said to be useful in rubbing off angles,” but “Stephen’s angles were not of the kind that rub off.” After a period at King’s College, Oxford, where he found life more congenial, he came up to Trinity College, Cambridge, in 1847. Here again he followed his own bent. He plunged at once into the intellectual life of Cambridge and enjoyed it to the full. His incessant search for truth impressed his contemporaries and he was long remembered as one of the company of “Apostles,” and in the Union debates was often the formidable opponent of William Harcourt, the future Liberal statesman and home secretary. He was regarded as belonging to the outspoken Johnsonian genus and his friends knew him as the “British Lion.” Yet in terms of conventional academic progress he was a failure; he was twice disappointed in attempts to gain a scholarship at Trinity, and in the May term of 1851 he went out in the “poll,” in other words, without taking honors.

An academic life was closed to him; there remained the Church, the bar, or the medical profession. He entered at the Inner Temple and was called to the bar in January 1854. He then read for a law degree in the University of London, and not only completed the course but also gained his only academic prize, a scholarship. He joined the Midland Circuit. The only two marks of distinction that fell to him during this period were the recordership of Newark in 1859 and the taking of silk in 1868.

In 1868, his life acquired a more concentrated and meaningful purpose. Henry Maine suggested that he might be appointed legal member of the governor-general’s council in India. India had for long captivated Stephen’s imagination. When he was a boy, Thomas Macaulay’s *Essays* had been his favorite book and he almost knew by heart those on Clive and Warren Hastings. The conquest of India fitted naturally into his conception of the destiny of the English which, to quote his own words, “has girdled the world with its empire, which rules those who submit, and strikes down those who resist, with more than Roman force and Roman justice.”

The spread of the English legal system and of its mode of administration was to him not only an indispensable concomitant of the British rule but a principal means in its civilizing mission. He served the British Empire and regarded himself as one of its rulers. He served it in a field that was particularly congenial to him and to his temperament. He shaped the laws which were to control the destiny of the Indian people. He was very good at it; maybe not as good as his illustrious predecessor, Lord Macaulay, but good enough to leave a distinctive mark. The influence of Jeremy Bentham on his work there was unmistakable.

In 1873, a year after he returned home, he stood for Dundee as a Liberal candidate in support of William Gladstone's government; nevertheless, later, Benjamin Disraeli thought of him as a leader of the Conservative party. And Disraeli was right in his assessment of his real political convictions, though he was wrong in thinking that he had the qualifications for a political career. In 1879, after twenty years of slow progress at the bar, he was suddenly promoted to a high and coveted position; he was appointed judge of the Queen's Bench Division. For his initial session he presided at the Old Bailey, the very court at which 25 years earlier he had held his first brief; a man accused of matricide was on trial for his life, and Stephen sentenced him to death. On the bench his portly figure, tall and solidly built, must have filled impressively the judicial robes. An air of critical deliberation was natural to his large, rather stern, features, the eyes narrowed by the constant exercises of probing things, the nose betokening a rational masculinity of character, the mouth set in determined lines and framed to pronounce judgments, the massive chin evincing the willed fulfillment of purpose that had given the man his power.

During this period, from 1879 to 1891, his capacity for achievement surpassed itself. After his busy and exacting duties in court—and many of the trials must have taxed him severely—he turned for recreation to be an author, a publicist, a member of commissions of inquiry, a codifier. Honorific recognition came his way and he enjoyed it with an unconcealed and fresh pleasure such as sometimes gives a surprising air of youth to those who toil hard and unremittingly. He was made an honorary fellow of Trinity College, an honorary doctor of laws of the universities of Oxford and Edinburgh, a corresponding member of the Institut de France,

an honorary member of the American Academy of Arts and Sciences, and a Knight Commander of the Star of India.

In 1887, his mental health seemed to be breaking down. This was made apparent during the trial of I. Lipski, charged with murder and whom he sentenced to death, and two years later, much more conspicuously, when he was a judge in a *cause célèbre*, that of Mrs. Florence Maybrick, an American by birth, accused of murdering her English husband by poisoning. A few months later, when Stephen was at assizes at Exeter, he was stricken by yet another similar attack. Questions began to be asked in parliament about a "certain judge of the queen's Bench Division" and comments became current in legal circles. Public anxiety was expressed in an article in *The (London) Times*, couched in grave and outspoken terms. Stephen at once took advice and tendered his resignation.

It must be a terrible experience for a once powerful man to become aware that his mental faculties are disintegrating. Stephen was spared this terrible awareness. He was also spared serious suffering and he died peacefully three years later. "I am surprised," he wrote towards the close of his life,

to find that, when I look back to that happiest and most blessed of days through the haze of upwards of thirty-two years, I do not feel in the least degree disposed to be pathetic over the lapse of life or the near approach of old age. I have found life sweet, bright, glorious. I should dearly like to live again; but I am not afraid, and I hope, when the time comes, I shall not be averse, to die.

When the end came, he was only on the threshold of old age, yet the sweetness and brightness had gone out of life.

Stephen's permanent contribution related to the criminal law, and his history of it stands out as a monument. The *History* (1883) suffers from certain limitations, but what he did must evoke our unqualified admiration, and it strikes one as ungracious and unfair to pick holes in a unique work of this magnitude. He was the first to interpret the present state of the criminal law by an examination of its antecedents and to blend the historical and the expository mode of analysis; the first to turn his back upon the amorphous and disjointed structure of all the current textbooks in which case was heaped upon case and statute upon statute, leaving the reader confused and bored. His statements are always lucid, succinct, and almost invariably

correct; and many passages of the *History* are brilliant. He could vividly communicate the majesty and pathos of a judicial process in which is wielded the power to affix guilt or rescue innocence when failings of human nature have led to crime, when tensions in society have jeopardized its peace, or when the vital interests of the state hang in the balance. He also regarded the history of punishments as "part of the history of the criminal law," and indeed perhaps "the most curious" part. He would be the first to raise his voice in dissent from those who even at the present day class such a treatment of criminal law as pure social history; as though a determination of the essence of an object can be reached by the simple dissection of its component parts, without a study of its growth. The *History* was saluted with respect by the learned of all nations, for it silenced at last the reproach against England for not possessing a key to the understanding of its criminal law. His labors have borne ample fruit.

His *magnum opus* was supplemented or accompanied by a number of other publications, especially his renowned *Digests*: it was reflected in several leading judgments of his which earned him the title "great master of criminal law"; and last but not least, it was enriched by his remarkable *tour de force*, the attempt to codify the criminal law of England. The Benthamite idea of codification nearly triumphed through his efforts, and although the code was ultimately thrown out, it exercised a tangible influence on penal legislation in several parts of the English speaking world.

His penal doctrine was monolithic; one might even be inclined to say, merciless. He has raised the hatred of criminals to the level of a penological principle. He expressed it in a forceful and clear cut way, and no survey of penal thought during the Victorian period can be complete without giving a prominent place to his views. But with all his conservative bent, he also made many proposals for the amendment of the criminal law and firmly upheld the liberal character of the English laws of evidence, and of the presumption of innocence.

The Victorian period was, in the main, stable; but subtle changes were taking place in moral attitudes, in religious beliefs, and in the social and political structure of society; and Stephen, acutely aware of them all, came forward as an umpire, often as a contestant, in many of the great controversies of his time. He certainly did not possess the intellectual and creative power

of John Stuart Mill, the dramatic, almost super-human, pathos and literary genius of Thomas Carlyle, the sensitive and civilized mind of Matthew Arnold, the profound originality of Henry Maine, or the massive learning of William Lecky. He is far from deserving a place among the greatest of the Victorian writers, but he was in many ways a very remarkable prophet and a singular critic and interpreter whom no student of the trends of thought and phases of opinion then prevailing or germinating can afford to ignore.

There was a puritan side to Stephen; and his puritanism derived viability from an almost physiologically reasoned acceptance of the survival of the fittest. He was convinced of the damned unworthiness of mankind and of their incurable apathy towards salvation. He was a preacher of the inevitability of pain and sorrow, our everlasting companions from the cradle to the grave, and of the individual insignificance of human life, especially when conceived, felt, and assessed in terms of a pleasurable experience. That he preferred William Thackeray to Charles Dickens was not remarkable, just a matter of individual taste in literature; but his tirade against Dickens' novels and their political and social implications reads like the indictment of a man guilty of sedition.

His book, *Liberty, Equality, Fraternity* (1873b), which first appeared in installments in the *Gazette*, was intended as a reply to Mill's lofty and moving essay *On Liberty*. Written hastily, it displays several of the defects usually associated with polemical writings. Nevertheless, the permanency of its merits is incontestable. It is described by Ernest Barker as the "finest exposition of conservative thought in the latter half of the nineteenth century," and it is listed by Hugh Cecil among the 13 works "instructive in the study of Conservative principles." But it goes beyond a statement of political alignments and ideologies. It is a contribution of lasting value to English political thought, and, to quote Lord Annan, though "a now neglected book," it deserves to be re-read; for it is in some ways even more topical today than when it first appeared.

Like Maine and Lecky, Stephen was one of the most mordant and persistent critics of democracy. Utilitarianism in his time was the Radical doctrine; and Stephen was strongly inclined to these views, yet so as to stop short of full allegiance. The utilitarians' empirical method of examining problems affecting the individual

and society, each on its own merits and according to the criteria of pain and pleasure and the principle of the greatest possible happiness of the greatest number, appealed to his case-trained mind, distrustful of generalizations. He also acknowledged the fertile contribution which they made to political thought, to political economy, and to criminal and civil jurisprudence. But in the formulation of his own opinions in any of these fields, though he was ready enough to apply their tests, yet more often than not they led him to conclusions which were much more akin to the tenets of Thomas Hobbes, Edmund Burke, or Thomas Carlyle than to those maintained by Jeremy Bentham and the two Mills. He was a realist and worked from a set of factors, largely immutable, which he recognized as deeply engrained in human nature, regulating social life, moulding the condition of a nation and determining the fate of mankind. Even Jeremy Bentham, for whom he had an unbounded admiration, was criticized by him for his lack of historical sense, for his disregard of the fact that the past contains a permanent legacy which the present cannot repudiate but must honor and, while modifying it to a certain extent, should not fail to transmit to posterity.

LEON RADZINOWICZ

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STEWARD, JULIAN H.

Julian Haynes Steward (1902-1972), a leading theorist in cultural anthropology, was the founder of the method of cultural ecology, a key figure in the neoevolutionist approach to cultural change, a pioneer in area studies, and an influential student of the social transformation of peasant populations. In keeping with these interests, his work was broad and synthetic, embodying a search for a general theory of culture. The method and direction of inquiry that he established has remained alive and fruitful to the present day.

Steward's vocation for anthropology began when, at the age of 16, he left his native Washington, D.C., to attend a private school in Owens Valley, California. There he first met the northern Paiute and Shoshoni Indians among whom he would later do research. After a year as an undergraduate at the University of California at Berkeley, he went to Cornell University, where he majored in zoology and geology—a concentration that formed the background for the naturalistic positivism of his subsequent work. He returned to Berkeley for graduate work in anthropology under Alfred L. Kroeber and Robert H. Lowie, both students of Franz Boas and tower-

ing figures in the anthropology of the first half of the twentieth century. He received his PH.D. in 1929 on the basis of library research on the clown theme in North American Indian ceremonialism.

Berkeley's anthropology department during the 1920s was small but its orientation diffuse, and some of Steward's earliest work was in archeology. He took this interest to the University of Michigan, where from 1928 to 1930 he founded anthropological instruction, and he continued it in earnest during his subsequent stay at the University of Utah. His archeological research in northern Utah opened the field of Great Basin prehistory, but his most important work was carried out in the southern part of the state, on the northern fringes of extinct Pueblo cultures. Using archeological, historical, and ethnographic materials, Steward traced the development of Pueblo social structure from its beginnings in hunting and collecting horizons through early agriculture up to the full development of classical Pueblo society. His approach was evolutionary, for it stressed the orderly emergence of institutions, but it also brought a functionalist perspective to archeology. In this respect, he can be credited with having made the study of prehistory a social science.

Steward left Utah to teach at Berkeley in 1933/1934, and spent the following year in ethnographic research among the western Shoshoni of Nevada and Utah. The Shoshoni research produced the theory of cultural ecology, probably Steward's most important contribution to the social sciences and the cornerstone of his later thought. The theory of cultural ecology is an attempt to explain social systems in terms of their accommodations to environmental and technological circumstances. It states simply that when a group of people bring a certain kind of technology to bear upon certain natural resources, then characteristic patterns of labor will emerge. This organization of work will, in turn, have a pronounced determining effect on other modes of social alignment; in this way, the material conditions of life have a causal relationship to the rest of culture.

The theory of cultural ecology was a product of Steward's training, the history of the time, and the study of the Shoshoni. Steward's concern with the environment followed the interests of Kroeber, and the stress on kinship and social organization was consistent with Lowie's orientation. His emphasis on economic and technological determinants of culture went far beyond

his mentors, however, and reflected more the intellectual climate of the depression period. His environmental focus was influenced largely by the nature of Shoshoni society, which from aboriginal times had been characterized by a simple technology, a harsh environment, and bare survival. Steward's classic monograph, *Basin-Plateau Aboriginal Sociopolitical Groups* (1938), demonstrated that thinly dispersed resources, unstable food yields, and a meager technology produced a fragmented organization of work and small, migratory bands of shifting membership among the Shoshoni. The result was a society in which the only stable and continuing social unit was the conjugal family and in which there were neither political offices nor larger corporate groups. That the Shoshoni experienced severe ecological constraints did not, however, result in a position of simple environmental determinism, for Steward cautioned that in more complex societies the impact of nature would be less pronounced.

In 1935 Steward moved to Washington where he spent the next 11 years in research and administration with the U.S. Bureau of American Ethnology and the Smithsonian Institution. There, Steward edited and contributed articles to the seven-volume *Handbook of South American Indians* (1946-1959), a work that set the stage for much subsequent ethnographic research in South America. His interests then branched from the Indian population of the continent to modern Latin American cultures, and he became instrumental in initiating and promoting community and regional studies. Many scholars shifting from one research area to another simply keep each topic separate and compartmentalized. Steward, however, always sought to expand his framework of inquiry, synthesizing new topics with older theories. His attempt to integrate the study of the modern nation with his earlier work resulted in his *Area Research: Theory and Practice* (1950), which issued a call, and produced a design, for interdisciplinary area research; the volume was a major impetus for the establishment of area institutes during the next two decades.

Steward's new interest in complex societies led him to a theory of cultural evolution that had two sources. The first, delineated in his book on area research, saw hierarchies of social complexity, or levels of sociocultural integration, to be emergent with the development of human society. Thus, the Shoshoni are at a family level of integration, whereas the family in complex

societies is imbedded in communities which, in turn, are parts of nation-states. The other source of Steward's evolutionism came from the theories of Karl A. Wittfogel and the evidence of archeology. Adopting the view that irrigation agriculture is conducive to the growth of social stratification and centralized social control, Steward demonstrated that climatic aridity and irrigation were associated in a causal manner with the rise of the state in a half dozen areas of the New and Old Worlds. The idea that aridity encouraged the development of irrigation which, in turn, required collective labor and centralized, coercive authority for its construction and maintenance, fit closely with the cultural ecological method. It produced an evolutionary theory, however, which applied to limited locales and societies. Social change and development could take a number of different directions, depending on setting and history, and for this reason Steward referred to his theory as "multilinear evolution."

In 1946 Steward left government service for a professorship at Columbia University, where he remained until 1952. He guided the work of a large number of graduate students, some of whom did doctoral research as members of Steward's Puerto Rican project. The Steward group divided the island into subareas characterized by types of economic activity, each researcher producing a community study within his district; the totality of the studies yielded a composite portrait of Puerto Rican culture in its ecological dimension and in historic depth (Steward et al. 1956). Steward pursued the study of complex society as research professor at the University of Illinois in Urbana, where he remained until his death in 1972. During the late 1950s, he undertook a massive cross-cultural study of the modernization of traditional peasant societies in which 11 anthropologists carried out studies on 4 continents. In his introduction to the three-volume project report, *Contemporary Change in Traditional Societies* (1967), Steward outlined regularly recurrent processes wherein peasant farmers are drawn increasingly into world markets and become alienated from their lands and neighbors, a trend that has since become even more evident.

Some of Steward's most important theoretical and methodological works were in the form of scholarly articles, which have been anthologized in two volumes: *Theory of Culture Change* (1955) and *Evolution and Ecology: Essays on Social Transformation* (1977). All of them, with

his book-length products, are oriented to the consistent theme that the trinity of technique, habitat, and work form a nexus upon which societies are molded and their histories influenced. Steward saw anthropology as a science on an equal footing with the natural sciences, sharing a common assumption that there are underlying forms of order and that we live in a determined and determinate universe. He saw in anthropology's comparative method and in its historicism the conceptual tools for probing the human aspect of this universe.

ROBERT F. MURPHY

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STRAUSS, LEO

Leo Strauss (1899-1973) was born in Kirchhain (Hesse), Germany. In 1921 he received his Ph.D. from the University of Hamburg for a dissertation, sponsored by Ernst Cassirer, on the

epistemology of F. H. Jacobi. His studies had been in philosophy, mathematics, and natural science, with little if any academic attention to the social sciences. He left Germany in the early 1930s, residing briefly in France and then in England before emigrating to the United States. He taught in the graduate faculty of the New School for Social Research in New York until 1949, when he left to join the political science department of the University of Chicago. After retiring from Chicago in 1967, he taught first at Claremont Men's College in California and then, until his death, at St. John's College, Annapolis.

Strauss is best known to social science as a critic of what he identified as its decisive ingredients or premises: a historicism that implied progress without a fixed frame of reference by which to recognize progress; and a scientism that lost contact with its phenomena, proclaimed itself normless, and fell into a dilemma of skepticism and dogmatism. Out of this *mélange*, he argued, grew both an incapacity for perceiving the fundamental issues and a conviction that they had all been disposed of. Inevitably, Strauss was controversial.

Strauss was not in the mainstream of academic social science practice, and he arrived at his conceptions of, and his reservations about, social science from without. Whether his detachment vitiated or fortified his judgments can be argued, but it is clear from his criticisms that he understood social science to have drawn its most important premises from sources outside itself but well within the theoretical regions in which he moved with assurance. A case in point is the appeal of social science to history as both a principle of explanation or causation and a species of surrogate for moral norms. The belief that the work of a thinker or artist cannot be understood except as a product of his times, like the view that standards of judgment and of morality are inseparable from their historical circumstances, presupposes a certain priority of change over permanence. This presupposition was not shared by Strauss, who had learned from theology to take eternity seriously and from classical philosophy the insight that led it to refer the changing to the unchanging for verification. His acknowledgment of this ancient understanding guided his judgment of historicism at the same time that it illustrated and thus confirmed his conviction that progressivist historicism must be, *qua* complacent about the past, mistaken, while, *qua* diffident before the future, merely hypothetical.

Just as the historicism of social science bore the impress of philosophy of history, so did the positivism and relativism that Strauss criticized in social science draw upon modern natural science for its conception of nature. The adoption of that antiteleological view of nature had prepared the way for the self-distinction of science from philosophy, the former the domain of facts and the latter of values. In its client or derivative relation to science, social science was seeking to develop a value-free knowledge of individual and social man, a project that Strauss criticized before generations of students as being both impracticable and harmful. It is impracticable, he maintained, because there is no way to pronounce on human affairs, except on a level of banality or technicality, without introducing what are called judgments of value. It is also harmful to attempt to do so, because the attempt itself induces an inability or unwillingness to recognize the wicked and the noble as they are bound to emerge in human action. Generally thought of as a political conservative, he feared for a liberalism that he saw as threatened with confusion by its own theory at a time when its survival demanded a capacity to call good and bad by their names and to act accordingly with conviction. The "scientific" disparagement of questions as being metaphysical or meaningless cannot effectually expel them from human concern or prevent them from arising out of the human condition, he argued, nor does the distinction of facts and values necessarily lead to the consignment of the moral issues to regions of arbitrary choice or insipid vagueness. Strauss believed classical political philosophy to be in itself a demonstration that social science was mistaken in this point.

The criticism of social science, though one of the most noticed of Strauss's activities, was not the major part of his work. Of his earliest writings, some half-dozen articles of the early 1920s, most were on the Jewish themes of Zionism and the problems of the Diaspora. In this context, his mind was led simultaneously to religion and politics—the nature of each and their mutual relation. The position of dispersed Jewry sharpened the question of how a people could exist without a political life and whether they could exist well with only a political life. The issue was complicated in this context by the fact that the people in question had perhaps had the alternative of remaining constituted by a revealed law attached to no earthly location and being a political body constituted by positive

and artificial law. Whether the alternative was effectual depended on whether the civil emancipation of Jews could lead to their acceptance and assimilation, and whether it might not lead to their ultimate absorption, more or less in spite of themselves. The question whether absorption should be welcomed or resisted attached itself urgently to the question of the status of revelation; for if revelation is what it declares itself to be, then assimilation should be resisted—unless revelation can somehow be interpreted to demand the reunion of all mankind through the supersession of each particular faith as merely sectarian. But if revelation is what it declares itself to be—namely, an emanation from a supreme source that must be forever mysterious to human reason wherever it has not disclosed itself—then the claims of reason and the pretensions of philosophy are decisively limited. By an uninterrupted course of reflection, Strauss moved from the problems of a Jewish existence, which was his primary experience, to the question of the standing of reason or philosophy, which was his abiding preoccupation. He approached similar issues by a collateral route that he sketched in a note, published in 1923, entitled “Das Heilige,” a comment on Rudolf Otto’s book of the same name. Beginning again with the absolute transcendence or unknowability of God, he emphasized the location of that “irrational” not in the human subject’s religious, psychological, or sociological experiences or conditioning but in the object itself; and he distinguished an older theology (of divine attributes, or of the object) from a newer theology (of man and his experiences, or of the subject) in a way that did not quite parallel, but did to some extent employ, the distinction of ancient and modern thought. Very early in his career Strauss conceived his reservations against subjectivism, against an unquestioned faith in modernity, and against a rationalism or scientism that was lax about establishing its own foundations or inexact in its self-understanding. Perhaps it is fair to say that the underlying question throughout is the relation of the rational and the irrational—whether they are necessarily mutually invulnerable and hence not subject to a ranking based on the suppositions of either; and whether philosophy constitutes, or points to, a clear victory for the unassisted human reason.

Strauss’s first book, *Die Religionskritik Spinozas* (1930), pursued these themes in detail. Strauss examined Spinoza’s foundation of the

higher criticism of the Bible in order to discover whether the explicit confrontation of Biblical revelation with liberal rational philosophy had produced the triumph of reason that philosophy was claiming, and he concluded that it had not. He approached Spinoza’s work by surveying the ancient and modern critiques of religion, beginning with the Epicurean, that formed the tradition in Spinoza’s background, and he detected in each case a theoretical or moral presupposition or intention that might be defensible or plausible but which was in some degree arbitrary, hence not capable of commencing an absolutely apodictic argument. In passing from the Epicurean “presupposition” in the form of a practical intention to banish fear of certain invisible powers to the modern grounds of critique of religion, Strauss anticipated in this early work not only the antithesis of antiquity and modernity that he elaborated in many writings, but also the division of modernity into phases or, as he would say some forty years later, “waves.” While still viewing modernity in the context of critique of religion, he saw a distinction between the moderns who resisted religion in the name of the present world and its gifts of fame and opportunity for heroism, and those other moderns whose interest was freedom to live and to think without accepting revelation—freedom to philosophize. The distinction between Machiavellian and Spinozistic modernity does not disturb the progress of the argument regarding “the rational” and its claim of priority in relation to revelation: the post-Biblical critics of religion contradicted but did not refute their opponents, having refused to take seriously the possibility that God has the attributes of omnipotence ascribed to him by Scripture. This means that the case for the unassisted reason, or the case for philosophy, either had yet to be made, or had already been made by the pagans, who were not constrained by contention with revelation or by any avowed intention to criticize religion or gods. Those pagans would be Socrates, Plato, and Aristotle.

Strauss pursued similar themes in *Philosophie und Gesetz* (1935), but in the meantime he had been at work also on Hobbes, and in 1936 he published *The Political Philosophy of Hobbes*, the first of his books on leading figures in the tradition of political philosophy. The intention behind his work on Hobbes was to develop his reflections on the ancient and modern rationalisms, keeping in sight the question of the self-sufficiency of reason in its successive self-under-

standings but concerned also with the ground that reason alone thought itself able to furnish or discover for morality. In Hobbes, Strauss found reason surrendering its claim of dominance over human life to the passions, which are more powerful, thus primary and more authoritative; but he found also the assertion of a claim on behalf of reason to be the arbiter of any dispute that may arise between it and anything representing itself as revelation. The manifest tension between the self-abnegation and the pretension of reason found an apparent resolution on the plane of morals and politics in the form of natural right. That is, the overriding human motive of fear of death at other men's hands, or, more positively, the supreme impulse toward self-preservation, pointed to the inalienable natural right to life and the means to it. In order to give effect to the right of nature, a political order in which the sovereign was also the chief priest, saving man from nature here on earth, would control our common existence. The banishment of "superstition" is rationalistic, to be sure, but it is in the interest of civil peace, not of philosophy. This manner of advancing yet profoundly diminishing the claims of reason, and at the same time apotheosizing preservation, was decisive for what Strauss referred to as the lowering of the sights of human aspiration characteristic of the birth of modernity. Strauss saw a manifestation of this moral retreat in the bourgeoisification of the moral perspective, a falling away from the typical identification of virtue with aristocratic virtue in proportion as man's fear of man was openly confessed to rule the human estate. It is worth noting that, as Strauss found the presuppositions or prejudgments of the critics of religion to have dominated those thinkers' rationalism, so he found that "[Hobbes's] moral attitude does not by any means appear only at the end of a long process, but directs this process from the beginning" (1936, p. 129). This view led Strauss to maintain, contrary to the position of many interpreters, that Hobbes's moral and political principles were not derivative from his natural science but were independent or primary in their own right.

After the publication of *The Political Philosophy of Hobbes*, Strauss began his academic career and also his residence in America. During a period of approximately ten years, he developed, through courses at the New School and in a succession of articles and reviews, a comprehensive conception of the relation of

ancient and modern thought that he brought together in the six Walgreen lectures in October 1949 that served as his inaugural at the University of Chicago. Those lectures, enlarged, appeared in 1953 as what is perhaps his best known book, *Natural Right and History*. In this work, Strauss contrasted classic and modern natural right, showing the penetration of the latter not only by the "lowering of the sights" but also by the historicization of the moral norms. Strauss's lifelong orientation upon the unchanging, his rejection of relativism, and his inclination to classical philosophy might suggest that he adopted some form of moral "absolutism" drawn from the strong teachings of Plato and Aristotle. However, a leading theme of *Natural Right and History*, and of Strauss's work altogether, is that classical moral norms, or the right by nature, avoided both the rigors of Thomism, which straitly links the moral law to perfect divinity, and those of Averroism, which radically linked morality to occasions and judgments. The Thomist and Averroist branches of Aristotelianism missed the intermediate mark of the natural right that is subject to differentiation or change according to circumstances, but that nonetheless has a shape. Its shape or direction is given ultimately by the telos or end of human activity, of human life as such, which is an end indicated by man's rationality as his natural essence. If man's highest function is thought, and his most human act is to philosophize, then the norms of morality are those norms that directly or indirectly best conform to, or least offend against, that natural end. It may also be said that the "end" or point of orientation for man is the unchanging truths, around which everything merely human, including the rules of moral and political existence, flows in a state of changeableness. Whatever else they contain, those truths include an ordering of the goods that are available to man, and by the light of that unchanging natural teleology men are charged with the task or endowed with the freedom of arranging their affairs in the myriad contexts of human life. Writing about classic natural right, Strauss declared, "There is a universally valid hierarchy of ends, but there are no universally valid rules of action" (1953, p. 162). The best political order, that cynosure of classical political philosophy, was understood by Strauss as the régime in which the ascendancy of philosophy is the guarantee that justice be accompanied by the discretion of wisdom.

Strauss published a number of important

books after 1953, including *Thoughts on Machiavelli* (1958) and a series on the ancients, but the preoccupation that he evinced in the article on the book *Das Heilige* with transcendence of the mutable by an eternal, and with the arbitrariness that silently enters the "rationalistic" dismissal of the eternal same itself remained the same. With this in mind, one could say without grave distortion of the facts that Jerusalem and Athens (the theme of his Frank Cohen lecture at the City College of New York in 1967) were and remained the poles of his life and thought. In his last years, devoted largely to the problem of Socrates, he published books addressed to the Aristophanic and the Xenophonic presentations of Socrates (1966; 1970; 1972) and completed an interpretation of Plato's *Laws* (1975) that was published posthumously.

Strauss philosophized in the medium of history of philosophy. It was open to him to do so because the critical history of philosophy is necessarily on the plane of its own object. His inclination toward the ancients could not be characterized, as such an orientation sometimes is, as affectation or nostalgia. As a historian of the tradition of philosophy in pagan antiquity, in Christendom, Judaism, and Islam, Strauss perceived that many authors expressed themselves cautiously if their words could harm either themselves or others. He employed and taught what came to be called "careful reading," but he did not use or impart a "method," for by the nature of the case there cannot be one, since reticent writing that could be made explicit through the application of rules would be a mere cipher and the interpretation of philosophic texts would be a form of cryptography. Strauss explained himself on this surprisingly provocative subject in several articles, including "Persecution and the Art of Writing," which gave its name to a collection of essays published in 1952. This merely instrumental aspect of his work gave rise to controversy that apparently must defy final resolution.

JOSEPH CROPSEY

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SWISHER, CARL B.

Carl Brent Swisher (1897-1968), American political scientist and constitutional historian, was a pioneer in the fields of judicial biography and American constitutional development. Born in rural West Virginia, the second child and only son of farmer parents with Methodist and Republican leanings, he studied at West Virginia Wesleyan College and Pomona College, California, before receiving his PH.D. degree from the Robert Brookings Graduate School of

Economics and Government in Washington in 1929. After serving as an instructor at Columbia University and as a special assistant to the attorney general of the United States, he joined the faculty of the Johns Hopkins University, where he succeeded W. W. Willoughby as Thomas P. Stran professor until his retirement. He held lectureships at other universities, including Chicago and London, and was elected president of the American Political Science Association.

Swisher secured his reputation in public law with several classic studies at opposite ends of the social science spectrum: life histories of individual judges and institutional histories of the United States constitution and Supreme Court. As a scholar he joined a distinguished group of political scientists, including Edward S. Corwin and Charles G. Haines, who in different ways pioneered recognition of the political functions of the Supreme Court in American society (Corwin 1938; 1964; Haines 1922). Both strands of his work were influenced by the sociological jurisprudence and legal realism that were fashionable during his formative professional years, though he was more interested in economic and social class analysis than in the Freudian interpretations advanced by Jerome Frank (1930) and Harold D. Lasswell (1948). A teacher at Pomona, Russell M. Story, and the interdisciplinary program at Brookings, including Walton H. Hamilton's seminar in law and economics, broadened Swisher's viewpoint. His work with Carl McFarland in the Justice Department stimulated a lifelong interest in judicial administration and administrative law (Sobeloff 1968). Challenges to constitutional government during two world wars and the great depression also affected his outlook.

Central to his thought were faith in the principles of American constitutionalism and skepticism of the myths of a fixed constitution and an objective judiciary. The student who failed to understand that the constitution was a living charter of government, and that the Supreme Court was "a broker between ancestry and posterity," had much to learn about judicial review and the character of American institutions (1946*b*, p. 560; 1946*a*, p. 17). Swisher's goal was to explain processes of constitutional change, and his search led him far beyond legal materials to the study of party politics, inter-governmental relations, social interests, and personalities. Yet he took a dim view of universal theories and uniformity. Microcosm, for him, was macrocosm (Rourke 1968, p. 56; Swisher

1946*a*, pp. 48–49). He also expected the scholar to evaluate as well as understand the performances of those who wielded public power. His own judgments were the more powerful for their dignity and fairness.

Treating Supreme Court decisions as political events within a broad historical and social context necessarily shifted emphasis from legal doctrines onto judges and other participants in the process of constitutional interpretation (Pritchett 1969, p. 35). Even his master's thesis, a study of state constitution making, employed this broad perspective (1930*a*). That the general revealed itself in the particular was strikingly demonstrated in his first major works, judicial biographies of Stephen J. Field (1930*b*) and Roger B. Taney (1935). Although Albert J. Beveridge's magisterial *Life of John Marshall* (1916–1919) had opened the way, Swisher's biographies were more daring in showing the union of judicial values with those of dominant social interests during the Jacksonian era and the age of enterprise. By exposing the inner workings of the high court, moreover, he demonstrated the potential of biographies as case studies in judicial politics (Murphy 1968; Murphy & Tanenhaus 1972). The two life histories rank among the best judicial biographies ever written.

The extended treatment of the national bank issue in *Taney* also marked Swisher's shift from biographies to constitutional history. His most influential work was the monumental *American Constitutional Development* (1943), the first full account of American constitutional growth in the stream of national history. For a generation, this concept of constitutional development pervaded undergraduate teaching of public law.

Several books of lectures, in which he appraised modern constitutional problems in the light of history, revealed much about Swisher's own values (1946*a*; 1948; 1958*b*). Taking seriously the notions that law and legal process constrain judicial discretion, he found it easier to justify the demise of old judicial activism in behalf of private property than to endorse the rise of judicial activism in behalf of economic underdogs and civil rights. While generally sympathetic with the goals of the Stone and Warren Courts, he criticized the centralization, self-assertion, and craftsmanship of their decisions. The growth of judicial policy making at the expense of legislatures and executives, where leadership properly belonged, also troubled him. But he acknowledged that political

default was largely responsible for judicial activism and that history's verdict would rest on the justice rather than the process of judge-made law (1946a, pp. 48-49; 1958b; 1960b, pp. 885-886).

The capstone of his career was his selection, alone among nonlawyers, to write one of eight official histories of the Supreme Court under the Oliver Wendell Holmes Devise (1974). Published after his death, *The Taney Period* was a prodigiously researched study that ranks among the leading histories of the Supreme Court from the death of Marshall to the Civil War. The analysis, largely independent of revisionist interpretations by historians, ranged broadly beyond constitutional issues into admiralty, patents, and judicial administration. Some readers may miss the grand sweep and succinct summaries of his earlier works.

As behavioralism spread to the study of public law, Swisher's approach came under attack, in part for not having gone far enough. Judicial biography fell in popularity among political scientists, who by and large also left the field of constitutional development to historians. Behaviorists criticized historical studies as descriptive, antitheoretical, and nonquantitative, and above all for focusing on ideas and institutions rather than patterns of political behavior (Schubert 1967; 1968). Champions of the group approach criticized judicial biographies as emanations of the cult of the robe (Peltason 1964). Swisher, to be sure, had little interest in empirical theory as distinct from normative political thought. Moreover, as an organizing device constitutional development risks saying both too much and too little about the policy functions of the American judiciary, to the neglect of questions of compliance with and impact of judicial decisions, and of issues pertaining to statutes, administrative review, and lower courts. Although Swisher was less vulnerable to criticism on these counts than were most contemporaries, his linkages between individual and institutional levels of analysis were crude causal explanations like "social forces" or personal "will to power."

Nevertheless, his writings reveal unusual sensitivity to the determinants of judicial behavior in their profound complexity. If anything, his work suffered most from attempting to explain too much. Advancing a broad, functional view of constitutions, even as he held a restrictive view of the judicial function, he saw judging as a highly complex travail (1946a; 1946b).

In personality he was a reserved man of courtly manner who warred not against his critics. Yet just as he had bathed the Supreme Court in the acid of historical realism, so he warned against the reverse emphasis on adjudication as "merely politics" (1960b, p. 879). To treat judging without law was to obscure what made courts and constitutions unique (Pritchett 1969; Shapiro 1964).

Swisher was thus an important transitional figure who made classic contributions to an understanding of the constitution and the Supreme Court in American political life. His studies of individual justices made him, willing or not, "one of the first . . . 'judicial behaviorists'" (Pritchett 1968, p. 708). His constitutional histories built bridges across time and among disciplines. He helped to return the study of public law to the mainstream of an emerging discipline of political science, while mapping essential relationships among courts and larger processes of constitutional government. The bedrock of his teaching was that law and politics were entwined in the government of the American people.

J. WOODFORD HOWARD, JR.

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TAEUBER, IRENE B. AND CONRAD

Few marriages have combined social scientists as productive as Irene Barnes Taeuber (1906–1974) and Conrad Taeuber, born in 1906, both leading figures in the field of demography over a span of some forty years. Intellectual products of the early depression years, they both maintained a pragmatic empiricism throughout their careers. However, their contributions to demography, and more generally to the social sciences, were divergent. Irene Taeuber was a “pure” scholar, totally committed to scientific research. Conrad Taeuber combined scholarship with major administrative responsibilities in the U.S. Department of Agriculture, the Food and Agricultural Organization of the United Nations, and the U.S. Bureau of the Census, in each of which he was a vital, if often anonymous, contributor to both the collection and the interpretation of data.

A remarkably productive scholar, Irene Taeuber was author, joint author, or editor of 18 books and monographs and some 168 articles and contributions to books, the great majority under her exclusive authorship. She had much to do with the growth of the fledgling science of demography in the 1930s and 1940s. She was an editor of the bibliographical *Population Index*, then the only regular publication in the field in the United States; it was largely her responsibility and product for the first 17 years of its existence (1937–1954). She regularly wrote what was in effect a research article, somewhat misnamed “Current Item,” in each quarterly issue. Although she was engaged in

other major research after 1954, she continued frequently to contribute leading articles to *Population Index*.

From 1937 to the end of her career Irene Taeuber, though physically located at the Library of Congress in Washington, was research associate (and from 1962 senior research demographer) of the Office of Population Research at Princeton University.

Like her husband, Irene Taeuber did her graduate work at the University of Minnesota (PH.D. in 1931) under the influence of such social scientists as Stuart Chapin and Lowry Nelson. She belonged to the tradition of data-oriented empiricists, mellowed by an anthropological viewpoint gained in her early training. Her work also reflected the feminist humanism that flourished in the United States before World War II and largely disappeared in the 1940s and 1950s, to emerge again in the more strident feminism of the 1970s. Although her research reflected rigid devotion to scientific objectivity, her underlying values were clearly humanistic.

The substantive content of her work included diverse subjects, such as “Heredity Factors in Mental Disease” (1938), but from 1940 on her many publications were almost exclusively in demography, though often in a broader sociological context. Her frequent research on the United States population was related to, or conducted in collaboration with, that of Conrad Taeuber at the Bureau of the Census. They were joint authors of two major and unique studies of the American population in historical, sociological, and economic context.

Although many aspects of demographic research were influenced by Irene Taeuber's contributions, perhaps the most important were international population studies, in which she displayed a strong interest. Her own background was purely Middle American, but her intellectual interests, professional contributions, and personal associations came to be world wide. She did much to bring an international and comparative perspective to a discipline too preoccupied with the United States and with local data. Certainly no demographer, perhaps no social scientist, has written serious research articles on so many countries, as diverse in size, population, climate, and culture as the islands of Micronesia, with their miniscule populations, China, Alaska, and Singapore, and covering one or more countries and regions in every continent. During World War II she wrote a series of studies, each based on in-depth research, of countries then affected by the war.

Her chief interest, however, was in the demography of east and southeast Asia. Her *magnum opus* was *The Population of Japan* (1958). In depth of research, breadth of focus, and detailed analysis of a non-European country undergoing expansion, war, defeat, and demographic transition, there is certainly no parallel or peer in demographic literature. It was translated into Japanese and widely used in that country. In her later years her principal research was devoted to Chinese demography in and outside mainland China, an area that engaged much of her attention at the time of her death. Unfortunately, the inevitable fragmentation of research on Chinese demography and Irene Taeuber's untimely death precluded publication of a work that might well have had an impact equal to that of her work on Japan.

She belonged to an era preceding the emphasis on mathematical models in demography and theory in sociology. Skeptical of orthodox demographic transition theory, she wrote frequently of qualifications and exceptions. Her lively suspicion of careless generalization was attributable in part to her early anthropological training. A meticulous scholar, she was given to imaginative and thorough analysis rather than to generalization and theory construction.

Irene Taeuber can be credited also for having nurtured an infant science in its most formative years. She played a major part in guiding its growth in scientific objectivity within a humanitarian value system. As a result, demography, as a science, has remained relatively free from

the wild rhetoric and popular exaggeration that have swirled around it in the media and in popular discussion.

Conrad Taeuber was one of the able and dedicated professionals attracted to Washington in the early days of the New Deal. At the Bureau of Agricultural Economics in the Department of Agriculture (1935–1946) he early established a reputation as an authority on the statistics of rural population and population movements in the United States. This focus broadened when he became chief statistician in the Food and Agricultural Organization of the United Nations (1946–1951). In later years (1951–1973), as associate director of the U.S. Bureau of the Census, he was a major architect of both the methods and the data used in the 1960 and 1970 censuses of the United States and of its publication program. This role, though usually anonymous, gave him a central position in the largest demographic, social, and economic survey in the world, providing data that were collected and processed with great care, to be used, often a bit cavalierly, by those unaware of the care and research involved in this vital service.

Taeuber was the primary force in the Census Bureau for promoting middle-level analysis of the prodigious census output. Largely through his initiative the bureau (in cooperation with the Social Science Research Council) undertook programs of census monographs for the 1950 and especially the 1960 censuses. In cooperation with his wife, Taeuber prepared major volumes on *The Changing Population of the United States* (1958), an historical survey of the census results from 1870 through 1950, and on *People of the United States in the 20th Century* (1971), which in turn placed the census of 1960 in broad historical context. The former was the first systematic monograph since that based on the 1920 census that analyzed and organized the major results of a population census in readable yet scientific form. The second work written primarily by Irene Taeuber, is a monumental volume of more than a thousand pages in which the statistical results are examined within a broad framework of historical, sociological, and economic change.

The latter volume in particular views the census as a statistical documentation of the vast social changes that have occurred in this country's history since 1790. It is the most ambitious extant first-level analysis and interpretation of the prodigious and sometimes confusing publications of the census, summarizing in socially

meaningful categories the enormous wealth of data whose significance is often concealed by their very abundance. The volume organizes the unwieldy mass of census figures into such topics as the changing geographical distribution of the population; urbanization; population structure (especially age); economic activity, education and income; the family; marriage; fertility; mortality; migration, mobility and stability; the dynamics of metropolitan population; and the dimensions and perspectives of the future.

Inevitably such a work is to some extent shaped and limited by the data it summarizes. But it provides a very solid foundation for more specific problem-oriented work. This is the heart of the Taeubers' contribution—to provide first a solid data base, and then first-level interpretation of the meaning and significance of the data. They built a solid foundation, in quality and usability, for both the decennial population census and the increasing volume of intercensal studies and surveys.

Conrad Taeuber's extensive contributions in more than one hundred publications are largely confined to data from the United States, but his interests and activities have included wide international contacts. He has served as president of the Inter-American Statistical Institute and participated in innumerable international meetings and consultations.

Although his scholarly contributions have been noteworthy, Conrad Taeuber's role could best be described as the professional promotion of better basic census data in the 1950s, 1960s, and the early 1970s. One of his many innovations to advance the usefulness of census data was the preparation of sample census tapes (e.g., one in a thousand) suitable for, and now widely used for, research and teaching purposes in the United States. The procedure is now adopted in many other countries.

After his formal retirement in 1973, Conrad Taeuber continued as director of the Georgetown University Center for Population Research and as a vigorous proponent of the application of the census in the measurement and identification of social problems, expressed in *America in the Seventies: Some Social Indicators* (1978).

The expertise of Irene and Conrad Taeuber is also a family affair. Their two sons and daughters-in-law have followed them in their profession of demography and related fields.

DUDLEY KIRK

WORKS BY THE TAEUBERS

WORKS BY IRENE B. TAEUBER

- 1938 Hereditary Factors in Mental Disease. Pages 156–161 in United States National Resources Committee, Committee on Population Problems, *The Problems of a Changing Population*. Washington: Government Printing Office.
- 1939–1973 *Population Index*. → Includes 47 research articles by Irene B. Taeuber. A substantial number of her publications in other scholarly journals were spin-offs, translations, and especially presentations of further research based on these articles, which covered a wide range of demographic subjects.
- 1958 *The Population of Japan*. Princeton Univ. Press. → This book was preceded and followed by some thirty publications in scholarly journals, Japanese translations, symposia, etc., relating to the Japanese population.

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WORKS BY IRENE B. AND CONRAD TAEUBER

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TANNENBAUM, FRANK

Born in Brod, a small town in Austrian Galicia, Frank Tannenbaum (1893–1969) emigrated to the United States in 1904. The America he grew up in was also an America growing up. The frontier may have closed in 1890, but it still possessed its mystique; the first decade of the new century was the age of Theodore Roosevelt and the great influx of immigrants, who hoped that the burdens of Europe would be lifted by the special grace of the New World. Although American history was presented as a chronicle of progress, there was another, darker side that the muckrakers exposed, including the sins of the city and the excesses of capitalism. Most Americans, however, considered these problems to be temporary and remediable once recognized and approached with planning and energy directed toward their solution.

In 1906, Tannenbaum began to attend night classes at the Ferrer School, "the first institution devoted to the constructive side of anarchism." There anarchist intellectuals, radical labor leaders, and social activists met together and were able to learn from each other about the world as it was and could be, sharing millennial visions and the feeling that by proselytizing they could realize them. Among the faculty of the Ferrer School were Emma Goldman, Lincoln Steffens, and Alexander Berkman (see *The New York Times*, 1911, Jan. 6, p. 20, col. 2; Jan. 15, part 5, p. 10; Jan. 17, p. 8, col. 3).

By the middle of the second decade of the century, Tannenbaum's interests centered on the problems of the poor workingman and the unemployed. He joined the "Wobblies," the Industrial Workers of the World, and soon became a local leader. For Thorstein Veblen, the IWW was an "exuberant" and "untidy" vanguard of dissent. It was a radical labor group that sought to effect social changes by sporadic violence and strikes. In the winter of 1913/1914, thousands of unemployed workers roamed the streets of New York, hungry and desperate, in vain search of jobs and the IWW message held out a promise of immediate relief. On March 1, 1914, Tannenbaum led a group of three hundred homeless poor into the First Presbyterian Church and demanded money for food and shelter. After some negotiation, he persuaded the church leaders to provide each of the "sit-down strikers" with thirty cents. This technique worked and was to be repeated at different churches; Tannenbaum exhorted his followers to organize and demand an 8-hour workday and a minimum daily wage of \$3 and to continue their sit-downs in churches until these basic demands were met.

Radicalism was essential to Tannenbaum's social philosophy; violence was not. His experiences with radical reform ideas, organized labor, arrest, and imprisonment became subjects of intellectual inquiry. His radical thinking, although close in some respects to the socialism of Eugene V. Debs, never developed or converted into a set ideology. He retained the Wobblies' goals of improving the worker's condition and his compassion for the underdog, but he came to distinguish between reform and apocalypse, to recognize the complexities of human organization, and to see law, as both an instrument of and an obstacle to progress.

Tannenbaum's activist role in labor developed

into a lifelong concern with the labor movement and the union. Surprisingly, for a radical who ran afoul of the law—ex-convict as he referred to himself on occasion—he did not have a political axe to grind, and his writings are free of venom or invective. He looked upon his involvement reflectively. When he wrote of labor and of most other subjects, he wrote as a historian with a sociological bent. The trade union, he believed, had a special purpose in industrial society; it was an institution with its own makeup and morale and mission; paradoxically, one of its major contributions was to ensure social stability. In his first book, *The Labor Movement: Its Conservative Functions and Social Consequences* (1921), which was "humbly dedicated to John Dewey," Tannenbaum wrote: "It is the ideal aim of the labor movement to abolish revolutions. It aims to eliminate the cost of human sacrifice due to social change by making change a pragmatic and deliberate thing" (p. 167). None of his writings on labor was pure economic history or mere sociological interpretation. His analysis of labor was an alloy of these and more. Although his viewpoint became increasingly detached, he never forgot his personal experience as a labor leader.

When he wrote his first book on crime and society, *Wall Shadows: A Study in American Prisons* (1922), Tannenbaum was concerned with the complex nature of crime and community organization in prisons. The book describes what happens to the criminal after conviction and, though the focus may appear narrow, Tannenbaum's attitude is nonetheless that of a reformer who hoped that the book would "help some few to take up the cause of society against our medieval prisons." Tannenbaum's most detailed study of the general problem was *Crime and the Community* (1938). Although he did not write it as a textbook, it soon became a basic source, widely adopted in criminology and sociology courses in colleges and universities throughout the United States; later, parts of the book were made required reading for the Chicago police. *Crime and the Community* was for the study of the organization of crime what *A Philosophy of Labor* (1951) was later for the study of labor. Both were written after many years of direct experience and firsthand knowledge; both represent a summing up of Tannenbaum's studies, reflections, and, to a degree, predictions about these two different subjects. He believed that prisons could, with difficulty, be improved,

but that crime itself was an inevitable problem permeating all levels of society.

Tannenbaum's experiences and studies in labor organization and in crime (as a necessary and natural aspect of society) taught him much about institutional structures; the habits people adopt and the roles they assume; the pressures placed on the human personality by the group or groups within which an individual acts; and the difficulties that attend any effort to change any institution radically. He held that we live in an "intractable" and "recalcitrant" universe (words that frequently recur among his writings), and interpreted the meaning of "utopia" in its etymological, literal sense as "not a place." "Until I went to school I thought there was only one way to accomplish an end," he once said. "Now I know that there are many ways. The study of history is dangerous to radicalism. One of the greatest blows I suffered in college was the realization that the world had a past as well as a present and a future."

Columbia College, to which he had been admitted in the fall of 1916, was a new and stimulating experience for Tannenbaum, and his attachment to the institution and its members continued to the end of his life. His first appetite for history was aroused by Carlton J. H. Hayes. His interest in economics developed from courses taken with E. R. A. Seligman, and his concern with education and the philosophy of experience grew from his friendship with John Dewey. He became good friends with these and other Columbia luminaries.

The university seminars at Columbia, over which Tannenbaum presided as director for 25 years, may be seen as an outgrowth of his contacts with friends and professors at Columbia. Near the end of World War II, the Columbia University administration asked the faculty to make suggestions for improving the intellectual quality and relevance of the institution. It was in response to this canvas that Tannenbaum, by then professor of Latin American history, came up with his suggestion of "University" (i.e., interdisciplinary) seminars for faculty members and invited outside associates, to concentrate on the discussion of selected lasting problems. In part, the seminars grew out of Tannenbaum's experiences with regular lunch meetings with friends. Partly, too, they evolved from his feeling that a classroom or traditional research situation had many obstacles to the free play of the intellect. And, finally, they de-

veloped out of his conviction that "the world could not be divided into political science, economics, sociology or history. . . . The subtleties, complexities, and interlacing of the threads that tied life together and made it meaningful lay beyond the reach of the specialized disciplines" (1965, p. 3).

All of Tannenbaum's works on Latin America had an underlying theme: a concern with the processes and mechanisms by which a stable political system functions—legitimacy, transfer of power, political parties, constitutionality, local government, and political culture. He was intrigued by both the British and American models of government, but considered the British system the better example of continuing effective political stability. Indeed, among his posthumous papers is a long unfinished manuscript on elements in the British system that allow it to operate with such uncommon smoothness.

Mexico and its revolution interested him for most of his academic life. He wrote three important books on various aspects of change in Mexico: *The Mexican Agrarian Revolution* (1929), *Peace by Revolution: An Interpretation of Mexico* (1933b), and *Mexico: The Struggle for Peace and Bread* (1950). The central theme in all was the course of the revolution, but Tannenbaum also took into account the history and the economic, social, racial, and cultural conditions of Mexico. In the first work, Tannenbaum closely examined the landholding patterns, especially the *hacienda*, as they changed under the impact of the revolution. As he was writing *Peace by Revolution*, he felt that "the social revolution that has intermittently torn Mexico during the last twenty years may be best understood as an attempt to liquidate finally the consequences of the Spanish Conquest. This explanation of the revolution is at the same time the best key to Mexican history" (1933b, p. 3). Tracing the dominant trends in the Mexican experience, he was not wedded, as he took pains to make clear, to a single or unitary explanation of historical or social phenomena.

Perhaps Tannenbaum's most important book, in terms of its impact on the scholarly world and American society, is *Slave and Citizen* (1947). Three leading contemporary historians, Stanley M. Elkins, David Brion Davis, and Eugene Genovese, have assessed it as a "pioneering essay" in the historiography of black slavery. The book developed out of an interdisciplinary

seminar on slavery held at Columbia in the late 1930s and early 1940s in which this peculiar institution was studied comparatively, as a common theme in the historical experience of different cultures.

The Latin American seminar at Columbia, over which Tannenbaum presided from its inception in 1944 to its end in 1969, was itself a peculiar institution. Over its 25-year life, it welcomed many important Latin Americans as guests, participants, and speakers. Professors, ex-presidents, journalists, poets, artists, economists, students, labor leaders, and diplomats were among those who came to the seminar, drawn by its reputation among Latin Americans as their forum in the United States.

Frank Tannenbaum was a man of diverse intellect. It is impossible to categorize him as a Latin American historian, for he was many things more than that. Perhaps the best label for him would be that of social philosopher. His focus basically was upon institutions singly and in competition with each other and he understood that all human experience takes place within an institutional framework of one kind or another. As he wrote in *The Balance of Power in Society*:

The road to social peace is the balance of the social institutions, and a wise statesman would strengthen those institutions that seemed to be losing ground, even if he were not addicted to them: for the only way to peace in this world of fallible human nature is to keep all human institutions strong, but none too strong; relatively weak, but none so weak as to despair of their survival. It is only thus that peaceful irritation and strife, so essential to social and individual society, can be maintained. (1969, p. 23)

JOSEPH MAIER

WORKS BY TANNENBAUM

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 (1922) 1975 *Wall Shadows: A Study in American Prisons*. New York: AMS Press.
 (1924) 1969 *Darker Phases of the South*. New York: Negro Universities Press.
 (1929) 1968 *The Mexican Agrarian Revolution*. Hamden, Conn.: Archon.
 1933a *Osborne of Sing Sing*. Chapel Hill: Univ. of North Carolina Press.
 (1933b) 1971 *Peace by Revolution: An Interpretation of Mexico*. Freeport, N.Y.: Books for Libraries Press.
 1934 *Whither Latin America? An Introduction to Its Economic and Social Problems*. New York: Crowell.
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 1947 *Slave and Citizen: The Negro in the Americas*.

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 1951 *A Philosophy of Labor*. New York: Knopf.
 1955 *The American Tradition in Foreign Policy*. Norman: Univ. of Oklahoma Press.
 1962 *Ten Keys to Latin America*. New York: Knopf. → A paperback edition was published by Vintage in 1966.
 1965 *A Community of Scholars: The University Seminars at Columbia*. New York: Praeger.
 1969 *The Balance of Power in Society, and Other Essays*. New York: Macmillan. → The essay, "The Balance of Power," first appeared in the December, 1946 issue of the *Political Science Quarterly*.
 1974 *The Future of Democracy in Latin America*. New York: Knopf.

TAX, SOL

Sol Tax, born in 1907, has been involved in the principal dialogues of anthropology spanning four decades of association with the University of Chicago's department of anthropology, from the commencement of his graduate study in 1931. Where these dialogues have concerned the role of anthropology in assisting indigenous peoples in maintaining cultural identity he has been a central figure, and in advocacy of self-determination for American Indians he has been the acknowledged leader.

Working in contexts of persistent culture contact, where values, vested interests, and levels of power vary markedly, and where concerns with identity and self-determination produce factionalism and conflicting group interests, Tax and his students have focused research and assistance to Indians on communication, leadership, and strategies for achieving the consensus requisite for action. Tax has addressed with equal dedication the challenges Indians face in uniting on goals and the obstacles those goals confront in the larger arena of negotiation with non-Indians. In the process he has learned much about the use and abuse of power and the wisdom of its self-limitation by those in positions of authority.

Tax's career began in the traditional way, with study of social organization relatively divorced from the issue of how the presence and status of the anthropological observer influences the observed society, but a turning point occurred in the late 1940s with his decision to place the anthropologists' knowledge and influence at the service of the Mesquakie (Fox) Indians he and students were studying. Two related convictions formed the basis of a phi-

losophy of applied anthropology which came to be known as "action anthropology" (Tax 1952a; Gearing, Netting, & Peattie 1960). One is the conviction that the anthropologist's influence and interaction with the observed society constitute an ever-changing and inescapable object of study for the responsible ethnologist, the more so given the origins of anthropology in colonialism and the power and brokering capability the anthropologist usually is perceived to have. The second conviction is that this interaction should be utilized to expedite the learning inherent in decision making, and, through feedback, to enhance effective action by the less powerful subcultures of pluralistic societies in pursuing self-determination.

In questioning the wisdom of distinguishing sharply between pure and applied research, action anthropology departed from the main stream of post-World War II applied anthropology, wherein the objective of easing acculturative strain was seldom tied to a theory of knowledge acquisition and development. In action anthropology, helping and learning are coequal goals in observing how feedback of what is known and happening to people changes their perceptions, attitudes, and behavior. Tax's insights into the cybernetic character of knowledge development may be his most lasting contribution to the social sciences, but during his career his more readily acknowledged contributions have been in championing the rights of indigenous peoples to self-determination and mobilizing anthropologists to this end. His philosophy of action prescribes "participant-intervention" by helping to identify options available to people, helping to clarify the implications of any given course of action, and helping to implement decisions once appropriately made. The philosophy of action anthropology parts company with much of applied anthropology in steadfastly refusing to let the authority and greater comparative knowledge of the observer lead to his interference in deciding what course of action is in the best interests of the observed.

Tax has followed this philosophy of action to a remarkable degree in his ever-expanding administrative and leadership roles, even when he has been as much an insider as his associates. From intratribal contexts, he moved to the arena of pan-Indian organization, experimenting with administrative mechanisms acceptable to Indians for communicating and addressing those objectives on which agreement could be

reached. He coordinated the first nationwide congress of Indians in 1961, and directed the Carnegie Cross-cultural Education Program for Indians in Oklahoma from 1962 to 1967. Simultaneously his ideas about effective action were brought to bear on other groups of diverse constituencies: the volatile university neighborhood of Hyde Park and the city of Chicago (1968); the national community of anthropologists (serving terms as president of the American Anthropological Association and editor of its journal); and the international community of anthropologists (founding and editing *Current Anthropology* in 1958 and organizing the ninth International Congress of Anthropological and Ethnological Sciences in 1973). In all these contexts Tax invited, indeed insisted upon, more grass roots participation in deliberation and feedback in decision making and publication of findings than most would have deemed either logistically feasible or desirable in terms of effective action. And yet feasible and effective these undertakings indeed proved to be. His success in these endeavors has encouraged him to explore late in his career the potential for revitalization of institutions and groups of many kinds through self-help and enlightened political leadership, notably the family and neighborhood in the United States (1976a; 1976b).

These experiments in action anthropology have overshadowed Tax's early and continuing interests in the training of anthropologists and the more central theoretical and methodological concerns of the discipline. Ralph Linton introduced him to anthropology in his undergraduate years at the University of Wisconsin, and Ruth Benedict supervised his first field work among North American Indians. His introduction to social anthropology and structural-functional analysis imported from Europe came at the hands of A. R. Radcliffe-Brown, who as a visiting professor at Chicago launched Tax and future colleague Fred Eggan on their respective doctoral studies of social organization among the Mesquakie and the Hopi Indians in the early 1930s. In his analysis of the Fox variant of the Omaha kinship system (1937b), Tax pioneered in developing the technique of transformational analysis (Coult 1967), anticipating the work of Floyd G. Lounsbury and others two decades later. While the influence of Radcliffe-Brown was apparent in his study of Mesquakie social organization, his principal guidance in graduate study came from Robert Redfield, his mentor

and close friend until the latter's death in 1958. It was Redfield who introduced Tax to research among Mayan Indians in Guatemala following Tax's marriage to Gertrude Jospe Katz and completion of his doctoral study at Chicago in 1934.

Tax's commitment to "one hundred per cent ethnography" in kinship analysis among the Mesquakie (i.e., gathering genealogical data from all the members of the Mesquakie community, checking them against earlier records of William Jones, and obtaining the kinship system from at least twenty individuals in order to uncover and explain discrepancies) was followed by comparably ambitious data gathering in Panajachel, Guatemala. The results in the area of Panajachel economy (1953), where Tax measured community income and expenditures for a full year, ordered all households by wealth, and examined the functions of those wealth differences, are as well known among economists as anthropologists. The Taxes' eight years of research in Guatemala, followed by four years of research and training of anthropologists in Mexico, were undertaken as part of the Carnegie Institution's research on contemporary Mayan Indians begun with Redfield's studies in the Yucatañ. Separated from the Yucatec Maya by extensive rain forests were large Mayan populations in the highlands of western Guatemala and Chiapas, Mexico. In two papers (1937*a*; 1941) Tax provided the definitive statement of cultural patterning among the western Maya, reporting more impersonalism in social interaction than Redfield had found in Yucatañ communities of comparable size, and raising questions about the origins of impersonal, atomistic, and pragmatic social relations in urban, industrial society given the same qualities of social interaction among the highland Maya. In a broader context, the status of Mesoamerican ethnology was first systematically assessed in a symposium chaired by Tax, whose proceedings he edited in the *Heritage of Conquest* (1952*b*).

In 1940, while involved in the Mesoamerican research, Tax was appointed a research associate at the University of Chicago, and following a year as visiting professor at the National Institute of Anthropology and History (1942/1943), he joined the faculty of Chicago's department of anthropology. His research among the Mesquakie Indians of Iowa resumed, producing the tenets of action anthropology, and he took charge of the revision of the graduate curriculum, providing an integration that distinguished Chi-

cago's program for two decades. He first became a national figure during the reorganization in 1945/1946 of the American Anthropological Association, of which he became editor in 1953 and president in 1959; and an international figure as editor of the 29th International Congress of Americanists in 1949, and in 1952 as a participant in the Wenner-Gren Foundation's "Anthropology Today" symposium and the principal editor of *An Appraisal of Anthropology Today* (Tax et al. 1953). These endeavors led to his appointment as a research associate of the Wenner-Gren Foundation, from which flowed the international associations that produced *Current Anthropology*. As its first editor (1958-1974), Tax was instrumental in developing a truly international journal, promoting dialogue effectively through printing the reactions of ten to twenty international scholars together with the author's response with each article.

The journal has also remained distinctive in its commitment to the integration of the subfields of anthropology. Tax has promoted such integration in a number of his other publication ventures as well, notably in his editorship of *Evolution After Darwin* (1960), *Horizons of Anthropology* (1964), and the *World Anthropology* series of nearly one hundred books of selected papers from the ninth International Congress of Anthropological and Ethnological Sciences.

Two publications provide commentary on Tax's contributions to anthropology. Action anthropology was the subject of a symposium and review by the international community of anthropologists in *Current Anthropology* in 1975. A *Festschrift*, *Currents in Anthropology* (Hinshaw 1979), contains papers by 27 students and colleagues of Tax, including his two daughters, Marianna Tax Choldin, and Susan Tax Freeman. The papers deal principally with North and Mesoamerican Indians and action anthropology. One of the papers, by David Blanchard, traces the formative influences and principal junctures in Tax's career in more detail than is permitted here.

ROBERT E. HINSHAW

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- 1937*a* The Municipios of the Midwestern Highlands of Guatemala. *American Anthropologist* 39:423-444.
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THOMAS, DOROTHY S.

Dorothy Swaine Thomas was born in Baltimore, Maryland, in 1899, and died in 1977. Trained as an economist, she became a leading demographer and sociologist, and a pioneer in the development of social statistics. Her work, however, cannot be classified under any of these headings, for it was always interdisciplinary, relating materials from different fields.

Her childhood was a difficult one. The only child of an improvident father, she was frequently separated from her mother. Always a good student, she won a scholarship to Barnard College and began her studies there in 1919.

At Barnard she continued her interest in the humanities but was increasingly drawn into

movements for social reform, becoming known as a "young radical." These interests led her into the social sciences and she applied for admission to the combined department of sociology and economics, then headed by William F. Ogburn. As an undergraduate she published two papers with Ogburn: "Are Inventions Inevitable?" (1922a), and "The Influence of the Business Cycle on Certain Social Conditions" (1922b). Thus, as an undergraduate she had already set the stage for decades of pathbreaking work, and her successful collaboration with Ogburn left her willing to undertake joint studies with a succession of leading scholars in several fields.

Graduating from Barnard in 1922 she turned to Great Britain for better data to examine the relationship between economic fluctuations and social events. At the London School of Economics (L.S.E.), she studied with Arthur L. Bowley, the statistician, and William Beveridge, the director of the L.S.E. In 1924, along with her PH.D., she was awarded the Hutchinson research medal, and her dissertation, *Social Aspects of the Business Cycle* (1925), was published. Using data from the United States and Britain, she had explored the relationships of birth, death, marriage, divorce, and crime rates, as well as pauperism and migration, to the business cycle. At the age of 24, she had thus established herself as a leader in an important area of research.

Even so, she found it difficult, as a woman, to obtain a position in a good university. Rather than settle for a professorship in a women's college, she became an economic statistician for the Federal Reserve Bank, remaining there until she received a postdoctoral fellowship from the Social Science Research Council in 1925. Intending to study the relationships between economic cycles and convictions for crime in New York State, she found that the necessary data were not available. William I. Thomas, the renowned sociologist to whom she turned for advice, was unable to help her with this project, but he offered her a job on a study of child development that he was beginning under Rockefeller sponsorship. Thus began an association that lasted until Thomas' death in 1947. They were married in 1938.

Both their personalities and their styles of work were complementary rather than similar. Dorothy Thomas was quick and impulsive; her husband was calm and reflective. He reveled in the analysis of documents, enjoyed speculation,

and was inclined toward theory, while she, always suspicious of armchair theorizing, demanded statistical proof and was reluctant to generalize. Together they wrote *The Child in America: Behavior Problems and Programs* (1928), for which she prepared the statistical materials, while he pursued the situational approach for which he was noted. This was the only work they coauthored, and W. I. Thomas continued to exercise a powerful influence on her work, partly in the selection of topics and partly in broadening her approach to include nonquantifiable materials.

In 1927 Dorothy Thomas accepted a position at Columbia's Teachers College, where she directed studies of the behavior of nursery school children—a project she continued at the Yale University Institute of Human Relations to which she moved in 1931. While the techniques and approach that she and her colleagues developed were influential in the evolution of small group and observational studies, she always felt that the project was an attempt to “quantify the unquantifiable.” She did not greatly value the two books and the articles that emerged from these studies, and welcomed the opportunity to return to the study of economic and social relationships.

From 1930 to 1936, the Thomases worked with Gunnar and Alva Myrdal on a study of the Swedish people that would be modeled after *The Polish Peasant in Europe and America*, (W. I. Thomas & Znaniecki 1918–1920), but that would also use statistical and economic data. Although the Thomases spent part of each year in Sweden, collecting materials and supervising a research staff, the study as envisioned never materialized—the Myrdals were drawn off into politics and W. I. Thomas became absorbed in the study of Swedish behavior documents. Dorothy Thomas, therefore, took over a study that Gunnar Myrdal had begun on population movements and industrialization in Sweden, broadening its historical context. With data available as far back as 1750, she was able to relate demographic movements to harvest cycles in the early years and to business cycles from the late nineteenth century onward (1941). Concurrent with her work in Sweden was her survey of the state of knowledge about migration in *Research Memorandum on Migration Differentials* (1938). Scholars immediately recognized the value of both works.

In 1940 Dorothy Thomas became a full pro-

fessor at the University of California at Berkeley—a life-long ambition—and for the first time she had students to teach. She began a research training program that resulted, within the first year, in joint publications with students. With the attack on Pearl Harbor and the subsequent internment of Japanese living on the west coast, she began a series of studies that form the major record of this segment of American history. Working with students and internees she amassed day-to-day records of life in the camps, along with life histories of the internees. She credits W. I. Thomas for being the guide in the collection and interpretation of the behavioral documents and for saving her from premature quantification. In 1946 the first volume from this study, coauthored by Richard S. Nishimoto, *Japanese-American Evacuation and Resettlement: The Spoilage*, appeared.

Before the second volume, *Japanese-American Evacuation and Resettlement: The Salvage* (Thomas, Kikuchi, & Sakoda 1952) was completed, W. I. Thomas died, and in 1948, Dorothy Thomas joined the sociology faculty of the University of Pennsylvania. Although she was indignant at the treatment of the Japanese, both *The Spoilage* and *The Salvage* were restrained, almost cold, accounts of a massive human tragedy and the efforts of a rejected people to reestablish themselves as patriotic Americans. These books were meticulously documented and the statistical record was as complete as possible. Again, she had completed a model study, but this one went beyond the quantifiable. Her relationship with W. I. Thomas had resulted in a broader, more insightful work than she otherwise would have attempted. For her, the greatest accolade was the acceptance of these books by the Supreme Court of the United States as evidence of the wrongs inflicted on the Japanese.

At the University of Pennsylvania, she and Simon Kuznets undertook a series of studies on population redistribution and economic growth that occupied her for the rest of her career. With Kuznets she supervised a massive data collection from old censuses and other statistical sources that are basic to the economic and demographic history of the United States. As soon as they were assembled the data were made available to scholars and thus became the basis for many later studies. Along with a talented group of younger scholars, she and Kuznets published three volumes, *Population Redistribution and Economic Growth: United States, 1870–1950*

(1957–1964), and supervised the research for many articles, reports, and data sets. When she retired from the University of Pennsylvania in 1970, the basic data for an economic–demographic history of the United States since the Civil War had been assembled and the main development set forth. Dorothy Thomas realized that the economic and demographic analyses had not been fully integrated and, until her research was interrupted by a stroke in the summer of 1974, she worked toward a fourth and integrative volume for the series.

After retiring from Pennsylvania she became a professorial lecturer at Georgetown University. There, as at Berkeley and Pennsylvania, she attracted the best students. A reluctant lecturer, she emphasized research and taught by precept or by the apprentice method. She was kindly, even protective of students, but for the best she erected the highest standards and subjected them to merciless criticism.

Her work habits were irregular. She worked in fits and starts, alternating periods of quiescence with furious activity. She carefully checked every calculation and statement repeatedly. Her prose was spare and graceful and, when in the mood, she could write pages of text in a few hours. She regarded herself as an inveterate procrastinator and always regretted the many things she had started but had not finished.

A great strength was her ability to discern important problems and then to find and organize neglected materials that bore upon them. The succeeding analyses were direct and simple. At the same time, the major shortcoming of her work stemmed from her unwillingness to generalize and to speculate beyond the data. Nevertheless, her work remains basic for further studies along the lines she blazed.

EVERETT S. LEE

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TINBERGEN, JAN

Jan Tinbergen, economist, was born in The Hague, the Netherlands, in 1903, as the oldest son in a family with four sons and one daughter, all intellectually gifted. His brother Nikolaas, professor in animal behavior at Oxford University since 1949, shared the 1973 Nobel Prize in biology with Konrad Lorenz and Karl von Frisch for his contributions to ethology. The youngest brother Luuk, who died at age forty, was a professor of zoology. The parents provided a stimulating environment for the development of their children's scientific interest. The father, Dr. D. C. Tinbergen, was a linguist and a language teacher at a secondary school in The Hague. He encouraged the use of simple language in explaining scientific theories. Although politics was not a major subject of discussion in the family, a strong social interest prevailed. Jan Tinbergen became a member of the socialist youth association and later an active member of the Social Democratic Labour party.

After attending secondary school, where he met his future wife, Tine de Wit, a colonel's daughter, Tinbergen studied physics from 1922 to 1926 at the University of Leiden, where Paul Ehrenfest was his most influential teacher. Ehrenfest's method of "successive approximation," for example, the method of reducing a problem to its hard core and then introducing step-by-step complicating factors, can be found often in much of Tinbergen's later work. In 1929 Tinbergen received his doctorate in physics with a thesis on "Minimum Problems in Physics and Economics." The subject reflects the shift in his interest, in the preceding years, from physics to economics, a shift that Ehrenfest had helped stimulate. The thesis also shows Tinbergen's belief in the utility of applying mathematics to economics.

Tinbergen's strong social feelings led him to refuse serving in the army for his conscription. As compensation, he worked first in the prison administration in Rotterdam and later at the Central Bureau of Statistics in The Hague. After having served as an assistant to Ehrenfest, he returned in 1929 to the Central Bureau of Statistics where he joined a new unit for business cycle research. He became the leading spirit of the unit, where he stayed, except for an interruption from 1936 to 1938 for work for the League of Nations, until 1945. In 1931, he became a part-time adjunct lecturer in statistics at the University of Amsterdam. Then in 1933

the Netherlands School of Economics in Rotterdam appointed him as (part-time) professor.

Econometric business cycle research. Tinbergen's research at the Central Bureau of Statistics provided the basis for his contributions to econometrics, the combination of mathematical economics and statistical analysis. The application of mathematics to economics has a long history that goes back to the nineteenth century, but the empirical verification of economic theory with the help of statistical data started only in the late 1920s. It led to the development of this new branch of economics in the 1930s, with Tinbergen as one of its pioneers. In a long series of articles in various international and national periodicals Tinbergen contributed to the empirical and quantitative knowledge of the ways in which markets and economies operate. Many of these studies were demand studies, estimating the influence of changes in income and prices on the demand for individual goods and services, or for such aggregates as exports, imports, and total consumption. His special interest was in cyclical movements, particularly the so-called "cobweb phenomenon," the movement of prices and volumes in opposite directions with a certain periodicity, for example for the shipbuilding market. This phenomenon arises with a dynamic market mechanism whereby supply reacts to prices with a certain time lag, while demand reacts without such a lag.

These studies were the building stones for more ambitious attempts to explain business cycle fluctuations for an entire national economy, Tinbergen's main concern in the 1930s. Although nonmathematical theories attempted to explain different aspects and phases of the cycle, Tinbergen, with other econometricians, was in favor of explaining the cycle as a whole through one set (model) of simultaneous causal economic relations. His first attempt to build such a model for the Dutch economy was published in 1936. The model, containing 24 equations, provided the prototype for the models that would be built for the Dutch economy in the next decades. It foreshadowed elements of Keynes's *General Theory* (1936) and of the Phillips curve. Yet, the fundamental and pioneering significance of this Dutch publication drew no international attention. An English version became available only in 1959 (Tinbergen 1959). Tinbergen never aimed at great publicity.

At the League of Nations in Geneva from 1936 to 1938, he continued his econometric

work on business cycles. He was asked to test statistically the various business cycle theories that Gottfried von Haberler had reviewed in an earlier book for the League. Tinbergen's results were published in 1939 in two volumes under the general title *Statistical Testing of Business Cycle Theories*. The first one, *A Method and Its Application to Investment Activity*, described the method of statistical analysis used with an application to investment theory; the second volume, *Business Cycles in the United States of America, 1919-1932*, rather than test various theories, provided a model explaining the economic fluctuations of the United States. These studies were received with great skepticism. Keynes gave volume one a very critical review, showing little understanding of the econometric method. Partly because of the attention given Keynes's *General Theory* and the outbreak of World War II, it was not until the 1950s that the historical value of Tinbergen's studies was recognized and an increasing number of econometric models was built explaining economic fluctuations in national economies. At the beginning of the war period, Tinbergen himself constructed a model for the United Kingdom for the period 1870-1914 (1951), following the same lines as the United States model.

During the German occupation of the Netherlands (1940-1945), Tinbergen continued his research at the Central Bureau of Statistics and published in international journals. He also broadened his interest—for example, to problems of long-term development—on which he wrote a pioneering econometric article explaining the trend movements for Germany, Great Britain, France, and the United States over the period 1870-1914. Several books, partly of a popularizing nature, were written during this period and published before or soon after the end of the war.

Economic policy and welfare economics. When the war ended in 1945, Tinbergen was appointed director of the newly established Netherlands Central Planning Bureau. Originally intended as an instrument for a more centrally directed economic policy, it soon became an advisory research agency for macroeconomic government policies. In this new capacity, Tinbergen gave his full attention first to problems of Dutch reconstruction, later, to short-term policy problems and international economic cooperation. These new fields of attention were also reflected in the scientific contributions that he continued to publish in the next decade.

Close integration between major current economic and social problems of the day and his practical advice and scientific work has been a continuing feature of Tinbergen's activities.

In this period, Tinbergen's contributions to the theory of economic policy were formulated, first in two short books, *On the Theory of Economic Policy* (1952) and *Centralization and Decentralization in Economic Policy* (1954) and then, in his most important book on this subject, *Economic Policy: Principles and Design* (1956a). Although Tinbergen himself attributes the essential ideas in these volumes to Ragnar Frisch and Swedish economists, he undoubtedly reshaped their concepts and added several new elements; thus one may consider his formulations as new and original. The hard core of the theory states that in order to achieve a set of quantitative *targets* of economic policy, at least an equal number of quantitative *instruments* (policy measures) must be used. A distinction is made between analytical and policy problems. In the first category, policy instruments are given (exogenous), and such variables as income, employment, and balance of payments deficit are unknowns (endogenous) to be determined in the framework of a mathematical macroeconomic model. For policy studies the same model can be used, but the role of these variables is *inverted*: the latter variables—income, employment, balance of payments—become given policy targets, and the policy instruments—for example, government expenditure, taxes—become the unknown (endogenous) variables. Fixed targets are considered to be an approximation of the optimal values of the policy aims in an objective or welfare function. The use of this quantitative framework for studying policy problems was illustrated in *Economic Policy* in a series of examples using wage, price, and fiscal policies and their effects on income, employment, and balance of payments that were based on actual problems facing the Dutch economy in the early 1950s.

More abstract but fundamental contributions to the theory of economic policy have been Tinbergen's studies of welfare economics, particularly of the theory of an optimum economic régime. This theory translates the conditions under which an economy's social welfare is maximized in terms of institutions, or degrees of centralization of decision making. Tinbergen concluded that an optimum régime must be a mixed system, that is, while in principle decentralized decisions are optimal, for production

processes characterized by external effects or economies of scale, more centralized decisions are necessary.

This theory provided also the theoretical background of his *convergence theory*, the view that the economic systems of the communist and Western countries are showing more similarities. Inspired by the movement for economic reforms in the early 1960s in the Soviet Union and eastern Europe, movements that demanded greater decentralization, and the increasing role of the government in the West, Tinbergen found in these trends support for his view on an optimum régime.

Development planning. In 1955 Tinbergen resigned as director of the Central Planning Bureau in order to devote himself full time to problems of the developing countries. After a one-year visiting professorship at Harvard University he was elected to hold a new and full-time chair in development planning at the Netherlands School of Economics; since 1965 that chair has also included the economics of centrally planned systems. At the Netherlands Economic Institute, a small staff for fundamental and applied research on problems of developing countries was established under his direction.

These changes mark the beginning of a period lasting until the early 1970s during which Tinbergen concentrated his activities fully on the problem he felt demanded highest priority in research and policy making: the unequal distribution of the world's welfare. His efforts to contribute to the solution of this problem have been broad in scope and nature. Through a continuous stream of articles, reports, and several books; through participation in conferences and working and expert groups; through lecturing at home and abroad for audiences ranging from rural housewives to heads of state, he explained and argued for the need for an ambitious international development policy. Through his research and scientific work he contributed to the theory of development, the methodology of national and international planning, and specific policy issues.

The desire of the new states of the 1950s and 1960s to direct their policies through a medium-term development plan created new planning and policy problems, since the experience of the communist and Western countries was of limited or no value in the circumstances of the developing countries. Tinbergen contributed to nearly every aspect of these planning processes.

His method of *planning in stages* is an attempt to plan in a number of successive stages decisions about the most desirable rate of growth (and savings) of the economy (the macro stage), the choice of industries and their distribution over regions (the middle stage) and the selection of projects and their location in centers of activity (the micro stage). The *semi-input-output method* was an original method to assist in the selection of the industries that a country should develop. This method calculates complementary bunches of investments in an international industry, producing goods or services that are internationally tradable, and national industries, producing goods that must necessarily be produced domestically. The total effects of such a bundle of investments can be compared for alternative international industries on the basis of some selection criterion.

In the early 1960s, when economists became interested in planning education in relation to economic development, Tinbergen, through his contributions to the Organization for Economic Cooperation and Development (OECD) and the United Nations Educational, Scientific and Cultural Organization (UNESCO), was one of the first to develop quantitative models for the planning of education. The problem of planning the best spatial distribution of production and investments over regions and centers of an economy led him also to elaborate methods and models for spatial planning. On international issues he emphasized, among others, the need for an international division of labor between the developed and the developing countries based on the principle of (dynamic) comparative cost. This emphasis led to his recommendation that the latter group of countries develop labor-intensive industries and technologies and the former group, capital-intensive industries, in order to provide maximum employment and income opportunities for the developing world. As coordinator of a study on the future world order, undertaken at the initiative of the Club of Rome and published in 1976 under the title *Reshaping the International Order* (RIO), Tinbergen emphasized, as he had on many earlier occasions, the need for supranational decision making in a number of areas of global interest, a recommendation based on the results of his work on welfare economics.

During this period Tinbergen served as adviser to the governments of various developing countries, including India, Egypt, Turkey, Surinam, Indonesia, Syria, Iraq, and Libya, and to

such international organizations as the World Bank, OECD, and various UN agencies. As chairman from 1966 to 1974 of the UN Committee for Development Planning, an advisory group of independent development experts, he had a large influence on the formulation of the UN International Development Strategy for the Second Development Decade (1971–1980), or, rather, on the committee's proposal for this strategy. For, to Tinbergen's deep disappointment, the proposal was accepted half-heartedly and only after a reformulation that omitted the commitments he felt were required. This experience in fact brought about a new phase in his scientific work.

Income distribution. The subject that has most concerned Tinbergen in his work as an economist is probably income distribution, or, more generally, inequality in welfare, conceived as the most concrete expression of social justice. His interest in the business cycle phenomenon, in global development problems, and in welfare economics finds a common denominator in this theme. It is therefore not surprising that in the early 1970s Tinbergen took up the theory of personal income distribution for industrialized countries as the topic of his major research. In 1946 he had already published a pamphlet in Dutch on "Equitable Income Distribution," which was followed by a fundamental theoretical article in 1956 "On the Theory of Income Distribution." The latter article provided the basis for further elaborations and econometric testing in a long series of articles, integrated in his book *Income Distribution: Analysis and Policies* (1975).

Tinbergen's theories of income distribution have positive and normative components. His positive theory of existing (labor) income differences is based on a supply-and-demand analysis for labor demanding different qualifications. The price of labor (income) is assumed to equalize supply and demand in the long run; hence, income is dependent on supply and demand factors. On the supply side, personality traits and performance characteristics, such as IQ's (intelligence quotients), level of education, and noncognitive characteristics, play a role. On the demand side, job characteristics, as measured through job requirements, are determining factors. Using data for the United States and the Netherlands, Tinbergen tested in various ways the influence of these factors.

Tinbergen's normative theory attempts to determine an equitable income distribution.

Equity is defined as equal welfare for all individuals, but not necessarily as equal income, because welfare is assumed to depend not only on income but also on factors such as occupation chosen and number of years of schooling. The "tension" between required and actual schooling, an element in the welfare function that Tinbergen considers to be measurable and similar for all human beings, should be minimized in an equitable income distribution. Income differences, in his theory, can be reduced by the deliberate creation of some oversupplies of skilled labor and, correspondingly, shortages of unskilled labor.

Both his analytical studies and, of course, his normative theory have met criticisms of various kinds, which he has accepted as challenges for further research.

Concluding remarks. Any review of Tinbergen's work, as given above, must be highly selective. It cannot give an impression of his person, his stimulating influence on those he worked with or met in conferences or discussions, his constructive and optimistic attitude, his confidence in the forces of good will, his modesty and simple life style, his humanity.

Tinbergen's outstanding and wide-ranging activities as an economist have increasingly been recognized through distinctions, awards, and honorary degrees at home and abroad. When in 1969, he received, together with Ragnar Frisch, the first Nobel Prize in economics for "having developed and applied dynamic models for the analysis of economic processes," a frequent opinion expressed was that he would have been an equally worthy candidate for the Nobel peace prize. Tinbergen's present grave concern for the world's future peace, culture, and welfare confirms this view of the driving force of his great achievements as an economist.

HENK C. BOS

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TINBERGEN, NIKOLAAS

In 1973 Nikolaas Tinbergen shared the Nobel Prize for physiology and medicine with Konrad Lorenz and Karl von Frisch. He has also won the Jan Swammerdam medal, awarded every

ten years to a Dutch biologist, and two honorary degrees for his contributions to ethology. He belongs to a gifted family; his brother Jan Tinbergen won the Nobel prize in economics in 1969.

Tinbergen was born in 1907 in the Netherlands. From an early age he enjoyed sports, which he almost chose over an academic career, and a love of natural history that brought him into touch with the work of J. Verwey and A. F. J. Portielje, well-known ornithologists of the 1920s and 1930s. At the end of his school days he was still undecided on a career, but his aptitude for biology was recognized by the theoretical physicist, Paul Ehrenfest, a friend of the family, who gave him the opportunity to spend several months at Vogelwarte Rossitten, the observatory at which the scientific ringing of birds was pioneered. Upon his return, he decided to enroll for a degree in zoology at the University of Leiden.

He was not a distinguished student. Most of his energy was spent on hockey or his own natural history pursuits, which did not always coincide with the requirements of his discipline. However, even his thesis of only 32 pages, the shortest on record at the university, indicated the route he would take. Studying wasps that fed on bees and dug burrows in the sand, he investigated how they were able to relocate their tiny burrows amid the sand hills. By using simple field experiments with marked individuals he was able to show that they found their way visually, and that they learned appropriate landmarks on short reconnoitering flights that they made on leaving the burrow. In his doctoral dissertation, Tinbergen used an approach that characterized much of his later work: careful observation followed by simple but carefully controlled experiments whose aim was to vary one factor at a time—not in an impoverished laboratory environment, but in the field. He received his PH.D. in 1932.

In the same year Tinbergen married Elizabeth Amélie Rutten, a chemistry student, and they set off on a 14-month "honeymoon" with a meteorological expedition to Greenland. The Tinbergens lived with the Eskimos, learned their language, and acquired an interest in the hunter-gatherers' way of life. There, Tinbergen embarked on several trails that he would follow persistently. He concentrated on the snow bunting, a small bird which arrived as the snows melted, expended much energy in territorial battles with rivals, and bred in the brief summer. The

observations he made formed the basis of much of his later thinking on the function of territory, on display, and even on the nature of war. He also published extensively on the rednecked phalarope and made diverse observations on a variety of arctic animals, including Eskimo dogs.

In 1933 Tinbergen became an instructor in his old department at Leiden. In the laboratory, he started to work with the three-spined stickleback, work which has since been carried on by Gerard Baerends, his first student, who later led a flourishing department at the University of Groningen; by Jan van Iersel, his successor at Leiden; and by Piet Sevenster, van Iersel's student. The stickleback proved to be an ideal choice: its natural environment could be imitated in an aquarium where simple experiments were possible. Tinbergen could map the reproductive cycle, analyze the stimuli eliciting attack and courtship from a territory-owning male, and show that the complex zigzag courtship dance was a compromise between two incompatible response systems—leading the female to the nest and attacking her. Tinbergen encouraged his students to engage in field research, initiating work on wasps, butterflies, hobbies, and herring gulls. He started an annual summer camp among the sand hills near his parents' home, a tradition that was reinstated after World War II. Many classic studies of fish, bird, and insect behavior were initiated at this time.

In 1936 Konrad Lorenz, who was later to share Tinbergen's Nobel Prize, visited Leiden, and the two immediately recognized their common ground. Tinbergen spent the spring of 1937 working at Lorenz' father's home near Vienna. Together they refined the methods of early ethology, Lorenz elaborating his comparative studies, Tinbergen inserting experimental probes into Lorenz' observations of hand-reared grey-lag geese. From this research came two of the classic studies of early ethology, one concerned with the manner in which a goose retrieves eggs that have rolled out of its nest, and the other with the way a goose recognizes a flying predator. Both enterprises were made possible by Tinbergen's skill in experimentation under natural conditions.

Tinbergen and Lorenz were separated by World War II. Tinbergen spent two years in a German hostage camp for protesting against the removal of Jewish professors from the university and later joined the Dutch Resistance.

Lorenz was drafted to serve as a doctor on the Russian front, and became a prisoner of war. After the war Tinbergen returned to Leiden, becoming a full professor in 1947. His work expanded to include studies of camouflage in a wide range of species.

By this time the earlier work of both Tinbergen and Lorenz was becoming known in the English-speaking world. At the suggestion of William H. Thorpe, the Society for Experimental Biology organized a conference on physiological mechanisms in animal behavior. This meeting permitted renewed contact between Tinbergen and Lorenz and led to the biennial ethological conferences that have continued since. In addition Tinbergen and Thorpe enlisted Frank A. Beach and others to start the journal *Behaviour*, which carried papers in English, French, and German.

Tinbergen was convinced of the sterility of much contemporary comparative and experimental psychology. He was appalled at the far-reaching generalizations made by experimenters, who were largely ignorant of the diversity of nature, on the basis of laboratory studies mainly confined to a few species of rodents. He felt the concentration on studies of causation to be unduly narrow. Deciding to teach in the English-speaking world, he accepted a lectureship at Oxford University in 1949. Although the facilities he had hoped for never fully materialized, he built up a research group that had a profound influence on the growth of ethology around the world. Leaving the bulk of the "stickleback problem" to his former pupils in Holland, he focused on the adaptedness of behavior. His work on herring gulls, started in Holland, blossomed into comparative studies of many gull species, with detailed data on a selected few. His most influential book, *The Study of Instinct*, based on a series of lectures given at the American Museum of Natural History in 1947, appeared in 1951. Only 228 pages long, it had a tremendous impact on the growth of ethology. Tinbergen's writing style is very different from that of his more ebullient colleague, Lorenz. Tinbergen writes precisely and with great clarity, arguing his points one by one. The book's impact was increased by his thumbnail sketches of the animals he was studying. In 1953, he published *Social Behaviour in Animals*, written largely while he was in the hostage camp.

One of Tinbergen's major contributions has been to emphasize clearly the distinction between the four basic questions about behavior:

its immediate causation, its development, its consequences and function, and its evolution, while at the same time showing how they are interrelated. *The Study of Instinct* contained 112 pages on causation, but only 23 on development, 34 on function, and 15 on evolution. From then on, however, the emphasis changed. Because the questions were interrelated, some of the work on gulls was inevitably concerned with the problem of causation, but the main emphasis was on questions of function (what is this behavior for?) and on evolution (how did it evolve?). Properly critical of armchair speculation on the functions of behavior, he used the powerful technique of simple field experiments. For example, noting that gulls remove the empty eggshells from the nest soon after the chicks hatch, and that the parents' absence exposes the chicks to predation, he argued that the removal must have an important function. By putting out eggs with or without empty shells, Tinbergen and his pupils found that eggs with empty shells nearby were more quickly found by predators. Another major line of work concerned the function of aggressive behavior in the black-headed gull. First, by studying the behavior of intruders onto territories in which the owners were immobilized with a stupefying drug, his student I. J. Patterson showed that aggressive displays have a deterrent effect. Territorial behavior promotes spacing out of the nests, and by taking advantage of natural variation in spacing in the colony, it was possible to show that spacing out reduces predation on pipped eggs and on newly hatched chicks. In this and other ways Tinbergen and his students investigated one aspect after another of the species under study.

Behavioral features can be as characteristic of a species as morphological ones: they had already been used by C. O. Whitman in the United States of America and by O. Heinroth and Lorenz in Germany to provide evidence of evolutionary relationships between species. Especially valuable in this context were the species-characteristic stereotyped movement patterns used in threat and courtship. Tinbergen, though concerned with this problem, was more interested in the related issue of the origin of interspecies differences in behavior—that is, of the way behavior had evolved. Since virtually no fossil evidence on the behavior of extinct species is available, scientific advance depends on comparisons between present-day species. Here, also, stereotyped movements proved valu-

able. If comparative studies reveal small differences between basically similar movement patterns in related species, it is possible to construct hypotheses suggesting which variants are phylogenetically older. For this purpose additional information may be essential—for example, knowledge about the function or causation of the movement. The differences between the presumed older forms and their modified versions indicate the evolutionary changes undergone by the latter. In this way, Tinbergen was able to sketch the probable evolutionary history of “derived movements”—movements originally serving one function, such as walking, flying, or feeding, which had gradually changed in evolution to serve as signals to other individuals. The changes which such movements had undergone to suit them for a signal function were referred to as “ritualization.” This comparative research led to studies of the adaptive radiation of behavior as a concomitant of speciation, and to the demonstration that the invasion of a new niche led to modification of diverse characters, including social structure.

In the late 1940s and early 1950s, the young discipline of ethology was criticized by comparative psychologists in the United States of America. The two principal issues concerned the development of behavior and its evolution. While American comparative psychologists had focused on the study of learning, and tended to equate the study of development with the study of the role of experience in development, Tinbergen, and also Lorenz, were impressed by interspecies differences that could not be accounted for by differences in experience. Such a genetic emphasis was contrary to the Anglo-American ideologies of the day (and perhaps equally to those of the communists). Second, while most American psychologists had equated the problem of the evolution of behavior with that of explaining the diverse levels of complexity in the entire range of the animal kingdom, Tinbergen had been concerned with problems of microevolution—the ways in which closely related species came to differ from each other. The chief criticism came from T. C. Schneirla, at the American Museum of Natural History, himself a prominent field naturalist. Schneirla had an involved literary style and published largely in journals that were not widely read; but in 1953 Daniel S. Lehrman, Schneirla's student, published a clearly written summary of their views. Though Lehrman had been persuaded to tone down his article by col-

leagues (especially Ernst Mayr and Frank Beach), it contained a sharp and in parts hard-hitting attack on many aspects of the ethological approach. But, largely because of Tinbergen's ability to be hard-headed when sure he was right, and yet to meet his critics half way when he was not certain, this paper led to a close friendship between the protagonists and a rapprochement between most comparative psychologists and ethologists. On the ontogeny issue, both sides changed their emphasis; on the evolutionary issue, the differences in approach were explicitly acknowledged.

As his move to Oxford demonstrated, Tinbergen was as much concerned with establishing the science of ethology as with his own contribution to extending the frontiers of research. He wrote freely in natural history journals and for the *Scientific American*, and two of his best books were written for the educated layman: *The Herring Gull's World* (1953a) and *Curious Naturalists* (1958). Recognizing that the quality of his work could best be conveyed on film, he put most of his energies in the 1960s into making scientific films. One of these, "Signals for Survival," made jointly with Hugh Falkus, won the Italian prize for documentary television films and the blue ribbon of the New York Film Festival. Many of his films have not only been widely shown on television, but have formed an integral part of his colleagues' lecture courses around the world.

In the late 1960s Tinbergen became increasingly preoccupied with the implications of the ethological approach for man. In 1968 he gave his inaugural lecture as professor of animal behavior at Oxford on the theme of "War and Peace in Animals and Man." He pointed out the similarities between man and species that hold group territories, in order to emphasize the possible consequences of the ineffectiveness in man of some of the mechanisms that reduce violence in nature. While believing that it is impossible to eliminate man's propensity to fight, he argued that the only hope is to find less dangerous and more useful goals. He took up this theme again in his Croonian lecture to the Royal Society (1972).

In using his experience with animals to reflect on the problems of humanity, Tinbergen did not fall into the trap of merely looking for similarities. Rather, he constantly emphasized the differences between animals and man and argued that some of the methods and principles derived from work with animals were ap-

plicable to humans. For example, the comparative method, applied by biologists to closely related species, could profitably be applied by social scientists to human cultures.

Tinbergen also became concerned with the reciprocal relationship between science and society (1976). He had long seen that scientific endeavor is a function of the attitudes in the society in which it occurs, and he had, for example, related the rise of behaviorism in the United States of America to the pioneering ideal—the belief that nature, including human nature, can be tamed by appropriate environmental manipulation. However, he later came to emphasize that science should influence society. He saw this as crucial in an era when, "because of our one-sided appreciation and our complacent acceptance of the blessings of our civilization: of reduced infant mortality, of increased affluence, of our 'spiritual life', and last but not least of science itself" (1976), man had failed to see the inevitable consequences of technology, of the psychosocial pressures to which he was subjected, and of his exploitation of natural resources and pollution of the environment. Tinbergen advocated a two-pronged attack, one aimed at making the environment more suitable, the other at making citizens of the future more capable of coping with their habitat. Among the tools for the latter task he placed first a more biologically balanced form of education, with less emphasis on instruction and more room for play and exploration. In calling for a bloodless revolution in ways of using the environment and treating other humans, Tinbergen urged intellectuals in general to help change the climate of world opinion, and ethologists in particular to guide their work toward increasing relevance to the human predicament.

The revolution in the study of animal behavior for which Tinbergen was largely responsible was based on a number of hard-headed attacks on a relatively few highly specific problems—for example, the nature of the stickleback's courtship, the stimuli causing a young herring gull to beg, the functional explanation of why gulls bother to remove empty eggshells from the nest. It was natural that his wide-ranging concern for the future of human society would also find expression in the study of a specific issue—the nature of early childhood autism. Just as his observational methods had shown gull threat postures to be the product of conflicting tendencies to attack and to flee from

the rival, so also Tinbergen and his wife, watching their own and other children, came to believe that children could be caught in a conflict between fear and a desire to make social contact. In extreme cases, the Tinbergens believe, this fear dominates the conflict unduly and can give rise to childhood autism. Ironically, as ethologists, they have had to stress the decisive influence of the early environment. Their views have been met with skepticism in medical circles, where autism is believed to have a strong congenital element. The Tinbergens' reply lies in their careful study of the children's nonverbal behavior and, concomitantly, of their early environment. In practice, they attempted to reduce the child's apprehension by permitting him to make contact gradually, in his own time, and thus to come slowly out of his shell.

From a life of observation, experiment, and reflection with subjects ranging from hunting wasps to autistic children, it is difficult to summarize briefly what Tinbergen stands for. To establish ethology as a scientific discipline? Perhaps, but that raises the question of the precise nature of ethology. Ethology, like any healthily growing science, has no clear boundaries. The best definition perhaps is that it attempts to answer, on a basis of sound description, the four questions about behavior that Tinbergen distinguished—the questions of causation, development, function, and evolution. This definition carries with it the implications first that observation must precede experiment, and second, that complete understanding will not come from the answer to any one of the questions. In this respect, the study of behavior is quite unlike physical science. Many issues that an ethologist meets simply do not exist in physics. No one thinks of asking "What is a cloud for?" Furthermore, the ethologist must always keep the whole system in mind as he analyzes it, in order to understand the nature of its parts. Thinking in terms of simple principles, or at one level of analysis, is unlikely to be fertile for long. The student of behavior must build links "downwards" to physiology (including genetics and endocrinology), "upwards" to psychology and psychiatry, social psychology, and sociology, and laterally to ecology and evolutionary studies. Moreover, because of man's present situation, the ethologist must be in the forefront of the battle against pollution, overexploitation, malsocialization, and other excesses of the modern world, constantly using the lessons learned from the similarities, and the equally im-

portant differences, between animals and man. The task of applying ethological approaches to man is now, in Tinbergen's opinion, becoming an important "growing point" of ethology—and of biology.

However, as his students and colleagues have known, Tinbergen's personal characteristics—energy and enthusiasm, humility, stubborn perseverance—have played as crucial a role in the growth of ethology as his ideas. Without his combination of obstinacy and flexibility, the rapprochement with comparative psychology might well have been delayed. Although this rapprochement has led to differences of opinion between the more conservative German ethologists and what Lorenz calls "English-speaking ethologists," there can be no doubt that Tinbergen has been the cornerstone of the new ethology. He has helped to promote a more balanced development of ethology than its exuberant and assertive early phases might well have inaugurated, a development that reflects much of what Tinbergen advocated as "the biological approach" to behavior.

R. A. HINDE

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TOYNBEE, ARNOLD J.

Arnold Joseph Toynbee (1889-1975) was born in London, educated at Winchester and Balliol College, Oxford, as a classicist, and spent the greater part of his professional life on the staff of the Royal Institute of International Affairs in London, charged with the task of writing an annual survey of world events. Gilbert Murray, whose daughter Toynbee married in 1913, was the man who most influenced the young Toynbee during his Oxford days, but even as an undergraduate, Toynbee evidenced a strong tendency to synthesize. In particular he set out to master the whole sweep of ancient Mediterranean literature and history, combining Greek and Roman studies into a single whole. His teachers recognized Toynbee's remarkable abilities; accordingly he started an academic career by teaching Greek to Balliol undergraduates (1912-1915).

World War I interrupted Toynbee's scholarly activity, and permanently altered its direction. He was declared unfit for military service, because of a bad case of dysentery he had contracted during a walking tour through Greece in 1911-1912. Nearly all of his Oxford friends and contemporaries volunteered, and many were killed in the trenches. Thereafter, the accidental way in which he had been saved from the fate of his fellows weighed upon his mind, imposing a special obligation upon him to work hard and do something meaningful with his life.

In 1915 Toynbee entered government service as a war propagandist—a role of which he later felt deeply ashamed. He wrote several popular and inflammatory pamphlets denouncing Turkish and German atrocities, and published a detailed indictment of Turkish official efforts to destroy the Armenians (1915a). His job brought him into touch with the power elite of Great Britain, and the young scholar was among the experts who attended the Paris Peace Conference in 1919, where he played a minor part in framing the Treaty of Sèvres with Turkey.

In 1919 Toynbee returned to academic life,

accepting a newly established Korais chair in modern Greek and Byzantine language, literature, and history at the University of London. This chair had been endowed by men of Greek descent who felt that English education had concentrated attention on ancient Greece to the exclusion of Byzantine and modern times, and wished to correct the disbalance. In accepting the appointment, Toynbee saw the possibility of combining his interest in ancient Mediterranean history with his new-sprung concern for contemporary international relations. But he had scarcely taken up his new role when a harsh collision between his ideas and those of the donors caused him to resign his chair in 1924. After some anxious months, Toynbee was appointed to the British (later Royal) Institute of International Affairs, where for the next 14 years he was responsible for preparing an annual survey of international affairs, while on the side he worked at what would be his *magnum opus*, the multivolumed *A Study of History*.

The clash of ideas leading to his resignation from the Korais chair was critical for Toynbee's later work and deserves closer attention. During and immediately after World War I, Toynbee shared a vaguely liberal faith in the beneficence of national self-determination. As was his wont, he recorded his views in a book, *Nationality and the War* (1915b), which explored ways in which self-determination could be reconciled with the strategic interests of the great powers. The long drawn-out agony of World War I may subsequently have called the virtues of nationalism into question in Toynbee's mind; but the decisive turn away from his previous outlook occurred only in 1921. The occasion was a visit to Anatolia, where in 1921 fighting between Greeks and Turks had broken out anew.

On arriving in Smyrna, he found ample evidence of Greek atrocities against Turks—brutality of exactly the sort that he had pilloried during the war in chronicling Turkish atrocities against the Armenians. Cruelty on such a scale appalled him, and required explanation. How could men, seemingly civilized and even charming in private encounters, engage in such bestiality, or condone it in others?

Toynbee's answer was conditioned by the fact that in 1920 he had read Oswald Spengler's *Der Untergang des Abendlandes* (1918-1922), and there, for the first time, encountered the idea of a plurality of civilizations, each following a fixed, cyclical pattern of growth and decay. Spengler's book affected him strongly, for Toyn-

bee himself, since 1914, had recognized a profound parallelism between ancient Greco-Roman history and that of modern Europe. World War I had seemed to him like a reprise of the Peloponnesian War, or perhaps, of the Punic Wars. Human history, he felt, fundamentally conformed to a tragic pattern, whereby pride and excess led, sooner or later, to disaster. He developed this theme in 1920 in a remarkable lecture later published as *The Tragedy of Greece* (1921). Here for the first time he used terms that were to be constitutive for his *Study of History*, applying them to Greco-Roman civilization, and using literary quotations to illustrate the changing "state of the soul" which he was analyzing. At this point in his thought, Toynbee was concerned only with Greco-Roman and modern European patterns of history and seems to have conceived of civilization as a single, if cyclical, creation, humanity's "supreme work of art." His reading of Spengler suggested to him that other peoples, perhaps, also exhibited the cyclical patterning discernible in Greco-Roman history, in which case civilization ceased to be singular and instead became plural.

What Toynbee saw in Anatolia in 1921 confirmed this hypothesis. He came to believe that the brutality he witnessed—Greek and Turkish alike—was a result of the breakup of a newly-discerned entity, Ottoman civilization. Moral restraints and social rules that had regulated human encounters in the heyday of that civilization had lost their power over the heirs of that dying civilization—Turks, Greeks, Armenians. Instead, ideas imported from western Europe, above all the idea of nationalism, sanctified the mass brutality and amorality of the Greek-Turkish War. Nationalism thus became an evil rather than a good; and the intrusive Westerners, who since 1699 had been exploiting their superior power to disrupt the fabric of Ottoman society and civilization, were partly (even mainly) responsible for the atrocities Toynbee had witnessed, as well as those he had accused the Turks of perpetrating during World War I.

If civilizations were, indeed, plural, as his experience in Anatolia in 1921 seemed to prove, the next question to ask was whether the tragic pattern he had already discerned in Greco-Roman history and glimpsed in modern European history was shared by other civilizations in other parts of the world, as Spengler had declared was the case. The death throes of Ottoman civilization, as analyzed in his book, *The*

Western Question in Greece and Turkey (1922), certainly predisposed Toynbee to suppose that similar tragic patterns held throughout the world; yet as an Englishman and self-styled empiricist, he thought that he ought to test the hypothesis by looking for analogues and parallels in the history of the rest of the world before, like Spengler, dogmatically declaring the truth of his new idea. Accordingly, as he was returning to London by train from Constantinople, he jotted down on a single sheet of paper the 13 headings around which his *Study of History* was later organized.

The ambition to test his great hypothesis was thereafter always in the forefront of Toynbee's mind, but during his years at the Royal Institute of International Affairs, he had to reconcile this project with the requirements of his job, which were heavy enough. For Toynbee was expected to write an annual volume of approximately five hundred pages, surveying international affairs, and to publish it as soon after the events as possible, so as to provide a basis for intelligent public appraisal of the problems of foreign policy. At first he had to catch up, twice publishing two volumes in a single year. By 1930 he had come abreast of events, and he could start work on the current year in November, and then write furiously until June, by which time the text had to go off to the printer so as to appear within a twelvemonth of the period being surveyed. This regimen gave him free time in summers to start writing *A Study of History*. Close interplay between current events and the sweep of world history was built into Toynbee's rhythm of work. He systematically shifted the geographical focus of each annual survey, partly in response to the drift of current events, but also in order to compel himself to master the historical background of each major part of the earth, preparatory to both the upcoming survey and his *Study of History*.

The first three volumes of his *magnum opus* appeared in 1934, followed by the second three volumes in 1939. Then came World War II, when Toynbee's career was once again interrupted by official duties in the Foreign Office. He resumed work on *A Study of History* in 1946 and brought out the final 4 volumes in 1954, 32 years after its grand structure had first been conceived.

During that length of time, Toynbee's ideas underwent far-reaching change. In the 1920s and early 1930s his mind was still fundamen-

tally shaped by his classical training. Civilizations were, in his eyes, the supreme achievement of humankind—a state of the soul, shared by millions, which defined what was good and thereby gave humanity goals to strive for, and made individual lives meaningful. All civilizations, he declared, were “philosophically contemporary” with each other and equally valuable. This was, indeed, one of the main thrusts of his first volumes. He sought to correct the myopia of Western minds which had previously assumed that the values of their own civilization were universal and uniquely valid.

Yet Toynbee’s relativism had its limits: for the other central proposition of these first volumes was that all civilizations conformed to the tragic pattern he had discerned in Greco-Roman history, passing from growth to breakdown, followed by the rise of a universal state and universal church. A church might, under propitious conditions, serve as chrysalis from which a new “affiliated” civilization could arise after the older civilization had finally disappeared under the blows of barbarian assault and proletarian revolt from within.

However entrancing such a grand structure might be, as World War II announced its approach, Toynbee found it harder and harder to believe that civilizations were the supreme creations of the human spirit. His distress was inflamed by family difficulties, climaxing with the suicide of his eldest son (1939) and a separation from his wife (1942). This was followed in 1946 by a divorce and remarriage to his long-time research assistant. His private hurts were matched by a sense of defeat in his professional life, for the whole point of his study of international affairs had been to head off renewed, suicidal war. Yet by 1938, Toynbee found himself urging the British government to resist Nazi aggression, even at the risk of war!

Little by little, Toynbee worked his way toward a reevaluation of the human condition. Personal mystical experiences, three times renewed, played a part in convincing him that a “supreme spiritual reality” existed, beyond the world of ordinary experience. Its gradual self-revelation to humankind was what gave history meaning. Such a vision can be understood as a shift away from Toynbee’s earlier classical, cyclical framework for viewing human affairs to a fundamentally Judeo-Christian, linear conception of history. The shift made his writing a faithful mirror of the deepest bifurcation in Western

civilization itself. Both the acclaim and the fierce attack that Toynbee’s ideas generated after 1947 reflected the provocative manner in which his writings mirrored these deep-set and fundamentally incompatible heritages of Western thought.

A few passages in the volumes of *A Study of History* published in 1939 reflected Toynbee’s new religiosity, but only after World War II did his altered point of view achieve clear expression. Yet, oddly enough, the extraordinary fame that came to him with the American publication of a one-volume abbreviation of the first six volumes of *A Study of History* (1947), rested more on his older classical and cyclical vision of the human condition than on his newer, transcendental outlook.

Toynbee’s renown was largely the work of Henry Luce and his magazines, *Time* and *Life*. Luce believed that Toynbee’s work demonstrated great rhythms of human experience that were compelling the United States to what he had already christened “The American Century.” Many Americans were in fact puzzled and dismayed at the sudden emergence of their nation as a great power, soon to be locked in cold war with the Soviet Union. Why had such a thing occurred? Who was responsible? Was the United States caught up in some vast historical process that worked independently of human will? Toynbee’s discussion of the genesis, growth, breakdown, and dissolution of civilizations spoke directly to such concerns. It is not, therefore, surprising that his ideas met with extraordinary response in the United States.

His sudden fame, in turn, required intellectuals in both Britain and the United States to come to terms with Toynbee’s ideas, and for the first time professional historians and other shapers of learned opinion began to pay attention to his grand synthesis. Their reaction was to find fault with his generalizations and charge him with carelessness about detail. Debate spread to Germany and France as well; even Poles and Russians felt compelled to criticize Toynbee’s ideas. Attacks on Toynbee became especially virulent after 1952, when he delivered the Reith lectures on British Broadcasting Corporation radio (published as *The World and the West*, 1953). In these lectures, Toynbee blamed Westerners for past aggressions against other peoples of the world, and had nothing kind to say about European achievements. Many in Britain felt indignant. Their empire was in

process of dissolution; the Soviet Union presented an ominous challenge to liberal values. To his critics, Toynbee seemed like a traitor to all that had made Britain and the Western world great.

The appearance of the next four volumes of *A Study of History* in the following year added fresh fuel to the controversy, for here Toynbee's religiosity became fully apparent. Unfriendly critics accused him of overweening pride in undertaking, like the prophets of old, to define God's purposes for all mankind. Toynbee did not respond directly to his critics, which enraged them even more; but in retirement he did eventually publish a volume of *Reconsiderations* (1961), reassessing his great work in the light of what others had said. In this volume, Toynbee freely admitted that he had made errors of detail; and he also altered the roster of civilizations as presented in *A Study of History*. On major points he stood firm, yet the grandeur of his original synthesis was diminished by the modifications and retractions he accepted in his *Reconsiderations*. On the other hand, his modest and irenic tone disarmed his fiercer opponents.

Toynbee, in effect, left the task of amending his great work to others. Instead, after his retirement in 1954, he returned to scholarly themes he had been compelled to drop in 1915. Two massive works resulted: *Hannibal's Legacy* (1965) and *Constantine Porphyrogenitus and His World* (1973). The first was a magistral summation of all that scholars could find out about Roman history between 266 and 133 B.C. and rebutted in act rather than by words the charges of those critics who accused him of carelessness with detail. However, it offered little in the way of new ideas; and the same was true of *Constantine Porphyrogenitus*. Both works, nevertheless, are admirable monuments of humanistic scholarship.

In addition to these works, Toynbee continued to write travel books, and many book-length essays seeking to interpret the meaning of the age in the light of both his metaphysical convictions and his continued observation of the current scene. After 1961, debate over his ideas waned in the Western world but in compensation his reputation mounted in Japan (and to a lesser extent in Latin America and India). There, Toynbee's critique of Western imperialism met a readier acceptance than at home, and his religious views struck a lively chord among heirs of Asian traditions. Several of Toynbee's conversations with questing spirits from the

non-Western world were published as books—e.g., *Surviving the Future* (1971) and *The Toynbee-Ikeda Dialogue: Man Himself Must Choose* (1976b). They show that toward the end of his life Toynbee became rather more cheerful than he had been during and immediately after World War II. The cold war did not develop into the final catastrophe he once had feared; and the open-endedness of history, upon which he had always insisted, even when most convinced that the troubles of his time attested the birth pangs of a new universal state for Western civilization, suggested to him that humanity and civilization might yet succeed in surviving the technical virtuosity of destruction that modern technology had called into being.

He also cultivated a new interest in the design and history of cities, inspired by encounters with the architect Constantine Doxiades and his Institute of Ekistics in Athens. This, too, produced several books, including *Cities of Destiny* (1967) and *Cities on the Move* (1970). At the very end of his life, Toynbee embarked on a general study of the ecological history of humanity; but the manuscript, published posthumously as *Mankind and Mother Earth* (1976a), fell far short of its programmatic goal.

Toynbee's ceaseless writing came suddenly to a halt in August 1974, when he suffered a disabling stroke. He died 14 months later, on October 22, 1975. His fame and the controversy that his writing provoked made him the best-known historian of his age. Whether writing history on the basis of civilizational units and ventures into metahistorical speculation will prove fruitful remains to be seen. Still, the significance of Toynbee's ideas as part of the intellectual history of the twentieth century can scarcely be doubted. He provoked passionate responses not merely among English-speaking peoples and west Europeans, but in Japan and other non-Western lands as well. No historian before him ever made such a cross-cultural impact within his own lifetime, and few worked harder to achieve a truly global perspective.

WILLIAM H. MCNEILL

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V

VANCE, RUPERT B.

Rupert Bayless Vance (1899–1975), who at his death was Kenan professor emeritus of sociology at the University of North Carolina at Chapel Hill, grew up on a farm near Plummerville, Arkansas. His father operated the farm and kept a general store in town. His mother taught school. When he was three years old an attack of polio left his legs paralyzed, and he moved on crutches and in wheelchairs for the rest of his life. Crippled from early childhood, he read omnivorously. His fascination with books and with events around him was encyclopedic, probing, and analytical. He could not do many of the things most boys did, but he did not isolate himself from his peers.

Vance spent his entire career in the South and identified strongly with the region. He began his sociological work in the great center of Southern regional studies developed by Howard W. Odum at Chapel Hill, where Vance's contemporaries included such scholars as Harriet L. Herring, Katharine Jocher, and Guy B. Johnson. Vance's early years as a sociologist thus coincided with the great depression of the 1930s.

The intersection of the great depression and Vance's identification with his native rural South profoundly affected the early writings that made his reputation. He already had studied Southern problems in his published dissertation, written under Odum's direction. Entitled *Human Factors in Cotton Culture* (1929), it was a remarkable interweaving of analysis of cotton farming as a social system and concrete

depiction of the lives of sharecroppers and small farmers. His next book, *Human Geography of the South* (1932), was a monumental treatise drawing from geology, geography, ecology, economics, sociology, and history to explain the differing ways of life in Southern subregions. Many of Vance's publications during the 1930s dealt with what were then called "human" problems of the South. Examples include "Human Factors in the South's Agricultural Readjustment" (1934), *How the Other Half Is Housed: A Pictorial Record of Subminimum Farm Housing in the South* (1936), and *Farmers Without Land* (1937). Policy-oriented research on the South was the primary emphasis of sociology at Chapel Hill during the 1930s, and Odum stimulated virtually all members of the department, but Vance, no mere disciple extending Odum's ideas, developed his own approaches.

Vance was not a narrowly disciplinary sociologist but a broad-gauged social scientist and a humanist. His boyhood reading had been mainly in English and American literature. In 1920 he had earned his A.B. at Henderson-Brown College in Arkansas, majoring in English and social sciences. From there he went to Vanderbilt University for an M.A. in economics. He then taught English from 1921 to 1925, two years in a high school in Talihina, Oklahoma, and two as instructor at South Georgia College.

During these early years, Vance learned firsthand about social conditions in several very different parts of the South. He saw poverty, pellagra, hookworm, rickety shacks, and worn-out people tilling wornout land. This dismal spectacle could have disheartened some sensi-

tive observers; but Vance was an optimist. Coming to believe that sociology could be a vehicle for the betterment of the region, he resolved to become a sociologist. He learned of the new department of sociology that had been created by Odum in 1920 at Chapel Hill; of Odum's Institute for Research in Social Science, founded in 1924; and of Odum's journal, *Social Forces*, which had begun publication in 1922 and at that time specialized in Southern problems. Vance went to North Carolina as a graduate student in 1926, received his PH.D. and an associate professorship in 1928, and remained at Chapel Hill throughout his academic career.

Vance never ceased to acknowledge his deep intellectual debt to Odum, but after the 1930s he had growing doubts about the utility of Odum's concept of regionalism as a tool for social science and planning. Regionalism was not an analytical concept or a theory, but an eclectic empirical rubric under which the social scientist brought to bear the facts and insights of whatever disciplines seemed appropriate to the solution or understanding of given problems. In the hands of a master like Vance, regionalism could be a useful orienting device; but a close reading of *Human Geography of the South* and Vance's subsequent writings during the heyday of Odum's regionalism leaves the impression that he could have written them whether or not he had been exposed to the concept. In his later years he came to regard the policy aspect of regionalism as essentially a framework for organizing the kinds of reform programs that were no longer essential in the South after World War II, where, as Vance once said, "the New Deal has been dealt."

From about 1940, Vance's research shifted from Southern poverty to demography and human ecology, though his writings continued to treat mainly the South. His third major work, *All These People: The Nation's Human Resources in the South* (1945), won acclaim from demographers as a model of its kind. It painstakingly surveyed the population composition and processes of the Southern people in 146 tables, 281 graphs, and 503 pages of text. Vance's most influential contribution to human ecology was his paper with Sara Smith, "Metropolitan Dominance and Integration" (1954). This paper succinctly analyzed the emerging urban networks of the South. It used an empirical study to show clearly and convincingly how human ecology is a matter of social orga-

nization, not just of spatial arrangements, especially in an age of instant communication.

Vance was, in a sense, a "professional Southerner," but decidedly not a parochial sectionalist or an agrarian romantic. He was a sober realist who believed that the South's destiny was, and should be, to enter the mainstream of an urbanized national society. But he also believed that the region could keep some of its rural and small-town quality of life and avoid the worst of the big-city problems that plagued other regions (1954). These hopes were prophetic for the North as well as for the South. Southern industrialization remained chiefly in small cities, small towns, and the countryside; and by the time of Vance's death, Northern industry was rapidly deconcentrating from central cities to greener, less crowded (and less taxed) places. The South made great strides during Vance's career, and his writings pointed the way. In his first major work he had surveyed King Cotton's oppressive realm. Before his retirement he wrote an article entitled "Beyond the Fleshpots" (1965), in which he pronounced the Mason-Dixon line "no longer an iron curtain against the Affluent Society" and summoned the region to the pursuit of high culture.

Vance's impact extended far beyond his writings. He served as a consultant to the National Resources Planning Board, the Rosenwald Fund, the Social Science Research Council, the National Institutes of Health, the Bureau of the Census, and the United Nations. His studies directly influenced the farm tenancy programs of the New Deal. He was editor of *Social Forces* for 12 years, for 8 of them jointly with Guy B. Johnson. He directed dozens of dissertations and helped many graduate students launch their scholarly careers with publishable research term papers long before this practice was common. He influenced the Bureau of the Census indirectly, through the work of several former students who held key positions there, as well as through his own studies.

Vance was not a systematic theorist. Many of his writings were aimed at solving concrete social problems. In his more purely academic writings, his objective was to enable readers to see social life whole. In both kinds of work his approach was eclectic; he was more interested in understanding specific situations completely, and in their complexity, than in developing theory. He drew together whatever ideas and facts he thought most pertinent and did not fit

them to procrustean beds of abstract theory that would sacrifice their richness of detail. Therefore his sociological legacy is more empirical than theoretical. For anyone who wants to understand the South as it struggled under the dead weight of a one-crop farm economy, suffered through the great depression, then burst swiftly into prosperity and urbanism, Vance's writings are unsurpassed. He successfully blended historical and statistical data, penetrating insight, and warmly human portrayal of the lives of ordinary people.

RICHARD L. SIMPSON

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VINER, JACOB

Jacob Viner (1892-1970), economist-scholar, was born in Montreal of immigrant parents. He graduated in 1914 from McGill University, where Stephen Leacock was one of his teachers.

In 1915 he began graduate studies at Harvard University; he was awarded his M.A. in that year and his Ph.D. in 1922. While at Harvard he formed a close friendship with Frank W. Taussig, one of America's most distinguished economists.

Viner joined the faculty at the University of Chicago as an instructor for the year 1916/1917. He left at the end of the year for government service in Washington, but returned in 1919. In 1925, at the age of 32, he was promoted to the rank of full professor, and he assumed the Morton Hull chair in 1940. As editor of the *Journal of Political Economy* for 18 years, he brought the *Journal* to the peak of its distinction. In 1946 he went to Princeton University, where he held the Walker chair from 1950 until his retirement in 1960.

In addition to his academic career, Viner had a record of distinguished public service. Starting with a post on the U.S. Tariff Commission under Taussig in 1917, he served briefly with the U.S. Shipping Board during World War I. As a special assistant to the U.S. Treasury and consulting expert under Secretary Henry Morgenthau during the period 1934-1939 and again in 1942, he gave advice on a wide range of monetary and financial matters. He was also a consultant to the Department of State and the Board of Governors of the Federal Reserve System.

Viner was noted for his wit, his independence of mind, his lavish devotion of time and energy to helping others, particularly younger scholars, and his extraordinary erudition in an astonishing variety of fields. As a bibliophile, his advice was frequently sought by the great libraries. As a student of economic history, philosophy, literature, and theology, he helped scholars in these fields and conducted many seminars in departments other than his own. Donald Winch, one of Viner's former students, has written:

. . . he did not conform to the specialist pattern of the modern academic. Moreover, he cannot be assimilated within any of the obvious doctrinal or ideological groupings. He was not an orthodox neo-classical economist, still less a member of what has become known as the "Chicago School," in spite of his long connection with the Department of Economics at the University of Chicago during the period when some of its doctrines were being formed. By the same token, he was not an orthodox anti-Keynesian on matters of theory or policy. If a label must be applied it seems best to describe him as a learned sceptic, distrustful of both technical

and ideological fashion, yet always constructive, never merely an iconoclast.

Viner achieved his international reputation early in his career. Virtually all of his books broke new ground and set an example for later analysis and research. While Viner's contributions spanned a remarkable range of subjects, his most noted contributions lay in three fields: the theory of cost and production, international economics, and intellectual history with emphasis upon the history of economic ideas.

Though his writings in the first of these areas are few, they constitute profound and lasting contributions. In 1921 he published "Price Policies: The Determination of Market Price" (reprinted in 1958, pp. 3-7), a remarkable essay of five pages in which he anticipated many of the ideas on monopolistic competition that Edward H. Chamberlin and Joan Robinson were to write about more than a decade later. In that paper, he notes the rarity in the real world of markets approximating the conditions of perfect competition; the requirements for perfect competition of homogeneity of products and responsiveness of prices to changing conditions; the negative slope of the noncompetitive firm's demand curve and the tendency toward stickiness of prices where monopolistic elements are present; the wide range and variety of market forms between the polar cases of pure competition and pure monopoly; the use of trademarks, brands, variety in styling and packaging as means to achieve differentiation of products—the attribute subsequently used to characterize monopolistic competition; competition by means of instruments other than price, such as advertising, packaging, and supplementary services; the manufacture and sale of consumers' goods as the natural province of product differentiation; the role of price leadership in the supply of producers' goods; and the kinked-demand-curve model of oligopoly pricing.

The kinked-demand-curve model achieved general notice only some two decades after Viner introduced it. It postulates an asymmetry in the reactions of oligopolists to price reductions and price increases by their competitors: price reductions are imitated to prevent loss of customers, but price increases are characteristically ignored, so that the firm that initiates price changes is likely to lose out in either case. This is still the most widely held theoretical explanation for the infrequency of price changes in oligopolistic industries.

Besides this extraordinary little piece, Viner wrote a far more noted article on cost and pricing. His "Cost Curves and Supply Curves" (1931, reprinted in 1958, pp. 50-84) introduced a set of ideas and analytic tools that are now found in every textbook. They include the definition of the concept of marginal cost as the derivative of the firm's total cost with respect to magnitude of output and the analysis of the relation of marginal to average cost; recognition of the fact that, in the absence of externalities, average cost including rent must be the same as marginal cost excluding rent (the basis of Joan Robinson's analysis of her four cost curves); analysis of the relation of constant and increasing costs to the equilibrium of the firm; analysis of the role of internal and external economies; the important distinction between pecuniary and technological externalities; the recognition that under imperfect competition marginal revenue must be less than price; and the formulation, using the terminology now universally employed, of the necessary condition for profit maximization, that marginal cost equal marginal revenue.

One of the major contributions of this article occurred fortuitously. As part of his analysis of the relation between the short- and long-run average cost curves of the firm, Viner asked Y. K. Wong, a young mathematician, to draw a diagram whose specification involved a mathematical contradiction. From their discussion of the problem emerged a fundamental result, now called the Wong-Viner theorem, which shows that the long-run supply curve is the envelope of the short-run curves; that is, it is composed of the lowest segments of the short-run curves corresponding to the different sizes of plant available to the firm when planning its construction program. In the initial publication of the article and even in its many subsequent reprintings, Viner was careful to include his original error and his report of the correction proposed by Wong, which served as the basis for all later analysis of the subject.

In the area of international economics, Viner's work covers the entire subject from the pure theory of international trade, through the theory and practice of commercial policy, to international monetary theory and policy.

His work on the pure theory of international trade was distinguished by an effort to extend and modernize the Ricardian doctrine of comparative costs—to show that it can be severed from the labor theory of value and stated instead in terms of generalized real costs (see, in partic-

ular, *Studies in the Theory of International Trade*, 1937, chapter 8). It is for this reason, perhaps, that Viner's contributions to pure trade theory are not widely read or appreciated today. Contemporary work on trade theory is cast in terms of opportunity costs, not real costs, following the contributions of Gottfried Haberler on the one hand and Eli Heckscher and Bertil Ohlin on the other. Viner's presentation of classical trade theory is, nevertheless, the definitive restatement of Ricardian doctrine.

His papers on commercial policy include a study and critique of American practice in the application of the most-favored-nation clause, arguing in favor of the "unconditional" form of that clause, a study of the problems of conducting trade between state-controlled national economies, and papers on trade policy in the postwar period (all these are reprinted in 1951, see especially chapters 1, 5, 14, 20, 24, 25). His most important work in this field, however, was his study of customs unions (1950), which supplied the conceptual framework for analysis that has been used by almost every other study of the problem, theoretical and empirical. (Often, however, that framework has been misapplied; its users do not always realize that the notions of "trade creation" and "trade diversion" introduced by Viner are useful only for assessing the effects of a union on the world as a whole, not on individual participants. It was typical of Viner's work to take a cosmopolitan rather than a national view.) His work on this subject also provided a remarkable example of the proposition that is now known as the theorem of the second best, which asserts that satisfaction of some subset of the conditions necessary for an optimum may in fact yield a net loss in welfare. Thus, Viner showed that a partial reduction in restrictions upon freedom of trade in the form of a customs union may not always prove beneficial to the world as a whole.

Viner's work on commercial policy was colored by his fear that governments would interfere increasingly with market forces, domestically and internationally, and the same concern colored his principal contribution to the literature on trade and development (1952). It is, in effect, an attack upon theories and strategies of development based upon production for the domestic market—upon the notion of balanced growth developed by Ragnar Nurkse and the import-substituting strategies advocated by Raúl Prebisch. It attacks, in addition, the proposition that the terms of trade of developing countries are

bound to deteriorate and the assertion that development must be based upon industrialization.

Viner's first book on international monetary economics, *Canada's Balance of International Indebtedness, 1900–1913* (1924), was one of the first thorough studies of balance-of-payments adjustment. It examined the manner in which Canada's trade balance adjusted the inflow of borrowed funds, tracing the effects of capital inflows on the Canadian money supply and Canadian prices, and the effects of price changes on Canadian trade. In his *Studies* (1937, pp. 413–414), Viner said of that Canadian study: "I concluded that the Canadian mechanism corresponded in all of its important aspects to the mechanism as formulated in the classical doctrine."

Viner's *Studies in the Theory of International Trade* deals mainly with the monetary side of international economics (although its best-known chapters deal with pure theory). Combining his interest in the history of economic thought with his interest in the balance-of-payments adjustment process, he produced a detailed history of the development of international monetary theory from the bullionists through neoclassical doctrine. During and after World War II, moreover, Viner wrote extensively on monetary reconstruction (1951, chapters 13, 15, 21, 22).

In his contributions to the history of ideas, Viner put to good use his remarkable erudition and his meticulous scholarship. Much of his work in this area was devoted to the theological writings of the Middle Ages and thereafter, to the doctrines of the mercantilists, and to the work of Adam Smith, with contributions on the work of Bernard Mandeville, Jeremy Bentham, John Stuart Mill, and Alfred Marshall. In style, the works range from the easy reading of the delightful lectures, *The Role of Providence in the Social Order* (1972), to the tour de force of his introduction to *John Rae's Life of Adam Smith* (1965), with its virtuoso display of information about intellectual life in the eighteenth century.

No small set of themes runs through these writings. But Viner was perhaps most fascinated by the influence of theological treatises upon the notions of the early economists and by the economic ideas of the theologians themselves. At least two works, both posthumously published, have as their central subject the relation between economic and religious ideas. The major work, *Religious Thought and Economic Society*

(1978), is composed of four chapters of an unfinished intellectual history of the economic aspects of Christian theology from the early church fathers up to the seventeenth century. The study deals with such groups as the scholastics, the Jansenists, the Jesuits, and the early Calvinists, and with topics such as private property, riches and poverty, usury, just price, and the Weber-Tawney thesis on religion and the rise of capitalism. *The Role of Providence in the Social Order* (1972) goes on to seventeenth- and eighteenth-century religious thought, particularly in England, and the manner in which it dealt with the threat to traditional beliefs posed by the continuing discoveries of science. Viner found that, despite the challenge of new ideas, theological thought continued to exert extraordinary power on economic ideas well up to the end of the eighteenth century. A striking example is the work of Adam Smith, whose economic ideas are taken by most modern readers to be entirely secular in content. It is now rarely recognized that "the Invisible Hand" was a phrase widely used by eighteenth-century deists (and Adam Smith was a deist) to refer to the "Author of the Universe." Thus, in *The Wealth of Nations* this term was not used as mere metaphor but as a description of the means Providence had used not only to constrain human selfishness but even to harness that selfishness as an instrument working for the general good. Viner also emphasized the extraordinary apologetic character of religious and economic thought in England during the seventeenth and eighteenth centuries, and its view that social and economic inequality—poverty and wealth—are divinely ordained and not subject to question. Viner concludes that in England "the period 1660 to the 1770's was . . . a period of entrenched, unchallenged and complacent conservatism" (1972, p. 98).

Another major subject of Viner's meticulous scholarship was the work of the mercantilists, a group composed of "practical men" writing between the fifteenth and eighteenth centuries about economic policy in its relation to international trade. Viner showed that many of the received characterizations of mercantilist doctrines were misrepresentations or oversimplifications. He demonstrated, for example, that mercantilists did not consider prosperity to be merely an instrument to promote national power but rather an end in itself, in general coequal with the objective of national power. He showed also that mercantilists were no homogeneous, undifferentiable group. Perhaps most striking is his evi-

dence of a strong strain of laissez-faire ideas among the mercantilist writers, at least some of whom questioned the competence of governments to regulate economic affairs. Others raised the classic problem about governmental regulation of the consequences of human selfishness—who, in such an arrangement, is to regulate the regulators? Thus, Viner concludes that "if Adam Smith had carefully surveyed the earlier English economic literature . . . he would have been able to find very nearly all the materials which he actually used in his attack on protectionist aspects of the mercantilist doctrine" (1937, p. 92.)

But perhaps Viner's greatest contribution was his role in providing standards of scholarship, of meticulous care and accuracy, which have served as goals for several generations of students interested in the history of ideas. His convictions on this score led Viner to write *A Modest Proposal for Some Stress on Scholarship in Graduate Training* (1953), a paper noteworthy for its balance and moderation and the wit that serves to convey all the more effectively the author's passionate belief in this subject.

Many honors were awarded to him during his lifetime, including 13 honorary doctor's degrees. To celebrate Viner's sixty-fifth birthday, his friends and colleagues collected some of his most original and influential articles in a volume entitled *The Long View and the Short* (1958). Viner was president of the American Economic Association in 1939/1940, and in 1962 he received the association's most distinguished honor, the Francis A. Walker medal, which is awarded only once in five years. Viner was made a fellow of the American Academy of Arts and Sciences, the British Academy, and the London School of Economics and Political Science, and he was also elected a member of the American Philosophical Society, the Royal Academy of Sweden, and the Accademia dei Lincei. The American Council of Learned Societies gave him a special award in 1958.

WILLIAM J. BAUMOL AND ELLEN VINER SEILER

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VOEGELIN, ERIC

Eric Voegelin, a leader in the attempt to recover a critical and comprehensive philosophy of man, society, and history, was born in Cologne in 1901. His family soon moved to Vienna, where he received his education. After graduating in law from the University of Vienna, he began his academic career as an assistant to Hans Kelsen. From 1924 to 1927 he traveled abroad as a Laura Spelman Rockefeller fellow, largely in the United States, where he attended the lectures of John Dewey, Alfred North Whitehead, and John R. Commons at their respective universities. His American ex-

perience, which was extremely important to him, was the basis of his first book, *Über die Form des amerikanischen Geistes* (1928).

Returning to Austria, Voegelin in time became an associate professor on the law faculty at Vienna. During this period (1929–1938) he published several works, including a critical study of the Kelsen-inspired Austrian constitution, a scholarly dissection of the biological claims of National Socialist race thinking, and a treatise on totalitarian movements as “political religions.” Forced to retire from his post after the Nazi annexation of Austria in 1938, Voegelin managed to escape to Switzerland. After he was joined by his wife, he emigrated to the United States, teaching for brief periods at Harvard University, Bennington College, and the University of Alabama, until he was invited in 1943 to Louisiana State University. There he taught government for many years and was ultimately appointed Boyd professor. In 1958 the University of Munich invited Voegelin to occupy its first chair in political science and to found and direct its Institute for Political Science. After his retirement from Munich in 1969, Voegelin, who had become a naturalized American citizen, returned to the United States to reside in Stanford, where he accepted an appointment as senior research fellow at the Hoover Institution. He continues to live in Stanford.

Eric Voegelin's publications reflect his assimilation of materials that range from institutional and foreign policy studies to textual analyses in many ancient and modern languages. His linguistic gifts, and his knowledge of the specialized literature of ancient cultures, have enabled him to pursue varied research interests, unbound by disciplinary conventions, in political science, philosophy, archeology, comparative religions, the philosophy of history, and literary criticism. He has also read widely in the other social sciences and in physics and biology. Voegelin was relatively unknown in the English-speaking world until the 1950s, when *The New Science of Politics* (1952) was published, and his *magnum opus*, *Order and History* (1956–1974), began to appear.

Voegelin's openness to the implications of newly discovered textual materials has led him substantially to revise his writing plans at critical points. Thus, in 1943 he abandoned a lengthy “History of Political Ideas” that followed conventional chronological organization to work out an important new theory of experience and

its symbolization. This theory became the basis for *Order and History*. Similarly, after publishing the first three volumes of *Order and History*, which in important respects preserved a historical time line from the ancients to the present, Voegelin recast much of the work in terms of principles spelled out in the introduction to the fourth volume of the series. The older "History of Political Ideas" remains today largely unpublished; however, in 1975 important portions of it were published as *From Enlightenment to Revolution*.

Voegelin's philosophy of political existence. Voegelin insists that the task of contemporary thought is, as written by T. S. Eliot, to "recover what has been lost" in the understanding of man, society, and history in the modern centuries. It would be a mistake, however, to call him a "traditionalist" or to apply any comparable label to his work, first, because his teaching can be criticized only on the basis of extensive philosophical and textual knowledge, second, because it cannot be encapsulated within one philosophical or political category. Voegelin himself has called such attempts "positionism," meaning that branding a thinker's teaching in terms of some unfashionable or minority "position" is a way of avoiding close and serious examination of its tenets.

Voegelin is emphatic in claiming for his work full "empirical" status. Initially, his philosophy of existence stemmed not from "vain and perishing curiosity," but from his experience of the horrors of political disintegration in the Europe of the 1930s. In order to understand how entire societies could collapse, and how unprecedented totalitarian régimes could replace them, Voegelin argued it was necessary to widen and deepen the inquiry of political science, moving from the level of institutions to that of the language symbols underlying the institutions. Nor could one stop with the political culture of a particular national community, for a given nation-state was but a fragment of a larger whole called a civilization. Finally, a civilization itself was not an insular entity, but a participant in the "drama of mankind" from its unknown origins to its unknown end.

Although Voegelin's quest for a comprehensive philosophy of human existence was initially motivated by his conviction that only such a philosophy could illumine the contours of the crisis produced by twentieth-century totalitarianism, he avoided the temptation to write a *livre de circonstance*. Instead he proceeded carefully,

step by step, empirically to construct a theory of order and history in relation to which the catastrophes of the present could more adequately be understood.

Voegelin's task, then, was at once theoretical and practical. The attempt to recover the reality of human political and social existence, a reality that had been overlaid and obscured by the "second-reality" constructions of ideological fanaticism, was no mere academic enterprise. At the same time, that quest could not rest with the language of conventional wisdom, but had to press on to a philosophical theory of the structure and process of the reality in which the human being participates. It thus could scarcely dispense with the language symbols in which past thinkers, engaged in a similar search, had expressed their experiences. Voegelin's complicated terminology is the result of his careful reporting of the often forgotten or misremembered language of past searchers.

Voegelin's mature political philosophy rests upon a theory of equivalent experiences and symbolizations of reality that he evolved after 1943, partly as a result of lengthy discussions with his philosophical and personal friend Alfred Schutz. Through close study of representative texts in a variety of civilizations widely dispersed in space and time, Voegelin was able to isolate key symbols relating to man's self-interpretation, to relate them according to their degree of "compactness" and "differentiation," and to discern the same "structure" of reality in "equivalent" experiences and symbolizations dispersed throughout the historical process. As a "trail of symbols" illumining the structure of reality, history is seen as a process moving between the poles of a divine beginning and a divine beyond.

The symbolism of the "between" (from Plato's *Metaxy*) is of central importance to Voegelin. An examination of "the material" of the "empirical" investigator will show that the human being, far from experiencing political and social reality as a set of phenomenal "facts" outside his consciousness, senses that he participates, from within, in a reality symbolized as a field of contrasting forces. The human condition is one of "tension" between life and death, openness and closure, the love of God and the love of self, immortality and mortality, eternity and time. Any political theory that ignores this basic experience of reality and proceeds as if it were possible to settle down on one of its two "poles," the beginning and the beyond, engages in a "de-

formation" of symbols and erects a "second reality" or dream world. A major defect of contemporary political and social science is that it has no basis for discriminating symbols that illumine the participatory reality of the between and those that reject or deform it; the latter are the "ideologies," *Ersatzreligionen*, and deformed "gnostic" symbolizations that erupted with murderous results in twentieth-century totalitarian movements.

Gnosticism and modernity. One of the most controversial aspects of Voegelin's work is his analysis of modernity as "gnostic" in essence. From a careful analysis of documents, Voegelin concludes that the core of ancient gnosticism was the drive toward self-redemption from evil and imperfection in the world of the "between." This drive to escape the world through a magical act of acquiring *gnosis* is transmuted by modern ideologies into the drive to *conquer* the world through one of a number of means ranging from science and technology to the violence of the "total revolution." In diagnosing modernity as spiritually diseased, however, Voegelin does not mean to overlook the strong persistence of classical and Biblical influences in parts of the modern world, together with an important element of "common sense," especially in the Anglo-Saxon political cultures. Nor does his more recent thought neglect the alchemic tradition and influences other than gnosticism that produce modern disorder.

Theophany and the manifestation of reality. According to Voegelin's analysis, both ancient gnostic and modern ideological systems of thought are closed to any experience beyond the ego and its drive to subjugate reality to its control and manipulation. In Volume 4 of *Order and History*, Voegelin coins the term "egophany" to represent this complex of experiences. By contrast, if consciousness is open to the reality of existing in the between, it will not fall prey to the illusion that man is self-created, but will recognize itself as dependent on an order of being that exists out of a divine ground.

The structure of reality in the between is illumined most vividly by key events described as "theophanies," or manifestations of the divine presence in and to the human consciousness. Although the experience of divine presence in the cosmos is symbolized compactly in the language of the myth, it is the great theophanies inspiring Greek philosophy and Israelite and Christian revelation that produce the awareness that consciousness itself is the

site of the interpenetration, the identity and nonidentity, of the human and the divine. A novel feature of Voegelin's work is his emphasis upon the "revelatory" character of both Greek philosophy and Israelite and Christian revelation. The Platonic vision of the *Agathon*, the Mosaic encounter with the self-revealing God in the burning bush, and the Pauline vision of the resurrected on the road to Damascus, are equivalent in their theophanic character.

Summary. To summarize, Voegelin's scholarly achievement has been to (1) set forth new interpretations of ancient texts; (2) offer radically new interpretations of modern political thought in the light of his understanding of pre-modern (and non-Western) experiences of order and their symbolization; (3) revise significantly the conventional dichotomy of philosophical "reason" and Biblical "revelation"; and (4) articulate an original theory of experiences of reality and their symbolization. This latter theory maintains that nonmetric reality is not a "something" external to the consciousness, but has always been present to the unobstructed experience of the participating consciousness. This consciousness (of a concrete human being) expresses itself in symbols; the symbols of the various open, participating consciousnesses over space and time may be compared for their compactness and differentiation. Reality (as reality experienced) unfolds itself in time, and the "trail of symbols" is the substance of history.

What must not be forgotten, Voegelin insists, is that the experiences and symbols constitute a unit; the symbols (say in a Babylonian hymn or a Platonic dialogue) must not be detached from their motivating experiences and treated literally as "doctrines." Voegelin is emphatic in stressing that experiences of reality may be inferred from the symbols articulated to express them. Also, people in the present "reenact" the experiences suggested by the symbols, thereby "verifying" them by their own experiences of open participation in the process of reality. They may also "disprove," so to speak, the truth claims of the ideological doctrinaire by reenacting the "deformed" experiences of reality (say, of a Bakunin or a Marx) suggested by his language symbols.

Conclusion. Eric Voegelin has often baffled his interpreters, one of whom has characterized him as "radical and reactionary at once" (Altizer 1975). He may be described as "radical" because of his theory of experiences and their symbolization, for example, which appears to threaten

orthodox Biblical criticism. Alternatively, he can be called "reactionary" because of his asides (often embedded in philosophical writing) on certain contemporary political events and controversies. Voegelin may be seen as resembling Aleksandr Solzhenitsyn in that some of his specific judgments on American and international politics are unacceptable to many readers who nonetheless agree with the thrust of his philosophical teaching. However, the comparison with Solzhenitsyn, though partially accurate, fails to reflect Voegelin's thorough knowledge of American politics and society. Regardless of their different assessments of current practical political controversies, fair-minded observers acknowledge the intellectual power of Voegelin's over-all theory of political reality, and feel compelled critically to address themselves to the fundamental questions which he has raised again for our century.

DANTE GERMINO

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W

WARNER, W. LLOYD

William Lloyd Warner (1898–1970) is considered one of America's most eminent social anthropologists of the twentieth century. He was a brilliant researcher and a creative theoretician, who synthesized concepts from several sources in his novel interpretation of social systems. He won deep loyalties from some and critical rejection from others, and exerted an unheralded but significant influence on the course of events of several public issues.

A native Californian, he enlisted in the U.S. Army in 1917, while still a senior in high school. Afterwards he attended first the University of Southern California, and then the University of California at Berkeley, where he received his B.A. in anthropology in 1926. At Berkeley, Robert H. Lowie aroused his interest in the social practices of native peoples, and Alfred L. and Theodora Kroeber became lifelong friends. In 1926, he was introduced to the British school of social anthropology through the sequential visits to Berkeley of Bronislaw Malinowski and A. R. Radcliffe-Brown. The latter was his mentor from 1927 to 1929, when Warner went to Australia to study the Murngin people of northeastern Arnhem land.

Warner's professional career breaks into four periods. The first was his undergraduate and graduate schooling at Berkeley and the three years he spent in Australia. The Harvard phase began with his appointment as assistant professor of anthropology in 1929. There he became acquainted with Elton Mayo of the Harvard Business School, with whose help he

launched his research on Newburyport, Massachusetts, later to be known as the Yankee City study. He also began a major research project of a biracial community in Natchez, Mississippi, with Allison Davis and Burleigh Gardner (Davis, Gardner, & Gardner 1941), and a social anthropological study of County Clare, Ireland, later reported by Conrad M. Arensberg and Solon T. Kimball (1940). Through Elton Mayo, Warner and some of his students were involved in the intellectual ferment centered in the Harvard Business School. It was there that data from the Western Electric study were being analyzed; that L. J. Henderson held seminars on Vilfredo Pareto and scientific method, and described findings from the fatigue research; that Warner and his students reported on the results of their studies of the social structure of community; and that business students were being taught principles of industrial organization based on Malinowski's study of ritual exchange in the Trobriand Islands.

Warner's acceptance, in 1935, of a position at the University of Chicago initiated the third period in his career. Over the next quarter century, he both reaped the harvest of the productive years at Harvard and turned his interests in new directions. He published the results of his Murngin research in the now classic *A Black Civilization* (1937a) and the first of the Yankee City series (Warner et al. 1941–1959). Although Warner held a joint appointment in anthropology and sociology, his research interests actively linked him with the committee on human development. In conjunction with such like-minded associates at the University of Chicago

as Ralph Tyler, Allison Davis, Robert J. Havighurst, Everett Hughes, David Riesman, and Burleigh Gardner, he continued to study small communities, but extended his search to urban institutions. His explorations of a midwestern community appeared in *Democracy in Jonesville* (Warner et al. 1949).

As knowledge of Warner's researches into American society began to spread beyond academic circles, he found himself increasingly sought out to participate in programs of major foundations and some segments of government. For example, Warner was among those social scientists who worked with M. L. Wilson, director of the Federal Extension Service, to introduce social science concepts into extension education. With Robert Redfield he was invited to publish on anthropology and agriculture in the U.S. Department of Agriculture *Yearbook* (1940). When John Collier, in 1941, as Commissioner of Indian Affairs, contracted with the committee on human development to conduct a major study of Indian personality, education, and administration, Warner assumed the major responsibility. He planned research strategy, selected the psychological tests, and interpreted results. In another area, his description of the adverse effects of the middle-class school environment on learning among lower-class children, in conjunction with Havighurst and Martin B. Loeb, had considerable impact on educators (1944).

Warner's interest in the sociology of racial and ethnic groups continued to expand. He believed that research could be a powerful weapon in the elimination of civil and social inequities by exposing such false disabilities as those based on race. For example, his analysis and publication of the caste-class structure of southern race relations (Warner 1936; Warner & Davis 1939) should be understood both as scientific findings and as an instrument of human welfare. Years later his influence reappeared in Gunnar Myrdal's *An American Dilemma* (1944), a document that was powerful in stimulating the great civil rights surge of the 1960s. He was research director of the Works Progress Administration project that gathered the data for *Black Metropolis* (1945). Just before the outbreak of World War II, Warner, Gardner, and Havighurst formed the Committee on Human Relations in Industry, designed to help incorporate migrant black workers from the South into industry. From this effort came Social Research, Inc., a Chicago-based consulting

firm that Warner served as adviser until his death. His interest in the origin and characteristics of top executives in government and business as well as in industrial workers emerged after World War II. In effect, it was an extension of the theoretical interest in the relationship between social personality and social systems that he first developed in the Murngin study. Inevitably, his attention turned to the institutional settings of his subjects—corporations and government agencies—and his focus became large-scale organizations and the concomitant problem of their interconnections. The reconciliation of the existing theory of community with a societal system of large-scale enterprise became the central theoretical problem.

When in 1959 Warner accepted the position as university professor of social research at Michigan State University, he had already begun the research that engaged him in the fourth and final phase of his career. As a result of his earlier work, he came to interpret America as a society of large-scale organizations, and he concluded that large-scale organization was part of the process of an "Emergent American Society," a concept he used in the title to the Ford Foundation lectures he delivered at New York University in 1961. On the occasion of their publication (1962), he wrote in a personal communication to the author: "You will see that it's a development of much earlier ideas that have been greatly changed through time and taken a new departure."

Combining a group drawn from his Chicago days with colleagues at Michigan State, Warner studied a variety of organizational systems in terms of the concept of a continuously emergent society. *The Emergent American Society* (1967) represented the product of their efforts. A second volume, analyzing data from the agricultural sector and providing an overview of American society, was never completed.

Contributions. Warner's conceptual influence has been substantial. Some of his formulations are so deeply embedded in current thought that users are unaware of their origins. For example, his comparison of the differential functions of the family of procreation and the family of orientation has long been an accepted idiom in the literature on the family. His research plan for the bank wiring room study in the Western Electric Hawthorne plant, requested by Elton Mayo, the director of the project, was the first new approach to the study of work since the time-and-motion studies of industrial engineers.

It has continued to be the prevailing model for industrial studies. Warner also proved that methods used in studying a primitive tribe could be transferred to the study of a contemporary community. The results reported in the Yankee City series contain both methodological novelties and substantive insights. His postulation of an American social class system attracted wide attention. Some protested that such divisions were un-American, while others objected to the methodological procedures. Administrators of programs in government, industry, and institutions harbored no such objections in applying these new insights to their problems.

Warner believed, however, that the most difficult methodological problem in the Yankee City study was the invention of an analysis that would permit "seeing the society as a total system of interdependent, interrelated statuses." He believed that such an achievement would be possible only if the basic unit of analysis was the relationship rather than the individual: "If the basic unit of analysis is the relation rather than the individual, then it should be possible in a community research to connect any given relation with all others and thereby construct an interconnected, interdependent system of total interaction" (1941, p. 791). According to Warner this end was first achieved in the Yankee City study. The sequential steps as he described them included an initial ranking of all individuals by class; the determination of the total number of statuses as a function of membership of an individual in different structures; determination of the system of integrated relations by examining memberships; and determination of the amount of interconnection between statuses within a given social class. Such an analytical operation results in "a map representing the social system of a community." It does not, however, describe specific events or their effects on the lives of individuals (1941).

During the latter half of the 1930s, Warner made more explicit the relevance of his research to race relations, the diagnosis and treatment of mental illness, and education. Today, conventional wisdom has incorporated his many insights into the importance of cultural background as a variable in learning, psychic and physical health, and relations among people. He called attention to the caste-class nature of race relations (Warner 1936; Warner & Davis 1939), demonstrated the relationship between mental disorder and the social system (1937*b*), and described the differential response to edu-

cation based on social class (Warner & Redfield 1940). The spread and incorporation of such ideas, nevertheless, has proceeded slowly. Educators still often blame the child for learning failure; psychoanalysts continue to look for inner dynamics to explain erratic behavior; and many explain race differences as genetic. Still, change in perspective over the decades on these and other matters has been massive.

Warner's enthusiastic and cooperative personal style seems to have been a factor in his influence on both colleagues and students. As examples, his discussions with Julian H. Steward on the connection between environment, technology, and social system, with Talcott Parsons on the family, with August B. Hollingshead on education and social class, and with Robert Havighurst on the method of community study seem to be reflected in the later writings of these men. Warner conveyed an appreciation of disciplined research to his students and shared with them the intellectual excitement of discovering the transformation of empirical data.

An innovation of lesser importance, although more widely known, was the Index of Status Characteristics (ISC) and Evaluated Participation (EP). Through a formula that laymen could use to calculate their own social standing and that of others, Warner and his associates brought the measurement of social class within the grasp of the high school social studies teacher (Warner, Meeker, and Eells 1949).

Theoretical perspective. Although Warner's reputation as a social scientist derives primarily from his research and findings about community and institutions in the United States, his theoretical base was formulated and validated in his research on the Murngin of Australia (1937*a*). These aborigines offered Warner an initial opportunity to test the validity of ideas derived from Lowie, Malinowski, Durkheim, Simmel, and Radcliffe-Brown. The basic conceptual framework that Warner developed continued to furnish the intellectual grounding for all his subsequent research.

Warner's basic views on humanity resembled those of most anthropologists, who postulated encompassing universalities that transcended variations of time and place, and believed that humans inherit and transmit the specific adaptive learning of their species as both primates and *Homo sapiens*. In other respects, however, Warner differed markedly from the Boasian, historical, diffusionist school that had dominated American anthropology for a quarter cen-

tury. Developmental questions of either an evolutionary or a diffusionist sort never interested him. Rather he was concerned with the functioning of whole communities as ongoing systems. From the conventional division of culture into the three segments of technology, social organization, and religion, he formulated a functional paradigm of interconnected subsystems of a society. Through technology, humans converted their physical environment to meet creature needs and provide comforts. Social organization ordered the relations among humans in groups and provided the moral order that sanctioned both social and technological behavior. Finally, there were those social logics, rational and nonrational, and the symbols of their expression, by which humans interpreted and defined their relationship to each other and to the uncertainties of the universe. This schema, in differing contexts and degrees of elaboration, was presented on four occasions. It appeared in *A Black Civilization* (1937a, pp. 443–450); in the first and fifth volumes of the Yankee City series (1941–1959, vol. 1, pp. 3–37, vol. 5, pp. 447–506); and finally in *The Emergent American Society* (Warner et al. 1967, pp. 10–21). Variations in the presentations are largely in the degree of elaboration and the context of explanation. In these discussions, Warner held that methods of research in anthropology could not be the criteria for distinguishing so-called primitive and civilized societies if a comparative sociology was to be achieved. Warner held that his schema and approach to research applied as readily to a modern society as to a nonliterate one. As he phrased it, “the simple Australian band and the vast metropolis in the United States are but two extreme varieties of the local community” (1941, p. 786).

The Murngin experience validated four additional concepts that Warner used in his later research. These included emphasis upon a synchronic and cross-sectional study of a society; the structural positioning of individuals according to status; and social personality as a function of participation. He also expressed the belief that a dominant structure connected with a pervasive theme could be found in each society. Among the Murngin, for example, the dominant structure was kinship based. An individual's status in a kinship system could explain to others his behavioral privileges and obligations. The theme was one of cyclical renewal of the cosmic order through ceremonies celebrating totems and death. The mythical

past and a recurring future were always a function of the behavioral present; hence social reality was always synchronous. When this perspective determined the focus of field work and data interpretation in Yankee City, social class was seen as the significant organizing structure, and social mobility, or as it was phrased years later by the Eisenhower Commission on National Goals, self-fulfillment, was the dominant theme.

Those who were historically minded were more than a little disturbed by Warner's findings, and any assessment of his contributions to social science, especially the Yankee City series, must include the conflicting responses that some of his writings precipitated. Indictments by both sociologists and historians have been particularly detailed and severe. Comments by sociologist C. Wright Mills (1942) and historian Stephan Thernstrom (1964) may be considered representative of the negative responses from these two disciplines. In his review of the first volume of the Yankee City series, Mills identified the research as a study in stratification and then listed a series of deficiencies in theory, definition, and design. The sociological criteria that he used as the basis of evaluation suggest that he was unacquainted with the functional approach to the study of community.

Thernstrom was even more precise in his bill of particulars, castigating both Warner's approach and findings. He accused Warner of an “unwillingness to consult the historical record.” Such ahistoricity produced flaws in the conceptualization of ethnicity and social class, and, he asserted, Warner also failed to measure mobility.

Actually, Mills's and Thernstrom's comments express the methodological and conceptual systems of their authors. Unfortunately, criteria derived from one discipline are seldom applicable in cross-disciplinary evaluation. Darwin's great insight that the observer sees only that for which he has been trained has as its important and relevant corollary that research results are a function of the questions asked and the operations performed—a proviso ignored by these and other critics. A more serious deficiency, however, has been their failure to grasp the interconnectedness of the conceptual procedures that Warner used in organizing and interpreting his data. An appropriate basis for criticizing Warner's research would be an evaluational approach based on criteria that mea-

sured the comparative effectiveness of diverse social science presentations as descriptions of the dynamics of social systems. One advantage of the Warnerian approach was that it was, in fact, applicable to such diverse societies as the Murngin, Yankee City, the Deep South, Ireland, Jonesville, or an emergent national community.

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WECHSLER, DAVID

David Wechsler was born to Moses and Leah (Pascal) Wechsler in Lespedi, Romania, on January 12, 1896, the last of seven children. He married Ruth Ann Halpern on July 21, 1939, and they have two children. Alfred Binet, with whom Wechsler much later would be ranked, was already 39 years of age in 1896 and had that year embarked on a program of research which, in 1905 and after many failures, would provide mankind with its first test of measured intelligence, the Binet-Simon Scale. Wechsler's father had been a scholar who emigrated to New York City with his family when Wechsler was six. The latter completed his primary and secondary education in New York City, following which he graduated from the College of the City of New York with an A.B. degree (1916), and from Columbia University with an M.A. degree (1917) upon completion of a thesis under Robert S. Woodworth. His other teachers included James McKeen Cattell, Edward L. Thorndike, and Thomas Hunt Morgan.

Wechsler's training in psychology was immediately called upon with the entry of the United States into World War I. While awaiting induction, Wechsler went to Camp Yaphank on Long Island where, under the direction of Edwin

G. Boring, he helped to score and evaluate the performances of several thousand recruits on the newly developed individual Army Alpha Test. After induction and basic training at the School of Military Psychology at Camp Greenleaf, Georgia, Wechsler was assigned to the psychology unit at Fort Logan, Texas. There his duties consisted largely in assessing recruits with the Stanford-Binet Intelligence Scale, the Yerkes Point Scale, and the Army Individual Performance Scales. While trying to evaluate the military fitness of subjects who repeatedly failed on standardized tests, but who nevertheless gave histories of adequate work performance and adjustment in civilian life, he first became aware of the need for both (1) a broader concept of intelligence than those then in vogue and (2) a test of measured intelligence better suited to adults, illiterate as well as literate.

As to the first issue, he became increasingly convinced over the years that the historical practice, which was espoused by Thorndike, C. E. Spearman, Lewis M. Terman, and Henry H. Goddard, of defining (and consequently of measuring) intelligence solely in terms of intellectual ability needed modification. Intelligence, Wechsler concluded, could not be separated from the rest of the personality. This view, one of his most important contributions, eventually found expression in his definition of intelligence (1939*b*, pp. 3-12) as a *global* and not a unique capacity, which involved affective and conative as well as cognitive components. But this idea, although developing even in his early years, was not fully articulated until his publication of the Bellevue-Wechsler Scale (1939*a*) and his book, *The Measurement and Appraisal of Adult Intelligence* (1939*b*), and then developed more fully in his 1941, 1944, and 1958 revisions. The second issue also was addressed in this 1939 Bellevue-Wechsler Scale and book with the inclusion in that scale of five nonverbal, performance subtests better suited to the assessment of New York City's foreign and illiterate adult populations than was the predominantly verbal Stanford-Binet, which had been standardized primarily on children. It would, however, be twenty years from the time of his induction in World War I to the publication of his first Bellevue-Wechsler Scale before these two ideas would be fully developed.

In the intervening time there was a long period of diverse involvements and varied pursuits. The first, while he was still in uniform in France, was his assignment (1919) as army

student to the University of London, which offered Wechsler the unusual opportunity of studying and working with Spearman and Karl H. Pearson. Both had great influence on his later thinking—Spearman by his concept of intelligence and Pearson by his innovative correlational methods. In the environment he encountered there, Wechsler was quickly won over to Spearman's "g" factor, a general factor common to every intellectual ability and one directly opposed to the competing argument of Thorndike and others, who argued that intelligence also consists of specific factors. Years later, reacting to the studies of Truman L. Kelley and L. L. Thurstone, as well as the impact of his own clinical observations, Wechsler largely, though reluctantly, abandoned Spearman's unique (by then bifactor) constitution-genetic theory of intelligence. Wechsler later explained his reasoning: "I look upon intelligence as an effect rather than a cause, that is, the resultant of many interacting abilities or factors" ([1939*b*] 1958, pp. vii–viii). Viewing measured intelligence as an effect instead of as a cause, as had Spearman, Terman, and others, allowed Wechsler to introduce the idea that later personality development, and thus not only genetic constitution, could influence measured intelligence. Although he would be a lifelong proponent of the strong role of inheritance in determining intelligence quotients (IQ's), Wechsler was one of the earliest clinicians (1926; 1939*b*) to posit the strong influence also of personality, educational, emotional, and sociocultural factors as influences on IQ's.

Soon after receiving his discharge from the army (August 1919), he won a fellowship from the Society of American Fellowships in French Universities and spent the following two years (1920–1922) at the University of Paris, combining work under both Henri Pieron at the *École des Hautes Études* and Louis Lapique in the *Laboratoire de Psychologie* at the Sorbonne. During this period, Wechsler carried on the greater part of his researches on the psychogalvanic reflex, which formed the basis of his later Ph.D. dissertation, "The Measurement of Emotional Reactions: Researches on the Psychogalvanic Reflex" (1925*a*), also under Woodworth at Columbia University. It was during this period that he met Théodore Simon and Pierre Janet.

Wechsler returned to the United States from Paris in the spring of 1922, and soon after was offered the position of psychologist to New

York City's newly created Bureau of Child Guidance, which he held from 1922 to 1924. He spent the intervening summer working with H. L. Wells at the Psychopathic Hospital in Boston, and attending William Healy's and Augusta F. Bronner's conferences in the same city. This summer experience and clinical training in Boston enabled him to participate effectively in the new programs being developed at the bureau in New York City, and also was highly useful in his subsequent clinical position at Bellevue Hospital. While at the bureau he also was attending Columbia University part time and completing his Ph.D. degree, which was awarded in 1925.

Wechsler became chief psychologist at Bellevue Psychiatric Hospital in 1932, with a concurrent faculty appointment at New York University College of Medicine (1933). He held both positions for 35 years until he became emeritus in 1967. Between 1925 and 1932 he was engaged in private clinical practice and held a number of other positions, including a brief stint as acting secretary of the Psychological Corporation (1925–1927), which at the time was still under the aegis of one of his earlier teachers, Cattell. Like Binet, Wechsler managed to combine research with clinical work. This is reflected in the titles of his published papers during this period. The most important of these for Wechsler was "The Range of Human Capacities" (1930), a forerunner of his 1935 book of the same title. Interestingly, he still considers this book his major opus. In these books and articles he showed that the range of most human traits and abilities, including those pertaining to intellectual performance, was relatively small. Of particular historical significance are the conclusions regarding the growth and decline of ability with age which he had reached by 1935 and included in his book.

Beginning with 1934, Wechsler's creative efforts were largely directed to two more of his most important contributions—the development and standardization of the intelligence scales that bear his name, and the substitution of a deviation quotient (so important in evaluating the intelligence level of adults), which related each person's raw intelligence test score to his or her own age group as a reference, rather than to a mental age and an upper age limit of 15 years for adults, as had been done by Binet, Terman, and others. The immediate spur to these two contributions was the need of a suitable instrument for testing an increasingly multilingual and otherwise diverse adult population referred to

him for psychological examination at Bellevue; diverse not only as to facility in the use of English but also as regards national origin, socioeconomic level, and the wide age range of subjects. And so, finding the Stanford-Binet Scale (and Army Alpha and Beta Tests) not clinically suitable for such adult patients, Wechsler began to experiment with a large number of published individual intelligence tests that seemed better suited to use with these adults. By trial and error this effort would ultimately culminate in a single battery called the Bellevue-Wechsler Scale (1939a). Before deciding to develop this first omnibus scale, however, Wechsler the clinician had had to examine a number of the ideas that his academician mentors, Spearman at London and Thorndike at Columbia, were then so vigorously debating. Unlike these theorists who, by the very nature of their interests, examined large groups of more or less anonymous students and normal individuals, Wechsler's responsibilities at Bellevue brought him into contact with the single patient for whom the psychometric results would figure prominently in the total psychological assessment, and thus, subsequent psychiatric or legal proceedings.

This broad, clinical knowledge of the unique socioadaptive history of each such patient, who of necessity was intensively examined at Bellevue, gave Wechsler the insight that, by itself, the IQ score is a good, but far from perfect, index of an individual's total adaptive-behavioral success, and thus, that personality, motivation, drive, cultural opportunity, presence of psychopathology, etc., each could profoundly affect such an individual's IQ score. Wechsler believed that the ten widely differing verbal and performance subtests with which he constituted the 1939 Bellevue-Wechsler I could not only produce a full scale IQ, verbal IQ, and performance IQ, but by also permitting qualitative and quantitative analysis, could themselves, in terms of subtest patterning and scatter, provide a rich clinical insight into some aspects of the personality of the individual under examination.

Wechsler considered this rich clinical potential of his multifaceted verbal and performance Bellevue-Wechsler Scale so important that he continued using a comparable multitest battery (of 10 or 11 such subtests) in the development of his (1) 1942 Army Wechsler (Bellevue-Wechsler II) for use by the U.S. Army in World War II (1946); (2) *Wechsler Intelligence Scale for Children* (1949); (3) *Manual for the Wechsler*

Adult Intelligence Scale (1955); (4) *Wechsler Preschool and Primary Scale of Intelligence* (1967); and (5) *Wechsler Intelligence Scale for Children—Revised* (1974b). He is still continuing this approach in the (6) Wechsler Intelligence Scale for Adults—Revised, upon which he and the Psychological Corporation began development and standardization in the last half of the 1970s. The development of his ideas about the nature of intelligence, which provided the framework for these Wechsler scales, can be found in Wechsler's papers, many of which were reprinted in a volume of his collected works, which he and Allen J. Edwards compiled (1974a).

During a rich and varied professional career, many honors have come to Wechsler, including the following. From the American Psychological Association, he received (1) the award for the distinguished contribution to clinical psychology from the Division of Clinical Psychology (1960); (2) a special tribute from the Division of School Psychology (1973); and (3) the distinguished annual professional contribution gold medal award of the American Psychological Association itself (1973). He also received a special award for distinguished contributions from the American Association on Mental Deficiency (1972), as well as comparable tributes from universities and professional groups from around the world.

In his early eighties, he is still a much sought after speaker both in the United States and in many countries of the world.

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WEINREICH, URIEL

Uriel Weinreich (1926–1967) was a major contributor to the integration of empirical and theoretical linguistics in the middle decades of the twentieth century. His contributions united the functional perspective of European linguistics with the structural orientation of American linguistics and gave strong impetus to the incorporation of semantics into the main body of

linguistic theory. His work established the theoretical foundations for the study of multilingualism and language contact in general; it laid out the principles of a structural dialectology and demonstrated ways of incorporating structural, cultural, and social factors into the explanation of dialect differentiation; it introduced the principles of structural linguistics into Yiddish studies; and it was a moving force in the growth of sociolinguistic research and the study of linguistic change in progress.

Weinreich was born in Vilna in 1926, into a family strongly involved in the intellectual development of secular Yiddish culture. His father was Max Weinreich, a distinguished Yiddish philologist and cultural historian who, born in Latvia, spoke German as his native language.

Thus Uriel Weinreich grew up speaking standard Yiddish as his primary language, but at the same time he acquired a command of Hebrew, German, Polish, Byelorussian, and Russian. In his later development of the principles of language contact, he was able to draw upon a lifetime of experience in multilingual situations.

When the Russians invaded Poland in 1939, father and son were stranded in Copenhagen en route to the International Linguistic Conference, which was to have been held in Brussels that year. They, and eventually the two other members of the most immediate family, made their way to the United States.

Uriel Weinreich attended Columbia College as a Pulitzer fellow, then entered Columbia's graduate program in linguistics, which had recently been established by Roman Jakobson and André Martinet. A year after receiving his PH.D. in 1951, he joined the staff of the linguistics department at Columbia. He held the Atran chair of Yiddish language, literature, and culture and was chairman of the department from 1957 to 1965. Through his editorship of the journal *Word* (1953–1960), he brought to American linguistics a new sense of breadth and explanatory possibility. Until his early death in 1967, he continued to work at Columbia, where he developed research and teaching programs in semantics, dialectology, and Yiddish studies, instituted the Language and Cultural Atlas of Ashkenazic Jewry, to be published under the editorship of M. I. Herzog, and exerted a wide influence on theoretical and empirical linguistics in Europe and America.

Languages in Contact. Weinreich's first target of linguistic investigation (1949/1950) was the Romansh-speaking area of Switzerland, where

he developed many of his insights into language-contact phenomena through the close examination of Romansh–Shwyzertutsch bilingualism. The general principles and overview of the field, presented in his 1951 dissertation on “Research Problems in Bilingualism, With Special Reference to Switzerland,” were condensed into a volume entitled *Languages in Contact* (1953b). This book, translated into a number of European languages, continues to be the basic linguistic resource for the study of bilingual and multilingual communities.

Languages in Contact used the techniques of descriptive linguistics to lay out a taxonomy of ways in which one language can influence another: in phonology, morphology, and lexicon. But the entire discussion was firmly set in the larger sociocultural context. Weinreich stated and exemplified the ways in which one language influenced another when they were both controlled (and used) by a single speaker, but he never assumed that these internal factors accounted for such a large part of the total situation that they could have predictive power. The main thrust of Weinreich’s argument was to show the promise of a combined sociolinguistic and structural approach to the analysis of language contact. Throughout the book, he gave close attention to the psychological variables that determine proficiency in acquisition of a second language and switching between languages. Even more attention was given to the sociolinguistic aspect: almost half the work is devoted to an analysis of the factors operating in the bilingual community. Weinreich showed how language loyalty may operate to preserve a language in spite of many unfavorable demographic factors, and distinguished this generalized language loyalty from the purism practiced by the elite.

Weinreich’s use of the term “interference” has been criticized by those who approach every multilingual situation as a communicative system in itself. He would undoubtedly agree that the negative connotations of “interference” can be unfortunate, but his use of the term was designed to stress, rather than underplay, the systematic character of the resultant system. For him, “the term interference implies the rearrangement of patterns that result from the introduction of foreign elements into the more highly structured domains of language” (1953b, p. 1). He also laid out the fundamental research issues concerning the nature of coexistent sys-

tems—i.e., whether they are compounded, or exist as coordinate structures (p. 9)—and stressed the importance of dealing directly with these systems as they are used: “the data must be obtained, first and foremost, from the flowing speech of bilinguals in the natural setting of language contact” (p. 33).

Dialect geography. In the field of dialectology, Weinreich’s most influential paper (1954) raised the question, “Is a Structural Dialectology Possible?” His simple formalism for representing “diasystems” was widely utilized, but more often than not, other dialectologists failed to penetrate the deeper questions that Weinreich’s structural arguments were designed to raise. Although the paper is most often viewed as a call for a structural dialectology, Weinreich’s final statement was an appropriate and cautious answer to the question of his title: a structural dialectology promises “to be most fruitful if it is combined with ‘external’ dialectology without its own conceptual framework being abandoned” (1954, p. 400). His own dialectological papers were models of the way in which important theoretical conclusions could be drawn when structural issues were located in their historical and spatial context. The paper (1952a) that first established Weinreich’s reputation among American linguists dealt with the apparent contradiction that the distinction of hushing *vs.* hissing sibilants was apparently restored in northeastern Yiddish—in defiance of the fundamental structural principle that mergers cannot be reversed. Weinreich demonstrated that the reversal was in fact accomplished by a new wave of immigrants from a territory that had never lost the original distinction.

In *Languages in Contact*, Weinreich laid out the great advantages of eastern European Yiddish as a research site for the study of the influence of bilingualism on language development. Before World War II, Yiddish was a native language in a vast territory of Europe ranging from Alsace to the Ukraine. Although the language had originally been formed in German-speaking territory, one branch had matured in a Slavic environment, while others had developed in contact with Rumanian, Hungarian, Lithuanian, and Lettish communities. The European Ashkenazic (Yiddish-speaking) Jewish community (as distinguished from the Sephardic community) was virtually destroyed in the Holocaust, but Weinreich believed that its linguistic and cultural traditions could be recorded for

posterity and studied through interviews with survivors of the Holocaust. The Language and Culture Atlas of Ashkenazic Jewry was conceived on a grand scale, over a larger and more complex terrain than any earlier dialect atlas. Computational resources were used at an early stage of data collection, so that for the first time phonetic records were taken in machine-readable form. The early results showed the immediate benefits of this modernization of dialectological technique. From the preliminary results, Weinreich was able to make a number of searching analyses that demonstrated the explanatory benefits of combining historical and spatial evidence. In "Four Riddles in Bilingual Dialectology" (1964a) he showed how any attempt to account for Yiddish phenomena in terms of direct influence of coterritorial languages failed; but, tracing the successive path of Jewish migration from Germany to Poland, Byelorussia, and the Ukraine, he demonstrated the ways in which the approach he had first used to explain the restoration of the hissing-hushing distinction could be enlarged to account for an entire series of baffling problems.

Yiddish studies. Throughout his career, Weinreich's extensive contributions to the study of the Yiddish language, literature, and folklore demonstrated his loyalty to his native tongue. He wrote the basic textbook *College Yiddish* (1949), and edited three volumes of *The Field of Yiddish* (1954-1969). In 1959, with his wife, Beatrice Silverman Weinreich, he coauthored *Yiddish Language and Folklore: A Selective Bibliography for Research*, which has become a standard reference in these fields. He and his wife were also coeditors of *Yid[d]isher Folklor* (1954-1963). In his course on semantics and dialectology he brought a rich variety of Yiddish materials to bear on theoretical issues and inspired many students to work actively in the Yiddish language. A detailed analysis of "the syntax of the Yiddish adjective" to be published in the second volume of his collected works, demonstrated that the transformational relationship between the predicate and attributive adjectives appeared much more closely in Yiddish than in other Germanic languages. Weinreich's last Yiddish project, on which he worked from 1947 to 1967, was completed in his last years: the *Modern English-Yiddish, Yiddish-English Dictionary* (1968a). Here Weinreich turned his long-standing commitment to the study of descriptive semantics and lexicography to the ser-

vice of the Yiddish language and provided a model for dictionary makers who sought to unite semantic theory with lexicographic practice.

Semantics. Weinreich's early familiarity with the logical analysis of linguistic form, and his strong sense of social realism, made it inevitable that he would reject the position of American structuralism that semantic description lay outside of linguistics proper. Throughout the 1950s he developed this area of linguistics almost singlehandedly. The results of his work in this early period are summed up in the essay "On the Semantic Structure of Language" (1966, in Weinreich 1979). His realistic assessment of the state of the field sounded a note that ran throughout his writings: "There exists a fatal abyss between semantic theory and semantic description, an abyss which dooms the former to emptiness and the latter to atomization" (p. 155). Weinreich's own work represents the major bridge across this abyss.

In the 1966 paper, Weinreich developed Hans Reichenbach's use of the predicate calculus to describe the combinatorial semantics of language. His analysis repeatedly called for a clear separation of semantics from syntax, and a restriction of "meaning" to a narrower range of relations, rather than its expansion to include all possible significata. He reduced semantic relations to two fundamental types: *linking*, a grammatical conjunction of two signs that yields a product of their designata found in the subject-predicate, adjective-noun, and adverb-verb relations; and *nesting*, a nonadditive relationship characteristic of an even wider variety of linguistic functions, such as verb-direct object, determiner-noun, or tense-verb; the operand to an operator fills an open slot in a "harmonious but nonadditive way."

The most important universal proposed in this paper was that in "all languages a combination of signs takes the form of either linking or nesting. . . . no further patterns are introduced by transformations" (p. 134).

The formalisms that Weinreich proposed were intended to simplify the task of description but never to reduce the richness of semantic structure to a set of oversimplified examples. He gave much attention to the properties of idiomaticity and vagueness as essential components of semantic structure. His realistic grasp of semantic description was demonstrated in his conclusion that languages were universally less symmetrical and logical than they would be if

they used fully all the devices they contained. The challenge to the linguist was to explain this "limited sloppiness" in language: Why does man fail to make full use of the linguistic resources that he has?

Weinreich welcomed the advent of generative grammar as a "rejuvenation" of linguistics after the long senescence of American structuralism. He developed semantic analyses that coupled with the generative framework, even during the early period when semantics was ignored by others. His "Explorations in Semantic Theory" (1966) was initially a critique of the first efforts of generative writers to project a semantic theory, but it was expanded into a large-scale articulation of semantic and syntactic structure. The formalism itself was too tightly coupled with the then current *Aspects* model of Chomsky (1965) to survive, but many insights into the relations of syntax and semantics have influenced later work. Weinreich's "transfer features" were an effective way of dealing with the problem that expressions like "give me some pillow" cannot realistically be rejected as ungrammatical. In Weinreich's treatment the feature of "mass noun" or [-count] is transferred to "pillow" from "some," and a reinterpretation of "pillow" follows. Weinreich's concern with metaphor and idiomaticity followed naturally from his earlier attention to the "limited sloppiness" of language and to the theoretical importance of going beyond the special case of "humorless, prosaic, banal prose" (p. 399). His final work on "Problems in the Analysis of Idioms" (1969) plunged more deeply into the kinds of indirect and subtle calculations needed to select the idiomatic rather than the literal meaning of expressions.

Theory of language. Weinreich's most important works were produced in the last few years of his life, when he realized that he had only a limited time ahead of him. A cancerous tumor removed in 1963 recurred in the fall of 1966. In the months that remained, Weinreich set aside other concerns to work intensively on the final version of a paper on "Empirical Foundations for a Theory of Language Change" (Weinreich, Labov, & Herzog 1968). He restated the inadequacy of a linguistic theory that took as its object a homogeneous linguistic model, and called for a theory that would account for "the facts of orderly heterogeneity" (p. 100). Perhaps his most important contribution to our understanding of language was to show that such heterogeneity is not a defect, but that the

absence of such orderly differentiation would be. This fundamental sociolinguistic principle appears throughout Weinreich's work in multilingualism, dialectology, and semantics.

Influence. Weinreich's own intellectual debts are plainly acknowledged throughout his work. The chief formative influences upon him were Reichenbach, Jakobson, and Martinet; but, more generally, his ability to enter into the work of others and use it productively was phenomenal. As a teacher he demonstrated this capacity to submerge himself to an extraordinary degree, and his influence on his students was even stronger as a consequence. His dialectological work was continued directly by M. I. Herzog; the sociolinguistic impetus was developed by William Labov; work in semantics was continued directly by Karl Zimmer (1969) and Edward H. Bendix (1966) and less directly in the semantic work of James McCawley; research on language standardization was continued by Paul Wexler (1974); Weinreich's approach to Yiddish literature is exemplified in the work of Dan Miron (1973), among others. Weinreich's wider influence reached linguists and dialectologists in Europe, the Soviet Union, and Israel, as well as the United States.

WILLIAM LABOV

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WHITE, LESLIE ALVIN

Leslie A. White (1900–1975) was a leading figure in the intellectual transformation undergone by American anthropology during the mid-twentieth century. White's early views reflected the antitheoretical outlook that dominated anthropology in the 1920s, when he was trained, but he soon began to reject these views and to pursue such theoretical approaches as cultural evolutionism and culturology. By the late 1950s or early 1960s he had restored these approaches to scientific respectability and was generally recognized as the foremost theoretician in American ethnology.

From his early years, White was impelled by the desire to understand the behavior of "peoples." This quest led him to pursue history and political science at Louisiana State University, and then philosophy, sociology, and psychology at Columbia University, but without finding in them what he was seeking. After receiving his master's degree in psychology from Columbia in 1925, he transferred to the University of Chicago. Although he had taken his first course in anthropology from Alexander A. Goldenweiser at the New School for Social Research in New York, it was not until he enrolled at Chicago that he began formal training in this subject, studying under Edward Sapir and Fay-Cooper Cole.

White received his PH.D. in anthropology in 1927 after writing a dissertation based on field work in the southwestern United States. Despite his renown as a theoretician, White was deeply interested in field work and continued to carry it out for more than thirty years. Altogether, he wrote five ethnographic monographs on the Keresan-speaking Pueblos of Acoma (1932*a*), San Felipe (1932*b*), Santo Domingo (1935), Santa Ana (1942), and Sia (1962).

White's first teaching position was at the University of Buffalo (1927-1930). His proximity to the Seneca Indian reservation, where Lewis H. Morgan had studied the Iroquois, led him to read Morgan's work, which he had previously ignored because his teachers had considered it "obsolete." Reading Morgan awakened in him an interest in cultural evolution and convinced him of the validity of this approach, despite its continued rejection by Franz Boas and his students.

In 1930 White moved to the University of Michigan, where he continued to teach until his retirement in 1970, and where he built the anthropology department into one of the strongest in the world. He was an effective and stimulating teacher and attracted many students to his courses. However, his unorthodox views, especially his strong opposition to organized religion, brought him into conflict with the university community. Furthermore, the theoretical positions he was developing in the 1930s and 1940s were considered heretical by most anthropologists, and for many years he faced the almost solid opposition of his profession. This engendered in him a strongly polemical attitude, especially toward members of the Boas school. This attitude continued to characterize his writings even after most of the battles he was fighting had been won (1963, 1966).

It was not until the late 1950s and early 1960s that anthropology as a whole began to accept much of White's work and to accord him a place of honor in the profession. He received the Viking Fund medal in General Anthropology in 1959 and was elected president of the American Anthropological Association in 1964.

White's major contributions to anthropology (aside from his field work) can be considered under the following headings: the symbol; culturology; evolutionism; cultural determinism; and cultural materialism.

The symbol. In a classic paper, "The Symbol: The Origin and Basis of Human Behavior"

(1940*b*), which has been reprinted many times, White argued that it was the ability to bestow meaning on things in which meaning did not inhere that lay at the root of culture. He called this ability the symbolic faculty and considered it to be unique to the human species. Because of its unique basis, he held that culture constituted an autonomous class of phenomena. White devoted a great deal of attention to the concept of culture and repeatedly criticized definitions of it that he regarded as inadequate. He made several attempts to clarify the notion, most notably in an article entitled "The Concept of Culture" (1959*a*).

Culturology. An autonomous level of phenomena called for an independent science to study it, and White proposed to call this new science "culturology." (He later discovered that in naming it he had been anticipated by the German chemist and philosopher Wilhelm Ostwald.) The task of culturology, as White saw it, was to explain culture in terms of itself, without reference to psychology. Thus the origin of clans was not to be sought in the motives of individuals but in the interplay of other cultural elements, such as local exogamy, unilocal rules of postmarital residence, a sharp division of labor along sex lines, the accumulation of property, and the like.

Although White elaborated culturology far beyond what anyone had attempted before, it was by no means his own innovation. Its roots, as he himself said, could be traced back to A. L. Kroeber's (1917) notion of culture as "super-organic" and to Émile Durkheim's (1895) assertion that "social facts are things."

Evolutionism. As White began to cast off his antievolutionism and to delve into the writings of the classical evolutionists, he found much in the work of Edward B. Tylor, Herbert Spencer, and Lewis H. Morgan that struck him as sound and illuminating. He was especially taken with the writings of Morgan and over the years edited some of Morgan's journals and letters for publication (1937; 1940*a*; 1959) and brought out the definitive edition of *Ancient Society* (1964), for which he wrote a long introduction. At his death White was regarded as the leading Morgan scholar in the world.

White's earliest article on cultural evolution was "Energy and the Evolution of Culture" (1943), and its appearance marked the first major step in the resurgence of cultural evolutionism in the United States. In this paper White

set forth the theory that culture evolves as the amount of energy harnessed per capita per year is increased, a proposition that today is often referred to as "White's Law."

White went on to write a series of articles on evolutionism (1945*a*; 1945*b*; 1947*a*; 1947*b*; 1947*c*), taking issue with the antievolutionists, especially Robert H. Lowie. Lowie had argued that the facts of diffusion negated any theory of cultural evolution. White replied that Lowie had confused the culture history of peoples with the evolution of culture as a whole. Diffusion might explain how a particular society came to have a certain trait, but not how the trait arose in the first place. To account for this, White said, an evolutionary approach was required.

The 1940s were the high-water mark of cultural relativism in American social science, and many anthropologists held, with Ruth Benedict, that cultures were "incommensurable." Thus, rating cultures as higher or lower or assigning them to a particular stage of evolution was, they said, subjective and invalid. To this claim White replied (1947*a*) that the Boasians failed to distinguish between "subjective" and "arbitrary." All standards of measurement are arbitrary, he said, but they need not be subjective. Anthropologists could legitimately rank cultures in terms of such objective criteria as the degree of differentiation and specialization in their social institutions and the extent of their utilization of energy.

White's major work on cultural evolution was *The Evolution of Culture* (1959*b*). Although this book presented White's evolutionism clearly and vigorously, it did not appreciably extend his theoretical contributions to the subject beyond what he had already done in previous articles. The chief value of this volume, some have argued, is the lucid and convincing way in which it presents human societies as functioning systems.

Cultural determinism. White denied the existence of free will and saw human behavior as strictly determined. Going beyond that, he held that the most powerful determinants of this behavior are cultural. What an individual regards as his personal choice is more realistically seen by the anthropologist as the outcome of an interplay of cultural forces (1949*b*, chapter 7). So deeply ingrained in a person's psyche are these cultural forces, though, that ordinarily he fails to recognize their external source.

White was a staunch opponent of the great

man theory of history, arguing that the accident of being at the focal point of great cultural-historical forces could make a "genius" or a "hero" out of someone with only moderate natural endowments (1949*b*, chapter 8). He cited the many instances of simultaneous but independent inventions as evidence that great advances in culture can best be understood as the maturing of general trends in cultural development, rather than as the inspired acts of supremely gifted individuals.

These views and others, which White expressed trenchantly in his collection of essays, *The Science of Culture* (1949*b*), brought him into sharp conflict with many of his professional colleagues who took a less deterministic and more humanistic approach to the subject matter of their science.

Cultural materialism. Although this term was coined by Marvin Harris (1968), it was Leslie White who first espoused this approach in anthropology and who championed it most vigorously. While White cannot accurately be called a Marxist (he eschewed the dialectic, for one thing), he was certainly influenced by Marx and was, in Marxian terms, a historical, even if not a dialectical, materialist. White saw technology as constituting the most powerful set of determinants of cultural systems and went as far as to say that human societies could be thought of as social ways of operating technological systems (1949*a*). He thus accepted the Marxian view that the sociological and ideological components of a society can be thought of as a superstructure reared on a technological and economic base.

In applying his culturological approach to explanations of culture as a whole, White held that the effect of environment was averaged out and could thus be eliminated from the equation. However, he was not unmindful of the fact that environment, as one of the material conditions of existence, played a significant role in shaping particular cultures. Although his own work was not, strictly speaking, "ecological," his general point of view was nonetheless sympathetic to this approach. In fact, it was a decided stimulus to those of his students who, like Elman R. Service (1960) and Marshall D. Sahlins (1958), went on to contribute to the field of cultural ecology. Through another of his students, Lewis R. Binford (1972), who assimilated both his evolutionism and his cultural materialism, White was instrumental in giving

rise to what is generally termed the "new archeology" (Willey and Sabloff 1974).

Besides dealing with broad theoretical issues, White contributed to the solution of more specific anthropological problems. Thus, elaborating on a theory first proposed by E. B. Tylor, White (1948a) explained the origin of the incest taboo by saying that extending the network of kin ties, as would occur by marrying outside the nuclear family, conferred a distinct advantage in terms of survival on those societies that hit upon it. White (1939) was also the first to argue that the peculiar types of kinship systems known as Crow and Omaha were not as baffling as had been thought, but were a terminological expression of strong clan organization, in which the clan that a person belonged to sometimes overrode in importance which generation he belonged to.

White always regarded anthropology as a science and was deeply interested in the philosophy of science. In "Science is *Sciencing*" (1938), he argued that science was a unified approach to the study of nature that received different names (e.g., physics, biology, anthropology) as it came to be applied to different orders of phenomena. Moreover, the three major ways in which the phenomena of each science could be studied—historical, functional, and evolutionary—were all legitimate and complementary and in no way antagonistic to each other. This, he said, held just as much for anthropology as it did for geology, and he looked forward to the day when the fact would be recognized within his own profession (1945b).

In "The Expansion of the Scope of Science" (1947d), White proposed the first major theory of the filiation of the sciences since Comte and Spencer had broached the problem in the nineteenth century. White's theory was that those sciences developed first and matured earliest that dealt with phenomena furthest removed from the determinants of human behavior. Thus, astronomy, which dealt with the distant stars, was the first science to arise and find acceptance, while culturology, which studied the most intimate and subtle, but at the same time the strongest, determinants of human conduct, had been the last. Indeed, so disturbing to people's accustomed views of themselves and their behavior was this new science that when first put forward it was rejected and opposed. And the fact that White's struggle to win a place for culturology in the firmament of science is still by no means completed may be taken as a

measure of the accuracy of his perception of the evolution of the sciences.

ROBERT L. CARNEIRO

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WHITING, JOHN W. M.

John W. M. Whiting was born in 1908 in Chilmark, Massachusetts, on the island of Martha's Vineyard. After graduating from Yale College in 1931, Whiting experimented with divinity school and with secondary school teaching before deciding to pursue his PH.D. at Yale in sociology, a department then dominated by the anthropologically oriented tradition of William Graham Sumner and Albert G. Keller. He received his PH.D. in 1938, and his dissertation, based on field work in New Guinea, foreshadowed his professional identity as a student of child development and socialization, fusing perspectives from anthropology and psychology (1941).

From 1938 to 1947 (except for a period as a naval officer), Whiting was a researcher at Yale's Institute of Human Relations. His work furthered the institute's aim of advancing behavioral science by encouraging interdisciplinary research. During this period, he extended his background in anthropology by continuing association with his teacher George P. Murdock, his fellow student Clellan S. Ford, and Bronislaw Malinowski during his years at Yale. He also received training in psychoanalysis from Earl Zinn and John Dollard and in the behavioristic learning theory then being developed by Clark L. Hull and Neal E. Miller. Expanding his mastery of method, he helped plan the analysis of culture ultimately used by the Human Relations Area Files, collaborated with Irvin L. Child in correlational studies of human responses to frustration and goal attainment and of features of human cultures, and with O. Hobart Mowrer conducted an experiment on animal behavior. Despite great diversity of method, these activities had a common aim of testing ideas relevant to Whiting's varied theoretical background.

As a researcher at the Iowa Child Welfare Research Station from 1947 to 1949, Whiting collaborated with Robert R. Sears and Vincent Nowlis on studies in child development. He spent the rest of his academic career at Harvard University, at first primarily in the Graduate School of Education's Laboratory of Human Development (of which he was director from 1953 to

1963), and later primarily in the department of social relations and of anthropology. From 1966 to 1973, as director of the Child Development Research Unit of the University of Nairobi, he supervised research on family life in various groups in Kenya. His style of work continued to be marked by collaboration with his colleagues, with his graduate students, and especially with his wife, the anthropologist Beatrice B. Whiting.

The main body of Whiting's work, beginning with *Child Training and Personality* (Whiting & Child 1953) has explored the several links in the following chain: society's environment and history shape its maintenance systems; these systems influence the conditions under which the infant and child develop; personal characteristics thus established, though modified by later pressures, have a constraining influence on the projective-expressive systems of the culture. The projective-expressive systems comprise those customs that, because not strongly determined by sheer survival needs, facilitate symbolic expression of personal motives, or encourage projection of personal characteristics into the interpretation of the world. Never denying other or reverse influences on each of the variables in this chain, Whiting has sought to develop specific testable hypotheses about this particular sequence, and to bring to this aspect of psychological anthropology the possibilities of disconfirmation and of replication upon which broad scientific generalization depends.

Whiting and his associates have especially devoted themselves to the study of the childhood origins of the various self-control systems found in adults. In "Sorcery, Sin and the Superego" (1959), Whiting delineated three mechanisms of self-control: fear of other people (influenced by severe anxiety about aggression), fear of gods and ghosts (influenced by the individual's history of dependence and independence), and conscience (influenced by identification with parents). A series of further studies focused on identification and on male conflicts of sexual identity. A person tends to identify with those who have power over the psychological and material resources that he desires; *status envy* is the term Whiting applied to this motive for identification. In some societies the care of young children, and thus immediate power over resources they desire, is almost exclusively assigned to women. The feminine identification thus developed in boys, if not later replaced, supports such adult male behavior as *couvade* practices. Many societies whose early treatment

of boys favors feminine identification have developed ordeals and ceremonies initiating boys into manhood; where these customs are found, *couvade* is often absent. In societies less inclined toward early male identification with the mother, initiation customs are less likely to be developed. The facts and theories emerging from this research go far beyond the question of self-control mechanisms, and bear as well on broader aspects of culture, especially on relations between the sexes and on the practical functions of religious ceremony (Munroe, Munroe, & Whiting 1979).

Whiting's research touches directly on biology at two points: (1) In considering how maintenance systems influence socialization practices, he traces the importance of nutrition. If a group's standard diet is low in protein content, a long nursing period will help insure an infant's survival (Whiting 1964). (2) In his study of the effects of socialization practices on later characteristics, one surprising discovery was the substantial influence of infantile stress upon stature and presumably other aspects of physical growth (Landauer & Whiting 1979).

Despite his great originality in theory, Whiting has always continued to emphasize empirical testing. His writings on method pertain both to cross-cultural correlations with their ethnographic base and to interindividual correlations with their base in the systematic observation of individual behavior, and incorporate his own contributions to a variety of studies of both kinds.

IRVIN L. CHILD

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WINCH, ROBERT F.

Robert Francis Winch (1911-1977), American sociologist, made important contributions to the sociology and social psychology of mate selection, marriage, and the family, as well as to social scientific methodology. He was born, raised, and educated in the Midwest of the United States and held university appointments there throughout his career. Trained at the University of Chicago by Ernest W. Burgess, William F. Ogburn, Samuel A. Stouffer, L. L. Thurstone, and Robert E. L. Faris, Winch carefully built innovative theories of mate selection, the impact of family upon personality, and familial organization which are basic to current work in these areas.

Winch was born in Lakewood, Ohio. In 1935 he received his A.B. in English literature from Western Reserve University in Cleveland. He entered the sociology department of the University of Chicago in 1937, receiving his A.M. in 1939 and his Ph.D. in 1942. From 1942 to 1945 he served with the United States Navy, and in 1945 returned to the University of Chicago to carry out a year's research on a Social Science Research Council fellowship. In 1946, he joined the sociology department of Vanderbilt University, and taught there until 1948 when he moved to Northwestern University. There he continued to teach until his death. He became a professor of sociology at Northwestern in 1955

and served as department chairman from 1967 to 1970.

Winch is best known for his original theory of complementary needs in mate selection and for *The Modern Family* (1952), in which he presented a general theory of the family applied to an analysis of the family in the United States. These accomplishments are aspects of a career dedicated to the development of an understanding of how the behavior of individual human beings is affected by their social environments.

In his first published work, "Personality Characteristics of Engaged and Married Couples" (1941), Winch called for intensive case studies to examine more deeply the questions raised by the questionnaire findings of Lewis M. Terman's *Psychological Factors in Marital Happiness* (1938). In his second article, "The Relation Between Courtship Behavior and Attitudes Toward Parents Among College Men" (1943), Winch began to examine a question on which he would continue to focus for more than a decade: the importance of psychoanalytic theory for explaining behavior differences. Winch formally tested Flügel's (1921) hypothesis that an excessively strong attachment of child to parent inhibits the subsequent development of normal courtship behavior. Although his findings supported Flügel's hypothesis, Winch drew the careful conclusion that "the present study demonstrates the difficulty, but also the possibility, of dealing with categories of an introspective social psychology in such fashion that observation can be quantified and subjected to statistical analysis" (p. 174). Winch's attempts to clarify the contributions of psychoanalytic thought in order to explain psychosocial development continued through his examination of the Oedipus hypothesis (1951) and the first edition of *The Modern Family* (1952). After this point, however, Winch questioned his early stress on the links between early childhood experience and adult behavior and personality.

Beginning in 1954, Winch published a series of articles setting forth a theory of complementary needs in mate selection and presenting data in support of that theory. Drawing on Murray's (1938) work on emotional needs, he developed an introspective social psychological theory and submitted it to an empirical test. He viewed his theory as an operationalization of the concept of love, defining love as "the positive emotion experienced by one person . . . in an interpersonal relationship in which the second person . . . either (1) meets certain important needs

of the first, or (2) manifests or appears (to the first) to manifest personal attributes (beauty, skills, status) highly prized by the first, or both" ([1952] 1971, p. 483). According to the theory of complementary needs in mate selection, homogamy in social characteristics creates for each person a field of eligible spouse candidates; within the field of eligibles, mate selection proceeds on the basis of complementary needs. Rather than selecting a mate psychologically similar to oneself, a person tends to select a mate whose pattern of needs is complementary to one's own. Winch tested this theory in 1950 on a sample of 25 young married couples through a complicated research design involving two interviews (one measuring the intensity of the interviewee's needs, the second gauging the connection between salient relationships in the interviewee's life and psychosocial development), plus the application of a Thematic Apperception Test (TAT). Attempts to replicate Winch's study have not equalled his standard of methodological purity, and the theory has consequently been a focus of debate for students of mate selection for more than twenty years (see Winch 1967).

Throughout his career, Winch was deeply concerned with methodological issues. He used a wide variety of measurement strategies and analytic techniques in his work, often employing techniques that had previously been little used. When he encountered a particularly thorny problem in research he wrote about it. In a 1947 *American Sociological Review* article he explained how factor analysis could help social scientists analyze data sets parsimoniously; in 1953 he evaluated the way research on the family reflected the application of the scientific method (Winch et al. 1953, pp. 1-17); in 1956 he explained how qualitative techniques such as the TAT can complement quantitative measures (Winch & More); in 1967 (Winch & Anderson) and 1969 (Winch, Mueller, & Godiksen), he examined questionnaires and coding problems; and in 1969 (Winch & Campbell) commented on the utility of tests of statistical significance. He wrote on these problems of methodology because, as a scientist, he believed that the clarification of methodological issues is basic to the explication of substance.

In 1957, together with Linton C. Freeman, Winch published the first article that he considered sociological rather than strictly social-psychological, "Societal Complexity: An Empirical Test of a Typology of Societies." Over the next

twenty years he published more than twenty articles and books on the family as a social system and the determinants of its structure and functions. Topics included marital stability (Winch & Greer 1964), extended familism (Winch, Greer, & Blumberg 1967), and societal complexity and family organization (Winch & Blumberg 1972-1973; Winch 1978). These works represented an attempt to complement the introspective social psychology that had begun in an examination of psychoanalytic issues with an examination of the impact of the larger society on the family (a factor that retained his interest as an important determinant of individual personality).

Winch's focus on the sociology of the family did not prompt him to abandon his interest in personality development. In 1962 he published *Identification and Its Familial Determinants*, which examined the individual's acquisition of behavior in the context of the family. Winch clearly differentiated his concept, "identification," from the more psychological concept of "socialization." He wrote that "socialization emphasizes the behaviors acquired and does not stress the model(s) from whom acquired, whereas identification directs our attention both to the behaviors acquired and to the interpersonal feature of acquiring them" ([1952] 1971, p. 392). This emphasis on processes of identification and the impact of familial structure on that process enabled Winch to develop a theory of behavior acquisition and personality development that took larger social forces into account much more explicitly than had psychological-reinforcement learning theories like Skinner's (1938), or sociological exchange-learning theories such as those developed by Homans (1961). First expounded in *Identification and Its Familial Determinants*, this theory of behavior acquisition was further developed by Winch in *The Modern Family* and in *Familial Structure and Function as Influence* (Winch & Gordon 1974).

In addition to this scientific work, Winch made important contributions as an educator. For nearly thirty years he was continually reworking three texts, *The Modern Family* ([1952] 1971); *Selected Studies in Marriage and the Family* ([1953] 1974); and *Family Form and Social Setting* (Winch & Adler 1971). *The Modern Family* presents a theory of the family that is both structural-functional and developmental. In it Winch identified a "basic societal function" carried out by the family (replac-

ment) and five "derived functions" (control, nurturance; parental, position conferral, and emotional gratification) and related them to functions principally carried out by other institutions (economic, political, religious, and socializing-educational) throughout the life cycle. *The Modern Family* is complemented by *Selected Studies*, which presented for college students the best current works in the sociology of marriage and the family, and *Family Form and Social Setting*, which presented basic concepts of the sociology of the family to secondary school students. All of these texts are among the most widely used in their fields.

Winch had considerable impact on the sociology of the family through both his writings and his students. His patrician personal style and his high standards for science dispelled the doubts of students and colleagues alike of the "importance of their enterprise." Of his 58 publications, 21 were written in collaboration, most with students. His high standard for scientific work and his concern for seeking broad-ranging determinants of personality were communicated directly to his students and colleagues through this collaboration.

The hallmark of Winch's research and writing was his stress on methodological rigor, hypothesis testing, and theory building. Because of this his texts formed a basis for the integration and development of sociological theories of the family, and his research broke ground for new areas of inquiry in the fields of behavior acquisition, mate selection, and societal evolution.

LOUIS WOLF GOODMAN

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WITTFOGEL, KARL A.

Karl August Wittfogel was born in Woltersdorf, Germany, in 1896. He arrived in the United States in 1934, was naturalized in 1941, and settled down in New York City. He thus grew up and received most of his education in the declining years of Imperial Germany, and the rest during the short-lived Weimar Republic. He received his PH.D. from the University of Frankfurt in 1928. He imbibed the grand old German academic tradition as well as the radical politics of the 1920s, an unusual and not always happy combination. By the time Hitler came to power, Wittfogel was already well known as a prolific writer on political matters, an activist and the author of several plays. The best known, perhaps, was *Wer ist der Dümme?* (1923). His literary interests led to the initiation, in the 1920s, of a Marxist critique of literature. Wittfogel joined the Communist party in 1921 by way of his earlier membership in the radical wings of the German youth movement. During the 1920s he became more and more of a Marxist, but even an early work, *Geschichte der bürgerlichen Gesellschaft* (1924), indicated that he was going to be as controversial in the history of Marxism as he was later to be in that of the American social sciences.

Among the better-known teachers with whom Wittfogel studied were Wilhelm Wundt, Karl Lamprecht, Max Wertheimer, Georg Lasson, and Alfred Vierkandt. Richard Ehrenberg turned him in the direction of social sciences, but it was Carl Grünberg, the economic historian, who inspired him to work for a PH.D at the University of Frankfurt. Grünberg was the first director of the Institut für Sozialforschung, which was founded by Felix Weil in 1922. Grünberg made it clear that the research method of Marxism would determine the institute's scientific task. This view suited Wittfogel but not Max Horkheimer, who eventually turned the institute away from "social science" and ended Wittfogel's organic connection with what came to be known as the Horkheimer "Frankfort School."

A man of unusual intellect and passion, Wittfogel had wide ranging intellectual interests that were always broader than his politics or the fashions of the time. Political commitment might initiate a given intellectual pursuit, but his academic conscience, in the end, usually dominated. The spectacular rise of the Chinese

Communist party in the 1920s added a further incentive to his interest in China, but the Chinese revolution did not initiate his involvement with China. That was already there, and it continued to be a strong intellectual commitment.

Wittfogel had begun serious Sinological studies as early as 1921 under A. Conrady and Eduard Erkes and like many others was strongly influenced by Richard Wilhelm's translations of the Chinese classics. It was his conviction that an understanding of the phenomenon of China was essential to an understanding of world history and of mankind.

Wittfogel's study of China was the occasion rather than the cause of his conflict with Soviet bureaucracy and its views of Marxism. In his *Wirtschaft und Gesellschaft Chinas* (1931) Wittfogel expanded on Marx's suggestion that Asiatic societies had social and economic structures radically different from those of western Europe, a concept that Marx had never fully developed. In this book Wittfogel elaborated the hypothesis that China exemplified an Asiatic mode of production. The Leningrad conference of 1931 denounced the Asiatic mode of production, possibly because it was an hypothesis that implied that there could be a bureaucratic ruling class that controlled but did not own the major means of production. The Soviets were not interested in even the theoretical possibility that the elimination of strong private property and private ownership of the means of production could lead, not to socialism, but to centralized despotic power.

Far from accepting the Stalinist theoretical position, Wittfogel pursued his reexamination of Marxism with increased passion in China, where he traveled after his release from a Nazi concentration camp in 1934. Clearly he was an academic and a Marxist first and a communist second. His main significance from the academic point of view is that he took Marxism as a starting point, not as a sacred text, and applied it to non-Western societies. Almost single-handedly he tried to universalize what he considered to be the scientific method of Marxism. He challenged it to expand theoretically.

The development of the theoretical challenge went forward in two stages. First, Wittfogel presented his concept of an Asiatic mode of production in his 1938 article, "The Theory of Oriental Society." This statement brought him support for what was intended to be a massive analysis of Chinese economic and political his-

tory on the basis of the dynastic histories. Out of this effort came the "History of Chinese Society: Liao" (Wittfogel & Chia-shêng 1949), a study of a conquest dynasty that introduced such controversial new concepts as the dynastic crisis cycle and the bureaucracy as a ruling class.

In the second stage, Wittfogel's theoretical approach was decisively influenced during the 1940s by two concepts culled from his relentless examination of Marxist literature—Marx's suggestion that Russia was "semi-Asiatic" and Georgii Plekhanov's warning that if the Bolshevik revolution failed there might be an "Asiatic restoration"—a return to the old despotic order. The search for "Asiatic" features in old and contemporary Russia led to a comparative study of all oriental societies and thus to his *magnum opus*, *Oriental Despotism* (1957). This construct attempted to do for non-Western societies, including pre-Columbian America, what Marx and Engels had tried to do for Europe. It distinguished between the social structure of societies based on industrialization and those based on hydraulic agriculture. The impact on Marxists was as startling as it was on non-Marxist social scientists.

Wittfogel's contribution was first to Marxism, second to the application of Marxism to non-Western societies, and then to the social sciences as we know them. Wittfogel accepted Marx's concept that the mode of production is the key to society and history, that the mode of production is determined by the means of production, and that the means are conditioned both by nature and society. He systematized Marx's basic concepts (such as the materialist conceptions of history, property, and property relations) and organized them into a fully developed and coordinated system that outlined a universal history of the development of mankind. He pointed out that it is important to explain why so many non-Western societies did not develop (stagnation). He distinguished between the lateral and vertical development of societies. He had to go beyond Marx's concept of the mode of production (mainly economic) because in his Oriental society the relations of production are determined more by the state than by the economics of the mode of production. In *Oriental Despotism*, Wittfogel used his new concept of overwhelming bureaucratic power to analyze the Soviet industrialized despotism. This he saw not as an "Asiatic Restoration" or the perpetua-

tion of a basically Oriental society but rather as a new industrial apparatus society of a general (state) slavery.

Wittfogel was one of the very few creative Marxists of the twentieth century. In the 1960s, the long dormant debate on the Asiatic mode of production burst open again in Europe and led to translation and republication of many of Wittfogel's works. The impact on social science in America is harder to measure because the contemporary expansion of the disciplines into the non-Western world followed a different pattern. Wittfogel raised broad and significant questions, contributed valuable insights to nearly all the social science disciplines, and provided a challenge to which few could remain indifferent. Critics of Marxism as such think that Wittfogel's approach was too apocalyptic, unitary, and awesome, and that the method was stronger than the facts. Others have not separated the scholarship from the alleged political positions of the author. Most would agree that even if the macrohistorical and ecological patterns he established are not proven, many individual insights have been seminal for American social science.

GEORGE E. TAYLOR

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WRIGHT, QUINCY

Quincy Wright was born in Medford, Massachusetts, December 28, 1890. His father, Philip Green, a professor at Lombard College, taught mathematics, astronomy, the financial history of the United States, and English. His parents held frequent literary gatherings in their home and original poems, first presented at these meetings by Philip Green and one of his students, Carl A. Sandburg, were subsequently printed on the press Philip Green had designed and built in his basement.

These family influences, including that of a great grandfather, Elizur Wright, who is considered the father of scientific life insurance and who was a passionate reformer (opposing slavery and high tariffs), were important in shaping Quincy Wright's career. He chose political science rather than one of the hard sciences in part because he felt less qualified in mathematics than either of his two brothers. Yet Quincy's mathematical skills were sufficiently sophisticated for him to pioneer in their use in both his *Study of War* (1942) and *Study of International Relations* (1955b).

A second family influence may well have been the tradition of practical involvement in community affairs. Quincy Wright's activism was reflected in his work through professional organizations in fostering the development of international standards, through his popular-

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izing of these standards, and by his direct participation in such political activities as the Committee to Defend America by Aiding the Allies. Wright's effectiveness as an activist was enormously enhanced by the assistance of his able wife, Louise Leonard.

Closely related to this activism were Wright's normative concerns in international relations. They were best synthesized in his chapter on "International Ethics" in his *Study of International Relations*. For a multicultural and increasingly interdependent world of sovereign states, Wright proposed a package of four "isms": pragmatism (whatever works is good), relativism (the value of tolerance), liberalism (emphasizing individual liberty), and humanism (brotherly love). While these parameters were broad and flexible, none could be ignored in determining the ethical position on any international problem, although the weighting of each depended upon the particular situation.

Implicit in this ethical approach is a quality that is present in all of Wright's work—that of balance. He examined problems of international law in their political context. He interrelated international with constitutional law and politics, just as he insisted on the balance of four values in his international ethics, and tempered his idealism with realism. In consequence, his publications have a seminal quality and his analyses acquire a historical importance for their accurate perceptions.

Wright completed his PH.D. in 1915 at the University of Illinois under the guidance of James W. Garner, and spent the following year as a research fellow at the University of Pennsylvania. There, he prepared his dissertation for publication under the title *The Enforcement of International Law Through Municipal Law in the United States* (1916). A three-year appointment (1916–1919) to the teaching staff of Harvard University brought him in close association with the distinguished international lawyer, George Grafton Wilson, and for the next four years, Wright was a member of the political science faculty at the University of Minnesota.

In 1921, he wrote *The Control of American Foreign Relations* (1922), in which he broadened the scope of his dissertation to include the larger problem of maintaining a viable democracy that would foster stable international relations. He recognized the need for enlightened executive leadership in foreign policy supported by Congress and public opin-

ion. The public, however, which is usually uninformed in foreign affairs, must never be allowed to lead, but must be educated. The dilemma of rigid and parochial national policies and publicly unacceptable international policies can therefore be resolved.

The national recognition thus accorded Wright led to his appointment in 1923 to the department of political science at the University of Chicago, where Charles E. Merriam was assembling a faculty that was destined to revolutionize the profession with its emphasis on quantification. At the University of Chicago Wright continued to expand his encyclopedic approach to international relations. As the first director of the Norman Wait Harris Foundation, he assembled annual conferences of leading experts and edited their findings. He eagerly sought out the insights of authorities outside the discipline, such as William Fielding Ogburn in sociology, Jacob Viner in economics, and his brother Sewall, who joined the Chicago faculty in 1926, in mathematics. In 1931, Wright's increasingly interdisciplinary approach to international relations culminated in his founding the first cross-departmental, degree-granting academic body—the Committee on International Relations.

Wright published his second major book, *Mandates Under the League of Nations*, in 1930. He had undertaken the research for the book as the first recipient of the Guggenheim fellowship. While he again employed the traditional legal-institutional analytical approach, the book nevertheless was judged the best in its field by such knowledgeable experts as William E. Rappard. The author viewed the mandate system as an intermediate stage between colonial government and sovereign states, in which the interests of the mandated area were substituted for those of the imperial country. Wright's restatement of this thesis in his chapter on colonial government in the *Study of International Relations* provoked the sharp disagreement of Harvard's specialist, Rupert Emerson, although the evolution of world affairs since the publication of that book has substantiated Wright's judgment.

The quantity and quality of Wright's publications remained dazzling throughout his life. (His bibliography, compiled by his wife in 1974, lists 23 books, 169 chapters or introductions of books, 424 journal articles, 332 book reviews, 143 encyclopedia articles, and 56 pub-

lished radio broadcasts. Two-thirds of his articles and book reviews appeared in legal and international journals, but he also wrote for the professional reviews of the national historical, sociological, economic, and political science associations.) His social science insights contributed a welcome flexibility to the often rigid interpretations of international law that characterized the 1920s and 1930s, and his knowledge of the law kept it from capitulating to power politics in the 1950s and 1960s. Wright was primarily interested in the problems of the real world; his theories were developed to analyze and suggest solutions for these problems. He was never tempted to develop abstract theories; for Wright, theory was a tool, not an end in itself.

Of all his publications, many consider Wright's *Study of War*, first published in 1942, his magnum opus. Karl W. Deutsch of Harvard University claimed in his introduction to the second edition (1965) that the work "marks the beginning of much that nowadays has become known as 'peace research'" in the way that Hugo Grotius' *On the Law of War and Peace* (1625) provided the basis for the study of international law. Not only did Wright gather a body of relevant facts, insights, and far-ranging questions about war, but the book pioneered different methods of research and drew upon vast scholarship in numerous disciplines. Its conceptual base had been elaborated in Wright's 1925 Geneva lectures entitled "The Causes of War and the Conditions of Peace," and his research on the subject continued throughout the 1930s with the assistance of graduate students and colleagues.

Wright added a chapter on World War II and the postwar experience in his second edition. He commented that people were growing more insecure, frustrated, and pessimistic about the future for themselves and their nations as they witnessed the wasting of resources for nuclear war. Yet Wright observed in the 1964 abridgment of the book that even "realists" in international politics were now admitting that nuclear war was not a rational instrument of policy, that lesser wars were likely to escalate, and that a policy can only be realistic if it can be achieved by peaceful means. It is relevant to note here that Wright was quick to recognize the importance of Lewis F. Richardson's unpublished manuscript on the mathematical study of war, and he collaborated in editing Richardson's *Statistics of Deadly Quarrels* (1960).

Wright's introduction to the Richardson book notes the many similarities and occasional differences between his conclusions and those of Richardson.

The *Study of International Relations*, Wright's fourth major work, was comparable in its encyclopedic dimensions to the *Study of War*. Although Karl Deutsch described it as the best and most comprehensive work in this field, it did not receive the same acclaim as the *Study of War* because the cold war climate of the mid-1950s preoccupied scholars with research on military and strategic affairs. It provides a masterful ordering of the meaning and objectives of international relations and a specific consideration of ten "practical" analyses (e.g. international politics) and six "theoretical" analyses (e.g. political geography). In the final chapters Wright introduced the field conception as an ordering idea in international relations. He related the field theory to earlier ordering conceptions (the world as plan, equilibrium, organization, and community), and demonstrated how the field theory incorporated all the others. He not only suggested which research methodologies might be expected to develop applications of the theory, but provided some specific applications as well.

Wright's public service activities multiplied during World War II. He had long been interested in the outlawing-of-war movement, having counseled the Chicago attorney, S. O. Levinson, whose efforts had culminated in the 1928 Kellogg-Briand Pact. This treaty provided Wright with the legal justification for the otherwise unneutral 1941 Lend-Lease Act, and during the war years he became a full time consultant for the lend-lease program. An insightful article (1945) on war criminals prompted his appointment in 1945 as advisor to Francis Biddle, a member of the war crimes tribunal, and he subsequently counseled the U.S. High Commissioner for Germany.

In the postwar years, Wright received many academic honors. He was president of the American Association of University Professors from 1944 to 1946. He was elected president of the American Political Science Association in 1949 and of the newly founded International Political Science Association the following year. He became president of the UN Association of Greater Chicago in 1953 and of the American Society of International Law in 1955. The American Council of Learned Societies voted him the award for outstanding achievement in 1961,

and in 1970 scholars from many countries nominated him for the Nobel Prize for peace. Wright's death on October 17, 1970, precluded him from consideration for this honor.

MARTIN B. TRAVIS

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WRIGHT, SEWALL

Sewall Wright was born December 21, 1889. A man of unusual physical and intellectual vigor, he has authored more than 200 scientific articles and completed his major work, *Evolution and the Genetics of Populations* (1968-1978), in 1978 when he was 88 years old. His major contributions have been to genetics, evolution, and biometry, and he is uniformly regarded as one of the foremost geneticists of his time. As early as 1920 he had developed the statistical method of *path analysis* which has become part of the standard methodology for quantitative animal and plant breeding. He also applied the method to economic analysis of corn and hog prices, but it was largely ignored by social scientists until the 1960s. As causal analysis has become more important in economics, sociology, political science, and psychology, Wright's methods have become increasingly popular, a half century after their invention.

His father, Philip Green Wright, taught for years at a tiny Universalist school, Lombard College in Galesburg, Ohio. Although he was an economist by training, he taught everything from mathematics to physical education. Later, in 1928, he wrote a book *The Tariff on Animal and Vegetable Oils*, to which Sewall contributed an appendix. As a child Sewall Wright demonstrated his arithmetic precocity by knowing how to extract cube roots before he entered the first grade. He had two gifted brothers, Quincy, who became a leading student of international law, and Theodore, later Civil Aeronautics administrator. The three boys operated their father's printing press in their house and, among other things, printed Carl Sandburg's first book of poetry, long before he became known.

Sewall Wright's interest in genetics was kindled by Wilhelmina Key, a biology teacher at Lombard College, who later became active in the eugenics movement. After graduating from Lombard and spending a year at the University of Illinois, where he received a master's degree, he became a graduate student at Harvard University and an assistant to William E. Castle, who was involved in early research on the genetics of rats. In 1915 he received the sc.d.

degree, based on an extensive study of coat color inheritance in guinea pigs.

From 1915 to 1925 Wright was senior animal husbandman at the U.S. Department of Agriculture in Washington. In this position his major project was the clarification of the roles of inbreeding and crossbreeding in the improvement of livestock. He continued an extensive experiment, already started, involving 23 lines of guinea pigs, each maintained by brother-sister mating. The decline of health, vigor, and fertility, the large differences between different inbred lines, the homogeneity within lines, and the immediate recovery of vigor on crossing between lines were all explained, in quantitative detail, as deducible consequences of Mendelian inheritance.

Out of these studies also grew methods of biometrical analysis that are now standard procedures in livestock and plant breeding. Application of these methods to livestock breeding required the measurement of inbreeding and relationship in complex pedigrees. His *coefficient of inbreeding*, now universally used, is a standard part of elementary genetics courses.

Wright used this to analyze the history of Shorthorn cattle, a breed for which records were available back to the foundation of the breed. He noticed that substantial improvement of breeds appeared to come, not only from selection within herds, but also by more or less random changes leading to an occasional outstanding herd, from which bulls were exported to upgrade other herds. While still working for the Department of Agriculture, he began to apply this observation to the more general problem of evolution by natural selection.

In 1925 Wright moved to the more academic environment of the University of Chicago and stayed there until 1955, when he moved to the University of Wisconsin. He has been professor emeritus at Wisconsin since 1960. Although he continued his research on physiological and developmental genetics of guinea pigs while at Chicago, the work for which he is best known is in evolutionary theory. The science that is now called population genetics was founded by three men, R. A. Fisher, J. B. S. Haldane, and Sewall Wright. The theory is essentially deductive. Starting with the rules of Mendelian inheritance, they worked out in great detail the consequences of selection of many kinds, of the mating system, of the mutation rate, and of the effects of random variables on gene fre-

quency change, this being regarded by all three as the elementary process of evolution.

Wright's unique contribution to evolution by natural selection is his "shifting balance" theory, which developed directly from his studies of livestock improvement, and from his observation that each gene in guinea pigs affects several characteristics and that the genes interact in ways that are often unpredictable. He viewed evolution as moving from one harmonious combination of gene frequencies to another that is even more harmonious and better adapted to the environment. The difficulty is that it is often impossible to move from one superior gene combination to another without passing through combinations that are worse than either. To cross this fitness "valley" between two "peaks" requires some process other than deterministic natural selection. Wright visualized this as happening, at least partially, by the influence of random factors. If one local population, like a strain of livestock, happens to develop a superior combination of genes, then this local population will grow faster, send out migrants, and eventually change the whole species. In this way, by a combination of random gene frequency drift in a local population, selection between such local populations, and migration from the best adapted, evolution can proceed from one harmonious gene combination to another. In Wright's view, local random gene frequency fluctuations, rather than being troublesome noise in the process, are a part of evolutionary creativity. Such a process will be facilitated if the population has a geographical structure or, for some other reason, mating pairs come from a restricted neighborhood. This view has led to much research on population structure as a factor in evolution.

Wright's theory has not met with complete acceptance. In particular it was opposed by Fisher, who thought it unlikely that any population would be located on a fitness peak such that it could not be improved by any of the enormous number of possible gene frequency or environmental changes that could occur. He thought that natural selection operates most efficiently in a large, more or less unstructured population where each Mendelian factor can be tested with the greatest statistical efficiency in the largest number of combinations. The question of which population structure is most conducive to evolutionary advance is still not settled. But, in the mid-1970s data began to

appear suggesting that those species, particularly among the mammals, where evolution has been most rapid have been those where there was independent evidence for considerable random differentiation, as the Wright theory would predict.

Unlike most biologists, Wright also has a deep interest in philosophy. His philosophy of organism is somewhat in the tradition of Leibnitz and asserts that, there being no mystical "emergence" of new properties with complexity, such properties as consciousness must necessarily reside in the most elementary particles.

As mentioned earlier, Wright's main contribution to the social sciences is the method of path analysis, used for genetic research from the 1920s but not in the social sciences until much later. The people primarily responsible for its current popularity are Hubert M. Blalock, Jr., Otis Dudley Duncan, and Arthur Goldberger.

The traditional method for analyzing data that are subject to uncertainty and errors of measurement is regression analysis, which leads to equations that give the best prediction of the values of the dependent variables from measurements on the independent variables. Path analysis differs from this in that its purpose is not prediction *per se*, but is rather an attempt to estimate the relative influence of different causal paths. In distinction to factor analysis, which employs similar mathematics, it assumes that the causal factors and their chains of influence are known. One of the simplest, but most useful aspects of the method is Wright's device of diagramming causal sequences so that paths of direct causation are indicated by directional arrows and correlations between unanalyzed anterior causes by two-headed arrows. Associated with each causal step is a *path coefficient*, which is a partial regression coefficient standardized by being measured in units of the standard deviation. These are the quantities to be estimated from the data. With such standardization there are simple rules whereby one can easily write the appropriate equations directly from a path diagram. The path coefficients then represent the relative influence of the corresponding path. If desired, the standardized partial regression equations can be converted into ordinary partial regression coefficients measured in concrete units.

One of Wright's early applications of path analysis was an attempt to measure the relative importance of heredity and environment in the

determination of intelligence quotient (IQ) scores. This pioneering study, published in 1931, utilized some comparisons by Barbara Burks of children reared by their own parents with those reared by foster parents. Wright concluded that heredity played a large role, but he was concerned to point out the uncertainties of both the measurements and the necessary assumptions.

As economics, sociology, political science, and psychology have become more concerned with causal models, path analysis has come into increasing prominence. In Wright's original formulation no distinction is made between population parameters and sample estimates, nor is any systematic or optimal method given to use when the number of equations exceeds the number of estimated parameters (that is, when the system is overidentified). With modern statistical methods these difficulties can be surmounted, and high speed computers have made possible the solution of large numbers of simultaneous equations generated by complex models. Yet, for all that, Wright's 1925 paper on corn and hog correlations is remarkably complete in its analysis, and many decades ahead of its time.

JAMES F. CROW

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