# Nuclear Democracy

## Political Engagement, Pedagogical Reform, and Particle Physics in Postwar America

*By David Kaiser**

**ABSTRACT**

The influential Berkeley theoretical physicist Geoffrey Chew renounced the reigning approach to the study of subatomic particles in the early 1960s. The standard approach relied on a rigid division between elementary and composite particles. Partly on the basis of his new interpretation of Feynman diagrams, Chew called instead for a "nuclear democracy" that would erase this division, treating all nuclear particles on an equal footing. In developing his rival approach, which came to dominate studies of the strong nuclear force throughout the 1960s, Chew drew on intellectual resources culled from his own political activities and his attempts to reform how graduate students in physics would be trained.

### INTRODUCTION: MCCARTHYISM AND THE WORLD OF IDEAS

Historians have studied several examples in which scientists framed details of their work in explicitly political language. German physiologists and physical scientists such as Rudolf Virchow, Ernst von Brücke, Emil Du Bois-Reymond, Hermann von Helmholtz, and others endeavored self-consciously to keep their dreams of political unity alive even after the crushing defeat of 1848 by pursuing methodological and epistemological unity within scientific knowledge. Just a few years later, the Swiss-French chemist Charles Frédéric Gerhardt suggested a "chemical democracy" in which all atoms within molecules

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would be treated as equals, a notion the German organic chemist Hermann Kolbe countered with a hierarchical, "autocratic" model of molecular structure. More recently, several Soviet theoretical physicists, including Yakov Frenkel, Igor Tamm, and Lev Landau, drew explicitly on their own life experiences under Stalin's rule—experiences that often included extended prison terms—when describing solid-state physics, referring, for example, to the "freedom" of electrons in a metal and particles' other "collectivist" behavior. The Indian astrophysicist Meghnad Saha emphasized during the 1920s that various chemical elements within the stars would respond to the same "stimulus" in varying ways, an analysis of atoms' agency deeply resonant with his own social and political struggles against caste hierarchies. Meanwhile, during the postwar period, the Japanese particle theorist Shoichi Sakata, thinking along explicitly Marxist lines, favored a strict hierarchy among subatomic particles, finding in such an arrangement appropriate base-superstructure relations.[1] Social metaphors abound within the physical sciences.

Like others in these earlier periods of tumult and turmoil, American scientists working after World War II experienced dramatic and fast-moving political currents. McCarthyism in America meant sweeping violations of civil liberties for thousands of citizens; blacklists and unfair firings from all manner of jobs affected people in and out of academia. But it also meant more than this: historians must supplement the tallies of such injustices with attention to the intellectual legacy of McCarthyism.[2] This essay explores the interplay during the early postwar decades between changing assumptions about political engagement, effective pedagogical approaches, and ideas about the behavior of subatomic particles. In particular, I will focus on the work of the prominent Berkeley particle theorist Geoffrey Chew and his concept of "nuclear democracy."

Beginning in the early 1960s, Chew railed against physicists' reigning approach to particle physics, quantum field theory, arguing that this framework offered no help for understanding the strong nuclear forces that kept atomic nuclei bound together. As he emphasized with great gusto at a June 1961 conference in La Jolla, California, quantum field theory was as "sterile" as "an old soldier" when it came to treating the strong interaction and hence was "destined not to die but just to fade away"—a memorable pronouncement that many of his peers repeated over the next several months.[3] In its place,

[1] On the German physiologists and physical scientists see Keith Anderton, "The Limits of Science: A Social, Political, and Moral Agenda for Epistemology in Nineteenth Century Germany" (Ph.D. diss., Harvard Univ., 1993), Ch. 2; cf. Timothy Lenoir, "Social Interests and the Organic Physics of 1847," in *Science in Reflection,* ed. Edna Ullmann-Margalit (Dordrecht: Kluwer, 1988), pp. 169–191. On Gerhardt's and Kolbe's positions see Alan Rocke, *The Quiet Revolution: Hermann Kolbe and the Science of Organic Chemistry* (Berkeley: Univ. California Press, 1993), pp. 208, 325 (my thanks to Michael Gordin for bringing this reference to my attention). On the Soviet physicists' outlook see Alexei Kojevnikov, "Freedom, Collectivism, and Quasiparticles: Social Metaphors in Quantum Physics," *Historical Studies in the Physical and Biological Sciences,* 1999, *29:*295–331; cf. Karl Hall, "Purely Practical Revolutionaries: A History of Stalinist Theoretical Physics" (Ph.D. diss., Harvard Univ., 1999). On Saha see Abha Sur, "Egalitarianism in a World of Difference: Identity and Ideology in the Science of Meghnad Saha," unpublished MS. On Sakata see "Philosophical and Methodological Problems in Physics," *Progress of Theoretical Physics,* 1971, *50*(Suppl.):1–248; Shunkichi Hirokawa and Shûzô Ogawa, "Shôichi Sakata—His Physics and Methodology," *Historia Scientiarum,* 1989, *36:*67–81; and Ziro Maki, "The Development of Elementary Particle Theory in Japan—Methodological Aspects of the Formation of the Sakata and Nagoya Models," *ibid.,* pp. 83–95 (my thanks to Masakatsu Yamazaki for discussions of Sakata's work).

[2] Cf. Loren Graham's suggestive work on the sometimes fruitful, generative appropriations of dialectical materialism by Soviet scientists: Loren Graham, *Science, Philosophy, and Human Behavior in the Soviet Union* (New York: Columbia Univ. Press, 1987); Graham, *What Have We Learned about Science and Technology from the Russian Experience?* (Stanford, Calif.: Stanford Univ. Press, 1998); and Graham, "Do Mathematical Equations Display Social Attributes?" *Mathematical Intelligencer,* 2000, *22:*31–36.

[3] A preprint of Chew's talk at the 1961 La Jolla conference is quoted in James Cushing, *Theory Construction and Selection in Modern Physics: The S Matrix* (New York: Cambridge Univ. Press, 1990) (hereafter cited as

Chew aimed to erect a new program based directly on the so-called scattering matrix, or *S* matrix, which encoded mathematical relations between incoming and outgoing particles while eschewing many of the specific assumptions and techniques of quantum field theory.

The single most novel conjecture of Chew's developing *S*-matrix program, and its most radical break from the field-theoretic approach, was that all nuclear particles should be treated "democratically"—Chew's word. The traditional field-theory approach, against which Chew now spoke out, posited a core set of "fundamental" or "elementary" particles that acted like building blocks, out of which more complex, composite particles could be made. As we will see in Section I, Chew and his young collaborators argued against this division into "elementary" and "composite" camps, instead picturing each particle as a kind of bound-state composite of all others; none was inherently any more "fundamental" or special than any other. Deuterons, for example, treated by field theorists as bound states of more "elementary" protons and neutrons, were to be analyzed within Chew's program in exactly the same way as protons and neutrons themselves; the "democracy" extended, in principle, all the way up to uranium nuclei. Chew described this notion by the colorful phrase "nuclear democracy."[4]

The larger physics community reacted swiftly to the string of early calculational successes that Chew and his Berkeley group produced under the "democratic" banner, one example of which I will examine in the next section. Ten months after his initial "call to arms" in La Jolla, Chew was elected a member of the National Academy of Sciences, an honor he attained before his thirty-eighth birthday. There followed a string of coveted invited papers at National Academy and American Physical Society meetings. "Such lectures invariably drew capacity crowds," Chew's department chair gloated to a dean in 1964, "since Chew is generally recognized as the outstanding exponent of a particular approach to the theory of elementary particles known as the S-matrix."[5] Earlier that year Murray Gell-Mann had introduced his "quark" hypothesis, whose proposed core, fundamental building blocks look to our eyes today like the very antithesis of Chew's nuclear democracy. Yet in his first papers on the quark hypothesis, Gell-Mann took pains to re-

**Cushing, *Theory Construction***), p. 143. See also Murray Gell-Mann, "Particle Theory from *S*-Matrix to Quarks," in *Symmetries in Physics (1600–1980),* ed. M. G. Doncel, A. Hermann, L. Michel, and A. Pais (Barcelona: Bellaterra, 1987), pp. 479–497. This portion of Chew's unpublished talk was incorporated verbatim in the introduction to his 1961 textbook, *S-Matrix Theory of Strong Interactions* (New York: Benjamin, 1961), pp. 1–2. Several physicists cited Chew's unpublished La Jolla talk during the October 1961 Solvay conference in Brussels, as recorded in the Solvay conference proceedings: R. Stoops, ed., *The Quantum Theory of Fields* (New York: Interscience, 1961), pp. 88, 132, 142, 179–180, 192–195, 214–215, 222–224. In a recent interview, Chew likened his strongly worded talk at the 1961 La Jolla meeting to a "coming out of the closet" speech: Geoffrey Chew, interview with Stephen Gordon, Dec. 1997, quoted in Stephen Gordon, "Strong Interactions: Particles, Passion, and the Rise and Fall of Nuclear Democracy" (A.B. thesis, Harvard Univ., 1998) (hereafter cited as **Gordon, "Strong Interactions"**), p. 32.

[4] Geoffrey Chew, "Nuclear Democracy and Bootstrap Dynamics," in Maurice Jacob and Chew, *Strong-Interaction Physics: A Lecture Note Volume* (New York: Benjamin, 1964), pp. 103–152, on p. 105. For more on Chew's *S*-matrix program see esp. Cushing, *Theory Construction;* Tian Yu Cao, "The Reggeization Program, 1962–1982: Attempts at Reconciling Quantum Field Theory with *S*-Matrix Theory," *Archive for History of Exact Sciences,* 1991, *41:*239–282; and David Kaiser, "Do Feynman Diagrams Endorse a Particle Ontology? The Roles of Feynman Diagrams in *S*-Matrix Theory," in *Conceptual Foundations of Quantum Field Theory,* ed. Cao (New York: Cambridge Univ. Press, 1999), pp. 343–356.

[5] Burt Moyer to Dean W. B. Fretter, 30 Dec. 1964, quoted in Raymond Birge, "History of the Physics Department, University of California, Berkeley," 5 vols., ca. 1966–1970 (unpublished; copies are in the Bancroft and Physics Department Libraries, Berkeley) (hereafter cited as **Birge, "History"**), Vol. 5, Ch. 14, p. 50. On Chew's election to the NAS and his other invited lectures see *ibid.,* pp. 49–52; "Physics Professor Wins Prize," *Daily Californian* [Berkeley student newspaper], 3 Jan. 1963, p. 5; and Stanley Schmidt, "The 'Basic' Particle—It's Out of Date," *ibid.,* 23 Oct. 1963, pp. 1, 12.

assure his readers that quarks were fully compatible with Chew's "democratic" framework.[6] By the mid 1960s, Chew and his fast-growing group at Berkeley had changed the way most theoretical physicists approached the strong interaction.

Though the idea of nuclear democracy was put forth repeatedly throughout Chew's 1961 lectures, it took several more years before Chew could, by his own lights, escape "the conservative influence of Lagrangian field theory." His updated lectures, published in 1964, were distinguished from the older ones, Chew explained, by their "unequivocal adoption of nuclear democracy as a guiding principle." He began by contrasting at some length "the aristocratic structure of atomic physics as governed by quantum electrodynamics" with the "revolutionary character of nuclear particle democracy." Chew left his students and readers with little doubt: "My standpoint here . . . is that every nuclear particle should receive equal treatment under the law."[7]

All of this bluster about "conservative" field theory versus "revolutionary" nuclear democracy might inspire a knee-jerk *Zeitgeist* interpretation: as Chew sat in Berkeley, with the 1964 "Free Speech Movement" taking flight all around him, a particular vision of "democracy" floated freely from Telegraph Avenue into the Radiation Laboratory. It is no coincidence, one might conclude, that "democracy" was "in the air" among increasingly radical Berkeley activists and students—and *therefore* also permeated the seemingly rarefied project of Chew and his students. Like most knee-jerk reactions, however, this one is both too hasty and misplaced: whatever Chew was up to, his physics sprang from more than a vague "spirit of the times." Many roots of his new program extended back to theoretical developments from the mid 1950s. Chew's work on nuclear democracy cannot be read, in other words, simply as another instantiation of the strong Forman thesis, with Chew "capitulating" or "accommodating" his physics to a particular "hostile external environment."[8]

Leaving aside claims of strong external determination, we are nonetheless left with a puzzle: Why did Geoffrey Chew combine these various theoretical developments, stretching as they did over the better part of a decade, into his particular "democratic" interpretation? Why, moreover, did physicists working further and further from Chew's immediate group in Berkeley attribute different meanings to Chew's "democratic" physics? If we take a longer view of Chew's activities, intriguing questions and associations arise. In both his developing political activism (as discussed in Section II) and his unusual attempts to reform the training of graduate students (Section III), certain specific meanings of "democracy"

[6] Murray Gell-Mann, "A Schematic Model of Baryons and Mesons," *Physics Letters,* 1964, *8:*214–215; and Gell-Mann, "The Symmetry Group of Vector and Axial Vector Currents," *Physics,* 1964, *1:*63–75. See also George Johnson, *Strange Beauty: Murray Gell-Mann and the Revolution in Twentieth-Century Physics* (New York: Knopf, 1999), pp. 209–214, 225–227, 234. After the tide had shifted away from the *S*-matrix program and back to (gauge) quantum field theories, it was Chew who suggested that quarks and the nuclear-democratic formulation could be made compatible: Geoffrey Chew, "Impasse for the Elementary-Particle Concept," in *The Great Ideas Today,* ed. Robert Hutchins and Mortimer Adler (Chicago: Encyclopaedia Britannica, 1974), pp. 92–125, on pp. 124–125.

[7] Chew, "Nuclear Democracy and Bootstrap Dynamics" (cit. n. 4), pp. 104–106.

[8] Paul Forman's famous argument regarding the acceptance of acausal quantum mechanics in Weimar Germany may be found in Paul Forman, "Weimar Culture, Causality, and Quantum Theory, 1918–1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment," *Historical Studies in the Physical Sciences,* 1971, *3:*1–115. James Cushing correctly dismisses an overly simplistic reading of Chew's "nuclear democracy" vis-à-vis the Free Speech Movement in *Theory Construction,* p. 217; cf. Gordon, "Strong Interactions," pp. 35–37. On the Free Speech Movement in Berkeley see esp. W. J. Rorabaugh, *Berkeley at War: The 1960s* (New York: Oxford Univ. Press, 1989), Ch. 1; and Todd Gitlin, *The Sixties: Years of Hope, Days of Rage,* rev. ed. (1987; New York: Bantam, 1993), pp. 162–166.

recurred: no one should be singled out as special, either for privileges or for penalties; all should be entitled to participate equally. In short, as he described his beloved nuclear particles in 1964, all should receive "equal treatment under the law." Weaving in and out of a series of specific contexts, these particular elements of "democracy" found explicit expression again and again. As evidenced by Chew's continuity of vocabulary when discussing his work in these various domains—a vocabulary that he took great care to hone—it appears that while developing his new program for particle physics, Chew drew on intellectual resources culled from his own efforts at political and pedagogical reform. Laboring to ensure a democracy among particles was only one way in which Chew's work took shape after the war.

Chew's work on particle theory during the 1950s and 1960s thus highlights how certain ideas about "democracy," brought to the surface by changing political and cultural conditions, could infuse a new vision both of how graduate students should be trained and of how nuclear particles interact. The story of "nuclear democracy" can thereby reveal certain connections—labyrinthine and indirect, to be sure, but connections nonetheless—between the Cold War national security state and changing ideas within theoretical physics. In the process, we learn why certain approaches and ideas within theoretical physics might have had special appeal or salience for Chew, as well as why these ideas found varying interpretations or associations outside of his immediate group.

It is important to realize, at the same time, that this is not a story about a well-formulated "ideology" or political philosophy steering the course of physical research. For one thing, Chew left no direct statements announcing that his interesting and influential ideas about particle physics were caused by his political convictions—nor would it make much sense to expect such pronouncements. American physicists after the war came to fashion themselves as eminently practical people, pragmatic tinkerers rather than philosopher-kings. The handful of theorists who did champion strong political or philosophical positions rarely acted on them with any consistency, as Sam Schweber's recent work on J. Robert Oppenheimer has shown. Moreover, those even rarer theorists who claimed to mingle such political commitments with their physical theorizing—such as David Bohm, dismissed from Princeton after pleading the Fifth Amendment before the House Un-American Activities Committee in 1949, or Sakata's Marxist group of particle theorists in Kyoto—were easily marginalized by mainstream American physicists, written off as "doctrinaire."[9]

Yet high-sounding principles and elaborate political philosophies are hardly prerequi-

[9] On Oppenheimer see Silvan Schweber, *In the Shadow of the Bomb: Oppenheimer, Bethe, and the Moral Responsibility of the Scientist* (Princeton, N.J.: Princeton Univ. Press, 2000). On Bohm see Russell Olwell, "Physical Isolation and Marginalization in Physics: David Bohm's Cold War Exile," *Isis,* 1999, *90:*738–756; Shawn Mullet, "Political Science: The Red Scare as the Hidden Variable in the Bohmian Interpretation of Quantum Theory" (B.A. thesis, Univ. Texas, Austin, 1999); and Alexei Kojevnikov, "David Bohm and Collective Movement," unpublished MS. On Sakata's school and their dismissal by American theorists see the sources in note 1, above; Robert Crease and Charles Mann, *The Second Creation: Makers of the Revolution in Twentieth-Century Physics* (New York: Macmillan, 1986), pp. 261–262, 295–296; and Johnson, *Strange Beauty* (cit. n. 6), pp. 202, 231–232. On postwar American physicists' views of themselves see Paul Forman, "Social Niche and Self-Image of the American Physicist," in *The Restructuring of Physical Sciences in Europe and the United States, 1945–1960,* ed. Michelangelo de Maria *et al.* (Singapore: World Scientific, 1989), pp. 96–104; David Kaiser, "The Postwar Suburbanization of American Physics," unpublished MS; and Schweber, "The Empiricist Temper Regnant: Theoretical Physics in the United States, 1920–1950," *Hist. Stud. Phys. Biol. Sci.,* 1986, *17:*55–98. American physicists' lack of explicit political philosophizing fit within broader trends during the American 1950s. See Daniel Bell, *The End of Ideology: On the Exhaustion of Political Ideas in the Fifties* (New York: Free Press, 1960); cf. Alan Brinkley, *Liberalism and Its Discontents* (Cambridge, Mass.: Harvard Univ. Press, 1998).

sites for political action. Rather than formulating an explicit political philosophy of democracy, Chew undertook a series of actions throughout the late 1940s and 1950s to fight for a (perhaps vague) notion of fair play and equal treatment. The fact that at the time these actions were sometimes interpreted quite differently by his peers, or that we might evaluate them today as something other than obviously or inherently "democratic," in no way diminishes the importance Chew himself invested in such experiences or the significance they carried for him. The absence of explicit pronouncements tying his political interests and physical research together in a neat and tidy package—the kind of "smoking gun" that is almost never to be found in historical investigations—hardly relieves us of the job of interrogating such episodes to tease apart the intellectual residue of McCarthyism in the world of ideas.

## I. DIAGRAMMATIC BOOTSTRAPPING AND A DEMOCRACY OF PARTICLES

Much of Geoffrey Chew's work during the late 1950s and throughout the 1960s centered on new ways of interpreting and calculating with Feynman diagrams. Richard Feynman had first introduced the stick-figure line drawings that bear his name in the late 1940s. The diagrams were designed for service as a handy bookkeeping device when making lengthy calculations within quantum electrodynamics, the physicists' theory for how electrons interact with light. The full calculation of electron-electron scattering, for example, could be broken up into an infinite series of more and more complicated terms, each associated with more and more complicated ways in which the two incoming electrons could scatter. In the simplest contribution, one electron could emit a photon or quantum of light, which would then be absorbed by the other electron, with each electron thereby changing from its original momentum.[10] Feynman illustrated this process with the diagram in Figure 1. Uniquely associated with this diagram, Feynman continued, was a mathematical expression that yielded the probability for two electrons to scatter in this way.

The process illustrated in Figure 1, however, was only the start of the calculation; the two electrons could scatter in all kinds of more complicated ways, and these correction terms had to be included systematically as well. Feynman thus used his line drawings as a shorthand to keep track of these correction terms; examples of the diagrams that entered at the next round of approximation are shown in Figure 2. The key to the diagrams' use, as Feynman and his young collaborator Freeman Dyson emphasized, was the unique one-to-one relation between each element of the diagram and each mathematical term in the accompanying equation. Dyson, in particular, demonstrated that these calculational relations between diagram elements and mathematical expressions could all be derived rigorously from the foundations of quantum field theory. Soon after Feynman introduced the diagrams, however, theorists like Chew began to adopt them—and subtly adapt them—for applications well beyond the domain of electromagnetic interactions.[11]

[10] Richard Feynman, "Space-Time Approach to Quantum Electrodynamics," *Physical Review,* 1949, *76:*769–789; F. J. Dyson, "The Radiation Theories of Tomonaga, Schwinger, and Feynman," *ibid.,* 1949, *75:*486–502; and Dyson, "The *S* Matrix in Quantum Electrodynamics," *ibid.,* 1949, *75:*1736–1755. "Virtual" particles, such as the photon exchanged in Figure 1, "borrow" excess energy and momentum for very short periods of time (as compared with the energy they would carry as free, noninteracting particles), as allowed by Heisenberg's uncertainty relation. According to quantum field theory, all interactions arise from the exchange of virtual particles.

[11] On the introduction and use of Feynman diagrams within quantum electrodynamics see Silvan Schweber, *QED and the Men Who Made It: Dyson, Feynman, Schwinger, and Tomonaga* (Princeton, N.J.: Princeton Univ. Press, 1994). On the dispersion of Feynman diagrams during the 1950s and 1960s for new and different types of calculations see David Kaiser, "Making Theory: Producing Physics and Physicists in Postwar America" (Ph.D. diss., Harvard Univ., 2000), pp. 271–470; and Kaiser, "Stick-Figure Realism: Conventions, Reification, and the Persistence of Feynman Diagrams, 1948–1964," *Representations,* Spring 2000, *70:*49–86.

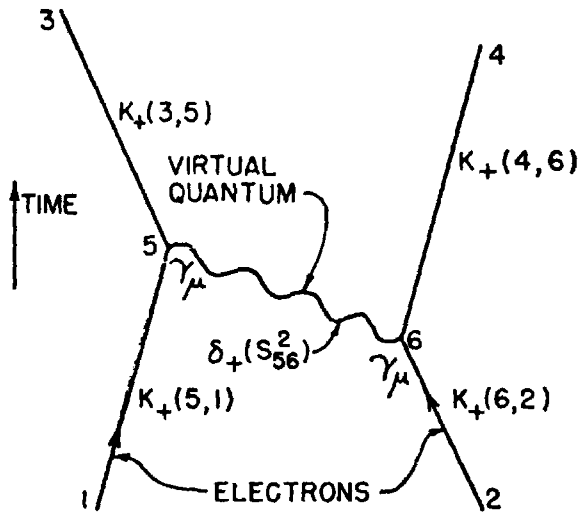


***Figure 1.*** *The simplest Feynman diagram for electron-electron scattering. From R. P. Feynman, "Space-Time Approach to Quantum Electrodynamics,"* Physical Review, *1949,* 76:*769–789, on p. 772. Reprinted with kind permission of the American Physical Society.*

Chew pushed his new interpretation of the diagrams further than most of his peers by proclaiming that the diagrams' content, meaning, and calculational role could be severed entirely from the original field-theoretic framework in which they had been introduced. He emphasized in his published 1961 lectures, for example, that many of the new results of his developing program were "couched in the language of Feynman diagrams," even though, contrary to first appearances, they did not "rest heavily on field theory." "It appears to me," he further prophesied, "likely that the essence of the diagrammatic approach will eventually be divorced from field theory" altogether. Whereas field theorists had lectured to their students for over a decade that the lines within Feynman diagrams could only represent elementary particles—that is, the particles whose associated quantum fields appeared in the governing unit or basic interaction term—Chew countered that this distinction was not borne out by the diagrams themselves. As he expounded in his lecture notes, the diagrams themselves "contain not the slightest hint of a criterion for distinguishing elementary particles [from composite ones]. . . . If one can calculate the *S* matrix without distinguishing elementary particles, why introduce such a notion?"[12] He emphasized the next year, during his summer-school lectures at Cargèse, that the conjecture underlying

[12] Chew, *S-Matrix Theory of Strong Interactions* (cit. n. 3), pp. 3, 5. See also Chew's 1962 lectures at the Cargèse summer school: Geoffrey Chew, "Strong-Interaction *S*-Matrix Theory without Elementary Particles," in *1962 Cargèse Lectures in Theoretical Physics,* ed. Maurice Lévy (New York: Benjamin), Lecture 11, pp. 1–37, on p. 8. Chew was referring in particular to Lev Landau's 1959 modified rules for using the diagrams, which in turn had been based on Chew's 1958 work on the so-called particle-pole conjecture. The particle-pole conjecture stipulated that the only pole-like singularities within the scattering amplitude for a given process would occur uniquely at the values of mass and momentum corresponding to an exchanged particle. Landau used this speculation to derive general rules for isolating the singularities within generic scattering amplitudes. See Chew, "Proposal for Determining the Pion-Nucleon Coupling Constant from the Angular Distribution for Nucleon-Nucleon Scattering," *Phys. Rev.,* 1958, *112:*1380–1383; Lev Landau, "On Analytic Properties of Vertex Parts in Quantum Field Theory," *Nuclear Physics,* 1959, *13:*181–192; Cushing, *Theory Construction,* pp. 109–113, 129–131; and Kaiser, "Do Feynman Diagrams Endorse a Particle Ontology?" (cit. n. 4), pp. 345–349.

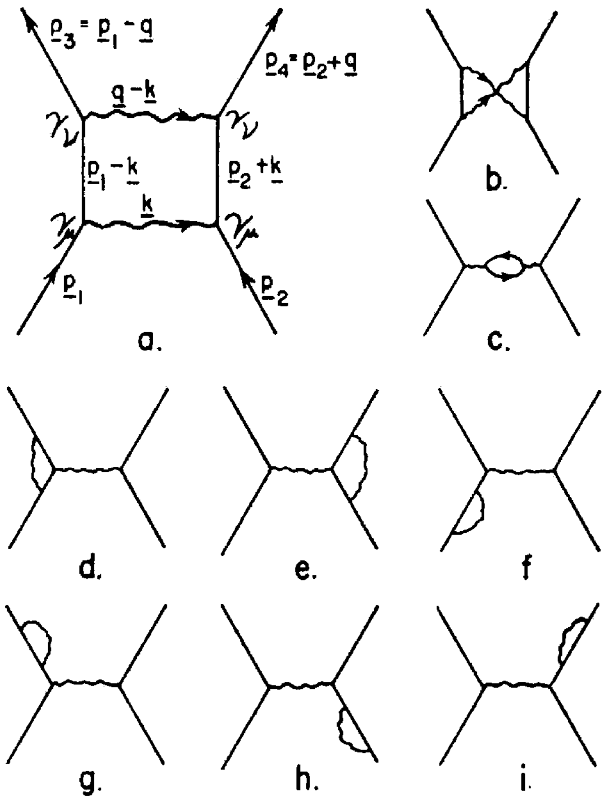


***Figure 2.*** *Feynman diagrams for electron-electron scattering correction terms. From R. P. Feynman, "Space-Time Approach to Quantum Electrodynamics,"* Physical Review, *1949,* 76:*769–789, on p. 787. Reprinted with kind permission of the American Physical Society.*

this diagrammatic equality "grew out of field theory, particularly from Feynman graphs, but it is now believed that the principle can be formulated completely within the framework of the S matrix."[13]

Drawing liberally on a smattering of theoretical developments from the mid and late 1950s, Chew pointed to the diagrams when formulating his new "democratic" conclusion.[14]

[13] Chew, "Strong-Interaction *S*-Matrix Theory without Elementary Particles," pp. 6–7. Here Chew was again referring to his own 1958 particle-pole conjecture. He later emphasized the "decisive" role played by his new interpretation of Feynman diagrams, writing that by the end of the 1950s it had become clear to him "that graphs of the type invented by Richard Feynman for perturbative evaluation of a Lagrangian field theory are relevant to the analytic S matrix, independent of any approximation based on a small coupling constant." Geoffrey Chew, "Particles as *S*-Matrix Poles: Hadron Democracy," in *Pions to Quarks: Particle Physics in the 1950s,* ed. Laurie Brown, Max Dresden, and Lillian Hoddeson (New York: Cambridge Univ. Press, 1989), pp. 600–607, on p. 601.

[14] In addition to his particle-pole conjecture and Landau's rules for Feynman diagrams, Chew drew especially on Feynman diagrams' crossing symmetry (as established in 1954), single- and double-variable dispersion relations (which had been formulated during 1954–1958), and Tullio Regge's analysis of complex angular momenta in potential scattering (from 1959). On the dispersion relations work see Marvin Goldberger, "Introduction to the Theory and Application of Dispersion Relations," in *Relations de dispersion et particules élémentaires* [Proceedings of the 1960 Les Houches Summer School], ed. C. de Witt and R. Omnès (Paris: Hermann, 1960), pp. 15–157; and J. D. Jackson, "Introduction to Dispersion Relation Techniques," in *Dispersion Relations: Scottish Universities' Summer School, 1960,* ed. G. R. Screaton (New York: Interscience, 1961), pp. 1–63.

A particle that in one orientation of a Feynman diagram looked like the fundamental building-block constituent of a more complicated particle would appear, upon various rotations of the diagrams, as either the exchanged particle responsible for a force between other particles or as the end-state composite of other constituents. Two of Chew's young colleagues at Berkeley, Frederik Zachariasen and Charles Zemach, built directly on this work in their 1961–1962 treatment of the ρ meson, a heavy, unstable particle that decayed via the strong force relatively rapidly into pairs of pions. The ρ had been discovered by Berkeley experimentalists at the famous cyclotron in 1960, though its existence had actually been predicted earlier by a pair of Chew's graduate students.[15] Zachariasen and Zemach leaned on a series of simple Feynman diagrams, interpreted now along Chew's lines, to guide their calculations.[16] Figure 3*a* depicted a ρ meson being exchanged between two incoming pions, much as the two electrons in Feynman's Figure 1 exchanged a photon. The exchange of the ρ in this case would give rise to an attractive force between the pions. Figure 3*b,* on the other hand, showed two pions coming together to form a new bound-state composite particle, the ρ meson, which, being unstable, later decayed into a new pair of pions. If two pions could create a ρ meson, however, then interactions like that shown in Figure 3*c* had to be considered as well, in which a ρ meson acted just like an "elementary" particle, scattering with an incoming pion; in Figure 3*c,* meanwhile, a pion appeared as the exchanged force-carrying particle. The field theorists' labels of "elementary," "force-carrier," and "composite" swapped places with each rotation of a given Feynman diagram, Chew and his young colleagues charged. The only consistent theoretical framework, they therefore maintained, was a "democratic" one that made no distinctions between "elementary" and "composite" particles.

From this reinterpretation of Feynman diagrams, only a short step led Chew to the essential point of his autonomous *S*-matrix program: not only were composite particles to be treated as equivalent to elementary ones, but all (strongly interacting) particles could in fact be seen as composites of each other. Diagrammatic initiatives akin to those in Figure 3 were central to the new scheme, as Chew explained in his summer-school lectures during 1960:

> The forces producing a certain reaction are due to the intermediate states that occur in the two "crossed" reactions belonging *to the same diagram.* The range of a given part of the force is determined by the mass of the intermediate state producing it, and the strength of the force by the matrix elements connecting that state to the initial and final states of the crossed reaction. By considering all three channels [i.e., orientations of the Feynman diagram] on this basis we

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Cushing treats the relation of these various ideas and approaches to Chew's *S*-matrix program in *Theory Construction,* Chs. 3–5.

[15] The experimental discovery of the ρ meson as a resonance in pion scattering was announced in A. R. Erwin, R. March, W. D. Walker, and E. West, "Evidence for a π-π Resonance in the $I = 1$, $J = 1$ State," *Physical Review Letters,* 1961, *6:*628–630. It had been predicted earlier in William Frazer and José Fulco, "Effect of a Pion-Pion Scattering Resonance on Nucleon Structure," *ibid.,* 1959, *2:*365–368; and Frazer and Fulco, "Partial-Wave Dispersion Relations for the Process $\pi + \pi \rightarrow N + \bar{N}$," *Phys. Rev.,* 1960, *117:*1603–1608. Frazer completed his dissertation under Chew's direction in 1959, and Chew helped to advise Fulco's dissertation, completed in 1962 in Buenos Aires. Years later, Frazer recalled that "Geoff advised us every step of the way" with this work "but generously decided not to put his name on the paper": Frazer, "The Analytic and Unitary S-Matrix," in *A Passion for Physics: Essays in Honor of Geoffrey Chew,* ed. Carleton DeTar, J. Finklestein, and Chung-I Tan (Singapore: World Scientific, 1985) (hereafter cited as **DeTar *et al.,* eds., *Passion for Physics***), pp. 1–8, on p. 4.

[16] Frederik Zachariasen, "Self-Consistent Calculation of the Mass and Width of the $J = 1$, $T = 1$ ππ Resonance," *Phys. Rev. Lett.,* 1961, *7:*112–113; erratum, *ibid.,* p. 268; and Zachariasen and Charles Zemach, "Pion Resonances," *Phys. Rev.,* 1962, *128:*849–858.

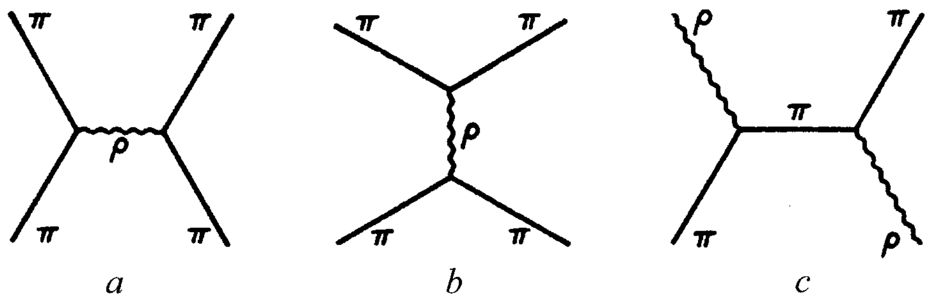


***Figure 3.*** *From Frederik Zachariasen and Charles Zemach, "Pion Resonances,"* Physical Review, *1962,* 128:*849–858, on pp. 850–851, 857. Reprinted with kind permission of the American Physical Society.*

> have a self-determining situation. One channel provides forces for the other two—which in turn generate the first.[17]

In this way, Chew explained his notion of a particle "bootstrap," saying simply that "each particle helps to generate other particles which in turn generate it."[18] Consider again the case of the ρ meson, one of the earliest successes for Chew's bootstrap model. In its force-carrying mode, as in Figure 3*a,* the exchange of the ρ would create an attractive force between the two incoming pions. Thus drawn together owing to this attractive force, the two pions could combine to produce a resonance—a new bound state or composite particle, as in Figure 3*b.* Next, the "self-determining" feature would come in: Chew and his young collaborators looked for self-consistent solutions such that the force-carrying process produced a resonance whose properties were precisely those of the force-carrying particle itself. If such a solution existed, then the ρ could, all by itself, bring the pions together so that they could produce a ρ. The ρ meson, in this series of diagrams, would

[17] Chew, *S-Matrix Theory of Strong Interactions* (cit. n. 3), p. 32 (emphasis added). The same paragraph appears in his summer-school lectures from 1960—Geoffrey Chew, "Double Dispersion Relations and Unitarity as the Basis of a Dynamical Theory of Strong Interactions," in *Dispersion Relations,* ed. Screaton (cit. n. 14), pp. 167–226, on p. 185—and in the proceedings of the 1960 Les Houches summer school—*Relations de dispersion et particules élémentaires,* ed. de Witt and Omnès (cit. n. 14), pp. 455–514. The duplication is not surprising since, as Chew explained, the 1961 lecture note volume "originated in lectures given at summer schools at Les Houches and Edinburgh in 1960": Chew, *S-Matrix Theory of Strong Interactions,* p. vi.

[18] Chew, "Nuclear Democracy and Bootstrap Dynamics" (cit. n. 4), p. 106. Chew's former postdoc and partner in the early bootstrap work, Steven Frautschi, explained simply in lectures from the 1961/1962 academic year that "bootstrap calculations lean heavily on 'crossing,'" that is, on the symmetries obeyed by scattering amplitudes as the associated Feynman diagrams underwent various rotations: Steven Frautschi, *Regge Poles and S-Matrix Theory* (New York: Benjamin, 1963), p. 176. The bootstrap notion was illustrated with the aid of crossed Feynman diagrams in Chew, "Nuclear Democracy and Bootstrap Dynamics," pp. 134, 136, and also in a text by one of Chew's former students: William Frazer, *Elementary Particles* (Englewood Cliffs, N.J.: Prentice Hall, 1966), p. 134. For more on Chew's bootstrap see James Cushing, "Is There Just One Possible World? Contingency vs. the Bootstrap," *Studies in History and Philosophy of Science,* 1985, *16:*31–48; Cushing, *Theory Construction,* Ch. 6; Cao, "Reggeization Program" (cit. n. 4); Kaiser, "Do Feynman Diagrams Endorse a Particle Ontology?" (cit. n. 4); and Yehudah Freundlich, "Theory Evaluation and the Bootstrap Hypothesis," *Stud. Hist. Phil. Sci.,* 1980, *11:*267–277. Chew expanded on his bootstrap idea in several popular pieces written after the idea had fallen from favor for most particle theorists. See Geoffrey Chew, "'Bootstrap': A Scientific Idea?" *Science,* 23 Aug. 1968, *161:*762–765; Chew, "Hadron Bootstrap: Triumph or Frustration?" *Physics Today,* Oct. 1970, *23:*23–28; and Chew, "Impasse for the Elementary-Particle Concept" (cit. n. 6). See also the interview of Chew by Fritjof Capra: "Bootstrap Physics: A Conversation with Geoffrey Chew," in *Passion for Physics,* ed. DeTar *et al.,* pp. 247–286.

have pulled itself up by its own bootstraps.[19] In Zachariasen and Zemach's numerical analysis, the self-consistent solutions for the mass and coupling constant of the ρ meson were surprisingly close to recent experimental results—this at a time when theorists working within more traditional field-theoretic traditions remained completely stymied in their attempts to analyze the reams of data pouring forth from the nation's accelerators.[20]

Chew and his postdoctoral student Steven Frautschi wondered whether every particle arose in this way: their bootstrap conjecture held that every strongly interacting particle was a composite particle, composed of just those other particles that were brought together by exchanging the first particle as a force. Chew elaborated on this point in his 1964 lecture notes:

> The bootstrap concept is tightly bound up with the notion of a democracy governed by dynamics. Each nuclear particle is conjectured to be a bound state of those S-matrix channels with which it communicates, arising from forces associated with the exchange of particles that communicate with "crossed" channels. . . . Each of these latter particles in turn owes *its* existence to a set of forces to which the original particle makes a contribution.

The bootstrap thus offered Chew the ultimate nuclear democracy: elementary particles deserved no special treatment separate from composite ones; in fact, there might not even exist any "aristocratic," elementary particles, standing above the composite fray. It was in this sense that Chew concluded that "every nuclear particle should receive equal treatment under the law."[21]

Feynman diagrams could be—and, indeed, were—deployed in a host of different ways throughout the 1950s and 1960s; the diagrams themselves did not dictate how physicists would use and interpret them. Chew built directly on his new and unprecedented interpretation of the diagrams to proclaim that all particles should be treated equally. Several of the ingredients for these new types of calculations had been forged over the previous decade (many of them by Chew himself); yet no one before Chew had put these particular elements of the calculation together in the same way. No one, moreover, had gleaned quite the same lesson about "democracy" from the structure of rotated Feynman diagrams. As we will see in Section IV, theorists working at Princeton at this time picked up a number of Chew's new calculational techniques. Yet they broke with him over the calculations' ultimate theoretical implications. Given the tremendous plasticity with which the diagrams had been appropriated ever since Feynman and Dyson originally introduced them, can we

[19] See G. F. Chew and S. C. Frautschi, "Unified Approach to High- and Low-Energy Strong Interactions on the Basis of the Mandelstam Representation," *Phys. Rev. Lett.,* 1960, *5:*580–583; Chew and Frautschi, "Principle of Equivalence for All Strongly-Interacting Particles within the S-Matrix Framework," *ibid.,* 1961, *7:*394–397; Chew and S. Mandelstam, "Theory of Low-Energy Pion-Pion Interaction," *Phys. Rev.,* 1960, *119:*467–477; Chew and Mandelstam, "Theory of Low-Energy Pion-Pion Interaction, II," *Nuovo Cimento,* 1961, *19:*752–776; and Chew, Frautschi, and Mandelstam, "Regge Poles in ππ Scattering," *Phys. Rev.,* 1962, *126:*1202–1208. See also the references cited in note 18, above, as well as Frederik Zachariasen, "Lectures on Bootstraps," in *Recent Developments in Particle Physics,* ed. Michael Moravcsik (New York: Gordon & Breach, 1966), pp. 86–151, and references therein.

[20] Zachariasen, "Self-Consistent Calculation" (cit. n. 16); and Zachariasen and Zemach, "Pion Resonances" (cit. n. 16). For further details on the steps within Zachariasen and Zemach's calculation see Kaiser, "Making Theory" (cit. n. 11), pp. 442–444. Zachariasen and Zemach improved on the closeness between their result and experimental data by including the exchange of three other particles in addition to the ρ in their calculation; Zachariasen's original calculation included only the pion-ρ interaction. Cushing treats some of the other early successes of Chew's *S*-matrix program in *Theory Construction,* pp. 145–151.

[21] Chew, "Nuclear Democracy and Bootstrap Dynamics" (cit. n. 4), p. 106.

understand what lay behind Chew's specific reading of them—let alone the fervor with which he turned the diagrams against their original field-theoretic birthplace?

Chew noted in his 1961 lectures that, more than anything else, "general philosophical convictions" (the details of which he left unstated) helped to guide him in his democratic reading of Feynman diagrams and in his conclusion that no particles were truly elementary. In these lecture notes, as elsewhere, Chew struggled to find a vocabulary that would support the conceptual breaks he aimed to make with quantum field theory. Finding the right terminology was no mean feat: "The language just didn't exist in physics," he later recalled. He remembers being "annoyed" by other physicists' sloppy terminology—it made him "gag"—"because the language wasn't there, there were no words, there was no way to say it."[22] The language he did produce to express his new physical concepts—his recurring incantations of "nuclear democracy," "equal treatment," and "equal participation"—therefore had to come from somewhere other than the stable repertoire of his fellow physicists. Guided in part by Chew's conscious choices of language, I will attempt in the next two sections to clarify what he called his "general philosophical convictions" and to study their evolution over time. In order to unpack Chew's nuclear democracy, we must begin not with his flipped Feynman diagrams of the early 1960s but, rather, with the battles fought in Berkeley over domestic anti-Communism, beginning in 1948.

## II. GEOFFREY CHEW: A SCIENTIST'S POLITICS OF DEMOCRACY IN 1950s AMERICA

Geoffrey Chew, born in 1924, came of age in a generation of American theoretical physicists just after that of Richard Feynman and Julian Schwinger. Growing up in Washington, D.C., where his father worked in the U.S. Department of Agriculture, Chew graduated from high school at the age of sixteen. Four years later he completed his college education with a straight-A record from George Washington University. It being 1944, one of his undergraduate advisors, George Gamow, helped to arrange for Chew to head straight to Los Alamos, where he joined Edward Teller's special theoretical division, working on early ideas for a hydrogen "superbomb." Entering graduate school in February 1946 at the University of Chicago, Chew completed his doctorate in less than two and a half years, under the tutelage of Enrico Fermi. Ph.D. in hand, he and his fellow graduate student Marvin "Murph" Goldberger headed off to postdocs under Robert Serber at Berkeley's Radiation Laboratory. Before long, Berkeley's physics department took notice of Chew, who was already developing a reputation for both brilliance and clarity. The physics department appointed Chew as an assistant professor, to begin in the fall of 1949.[23] Chew's transit from the East Coast to the West Coast included some of the best stops along the way for young physicists at that time, and by the age of twenty-five he had arrived, this physicist's manifest destiny complete. (See Figure 4.)

[22] Chew, *S-Matrix Theory of Strong Interactions* (cit. n. 3), p. 4 ("general philosophical convictions"); and Chew, interview with Gordon, Dec. 1997, quoted in Gordon, "Strong Interactions," pp. 31–32.

[23] These biographical details are taken from Raymond Birge to Dean A. R. Davis, 27 Feb. 1949, in Raymond Thayer Birge Correspondence and Papers, call number 73/79c, Bancroft Library, University of California, Berkeley (hereafter cited as **Birge Papers**); and A. C. Helmholz to Dean Lincoln Constance, 25 Mar. 1957, Birge Papers, Box 40, Folder "Letters written by Birge, January–May 1957." Some clarification may be helpful here. Letters written by Birge are filed chronologically. The items cited in this essay are from Boxes 39 and 40; explicit folder titles will not be cited. Letters written to Birge (and other pertinent materials in this collection) will be cited with box number and folder titles. For further biographical information see also Birge, "History," Vol. 5, Ch. 19, pp. 43–51; and DeTar *et al.,* eds., *Passion for Physics.*

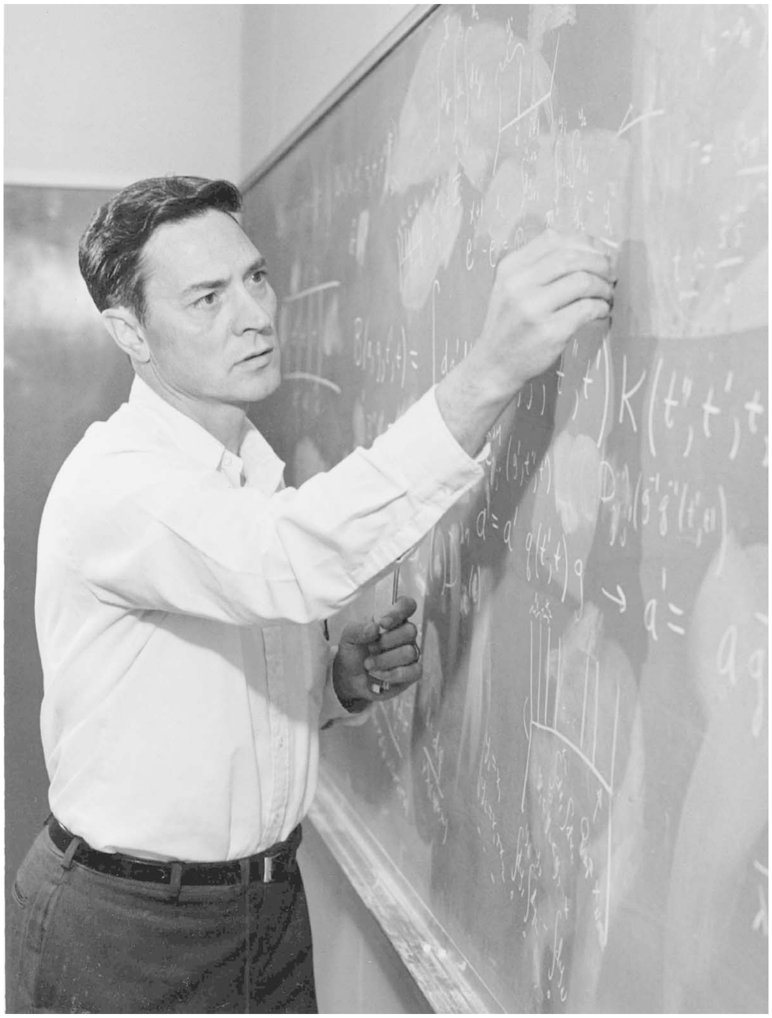

***Figure 4.*** *Geoffrey Chew, circa 1960. Lawrence Berkeley National Laboratory, University of California, Berkeley. Courtesy of the Emilio Segrè Visual Archives, American Institute of Physics.*

This simple picture of professional progress grew complicated, however, nearly as soon as Chew was hired. Chew became increasingly engaged with political issues in the late 1940s, continuing with greater and greater intensity throughout the 1950s. His activities took him on an extended orbit that began in Berkeley and, seven years later, brought him back there again. His reasons for leaving Berkeley in 1950 can be understood only if we consider the political situation in which physicists found themselves soon after World War II and how Berkeley physicists, in particular, experienced the early years of the Cold War. The fast-moving descent into McCarthyism affected daily life in Berkeley's Radiation Laboratory and Department of Physics, shaping hallway discussions, straining old friendships, and altering many young physicists' career paths.

### *Politics and Physics at Berkeley, 1949–1954*

Few American physics departments experienced the pains of transition to the postwar political scene more abruptly, or more publicly, than that at the Berkeley campus of the University of California. The House Un-American Activities Committee (HUAC) turned its sights on atomic espionage directly on the heels of its sensational probe of alleged Communists in the film industry.[24] Its first stop: Berkeley's Radiation Laboratory, built up to international prominence during the 1930s by Ernest O. Lawrence. During the war, Lawrence's famed laboratory on the hill had been staffed with teams endeavoring to separate the scarce, fissionable uranium-235 isotope from its more ubiquitous cousin, uranium-238. Some of this staff, HUAC began to insinuate nearly as soon as Chew arrived at the Rad Lab as a postdoc in 1948, had been "red."

Startling headlines greeted students and faculty returning to Berkeley in September 1948: a physicist who had worked at the Radiation Laboratory during the war, identified by HUAC only as "Scientist X," purportedly had leaked vital atomic secrets to the Soviets. Five physicists who had worked in the wartime Radiation Laboratory were singled out for intensive questioning. Though no evidence of espionage at the Rad Lab was ever uncovered, all were convicted of contempt of Congress and one of perjury, based on his testimony. Each lost his job immediately upon being indicted by Congress.[25]

By the time HUAC began its investigation, only one of these Rad Lab physicists remained in Berkeley. In September 1949 his case grabbed local attention when Berkeley's student newspaper, the *Daily Californian,* reported the front-page story, "T.A. Queried on Communist Ties." I. David Fox, the teaching assistant in question, had worked at the Radiation Laboratory during the war and was currently a graduate student in Berkeley's physics department. Fox refused to name names for his HUAC investigators, invoking the

[24] The literature on HUAC and McCarthyism is, of course, vast. On McCarthyism and American higher education in particular see esp. Ellen Schrecker, *No Ivory Tower: McCarthyism and the Universities* (New York: Oxford Univ. Press, 1986); Sigmund Diamond, *Compromised Campus: The Collaboration of Universities with the Intelligence Community, 1945–1955* (New York: Oxford Univ. Press, 1992); James Hershberg, *James B. Conant: Harvard to Hiroshima and the Making of the Nuclear Age* (Stanford, Calif.: Stanford Univ. Press, 1993), Chs. 19, 21–23, 31; Noam Chomsky *et al.,* eds., *The Cold War and the University: Toward an Intellectual History of the Postwar Years* (New York: New Press, 1997); Catharine M. Hornby, "Harvard Astronomy in the Age of McCarthyism" (A.B. thesis, Harvard Univ., 1997), esp. Ch. 2; Jessica Wang, *American Science in an Age of Anxiety: Scientists, Anticommunism, and the Cold War* (Chapel Hill: Univ. North Carolina Press, 1999); Lawrence Badash, "Science and McCarthyism," *Minerva,* 2000, *38:*53–80; Naomi Oreskes and Ronald Rainger, "Science and Security before the Atomic Bomb: The Loyalty Case of Harald U. Sverdrup," *Studies in History and Philosophy of Modern Physics,* 2000, *31:*309–369; and Schweber, *Shadow of the Bomb* (cit. n. 9). On the politicization of American scientists before World War II see Peter J. Kuznick, *Beyond the Laboratory: Scientists as Political Activists in 1930s America* (Chicago: Univ. Chicago Press, 1987).

[25] On the establishment of the Radiation Laboratory see John Heilbron and Robert Seidel, *Lawrence and His Laboratory: A History of the Lawrence Berkeley Laboratory,* Vol. 1 (Berkeley: Univ. California Press, 1990). On the HUAC investigation of the Rad Lab physicists see the San Francisco–area newspaper clippings in Department of Physics, University of California, Berkeley, Records, ca. 1920–1962, call number CU-68, Bancroft Library (hereafter cited as **Dept. Physics, Berkeley, Records, ca. 1920–1962**), Folder 4:12; "Thomas Committee Calls Ex-Instructor," *Daily Californian,* 22 Sept. 1948, p. 5; Louis Bell, "'No Great Surprise'; Identity of Scientist X Suspected Here," *ibid.,* 3 Oct. 1949, p. 1; "Ex-Physicist at U.C. Held for Perjury," *ibid.,* 26 May 1952, p. 1; and Schrecker, *No Ivory Tower,* pp. 126–148. The specifics of the legal case against the Rad Lab physicists are outlined in Carl Beck, *Contempt of Congress* (New Orleans, La.: Hauser, 1959), pp. 65–70. Two other cases involving Berkeley scientists also caught the attention of HUAC and the national media in 1948: those of Martin Kamen and E. U. Condon. See Martin Kamen, *Radiant Science, Dark Politics: A Memoir of the Nuclear Age* (Berkeley: Univ. California Press, 1985), Chs. 11, 12; and Jessica Wang, "Science, Security, and the Cold War: The Case of E. U. Condon," *Isis,* 1992, *83:*238–269. Condon was at the time the head of the National Bureau of Standards, having formerly been an undergraduate and graduate student at Berkeley and a consultant at the Berkeley Rad Lab during World War II. See also Schweber, *Shadow of the Bomb.*

Fifth Amendment twenty-five times. Three months later, the Board of Regents of the University of California dismissed Fox, without making any formal charges against him.[26] Even before Senator Joseph McCarthy had begun the anti-Communist activities with which his name forever will be associated, physicists in Berkeley were feeling the brunt of McCarthyism.

The politics of domestic anti-Communism invaded physics departments far beyond Berkeley in the ensuing months and years. Physicists across the country debated the new proposal, in 1949, to require full background security checks for all recipients of Atomic Energy Commission graduate student fellowships. Thirty-four Berkeley physics graduate students wrote to the *San Francisco Chronicle* to protest what they saw as the proposed exclusion from training of "those among us who hold unpopular viewpoints," arguing that "education in a democracy must be available to everyone." The next year, after the final, contentious establishment of the National Science Foundation (NSF), prominent scientists such as Berkeley's iconic physics department chair Raymond Birge objected to congressional discussion that considered prohibiting NSF grants to members, past or present, of any organization listed on the attorney general's "subversive" list.[27]

With the outbreak of fighting in Korea that June, department chairs such as Birge found more and more of their time devoted to draft deferments for their students and young faculty and to problems involving personnel security clearances.[28] Berkeley physicists, like their colleagues across the country, found themselves routinely denied passports for foreign travel; meanwhile, foreign physicists experienced insulting delays or even rejections of their applications for visitors' visas. The Rad Lab, since 1947 under the auspices of the Atomic Energy Commission, was no longer permitted to have foreign scientists conduct any work there—classified or unclassified, paid or not.[29]

[26] "T.A. Queried on Communist Ties," *Daily Californian,* 28 Sept. 1949 [misprinted as 1948], p. 1 (see also the *Daily Californian* stories on 4 Jan. 1950, p. 1; 27 Mar. 1950, p. 7; and 31 Mar. 1950, pp. 1, 4); Schrecker, *No Ivory Tower,* pp. 126–127; and David Gardner, *The California Oath Controversy* (Berkeley: Univ. California Press, 1967), pp. 91–94.

[27] For the graduate students' protest see Letter to the Editor, *San Francisco Chronicle,* 27 May 1949; I. David Fox was among the signers. See also Birge to Lyman Spitzer, Jr., 26 May 1949, Birge Papers; E.R. [Eugene Rabinowitch], "The 'Cleansing' of AEC Fellowships," *Bulletin of the Atomic Scientists,* June–July 1949, *5:*161–162; "The Fellowship Program: Testimony before the Joint Committee," *ibid.,* pp. 166–178; "Loyalty Tests Cause Cut in AEC Fellowship Program," *ibid.,* Jan. 1950, *6:*32; "The Curtailment of the AEC Fellowship Program," *ibid.,* pp. 34, 62–63; "Loyalty Tests for Science Students?" *ibid.,* Apr. 1950, *6:*98; and Wang, *American Science in an Age of Anxiety* (cit. n. 24), Ch. 7. On the debates over the founding of the NSF see Daniel Kevles, "The National Science Foundation and the Debate over Postwar Research Policy, 1942–1945," *Isis,* 1977, *68:*5–26; Kevles, *The Physicists: The History of a Scientific Community in Modern America,* 2nd ed. (1978; Cambridge, Mass.: Harvard Univ. Press, 1987), Chs. 11–12; Nathan Reingold, "Vannevar Bush's New Deal for Research; or, The Triumph of the Old Order," *Hist. Stud. Phys. Biol. Sci.,* 1987, *17:*299–344; and Jessica Wang, "Liberals, the Progressive Left, and the Political Economy of Postwar American Science: The National Science Foundation Debate Revisited," *Hist. Stud. Phys. Sci.,* 1995, *26:*139–166. On the issue of the attorney general's list of "subversive" organizations and NSF grants see Birge to Robert G. Sproul, 14 Mar. 1950, Birge Papers.

[28] On draft deferments see Birge to R. C. Gibbs, 10 Aug. 1950, and Birge to Local Board No. 62, Santa Clara County, 8 June 1953, Birge Papers; and David Kaiser, "Putting the 'Big' in 'Big Science': Cold War Requisitions, Scientific Manpower, and the Production of American Physicists after World War II," unpublished MS. The concerns with draft deferments for physics students persisted well after fighting had ceased in Korea; see the correspondence from 1958 in the American Institute of Physics, Education and Manpower Division, Records, 1951–1973, Box 4, Folder "Scientific Manpower Commission, Washington, D.C." These records are held in the American Institute of Physics, Niels Bohr Library, College Park, Maryland, call number AR15. On security clearance troubles see Birge to K. K. Darrow, 11 Jan. 1955, Birge Papers; Wang, *American Science in an Age of Anxiety;* Ellen Schrecker, *The Age of McCarthyism: A Brief History with Documents* (Boston: Bedford, 1994), pp. 37–40, 150–164; and Adam Yarmolinsky, ed., *Case Studies in Personnel Security* (Washington, D.C.: Bureau of National Affairs, 1955).

[29] Regarding passport and visa problems see Birge to Darrow, 26 May 1955; Birge to Congressman Francis

Not all of the reactions to these fast-moving events were glum. Five years after the HUAC investigation of purported espionage, Berkeley student reporters concluded their five-part series on the Radiation Laboratory in December 1953 with the light-hearted story "Espionage at the Rad Lab—Naw!" After describing dozens of "scattered instruments painted a bright red with the white letters 'USSR' printed on them," the reporters explained that the letters stood for "United States surplus reserve," an old joke up at the laboratory. The gag, and visitors' stunned reactions to it, could still evoke "guffaws" from "the six foot, amiable, loose-jointed physicist" who gave these student reporters their tour.[30]

Still, the levity could only go so far. Birge remained more somber, hesitating before accepting his nomination as vice-president of the American Physical Society (APS). Writing in May 1953 to the society's secretary, Karl Darrow, he recalled a former "time when the American Physical Society was concerned only with physics. At the present time, however, I am afraid it is concerned almost as much with politics as it is with physics and I must say I do not like politics." Almost exactly one year later, just as Birge was preparing to assume the presidency of the APS, he and his Berkeley department were stunned over the decision by the Atomic Energy Commission to deprive J. Robert Oppenheimer, Berkeley's former star theorist and the world-famous "father of the atomic bomb," of his security clearance.[31]

These many developments shaped physicists' experiences across the country in the late 1940s and 1950s; reactions ranged from graduate student protests to the hand-wringing of the president of the American Physical Society. At Berkeley, however, all of these events took shape in the shadow of a local situation that dominated hallway discussions and faculty meetings for the better part of a decade: the "loyalty oath" controversy at the University of California. As we will see, the loyalty oath helped to prompt Geoffrey Chew's own political engagement as he struggled to define a working definition of "democracy" for scientists in Cold War America.

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Walter, 15 July 1955; and Birge to Senator Harley Kilgore, 17 Nov. 1955: Birge Papers. The Federation of American Scientists focused on passport and visa problems; many of their efforts were reported in the *Bulletin of the Atomic Scientists,* a special issue of which (Oct. 1952, *8*) was dedicated especially to these problems. On the exclusion of foreign scientists from the Rad Lab see Birge to Walter Thirring, 8 Jan. 1952, Dept. Physics, Berkeley, Records, ca. 1920–1962, Folder 5:117. On the establishment of the AEC national laboratory system see Richard Hewlett and Francis Duncan, *A History of the United States Atomic Energy Commission,* Vol. 2: *Atomic Shield, 1947–1952* (University Park: Pennsylvania State Univ. Press, 1969), Ch. 8; Robert Seidel, "A Home for Big Science: The Atomic Energy Commission's Laboratory System," *Hist. Stud. Phys. Sci.,* 1986, *16:*135–175; and Peter Westwick, "The National Laboratory System in the U.S., 1947–1962" (Ph.D. diss., Univ. California, Berkeley, 1999).

[30] Sandra Littlewood and Skip Garretson, "Espionage at the Rad Lab—Naw!" *Daily Californian,* 11 Dec. 1953, p. 8. It is interesting to note that this was the only segment of the five-part series that Birge clipped and saved with his other newspaper clippings, which may be found in Dept. Physics, Berkeley, Records, ca. 1920–1962, Folder 4:12.

[31] Birge to Darrow, 22 May 1953, Birge Papers; Atomic Energy Commission, *In the Matter of J. Robert Oppenheimer: Transcript of Hearing before Personnel Security Board* (Washington, D.C.: Atomic Energy Commission, 1954); Philip Stern, *The Oppenheimer Case: Security on Trial* (New York: Harper & Row, 1969); John Major, *The Oppenheimer Hearing* (New York: Stein & Day, 1971); and Barton J. Bernstein, "'In the Matter of J. Robert Oppenheimer,'" *Hist. Stud. Phys. Sci.,* 1982, *12:*195–252. Of course, not all members of the Berkeley department were shocked by the news; some had even lobbied behind the scenes to ensure the outcome. On Berkeley involvement in and reactions to the hearing see "Oppenheimer Conflict: Former Professor Center of Dispute," *Daily Californian,* 15 Apr. 1954, p. 1; Birge to Edwin A. Uehling, 28 Mar. 1955, Birge Papers; Birge, "History," Vol. 5, Ch. 17; Nuel Pharr Davis, *Lawrence and Oppenheimer* (New York: Simon & Schuster, 1968), Chs. 8–10; Luis W. Alvarez, *Alvarez: Adventures of a Physicist* (New York: Basic, 1987), pp. 179–181; and A. Carl Helmholz with Graham Hale and Ann Lage, *Faculty Governance and Physics at the University of California, Berkeley, 1937–1990* (Berkeley: Regional Oral History Office, Bancroft Library, 1993), pp. 152–157, 276–279.

### *The California Loyalty Oath*

Only a few months before David Fox was dismissed from his Berkeley teaching-assistant position, the Board of Regents of the University of California imposed a new "loyalty oath" on all university employees. This anti-Communist oath was drafted hastily during the lunch break of an otherwise routine monthly meeting of the regents on 25 March 1949, with only eleven of the twenty-four regents in attendance; these eleven adopted it unanimously. As historians such as David Gardner and Ellen Schrecker have emphasized, the new oath sprang more from questions regarding self-governance than from overriding fears on most of the regents' part about actual Communist infiltration of the university system. The California State Legislature was at that moment considering a proposal to wrest control over adjudicating the "loyalty" of the university faculty from the regents; enacting the new oath offered one way for the regents to show the legislature that it could manage such issues on its own. Soon the oath set off a long and bitter struggle between the regents and the faculty over this same question of self-governance: most members of the faculty claimed that the regents' act violated the faculty's traditional role of choosing its own members and, when necessary, keeping them in line.[32]

Adding to the furor, the regents failed to inform the faculty of the new requirement until mid-June 1949—over two months after the oath officially had been adopted and at just the time when many faculty were leaving Berkeley for the summer. The oath required all university employees—from janitorial staff to graduate student teaching assistants to tenured faculty—to swear that they were not members of the Communist Party; each signing had to be witnessed by a notary public. Letters to faculty members soon revealed that their "reappointment," and the payment of their salaries, was now conditional upon their signing the oath—even for those who believed that tenure had already removed all questions of reappointment.[33]

A vocal minority of the faculty, including several professors who had fled European dictatorships before and during World War II, immediately decried the oath as an infringement on academic freedom. Yet during the fall of 1949, in the midst of reports of the Soviets' detonation of their own atomic bomb and the "fall" of China to Communist leaders, the objections of most faculty members did not concern the ban on Communists per se. Instead, they opposed the fact that university employees had been singled out and held to an oath more stringent than that required of other state employees. Hundreds of faculty members at first refused to sign the oath in protest, a recalcitrance that, in turn, only strengthened the view of certain key regents that university faculties could not be trusted to govern themselves. Crowds of fifty to two hundred professors met each week at Berkeley's faculty club throughout the academic year 1949/1950 to discuss tactics and strategies.[34]

[32] Gardner, *California Oath Controversy* (cit. n. 26); and Schrecker, *No Ivory Tower* (cit. n. 24), pp. 116–125. See also George Stewart, *The Year of the Oath* (New York: Doubleday, 1950); Birge, "History," Vol. 5, Ch. 19; and Helmholz with Hale and Lage, *Faculty Governance,* pp. 96–97, 152–157. Gardner cautions against reading the regents' enactment of the oath too narrowly as a direct reaction to the state legislature's proposals, which, he concludes, provided only a clearly articulated measure of the "mood" of the times: Gardner, *California Oath Controversy,* p. 10.

[33] See the forms and notices in Dept. Physics, Berkeley, Records, ca. 1920–1962, Folder 3:41; and Gardner, *California Oath Controversy,* pp. 52–54.

[34] One of the early faculty leaders who spoke out against the oath because of what he perceived to be ties to fascist practices was Ernst Kantorowicz, a German-born medieval historian. See Grover Sales, Jr., "The Scholar and the Loyalty Oath," *San Francisco Chronicle,* 8 Dec. 1963, pp. 27–30, written soon after Kantorowicz's death. Robert Serber recalled that Gian Carlo Wick, a physicist at Berkeley who refused to sign the oath, said

Meanwhile, by February 1950 the regents' line had hardened: any university employee who had not signed the oath by the end of April would be dismissed. Alumni and the president of the university managed to pry a compromise from the regents, who agreed that the cases of all nonsigners would be reviewed on an individual basis by the faculty's Committee on Privilege and Tenure. Yet when North Korea invaded South Korea in late June 1950 and the United States entered the conflict, most of the remaining faculty holdouts simply signed the oath. Dismissing the faculty committee's recommendations, the regents then fired the remaining thirty-one "non-signers" on 25 August 1950. Even though none of those fired had ever been accused by the faculty or the regents of Communist Party membership or sympathy, their fates drew national attention to the question (and, as many proclaimed, the danger) of Communists in the classroom.[35]

The ensuing seven-year battle between the fired nonsigners and the regents, weaving in and out of the California State Supreme Court, left no department on the California campuses untouched. Yet few felt the full brunt of the controversy more, or in more ways, than Berkeley's Department of Physics. It is difficult to overestimate the effects of the oath controversy on daily life within the department. The department secretary had to rush off lists of graduate students who had signed the oath after an initial "oversight" to prevent their termination as course graders or teaching assistants. The examining committees for several students' dissertation defenses had to be rearranged at the last minute, since the regents ruled that faculty nonsigners could no longer serve on such committees. Birge circulated a memorandum to the department's faculty in early April 1950, cautioning them against putting their personal opinions regarding the oath in writing or even discussing them "at a meeting of a fairly large group." Writing to university president Robert Sproul in the midst of the controversy, Birge found himself wondering whether the "wave of hysteria now sweeping the country," as evidenced locally by the loyalty oath disaster, might even put "the entire democratic structure of this country . . . in some danger."[36] Whether interpreted ultimately as an anti-Communist witch-hunt, a principled fight over academic freedom, or a local power play pitting faculty against administration, the effects of the loyalty oath controversy on mundane daily life were palpable to students and faculty alike.

Several members of the physics department did more than just absorb the oath's after-

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at the time that "he had been coerced into taking an oath once before in Italy, where he had to swear loyalty to Mussolini; he said he'd regretted it ever since and wasn't going to make the same mistake twice." Robert Serber with Robert Crease, *Peace and War: Reminiscences of a Life on the Frontiers of Science* (New York: Columbia Univ. Press, 1998), p. 171. Another Berkeley physicist, Emilio Segrè, on the other hand, later wrote that he had signed at least fifteen loyalty oaths while in Mussolini's Italy; thus he found them all meaningless, and therefore, as a practical matter, he saw little reason not to sign the California oath as well. In fact, as he put it, "I even remembered a pronouncement by Pope Pius XI, elicited by a Fascist oath, explicitly stating that under certain circumstances one could take such oaths with mental reservations that made them void. I dug the papal document out in the library and translated it, and some colleagues to whom I had sent it posted it in Los Alamos, which administratively depended on the Regents of the University of California. At Berkeley it circulated less openly." Emilio Segrè, *A Mind Always in Motion: The Autobiography of Emilio Segrè* (Berkeley: Univ. California Press, 1993), pp. 235–236. On the weekly meetings in the faculty club see Gardner, *California Oath Controversy,* p. 87.

[35] Gardner, *California Oath Controversy,* Chs. 5–6; and Schrecker, *No Ivory Tower* (cit. n. 24), pp. 120–122.

[36] O. Lundberg, University Controller, memo to "chairmen of departments, administrative officers, and others concerned," 27 Nov. 1950; RLY [Rebekah Young], Physics Department Secretary, to Lundberg, 30 Nov. 1950; M. A. Stewart, Associate Dean of the Graduate Division, memo to Physics Department Graduate Advisers, 14 Dec. 1950: Dept. Physics, Berkeley, Records, ca. 1920–1962, Folder 3:41. Rebekah Young to Robert Serber, 18 July 1951; and Serber to Young, 25 July, 1951: Dept. Physics, Berkeley, Records, ca. 1920–1962, Folder 3:4. Birge, "Memorandum to Members of the Physics Department Staff," 6 Apr. 1950; and Birge to Sproul, 14 Mar. 1950: Birge Papers.

effects. From the start, the department included active members on all sides of the controversy. The teaching assistant David Fox—who, ironically, had signed the oath—was nonetheless touted by certain regents as an all-too-visible reminder of the need to enforce loyalty among the faculty. Professor Francis Jenkins joined the "Operating Committee of Seven" to help coordinate faculty opposition to the oath and later served on the faculty's Committee on Privilege and Tenure. Robert Brode, also a senior professor in the department, served as the official custodian of the funds raised by the Operating Committee of Seven, which were intended in part to help pay the expenses of the nonsigners who had stopped receiving their salaries. And department chair Raymond Birge often played the part of back-room negotiator, pushing certain people (such as Wendell Stanley, later a Nobel laureate in biology) into visible positions on faculty committees, while presenting a series of prepared remarks against the regents' actions before Academic Senate and departmental meetings.[37]

On the other side, the physics department's Victor Lenzen, chair of the Committee on Privilege and Tenure for the northern section of the Academic Senate, helped to engineer the composition of this crucial committee by removing all nonsigners before agreeing to retire from the committee himself. In less explicit ways, Ernest Lawrence, Edwin McMillan, Luis Alvarez, and others, while remaining "aloof" from the campus-wide discussions of the controversy, created an "atmosphere" at the Radiation Laboratory that convinced several physicists that neither nonsigners nor their supporters would be welcome there. For young physicists at the Rad Lab, there was little room left for doubt: postdocs who had not signed the oath found notes on their desks on 30 June 1950, informing them that they had to turn in their badges and keys, clear off their desks, and leave by the end of that day.[38]

These many different types of participation in the controversy complicated daily life in the physics department. Even more concretely, Berkeley's physics department fell victim to the oath in losing six faculty members within one year. Two professors, Harold Lewis and Gian Carlo Wick, allowed themselves to be fired in August 1950, when the regents finally dismissed all remaining nonsigners. By June 1951 four other professors—Robert Serber, Wolfgang "Pief" Panofsky, Howard Wilcox, and Geoffrey Chew—had resigned in protest. Among those who left were all of the department's theoretical physicists. The very first to resign from the physics department over the issue—and perhaps the first in the entire university—was Geoffrey Chew.[39]

[37] Birge, "History," Vol. 5, Ch. 19, pp. 1–15; and Gardner, *California Oath Controversy* (cit. n. 26), pp. 123–124, 171–172. Some of Birge's speeches at Academic Senate and physics department faculty meetings are reprinted in Birge, "History," Vol. 5, Ch. 19, pp. 8–12.

[38] Gardner, *California Oath Controversy,* p. 248; reference to the "atmosphere" at the Rad Lab comes from Geoffrey Chew's letter of resignation, quoted below. See also Helmholz with Hale and Lage, *Faculty Governance* (cit. n. 31), pp. 96–97, 153; Serber with Crease, *Peace and War* (cit. n. 34), pp. 171–172; and Segrè, *Mind Always in Motion* (cit. n. 34), pp. 234–237. Jack Steinberger was one of the postdocs dismissed on 30 June 1950 for not signing the oath. See Jack Steinberger, "A Particular View of Particle Physics in the Fifties," in *Pions to Quarks,* ed. Brown *et al.* (cit. n. 13), pp. 307–330, esp. p. 311; and Steinberger, "Early Particles," *Annual Review of Nuclear and Particle Science,* 1997, *47:*xiii–xlii, esp. pp. xxxix–xl.

[39] Birge suggests that Chew was "apparently" the first professor to resign from all of the University of California over the oath controversy in Birge, "History," Vol. 5, Ch. 19, p. 45. Robert Serber recounts his own decision to leave in Serber with Crease, *Peace and War,* pp. 171–172. Serber had endured an extended, and at times hostile, personal security review in 1948, though perhaps because of his close affiliation with Ernest Lawrence his case did not stir the same media attention as the HUAC Rad Lab investigation did (*ibid.,* pp. 162–165). For more on Serber's continuing security woes see Barton Bernstein, "Interpreting the Elusive Robert Serber: What Serber Says and What Serber Does Not Explicitly Say," *Stud. Hist. Phil. Mod. Phys.,* 2001, *32:*443–486. Wolfgang Pauli kept abreast of the developments in Berkeley's physics department via his friend Erwin

### *Geoffrey Chew and the Politics of Democracy*

Though no one on the faculty could have known it at the time, Chew's official letter of appointment to his assistant professorship arrived exactly one week after the regents secretly passed the new loyalty oath requirement.[40] Chew, who still maintained Q clearance to work on classified nuclear weapons projects, refused to sign what he called, in a letter to Oppenheimer, "the objectionable part of the new contract," which seemed to him to threaten "the right of privacy in political belief." He became further frustrated with what he saw as weak attempts by the rest of the faculty to fight the oath. At the end of his very first year of teaching Chew acted on his convictions, becoming the first person to resign from the physics department over the issue. As he explained to Birge in July 1950, one month before the regents finally dismissed the remaining nonsigners, Chew had decided "to get away from an intimidating and precarious situation."[41]

The firing of David Fox, Chew told Birge, had shown beyond doubt that the regents seemed bent on removing from the faculty the "right" to "maintain its own qualifications." The regents' actions with the oath, furthermore, aimed at nothing less than to "root out the last resistance" among the faculty. The few signs of "faculty solidarity" with the nonsigners had all but vanished when fighting broke out in Korea in June 1950. As Chew pressed Birge one month later, "In a war-time situation, what security can a non-conformist have?" With the outbreak of fighting, the few remaining nonsigners on campus "have now become lepers who must keep out of sight." On top of this, Chew reported that the Radiation Laboratory, which was "the chief stimulus" of his scientific work, had made it clear that it "does not welcome non-signers. Even if I were allowed to maintain my affiliation [with the laboratory], the unsympathetic atmosphere would not be pleasant. This would be a more subtle form of intimidation." Though Chew found it a difficult decision, he left Berkeley in July 1950 and accepted a position at the University of Illinois in Urbana. He was promoted from assistant to associate professor within a year and became a full professor at Urbana in 1955, at the age of thirty-one.[42]

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Panofsky, a senior art historian at the Institute for Advanced Study, whose son Wolfgang was one of the experimentalists to leave Berkeley's department because of the loyalty oath. In October 1950 Pauli forwarded to the elder Panofsky news that he had heard from the young theorist J. M. Luttinger. Pauli quoted from Luttinger in his letter to Panofsky: "Apart from Physics, the atmosphere is very unpleasant in Berkeley. Both [Gian Carlo] Wick and [Harold] Lewis have been fired for refusing to sign a Loyalty Oath, and both (so far as I know) are fighting the case in court. They have only a very slim chance of winning—on the whole it is a degrading business. *In addition to that the lab is full of secret work, and is overrun by petty officials and bureaucracts of all kinds.*" Wolfgang Pauli to Erwin Panofsky, 23 Oct. 1950, in Wolfgang Pauli, *Wissenschaftlicher Briefwechsel,* ed. Karl von Meyenn (New York: Springer, 1996), Vol. 4, Pt. 1, p. 179.

[40] Birge notes that Dean A. R. Davis sent the official letter of appointment to Chew on 1 Apr. 1949 (Birge, "History," Vol. 5, Ch. 19, p. 45); the regents enacted the new loyalty oath in their meeting on 25 Mar. 1949. While still a postdoc at the Rad Lab, Chew had delivered several talks on his research to the physics department, both formal and informal, so that Birge could introduce Chew as already "well known" at the Sept. 1949 departmental meeting. See Birge's handwritten notes, "First Dept. meeting, Wed., Sept. 28, 1949," Dept. Physics, Berkeley, Records, ca. 1920–1962, Folder 2:4.

[41] Geoffrey Chew to J. Robert Oppenheimer, 11 May 1950, quoted in Birge, "History," Vol. 5, Ch. 19, p. 45 (Birge also notes discussions with Chew over his frustration with Academic Senate resolutions regarding the oath); and Chew to Birge, 24 July 1950, Birge Papers, Box 5, Folder "Chew, Geoffrey Foucar, 1924–."

[42] Chew to Birge, 24 July 1950; and Birge, "History," Vol. 5, Ch. 19, p. 47. Chew's emphasis on the importance of the Korean war is echoed in several historians' recent studies of postwar American science policy. See Daniel Kevles, "Cold War and Hot Physics: Science, Security, and the American State, 1945–56," *His. Stud. Phys. Biol. Sci.,* 1990, *20:*239–264; Hershberg, *James B. Conant* (cit. n. 24), Chs. 27–28; Wang, *American Science in an Age of Anxiety* (cit. n. 24), Ch. 8; and Kaiser, "Putting the 'Big' in 'Big Science'" (cit. n. 28). The University of Illinois also had a standing loyalty oath requirement at the time Chew accepted his job there, but one that did not mention the Communist Party or any other group by name. Birge and University of California president

A few months after he left Berkeley, and after the regents dismissed the thirty-one remaining nonsigners, Chew reported on the struggle in the *Bulletin of the Atomic Scientists.* Like most of the faculty at the University of California, Chew objected that the faculty had been singled out and subjected to a more specific loyalty oath than was required for any other state employees. The fact that the regents then went beyond this, to threaten and eventually dismiss tenured faculty, constituted a further violation of "the cornerstone of academic freedom." He refrained from detailing any of his own experiences, reviewing instead the positions taken by the university president, the Academic Senate, and various factions within the Board of Regents. The controversy had been fanned in part, Chew explained, by what he called "fundamentalists," people who struck principled stands on questions like academic freedom even though they had lived uncomplainingly since 1942 with an official university policy excluding Communists from teaching there. The "moral of this very sad story," Chew concluded, was that more explicit procedures needed to be defined for tenure. The rights and roles of the faculty, the Academic Senate, and the Board of Regents needed similar attention and explication, to guarantee that faculty would not be singled out for special treatment again in the future. The procedures of due process might then guard against a repeat of "the present sad and humiliating situation."[43]

Over the course of the 1950s, while teaching in Illinois, Chew became more and more active in what has been called "the atomic scientists' movement." Soon after arriving on campus, Chew founded Urbana's local branch of the Federation of American Scientists (FAS), a national organization dedicated to moderate liberal causes. As Jessica Wang has detailed, by the late 1940s the FAS had become largely a bureaucratic organization, collecting information about some of the more severe abuses of McCarthyism and lobbying certain legislators for reform. In part because of pressure from HUAC and the FBI, the FAS had begun to refrain from its earlier pattern of public demonstrations by the time Chew joined the group, choosing instead the route of "quiet diplomacy."[44]

Chew participated directly in this FAS diplomacy, both on the Illinois campus and, soon, as a visible leader within the national organization. After founding the local FAS branch, Chew immediately began inducting friends and colleagues, such as his fellow physicist Francis Low. As Low recalls, Chew strode up to him soon after he arrived in Urbana and asked simply, "Okay, are you ready to join FAS?" "I was happy to do it," Low continues. "I thought it was a good organization. Geoff's position was very good, and I was happy to take part in it. It was a serious time." Under Chew's direction, the group organized monthly meetings on campus and hosted speakers on topics like the Fifth Amendment. On Chew's initiative, they also became a clearinghouse for campus-wide complaints about unfair treatment, such as problems in obtaining passports. Soon Chew's activities extended

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Sproul found it ironic that Chew would agree to go to Illinois, but Chew explained that in Illinois this was "the same oath required of all state employees . . . [and] no one feels it to be a restriction on his political activity. . . . The intent of the trustees, therefore, does not seem inimicable [*sic*] to academic freedom." In other words, as far as Chew was concerned, the Illinois oath did not single out faculty for special treatment or unfair scrutiny. Chew to Birge, 24 July 1950.

[43] Geoffrey Chew, "Academic Freedom on Trial at the University of California," *Bull. Atom. Sci.,* Nov. 1950, *6:*333–336, on p. 336. The objection that faculty were singled out for closer scrutiny than other people was a common one among Berkeley faculty. Chew noted this in passing on p. 334 of his article; he gave it a more extended discussion in his letter to Birge of 24 July 1950. See also Birge, "History," Vol. 5, Ch. 19; Gardner, *California Oath Controversy* (cit. n. 26), Ch. 3; and Schrecker, *No Ivory Tower* (cit. n. 24), pp. 122–123.

[44] See esp. Wang, *American Science in an Age of Anxiety* (cit. n. 24). On the founding and early years of the FAS see also Alice Kimball Smith, *A Peril and a Hope: The Scientists' Movement in America, 1945–7* (Chicago: Univ. Chicago Press, 1965).

beyond the Urbana campus. In November 1955 he testified before a U.S. Senate subcommittee as chair of the FAS Passport Committee. The FAS objected to the State Department's unwritten policy of denying passports to scientists for political reasons and of further denying the applicants any rights of due process or means of appeal. Usually passports were denied with no reasons given, and appeals met delays lasting months and even years. One of the most famous cases at the time concerned Linus Pauling, who finally received a passport in 1954—after more than two years of attempts—when he applied to go to Sweden to receive his Nobel Prize in chemistry.[45]

In his testimony before the Senate Subcommittee on Constitutional Rights, Chew used several lesser-known cases to lobby for fairer treatment. A passport, he urged, must be "recognized as a right of the U.S. citizen, not merely a privilege." Due process must attend all dealings with passport applications, and only problems with demonstrated relevance to national security issues should result in denials. Applicants denied passports should be supplied with an explicit list of charges against them and given the opportunity for a prompt appeals hearing, at which "confrontation of witnesses and no concealment of evidence, should apply." All appeals hearings should be transcribed and copies made available to all parties. Most important of all, Chew argued, a channel outside of the State Department should be set up to handle further appeals: "We should like to see a well-defined channel" established, "so that applicants will have no uncertainty as to what to do." Both the loyalty oath and the passport situation convinced Chew that only "well-defined channels," operating under due process, could protect the equality and rights of academics. Just as he had explained in his report about the California loyalty oath, Chew labored to make it clear for the committee of senators during his 1955 testimony that academics, and scientists in particular, should neither be singled out for "special privileges" nor subject to special scrutiny or bias.[46] Clear and unambiguous procedures needed to be established so that disagreements would be settled fairly, providing equal treatment to all those affected. With these safeguards in place, Chew believed, scientists could participate in a democratic America as citizens, each equal under the law.

### III. PEDAGOGICAL REFORMS: "SECRET SEMINARS" AND "WILD MERRYMAKING"

Chew's pedagogical efforts in the years following his congressional testimony resonated with his more explicitly political activities. Demonstrating the same attitudes as in his lobbying with the FAS, Chew endeavored to make certain that graduate students could work in such a way that none was singled out unfairly and all were encouraged to participate equally. His activities with his own graduate students shaped his approach to

[45] Francis Low, interview with the author, MIT, 11 Apr. 2001; David Kaiser, "Francis E. Low: Coming of Age as a Physicist in Postwar America," *Physics @ MIT,* 2001, *14:*24–31, 70–77, on pp. 71–72; and "Summary of Testimony of Linus Pauling," *Bull. Atom. Sci.,* Jan. 1956, *12:*28.

[46] Geoffrey Chew, "Passport Problems," *Bull. Atom. Sci.,* Jan. 1956, *12:*26–28, on p. 28. This article includes Chew's testimony from 15 Nov. 1955. See also "FAS Congressional Activity in 1955," *ibid.,* p. 45. The specific items Chew lobbied for were conspicuously absent in all kinds of hearings from this period, having been denied to witnesses in HUAC hearings, local security-clearance boards, and often even university committees. See Wang, *American Science in an Age of Anxiety* (cit. n. 24); Schrecker, *No Ivory Tower* (cit. n. 24); and Ellen Schrecker, *Many Are the Crimes: McCarthyism in America* (New York: Little, Brown, 1998). The FAS was quite active during the mid 1950s on the issue of passports and visas for scientists. The entire issue of the *Bulletin of the Atomic Scientists* for Oct. 1952 was dedicated to the topic. See also E.R. [Eugene Rabinowitch], "How to Lose Friends," *Bull. Atom. Sci.,* Jan. 1952, *8:*2–5; Victor Weisskopf, "Visas for Foreign Scientists," *ibid.,* Mar. 1954, *10:*68–69, 112; "American Visa Policy: A Report," *ibid.,* Dec. 1955, *11:*367–373; and John Toll, "Scientists Urge Lifting Travel Restrictions," *ibid.,* Oct. 1958, *14:*326–328.

enlisting collaborators for his autonomous *S*-matrix program. Years later, when his program lay largely abandoned by most particle physicists, Chew continued to assess the turnaround using the language of democratic participation.

### *Chew's "Little Red Schoolhouse" in Berkeley*

While neglecting several large issues concerning tenure, academic freedom, and the legality of state-imposed loyalty oaths, the California Supreme Court ruled in favor of the dismissed nonsigners in October 1952, ordering that the regents reappoint them. This court decision, however, left unresolved the question of back pay, and so for several people the oath controversy lumbered on. This last issue was settled by the court, again in the nonsigners' favor, only in the spring of 1956. Yet as early as 1951, certain senior professors in Berkeley's physics department began to consider how best to lure Chew back to Berkeley. Soon after the first court decision was handed down, Birge tried to entice Chew to return. Reluctantly, Chew decided to stay in Illinois, which had made him a very generous counteroffer on hearing of Berkeley's actions. Still excited by the prospect of returning to Berkeley's stimulating campus, however, Chew spent the spring semester of 1957 there as a visiting professor. Eager to keep Chew in Berkeley, the new department chair, Carl Helmholz, performed some impressive financial gymnastics to convince the administration that it could afford to hire Chew as a full professor. Helmholz's schemes worked, and Chew accepted an appointment as a full professor, beginning in the 1957/1958 academic year.[47]

Immediately Chew began advising a large and growing group of graduate students within the department. Its size was especially notable in that Chew and all of his students were theoretical physicists, for whom working in large groups was still unusual. Often ten or more students would be under Chew's wing at a time, and Chew himself would be engaged in collaborative work with four or five of them; postdocs and research associates made the group even larger. A steady stream of Chew's students completed their Berkeley dissertations beginning in 1959, often with four or five finishing each year.[48] In choosing to train his students in this manner, Chew followed a pattern similar to that set by Oppen-

[47] With regard to Chew's return to Berkeley see the handwritten notes between Francis Jenkins, Robert Brode, and Raymond Birge, undated, ca. Apr. 1951, Dept. Physics, Berkeley, Records, ca. 1920–1962, Folder 5:25; on Chew's 1953 offer to return to Berkeley see Chew to Birge, 21 Apr. 1953, Birge Papers, Box 5, Folder "Chew, Geoffrey Foucar, 1924–"; and on his 1957/1958 appointment see Helmholz to Constance, 25 Mar. 1957, Birge Papers, Box 40, Folder "Letters written by Birge, January–May 1957." Helmholz's financial jockeying becomes clear in both *ibid.* and Helmholz to Chancellor Clark Kerr, 5 Mar. 1957, Dept. Physics, Berkeley, Records, ca. 1920–1962, Folder 1:26. The reappointment of the nonsigners was conditional on their signing a new statewide loyalty oath, the so-called Levering oath, which was even more explicitly anti-Communist than the original university oath had been. The key difference was that the Levering oath was imposed on all state employees, so that university faculty were no longer singled out for special treatment. See Gardner, *California Oath Controversy* (cit. n. 26), pp. 250, 253–254; and Schrecker, *No Ivory Tower,* pp. 123–125.

[48] Many of Chew's former colleagues and students recalled that his group was unusually large and that he still made time to work carefully with each of them. See Birge, "History," Vol. 5, Ch. 19, p. 51; Frazer, "Analytic and Unitary S Matrix" (cit. n. 15), p. 7; Georgella Perry, "My Years with Professor Chew," in *Passion for Physics,* ed. DeTar *et al.,* pp. 14–16, on p. 15; Steven Frautschi, "My Experiences with the S-Matrix Program," *ibid.,* pp. 44–48, on p. 44; Carleton DeTar, "What Are the Quark and Gluon Poles?" *ibid.,* pp. 71–78, on p. 77; David Gross, "On the Uniqueness of Physical Theories," *ibid.,* pp. 128–136, on p. 128; C. Edward Jones, "Deducing T, C, and P Invariance for Strong Interactions in Topological Particle Theory," *ibid.,* pp. 189–194, on p. 189; William Frazer, interview with the author, 7 July 1998; Jerome Finkelstein, interview with the author, 24 July 1998; Eyvind Wichmann, interview with the author, 13 Aug. 1998; and Henry Stapp, interview with the author, 21 Aug. 1998 (all interviews were conducted in Berkeley). A list of Chew's former graduate students, together with their years of graduation, appears in Frazer, "Analytic and Unitary S Matrix," pp. 7–8.

heimer at Berkeley in the 1930s: the students worked collectively, discussing their research projects regularly with the entire group. The large, close-knit group format contrasted starkly with the approach of Julian Schwinger, for example, who famously advised ten or more Harvard graduate students at a time during the 1950s and 1960s but met with any of them individually only rarely—and never with the whole group.[49]

Whereas Oppenheimer could intimidate students and colleagues alike with his notoriously sharp tongue, Chew's students uniformly recall a much more encouraging advisor, one who, in the words of a former student, "treat[ed] us as full partners in a common effort." In a further gesture of equality, Chew regularly joined the group for informal lunches in the Rad Lab cafeteria. Chew took Oppenheimer's pedagogical model a step further when he instituted what came to be known as the "secret seminar." His entire group of students met weekly to hear presentations from one another; often the meetings took place at Chew's house. The seminar sessions were "secret" because faculty members (other than Chew) were actively discouraged from attending: the goal was to make certain that no graduate students were too intimidated to participate equally with their peers. From deep within Lawrence's sprawling Radiation Laboratory, the original site of American "big science," Chew carved out what one of his former students described as a "little red schoolhouse."[50]

This "little red schoolhouse" approach also shaped how Chew and some of his Berkeley colleagues organized a special conference on the strong interactions, held in Berkeley in December 1960. As handwritten notes from an early planning meeting reveal, Chew, Carl Helmholz, Donald Glaser, and the other members of the committee wanted their conference to bring "new people up to date" on the status of strong-coupling particle physics. As emphasized in these notes, the conference was to be "non-exclusive." Minutes from this planning meeting likewise noted that graduate students' research, as part of the work of Berkeley's department, "should be strongly represented" at the conference.[51]

Meeting these goals would not be easy: physics conferences on special topics were rarely aimed at bringing nonspecialists up to speed, much less highlighting the contribu-

[49] On Oppenheimer's pedagogical approach see Robert Serber, "The Early Years," *Phys. Today,* Oct. 1967, *20:*35–39; Serber with Crease, *Peace and War* (cit. n. 34), Ch. 2; Alice Kimball Smith and Charles Weiner, eds., *Robert Oppenheimer: Letters and Recollections* (Cambridge, Mass.: Harvard Univ. Press, 1980), Ch. 3; and Kevles, *Physicists* (cit. n. 27), pp. 216–219. In 1958 Schwinger was technically advising sixteen Harvard graduate students; see "1958–59 Department Lists," in Department of Physics, Harvard University, Correspondence, 1958–60, Box A–P, Folder "1958–59 Department Lists," call number UAV 691.10, Harvard University Archives, Pusey Library, Cambridge, Massachusetts. Bryce DeWitt, who completed his dissertation under Schwinger in 1949, talked about Schwinger's style with me during several discussions; William Frazer raised the contrast between Chew and Schwinger during our interview.

[50] Gross, "Uniqueness of Physical Theories" (cit. n. 48), p. 128 ("full partners"). Both Carleton DeTar and Steven Frautschi recalled the lunches during their interviews with Stephen Gordon; see Gordon, "Strong Interactions," pp. 27–28. Details on Chew's "secret seminars" come from Frautschi, "My Experiences with the S-Matrix Program" (cit. n. 48), p. 44; A. Capella *et al.,* "The Pomeron Story," in *Passion for Physics,* ed. DeTar *et al.,* pp. 79–87, on pp. 86–87; and my interviews with Frazer, Finkelstein, Wichmann, and Mandelstam. The term "little red schoolhouse" comes from an interview between Carleton DeTar and Stephen Gordon, conducted May 1997, and quoted in Gordon, "Strong Interactions," p. 29. Several more of Chew's former students with whom Gordon spoke also recalled Chew's "secret seminar." In the 1930s Berkeley's physics department held informal weekly seminars, attended by faculty and graduate students alike, though this single department-wide meeting disappeared after World War II. Carl Helmholz and Howard Shugart discussed these older seminars during my interviews with them in Berkeley on 14 July 1998 and 29 July 1998, respectively.

[51] See the handwritten notes, dated Mar. 1960, on "Special Meeting APS," in Dept. Physics, Berkeley, Records, ca. 1920–1962, Folder 1:39, and the typed minutes from a planning meeting held on 4 Mar. 1960, in the same folder. The handwritten notes are probably by either Carl Helmholz, chair of the department at this time and head of the conference-planning committee, or Howard Shugart, who was a secretary to the conference-planning committee; the notes appear to match Helmholz's handwriting.

tions of graduate students. The Berkeley committee gained some help from the well-known MIT theorist Victor Weisskopf, who had worked in the FAS with Chew throughout the 1950s. As a member of the planning committee for the APS-sponsored special meeting in Berkeley, Weisskopf lobbied hard to obtain additional funding from the National Science Foundation so that the Berkeley conference could indeed include these younger participants. "You can well understand and I am sure you agree," Weisskopf urged, "that such conferences with open attendance are very important for the stimulation of young people or other people who are new in the field." Such openness was especially needed in particle physics, he continued: "The field of high-energy physics is, as you know, very strongly in the hands of a clique and it is hard for an outsider to enter. The Rochester conferences were the only conferences that dealt with that subject and they limited it to invited people only. The Berkeley conference is supposed to break this custom." "Open" meetings, intended for newcomers and students as much as for members of a "clique," were unusual in 1960. It took work on the part of Chew, Helmholz, Weisskopf, and the others to break the mold and keep their meeting "non-exclusive." The "open" meeting attracted about three hundred physicists.[52]

The theoretical portion of this special conference focused almost exclusively on recent work by Chew, Stanley Mandelstam, and Richard Cutkosky on a new framework for approaching particle physics. Just six months before Chew's more outspoken break with quantum field theory at the La Jolla meeting, the material discussed at the "open" Berkeley meeting helped to form the core of Chew's emerging *S*-matrix program. As Chew came to articulate more and more explicitly, the *S*-matrix approach relied on several general principles but eschewed much of the specific formalism of quantum field theory.[53]

### *The* S *Matrix and a Democracy of Practitioners*

The independence of the *S*-matrix program from many of the esoteric niceties of field theory was given a doubly "democratic" spin by Chew as he championed the new approach and quickly spread its gospel far and wide. First, Chew began to argue for his concept of "nuclear democracy"—that all nuclear particles, subject to the strong nuclear force, should be treated equally, without dividing them into "elementary" and "composite" factions. As we have seen in Section I, Chew argued for this democratic treatment, and his related notion of the bootstrap, largely on the basis of his unusual interpretation of Feynman diagrams. In the rotated diagrams of Figure 3, for example, Chew and his students saw

[52] Victor Weisskopf to J. Howard McMillen, 14 Mar. 1960, Birge Papers, Box 29, Folder "Weisskopf, Victor Frederick, 1908–." As it turned out, the NSF refused any financial aid because the Berkeley meeting was under the auspices of the APS; additional funding for the meeting was provided by the AEC and United States Air Force. See the typed report "Conference on Strong Interactions," undated, Dept. Physics, Berkeley, Records, ca. 1920–1962, Folder 1:39; an unsigned, undated postconference report in this folder gives the attendance figure. The Rochester conferences on nuclear and particle physics began in 1950 at the University of Rochester; when they began to move to different venues during the mid 1950s, they still retained the name "Rochester conference." See, e.g., John Polkinghorne, *Rochester Roundabout: The Story of High-Energy Physics* (New York: Freeman, 1989). Like Chew, Weisskopf became quite active with the FAS during the 1950s; while Chew chaired the Passport Committee, Weisskopf headed the Visa Committee. See Victor Weisskopf, "Report on the Visa Situation," *Bull. Atom. Sci.,* Oct. 1952, *8:*221–222; and Weisskopf, "Visas for Foreign Scientists," *ibid.,* Mar. 1954, *10:*68–69, 112.

[53] Schedules and reports on the presentations at the 1960 Berkeley meeting may be found in Dept. Physics, Berkeley, Records, ca. 1920–1962, Folder 1:39. The general principles on which Chew and his collaborators hoped to build their non-field-theoretic *S*-matrix theory included analyticity, unitarity, Lorentz invariance, and crossing symmetry, not all of which are independent from each other. See esp. Chew, *S-Matrix Theory of Strong Interactions* (cit. n. 3); and Cushing, *Theory Construction,* Chs. 5–7.

the ρ meson move from force-carrier to bound-state composite to seemingly elementary particle. Arguing from the structure of these Feynman diagrams, Chew taught his many students to treat all nuclear particles the same way. But his "democratic" sentiment did not end with the new interpretation of Feynman diagrams; Chew fashioned his *S*-matrix work as "democratic" in a second sense as well. In addition to "democratic" diagrams, Chew championed a "nuclear democracy" among practitioners. Consider his remarks near the close of an ebullient invited lecture at the 1962 New York meeting of the American Physical Society: "I am convinced that a wild period of merrymaking lies before us. All the physicists who never learned field theory can get in the game, and experimenters are just as likely to come up with important ideas as are theorists. They may even have an advantage over us." Chew returned to this theme of nonexperts' advantage over field-theory experts in the *S*-matrix realm in a lecture at Cambridge University in 1963, reporting that "the less experienced physicists [have] an advantage in working with a new framework. (The inverse correlation of productivity with experience in a situation like this is remarkable.)" Meanwhile, Chew made good on his pledge to bring outsiders into the fold, delivering special lectures and seminars on the new material especially for experimentalists at Berkeley. (See Figure 5.) The 1963 *S*-matrix textbook by Chew's colleagues Roland Omnès and Marcel Froissart, *Mandelstam Theory and Regge Poles,* similarly carried the subtitle "An Introduction for Experimentalists."[54]

As Chew traveled around the country and beyond, feverishly working his campaign of "wild merrymaking," few could miss his obvious charisma and enthusiasm. As John Polkinghorne later recalled, "We used to call Geoff Chew 'the handsomest man in high energy physics.' I know of at least one senior secretary in a British physics department who kept a photograph of him near her desk. That frank and open face, with just a hint of his one-eighth Burmese ancestry, and his tall commanding figure, made him one of the few theorists in the pin-up class." Rumors spread far and wide that Chew had given up a potential career in professional baseball to work in particle physics. His personal charm and enthusiasm made Chew into an effective "salesman." Polkinghorne attests that "Geoff was definitely a man from whom one would be happy to buy a used car." His talks "were always eagerly awaited," given "their inspirational and encouraging tone."[55]

[54] Geoffrey Chew, "*S*-Matrix Theory of Strong Interactions without Elementary Particles," *Reviews of Modern Physics,* 1962, *34:*394–401, on p. 400 ("merrymaking"); Chew, "The Dubious Role of the Space-Time Continuum in Microscopic Physics," *Science Progress,* 1963, *51:*529–539, on p. 538 (nonexperts' advantage) (this article contains the text of Chew's 1963 Rouse Ball Lecture at Cambridge); and Roland Omnès and Marcel Froissart, *Mandelstam Theory and Regge Poles: An Introduction for Experimentalists* (New York: Benjamin, 1963) (Froissart spent time working with Chew's group in Berkeley during the early 1960s). Owen Chamberlain in particular recalled Chew's special lectures for experimentalists; see Owen Chamberlain, "Interactions with Geoff Chew," in *Passion for Physics,* ed. DeTar *et al.,* pp. 11–13, on p. 13. Chew's unusual ability and interest in instructing experimentalists was by this time long standing. As a visiting professor at Berkeley in 1957, Chew gave a seminar on the pion-nucleon interaction, which drew an unusual number of experimentalists from the Radiation Laboratory and from Livermore, in addition to the graduate students enrolled in the course. Carl Helmholz reported that the experimentalists "are getting considerable benefit from it even though the subject is quite abstract and mathematical": Helmholz to Constance, 25 Mar. 1957 (cit. n. 47). William Frazer also discussed Chew's informal seminar for experimentalists during our interview.

[55] John Polkinghorne, "Salesman of Ideas," in *Passion for Physics,* ed. DeTar *et al.,* pp. 23–25, on p. 23. Polkinghorne worked on *S*-matrix theory while based in Cambridge, England. For a similar analysis of the role of charisma in modern physics see Charles Thorpe and Steven Shapin, "Who Was J. Robert Oppenheimer? Charisma and Complex Organization," *Social Studies of Science,* 2000, *30:*545–590. Owen Chamberlain and Georgella Perry (Chew's former secretary) both mentioned Chew's baseball pretensions: Chamberlain, "Interactions with Geoff Chew," pp. 12–13; and Perry, "My Years with Professor Chew" (cit. n. 48), pp. 14–16. Several other physicists recalled these same rumors during interviews with Stephen Gordon: Gordon, "Strong Interactions," p. 15.

***Figure 5.*** *Geoffrey Chew delivering an informal lecture at Berkeley, 1961. Reprinted with kind permission of the Lawrence Berkeley National Laboratory.*

His enthusiasm quickly suffused Berkeley's department and encouraged his graduate students and postdocs to participate as equals in the *S*-matrix campaign. Louis Balázs, who completed his Ph.D. under Chew's direction in the mid 1960s, reminisced recently that "it was an exciting experience being one of Chew's graduate students at UC-Berkeley in the early 1960s. New ideas were being discussed and developed continually and vigorously, particularly by the postdocs, and it seemed we were on the threshold of a new era in Physics." Those who were pushing the boundaries of the "new era" seemed to be drawn to Berkeley, if they weren't there already: "There was a constant stream of distinguished visitors," Balázs continued, "who seemed to be eager to learn about the new developments in Berkeley." And just as Chew had announced so exuberantly at the 1962 meeting, Balázs too recalled that "even graduate students found that they could make new independent contributions at that time." Another former graduate student, William Frazer, concurs, emphasizing that Chew worked hard to make sure that no students felt intimidated. "He really made a wonderful atmosphere for us to work in."[56] Both students who were still "innocent" of the elaborate quantum field theory formalism, as Chew put it, and experimentalists who had "never learned field theory" stood to contribute as equals in Chew's *S*-matrix program.

[56] Louis Balázs to David Kaiser, 6 Aug. 1998; and Frazer interview. See also Perry, "My Years with Professor Chew," pp. 15–16.

Chew reflected in several places on the best way to train these potential *S*-matrix contributors. As early as 1961, he noted that students who had not learned quantum field theory, and the usual ways of using and interpreting Feynman diagrams, seemed to fare best when approaching the new *S*-matrix material. He assured readers of his 1961 lecture note volume that "it is . . . unnecessary to be conversant with the subleties of field theory, and a certain innocence in this respect is perhaps even desirable. Experts in field theory seem to find current trends in *S*-matrix research more baffling than do nonexperts." The same sentiment appeared five years later, when Chew remarked in the preface to his 1966 textbook: "No background in quantum field theory is required. Indeed, as pointed out in the preface to my 1961 lecture notes, lengthy experience with Lagrangian field theory appears to constitute a disadvantage when attempting to learn *S*-matrix theory."[57]

Chew's own students largely followed this prescription. William Frazer, the first student to complete his dissertation under Chew after his return to Berkeley, worked through Eyvind Wichmann's "very rigorous field theory course" as a graduate student, though he found his work with Chew to be much more interesting. "For the student," Frazer recalled recently, "life was a bit schizophrenic. Either you read on your own the very dry mathematical structure of axiomatic field theory, or you tried to follow the more exciting material." Later students, such as Jerome Finkelstein, recalled that "there were quite a few of us in Chew's group who really did not spend a lot of time on field theory. I'd taken a course in it during my first or second year of graduate school, but that was all." Ramamurti Shankar, another of Chew's students from this time, recently put a humorous turn on this pedagogical approach:

> I had a choice: either struggle and learn field theory and run the risk of blurting out some four letter word like $\varphi^4$ in Geoff's presence or simply eliminate all risk by avoiding the subject altogether. Being a great believer in the principle of least action, I chose the latter route. Like Major Major's father in *Catch 22,* I woke up at the crack of noon and spent eight and even twelve hours a day not learning field theory and soon I had not learnt more field theory than anyone else in Geoff's group and was quickly moving to the top.

David Gross, who completed his dissertation under Chew's direction in 1966, paraphrased an often-heard refrain from colleagues who didn't realize that Gross, since the 1970s a preeminent field theorist, had come from Chew's group: "Funny—you don't look Chewish." One would search in vain to find much usage of specifically field-theoretic techniques in the sixty dissertations by Chew's graduate students.[58]

So much for Chew's immediate circle in Berkeley. With several well-known texts on quantum field theory already in print, the next question was how to spread the physics of "nuclear democracy" beyond Chew's large but geographically limited Berkeley group. Just as politicians debated a purported "missile gap" with the Soviets, Chew and his collaborators in the early 1960s faced a "textbook gap": given that so many physicists conceivably could participate in developing the "democratic" *S*-matrix program, the challenge was to reach these students, experimentalists, and other theorists and deliver the *S*-matrix message.

[57] Chew, *S-Matrix Theory of Strong Interactions* (cit. n. 3), pp. vii–viii; and Geoffrey Chew, *The Analytic S Matrix: A Basis for Nuclear Democracy* (New York: Benjamin, 1966), p. v.

[58] Frazer interview; Finkelstein interview; Ramamurti Shankar, "Effective Field Theory in Condensed Matter Physics," in *Conceptual Foundations of Quantum Field Theory,* ed. Cao (cit. n. 4), pp. 47–55, on p. 47; and Gross, "Uniqueness of Physical Theories" (cit. n. 48), p. 128. Dissertations by Chew's students from 1959 to 1983 may be found in the Berkeley Physics Department Library.

Toward this goal, Chew and many of his *S*-matrix students and collaborators delivered many sets of summer-school lectures—sometimes, like Chew in 1960, giving the same set of lectures at two different schools in the same summer.[59]

Chew and his postdocs also began to publish their lecture notes in inexpensive editions, nearly as soon as the lectures had been delivered. These books themselves reveal much about the quest to attract students to the *S*-matrix team. Most of the important *S*-matrix textbooks were part of the "Frontiers in Physics" series, which began to publish collections of lecture notes and reprints in 1961. Chew's 1961 *S-Matrix Theory of Strong Interactions,* based on his 1960 summer-school lectures, was one of the first books to be included in the new series. By 1964 *S*-matrix tracts constituted nearly one-third of the "Frontiers in Physics" books, even though the series was meant to treat all aspects of physics and not only particle theory.[60]

These books were rushed into print. *Regge Poles and S-Matrix Theory,* by Chew's postdoc Steven Frautschi, stemmed from lectures Frautschi had given once at Cornell University in 1961/1962 and then augmented for delivery at a June 1962 summer school. The lectures presented by Maurice Jacob and Chew in their 1964 *Strong-Interaction Physics* also had been delivered only once, during the academic year 1962/1963. As the series editor David Pines explained, "Frontiers in Physics" was intended to feature just this kind of "rough and informal" lecture notes rather than polished monographs. The very production of the books reflected their mission, as Pines explained: "Photo-offset printing is used throughout, and the books are paperbound, in order to speed publication and reduce costs. It is hoped that the books will thereby be within the financial reach of graduate students in this country and abroad."[61]

The progress of *S*-matrix theorists toward establishing an axiomatic foundation for their new work can also be read immediately from the material form of their books. Chew's early reports on the developing theory in 1961 and 1964 were printed in books that had not been carefully typeset but were printed on inexpensive paper and hurried into print. Chew's first textbook to treat the newly proposed axiomatic version of *S*-matrix theory, *The Analytic S Matrix* (1966), on the other hand, was not published as part of the "Frontiers in Physics" series. Its professional typesetting and glossy pages present a clear contrast with the earlier volumes.[62]

From summer-school lectures to "Frontiers in Physics" volumes, then, the task of cre-

[59] Chew's "Double Dispersion Relations and Unitarity as the Basis of a Dynamical Theory of Strong Interactions" (cit. n. 17) appeared in both *Relations de dispersion et particules élémentaires,* ed. de Witt and Omnès (cit. n. 14), pp. 455–514, and in *Dispersion Relations,* ed. Screaton (cit. n. 14), pp. 167–258. See also the "Editor's Note" *ibid.,* p. viii. One might compare Chew and his "missionaries" with Niels Bohr and the spread of the "Copenhagen spirit" as studied in John Heilbron, "The Earliest Missionaries of the Copenhagen Spirit," *Revue d'Histoire des Sciences,* 1985, *30:*195–230.

[60] Lists of the titles published in the "Frontiers of Physics" series were included in the front of each of the books within the series. The *S*-matrix books in the series published between 1961 and 1964 included Chew, *S-Matrix Theory of Strong Interactions* (1961); Omnès and Froissart, *Mandelstam Theory and Regge Poles* (1963); Frautschi, *Regge Poles and S-Matrix Theory* (1963); E. J. Squires, *Complex Angular Momenta and Particle Physics* (1963); and Jacob and Chew, *Strong-Interaction Physics* (1964). During this same period, only two books that focused on quantum field theory for particle physics were included in the series.

[61] See the "Editor's Foreword" by David Pines, dated Aug. 1961, which appears within each of the volumes in the series. Frautschi's book, for example, was reproduced from hand-typed originals. Chew mentioned his friendship with Pines, also a physicist at the University of Illinois at Urbana, as one of the reasons he decided to publish his *S*-matrix lecture notes in this series: Chew to Kaiser, 11 May 1998 (email).

[62] This reading of the material production of Chew's textbooks is inspired by the example of David Cressy, "Books as Totems in Seventeenth-Century England and New England," *Journal of Library History,* 1986, *21:*92–106.

ating and maintaining a community of *S*-matrix theorists involved far more than publishing research articles in the *Physical Review.* Chew was well aware that the dissociation of Feynman diagrams from their circumscribed meaning within quantum field theory would be difficult for students who had already mastered quantum field theory to grasp. His autonomous *S*-matrix program promised opportunities for those students, experimentalists, and other theorists who had not become overly attached to quantum field theory. Inexpensive pedagogical resources, such as lecture notes and reprint collections, could thus serve to broaden the base of "democratic" *S*-matrix practitioners. There is some evidence that this aggressive, democratizing textbook campaign worked: one physicist explained years later that, although he had not been a direct student of Chew's, he had learned particular calculational details of the *S*-matrix program, together with some sense of its special "philosophy," from reading Chew's 1961 *S-Matrix Theory of Strong Interactions.*[63]

### *The Language of Democracy for a Program in Decline*

In addition to emphasizing his democratic team of contributors, Chew drew an increasing contrast during the 1960s and into the early 1970s between the kind of work his *S*-matrix program fostered as compared with that of the field theorists. The field theorists sought to explain the phenomena of particle physics with reference to a small set of basic or unit interactions taking place between a core set of "fundamental" or "elementary" particles. Chew mocked this approach of the "fundamentalists," arguing that such an "aristocratic" arrangement of fundamental particles could not provide an adequate framework for describing the strong interactions.[64] As in his 1950 article describing the loyalty oath controversy at the University of California, Chew reserved the term "fundamentalist" for colleagues who espoused a position at odds with his own "democratic" ideals.

By the late 1960s Chew's program appeared to many physicists to have lost its original focus, becoming mired in details and complexity. Treating the ρ meson within a democratic bootstrap framework was one thing, but moving beyond such a simple system to more complicated calculations had become much more frustrating. Quantum field theory, meanwhile, now augmented with a new emphasis on gauge symmetries and the quark hypothesis, was again attracting the attention of most particle physicists. Chew lamented this turn of events, arguing that it sprang more from physicists' "psychology" than from stubborn experimental data. The trouble, he wrote in the early 1970s, was that the "fundamentalists" dreamed "of the press conference that will announce to the world a dramatic resolution of their quest." Unlike the night thoughts of these fundamentalists, the autonomous *S* matrix was "the cumulative result of many steps stretching out over decades."[65]

Progress for the *S*-matrix camp, Chew explained, was necessarily a gradual game of

[63] Al Mueller, "Renormalons and Phenomenology in QCD," in *Passion for Physics,* ed. DeTar *et al.,* pp. 137–142, on p. 137.

[64] Chew used the language of "fundamentalists" throughout his articles "Hadron Bootstrap: Triumph or Frustration?" (cit. n. 18) and "Impasse for the Elementary-Particle Concept" (cit. n. 18). See also Chew, "'Bootstrap': A Scientific Idea?" (cit. n. 18).

[65] Chew, "Hadron Bootstrap," p. 24; and Chew, "Impasse for the Elementary-Particle Concept," p. 124. On the resurgence of quantum field theory see Cushing, *Theory Construction,* Chs. 6–7; Gordon, "Strong Interactions," Chs. 4–5; Andrew Pickering, *Constructing Quarks: A Sociological History of Particle Physics* (Chicago: Univ. Chicago Press, 1984), Chs. 4–8; Abraham Pais, *Inward Bound: Of Matter and Forces in the Physical World* (New York: Oxford Univ. Press, 1986), Ch. 21; Tian Yu Cao, *Conceptual Developments of Twentieth-Century Field Theories* (New York: Cambridge Univ. Press, 1997), Chs. 8–11; and Lillian Hoddeson, Laurie Brown, Michael Riordan, and Max Dresden, eds., *The Rise of the Standard Model: Particle Physics in the 1960s and 1970s* (New York: Cambridge Univ. Press, 1997).

constructing more and more partial models, incorporating the effects of more and more particle exchanges and interactions, all under the rubric of several reigning general principles (such as causality). Chew's program was therefore built on the assumption, he wrote in 1974, "that there will gradually develop a more and more dense coverage of the nuclear world by interlocking models no single model having preeminent status. Such a pattern might be characterized as a 'democracy of models.' " Chew reminded physicists in 1970 that "even though no press conference was called," the stepwise construction of these interlocking models within the *S*-matrix program had already scored several "breakthroughs," a fact he attributed to the "brilliant collective achievement of the high-energy physics community." Reinforcing this point with a history lesson, Chew recalled the "precedent of classical nuclear physics": "This model enjoyed an aristocratic status for almost thirty years, but eventually it was democratized."[66]

This notion of constructing interlocking models through a series of "collective achievements" fit well with Chew's "secret seminar" approach to training graduate students. His students' dissertations reveal this close-knit, mutually buttressing approach: Bipin Desai, completing his dissertation in April 1961, built directly on work by James Ball in his dissertation, filed almost exactly one year earlier; Yongduk Kim's dissertation from June 1961 in turn drew explicitly on the work by Desai and Ball. This pattern continued throughout the 1960s. The abstract from Shu-yuan Chu's May 1966 dissertation, for example, explained that "the method [employed in the dissertation] is an extension of [C. Edward] Jones' proof in the single-channel case, making use of an explicit expression of the determinant D constructed by [David] Gross." Jones completed his dissertation in 1964 with Chew, and Gross submitted his dissertation a few months after Chu.[67] Citations to work, both published and unpublished, by other graduate-student members of Chew's group, and acknowledgments of extended discussions with fellow students, fill nearly every one of Chew's students' dissertations. Chew's pedagogical ideal of equal participation melded seamlessly with his program for the piecewise construction of "interlocking models" by a "collective" of researchers.

By the 1970s, with his program all but abandoned by most physicists, Chew bemoaned the failure of his collective vision to attract those physicists who insisted instead on searching for a single "glamorous-sounding fundamental entity": "Few of the stars of the world of physics are content with the thought that their labors will constitute only a *fraction* of a *vast mosaic* that must be constructed before the complete picture becomes recognizable and understandable." These "stars" of physics, now nearly all within the "fundamentalist" camp, routinely embraced only models based on Lagrangian field theories, Chew noted. They accorded these models "special status," failing to "consider on an equal footing" models not derived from such a field-theoretical basis. The task of the collective-minded *S*-matrix theorist, on the other hand, remained "to view any number of different partially successful models without favoritism."[68]

[66] Chew, "Impasse for the Elementary-Particle Concept," p. 124; and Chew, "Hadron Bootstrap," p. 25.

[67] James S. Ball, "The Application of the Mandelstam Representation to Photoproduction of Pions from Nucleons" (Ph.D. diss., Univ. California, Berkeley, 1960); Bipin Desai, "Low-Energy Pion-Photon Interaction: The ($2\pi$, $2\gamma$) Vertex" (Ph.D. diss., Univ. California, Berkeley, 1961); Yongduk Kim, "Production of Pion Pairs by a Photon in the Coulomb Field of a Nucleus" (Ph.D. diss., Univ. California, Berkeley, 1961); and Shu-yuan Chu, "A Study of Multi-Channel Dynamics in the New Strip Approximation" (Ph.D. diss., Univ. California, Berkeley, 1966). Ball's, Desai's, and Kim's dissertations drew heavily on the work by William Frazer and José Fulco, in particular "Effect of a Pion-Pion Scattering Resonance on Nucleon Structure" (cit. n. 15) and "Partial-Wave Dispersion Relations for the Process $\pi + \pi \rightarrow N + \underline{N}$" (cit. n. 15).

[68] Chew, "Impasse for the Elementary-Particle Concept" (cit. n. 18), p. 124 (emphasis added); and Chew, "Hadron Bootstrap" (cit. n. 18), p. 27.

The late 1960s and early 1970s were a difficult time to be a young physicist in the United States, over and above Chew's frustrations with the fate of "nuclear democracy." With the onset of détente and dramatic cuts in defense spending, U.S. physicists rapidly slid into the worst job shortage the profession had ever witnessed. Enrollments in the placement service registries of the American Physical Society tell the grim tale: in 1968, nearly 1,000 applicants fought for 253 jobs; the next year, almost 1,300 competed for 234 jobs. Only 63 jobs were on offer at a 1970 American Physical Society meeting at which 1,010 young physicists were looking for work; 1,053 competed for 53 jobs at the 1971 meeting. Anecdotal evidence suggests that students who were steeped too heavily in *S*-matrix methods, to the exclusion of field-theoretic techniques, felt the crunch disproportionately. Although many of Chew's students from this later period have gone on to academic careers as theoretical physicists, several *S*-matrix students left the field after earning their Ph.D.'s to become medical doctors or lawyers; others were denied tenure at places like MIT. Although it is difficult to disentangle the root causes of these few theorists' difficulties from the overwhelming across-the-board cutbacks, several physicists to this day continue to associate *S*-matrix training with job-placement difficulties during the late 1960s and early 1970s.[69]

In frustration as in triumph, Chew spoke of his program in distinctly "democratic" terms. In the early 1960s it had seemed open to all: everyone could participate equally, and no one was singled out for special privileges. Later, when the program fell into neglect, the language of Chew's complaints was likewise laden with the tropes of democratic participation. Field theorists improperly elevated an "aristocracy" of particles and granted "special status" only to certain kinds of models. *S*-matrix theorists, on the other hand, strove for an equality of particles, models, and practitioners, all judged "without favoritism" as members of a collective. "Patience" should be the order of the day, Chew wrote, not the yearning for press conferences and the special privileges (such as increased government funding) such singular attention could foster.[70]

In between his many speeches and lectures, Chew built an approach to training students and colleagues that emphasized equal participation and the collective strivings of the group over the "cliquish" dreams of the "fundamentalists." In establishing his "secret seminar," designing new lecture note volumes and textbooks, and delivering special lectures and seminars for experimentalists, Chew sought to make theoretical particle physics a particularly democratic activity. Just as he lobbied for fair treatment of academics and scientists under a controlling state in the Cold War, Chew tried to produce within physics a community of peers, neither singled out for special treatment nor splintered between idea-producing theorists and fact-checking experimentalists. Each contributor to the "vast mosaic" of *S*-matrix theory was to be an equal partner under the law.

## IV. THE VIEW FROM PRINCETON

Traces of Chew's outspoken stance on the failure of quantum field theory and on the need to treat all nuclear particles democratically may be found throughout his students' dissertations. Peter Cziffra, writing a year before Chew's famous La Jolla talk, echoed his ad-

[69] Several physicists drew these connections during their interviews with Stephen Gordon: Gordon, "Strong Interactions," pp. 50, 53–54. The statistics come from Kaiser, "Putting the 'Big' in 'Big Science'" (cit. n. 28), pp. 33–35.

[70] Chew, "Impasse for the Elementary-Particle Concept" (cit. n. 18), p. 125.

visor's attitude when he opened his dissertation by noting how "stymied" ordinary perturbative quantum field theory remained when treating the strong interactions. Well into the campaign for nuclear democracy, Akbar Ahmadzadeh reminded readers of his dissertation that insisting with the field theorists on a strict division between "elementary" and "composite" particles often leads to "absurd conclusions" when studying the strong interactions; instead, all particles should be treated as bound states, "on an equal footing." Henry Stapp, a research associate at the Rad Lab when Chew returned there in the late 1950s, pursued an axiomatic foundation for *S*-matrix theory in the early 1960s. "Early on," Stapp has recalled recently, "I was not really in close touch with Chew; I picked up the *S*-matrix ideas by osmosis, since Chew's ideas were permeating the area."[71]

Still, despite the group's successes in spreading the word via summer-school lectures and "Frontiers in Physics" volumes, their ideas did not "permeate" all departments of physics in the same way. Princeton's department, in particular, provides a telling contrast with Chew's Berkeley. By the early 1960s Princeton boasted a large and active group of theorists working on many aspects of particle physics. In fact, the Princeton group, like Chew's group in Berkeley, championed and extended many of the nonperturbative, diagram-based tools with which Chew was tinkering. Though a large group of theorists at Princeton pursued topics that fell under the *S*-matrix rubric, they did not share Chew's zeal for a "nuclear democracy." Comparing the work by these Princeton theorists with that of Chew's group may help, therefore, to highlight which elements of Chew's many-faceted "democracy" remained particular to his Berkeley group.

One of the leaders of Princeton's group had shared many early stops with Chew along a common trajectory. Marvin "Murph" Goldberger had been a graduate student with Chew in Chicago immediately after the war. The two became fast friends, sharing office space, arranging social outings together, completing their dissertations at the same time, and moving together to Berkeley's Rad Lab as postdocs in 1948. Following his postdoc Goldberger took a job back at the University of Chicago, while Chew taught for one year at Berkeley and then resigned over the loyalty oath. Once Chew landed in Urbana, he and Goldberger were again in close proximity; they struck up an active collaboration during the mid 1950s, together with Francis Low and Murray Gell-Mann, also both in the Midwest at that time. Just when Chew left Urbana to return to Berkeley, Goldberger left Chicago to take a position at Princeton, starting in February 1957. Though now separated by the length of the continent, Goldberger and Chew continued to correspond.[72]

[71] Peter Cziffra, "The Two-Pion Exchange Contribution to the Higher Partial Waves of Nucleon-Nucleon Scattering" (Ph.D. diss., Univ. California, Berkeley, 1960), p. 4; Akbar Ahmadzadeh, "A Numerical Study of the Regge Parameters in Potential Scattering" (Ph.D. diss., Univ. California, Berkeley, 1963), pp. 2–3; and Stapp interview. See Henry Stapp, "Derivation of the CPT Theorem and the Connection between Spin and Statistics from Postulates of the *S*-Matrix Theory," *Phys. Rev.,* 1962, *125:*2139–2162; Stapp, "Axiomatic *S*-Matrix Theory," *Rev. Mod. Phys.,* 1962, *34:*390–394; Stapp, "Analytic S-Matrix Theory," in *High-Energy Physics and Elementary Particles,* ed. Abdus Salam (Vienna: International Atomic Energy Agency, 1965), pp. 3–54; and Stapp, "Space and Time in *S*-Matrix Theory," *Phys. Rev.,* 1965, *139:*B257–270. Stanley Mandelstam, a close collaborator of Chew's and an architect of many of the *S*-matrix techniques, resisted following Chew and his group in renouncing field theory. As early as the Dec. 1960 Berkeley conference, a conference report noted that Chew's presentation based on Mandelstam's work "did not evoke Mandelstam's full assent": "Conference on Strong Interactions," p. 10, Dept. Physics, Berkeley, Records, ca. 1920–1962, Folder 1:39. See also Cushing, *Theory Construction,* pp. 131–132, 145.

[72] Annual Report 1956–1957, pp. 1–2, in Department of Physics, Princeton University, Annual Reports to the [University] President, Seeley G. Mudd Manuscript Library, Princeton Univ., Princeton, N.J. (hereafter cited as **Dept. Physics, Princeton, Annual Reports**); Marvin Goldberger, "Fifteen Years in the Life of Dispersion Relations," in *Subnuclear Phenomena,* ed. A. Zichichi (New York: Academic, 1970), pp. 685–693; Goldberger, "Francis E. Low—A Sixtieth Birthday Tribute," in *Asymptotic Realms of Physics,* ed. Alan Guth, Kerson Huang,

At Princeton Goldberger joined Sam Treiman, who had earned his Ph.D. from Chicago four years after Goldberger and Chew; a year and a half later, Richard Blankenbecler joined the group as a postdoc and later became a regular faculty member. Goldberger, Treiman, Blankenbecler, and their many graduate students spent much of their time during the late 1950s and 1960s on topics that Chew would have called *S*-matrix theory—just the sort of "interlocking models" that he hoped would bring clarity to the strong interaction. Goldberger and Treiman investigated the decay of unstable particles without resorting to field-theoretic Lagrangians; students completed dissertations on the analytic structure of scattering amplitudes and on how to incorporate unitarity, some of the key general principles from which Chew aimed to construct his autonomous *S*-matrix theory. On the surface, these projects all sound as if they could have been completed by Chew's students in Berkeley. Yet when Goldberger reported in 1961 on the Princeton group's many accomplishments, he categorized all of their work, with his characteristic sense of humor, as "the engineering applications of quantum field theory." "This work," Goldberger continued, "is complementary to the purer aspects of quantum field theory." Just at the time when Chew was announcing his clear and decisive break with quantum field theory in La Jolla, the Princeton group celebrated the close fit between their research and Chew's nemesis.[73]

The Princeton group's research was no closer to "engineering" than any of Chew's work; it only seemed more "applied" when compared with the work streaming out from Princeton's other group of theoretical particle physicists, headed by Arthur Wightman. Wightman championed an axiomatic approach to quantum field theory; his research remained at that time, unlike Chew's or Goldberger's, far removed from the details of recent experiments. No doubt with reference to Chew's standing-room-only invited lectures before the American Physical Society, the Princeton group reported in 1966 that "there are presently two approaches to relativistic quantum theory. These are axiomatic field theory and dispersion or S-matrix theory. Notwithstanding some passionate claims, there is not yet any evidence that the two are really different. . . . Both theoretical approaches have been and will continue to be pursued actively at Princeton."[74] Together with Wightman, then, Goldberger and Treiman erected a "big-tent" approach to quantum field theory: "engineering applications" based on *S*-matrix calculational techniques would coexist peacefully with the more "pure" investigations into the structure of quantum field theory. At Berkeley, meanwhile, there were no longer any senior field theorists in town to respond to Chew's challenge; the loyalty oath controversy had ensured that, when figures such as Gian Carlo Wick were fired as nonsigners. Years later, Chew mused on how he might have been more "intimi-

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and Robert Jaffe (Cambridge, Mass.: MIT Press, 1983), pp. xi–xv; and Goldberger, "A Passion for Physics," in *Passion for Physics,* ed. DeTar *et al.,* pp. 241–245. Andrew Pickering has examined the active collaboration of Chew, Low, Goldberger, and Gell-Mann, emphasizing the importance of their geographical proximity: Andrew Pickering, "From Field to Phenomenology: The History of Dispersion Relations," in *Pions to Quarks,* ed. Brown *et al.* (cit. n. 13), pp. 579–599.

[73] M. L. Goldberger and S. B. Treiman, "Decay of the Pi Meson," *Phys. Rev.,* 1958, *110:*1178–1184; on the fit between the Princeton work and Chew's see Goldberger, "An Outline of Some Accomplishments in Theoretical Physics," Annual Report 1960–1961, pp. 12–13, Dept. Physics, Princeton, Annual Reports. On other work in the department see Annual Report 1958–1959, pp. 2, 7; Annual Report 1960–1961, pp. 12–13; Annual Report 1962–1963, p. 4; Annual Report 1963–1964, p. 66; Annual Report 1964–1965, pp. 79–81; Annual Report 1965–1966, pp. 4–5; and Annual Report 1967–1968, pp. 23–24: Dept. Physics, Princeton, Annual Reports. See also Treiman, "A Connection between the Strong and Weak Interactions," in *Pions to Quarks,* ed. Brown *et al.,* pp. 384–389; and Treiman, "A Life in Particle Physics," *Ann. Rev. Nuclear Particle Sci.,* 1996, *46:*1–30.

[74] Arthur Wightman, "The General Theory of Quantized Fields in the 1950s," in *Pions to Quarks,* ed. Brown *et al.,* pp. 608–629; and Annual Report 1965–1966, pp. 4–5, Dept. Physics, Princeton, Annual Reports (quotation).

dated," and less likely to dismiss quantum field theory outright, had Wick still been in Berkeley. A similar restraint might have been exercised by Francis Low, with whom Chew worked in Urbana in the mid 1950s. Although Low credits Chew with having provided most of the original ideas during their fruitful collaboration, it is likely that if they had remained together in Urbana, Chew's later flamboyant pronouncements about the death of quantum field theory would have been muted by Low's impressive and authoritative grasp of field theory's tenets.[75]

In keeping with the double-barreled approach to field theory at Princeton, and in clear contrast to Chew's group in Berkeley, Goldberger's and Treiman's graduate students actively studied quantum field theory as an essential part of their training. Stephen Adler, who completed his Ph.D. under Treiman's direction in 1964, remembers auditing Wightman's course, since Wightman's work "was seen as undergirding" the calculational techniques of dispersion relations and *S*-matrix theory. "We were doing an evasive physics," Adler continued: since no one knew how best to treat the strong interactions, "we used whatever methods we could. . . . Dispersion relations were a tool, but we also learned field theory methods because they were useful for treating symmetries." Other Princeton students from this period similarly recall an emphasis on learning quantum field theory.[76] This peaceful coexistence of *S*-matrix-style calculations with axiomatic quantum field theory also led to a different appraisal of Chew's *S*-matrix program than the one "permeating" the Berkeley area.

Adler recalls that when Chew came to Princeton to give a talk, "he sounded very messianic." Adler was hardly alone in comparing Chew's active campaigning to religious indoctrination. In fact, many of Chew's colleagues and former collaborators, now working at a distance from him, began to characterize his vigorous pronouncements as religious proselytizing. Goldberger wrote to Murray Gell-Mann in January 1962 that Chew had become "the Billy Graham of physics." He added playfully: "After his talk I nearly declared for Christ. Since I had already changed from Jewish to Regge-ish, it was the only thing I could think of." Years later, John Polkinghorne recalled that Chew proclaimed his ideas "with a fervour" like that "of the impassioned evangelist. There seemed to be a moral edge to the endeavour. It was not so much that it was expedient to be on the mass-shell of the *S* matrix as that it would have been sinful to be anywhere else." Rather than a ringing democratic political campaign, Chew's efforts often struck his former colleagues as runaway zealotry.[77]

[75] Geoffrey Chew, interview with Gordon, Dec. 1997, quoted in Gordon, "Strong Interactions," p. 33 (see also pp. 32–35). Low credited Chew with the main originality in their collaboration in his interview with me; see also Kaiser, "Francis E. Low" (cit. n. 45), pp. 71–72.

[76] Stephen Adler, telephone interview with the author, 16 Feb. 1999. This emphasis on semiphenomenological tools rather than overarching theory construction became a hallmark of Treiman's group and helped shape Adler's later work on current algebras. See Stephen Adler and Roger Dashen, *Current Algebras and Applications to Particle Physics* (New York: Benjamin, 1968); and Sam Treiman, Roman Jackiw, and David Gross, *Lectures on Current Algebra and Its Applications* (Princeton, N.J.: Princeton Univ. Press, 1972). See also Pickering, *Constructing Quarks* (cit. n. 65), pp. 108–114; and Cao, *Conceptual Developments of Twentieth-Century Field Theories* (cit. n. 65), pp. 229–246. The views of other former Princeton students are expressed in John Bronzan to Kaiser, 15 May 1997 (email), and E. E. Bergmann to Kaiser, 16 May 1997 (email). The same attitude is drawn out in the course notes taken by Kip Thorne when he was a graduate student at Princeton in the early 1960s. See, in particular, Thorne's notes from "Properties of Elementary Particles," a course given by Val Fitch (Spring 1963); "Elementary Particle Physics," taught by Sam Treiman (Spring 1963); "Intermediate Quantum Mechanics and Applications," taught by Goldberger (Fall 1963); and "Elementary Particle Theory," taught by Blankenbecler (Spring 1964). All notes in the possession of Professor Kip Thorne; my thanks to him for sharing copies of these notes.

[77] Adler interview; Marvin Goldberger to Murray Gell-Mann, 27 Jan. 1962, quoted in Johnson, *Strange Beauty*

As Adler recalled, Chew announced in his Princeton lecture "a great hope; but at the same time, Treiman was always a bit skeptical of any grand theory." Indeed, Treiman was skeptical. Whereas Chew's January 1962 lecture before the American Physical Society predicted a "wild period of merrymaking," Treiman's own lecture on *S*-matrix material, delivered ten months later, focused instead on how even the most promising-looking "partial results and insights" remained "all tangled up with approximations which have inevitably to be introduced and which vary in style and severity from one application to another, one author to another." It is important to note that Treiman was no critic of approximations, even "severe" ones, per se. His 1958 work with Goldberger on pion decay relied, in another reviewer's words, on "drastic assumptions" and "feeble arguments." Even in Treiman's own estimation, these were "hair-raising approximations" that he and Goldberger were "quite unable—apart from hand waving—to justify."[78] It wasn't the recourse to approximations that irked Treiman about Chew's program; it was the vehemence with which Chew pitted his work against field theory.

Treiman went on to dismiss the bootstrap hypothesis, trumpeted by Chew as the logical conclusion of nuclear democracy, as "amusing." As we have seen in Section I, the goal of the bootstrap work was to find a single self-consistent solution that would show that strongly interacting particles might each produce the very forces that led to their own production by other particles; each strongly interacting particle, then, might be said to "pull itself up by its own bootstraps." What had captured the imagination of the Berkeley group left Treiman unimpressed. Throughout his 1962 lectures, for example, Treiman emphasized the "conjectural" basis of the bootstrap work and "indulg[ed]" in what he characterized as "pessimistic remarks" regarding the bootstrap program.[79] When it came to relations between *S*-matrix theory and quantum field theory, Treiman was not Chew—and Princeton was not Berkeley.

These differences led to some subtle reinterpretations of the meaning of *S*-matrix work, not only of its relative importance. Princeton's L. F. Cook, another faculty member in the Goldberger-Treiman-Blankenbecler group, reported on his research on Chew's beloved bootstrap mechanism. Despite Treiman's deflating judgment, the bootstrap remained to Chew, Frautschi, and most other members of the Berkeley group the sought-for culmination of nuclear democracy, a kind of holy grail for the equal treatment of all nuclear particles: it *meant* that there were no "elementary" particles; each was a bound-state composite of others. Yet for Cook, surrounded by Princeton's field theorists, elementarity remained central. In his 1965 gloss, "The bootstrap embodies a philosophy which supposedly enables one to calculate" various parameters for "*elementary* particles." Cook's work went on to emphasize the lack of agreement between this favorite topic of Chew's

(cit. n. 6), p. 211; and Polkinghorne, "Salesman of Ideas" (cit. n. 55), pp. 24–25. Francis Low similarly remarked that Chew's later efforts seemed "religious" in character: Low interview.

[78] Adler interview; Sam Treiman, "Analyticity in Particle Physics," in *Proceedings of the Eastern Theoretical Physics Conference, October 26–27, 1962,* ed. M. E. Rose (New York: Gordon & Breach, 1963), pp. 127–174, on p. 149; Jackson, "Introduction to Dispersion Relation Techniques" (cit. n. 14), p. 50; and Treiman, "Life in Particle Physics" (cit. n. 73), p. 16. As Treiman himself later remarked of this calculation, "No one but Goldberger and I would have had the effrontery to do what Goldberger and I did": Treiman, "Connection between the Strong and Weak Interactions" (cit. n. 73), p. 388.

[79] Treiman, "Analyticity in Particle Physics," pp. 163, 143. Blankenbecler and Goldberger similarly characterized Chew's bootstrap work as a collection of "interesting speculations" lacking a "physical basis." Making explicit reference to Chew's 1961 La Jolla talk, they dismissed the entire discussion as having merely a "religious nature." R. Blankenbecler and M. L. Goldberger, "Behavior of Scattering Amplitudes at High Energies, Bound States, and Resonances," *Phys. Rev.,* 1962, *126:*766–786, on p. 784. This article was based on Blankenbecler and Goldberger's own talk at the 1961 La Jolla meeting, as indicated in a footnote on p. 766.

and existing experimental data.[80] Even when they turned to the topics most central to Chew's "democratic" campaign, then, Princeton's field theorists clung to the language of "elementary" particles rather than following Chew's group in their vocal break with the "fundamentalists." No matter how completely Chew's democratic vision might have permeated the Berkeley area, that vision did not command a single, unchanging interpretation from physicists further and further removed from Berkeley.

## V. CONCLUSIONS: CONDITIONS OF DEMOCRATIC POSSIBILITIES

Perhaps it bears emphasizing that we needn't agree that Chew's many activities were inherently democratic. He maintained Q clearance for several years after his wartime Los Alamos work; such clearance already implied unequal access to research and resources.[81] His "secret seminar" actively discouraged other faculty members from "participating equally." The 1960 Berkeley conference served as a stepping stone for his own *S*-matrix work, featuring primarily the research of his collaborators. Several friends and former collaborators saw not a democratically minded enrollment campaign but, rather, quasi-religious zealotry in Chew's efforts to interest others in his "democratic" *S*-matrix program. It is not clear what his thoughts or actions were during the 1964 Free Speech Movement. And so on. Nor is it clear whether Chew was working with a fully articulated or consistent political ideology. While Chew was growing up in Washington, D.C., his father had worked in the Department of Agriculture, which was hardly a politically neutral bureaucracy during the New Deal; perhaps Chew had an ingrained interest, based on his early years, in political issues. Yet whether or not we agree on how "democratic" Chew's efforts ultimately were, or on whether they stemmed from a clear and consistent ideology, one thing remains crucial: Chew and many of his students and colleagues saw his program for strong-interaction particle physics as specifically "democratic"—and as special for that reason.

How, then, are we to interpret the self-proclaimed "democratic" work of Geoffrey Chew? Hard on the heels of his most overtly political activities, Chew began to teach graduate students in a manner consistent with his ideas about democracy. Springboarding from his "open," "noncliquish" conference at Berkeley in 1960, Chew proclaimed at every opportunity that his new physics invited all kinds of participation, from "innocent" students to experimentalists who had "never learned" the rival field-theory formalism. In the midst of these activities, which took Chew from Senate subcommittee hearings to "secret seminars," he began to articulate a new and unprecedented vision of how particles behave and how their interactions should be studied: a successful theory of the strong interactions—unlike quantum field theory, with its "aristocratic" elements—must make no distinctions between the many types of particles. None should be singled out, either for special privileges or for special neglect; all must receive, diagrammatically and mathematically, "equal treatment under the law."

[80] L. F. Cook, in Annual Report 1964–1965, pp. 79–80, Dept. Physics, Princeton, Annual Reports (emphasis added). Interestingly, Cook conducted this research with C. Edward Jones, a new Princeton postdoc who had just completed his dissertation under Chew's direction in Berkeley. Each of the courses on particle theory that Kip Thorne attended as a graduate student at Princeton during this period was labeled "elementary particle physics" or "elementary particle theory," further reinforcing this nonbootstrap view of the field. A second center on which it would be interesting to focus, to extend the analysis of the heterogeneity among *S*-matrix groups, would be Cambridge, England, which hosted a group centered on R. J. Eden, P. J. Landshoff, D. Olive, and J. Polkinghorne; see their textbook, *The Analytic S-Matrix* (New York: Cambridge Univ. Press, 1966), and references therein. Paul Matthews delivered his inaugural lecture at Imperial College in Nov. 1962 with the title "Some Particles Are More Elementary than Others" (London: Imperial College, Nov. 1962).

[81] On this point see esp. Oreskes and Rainger, "Science and Security before the Atomic Bomb" (cit. n. 24).

Were Geoffrey Chew's ideas about particle physics determined by these particular cultural and political ideas, dug up and exposed by the loyalty oath and McCarthyism? The unidirectional, causal story falls short here.[82] To begin with, not every physicist who experienced the rapid postwar political transitions in Berkeley went on to embrace a "democratic" physics in the way that Chew did. More important, Arthur Wightman—a quintessential "fundamentalist" in Geoffrey Chew's eyes for his insistence on retaining quantum field theory—became, with Chew, an active member of the Federation of American Scientists during the 1950s. No crude equations between voting behavior and theory choice will suffice here.

But the failure of such crude equations is just the beginning of our work as historians, not the disappointing end. Just as the straw-man story of sociopolitical determinism fails, so too does the contention that there was simply no connection between Chew's political engagement, pedagogical reforms, and particle physics. At the biographical level, perhaps Chew's frustration with unfair, anti-Communist practices at Berkeley helped to strengthen an iconoclastic resistance to unquestioned authority and a desire to follow out otherwise-unexplored options—he was, after all, already a self-proclaimed "non-conformist," as he wrote in his letter of resignation to Birge in July 1950. Speaking out against university regents and State Department officials, despite the prevailing orthodoxy, could well have prepped Chew for his similarly outspoken challenges to the field-theory orthodoxy some years later.[83]

Even so, iconoclasm alone cannot explain the specific details of Chew's nuclear democracy—its particular elements and the interpretation he gave to them. Substantive links—not just a consistent underdog stance—appear between the three realms of his postwar activities. In particular, the same language recurs again and again ("fundamentalists," "special status," "without favoritism," "equal partners"); models, particles, and collaborators were all "democratized." This was a vocabulary Chew had honed over a decade filled with angst and activism, in front of regents and senators, years before he began to apply it to Feynman diagrams and ρ mesons. Rather than asking "How much did politics affect Chew's physics?" or "What complicated admixture of politics and culture and society interacted in which complicated ways to produce Chew's ideas in physics?" we can

[82] Marcello Cini, who worked as a dispersion-relations theorist during the 1950s, has offered a somewhat strained argument that dispersion relations, which promised "utilitarian" correlations among the newly acquired reams of experimental data, took hold because of its fit with "the dominant ideology in the U.S." Marcello Cini, "The History and Ideology of Dispersion Relations: The Pattern of Internal and External Factors in a Paradigmatic Shift," *Fundamenta Scientiae,* 1980, *1:*157–172, on p. 157.

[83] On the question of iconoclasm, Chew's case warrants comparison with that of David Bohm. Just as Bohm was losing his Princeton job for not cooperating with HUAC, he published two long articles questioning the dominance of the standard interpretation of quantum mechanics and offering a new one in its place. Much as Chew would do in 1961, Bohm thus challenged the accepted physics orthodoxy in a manner consistent with his postwar political convictions. Unlike Chew, however, Bohm (at least later, starting ca. 1960) proclaimed that his ideas in physics were actually inspired by his political thinking. See David Bohm, "A Suggested Interpretation of the Quantum Theory in Terms of 'Hidden' Variables: I and II," *Phys. Rev.,* 1952, *85:*166–179, 180–193; Olwell, "Physical Isolation and Marginalization in Physics" (cit. n. 9); James Cushing, *Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony* (Chicago: Univ. Chicago Press, 1994); F. David Peat, *Infinite Potential: The Life and Times of David Bohm* (Reading, Pa.: Addison-Wesley, 1997), Chs. 5, 6, 8; Mullet, "Political Science" (cit. n. 9); and Kojevnikov, "David Bohm and Collective Movement" (cit. n. 9). Wolfgang Pauli, for one, saw fit to merge Bohm's work in physics with his political troubles, describing Bohm's "younger fellow-travellers (mostly 'deterministic' fanaticists, more or less marxistically coloured)." Pauli believed that Bohm had blurred the line between political engagement and physical theorizing. See Pauli to Léon Rosenfeld, 16 Mar. 1952, in Pauli, *Wissenschaftlicher Briefwechsel,* ed. von Meyenn (cit. n. 39), Vol. 4, Pt. 1, pp. 582–583. Cf. Pauli to Abraham Pais, 7 Mar. 1952, in which he labels Bohm a "*Sektenpfaff,*" or, roughly, "cult leader" (*ibid.,* pp. 626–627).

thus build on Chew's curious continuity of language to turn the question around: Why did Chew's work emerge in the form that it did, at the time and in the place that it did? What were the conditions, in other words, that made "nuclear democracy" an intellectual possibility—and indeed not just a "possibility" but the dominant set of techniques for strong-interaction particle physics throughout the 1960s? Why, moreover, was the work subject to so many different, competing interpretations by physicists further and further removed from Chew's immediate group in Berkeley?

When we phrase the question this way, we are no longer driven to squabble over competing ledger sheets, trying to count up "how much political factors determined Chew's physics." Instead, when we ask, "Why then? Why there?" certain plausible connections relate Chew's choices of what to work on and what to lobby for. Feynman diagrams, those staple tools of quantum field theory, appealed to Chew with a usefulness and immediacy far beyond their narrow field-theoretic definitions—an appeal noted by scores of other theorists throughout the 1950s and 1960s as well. Deciding that the diagrams' "true" meaning was not dictated by field theory alone, Chew had some choice in how he would use and interpret them—just as scores of other theorists chose to read and interpret the diagrams in still different ways, toward different calculational ends. As Chew devoted more and more time to political and pedagogical alternatives to what he saw as infringements on equal treatment, perhaps a similarly democratic reading of Feynman diagrams, and of the particles they purported to describe, seemed particularly salient. In other words, perhaps this particular notion of "democracy" and "equal treatment" comprised what Chew mentioned in passing within his 1961 lecture notes as his "general philosophical convictions." That association, at least, would certainly help to explain why it was Chew who produced this fervently "democratic" reading of Feynman diagrams, amid the many other interpretations they received from other theorists. In this sense, "nuclear democracy" seems thoroughly enmeshed with Chew's time and place—it bears the marks of McCarthy-era Berkeley.[84]

These specific resonances, no doubt aided but not uniquely determined by the particular environment of late-1940s and 1960s Berkeley, further help to explain why many other physicists who worked on *S*-matrix theory at this time did different things with it. To Chew and his many postdocs and students, nuclear democracy and the bootstrap meant that quantum fields and virtual particles simply did not exist and that there was no such thing as an "elementary" particle. Yet many young theorists, such as Sam Treiman's graduate students at Princeton, completed dissertations that treated *S*-matrix theory neither as a self-consciously "democratic" pursuit nor, much less, as a raised-fist competitor to "aristocratic" quantum field theory—indeed, the bootstrap, at best, seemed to them to offer just one more technique for detailing the properties of the truly "elementary" particles. Pieces of Chew's new calculational machinery were picked up and taught at various places, often bundled with still different theoretical tools and deployed toward different calculational

[84] Regarding possible ties between Berkeley's political culture and his physics program, Chew responded recently that he "had never thought about it," though such connections are "a possibility worth considering": Chew, interview with Gordon, Dec. 1997, quoted in Gordon, "Strong Interactions," p. 37. Links between his specific pedagogical efforts and his theoretical approach to particle physics strike a similar chord these days from Chew, who recalls only that "I might have had some idea of that," though "it's hard to recapture the way one was thinking in an earlier period"—which, after all, occurred four decades ago: Geoffrey Chew interview with the author, Berkeley, 10 Feb. 1998. More recently, Chew wrote that an earlier draft of this essay, which made the case for substantive intellectual links between his politics, pedagogy, and physics, was "perceptive and accurate": Chew to Kaiser, 19 Aug. 1999.

ends. Just as Chew appropriated Feynman's diagrams, so too many theorists outside of Berkeley converted Chew's program into their own.

With this tale of democracy in postwar America, we may thus scrutinize and interrogate some complicated, attenuated connections between an intellectual legacy of McCarthyism and reactions to it, and certain ideas and practices within theoretical particle physics. Geoffrey Chew's repeated refrain of "equal participation" and "equal treatment under the law" marked his work—in its various guises and arenas—as a product of his specific time and place. Chew's postwar work thus provides a fertile test case to explore the politico-cultural changes of postwar America and physicists' changing place within it.