

Against the Tide: Mulliken, Slater, and the Niche of Quantum Chemistry in the American Physics Community after World War II*

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제2차 세계대전 후 미국 물리학회에서 양자화학의 위상: 시카고 대학과 MIT 대학을 중심으로

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요 약

제2차 세계대전을 거치면서 미국 물리학은 많은 발전을 거듭했다. 물리학자들의 수가 급속도로 증가했을 뿐만 아니라, 그들의 연구를 위한 물적 지원도 크게 늘었다. 이런 변화는 물리학의 여러 분야 중에서 핵물리학과 전자기학에서 두드러지게 나타났고, 이와 관련하여 거대과학의 등장, 군사 연구의 확장, 산업-대학-정부의 연계 등이 과학사에서 흥미로운 주제로 다루어져 왔다. 본 논문은 전후 미국 물리학계의 변화를 다른 각도에서, 즉 물리학의 균형된 발전을 도모했던 과학자들의 시점에서 보고자 한다. 좁게는 초기 양자화학의 발전에 크게 공헌했던 시카고 대학의 멀리켄(Robert S. Mulliken, 1896~1986)과 MIT 대학의 슬레이터(John C. Slater, 1900~1976)가 각기 물리학과 내에서 겪었던 경험을 주로 분석하며, 좀더 넓게는 미국 물리학회(American Physical Society)에서 화학물리부(Division of Chemical Physics)와 고체물리부(Division of Solid-State Physics)의 형성이 가지는 의미를 생각해 본다. 이를 통해 필자는, 멀리켄과 슬레이터가 세계대전 이후 핵물리학과 전자기학 분야에 사회적 지원과 관심이 편중됨을 목격하였고, 이러한 시대 상황 속에서도 각자의 연구분야에서의 꾸준한 공헌을 했으나, 그들의 은퇴와 더불어 시카고 대학과 MIT 대학의 물리학과 내에서 양자화학과 관련된 연구의 비중은 현저히 줄었음을 보이고자 한다.

주요어 : 멀리켄, 슬레이터, 양자화학, 미국 물리학회, 시카고 대학, MIT 대학

* I am grateful to Owen Hannaway, Stuart W. Leslie, Harry Marks, and John W. Servos for their thoughtful comments. The following abbreviations are used: **RMP**, the Robert S. Mulliken Papers, the Special Collection of the University of Chicago Library, Chicago; **JSP**, the John C. Slater Papers, the American Philosophical Society Library, Philadelphia; *HSPS*, *Historical Studies in the Physical and Biological Sciences*.

Quantum Chemistry should not hesitate to use their device to destroy the monopoly of world government at present held by Nuclear Physicists(*Acta Vålådalenses*, 1958).¹⁾

The decade after World War II was the heyday of American physicists. In return for their notable contributions during the war, often dubbed the “physicist’s war,” they came to see the number of physics students multiplying, the amount of funding increasing, and the public attention to their discipline rising. As the renowned physicist Lee DuBridge saw in 1949, physicists became more numerous, more affluent, and much more famous.²⁾ This trend was reinforced during the Korean War in the early 1950s and continued during the Cold War, although there were some ups and downs. In general the postwar reconstruction of the physics profession was undertaken in this halcyon context, which provides historians with ample opportunities to study the interrelated changes in patterns of funding, the goals and practices of science, and size of workplace.³⁾

1) This quote appears in *Acta Vålådalenses*, Vol. 1, no. 3 (August 28, 1958), pp. 1-2, an issue made by participants of the Quantum Chemistry Summer School in Sweden. Mulliken was invited, along with Linus Pauling and other quantum chemists, to discuss the correspondence between chemistry and quantum chemistry. This jocular description of the situation of the physics profession, I think, captures the quantum chemist’s sour sentiment against nuclear physics. “Löwdin’s Summer School, Vålådalen, Sweden, July 26-Aug. 30, 1958,” **RMP**: 66.14.

2) For Lee DuBridge’s comment, see Silvan S. Schweber, *QED and the Men Who Made It: Dyson, Feynman, Schwinger, and Tomonaga* (Princeton: Princeton University Press, 1994), p. 144.

3) The literature on the effects of World War II on postwar American physics is vast. While Daniel J. Kevles, *The Physicists: The History of a Scientific Community in Modern America* (Cambridge: Harvard University Press, 1971) is still useful, a number of detailed case studies are available: for example, Paul Forman, “Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940-1960,” *HSPS* 18 (1987), pp. 149-229; S. S. Schweber, “The Mutual Embrace of Science and the Military: ONR and the Growth of Physics in the United States after World War II,” in Everett Mendelsohn, M. Roe Smith, and Peter Weingart, eds., *Science, Technology, and the Military* (Boston: Kluwer Academic, 1988), pp. 3-45; Peter Galison, “Physics Between War and Peace,”

However, not every field of physics enjoyed an immediate social reward at the same level. While there were aggressive efforts to recruit and keep nuclear physicists represented by the Los Alamos generation, and while there were renewed demands for physicists specialized in electronics, such changes were less visible in atomic and molecular spectroscopy, solid-state physics, and quantum-mechanical studies of chemical bonds and magnetism. The physicists working in these fields had proudly led the “coming of age of American physics” in the early 1930s⁴), but they returned from the wartime mobilization only to find themselves trying to reclaim adequate space and staff for resuming their research programs. This comparison does not necessarily mean that they did not benefit from postwar opportunities; rather, it indicates that, given a differential distribution of resources and privilege, they had to stake out the boundaries of their research areas. When the physics community generally moved toward the “twin poles of radar and the atomic bomb,” or nucleonics and electronics⁵), those physicists in between were sometimes fighting against and sometimes adapting to the tide of the postwar context.

This essay seeks to investigate the changing balance among the subdisciplines of physics after the war, with the cases of Robert S. Mulliken at the University of Chicago

in *ibid.*, pp. 47-85; Paul Hoch, “The Crystallization of a Strategic Alliance: The American Physics Elite and the Military in the 1940s,” in *ibid.*, pp. 87-118; Daniel Kevles, “Cold War and Hot Physics: Science, Security, and the American State, 1945-56,” *HSPS* 20 (1990), pp. 239-64; Stuart W. Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (New York: Columbia University Press, 1993).

4) For when American physics came of age, see J. H. Van Vleck, “American Physics Comes of Age,” *Physics Today* (June, 1964), pp. 21-26.

5) For the emergence of radar and the atomic bomb as new cultural symbols in the postwar era, see Galison, “Physics Between War and Peace.” He also gives an useful description of nucleonics—“the study of a broadly construed nuclear physics, situated at the nodal point of research problems of cosmic rays, nuclear medicine, quantum electrodynamics, nuclear chemistry, and the practical imperatives of industry and defense.”

and John C. Slater at MIT⁶). Working in various problems of the atom, molecule, and solid-state with the tool of quantum mechanics, they were among the scientists who helped quantum chemistry flourish in America before the war⁷). Facing World War II in their forties, which might be considered the prime time for scientific productivity, Mulliken and Slater saw not only their research disrupted but also their field of study overshadowed. In particular, they worked in the institutions hosting such large-scale military projects as the Metallurgical Laboratory and the Radiation Laboratory, and there, the postwar reorganization was visibly directed in favor of nuclear physics and electronics. Under these circumstances, Mulliken and Slater were tempted to change their research area in line with the general tide, but they decided to remain and reconstruct their respective research groups — the Laboratory of Molecular Structure and Spectra (LMSS) at Chicago and the Solid-State and Molecular Theory Group (SSMTG) at MIT. Examining the rise and fall of these groups, I will show hopes and frustrations, plans aborted and adopted, power struggles and infighting that Mulliken and Slater had to cope with, and their efforts to find an appropriate social niche in the postwar American physics community.

6) The recent scholarship of postwar American science in general notes the polarization of the physics profession towards nucleonics and electronics; but it does not pay much attention to what happened to fields other than these. A few exceptions are Alexi Assmus, "The Americanization of Molecular Physics," *HSPS* 23 (1992), pp. 1-34; and Loren J. Butler, "Robert S. Mulliken and the politics of science and scientists, 1939-1946," *HSPS* 25 (1994), pp. 25-45.

7) Mulliken and Slater did not consider themselves to be working in quantum chemistry: Mulliken tended to identify himself as a molecular physicist, and Slater a solid-state physicist. However, they were honored as great pioneers of quantum chemistry in the International Symposium of Quantum Chemistry on Sanibel Island, respectively in 1965 and 1967. Historians, as well as scientists, have regarded them as two of founding fathers of quantum chemistry. Kostas Gavroglu and Ana Isabel Simoes, "The Americans, the Germans, and the Beginning of Quantum Chemistry: The Confluence of Diverging Traditions," *HSPS* 25 (1994), pp. 47-110; and S. S. Schweber, "The Young John Clarke Slater and the Development of Quantum Chemistry," *HSPS* 20 (1990), pp. 339-406.

1. War Interruption

To most scientists, the wartime mobilization meant an interruption. Their usual academic tasks, research and education, were subordinated for years to the absolute priority of the war effort. Many laboratories and seminar rooms were turned to military purposes, and a number of students were mustered. The research contract system, which characterized the basic relationship between the federal government and universities during World War II, enabled some scientists to work in their own laboratories: but this system did not prevent the extensive dislocation of scientific personnel.⁸⁾ The MIT Radiation Laboratory was mostly staffed with non-MIT scientists, including its director, Lee A. DuBridge from Rochester University. In the Metallurgical Laboratory at Chicago, Enrico Fermi from Columbia University led the chain-reaction research, while Arthur Compton of Chicago handled administration and policy. And an unprecedented research environment was created at Los Alamos, where physicists coming from various institutions worked on the atomic bomb. Most scientists working in such wartime organizations were dislocated, both physically and mentally, from their normal life of knowledge production. The recollection of E. Bright Wilson, a quantum chemist of Harvard who served in the Underwater Explosives Research Laboratory at Woods Hole, epitomized it: "[During the war, I] was far removed from quantum mechanics. It did bring me into rather close contact with Jack Kirkwood and John von Neumann, but we talked about explosions and not about wavefunctions. In fact, when I returned to Cambridge and science in 1945, I remember being asked to comment on a theoretical manuscript. The author was rather disgusted with my inability to understand his work and I was very much distressed myself with what four years had

8) Roger L. Geiger, *Research and Relevant Knowledge: American Research Universities Since World War II* (New York: Oxford University Press, 1993), pp. 6-13.

done to me.”⁹⁾

Mulliken's war-experience was no exception. He saw his research disrupted by the war as the physical science division of the University of Chicago was reorganized around the Metallurgical Laboratory in 1942. Mulliken's spectroscopic laboratory and precision equipment in the basement of Eckhart Hall were dismantled, while his graduate students and postdoctoral fellows went to war. Mulliken himself was assigned to a mission almost unrelated to his expertise. Although he expected to be named Director of the Chemistry Division of the Metallurgical Laboratory, he was instead given the job of Director of the Information Division, supervising the production and distribution of classified reports. For the first — and also the last — time in his sixty-year professional career, Mulliken stopped publishing papers for four years following 1943. As he recollected, the war created an entirely different environment at Chicago: “I now envisaged the physics building in its war-time activity as a totally different place than before; all its objectives and functions had been transformed and familiar things appeared in a wholly new light. At meetings in what used to be the seminar room in Eckhart Hall, I had the feeling that at any time the Germans, who we thought could be ahead of us, might drop an atomic bomb on us.” Certainly, Mulliken's mind was detached from the daily work: “During my time on the project I learned how to go home at the end of the day and forget the day's problems, and go to sleep promptly. This was a great change from previous times when my sleep had been interlaced with thinking about research problems.”¹⁰⁾

Slater's situation at MIT was hardly better than Mulliken's at Chicago. In the

9) E. Bright Wilson, “Some Personal Scientific Reminiscences,” *International Journal of Quantum Chemistry: Quantum Chemistry Symposium 14* (1980), pp. 17-29, on p. 24.

10) Robert S. Mulliken, *Life of a Scientist* (Berlin: Springer-Verlag, 1989), pp. 114-115. See also Butler, “Mulliken,” pp. 37-38.

course of organizing and staffing the Radiation Laboratory, the physics department which Slater had headed for a dozen years was not consulted or even informed. MIT physicists were diverted to various war projects, while a number of outside physicists were coming in. To Slater, it looked like an "invasion of the Institute by physicists from all over."¹¹ Slater himself was asked to explore the theory of the magnetron, a microwave generator that, after having been shipped from Great Britain, triggered the radar project in the United States. At that time, Slater "knew nothing of what a magnetron was, except for the knowledge that molecular spectroscopists had made very primitive versions for far infrared or millimeter spectroscopy." Nonetheless he launched into very concentrated calculations, initially setting up the problem as he had done for understanding the phenomenon of superconductivity. He then applied to this problem the idea of calculation by iteration, which had been used in the problem of the self-consistent field in an atom. In the sense that he was able to deal with Maxwell's equations and the Schrödinger equation during the wartime, Slater was in a better position than Mulliken. But he soon found no appropriate role in wartime MIT; so he moved to the Bell Laboratories in the fall of 1941, where he spent most of his time until the end of the war.¹²

The cases of Mulliken and Slater suggest the variety of the scientists' war experience. For younger physicists like Richard Feynman and Joseph Hirshfelder, the war provided a chance to meet the giants like Robert Oppenheimer and Hans Bethe.¹³

11) John C. Slater, *Solid-State and Molecular Theory: A Scientific Biography* (New York: John Wiley & Sons, 1975), p. 209.

12) *Ibid.*, pp. 209-216.

13) Joseph O. Hirshfelder, "The Scientific and Technological Miracle at Los Alamos," in *Reminiscences of Los Alamos, 1943-1945*, eds. Lawrence Badash, Joseph O. Hirshfelder, and Herbert P. Broida (Dordrecht, Holland: D. Reidel Publishing Company, 1980), pp. 67-88; Richard P. Feynman, "Los Alamos from Below," *ibid.*, pp. 105-132.

For established scientists like Oppenheimer and Arthur Compton, it offered an opportunity to learn how to manage big budgets and large organizations. Some scientists like Linus Pauling¹⁴⁾ and Fermi were fortunate enough to continue their peacetime research; others like Mulliken and Slater lost all momentum. Thus it is difficult to demarcate the extent to which the war interrupted individual scientific activities simply along disciplinary or subdisciplinary lines.

And yet, evidently the war accelerated nuclear science and revitalized electronics, two fields that found an immediate military use. Mulliken already experienced the changing balance within Chicago's physics department during the war when he suggested the appointment of Gerhard Herzberg, a distinguished spectroscopist who later became a Nobel laureate.¹⁵⁾ The tide for nuclear physics was so strong that Mulliken himself wondered "whether [he] should continue in pursuit of molecular spectra and structure or perhaps shift to nuclear or other problems."¹⁶⁾ He finally decided to continue his study of molecular problems. Slater also felt "profound effects [of the war] on physics at MIT."¹⁷⁾ Although he strove to keep the tradition of diversity in physics that he had fostered within the prewar department, he could not but agree to the postwar mandate that nuclear physics and electronics should expand. As for his own research, Slater spent several years working on microwave electronics until he made up his mind to return to solid-state and molecular problems. Mulliken and Slater returned to their fields after having second-thoughts during the period of postwar planning and reconstruction.

14) Pauling was able to continue his research in immunochemistry, arguing its bearing on the war effort. See Thomas Hager, *Force of Nature: The Life of Linus Pauling* (New York: Simon & Schuster, 1995), pp. 260-263.

15) See Butler, "Mulliken," p. 38.

16) Mulliken, *Life*, p. 116.

17) Slater, *Solid-State and Molecular Theory*, pp. 209-234.

2. Mulliken and the LMSS

As the war came to an end, physicists at the University of Chicago began planning a postwar reconstruction of the department. There was discussion of the adequate distribution of departmental resources, given the influx of new professors like Enrico Fermi and Edward Teller. There was also a need for establishing a healthy relationship between the department and interdepartmental research institutes, such as the Institute for Nuclear Studies, the Institute for the Study of Metals, and the Institute of Radiobiology and Biophysics.¹⁸⁾ Moreover, the physics department needed immediately to choose a new chairman, with the impending departure of Arthur Compton to Washington University. Mulliken involved himself deeply in departmental politics. For instance, he strongly opposed the appointment of the nuclear physicist Samuel Allison from the Metallurgical Laboratory.¹⁹⁾ In a memorandum to President Robert M. Hutchins, he argued that the new chairman should be able to maintain the balanced development of physics; and he cast doubts on Allison's administrative capability and vision. He wrote: "The prime need for the department is first to restore its peacetime strength and then to increase its strength further. Particular attention should be given to the careful selection of personnel at all levels. . . . A well-rounded development in both newer and older fields of physics should be sought, building on existing human and materials resources as foundations. This development should not

18) The move to lure Enrico Fermi to Chicago directly from the Metallurgical Laboratory aggravated many physicists, especially Isidor I. Rabi who was concerned about rebuilding his depleted physics department at Columbia University. See Stuart M. Feffer, "Atoms, cancer, and politics: Supporting atomic science at the University of Chicago, 1944-1950," *HSPS* 22 (1992): 231-261, esp. 235-239; and Robert W. Seidel, "Accelerating Science: The Postwar Transformation of the Lawrence Radiation Laboratory," *HSPS* 13 (1982), pp. 375-400, esp. 378-9.

19) On Mulliken's opposition to Allison's appointment, see Butler, "Mulliken," p. 41.

attempt to cover all possible fields of physics, but in any event should include certain basic fields definitely.”²⁰⁾ In the end, F. W. H. Zachariasen, an X-ray crystallographer whom Mulliken recommended, became the chairman, while Allison was appointed Director of the Institute for Nuclear Studies.

Mulliken had an ambitious plan for revitalizing quantum chemistry, or what he termed molecular physics: that was to create an Institute of Molecular Physics comparable in size and prestige with the Institute for Nuclear Studies. As a step towards realization of this dream, he proposed a small-scale Institute (or Laboratory) of Spectroscopy and Molecular Structure.²¹⁾ His potential supporters, such as Otto Struve of the astronomy department (who was a director of the Yerkes Observatory) and James Franck of the chemistry department, welcomed the idea of interdepartmental cooperation but showed little enthusiasm for the idea of an elaborate formal organization.²²⁾ Nonetheless, Mulliken continued to campaign for the establishment of his institute by spelling out its essential and practical needs. The most urgent problem, he stressed, was to define its status in the physics department and in the physical sciences division, since this definition was intertwined with such problems as the distribution of funds, the allocation of space, and the administrative mechanism. Mulliken thus thought it essential to

20) Mulliken to Robert M. Hutchins (May 10, 1945), **RMP**: 108.17.

21) Mulliken to Otto Struve (Aug. 13, 1945); Mulliken to Hutchins (Aug. 15, 1945); Mulliken to Zachariasen (Aug. 16, 1945); Mulliken to James Franck (Aug. 16, 1945); Mulliken to Allison (Aug. 16, 1945), **RMP**: 108.14. Some carbon copies of these letters were also delivered to Gustavson and Zachariasen.

22) Struve to Mulliken (Aug. 21, 1945), **RMP**: 108.14. Struve preferred “a smaller organization, perhaps the name” institute “but without elaborate facilities or complicated organization.” For Struve did not think it desirable for his astrophysical spectroscopists to spend much of their time in the institute burdened with specific duties and other obligations. James Franck was of the same opinion.

1. Clearly establish equitable relations to Physics Dept. and to I.N.P. [Institute of Nuclear Physics] and other institutions as to space, facilities, support.
2. Determine location: Eckhart, later Metals Inst. Bldg.
3. Establish official approval for inclusion of faculty members and graduate students from Chemistry, Astronomy, etc.
4. Establish approved mechanism for proposal of appointments in chemistry and physics.
5. Establish war damage restoration fund (note that losses in personnel, equipment, momentum were especially heavy in this field.)²³⁾

In addition, he listed his practical needs: staff members, assistants, and fellows; technical assistance and services; and research supplies and equipment.²⁴⁾

As well as defining the status of the institute and listing the needed resources, Mulliken laid out a research agenda for the institute. On the one hand, the plan had to do with how much to continue prewar research programs and how much to explore new research topics. He considered diversification of the postwar program from the grating-ruling project, which had been a core of spectroscopic studies at Chicago, into other areas, such as infra-red spectroscopy and mass spectroscopy. On the other hand, there was the question of maintaining a balance between experimental work and the theoretical program in the spectroscopy laboratory. He pondered the possibility of creating a computing center within the institute.²⁵⁾

For all his efforts, however, Mulliken could make only a modest start. His laboratory was reconstructed in the basement of Eckhart Hall, but without a formal recognition of his proposed Institute of Spectroscopy and Molecular Structure. John R. Platt, a newly appointed Assistant Professor, joined forces with him, along with several

23) Robert S. Mulliken, "Essential Needs of Laboratory of Molecular Structure and Spectroscopy" (Sept. 17, 1945), **RMP**: 108.13.

24) Robert S. Mulliken, "Practical Needs and Problems for Institute of Molecular Structure and Spectroscopy," Circa 1945, **RMP**: 108.13.

25) *Ibid.*

graduate students. Mulliken managed to run his laboratory for a year with funds remaining from the prewar Rockefeller Foundation grant, until he received generous peacetime contracts from the Office of Naval Research and other military agencies. Mulliken's group soon issued red-covered annual reports on the progress of contract research under the name of the Laboratory of Molecular Structure and Spectra (LMSS). This became its official name in 1952.²⁶⁾

The activities of the LMSS were divided geographically between the experimental group working in the basement of Eckhart Hall (hence the "downstairs group") and the theoretical people who had rooms on the third floor of the Ryerson Physical Laboratory (hence the "upstairs group"). In the downstairs various spectroscopic instruments were set up, and Philip G. Wilkinson, a research associate, was in charge of supervising experimental work throughout the 1950s. The theoretical group upstairs had a large conference room for presentation of progress reports and discussion of problems, and also for keeping boxes of their own reprints and of others sent from fellow scientists elsewhere in the world. Upstairs, Clemens C. J. Roothaan ruled the roost. He took his doctoral degree in 1950 under the supervision of Mulliken, and after that, he joined the faculty of the physics department to lead the LMSS's project of computing integrals. Mulliken's office, which he had inherited from Albert A. Michelson, was located upstairs, but he was rather a coordinator of, or what he called a "middle-man" between, experimental and theoretical workers.²⁷⁾ Mulliken assumed the new role of the research manager for his group.

In the 1950s, Mulliken's laboratory emerged as an attractive place for a number

26) On Mulliken's laboratory after the war, see Mulliken, *Life*, pp. 117-135.

27) *Ibid.*, p. 139. For a "middle-man," see Robert C. Brasted and Peter Farago, eds., "Interview with Robert S. Mulliken by Norman H. Nachtrieb," *Journal of Chemical Education* 52 (1975), pp. 560-564, on p. 561.

of promising young scholars in quantum chemistry who came as postdoctoral fellows, research associates, or visiting professors.²⁸⁾ This situation, however, did not continue into the 1960s. Mulliken reached the official retirement age in 1961. Although he remained at Chicago as Professor of Physics and Chemistry, he gradually decreased his activities there, spending more time outside. In 1965 Mulliken accepted an offer of a Research Professorship of Chemical Physics from Florida State University, Tallahassee, in which he lived a life of quasi-retirement during the winter term. Furthermore, after winning the Nobel Prize in 1966, Mulliken was increasingly engaged in what he called post-prize activities. Given that some of key members had already left, and that no leadership emerged after Mulliken's official retirement, the LMSS declined so precipitously that the university eventually took over most of its basement space for an extension of the physics-mathematics library.

3. Slater and the SSMTG

Like Mulliken, Slater was concerned about the well-rounded reconstruction of the physics department at MIT after the war, and this concern stemmed from the growing social and intellectual status of nuclear physics. Slater even noticed a haughty attitude among nuclear physicists: "There has been too much emphasis on the part of some physicists to neglect the second phase, of consolidation and development [of fields already explored], in favor of the first, of pure discovery."²⁹⁾ Slater did not belittle nuclear physics, but he maintained that there were fields of equal importance. He believed that the linkage of physics with its practical applications should not be forsaken too early. He thus suggested to MIT President Karl Compton that the tradition

28) Mulliken, *Life*, pp. 167-203.

29) John C. Slater, "Activities of the Physics Department after the War," *JSP*: MIT #7.

of keeping "breadth and balance" in physics should be continued and encouraged. For Slater, "no revolutions in the department will be necessary."³⁰⁾

However, the physics department at MIT did undergo a thorough postwar reconstruction to the extent that its structure and function were never again the same as they had been earlier. Although physicists taught students in the department, many of them conducted research in interdepartmental institutes having their own independent entities, administrative staffs and budgets, and technicians and other employees. No sooner had the war ended than the Research Laboratory of Electronics (RLE) was created with the purpose of keeping intellectual and material resources mobilized for the Radiation Laboratory. It was soon followed by the creation of the Laboratory for Nuclear Science and Engineering (LNSE). Gone was Slater's hope of a balanced development of physics, as the RLE and the LNSE emerged as principal sites for producing PhDs.³¹⁾

In this changed landscape, there was little chance for Slater to build up a "school of theoretical physicists," who would take a leading role in the development of all branches of physics and its applications.³²⁾ Theoretical physicists were scattered among various research institutes, and Slater found himself associated with the Research

30) *Ibid.*

31) John C. Slater, "History of the MIT Physics Department," *JSP: MIT* #7. See also Leslie, *American Science*, pp. 25-32, 142-159; and Silvan S. Schweber, "Big Science in Context: Cornell and MIT," in *Big Science: The Growth of Large-Scale Research*, eds. Peter Galison and Bruce Hevly (Stanford: Stanford University Press, 1992), pp. 149-183. Christophe Lecuyer indicates that the LNSE had more difficulties in integrating people from various departments than the RLE did. Christophe Lecuyer, "The Making of a Science Based Technological University: Karl Compton, James Killian, and the Reform of MIT, 1930-1957," *HSPS* 23 (1992), pp. 153-180. MIT created a separate department of nuclear engineering in 1951; and thus the LNSE was renamed into the LNS, dropping engineering from it.

32) Slater, "History of the MIT Physics Department."

Laboratory of Electronics. For some years, he helped construct a microwave linear electron accelerator at the RLE and worked on the application of low-temperature physics to electronics. The continuing power struggle within MIT also affected him. In 1948 President Karl Compton, who had hand-picked Slater and led the transformation of MIT into a science-based technological university, was forced to step down. A string of new appointments particularly favored those who had consulted for the government or participated in wartime research organizations.³³⁾ Slater gradually lost his administrative power in the physics department, and his infighting with nuclear physicists became intense. In 1951 he accepted a newly-created Institute Professorship, relinquishing the position of department head.³⁴⁾ It was certain that the MIT physics department evolved along the path deviating from his ideas, and Slater was concerned about the lack of the old-time peace and quiet:

A building, then, is our greatest physical need. Our greatest spiritual need is peace and quiet. The department has lived a hectic life since the war. Everybody for almost ten years has been busier than he ever was before. This makes for conspicuous and impressive programs, but perhaps not for the deep thought that is needed to make real and fundamental advances. Physics after all is as much a contemplative subject as any. We have finished our post-war

33) Lecuyer points out, on the basis of George Harrison's unpublished biography of Compton, that the executive committee of MIT forcibly replaced Compton by James Killian. Lecuyer also notes the momentous changes that war activities wrought to the administration at MIT: "[The war activities split the new generation of junior faculty members and young administrators that Compton and Bush had hired and nurtured during the 1930s. Those who consulted for the government and espoused its patronage, Gordon Brown in electrical engineering and Julius Stratton in physics, for example, gained power and prestige at the Institute. . . . In contrast, those who taught at the radar school and failed to participate in the war research effort at MIT saw their position in jeopardy." Slater certainly belonged to the latter group. Lecuyer, "Science Based Technological University," pp. 171-175.

34) The infighting became so intense that "Slater and Zacharias almost came to blows." H. Feshbach, interview with Silvan S. Schweber, February 1988, in Schweber, "Big Science," footnote 123. For the change in Slater's position, see Slater, *Solid-State and Molecular Theory*, pp. 237-238.

reorganizations, we may hope to get our building, and then we should settle down and work. But in these unfortunate days, a lack of peace and quiet spreads over a wider area than our local scene. We shall probably get our building, in the course of time; the peace and quiet may be harder to come by.³⁵⁾

In the midst of these changes, Slater returned to quantum-mechanical studies of molecules and solids. Two important scientific developments, among others, rekindled his interest in this field. One was the invention of the transistor by John Bardeen, Walter Brattain, and William Shockley at the Bell Laboratories. Their work was an outcome of studies on the properties of semiconductors with the tool of the energy band theory. Yet Slater saw a number of other problems waiting for further investigation. "The time has come for an expansion of our efforts to understand these energy bands, proceeding on a number of fronts," he said at the 1949 American Physical Society.³⁶⁾ He had in mind the theoretical calculation of energy bands. The other advance in the study of molecules and solids was a calculation of the cohesive energy of alkali halide crystals by the Swedish scientist Per-Olov Löwdin. This "brilliant piece of work," as Slater praised it, was Löwdin's dissertation work at the University of Uppsala. Slater was particularly impressed by Löwdin's handling of a prodigious amount of detailed computations, including those of all the difficult exchange integrals, which had discouraged quantum chemists before. He later described his encounter with Löwdin's work: "Needless to say, I was enormously pleased [with it]. I immediately gave a colloquium talk on the subject, and looked forward to meeting this remarkable young

35) *Ibid.*, p. 73.

36) John C. Slater, "Open Problems in the Physics of Solids," talk for Conference on the Solid State, Metropolitan Section of the American Physical Society and Polytechnic Institute of Brooklyn, Oct. 15, 1949, *JSP: Slater/Open Problems in the Physics of Solids*. For his return to his "first interest," see also Slater, *Solid-State and Molecular Theory*, pp. 226-234. For the invention of the transistor, see Lillian H. Hoddeson, "The Discovery of the Point-Contact Transistor," *HSPS* 12 (1981), pp. 41-76.

man whom Waller had praised so highly."³⁷⁾ In the 1950s, Löwdin came to MIT several times to collaborate with Slater as a research associate.

While some scientific developments rekindled Slater's interest in solid-state and molecular problems, the situation at the MIT physics department might have been a factor in his decision to form a research group in the field where he had been a world-leader. In particular, as his antagonism against nuclear physicists escalated, and with his leadership being challenged in the department, Slater needed a group of people who could support him. This was reflected in his address at a conference on the solid state held at Polytechnic Institute of Brooklyn in October 1949, where he announced his return to molecular and solid studies. Before sharing his excitement over the invention of the transistor and other progress, he first expressed his dismay about the undesirable atmosphere in the physics community, caused by nuclear physicists' attitude toward non-nuclear physicists.

There is a very marked tendency, on the part of a group of theoretical physicists who have been active in the exploratory, inductive part of the development of the quantum theory, and the theories of atomic and nuclear structure, to be very scornful of any branch of science which reaches this later, applied phase. They consider that when the principles of a subject are established, it has ceased to be science, and no longer interests them. *To this group, the only research in physics at present which deserves the name is in the field of nuclear forces, quantum electrodynamics, and the theory of high energy particles. Only an engineer, to their way of thinking, would concern himself with such things as spectroscopy, electronics, optics, acoustics, or the physics of solid; and they have a very low opinion of engineers.*³⁸⁾

This criticism was not only an expression of his bitterness against nuclear physicists but also a way of legitimizing non-nuclear fields. Slater thus attacked the above

37) Slater, *Solid-State and Molecular Theory*, p. 230; and idem, "Open Problems."

38) Slater, "Open Problems." The emphasis is mine.

attitude as “very dangerous,” on the ground that there was still much room for physicists to study the fundamental principles in most non-nuclear fields before the engineers could take them over. Moreover, he was worried about the unbalanced distribution of intellectual prestige which might result in a shortage of manpower for socially useful disciplines: “by attaching a social stigma to physicists who continue in the field [of non-nuclear physics], there is a great tendency to drive all the able workers out of the field into more exciting, socially acceptable branches of science.” Admitting that many of non-nuclear fields were being transformed from pure to applied science, Slater’s strategy of restoring their values was to exalt any effort to fill the gap between purely theoretical knowledge and practical application. He thus stressed that there were plenty of open problems in this gap, which “can only be filled by the same high level of physical intuition and effort that went into the original understanding of the principles of the subject.” To him, it was the gap that was important to fill, especially “in the American scene, with a great tradition of applying the results of science to practical ends.” In the choice of a material to work on, for instance, Slater insisted that “we should be guided not only by theoretical interest, and ease of preparation of samples, but also, other things being equal, by the interest of practical people in the results.”³⁹⁾ Against the intellectual and social tide in favor of nuclear physics, he reiterated the balanced development of physics and the liaison between pure and applied research.

In the fall of 1950, Slater proposed the formation of the Solid-State and Molecular Theory Group at MIT. The research agenda he set out for the group was the problem of interatomic forces and chemical binding and the theory of energy-band in crystals. Looking back, Slater regretted that the principal investigators of these problems,

39) *Ibid.*

including himself, had been so irresponsible as to let the whole field give "the impression of having fallen into a state of disrepute": the theory of chemical valence was taken up by the chemists, who made unjustified semiempirical extensions of the theory without developing its mathematical ground; no fundamental progress had been made in the theories of metallic structure and of ferromagnetism since 1936; and the theory of crystals, despite Löwdin's recent work, still remained largely unexplored.⁴⁰⁾ While pursuing the fundamental theory of molecules and solids, Slater nonetheless paid keen attention to what was needed by society. "Industrial and government laboratories are clamoring for solid-state physicists," he noted. "What they find, when they look for them, is that experimental solid-state physicists are hard to find, and theoreticians in the field, particularly those with a good understanding of practical problems, are almost unobtainable." It was indeed in the context of supplying trained students in a socially significant field that Slater justified the formation of a theoretical group. By the time he wrote the proposal, Slater had already begun the theoretical group informally with several graduate students working with him.⁴¹⁾ His plan was to get more students (around fifteen students), have a few "mature men" (postdoctoral research associates), and invite other faculty members' help in the management of the group which had a proposed annual budget of \$77,000.

The Research Laboratory of Electronics provided a home for Slater's group for several months, but the group was soon ready to start on its own. The aim was ambitious, as Slater later explained: "My reason for breaking away from the Electronic Laboratory was that I felt that a new development comparable in size with the Electronics and Nuclear Laboratories should eventually be made in the solid-state field,

40) John C. Slater, "Proposal for a Molecular Theory Group in the Physics Department at MIT," *JSP*: Slater/Proposal for a molecular theory group.

41) *Ibid.*

and Jerry Wiesner, Ralph Sayers, and the others in RLE felt that it would mean too much of an enlargement of RLE to built [sic] up its solid-state area as much as I felt desirable."⁴²⁾ In February 1951, Slater's Solid-State and Molecular Theory Group started its independent operation through a grant from the Office of Naval Research, and the group issued its first quarterly progress report in July. Before long, the SSMTG was given new quarters in the physics laboratory, in a space that had been previously used for the physics-chemistry-mathematics departmental library.⁴³⁾

Nevertheless, Slater continued his efforts to have diverse links with research laboratories, especially with the Lincoln Laboratory, which had been constructed during the Korean War to deal with practical problems associated with air defense.⁴⁴⁾ The benefit was not only the social recognition of the usefulness of the SSMTG but also financial support for this theoretical group: in fact, several of its members were on the Lincoln payrolls throughout the 1950s. More importantly, Lincoln's prestigious Whirlwind computer — the brain of its SAGE (Semi-Automatic Ground Environment) project — became available, though only during the night hours, to the members of the SSMTG.⁴⁵⁾ In 1962, Slater's theoretical group came to serve as a nucleus for the establishment of the Center for Material Science and Engineering, a coalition of several existing and proposed laboratories.⁴⁶⁾ By that time, it had grown to a research group granting six doctoral degrees a year and having a total of thirty-six persons: 4 faculty members, 14 post-doctoral workers, 13 graduate students, 3 temporary staff members, and 2 secretaries. One of the faculty members, Michael P. Barnett, was in charge of

42) Slater to Victor F. Weisskopf (Oct. 11, 1966), JSP: MIT #131-132.

43) Slater, *Solid-State and Molecular Theory*, pp. 237-238.

44) On the creation and development of the Lincoln Laboratory, see Leslie, *American Science*, pp. 32-43.

45) Slater, *Solid-State and Molecular Theory*, p. 265.

46) For a detailed account of the creation of the Center for Materials Science and Engineering, see Leslie, *American Science*, pp. 201-211.

the work on the theory of polyatomic molecules and helped other members' calculations by running the Cooperative Computation Laboratory.⁴⁷⁾ This was a far cry from the days when Slater alone had conducted research. Slater now had researchers to work with and computers to calculate with. Although his SSMTG never transformed itself into a research institute comparable to the RLE or the LNSE, it certainly represented a changed way of conducting research. Like Mulliken, Slater became a leader of his own group.

In 1964, as Slater got closer to his retirement age at MIT, he decided to move to the University of Florida in Gainesville to join the Quantum Theory Project of which Löwdin was director. Löwdin had already established the Quantum Chemistry Group at Uppsala in 1955 as a "miniature" of Slater's SSMTG, and five years later started another "sister group" in Florida, dividing his time to run both groups. They were modeled after SSMTG not only in terms of research programs but also in terms of organizational objectives — interdepartmental, interdisciplinary, and also international.⁴⁸⁾ As part of the Florida group, Slater was able to continue his research on the electronic structure of molecules, crystals, and metals, until his death in 1976.

The distance between Cambridge and Gainesville, however, did not deter Slater from having a certain role at MIT. He was particularly anxious to keep informed of, and sometimes influence, what was going on in the SSMTG — its research, accounting, funding, and appointments — and furthermore tried to spend summer with its members for some years.⁴⁹⁾ But without Slater's leadership, the SSMTG fell into a decline.

47) "Proposal for Extension of [ONR] Contract Nonr-184(34) for Solid-State and Molecular Theory Group, from December 1, 1964 to November 30, 1965," **JSP**: MIT #78; G. F. Koster to Slater (Nov. 22, 1964), **JSP**: MIT #86.

48) Löwdin to Slater (Sept. 21, 1955), **JSP**: Löwdin #8; Löwdin to Slater (Nov. 10, 1959; Mar. 31, 1960), **JSP**: Löwdin #12.

49) For example, see Slater to George F. Koster (Oct. 27, 1964; Nov. 4, 1964; Jan. 28, 1965; Feb. 10, 1965;

Barnett returned to England, and the decision was soon made to close the Cooperative Computation Laboratory.⁵⁰⁾ Moreover, the future of John H. Wood, who was working in the calculation of energy bands of metals and semiconductors, was also uncertain.⁵¹⁾ Among the faculty members, only George F. Koster remained active, conducting research on symmetry properties of crystals and the splitting of the energy levels of ions in paramagnetic crystals. Accordingly, the contract with the Air Force Cambridge Research Laboratory, which had supported some of the polyatomic work, was discontinued in April 1964; and the NSF grant for the SSMTG was decreased, as Slater carried some portion of it to Florida. The SSMTG limped along on ONR support of \$35,000 a year.⁵²⁾

In the beginning of 1965, deeply concerned about the future of the SSMTG, Slater asked Koster: "What are your present thoughts about what you want to see eventually? Do you want to keep the SSMTG going as such, or merely to have your own students? Do you want to continue the QPR [Quarterly Progress Report]'s? I'm quite contented, if you would prefer, to close them down at an appropriate time, and maybe start something equivalent here. I feel the whole future of the effort at MIT should be thought about very definitely now, so that we can take any necessary steps this summer while I am on hand."⁵³⁾ Koster, almost left alone, had more modest ambitions for SSMTG. "My own thoughts about the future are not completely formulated in detail but I have a fair idea of what I envision," he replied. "Right now I do not visualize

Dec. 2, 1965; Sept. 15, 1966) and Slater to W. W. Buechner (Sept. 16, 1966), *JSP*: MIT # 85, 86, 133, 149, and 130.

50) Koster to Slater (Nov. 22, 1964), *JSP*: MIT #86.

51) *Ibid*.

52) Proposal for Extension of [ONR] Contract Nonr-184(34) for Solid-State and Molecular Theory Group, from December 1, 1964 to November 30, 1965, *JSP*: MIT #78.

53) Slater to Koster (Jan. 28, 1965), *JSP*: MIT #133.

myself running as large a group as you ran while you were here. I think a fair estimate of what I would like in the near future is something of the order of four graduate students, a post doctoral worker and possibly the ability to support an Assistant Professor. This, as I remember, was much as things were when you first started our group and really seems to be the only realistic thing that I should do now. With John Wood leaving and no additional faculty member in our field, I see no need for anything larger than this."⁵⁴) The SSMTG continued at this reduced level until 1970, when it became "practically defunct."⁵⁵)

Who should be held responsible for the demise of this once successful and productive research group in the solid-state and molecular theory? To historians who are familiar with the fate of research schools evolving along the professional life of their leaders, the case of the SSMTG presumably comes as no surprise: it started as Slater's small research group, developed according to his vision, and lost its vitality after he left. Mulliken's LMSS took a similar path. For Slater, however, the decline of the SSMTG ought to be ascribed to the lack of commitment by MIT administrators. He thought their failure to make new appointments stemmed from the situation of a physics department dominated by nuclear physicists. His worry and distress peaked when he heard Victor Weisskopf became head of the department in 1966. In his letter to Weisskopf, therefore, Slater offered a short congratulation combined with a long admonition about the importance of maintaining the department balanced and broad.⁵⁶) "I still take a lot of interest in the M.I.T. department in general, and more particularly in the Solid-State and Molecular Theory Group, and in the Center for Materials Science and Engineering," he explained. "These are the places where my major effort has gone

54) Koster to Slater (Feb. 18, 1965), **JSP**: MIT #133.

55) Slater, "Abstract of book," p. 16, **JSP**: Slater/Abstract of book.

56) Slater to Weisskopf (Oct 11, 1966), **JSP**: MIT # 131. The emphasis is mine.

since I stopped directing the department in 1951, and I am very anxious to see them continue to prosper. Consequently I thought it worth while writing you some of my views about this part of the department and the Institute, *since I suspect they have been rather far from your interests.*"

After briefly describing to Weisskopf the ways in which the SSMTG and the CMSE came into their present state, Slater addressed what he considered the most urgent problems. First of all, he pointed out unbalanced appointments.

In spite [sic] of all this formal organization and building, however, I don't feel that we have as good a staff in the physics department in the solid-state field as we should have. For several years I have been trying to encourage additional appointments in the field, so far with small success. Of course, a number of the appointments whom I have pushed since the war have added to the department strength in the field. . . . But I still feel that both on the theoretical and the experimental side we need additional strength. *We do not have a big enough staff in the solid-state and non-nuclear field to balance the great size of the nuclear and high-energy part of the department, though of course we have great strength.* . . . But we rather lack men of the outgoing type who would give leadership in the solid-state area. . . . We need more people, and of course several strong efforts were made during the past few years to make first-class appointments, but without success.⁵⁷⁾

Another great concern was the relationship between the physics department and the engineering departments, which arose in connection with the physics department's commitment to the CMSE. While more active work in energy bands was being carried out in the electrical and metallurgy departments than in physics, he found physics department members "tend to be very narrow, and consider what is going on in their own department." "This is," he maintained, "what the interdepartmental laboratories are supposed to prevent." He thus advised the new department head to "take a very active interest in the efforts of other departments to incorporate the results of physics

57) *Ibid.*, pp. 2-3. The emphasis is mine.

into their fields." He felt certain that "these two departments of metallurgy and electrical engineering would welcome active cooperation of the physics department in the teaching and research in solid-state physics and its applications"; and that "they would enormously appreciate a chance to talk over with you, and the solid-state physicists, some of the problems of joint interest to the departments."⁵⁸⁾

Slater never concealed his impression that non-nuclear fields had been the "step-child of the department" during the last few years. He ended the letter, with the hope that Weisskopf would aim "to have a well-balanced department, where all parts of the field will receive their proper emphasis and support."⁵⁹⁾ Weisskopf, in reply, promised that he would work toward resuming the tradition of a well-balanced department, since he was also interested in the unity of physics. As for the decline of the SSMTG, however, his diagnosis was different from Slater's. "I am very much worried about the Solid State Division since it has never been very satisfactory," he told him. "There are a number of very good people in it, but there is a lack of unity of purpose."⁶⁰⁾ To Weisskopf, the real problem was management of the SSMTG, rather than the level of administrative. Understandably, Slater never retreated from his view that the department had been "literally captured by the nuclear theorists."⁶¹⁾

4. The social niche of quantum chemistry

The creation of the LMSS and the SSMTG and their fall in less than two decades certainly reveal the difficulties that Mulliken and Slater had to face at Chicago and

58) *Ibid.*, p. 4.

59) *Ibid.*, p. 5.

60) Weisskopf to Slater (Dec. 1, 1966), *JSP*: MIT #132.

61) Slater, *Solid-State and Molecular Theory*, p. 275.

MIT during the postwar period. Were these cases examples of personal travails that happened in local settings, or did they epitomize a larger trend within the physics community? In other words, was the American physical profession “balanced” with respect to its subdisciplines? As we move on to a larger context of American physics, the issue of balance that Mulliken and Slater often raised within their departments becomes less transparent and more complex.

In the 1940s there was the movement to create divisions within the American Physical Society (APS).⁶²⁾ The initiative came largely from industrial physicists who thought themselves underrepresented in the society. As one letter to the society complained, it seemed that “modern physics has become largely a study of electrons and nuclei with less attention given to anything as large as a molecule.”⁶³⁾ Some specialties had already broken away from the APS: the Optical Society of America (1916), the Acoustical Society (1929), and the Society of Rheology (1929). Worry about the fragmentation of the physics community had been strong after World War I, but weakened during the Great Depression when industrial researchers became more concerned about keeping their jobs than forming independent groups. In the early 1940s this worry surfaced again. New organizations were created, such as the American Society for X-ray and Electron Diffraction (1941) and the Electron Microscope Society (1942); and two proposals were sent to the APS in 1942 for a division of textile physics and a division of industrial physics. Both proposals failed to get endorsement from the APS Council, probably because one seemed too narrow and the other too separative.

62) A detailed description of this movement, especially on the creation of the solid-state physics division, is in Spencer R. Weart, “Solid Community,” in *Out of the Crystal Maze: Chapters from the History of Solid-State Physics*, eds. Lillian Hoddeson, Ernest Braun, Jürgen Teichmann, and Spencer Weart (New York: Oxford University Press, 1992), pp. 617-669.

63) Quoted in *ibid.*, p. 632.

However, these proposals at least provoked a further discussion of divisions and helped to set up guidelines for them. The next year, Ladislaus Marton who had recently moved from RCA to Stanford, led a petition for the Division of Electron and Ion Optics. Announcing the Council's endorsement of the first division, its secretary Karl K. Darrow invited suggestions and propositions for other divisions, "each to be limited in scope to a particular field or fields of physics." Interestingly, the limitation should be "reasonably clear-cut" but at the same time inclusive enough to work toward "a unification of the physicists called *academic* and the physicists called *industrial*."⁶⁴ In 1944, along with the Division of Electron and Ion Optics, the APS formally endorsed the Division of High-Polymer Physics, which extended the proposed division of textile physics to include the physical studies of plastics, paper, paint, and elastic polymers. In addition, the Council received tentative proposals for divisions of biophysics, spectroscopy, chemical physics, and solid-state physics.

Mulliken was one of the supporters for the division of spectroscopy. At the meeting of the American Physical Society held in December 1944, Mulliken advocated the general rationale behind establishing divisions — encouraging closer integration of academic and industrial physicists.

Referring first to the relations between science and industry, I believe the natural and desirable trend is for these to become closer. This trend is well illustrated by the word "electronics, . . ." This trend in its turn is part of a larger movement toward the re-integration of the sciences with one another, and of science with industry, government, and the whole of society.

There is however, a counter-trend in which the academic scientists and the industrial or applied scientists each tend to flock by themselves, because the academic scientists sometimes lack interest in the problems of industry, and the industrial people find the papers presented by the academic scientists too theoretical or too highbrow. Many of us believe that a divisional organization in the Physical Society presents possibilities for more readily bridging this gap

64) Karl K. Darrow, "Current Trends in the American Physical Society," *Journal of Applied Physics* 14 (1943), pp. 437-438.

between academic physicists and industrial physicists in each special area. . . . The bridge to be built should be primarily for the purpose of increasing contact and free interchange of ideas between academic physicists and industrial physicists.⁶⁵⁾

The proposed spectroscopy division, Mulliken believed, would serve this mission of bridging the gap between academia and industry. According to him, spectroscopy was in its narrowest sense a branch of science which dealt with the analysis of light from various natural sources and the theory of the instruments and techniques for doing this; in a broader sense, spectroscopy also embraced a fundamental understanding of spectroscopic phenomena, which led to the fields of atomic and molecular structure, biochemistry, and astrophysics. Mulliken saw no clear boundary between spectroscopy as such and applied spectroscopy in the service of other fields, which themselves were largely pure sciences. Furthermore, he claimed a fertile ground between these fields of spectroscopy and spectrochemical analysis, which was largely, though not entirely, industrial spectroscopy. To his disappointment, however, the division of spectroscopy failed to gain endorsement, probably because it appeared to overlap with the existing Optical Society or the other proposed divisions.

The creation of the Division of Solid-State Physics involved long and fierce internal battles.⁶⁶⁾ J. H. Van Vleck staunchly opposed the proposal on the grounds that it would only foster the "Balkanization" of the Physical Society. As Darrow noted, there was a "marked difference between those who want the Divisions restricted to marginal topics such as have not hitherto been fully represented in the programs of the Society [like the Division of High-Polymer Physics and that of Electron and Ion-Optics], and those who have no such restrictions."⁶⁷⁾ Solid-state physics, embracing all solid matter

65) Robert S. Mulliken, "Remarks on a Possible Division of Spectroscopy in the American Physical Society," *Review of Scientific Instruments* 16 (1945), pp. 42-43.

66) For this story, see Weart, "Solid Community," pp. 629-640.

whether it was a crystal or metal, seemed to be so broad as to possibly bring a reorganization of a major portion of the physics community. Through numerous Council meetings which often "turned to its favorite pastime of discussing the question of Divisions,"⁶⁸⁾ a decision was made in favor of broad divisions, and solid-state physicists could finally have their own division in 1947. Presumably this decision also affected the formation of yet another broad one, the Division of Chemical Physics (1951), in which Mulliken came to play a central role.⁶⁹⁾

By the early 1950s, therefore, Mulliken and Slater had not only research groups in their departments but also divisional grounds within the APS. The establishment of these institutional footholds, on the one hand, reflected continuous growth of their specialties. Occasionally non-nuclear physicists dominated some APS meetings, especially the March ones where the solid-state division usually held their annual meeting, to the extent that nuclear physicists would "feel rather lonesome."⁷⁰⁾ When a debate took place in 1955 regarding the split of the APS's *Physical Review*, the Council proposed two sections — the section A of "Solid State," "Atoms, Molecules and Ions," and "General Physics," and the section B of "Nuclear Physics" and "High Energy."⁷¹⁾ Certainly the research production of non-nuclear physics was comparable to that of non-nuclear counterpart. By 1970, physics of condensed matter (solid as well as liquid, glassy, and other amorphous substances) alone far exceeded nuclear physics

67) Quoted in *ibid.*, p. 639.

68) In one of Darrows record of the Council Minutes of APS in 1947. Quoted in *ibid.*

69) Formation of a Division of Chemical Physics in the American Physical Society, *Bulletin of the American Physical Society* (Nov. 24, 1950), p. 4.

70) Karl K. Darrow, "Pittsburgh Meeting, March 8-10, 1951," *Bulletin of the American Physical Society* 26 (1951), pp. 3-4.

71) For this proposal, see Weart, "Solid Community," p. 642. It was not until 1964 that *Physical Review* was broken down into sections A and B, as had been proposed a decade before.

(including high-energy and particle physics) in terms of manpower and publications.⁷²⁾ Presumably this impressive growth in numbers had to do with a successful linkage between academic and industrial physicists, as Mulliken and Slater had wished to establish.

On the other hand, the overall development of non-nuclear divisions does not necessarily mean that intellectual and social prestige followed in proportion. To the contrary, as divisional symposia were concentrated on a few APS meetings of the year, the Annual Meeting in January came to be "dominated by nuclear physics, and its generality [was] in danger of being lost."⁷³⁾ At the Washington meeting of April, which was usually the largest gathering of the year, "the preponderance of nuclear physics [had] become customary."⁷⁴⁾ Despite ever increasing divisional activities, nuclear physicists still dominated the APS, so that they felt no need to set up a special division of their own.⁷⁵⁾ This dominance was even more striking in the physics departments of the U.S. universities. In the 1970s, for example, about a half of the physics faculty at MIT were nuclear physicists, while solid-state physicists counted not more than a tenth. At Chicago, nuclear physicists constituted a third of faculty members; and most of other major universities kept nuclear physicists in the range between a third and two third of the physics faculty. When it comes to the distribution of budget, the dominance of nuclear physics was even more glaring. In many universities, this field took more than two thirds of the department budget. Particularly where there were research institutes like the Laboratory of Nuclear Science of MIT and the Lawrence

72) National Academy of Sciences, *Physics in Perspective Vol. 1* (Washington D.C., 1972), pp. 87-91.

73) Karl K. Darrow, "1952 Annual Meeting at New York," *Bulletin of the American Physical Society* 27 (1952), p. 3-5.

74) Karl K. Darrow, "Proceedings of the American Physical Society: Minutes of the 1953 Spring Meeting Held at Washington D.C.," *Physical Review* 91 (1953), p. 428.

75) For the dominance of nuclear physicists with APS, see also Weart, "Solid Community," p. 641.

Laboratory of Berkeley, the money from these organizations to nuclear physics tripled and quadrupled the total of the department budget (See Appendix). Clearly the balanced department Mulliken and Slater had sought had fallen out of fashion.

Within two decades after the war, the rank of occupational status of nuclear physicists jumped from the fifteenth to the third, ahead of everyone except Supreme Court Justices and physicians, in American minds.⁷⁶⁾ "Physics is fun," a watchword often heard from the Los Alamos generation like Richard Feynman, reflected their newly won status.⁷⁷⁾ To Mulliken and Slater, as well, physics was undoubtedly fun. But after the war, they had to justify their brand of physics not so much by saying it was fun as by painstakingly saying it was useful. Although physics was so integrated into American society in the postwar period that some physicists could claim their study for its own sake, not every physicist felt the same way. The social niche of quantum chemistry — a field on the borderland of molecular physics, chemical physics, physical chemistry, and solid-state physics — did not come as a social reward for the wartime contribution. Instead, it was earned by personal struggles against a dominant force in the physics community, nuclear physics. Examining the cases of Mulliken and Slater, this essay does not deal with quantum chemistry in the context of chemical research and education. But it is small wonder that these days see more quantum chemists associated with the chemistry department than the physics department.

76) Kevles, *Physicists*, p. 391.

77) 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*, eds. Michelangelo de Maria, et al (Singapore: World Scientific, 1989), pp. 96-104. He argues that as physics was strongly integrated with other social sectors in postwar America, the physicist's self-image was changed from the "virtuous" or the guardian of morality to the "virtuoso" or the hedonistic expert. See also Mary Jo Nye, *Before Big Science: The Pursuit of Modern Chemistry and Physics 1800-1940* (London: Twayne Publishers, 1996), pp. 225-229. It seems to me, however, that not every physicist could afford such a transformed self-image.

Appendix: Distribution of faculty and budget in American physics departments in 1975

Institution	Fields	Number of Faculty	Size of Budget (\$)
MIT	Nuclear	46 (50)*	68,000 (3)
	Atomic and Molecular	5 (5)	812,000 (37)
	Solid-State	8 (9)	808,000 (37)
		total: 93	total: 2,209,000 cf) Laboratory of Nuclear Science had \$7,350,000.
Chicago	Nuclear	24 (34)	1,396,000 (30)
	Atomic and Molecular	5 (7)	230,000 (5)
	Solid-State	10 (14)	373,000 (8)
		total: 71	total: 4,604,000
Cornell	Nuclear	19 (26)	Laboratory of Nuclear Studies had \$4,058,000.
	Atomic and Molecular	6 (8)	
	Solid-State	20 (28)	Laboratory of Atomic and Solid-State Physics had \$1,400,000.
		total: 72	
Berkeley	Nuclear	28 (41)	226,000 (21)
	Atomic and Molecular	5 (7)	33,000 (3)
	Solid-State	13 (19)	451,000 (41)
		total: 68	total: 1,100,000 cf) The Lawrence Laboratory's support of nuclear programs which included physics graduate students amounted to \$5,000,000.
Yale	Nuclear	42 (75)	3,400,000 (88)
	Atomic and Molecular	11(20)	325,000 (8)
	Solid-State	.	.
		total: 56	total: 3,875,000
Princeton	Nuclear	13 (30)	1,951,000 (62)
	Atomic and Molecular	.	.
	Solid-State	6 (14)	117,000 (4)
		total: 44	total: 3,126,000
Columbia	Nuclear	26 (60)	4,222,000 (62)
	Atomic and Molecular	5 (12)	619,000 (9)
	Solid-State	4 (9)	275,000 (4)
		total: 43	total: 6,799,000
Harvard	Nuclear	17 (45)	1,480,000 (60)
	Atomic and Molecular	4 (11)	360,000 (15)
	Solid-State	7 (18)	305,000 (12)
		total: 38	total: 2,450,000
Stanford	Nuclear	13 (57)	2,950,000 (78)
	Atomic and Molecular	4 (17)	300,000 (8)
	Solid-State	.	.
		total: 23	total: 3,760,000

Source : American Institute of Physics, *Graduate Program in Physics, Astronomy and Related Fields, 1976-77* (New York: American Institute of Physics, 1976).

* Numbers in parenthesis denote percentages.