Science and Technology Studies: From Controversies to Post-Humanist Social Theory[1]

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Science and Technology Studies (STS) names a heterogeneous body of research, scholars, journals, professional associations, and academic programs that focus on the history, social organization and culture of science and technology. Begun in the 1960s in response to the recognizable growth in science in the contemporary world and to the educational and economic policy implications of this explosion of scientific research and development, STS also responded to issues of public responsibility that seemed to be engendered by technological innovation. In the 1960s, the Vietnam War encouraged scientists to become politically active; in 1975, the Asilomar Conference on Recombinant DNA set a precedent in which scientists regulated the own community, established formal norms, and supported legally enforceable guidelines for research; and in the early 1980s, public recognition of the AIDS epidemic sparked rumors of the viruses' origin in laboratory mishaps. The burgeoning synergy of attention and concern in the late 20th century produced, by the 21st century, a continuous concatenation between science and public policy concerns. By the time STS first emerged as an interdisciplinary conversation, significant accounts of the work of scientists, the production of scientific knowledge, and the impact of technological innovation had been produced in each of the social sciences from their distinctive disciplinary perspectives. Across the diverse research traditions, however, there seemed to be a shared or received view of science as the work of great minds, usually male, discovering nature's hidden patterns and mechanisms. If the 'focused confluence" of research begged for integration (Edge 1995:3-24), STS scholarship not only integrated the existing scholarship but revised these conventional accounts of science.

Early Sources

The earliest roots of STS can be traced to the sociology of knowledge and the philosophy of science, representing opposing positions on the possibilities of transcendent, universal knowledge of nature. The theoretical materials of STS developed from debates between those seeking to establish secure empirical methods for understanding nature and the sociologists who insisted that our access to nature, as well as other minds, was inevitably

[1] We are very grateful for the thoughtful comments and suggestions by Douglas Goodman, Stefan Helmreich and David Kaiser that have saved us from many an embarrassing error. Any remaining inadequacies are our responsibility alone.

filtered through our collectively created forms of cognition and communication. If Comte (1896: Vol 1, p 2) taught that we must discover "invariable relations of succession and resemblance" in human society using the same scientific methods as we study the world of nature, Durkheim challenged the reductionism of science for understanding human society, insisting that we consider social phenomena *sui generis*, as things in the themselves. Although "social facts" are the consequences of human interaction, they are nonetheless "endowed with coercive power," constraining the possibilities of human action and agency (Durkheim 1950).

In the early decades of the twentieth century, Mannheim, building on Kant and Durkheim and enamored of Dilthey, Mannheim sought a sociology that would provide objective knowledge while also capturing 'authentic experience,' empathetic, lived experience of persons that was more than could be represented by simple calculable and external facts and statistics (Kaiser 1998). In his major work, *Ideology and Utopia* (1936), Mannheim developed the concept of ideology into a full blown theory of knowledge that became both the impetus for both American and European sociology of science. Mannheim extended the notion of ideology from mere interests, psychological, material or otherwise, to a more comprehensive world view, with embedded assumptions, perspectives, and lenses through which experience and information are inevitably apprehended and interpreted. Mannheim argued that all knowledge develops from particular, concrete situations which provide the constitutive presuppositions that ground all knowledge making and knowledge claims. Thus, sociology of knowledge must, according to Mannheim, analyze all knowledge claims to expose their ultimate presuppositions, as well as the social and historical situation from which they emerged. Mannheim was adamant, however, that he was not describing moral or epistemological relativism, which he believed was as ethically dangerous as the moral poverty of the natural sciences. Rather, Mannheim argued, that by identifying the particular social bases of perspectives, we can place knowledges in relation to each other, what he described as relationism, producing a new kind of objectivity. In America, Merton (1937, 1941, 1945, 1973) critiqued Mannheim's account of the sociology of knowledge for its failure to differentiate among different types of knowledge and for failing to recognize the uniqueness of science as a way of making knowledge. If Mannheim had devoted less attention to the natural sciences, it was because modes of understanding nature, he thought, had produced unprecedented danger: science had led to disproportionate development of human capacities without developing parallel capacity for understanding human action and the governance of people rather than things. If he project of analyzing the production of knowledge was incomplete, it would be taken up in the 1970s by the Edinburgh school of STS, known as the sociology of scientific knowledge, by subjecting science to Mannheim's sociological critique, which Merton had failed to do.[2] But we get ahead of ourselves.

Before the sociology of science and knowledge is taken up in STS of the 1970s, it was resisted at home, in Europe, by scientists and by philosophers. The logical positivists of the 1920s Vienna Circle were part of a continuing effort to secure for empirical science epistemological foundations challenged by the constructivism of sociology. Whereas 19th

[2] For an excellent and thorough analysis of the geneology of the sociology of scientific knowledge from Mannheim through Merton to Barnes and Bloor whom we will discuss below, see Kaiser 1998,

century Comtean and 20th century logical positivism were initially defined by their embrace of the verifiability principle, which posits that statements bear meaning only if they can be confirmed by sense data (Ayer 1936), positivism has since become a label used polemically to refer to naïve empiricism. Positivists assigned discussions about 'what is real' to metaphysics, irrelevant and beyond the purview of science. Positivists were concerned with specifying what could science legitimately reveal, and how far beyond immediate sense data could we make empirical claims. Thus, one of the central philosophical questions of logical positivism concerned processes of scientific induction, especially generalizations (across observations) that are themselves incapable of direct empirical verification. Rudolf Carnap of the Vienna circle of logical positivists attempted to resolve the problem of induction by defining simplicity as an ideal of explanatory parsimony that could be applied to validate unverifiable generalizations.

Karl Popper, writing in Vienna at a time when the dominant intellectual movements were Marxism and psychoanalysis as well as logical positivism, produced a theory of scientific fact-making that inverted the positivists' problem of verifiability by focusing on processes of falsifiability. By his forceful development of a positivist program of research, Popper may have provided an opening toward the constructivism (that would emerge prominently in STS). Although Popper was not an admirer, in any way, of constructivism, by describing how scientists use powers of both deduction and falsification to make predictions, disproving hypotheses rather than verifying statements, Popper provided an opening for others. Popper took issue with conventionalists' claim that theoretical systems are neither verifiable nor falsifiable, arguing that the entire effort of science was to falsify claims and hypotheses. He specifically argued that the distinction between falsifiable and non-falsifiable systems could be made on the basis of experimental methodology, and much of his work identified and developed the techniques for positivist scientific methods.

Ludwik Fleck, working in Poland at about the same time as Popper and Mannheim, was writing against the Vienna Circle and logical positivism. Building on Durkheim's injunction to sociologists to treat 'social facts a things,' Fleck theorized not social facts but the production of *scientific* facts, naming the community of persons who mutually exchange ideas and maintain intellectual interaction a "thought collective." The 'thought collective," a carrier for the historical development of a field of thought, parallels Durkheim's "social group" whose continuous interactions generate inescapable, normative practices and constraints, i.e. social facts. In this way, Fleck defines a scientific fact as a thought-stylized conceptual relation that can be investigated from the perspective of history and psychology (both individual and collective), but argues that it cannot be constructed exclusively from these perspectives alone. Fleck argues that a thought collective and its thought style leads perception, trains it, and produces a stock of knowledge. Thought style sets the preconditions for any cognition, determines what can be counted as a reasonable question and a true or false answer, provides context, and sets limits to judgment about the nature of "objective reality." In this way, Fleck emphasized the theory-ladenness of observations, directly challenging the naïve empiricism of the logical positivists for whom sense data came first and inductive theorizing followed.

By the 1930s, Fleck and Mannheim aside, science was understood to be a bounded activity in which science impacts society, and technology—as applied science--develops linearly from (basic) science. The entire process was regarded as a value free, amoral enterprise that is legitimated by both the claim that its truths exist independent of, and prior to, any social authority and that it has provided the grounds of human progress. This "internalist" account described an essentially autonomous and asocial process consistent with positivistic philosophies of science as a self-regulated search for timeless, universal, irrefutable facts. Facts are themselves understood, in this received or traditional conception of science, to exist independent of the procedures for making or discovering them. "Scientific facts were considered to exist in a realm outside of the blood, sweat and tears of our everyday sensual and material world, outside of history, outside of society and culture"(Restivo 2005: xi). This understanding of science is best illustrated by the work of Robert K. Merton (1942). Writing during and immediately after World War II, Merton believed that Mannheim had mistakenly treated all knowledge and knowledge production as the same, failing to understand how the practices and norms of science were distinct. He first identified four norms that supposedly governed the activities of scientists: universalism, communalism, disinterestedness, and organized skepticism (Merton 1942; cf. Hollinger 1983). In later work, he identified norms of originality, reward, and humility (1957). Ironically, by taking up Mannheim's project, Merton seem to come to a very different conclusion, describing science in terms more consistent with the historically conventional internalist account than with the sociology of knowledge: autonomous scientific practices characterized by a unique and timeless ethos. Fine work by Merton and his students on the social organization and institutionalization of science was soon swamped by the criticism for its failure to emphasize ways in which these norms failed to provide an accurate picture of scientific behavior or to recognize shared practices across different forms of knowledge production. Gieryn, a student of Merton, would later break with Merton's theory by describing how the very distinction between science and non-science must be kept up, maintained and sustained. In his model of "boundary work"— the expulsion of that which is defined as non-science, the expansion of science to maintain explanatory authority over previously non-scientific realms, and the maintenance of scientific autonomy, Gieryn (1999) eschewed the structural-functionalism that constrained Merton's work but built from it and promoted a thoroughly constructivist account of science.

By the 1960s, however, few realms of human action were immune from acknowledgement of their historicity, including science. Within each of the traditional social science disciplines (history, philosophy, sociology, economics, anthropology, and political science), germs of a more complex understanding of science and technology were developing. Even within the sciences, critical thinking about basic assumptions and paradigms was developing, for example, work by biologists Stephen Jay Gould and Richard Lewontin about the sciences of race (cf. Harding 1993; Chorover 1979; Gould 1981, 1996; Lewontin, Rose and Kamin 1984; Lewontin 1991; Hammonds forthcoming). Despite diverse theoretical, pragmatic and disciplinary sources, science and technology studies seemed to force an orienting consensus that science is a social institution. Thomas Kuhn, deeply influenced by the work of Ludwik Fleck, argued in his groundbreaking *The Structure of Scientific Revolutions* (1962) that science does not progress by accumulating ever more accurate descriptions of nature. Rather, new scientific paradigms are produced in opposition to previous paradigms, but the shape of the new paradigm cannot be predicted in advance. Kuhn defined paradigms as a coherent body of knowledge, as "an accepted model or pattern" (23) with a series of questions defined and refined by scientists that constitute a scientific tradition that shapes the way questions are asked and information is gathered. Paradigms become dominant because they are more able than their competitors to answer questions that are deemed relevant at a particular historical moment while accounting for anomalies that have accumulated under the previous paradigm. By rejecting the belief that science followed a logical progress towards truth and placing Popperian theory within historical context, Kuhn set the groundwork for later analyses of scientific knowledge production, namely the Sociology of Scientific Institutions (SSI) and the Sociology of Scientific Knowledge (SSK).

As in any field of cultural production, STS is constituted more by its oppositions and debates than by a single theoretical paradigm, set of research questions, or canon of readings. Although engaged discourse may generate scholarly production, STS may be more fractious than other scholarly fields or interdisciplinary engagements. Because STS scholarship takes the creation of knowledge as its object of study, it has been hyper-reflexive about its own knowledge production practices, leading to extended yet insightful debate. Sometimes referred to as the science wars, these scholarly disputes suffused much academia in the 1990s where they went by a more generic label as 'culture' wars. One line of cleavage developed about the strength and depth of a constructivist account and the sufficiency of internalist histories of science. Another derives from the conjunction of science and technology within the same intellectual rubric, and yet other lines of cleavage developed from epistemological debates and professional competitions among the constituent disciplines. This self-reflexive critique in a heterogeneous joining of topics and disciplines has produced an abundance of shorthand expressions and acronyms to describe the distinctive camps and orientations. For example, some observers distinguish the scholarship of STS from the subject of study, the latter (science, technology and society) a subject that can be studied via STS or through any traditional discipline such as history, sociology, or philosophy without adopting any particular epistemological position with regard to the social construction of science. Those who focus on the sociology of scientific knowledge (SSK) distinguish themselves from those who do the social construction of technology (SCOT) or the social history of technology (SHOT) or the sociology of scientific institutions (SSI). The STS coalition probably bespeaks more about the marginality of science and technology to the central concerns of the constituent disciplines than to any necessary or comfortable marriage between the study of science and of technology or across the disciplinary perspectives. Because the history, social organization, and logic of science has been a topic of minor interest for each of the disciplines (in comparison, for example, to concerns about state development, inequality, or freedom), scholarly communities addressing science and/or technology in each discipline were relatively small and perhaps particularly guarded. Nonetheless, the divergent perspectives and heated debates have energized the field, producing an abundant literature in books and academic journals, a substantial network of professional associations, and dozens of departments offering undergraduate and advanced degrees in STS.[3]

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[3] Journals include, for example, *Social Studies of Science* for science studies generally, *Isis* for the history of science, *Science Technology and Human Values* covering contemporary science, policy and culture, *History and Technology*, *Science in Context*, *Minerva, Osiris, Technology and Culture, Studies in History and Philosophy of Science*, and wide range of specialized and regional publications such as *Metascience, Science Studies, Knowledge and Technology in Society, Public Understandings of Science, History of Science, Philosophy of Science, British Journal of the Philosophy of Science, British Journal of the History of Science, Science for the People, and Science Technology and Societe*. Professional associations include, for example, Society for the Social Studies of Science, Society for the History of Technology, ICOHETEC - International Committee for the

Covering an enormous array of topics, STS scholarship has proliferated beyond easy categorization. Several recent publications have nonetheless built synthetic, yet varying, accounts of STS from its various disciplinary streams (e.g. from history Golinski 1998; Proctor 1991; from sociologists Shapin 1993; Barnes, Bloor and Henry 1996; from political theory, Rouse 1987; from philosophy of science, Hacking 1999. In addition, see Rouse 1992; Traweek 1993; Haraway 1994; Hess 1997; Biagoli 1999; Sismondo 2004; Hackett 2007; Fischer 2007). For purposes of textual organization, we will describe STS scholarship within two very general rubrics: first, work that looks at the institutionalization, reception, and appropriation of science and technology and second, research that looks more centrally at the production of science and technology than at their appropriation, distribution, regulation and use. Across this diverse collection of research, one finds an array of theoretical positions and resources. If there was a structural functional orientation in Merton's early work, those who took up the topic of the institutionalization of science pursued diverse theoretical paths, none of which were unique to STS. If one can find a common thread, and it is tendentious at best, there was a consistent muckraking materialism that revels in exposing the play of interest, power and privilege where Merton had observed norms of disinterest, humility, communalism, and universality. Where Merton had identified a basic norm of skepticism, the STS critics describe convention and credulity. If the Sociology of Scientific Institutions (SSI) was a project devoted to discovering how scientific facts were produced through institutional hierarchies of interest and power as well as debate and consensus, the sociology of Scientific Knowledge (SSK) was more concerned with the content rather than context of science. This second strand of STS scholarship produced a more thoroughly constructivist account of science and technology (which was nonetheless also present in the studies of power and interest in policy and institutions). And while rival theoretical approaches were contested, and heated debates ensued over nuanced distinctions as well as clear oppositions, this stridently constructivist program of scholarship produced the distinctive theoretical contributions of STS scholarship that have influenced scholarship across the social sciences: an intensively researched and theorized account of the social construction of knowledge and expertise, and the identification of things as well as persons as active agents in the networks of interactions that constitute the social. This distinctive post-humanist perspective emerged directly from the unique research site of STS scholars: from the close observation of scientists at work in their laboratories. The extensions and conceptualization of processes and tools of social construction developed from close study across diverse fields, such as economic markets, banks, weapons design as well as scientific laboratories

STS Studies of the Institutionalization, Reception and Appropriation of Science and Technology

Although it had long been clear that science and technology impact society, an impact that was already documented in historical scholarship and economic development, science and technology studies explored the ways in which social forces constitute the organization and dissemination of science, but also the content and substance of scientific knowledge itself.

History of Technology, HSS - History of Science Society, IASTS - International Association for Science, Technology and Society. A list of departments offering undergraduate and advanced degrees in STS can be found at http://web.mit.edu/hasts/about/index.html.

Merton's four norms of scientific institutions claimed that science is socially influenced, although without going so far as to suggest that the content of truth-claims is sociological in nature, as Sociologists of Scientific Knowledge later would. Merton also weaved Marxian arguments regarding the determination of belief systems by class structure into his theory. Merton's question was partly pragmatic in nature: he sought to identify the variables that affect the development of science, the goal of which was "the extension of certified knowledge" (226). Merton's norms (disinterest, universality, communism and organized skepticism), parallel those of the Protestant ethic and owe a debt to Weber in this respect, are embedded in institutional values internalized by scientists. However, Merton claimed that these institutional norms clashed with the value placed upon scientific authorship, credit regimes, and funding, creating a pathogenic culture in which phenomena such as plagiarism and fabrication of data is endemic (cf. Silbey and Ewick 2002). Writing in the years leading up to World War II, Merton was acutely aware of the external political influences that can shape scientific practice, "the ways in which logical and nonlogical processes converge to modify or curtail scientific activity" (255). However, Merton believed these instances to be the exceptions that prove the rule, as political interests usually run counter to the scientific norms Merton set forth. Recent developments in the intellectual property regime have, however, transformed the basic norms of contemporary science such that it would be difficult to claim disinterest or communism as institutional constraints.

As STS developed in the early 1960s and 1970s, it was animated less by the theoretical issues driving Merton in sociology specifically and the theoretical debates in the social sciences generally than by more immediate policy concerns where the role of science and technology seemed to be both a product and a driving force. These early policy concerns developed into a flourishing industry on scientific and technological controversies (e.g. Nelkin, 1979, 1982; Nelkin and Pollack 1981). Such work exposes the divergent theoretical assumptions, rival experimental designs and contrary evidentiary interpretations, at the same time displaying the communally developed procedures for reaching closure on debate to restore continuity and consensus (Hagstrom 1965; Harry H.M. Collins and Trevor Pinch, 1993, 1998a; *1998b).*

Although these institutional and policy topics were present in the pre-STS work, science and technology studies developed not only a more nuanced but also a more critical stance toward science and technology than had prevailed in the earlier, pre-1960s disciplinary accounts of autonomous, progressive scientific development. STS contributed its critical dimension by revealing and unpacking the embedded, often unreflective claims of scientific expertise in law and elsewhere. Emerging simultaneously within periods of intense public skepticism of the roles of science and technology in the anti-Vietnam war in the United States in the 1960s and 1970s, and the growing anti-nuclear and environmental movements in the United Kingdom and Europe in the 1980s, the constructivist position that social forces constitute not only the context but also the content of science developed from roots in sociology and anthropology and spread from there. At the same time, researchers explore the ways in which such expert authority is constructed and legitimated in and through government policies and programs (Wynne, 1987; Hilgartner 2000). STS scholars also study public and private systems of risk analysis in such diverse fields as weapons, environmental management, and financial markets (MacKenzie 1990, 2001; 2006; Gusterson 2004; Masco 2006). Some, not all, of this research adopts a distinctly progressive, democratic stance, worrying about the consequences of concentrated expertise and public exclusion from critical decisions and the public responsibilities of science (*Collins and Evans, recent DeVries). Perhaps this was an outgrowth of movements such as *Science for the People* that emerged as organized opposition to the American war in Vietnam but continue to this day in studies concerning such issues as genetically modified foods, explosion in the use and marketing of pharmaceuticals, as well as global warming and world-wide environmental degradation, unplanned growth, resource depletion and inequality. Much feminist scholarship on sex and gender also emerged from grass roots activism, pioneered for example in groups such as the Boston Women's Health Collective which produced an informed, gender sensitive and critical account of women's health and sexuality, *Our Bodies, Ourselves*, in 1973, now in its 12th edition, and translated into twenty languages and Braille. Although some of this early literature was quite essentialist, arguing for fundamental differences from nature, not social organization, critical reactions generated some of the more important and longer-lasting theoretical advances, for example in the work of Marilyn Strathern (1980) and Donna Haraway (1991; cf. Tuana 1989; Merchant 1990).

Considerable lines of research in this general rubric follow the Mertonian lead, focusing on science institutions and funding, science education and public understandings of science, and technological innovation, planning, and assessment. Closely related are studies of the role of science and science advising in government (e.g. Mukerji 1989; Jasanoff 1990) and the role of scientific evidence in law (Smith and Wynne 1989; Jasanoff, 1995; and Cole 2001). Since the 1980s, when American law changed markedly as a result of the Bayh-Dole Act, allowing the results of publicly funded research to be patented and licenses, the institutional and distributional issues associated with technology licensing and transfer have been the subject of extensive research (Owen-Smith).

A corollary of research on the Public Understanding of Science (PUS) and social movements is the question of how social groups organize and define themselves around scientific facts, a phenomenon anthropologist of science Paul Rabinow terms "biosociality"—that is, a mode of sociality in which "nature will be modeled on culture understood as practice; it will be known and remade through technique, nature will finally become artificial, just as culture becomes natural." (1992: 10). Examples of social movements developing around scientific information include groups of people sharing a genetic illness (Taussig, Rapp, and Heath 2003). Still more recently, science studies scholars have turned towards questions of environmental risk and global inequalities, synthesizing Ulrich Beck's work on the dynamics of environmental and technological risk in a period of reflexive modernization with social movement theory. For example, Kim Fortun's *Advocacy After Bhopal* analyzes protest in the global South, and Adriana Petryna's *Life Exposed*, adapts Rabinow's biosociality to argue that "biocitizenship" is a means by which people call upon their shared disordered biology in order to claim government resources and medical care. Using science to make policy, law, and property constitutes a thick strand of STS scholarship.

Recognition of the historical embeddedness of science drew scholars away from philosophical questions regarding how scientific knowledge is logically generated and verified, and towards questions of the material practices that embody the *work* of doing science. This historical social constructivist orientation probably claims more than some in the field would admit. It has been the source of shared interests as well as extended controversy among science and technology scholars and between the field and the practitioners under study: scientists, engineers, and policymakers.

Production of Scientific Knowledge: Elusive Boundaries and Post-Humanist Social Science

While STS scholarship is marked by a multitude of varying approaches and schools of thought, one theoretical aspect has unified much of the research —that is, the question of the ontology of scientific things and the relations of diverse heterogeneous people, animals, machines, and things to one another. Whether taking the name of "assemblage" (Callon), "network" (Latour), "cyborg" (Haraway), "parliament of things" (Latour), "capillary" (Foucault), "the body multiple" (Mol 2002) or "rhizome" (Deleuze), the emergent properties of the Rube Goldbergesque complex systems that refuse encapsulation within the boundaries that distinguish what is interior or exterior to science, agential or passive, living or inert, intentional or otherwise, has been of prime importance to scholars of science. Indeed, recognizing how diverse elements become 'black-boxed' as things and determining what kinds of knowledges are deployed and what powers assembled in this process of "entification" is where science studies both draws upon and makes it distinctive contribution to social theory at large. It begins from the "strong programme" exploring the construction of social scientific knowledge and leads to recent publications in post-humanist social science.

In defining what became known as the Edinburgh school's strong programme of the Sociology of Scientific Knowledge (SSK), David Bloor listed four central tenets: 1) SSK is concerned with the conditions that cause certain knowledge claims; 2) SSK should not prejudice research by observing and treating statements regarded as true differently than those that are regarded as false; 3) SSK should explain different belief systems symmetrically; and 4) SSK should reflexively apply these methods to itself. Specifically rejecting Merton's distinction that science constituted a unique mode of producing knowledge, SSK scholars, in effect, pursued a rigorous constructivist sociology of knowledge, subjecting science to the same intensive examination that Mannheim had applied to social or cultural knowledge. In doing so, however, SSK erased a distinction between knowledge of things and knowledge of persons that Mannheim believed was essential to understanding not the uniqueness of science but the uniqueness of human, sentient life, and re-introduced a different flavor of scientism within the sociology of science. "Where Mannheim and his mentors and colleagues distinguished between *Verstehen* [understanding] and *Erklarung* [explanation], Bloor … called for studies which are simply "causal" and "empirical" (Kaiser 1998, 76).

Producing an accurate analysis of the construction of scientific facts required a new methodology with which to examine scientific activity. Beginning in the 1970s, scholars approached scientific culture as a field of social practice like any other, and hence subject to the same tools of investigation and analysis as had been used by anthropologists and sociologists in other social fields (Latour and Woolgar, 1979; Karin Knorr-Cetina, 1981; Michel Callon 1986; Michael Lynch, 1985; 1993; Traweek 1988; Harry Collins, 1985; 2004; Clarke and Fujimura 1992; Rheinberger 1997; Gieryn 1999; Andrew Pickering; Dumit, 2004; Franklin 2007; Landecker 2007). Although the research methods were not new and much of the theoretical apparatus with which anthropologists and sociologists undertook closely observed ethnographic studies of laboratory practices, processes of scientific discovery and technological invention were also not new, some vigorous touted the attention to subjects closer to home as an innovation. As we have been suggesting throughout, science studies built on nearly a century of social constructivist theorizing and empirical research about the widest range of activities. What was new was subjecting scientists, and later engineers in work groups, to the same scrutiny and in-depth analysis of social organization, culture and epistemology that anthropologists had long applied to small scale, often pre-industrial societies and tribes and sociologists has applied to street gangs, police, and factory workers. These early forays into laboratory studies self-consciously appropriated the ethnographic voice in analyzing scientific activity, producing rich descriptions of the unarticulated and often tacit understandings that made science and scientists. In the preface to Karin Knorr-Cetina's *The Manufacture of Knowledge*, Rom Harre describes laboratory studies: "Suppose that instead of approaching the scientific community with Marx or even Goffman in hand, one were to adopt the stance of the anthropologist coming into contact with a strange tribe…. Laboratories are looked upon with the innocent eye of the traveler in exotic lands…" (1981: vii-viii).

These studies critiqued, and also built on, Merton's research that had identified functional, normative requisites for scientific communities as well as Kuhn's (1962) account of the paradigmatic development of scientific theories. While both Merton and Kuhn had described the structures of normal science, for example dialectical developments among theory, experimentation, and career advancement, the laboratory studies added to the mix insights from critical theory, ethnomethodology, and symbolic interaction, to pay particularly close attention to the cumulative consequences of micro-transactions, discursive strategies, and forms of representation within the production of a particular scientific fact or practice. These same perspectives and research methods were also adopted to study technological innovation, engineers and designers (Gusterson, 1996; Downey, 1998; Helmreich, 1998; Henderson, 1999; Diana Forsythe, 2001; Pinch, 2002). These closely observed studies of scientific and engineering practice have also led to extensive research on processes of cognition and categorization (Bowker and Star, 1999).

Much of this work was influenced by Garfinkel's *Studies in Ethnomethodology,* in which he argued that "the objective reality of social facts" is an "ongoing accomplishment of the converted activities of daily life" that must be studied by closely examining the *ad hoc* activities and utterances of daily life (1967: vii). Scholars of science like Gaston Bachelard drew upon Garfinkel's work in calling for a focus upon scientific projects rather than scientific objects, where a "project" is the activity of giving body to reason. Gilbert and Mulkay similarly applied an ethnomethodological discourse analysis to science, arguing that scientific worlds were constituted by "an indefinite series of linguistic potentialities" (1984: 10). In this way, they demonstrated that science is not a distinct realm of social action, but is like other social settings, rife with conflict, compromise, pragmatic adjustments and power, as well as taken-for-granted habits that make social settings transparent and familiar to socially competent members but alien and uninterpretable to non-member outsiders.

In *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science* (1981) Knorr-Cetina explicitly adopted the literary devices of ethnography to frame her study of a food science lab in Berkeley, in which she examined how facts are fabricated within the context of social life. Rather than taking a hard-line constructivist position, she argued that culture imposes the constraints and "the system of reference which makes the objectification of reality possible" (2). Further, "the experimenter is a *causal agent* of the sequence of events created" (3), all experimentation is a process of production, and facts are fabricated by social consensus and experimenters' "expectation-based tinkering." The scientific facts produced in this manner, Knorr-Cetina argued, are geared towards reaching previously predicted solutions rather than solving open-ended problems and are marked by analogical reasoning and the manipulation of scientific concepts through analogy and metaphor. In order to understand such processes, Knorr-Cetina adopted a position of "methodological relativism" that emphasized letting one's subjects speak. Instead of referring to scientific cultures or social groups, Knorr-Cetina described the objects of her research as "variable transscientific fields"—opportunity-directed networks of scientists connected through resource relationships, resources being either materials and tools necessary for experimentation or the raw material of ideas that can be converted into success through the consensus of a scientist's peers. In her later *Epistemic Cultures: How the Sciences Make Knowledge* (1999) Knorr-Cetina revised the focus of her analysis, not examining the construction of knowledge as she and other early laboratory ethnographers had, but rather the way the machineries of knowledge construction are themselves constructed. She compared high-energy physics and molecular biology labs as knowledge cultures in order to expose the "knowledge machineries of contemporary sciences." Rather than looking at the kinds of things found in laboratories, she attended to the unique relations between things that are brought together in laboratories.

By the 1980s, it was well understood, and in some scholarly networks taken for granted, that science is in this regard the same as all other human activities, a socially constructed phenomenon: the product of collectively organized human labor and decision-making. "Facts do not fall out of the sky, they are not 'given' to us directly, we do not come to them by means of revelation…[W]ork is embodied in the fact, just as the collective toil of the multitude of workers in Rodin's workshop is embodied in *The Thinker*. This is what it means to say that a fact is socially constructed." (Sal Restivo, 2005: xiii). This does not mean that any statement can secure the status of scientific fact; social construction is not a recipe for cognitive solipsism or moral relativism. Nor does it mean that scientific facts are completely arbitrary accidents. It means only that scientific facts are contingent: the ways in which a fact is produced—the choice of topic, location of research, the constraints of resources, the accumulation of empirical evidence, the transparency of methods—are part of the constitution or construction of the fact.

Collins coined the term "experimenters' regress" to refer to the impossibility of definitively proving the results of an experiment by replicating those results. Collins argues that agreement regarding the results of experiments is arrived at socially, as scientists reach a consensus founded upon "shared perception" and "the forms of life or taken-for-granted practices—ways of going on—in which they are embedded" (1985: 9, 18). Such consensual agreement breaks the logical short circuit of infinite regress by using "tacit knowledge," (Polanyi 1997) which allows practitioners to separate relevant and meaningful information from data that can be qualified as insignificant or artifactual. Collins pointed out that all experiments have a certain amount of interpretive flexibility built into them—as well as a dose of uncertainty—because (i) the results of an experiment are predicated upon faith in the equipment used in the experiment, (ii) the conclusion rests upon belief that enough data has been gathered in order to adequately prove a theory, and (iii) belief that the data is not an artifact of experimental noise. Pickering (*) also claimed that scientific narratives obscure the fact that scientists agree only *retrospectively* upon the validity of a theory (cf. Collins and Pinch*).

Ian Hacking connected this social constructivist work emerging in the 1980s with the earlier philosophy of science. He responded to Carnap's, Popper's, and Kuhn's theories of so-called "mummified" science by also drawing attention to what scientists *do rather than how they think*. In so doing, he articulated the relation of theory and experiment—"We represent in order to intervene, and we intervene in the light of representations" (1983: 31). To address the persistent issues of the relationships among observable sense datum, statements, meanings, induction across observations and generalizations, Hacking distinguished several different types of realism, differentiating between the realism of concepts and the realism of things. Further, he elaborated upon Kuhn's claim that different scientific paradigms are incommensurable by pointing to three different forms of incommensurability—topic, dissociation, and meaning incommensurability. Topic incommensurability refers to when different paradigms take different types of questions to be theoretically relevant. Dissociation refers to the way in which different types of phenomena are classed into sets that reflect upon certain types of questions—that is, what kinds of phenomena can be used in tandem to reach an inductive account of a principle. Meaning incommensurability, the most radical incommensurability, refers to when the terms of one paradigm cannot be explained or accounted for in the terms of another. Hacking identified three sticking points that characterize the debate between adherents and detractors of social constructivism as it was developing in STS scholarship: contingency (constructivists believe that there is no one accurate system that is inevitable for producing a successful science—"a research program that does not incorporate anything equivalent to the standard model, but which is as progressive as contemporary high energy physics" [1999: 70]), metaphysics (classifications are convenient ways to represent the world, but not determined by an objective reality), and stability (constructivists provide external conditions for the stability of a concept, whereas non-constructivists offer internal explanations). Contingency does not imply randomness—Hacking conceded that "scientists who do not simply quite have to *accommodate* themselves to that resistance [of the world]" (71) and that the fit between theory, phenomenology, schematic model, and apparatus is a robust one (72). Thus, constructivism, according to these conditions, claims that X is not pre-determined, although that which can be counted as X is specified by resistance and robustness.

Thus while some of the early work focused on the contextual shaping of the content of science and technology, STS as a mature field rejected the notion of a natural or fixed boundary between science and its context. Considerations of organization, resources, and human capacity seemed obvious with respect to technological innovation but in the traditional disciplines had often been relegated to the boundaries of science or the social conditions of its making. What became known through Gieryn's work (1999) as "boundary work"—the discourses and practices of institutional legitimacy and exclusion—became a central focus of STS research tracking the human transactions—symbolic and material—that shaped scientific facts as well as membership in scientific communities. They attended to the ways in which science is internally defined as a privileged site of knowledge production, focusing their attention on the indistinguishability of science from non-science. For example, Daston & Park (2001) dismiss of distinctions between medieval and early modern periods and attendant distinctions of pre-scientific from scientific inquiry and feminist scholars claim that feminine science has historically been devalued as mere "art" (Hubbard 1990). Others looked at activities not heretofore categorized as science by contemporary scientists, such as Newton and Boyle's alchemical interests and the relationship of these to the works that are taken to have made the scientific revolution (Dobbs 1975; Newman and Principe 2002). No longer do scholars regard it as appropriate to isolate the elements of scientists' work that have over time proven useful and scientifically productive, discarding what modern science has rejected as aberrational or simply wrong.

Similarly, any hard and fast distinction between basic science and applied technology became difficult to sustain, once the work practices of scientists and engineers were closely observed. The advance of modern physics, for example, is described as a productive collaboration between theory, instrumentation, and experiment in Peter Galison (1997), *Image and Logic*. Galison breaks with both logical positivism and antipositivism by arguing that physics communities are heterogeneous and intercalated with one another within "trading zones," areas of cultural contact in which scientists deploy pidgins with which they are able to converse across sub-disciplinary lines. Finally, any hard and fixed division between the disciplinary approaches to the production or reception of science began to merge in important studies. Steven Shapin and Simon Schaffer's (1985) influential book, *Leviathan and the Air-Pump*, encouraged scholars to move among the historical, anthropological and sociological approaches to the study of science and technology.

Recent studies of elusive boundaries between science and non-science have focused on the ways in which non-scientists participate in the construction of scientific knowledge. Steven Epstein (1996), for example, described the ways in which gay rights activists became expert analysts of the existing medical knowledge concerning AIDS when the epidemic first took hold and eventually became co-producers of new knowledge, especially the treatment protocols and drug trials). Emily Martin's (1987) research responded to critiques of both the science and pseudo-science of gender and reproductive medicine while exploring the production and appropriation of scientific knowledge and lay models of scientific information. The scholarly work on reproductive medicine and technology as the work on AIDS followed upon grass-roots activism that exposed the limitations, and often ideological or biased assumptions, of the then conventional science in these areas.

In the attempts to produce fuller, more comprehensive and complex accounts of science, its methods and its subject matter, scholars have also looked far beyond the borders of Europe and the U.S. to understand, for example, the ways in which mathematical equations are understood in some African cultures (Verran 2001), or to investigate more carefully postcolonial science (Mitchell 1991, Abu El Haj 2001, Prakash 1999, Ong 2005, Redfield 2000, Tsing 2005). These studies have emphasized how scientific knowledge is produced and disseminated in service of the state, how colonial resources and lay knowledge have been exploited to further scientific and technological growth in the metropol (Hayden 2003, Helmreich 2007), and how Otherness and the exotic have been constructed by scientific projects embedded in colonial legacies (Schiebinger 1993, Reardon 2004, , Jasanoff 2005).

This burgeoning increase in empirical observation of the practice of science has produced two notable contributions: the work of the "strong programme" of SSK (sociology of scientific knowledge) which we have already mentioned and ANT (actor network theory) to which we turn now. Latour's Actor-Network Theory (ANT) posits that scientific facts are *things* in motion that must be followed in order to understand how scientists circulate scientific texts and inscriptions—"immutable mobiles"—as a means of gathering support for their theories by enrolling the support of colleagues. According to Latourian theory, facts and machines in the making are underdetermined and are collectively constructed by actors and actants, where an actant is an agent that cannot speak, and thus must be represented by a spokesperson. Central to ANT is the claim that the settlement of controversy is the cause of natural facts, not the result of them, and similarly that the settlement of scientific controversies causes, and is not the result of, social stability. Central to the production of facts, as Latour argues, is the process by which scientific facts come to be accepted as *facts*—that is, the way in which supporters are enrolled and actor networks are extended by trials of strength until the cost of dissent becomes too high (1987). Scientific facts are produced under constraints that vary historically and culturally; thus scientific inquiry is both enabled and constrained by what is already known, by technological capacity and the material resources that are available, and human capacity for work, imagination, collaboration, and communication. Those constraints shape the content of the science as well as the process of producing that content. The contingency of scientific facts implied by social constructivism is potentially prescriptive: if scientific facts are produced in particular contexts and are shaped by social factors, then they are contestable. As Hacking put it in *The Social Construction of What?*, to claim that something is socially constructed is to claim that it is not inevitable, and hence it is possible to say that "X is quite bad as it is" and "we would be much better off if X were done away with, or at least radically transformed" (1999: 6).

In the development of ANT, Bruno Latour and Callon profoundly influenced the course of science studies by arguing that objects - things rather than persons or animate beings - are *agential*, operating in concert with humans within extended heterogeneous networks of objects and persons. The analysis of the scientific fact as a constructed thing is extended to the full range of obdurate materiality. Latour made no fundamental distinctions between people and things, treating their influence upon scientific action as symmetrical, in this sense extending SSK's injunction to treat all belief systems and truth claims as symmetrical to the treatment of all phenomenon symmetrically. Alongside Latour and Callon, Haraway also promoted what would eventually be considered a post-human sociology that identifies and maps distributed agency. The very title of Haraway's book, *Simians, Cyborgs and Women* (1991) highlights her interest in the ways in which different forms of agency, capacity and effectiveness, circulate in practices and accounts of technoscience. Writing specifically against Latour and Callon, however, Collins and Yearley (1992) pointed out that despite claims to the contrary, the relation of human and nonhuman actors is asymmetrical in ANT. Although Latour and Callon may have symmetrically attributed agency to inanimate matter, as they claimed in their studies of scallops and door closers, critics focused on the differential interpretive apparatus required for theorizing the action of persons and the action of things. If the "French School" insisted on the symmetrical treatment of persons and things, crtics claimed, they would be unable to distinguish, even if they did not wish to valorize, the true from the false and would fall into the 'relativist's regress.' "Symmetry of treatment between the true and the false requires a human-center universe," Collins and Yearley (1992:303) wrote.

This turn towards the agency of things has been embraced by post-humanist theory, both within science studies and more broadly. Three ideas are combined variously by different authors in post-humanist theory: the hybrid assemblage of social and material elements in our world; the agency (Latour 2005) or "performativity and power" (Pickering 2005) of the material world, and finally, the resistances enacted by social and material phenomena in their interplay with each other. Within science studies, post-humanist theory is particularly noticeable in analyses the human-machine interface from the point of view of instrument design as well as the role of technology, for example computers, in human relations and development (Sherry Turkle, *The Second Self* 1984; Allucquere Stone, *The War of Desire and Technology at the Close of the Mechanical Age* 1995; Suchman 2006). Other research focuses on human relations with animals or nature in general (for example, Donna Haraway, *Primate Visions* 1989; Bruno Latour, *Politics of Nature*, 2004). Work on human-animal relations followed two intellectual trajectories: first, a thread of laboratory studies that examines the role of model organisms in the production of scientific knowledge (Kohler 1994, Creager 2002, Rader 2004) and second, feminist science studies that interrogated the relationships of scientists to animals, particularly in reference to how animals stand in for humans in scientific narrative (Haraway 1990), a research agenda that gained momentum following a series of legal decisions establishing that biological materials were patentable, alienable, and commodifiable technologies (*Diamond v. Chakrabarty; Moore v. The Board of Regents of the University of California*). In essence, this thread of STS scholarship marries in-depth technical knowledge of particular scientific fields or pieces of technology with examinations of the public and privates uses for business, management, government, and interpersonal relations.

Conclusion

Over the decades, STS has produced a set of useful concepts that together constitute something much more than Mannheim's sociology of knowledge: black-boxes, the Matthew principle, trading zones, boundary objects and boundary work, experimenter's regress, epistemological symmetry, and actants are now part of the general sociological repertoire for describing, and explaining, the processes of social construction.

Most recently, science studies generally have become concerned with how theories developed to study science can be made relevant to the rest of the social sciences, to any and all claims of knowledge, whether scientific, social, or political. STS has become a generalized study of expertise. Partly, this interest has arisen from the ugly battles of the Science Wars, in which STS scholars were attacked for critiquing scientific knowledge to the point of vertiginous relativism if not outright solipsism. In part, however, is it simply a logical extension of the unrelenting reflexivity of the strong programme of SSK in which all beliefs and all knowledge claims should be subject to symmetrical, impartial examination.

However, unrelieved skepticism about the construction of knowledge has had, what should have been sociologically expected, unintended political consequences. The science wars - between scientists and STS scholars - may be over but this issue has a newfound critical importance in contemporary political debates. For example, in political debates surrounding climate change, many science studies scholars were disturbed to realize that their own critical tools were used to question scientific facts and to reopen black boxes. In the case of current debates over climate change, the tools of STS have been deployed not in order to point towards the contingency and under-determination of social circumstances, but invoked by global-warming deniers to delay any political or material response to compelling empirical evidence of climate change. Other politically driven right wing groups have also adapted the constructivist argument to suggest that 'intelligent design' of the universe is as appropriate an account as is natural selection and Darwinian evolution. Thus, as one STS scholar confessed, his worst fears came true. Mobiling the rhetorical staples of SSK, British STS scholar Steven Fuller testified in the Dover, PA, USA trial that 'intelligent design' deserved time in science classes equal to that devoted to evolution; neither have determinant nor otherwise compelling status as more legitimate science.

Such developments have prompted a new round of debates among science studies scholars, echoing the field's origins. Renewed concerns about the implications for democracy of the complexity and inaccessiblity of scientific knowledge, and yet its increasing importance for our collective survival are producing what Collins and Evans (2002) call the third wave of science studies. STS scholars have joined the age-old discussions among philosophers, political scientists, and sociologists generally about politics: "how we shall live together in the polis?" (Devries 2007; Latour 2005). The discussion for STS is not longer just about science; it never was. However, a renewed empirical consensus seems to be emerging. As Latour reiterates in "Why Has Critique Run out of Steam?" the goal of science studies was "never to get *away* from facts but *closer* to them," to "renew empiricism" (2004).

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