The Local Origins of United States National Science Policy

by

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ABSTRACT

World War II is frequently used to demarcate eras in United States science policy. However, that perspective masks the continuity implicit in the careers of those who administered the US government's research during World War II. Here attention is called to how the pre-war experiences of these administrators shaped national policy. This approach fills a lacuna in the historiography of American science and illuminates the ideals and politics that produced the pluralist science policy apparatus of the US government.

The importance of MIT to the managers of the government's wartime research is the basis for the dissertation's structure. Vannevar Bush, the chairman of the National Defense Research Committee (NDRC), had spent twenty years as professor and vice-president of MIT. His fellow committee members, most notably MIT's president Karl Compton, were men Bush had dealt with during his tenure at MIT. And the largest NDRC university contractor during the war was MIT. Part I examines the conditions that led the MIT Corporation to hire Compton as president in 1930. Part II discusses the relationships Compton and Bush sought among the MIT administration, the faculty, and external patrons of research. Part III argues that NDRC practiced the style of patronage they had wanted to receive at MIT. Part IV analyzes the debate over establishing a postwar science agency as a response to the administrative framework they wished to impose on science policy.

The term "local" in the title reflects both that the ideals of important founders of science policy originated within MIT, and that their desires exacerbated federalist tensions over dividing power between local and national authorities. Within MIT, Compton increased the powers of the presidency in order to promote specialization through multi-disciplinary cooperation rather than intra-disciplinary competition. NDRC rewarded universities whose administrators assembled multi-disciplinary teams that could address military functions rather than disciplinary groups to discover and apply knowledge. They were unable to institutionalize their approach in a permanent national science agency because partisans of presidential management of the government would not accept an agency devoted to strengthening local authorities.

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"Man ist was man ist." Ever since botching Feuerbach's aphorism during the dictation section of a German exam—despite having already read him in English—I have been fond of playing with it to justify assorted lines of inquiry. We are what we do for a living (labor studies); we are what we accept as just authority (political studies); we are what we use our money for (economic studies); etc. When I reach intellectual history, I arrive at: "we are what we read." Assigning books and arguing their importance are professors' principal means to influence students. I hope mine, especially Loren Graham, Eugene Skolnikoff, Ted Greenwood, Merritt Roe Smith, and Peter Buck, feel gratified in the way this dissertation reflects and transforms the books they urged upon me. And I hope the archivists who assisted me, especially Kathy Marquisse and Helen Samuels, likewise feel gratified in the way I have used preserved documentation for a contemporary audience.

Many people have helped me in other-than-intellectual ways, so I will only name those I would feel abjectly remiss not to. Don Blackmer and Dick Samuels have been patient, sympathetic academic administrators. Joan Warnow-Blewett and Spencer Weart, my supervisors at the Center for History of Physics, have backed their encouraging words with a flexible attitude towards balancing the pressures of my job and this work. Molly and Saul Genuth's financial support was indispensable. I often asked Abigail Cooper to listen and respond to me; she always made time no matter the difficulties she was facing. Peter Luton and Elizabeth Krier, though less frequently asked, did likewise. I reserved and paid for Joan Sealy's time, but it is no less appreciated. My children, Miriam and Naomi, daily reminded me that we build the future on our view of the past, so we damn well better have an empirical basis for that view. They owe their existence to my wife, Sara Schechner Genuth, whose constancy of love has sustained me to enjoy this season and to look forward to others.
CHAPTER 1
INTRODUCTION: WHAT’S IN A TITLE?

This dissertation’s title is shorthand for its central historical claim. The people who mobilized non-governmental scientists for government-sponsored research during World War II, and who pressed for a distinctively structured government agency to continue federal support for non-governmental research following the war, generalized on their pre-war experiences in administering localized research institutions and believed that localized research institutions should be the principal managers of their scientists’ research ambitions and specializations. This dissertation is not entitled "Foreign Precedents for State-Supported Research and the Origins of U.S. National Science Policy," or "The Development of National Scientific Communities and the Origins of U.S. National Science Policy," or "The Rationalization of Industrial Standards and the Origins of U.S. National Science Policy," or "The Needs of Public Administration and the Origins of U.S. National Science Policy." A post-war science agency had, at minimum, the potential to affect these areas, and there were individuals, scientists and non-scientists, who wanted an agency designed as a response to one of these areas. However, those who held administrative power during World War II were not of their ilk and were both intellectually and institutionally capable of setting forth their version of a post-war science agency. The contrasting design criteria and the coalitions that formed among the advocates of various perspectives made the creation of a federal science agency politically problematic.

The rationale for approaching national science policy as a generalization on the policies of a local research institution lies in an institutional commonality among important individuals. Vannevar Bush, the chairman of the National Defense
Research Committee (NDRC), which was created at his behest in 1940 to organize non-governmental scientists for military research, had spent twenty years as a faculty member and administrator of the Massachusetts Institute of Technology. His fellow committee members, Karl Compton, president of MIT, Frank Jewett, president of Bell Laboratories, James Conant, president of Harvard, and Richard Tolman, dean of graduate students at the California Institute of Technology, were men Bush had dealt with, cooperatively or competitively, during his tenure at MIT. And the largest NDRC university contractor during the war was MIT. These men all brought to government service ample experience in judging research programs, inspiring enthusiasm for the work they thought promising, and securing adequate funding and providing appropriate administration for the researchers. If we assume that a group’s present efforts and imagined future are built on its past experiences, then it is imperative to interpret the efforts of NDRC’s organizers during World War II and the post-war science agency they envisioned for the peace in terms of the regime that Compton and Bush carved out for research at MIT in the 1930s.

Capturing the continuity from pre-war practices to post-war plans poses the historiographic problem of choosing an appropriate, enlightening locus of activities to examine. The solution pursued here is to begin with the nexus among the formation and pursuit of research specialties within MIT, the administrative structure of MIT, and the policies and preferences of patrons; and then to move to the nexus among the formation and management of wartime research projects, the administrative structure of NDRC, and the political rhetoric and partisan character apparent in discussions of the division of powers within the United States’ national government. The presumption, which the history to follow is supposed to bear out, is that scientists politick for the administrative frameworks that are conducive to the style of specialization with which they have grown intellectually and professionally comfortable. The heart of the thesis, Parts II and III, show that Karl Compton consistently used the MIT presidency to attract philanthropic support for particular kinds of intra-MIT research specialties and that Vannevar Bush organized NDRC to function within the national government in the way Compton wanted the Rockefeller
Foundation to function with respect to universities in the 1930s. Because research specialties form and develop in a variety of ways, and because the administrative framework and scope of a permanent government agency had to be outlined in its legislative charter, differences among scientists in their intellectual experiences and research priorities became politicized when World War II inspired contrasting proposals for a peacetime science agency.

This approach to the origins of US national science policy yields history that is unconventional in its combination of contours and periodization. While the contours of socially oriented histories of science are typically a single discipline and its development across a range of localized institutions—a point quintessentially announced in the title of Daniel Kevles' well-known book, *The Physicists: The History of a Scientific Community in Modern America*\(^1\)—this history seeks to compare the development of multiple specialties within a university. The virtue of this approach is its illumination of what the researchers who became university administrators and then national policy-makers understood to be the proper division of authority among patrons, university administrators and research faculty. In Kevles' disciplinary perspective, even though his work more than encompasses the period covered here, Bush, an electrical engineer, and Compton, a physicist whose research in the 1920s was not strategic to the development of quantum mechanics, barely appear until they became influential in national policy. Within the framework of an intra-university comparison of specialties, however, their pre-war research and their management of others' research are central. The history to follow may seem parochially MIT-centered, but that is one of its points; such an approach is necessary to understand Bush's and Compton's policy preferences after they had gained governmental power.

Institutionally oriented histories of modern American science typically use World War II as a beginning or end point of their periodization because of the

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\(^1\) (New York: Alfred Knopf, 1977); see also *Osiris. Historical Writing on American Science*, 1 (1985), Sally G. Kohlstedt and Margaret W. Rossiter, eds., whose largest section is devoted to "Science in Specialties."
quantitative and qualitative changes that the war forced on government agencies and the nation's research universities. A. Hunter Dupree's now classic work, *Science in the Federal Government*,\(^2\) terminates at 1940 with a chapter entitled "Prospect and Retrospect at the Beginning of a New Era," and this periodization continues to be followed.\(^3\) However, here no institution or set of institutions is itself the focus, but rather the objective is to show how the problems and prospects of particular institutions shaped the views of individuals whose careers spanned the war and whose efforts spawned a post-war political battle. Thus the wartime practices of NDRC are treated as the link between the pre-war organization of specialties at MIT and the post-war controversy over establishing a National Science Foundation. The history to follow may seem to shift institutional levels discontinuously as it moves forward in time—from the efforts of faculty to establish research specialties, of university administrators to negotiate arrangements for research with professional philanthropists, of government executives to define the scope and structure of a wartime agency's programs, of participants in a widespread debate over the political values and research strategies that a permanent government science agency ought to embody. But that again is one of this history's points; such an approach captures the continuity and political conflicts engendered when individuals assumed positions of broad influence on the strength of their successes in particular circumstances.

Part I of the dissertation examines the interaction of faculty research ambitions with the MIT presidency between 1900 and 1930. It provides a framework for understanding the character of Compton's presidency by establishing what the precedents were at MIT for connecting the formation of research specialties to MIT-wide administration and policies, what the responses of MIT's presidents had been to


the pressures that faculty research placed on them, what research groups had gained the intra-MIT prominence to influence MIT-wide administration, and thus what the forces were that brought Compton, an atomic physicist with no university-wide administrative experience, to the MIT presidency in 1930. The medium through which this framework is built is a comparison of the efforts of four faculty members to plant their favored research specialties within MIT: William Sedgwick in biology, Arthur Noyes and William Walker in chemistry, and Dugald Jackson in electrical engineering. These four men were the first faculty with post-graduate educations that inclined them to make research part of their and their colleagues’ duties.

Historiographically, the treatment accorded these men takes its cues from the work of Spencer Weart and Charles Rosenberg. Weart has called attention to variations in the ways research fields form. Solid state physics, he points out, did not form in the vegetative manner that specialties are presumed to form—that is, as offshoots from a central stalk when bodies of theory or experimentation become sophisticated enough that their pursuit become a full-time occupation. Instead, solid state physics formed by reclassifying diverse, extant specialties as a single field on the basis of shared intellectual elements and the beneficial social relations reclassification would foster.4 Rosenberg, in calling for an "ecology of knowledge" that clarifies relations between learned disciplines and professional practices, stresses the importance of understanding how university departments and the division of authority between departments and central university administrations shape the vocational identities and research aspirations of scholars.5

The objectives of the discussions of research programs in Part I are to identify (à la Weart) the resources from which the research leaders hoped to build viable

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specialties and to identify (à la Rosenberg) the administrative relationships and the intra-university organization of power that each research leader believed would provide him with the resources he desired. Thus the role of MIT’s president and the ambitions of individual faculty are viewed as creating both positive and negative feedbacks. As researchers envisioned productive research programs, they lobbied presidents (or were frustrated by their lack of clout or poor political instincts) to orient the presidency towards fostering such work. As presidents politicked for the relations they wanted among the presidency, faculty members, and patrons, they attracted faculty whose research ambitions fit the social framework (or discouraged faculty whose ambitions did not). The selection of Karl Compton as MIT’s president in 1929 represented the victory of advocates of a particular set of feedbacks that originated, within MIT, in William Sedgwick’s Biology Department.

Part II comprises a discussion of Compton’s presidency. The historiographic themes from Part I are continued, but the center of attention rests further from faculty members and the pressures they placed on the presidency and closer to Compton and how his policies, selection of MIT-wide administrators (most notably Vannevar Bush as vice-president), and intellectual predilections placed pressures on research-oriented faculty. The objective is to show that Compton made the MIT presidency an activist broker between faculty and research patrons so that intra-MIT politics, rather than the deliberations of the national philanthropic foundations or the intellectual organization of the national and international scientific societies, would be the principal institutional arbiter of the faculty’s research specialties. The result was a conscious effort to downplay departmental distinctions and disciplinary unity based on the urge to deepen or explain shared cognitive commitments or the urge to improve production processes of an industrial sector. Instead, Compton’s MIT stressed the study of those natural objects and the development of those scientific instruments that fit into a multiplicity of disciplinary contexts. His self-assumed role was to stimulate the creation of new specialties through local reorganizations of members of multiple disciplines and specialties around their common interests in improving the power and flexibility of particular instruments, experimenting on particular natural objects, and
broadening the scope of theories that were relevant to these instruments and objects.

Part III shifts the center of discussion to NDRC. With the character of Compton’s presidency specified against the backdrop of intra-MIT precedents and through analysis of his reform efforts at MIT, Compton’s and Bush’s efforts for NDRC can be understood as building a government program on the basis of their past parochial successes. The disgruntlement NDRC’s policies generated can likewise be understood as the intrusion of concerns that Compton’s powers had stymied within MIT. The boundaries of NDRC’s self-definition are discerned through a comparison of the radar and atomic bomb projects—why the first was warmly embraced shortly after the creation of NDRC, remained under NDRC jurisdiction throughout the war, and yielded no lasting antagonisms among researchers and administrators over post-war planning while the latter initially induced grudging, low-level support in NDRC, was transferred to the Army’s jurisdiction once earnest support was forthcoming, and yielded a politicized atomic scientists movement that distrusted administrative authorities on post-war planning.

The relationship between organizational strategy and structure, as elaborated by Alfred Chandler in a business history context, guides this section. The objective is to show that NDRC structured itself as a governmental analog to the decentralized, multi-departmental corporation in order to build the relations among patrons, university officers, and researchers that Compton had sought from the MIT presidency. NDRC’s research strategy was consequently to support the kinds of locally managed, multi-departmental, and instrumentation-driven projects that Compton had made the object of policy at MIT. Vannevar Bush’s administrative achievements as creator and chairman of NDRC were to identify the organizational model of a government agency that would grant authority to local research administrators and to keep NDRC’s jurisdiction from being either narrowed or expanded to the point that it would centralize its powers. For Bush, a national

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science agency was best headed by a council of research administrators who could thrash out the institutional difficulties raised when local administrators tried to focus the attention of researchers with varying disciplinary perspectives on objects of common interest, not by an executive who would be attracted to the problems of coordinating the efforts of functionally differentiated groups of researchers.

The final section uses this understanding of NDRC’s structure and strategy to reinterpret the post-war political debate over creating a National Science Foundation. Instead of focusing on areas of conflict between academic and political values, conflict over the proper legislative provisions for a NSF is interpreted as due to a variety of responses among scientists and politicians to the prospects of a permanent, decentralized, committee-managed science agency. Disagreements among scientists over the legislation are traced to their professional sensitivities over the proper basis for specializing and the appropriate administration for managing specialization. Differences among politicians over the legislation are traced to their sensitivities over the division of powers between legislative and executive branches and the appropriate degree of authority to grant the president to manage executive branch agencies.

Methodologically, this section takes inspiration from J.G.A. Pocock. Pocock calls for understanding political thought and debate as responses to more immediate realities than are apparent in the universalistic, grandiose terms that typically characterize political debate and certainly characterized this one. NDRC’s structure and strategy and their suitability to serve as a peace-time model were obvious, concrete touchstones for those interested in building a permanent link between academic scientists and the national government. Substantially, this section takes its inspiration from Don Price, who has pressed for an understanding of science policy’s

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exacerbations of governmental difficulties inherent in U.S. federalism. The conclusion drawn from this history is that science policy could not transcend the problems of federalism because of disputes within the scientific community over the organization of power for the management of specialization. Compton’s and Bush’s preference for university-level management, where presidents and deans could organize disciplinary interests around instruments or natural objects with multidisciplinary appeal, dovetailed with the American political tradition of limiting the national government’s powers, and especially the president’s powers, in favor of maximizing the powers of local authorities. Their critics, who favored specializing intra-disciplinarily on cognitive foundations, gravitated towards the ideal of a science agency that could serve as the presidency’s source of inspiration for initiating or reorganizing government research programs. The advocates of national science policies could not help but recreate the tensions and stresses that have characterized American federalism.

PART I: THE INSTITUTIONALIZATION OF RESEARCH AT MIT
CHAPTER 2

PRECEDEMENTS FOR RESEARCH TRADITIONS AND POSSIBLE ROLES FOR THE MIT PRESIDENCY

In the last decades of the nineteenth century, the notion that university faculty should advance their fields through research, as well as teach their fields to their students, became more than rhetoric in the United States. Americans had been earning doctorates at German universities, where the research seminar and research laboratory were well-established institutions,¹ and returning to the United States eager to continue exercising their freshly acquired skills. With the founding of Johns Hopkins (1876), Clark (1889), Stanford (1891), and the University of Chicago (1892), which all gave pride-of-place (or at least made generous provision for) graduate education and research, not only was a domestic alternative to European study created, but the older universities were forced to innovate in order both to recruit junior faculty and to hold senior faculty who were attractive to (and attracted to) one of the new universities.²


At MIT, the realizations that research was important and innovation was necessary did not lead straightforwardly to conclusions about either the proper content of research or the proper administration for research. No president imposed an intellectually consistent plan for meshing research programs with the school's curricula, its students' professional ambitions, and its need for external financial support. Rather, in the years 1890-1917, the efforts of three faculty members to establish research traditions set possible precedents for relations among faculty, administrators, and patrons: William Sedgwick, who came from Johns Hopkins to lead MIT's efforts in biology; Arthur Noyes, an MIT undergraduate who returned to teach Theoretical Chemistry after obtaining a Ph.D. from Leipzig; and William Walker, who challenged Noyes' leadership of the Chemistry Department on the strength of a Göttingen doctorate and experience as an industrial consultant.

These three men all joined the MIT faculty in the last two decades of the 19th century. Each developed a distinctive approach to turning research opportunities into specialties with firm social foundations. Sedgwick used the diversity of specialties at MIT and Harvard to create a research laboratory and curriculum by reconfiguring the relevant portions of the several disciplines that were strategic to the public health problems of his day. Noyes dedicated a research laboratory to compete internationally with similar laboratories in examining phenomena relevant to the paradoxes and ambiguities in chemical theory of his day; he made his course in chemical theory the apex of chemical education and the entry point to research. Walker laid out a taxonomy of manufacturing processes in chemical industries and structured a curriculum and research laboratory that made his categories the national standard for intellectual specialization in chemical engineering.

Each man enjoyed notable success during his MIT career, but each failed to forge sustainable, productive relations on which his successors could build. The origins and development of each man's program forms the content of this chapter; their respective demises are the subject of the next. Together these chapters exhibit the range of approaches to and problems with the organization of research at MIT. They show that the varied research strategies employed by these three men imposed
contrasting obligations on the MIT presidency and that MIT's presidents lacked the ability or will to create principled policies that would make institutional sense out of faculty research activities.

What turned MIT from an unsystematic spawner of individual research ambitions into a dedicated supporter of particular types of research traditions were the ambitions and powers of its electrical engineers in the 1920s. Dugald Jackson, who joined the MIT faculty as chairman of electrical engineering in 1905, encouraged his appointees to build research programs in Sedgwick's style. When his proteges encountered extra-departmental limitations, the department's size and its powerful supporters compelled presidential attention in a manner that would never have even occurred to Sedgwick. The last chapter in this section shows that Karl Compton came to the MIT presidency to make the formation of locally organized, multi-departmental research laboratories a sustainable academic tradition. His initiatives and their significance for research priorities and for relationships among faculty, university administrators, and patrons are discussed in Section II.

William Sedgwick and "Public Health Science"

The career of William Sedgwick exemplifies one constellation of intellectual and institutional factors that together offered prospects for creating an enduring research tradition. While Sedgwick's graduate education was part of an international movement to produce intellectual reform in biology, at MIT he subsumed his philosophical agenda to the prospects of mobilizing a range of talents within MIT to satisfy the technological needs of a local agency. He found that by selecting biological objects for study on the basis of their relevance to technological processes that interested others at MIT, he could exercise his biological curiosity, attract MIT's vocationally motivated students, and thereby create a new field of teaching and research.
After an unhappy and quickly aborted start at Yale Medical School, Sedgwick obtained his Ph.D. in biology at Johns Hopkins. He studied under Henry Newell Martin, a protege of Michael Foster and Thomas Henry Huxley, and only the second person to teach a course in physiology in the United States. Martin was "deeply committed to establishing the independence of physiology from the needs and attitudes of clinical medicine," a perspective that Sedgwick must have appreciated given his dislike of medical education. Martin's scientific reputation rested on his ability to isolate surgically and experiment on mammalian hearts, and Sedgwick joined him in this work.

This kind of laboratory research, aimed at elucidating the physical workings of individual organs, placed Sedgwick in "the revolt from morphology." As Sedgwick himself interpreted biology's recent past, biologists in the English-speaking world had let the debate inspired by Darwin to dominate their research excessively, and they had persisted in morphological studies well past the time that drawing lines of descent did

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5Martin and Sedgwick, "Observations on the Mean Pressure and the Character of the Pulse Wave in the Coronary Arteries of the Heart," Journal of Physiology, 3 (1881), 165; and "A Study of Blood Pressure in the Coronary Arteries of the Mammalian Heart," Transactions of the Medical and Chirurgical Faculty of Maryland, 83 (1881), 206. On the connection between studies of heart mechanics and Martin's training, see Geison, 266-267.

anything other than affirm Darwin’s insights. \textsuperscript{7} Physiology was relegated to the medical schools, and he saw himself as part of a movement in which "[a]t last…the zoologists and botanists are returning to their own and taking up their old work."\textsuperscript{8} Sedgwick contributed to shifting the balance of biological thought towards physiological and experimental lines and away from morphological and observational ones with his own introductory textbook in "general biology." The intellectual drive to study biology, he asserted, stemmed from the need to understand the differences between lifeless and living matter, not to understand life’s origins and diversification; Sedgwick’s version of general biology did not survey the biological world but consisted of extended discussions of a representative animal (the earthworm) and a representative plant (the fern).\textsuperscript{9}

MIT hired Sedgwick in 1883 as an assistant professor of biology in the Department of Natural History. Initially he promoted his courses in physiology as appropriate preparation for entry to Harvard Medical School, but that plan received support from neither Harvard nor prospective medical students.\textsuperscript{10} However, Sedgwick proved flexible in seeking ways to render his training and research skills useful to and within MIT. The Massachusetts Board of Health had from its inception recruited local experts to aid in the resolution of controversial issues, and William R. Nichols, a chemist at MIT and consultant to the Board, asked Sedgwick to join him in

\textsuperscript{7}Sedgwick, "The Expansion of Physiology," \textit{Science}, 25 (1 March 1907), 332-337, especially 335. Physiology blossomed, Sedgwick maintained, where Darwin seemed less engrossing and a stronger pre-Darwin experimental tradition existed, i.e. in Germany where Johannes Müller and his students remained active.

\textsuperscript{8}Ibid., 337.


\textsuperscript{10}Jordan, et. al., \textit{Pioneer}, 28-30. Nationally, only 8\% of medical students held bachelors degrees, and while Harvard attracted the wealthier and better-educated, still anywhere from half to three fourths of its students did not have bachelors degrees between 1880 and 1890. William G. Rothstein, \textit{American Medical Schools and the Practice of Medicine: A History}, (New York: Oxford University Press, 1987), 93.
a study of the relative safety of coal and water gas as domestic fuel for heat and light. This study opened Sedgwick to new possibilities for his biology's social relations. His work with Nichols required them to pool their skills and perspectives. Chemically speaking, Nichols determined how much carbon monoxide was in a unit of each gas. Physiologically speaking, Sedgwick established that carbon monoxide poisoning was a threshold effect and determined the concentration at which the gas became toxic. Unifying these disciplinarily distinct studies into a coherent report was the authors' concern with whether dangerous levels of carbon monoxide would accumulate in a room in which unlit burners expelled gas. By being sensitive to the needs of the Board and open to the possibility of combining disciplinary perspectives, Sedgwick found he could produce intellectually satisfying and socially useful research.

Sedgwick pursued his relationship with the Board of Health. He was officially appointed biologist to the Board in 1888 and availed himself of the Experiment Station that the Board opened at Lawrence, Mass. in 1887. It did not take long for him to duplicate the kind of success he had had with Nichols, but on a grander scale. The staff at Lawrence was experimenting with various arrangements of intermittent filtration of sewage with soil. As the Board's biologist, Sedgwick's official duty was to develop techniques for determining the biological content of the filters' intake and effluent, since the Board needed a metric on which to judge the efficacy of the filters. But what went on in the filters, he realized, could also be construed as a

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13Rosenkrantz, Public Health, 99-101. MIT had neither the money nor space to support Sedgwick's research work. See Jordan et. al., Sedgwick, 34-35 and 38-39 for descriptions of the Biology Department's finances and quarters at that time.

14Although Sedgwick's prior research had been in mammalian physiology, at Hopkins he had learned the germ theory of disease and embraced it as contributing to biologists' reorientation to laboratory and physiological studies. See Sedgwick, "Bacteria and the Germ Theory of Disease,"
biological issue. He noticed that the filters worked only in the presence of oxygen, that nitrates were present in working filters, and that the filters worked only after they had been in operation for some time, and then never clogged or required cleaning. To account for these results, Sedgwick postulated the need for a "biological theory" of sewage filtration: "an intermittent filter is no longer regarded as a mechanical strainer [which would clog]; nor is it merely a chemical furnace; it resembles a living organism." He suspected that some organism in the filters' sand would, in the presence of oxygen, consume bacteria and excrete nitrates.

With this insight, Sedgwick had fused biological inquiry into the physiologies and relationships of a group of organisms with technological inquiry into the design of sewage filters. Instead of holding to a division of labor among biologists, chemists, and engineers, Sedgwick opened prospects for joint studies in an area of common interest. To his students and junior colleagues who built careers by spreading and implementing the research results and improved filters, the Lawrence Experiment Station represented a singular accomplishment in the sociology of knowledge: "for the first time the sciences of engineering, chemistry, and biology were combined and brought to bear on the problems of water purification and sewage treatment." But from the perspective of MIT's history and the relationship of university researchers to external patrons, Sedgwick's significance lay in his resolve to make MIT a center for the combined study of bacteriology, civil engineering, and public health and in his

Science, 3 (1 February 1884), 133-135. He and his students found they could readily apply Robert Koch's new method for culturing bacteria to the problem of getting bacterial counts, though more technical ingenuity was required to count other micro-organisms. See Sedgwick, "A Report of the Biological Work of the Lawrence Experiment Station," in Experimental Investigations of the State Board of Health of Massachusetts upon the Purification of Sewage..., (Boston: 1890), 799-816.

15Ibid., 584-586.

16Ibid., 847-848.

17Ibid., 859-860.

18Ibid., 861.

resolve to convince the broader scientific community that the creation of such a hybrid, local specialty was both intellectually fruitful and politically sensible research policy.

Sedgwick had to move gradually towards making his intellectual horizons an institutional reality. At first, in 1889, the best he could do was to convince the Civil Engineering Department to offer a Sanitary Engineering option that reduced the course work in railroads, bridges, and mechanical engineering in favor of courses in chemistry and biology. But as his research at Lawrence and the incorporation of the results into his courses attracted growing numbers of students and earned him promotions within MIT, he was able to build from a staff of three, including himself, to eight, and in 1899, he received additional space for biology on MIT’s cramped campus.

In 1903, the year two more instructors were added to the staff, Sedgwick reassessed curricular priorities. He narrowed the scope of advanced teaching to promote greater sophistication in the courses relevant to public health:

Botany receives considerable attention in the second year, and in this year also is given a general elementary course in zoology, but no attempt is made to teach either advanced botany or higher zoology, the aim being rather to lay a solid foundation in the elements of these subjects and upon this to

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\(^{20}\)Annual Report of the President and Treasurer of the Massachusetts Institute of Technology, 11 December 1889, 44-46. Sedgwick also succeeded that year in convincing the Geology Department to take over the Natural History courses dealing with evolution. See the Annual Catalogue of the Massachusetts Institute of Technology, 1889, 38.

\(^{21}\)In 1889, enrollment was at a paltry all-time high of 22 students, 9 regular students working towards degrees and 13 special students seeking to enhance their professional standing or prepare themselves for graduate studies. Five years later, biology remained unpopular among regular students, but special students had swollen enrollments to 103 students, and Sedgwick was understandably complaining about his allotment of space. See the Annual Report, December 1889, and the Annual Report, December 1894. In 1891, Sedgwick was promoted to full professor.

\(^{22}\)For the acquisition of better quarters, see Jordan, et. al., Sedgwick, 40-43; evidence for the growth of the staff is drawn from the Annual Catalogues.
build such professional attainment in bacteriology, industrial biology, physiology, sanitary biology and public health subjects as the time allows.\textsuperscript{23}

With the staff better able to focus its pedagogic efforts, a string of textbooks soon emerged, including Sedgwick’s often-reprinted *Principles of Sanitary Science and the Public Health*.\textsuperscript{24} While MIT’s administration made no permanent provisions (of either space or money) for research, from 1903-1919 the annual contributions of an anonymous donor (Mrs. W. H. H. Hughes), enabled the Biology Department to maintain a Sanitary Research Laboratory and Sewage Experiment Station in conjunction with the city of Boston.\textsuperscript{25} In 1911 the obvious received formal recognition as the Department’s name was changed from the Department of Biology to the Department of Biology and Public Health.

Sedgwick’s institution-building culminated in an ad hoc union with Harvard Medical School. In 1911, Harvard’s President Lowell appointed a former Sedgwick student, George Whipple, Harvard’s first professor of sanitary engineering and directed Whipple to explore with Sedgwick and M. J. Rosenau, chairman of Harvard Medical School’s Department of Preventive Medicine, the possibility of a cooperative arrangement in public health. The three men worked out a School for Public Health Officers that accepted both medical doctors and students with two years advanced work in relevant fields of science or engineering to a largely elective curricula that came to cover preventive medicine, sanitary science, personal hygiene, laboratory biology, communicable diseases, sanitary and municipal engineering, demography,

\textsuperscript{23}Annual Catalogue, 1903, 217. See also Sedgwick, “The Claims of Sanitary Science to a Place in the Curriculum of Engineering Education,” *Proceedings of the Society for the Promotion of Engineering Education*, 6 (1898), 300-319.


\textsuperscript{25}Jordan, et. al., *Sedgwick*, 39-40; “Research Work, Department of Biology and Public Health,” enclosed with Sedgwick to Richard Maclaurin, 21 November 1911, MIT Archives, AC 13, Box 18, Folder 530.
and industrial hygiene.\textsuperscript{26} The three organizers served as the school’s administrative board. Sedgwick deemed the arrangement "advantageous to both sides, Harvard gaining the prestige of the previous work of the Institute, and the Institute gaining the advantage of connection with a Medical School," but he was perturbed at Harvard’s insistence on numerically dominating the board to administer the school.\textsuperscript{27} Nevertheless, the school, the first of its kind, functioned smoothly from 1913-1921 and proved successful at attracting and placing students intent on becoming municipal health officials.\textsuperscript{28}

Sedgwick’s newfound concern with bacteria, sanitary engineering, and the quality of Massachusetts’ drinking water did not fundamentally alter his understanding of biology as an experimental and analytic science; the second edition of his introductory textbook in biology (1895) simply added a section on unicellular organisms following his discussion of the earthworm and the fern. Put Sedgwick began to view the combination of disciplinarily distinct perspectives involved in tracing bacteria through natural and man-made environments as an intellectual field in its own right (though with its foundations in biology). He spoke of "sanitary science" and "public health science" as he would biology or physics:

I accept the term public health science without hesitation, for any division of human knowledge which has worked out its own laws ... and which has reached results enabling it to predict with accuracy ... is entitled to a place ... among the physical sciences.\textsuperscript{29}

In more national forums, Sedgwick pressed his claim that "public health science" was a viable division of intellectual life. He not only defended what he considered public health science’s rightful turf from excessive incursions from the

\textsuperscript{26}Jean Alonzo Curran, \textit{Founders of the Harvard School of Public Health}, (New York: Josiah Macy Foundation, 197\textit{c}.), 1-4 and 35-36.

\textsuperscript{27}Sedgwick to Maclaurin, 15 May 1913, MIT Archives, AC 13, Box 18, Folder 530.

\textsuperscript{28}Jordan, et. al., \textit{Sedgwick}, 75; Curran, \textit{Founders}, 17-19.

\textsuperscript{29}Sedgwick, "The Relation of Public Health Science to Other Sciences," \textit{Science}, 21 (16 June 1905), 905.
neighboring domains of medicine, whose proper sphere, Sedgwick argued, was personal hygiene,30 and of partisan politics, which he considered a threat to a professional civil service staffed by his former students.31 He also pleaded for the academic community to embrace "public health science" even though its development did not conform to the conventional image of a new specialty that extends the applicability of some theory or increases the power of some experimental technique.

What Sedgwick thought his experiences demonstrated was:

Like other applied sciences, this one [public health science] has not grown as a branch grows from a tree .... That common simile ... is a grotesque survival of a time when neither trees nor science were understood .... [Instead] inductive science is [like] a river, rising from various and often obscure streams, and finally pouring its substance into the mighty ocean of accumulated human knowledge.32

The lone problem with Sedgwick’s similes, from a historian’s perspective, is that he has left out human agency in general and his own role in particular. Inanimate forces bring streams together into rivers and rivers into the ocean, but what brought the previously independent traditions of vital statistics, civil engineering, bacteriology, and physiology together into a new rubric was Sedgwick’s skillful exploitation of his own education, Massachusetts’ concern with epidemics and its water supplies, and the range of perspectives at MIT.


31Sedgwick, "The Experimental Method in Sanitary Science and Sanitary Administration," Science, 23 (9 March 1906), 362-367 deems politically-appointed boards of health "a failure experimentally demonstrated. Sedgwick served at times as president of both the Boston and Massachusetts Civil Service Reform Associations; see Jordan, et. al., Sedgwick, 117 and 131-135.

32Sedgwick, "Relation of Public Health Science," 907 More recently, Spencer Weart has contrasted "the usual, vegetative model of specialization" in which new specialties split off from the old "as when ivy splits into branches as it creeps up a wall" with the emergence of solid state physics, "a grand rearrangement of an entire array of specialties, old and new, into a novel constellation." "The Solid Community." in Lilian Hoddeson, et. al., eds., Out of the Crystal Maze: Chapters from the History of Solid-State Physics, (New York: Oxford University Press, 1992), 617-669.
Sedgwick willingly drew a general political message from his particular success in fostering scientific innovation by pooling disciplinary approaches in response to the local needs of Massachusetts and the available talent at MIT. He believed he understood how science and engineering could together flourish under governmental patronage to yield both better public policies and more harmonious relations in the academic community. In a panel discussion among naturalists on "The Attitude of the State towards Scientific Investigation," Sedgwick offered a striking resolution to an implicit conflict.

Henry Osborne, professor of zoology at Columbia and curator of vertebrate paleontology at the American Museum of Natural History, opened the discussion with a paper arguing that the United States government should fund scientific research as an investment which any enlightened state would make. However, according to Osborne, a student of biological evolution, dividends would not be reaped from that investment by incorporating economic or technological criteria into the organization of scientists and their choice of projects. He quoted at length the German physicist Herman Helmholtz on the fruitlessness of seeking immediate practical utility in scientific advances and the necessity of presently cherishing any new contribution to an intellectual structure while maintaining faith that it may, "in its own due time, bear practical fruit." The taxpayers' dividends would consist of the availability of such disciplined, enlightened thinkers to address issues of public policy. He called upon scientists to treat cooperation with the government in investigations of governmental matters as a duty, and he stressed the importance of incorporating science into a curriculum to train students for government service.

33 Annual Discussion before the American Society of Naturalists, "The Attitude of the State Towards Scientific Investigation," Science, 13 (18 January 1901), 81-82. Helmholtz's statement was part of an argument justifying the continued life of the traditional university, with its four faculties of liberal arts, theology, law, and medicine, on the grounds of intellectual commonalities between the natural and moral sciences. See Hermann Helmholtz, "On the Relation of Natural Science to General Science," Popular Lectures on Scientific Subjects (New York: D. Appleton, 1895), 1-32.

34 Ibid., 84.
science partnership as an exchange relationship in which scientists received public money for their own research in return for infusing the government with the kind of thinking and thinkers their research helped to produce.

The Department of Agriculture entomologist L. O. Howard accepted Osborne's notion that scientists and their government should maintain an exchange relationship. But in Howard's opinion, the relationship envisioned by Osborne gave the government the short end of the stick because it did not direct scientists towards the direct pursuit of economically and technologically pressing problems:

I have a strong conviction that humanity gains far more from scientific work undertaken with an economic aim than from the labors of the other class of scientific men, and I believe it to be a most unfortunate condition of affairs that hundreds of the men best fitted ... to attack the many economic problems crying for solution are delving away in their search for truths and principles which when found have only a remote bearing, if any at all, upon the sum total of human happiness.\(^{36}\)

Howard conceded that pure scientific work had ultimate practical importance, but he offered no examples of how USDA practices used scientific results. He would certainly have acknowledged the importance of sound, sophisticated training to the ability of scientists to succeed at work with an economic aim, but such training required that universities and their faculty members internalize pedagogic versions of agricultural practices.

Rather than choose sides in the dispute built into an exchange model of science-government relations, Sedgwick tried to combine Howard's utilitarianism and Osborne's notion of pure science as an investment, calling the pursuit of pure science "the highest and truest utilitarianism."\(^{37}\) Following Howard, Sedgwick argued for justifying public expenditures for research on the grounds of technological utility; he reported that the Massachusetts legislature had been forthcoming with funds when

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\(^{36}\)Annual Discussion, "Attitude of the State," 89.

\(^{37}\)Ibid., 93.
informed research was necessary to find ways to protect the state’s inland water resources. But in his description of doing the research, Sedgwick echoed Osborne’s ideals: "the most thoroughgoing utilitarianism has proved to be scientific investigation pushed to its utmost limits" — that is, investigations of how bacteria live and die in laboratory, natural, social, and sewage-treatment environments. Any dispute over the merits to the government of pure and applied science seemed like nonsense to Sedgwick for "the barrier between them is fading away because they are constantly drawing nearer together and overgrowing one another." Again Sedgwick has left out himself and human agency generally. Pure and applied science were overgrowing one another when a man like Sedgwick had the inclination and opportunity to weld parts of several disciplines into a new grouping whose program in research and teaching satisfied local needs for knowledge and personnel.

While Sedgwick’s career began with a distaste for vocational training and a passion for reorienting biological thought through laboratory studies of individual organs, he learned, during his twenty years at MIT, to pursue the latter as a junior partner to the reform and invigoration of the former. Public health science directly promoted not "general biology" and a new way of thinking about life but the part of general biology relevant to tracing the movement of disease-causing bacteria and a new form of professional specialization for civil servants. Public health science’s development rested on two institutional foundations: the Massachusetts Board of Health, which enlisted university professors to help it develop and set standards for the technology it would manage; and MIT, which housed a diverse group of experts and permitted Sedgwick to fashion a new field out of the interactions of the researchers helping the Board of Health. Sedgwick’s arguments to secure public health science’s acceptance and to generalize on his experiences were pleas to university scientists not to let their ambitions to influence scientific thought dominate

\(^{38}\text{Ibid.}, 94.\)

\(^{39}\text{Ibid.}, 93.\)
their organization and to universities' patrons and administrators to encourage local experiments in the reorganization of knowledge.

Between the lack of support for building physiology as an independent discipline to feed students into medical school and the successful generation of research projects by collaborating with chemists and engineers in studying technologies of concern to the Massachusetts Board of Health, Sedgwick came to sense that he would best build his reputation at MIT not by studying biological processes common to many organisms (even though biologists nationally and internationally might applaud such work) but by studying organisms, such as disease-causing bacteria, as they moved through natural and man-made environments. Such a research strategy led Sedgwick to form lateral ties with faculty in those departments on whose work bacteria would impinge and to pledge professional fidelity to a hybrid specialty with local institutional supports. That strategy also made him dependent on the cooperation of those over whom he had no direct administrative leverage, especially in the case of his arrangements with Harvard for the joint School of Public Health. Though not apparent to him as he successfully explored the dimensions of "public health science," Sedgwick was making himself institutionally vulnerable and cultivating the need for an administrative champion who would maintain the alliances he formed. He was opening a role for the MIT president as an activist broker who reassessed departmental boundaries and mobilized support for faculty whose intellectual ambitions depended on novel institutional arrangements.

Arthur Noyes and the Expansion of Theoretical Chemistry

While Sedgwick's perspective on the formation of research specialties and harmonizing the interests of pure and applied science might seem to constitute a general virtue for MIT's presidents to build on, MIT's Chemistry Department was the scene of precisely the type of tension inherent in Osborne's and Howard's views on science and the state. The protagonists of the struggle in MIT's Chemistry Department were Arthur Noyes and William Walker. Rather than seeking a president who would broker multi-departmental arrangements, both Noyes and Walker wanted a
president who would set departmental boundaries within which faculty could autonomously operate. They turned out to be hopelessly at odds, however, over the ends that intra-departmental faculty autonomy within the Chemistry Department should serve.

Arthur Noyes' career at MIT is testimony to the ambitions that an immersion in conceptually novel science can inspire. To an even greater degree than Sedgwick's, Noyes' graduate education placed him in the midst of theoretical ferment, but unlike Sedgwick, Noyes did not seek to advance his academic standing by seeking and exploiting points in common between his education and the concerns of other departments. Instead Noyes expected MIT to reward him for making MIT the major American center for the exploration and dissemination of a new way of thinking about chemical reactions. The impulse to theorize, the urge to compete in the solution of theoretically defined problems and the production of theoretically relevant data, and the availability of philanthropic support for cultural pursuits were the forces Noyes mobilized to build a research tradition at MIT.

Noyes obtained his B.S. and M.S. from MIT in 1886 and 1887 respectively. In the summer of 1888, he went to Germany, fortuitously ending up in Leipzig where he abandoned his initial plan to study organic chemistry because of the influence of Wilhelm Ostwald.40 In the 1880s, Ostwald, Svante Arrhenius, and J.H. van't Hoff had collectively realized that ionic theory and the ideal gas law could be combined to account for the behavior of at least some substances in some solutions.41 With


thermodynamics, ionic theory, and Daltonian proportions potentially forming the
conceptual foundation for an expansive science of chemical kinetics and equilibria in
solution. Leipzig, in 1887, called Ostwald to its chair of physical chemistry, the only
such chair in Germany. He also founded the Zeitschrift für Physikalische Chemie as
an outlet for publishing and debating Leipzig’s output.\textsuperscript{42}

Young Noyes no doubt sensed the intellectual vistas Ostwald was opening:
The rapidity with which van’t Hoff, Arrhenius, and Ostwald erected a
theoretical structure covering much of the data of physical chemistry could
not help but create a sense of excitement and momentum. At the same time,
the structure in many places was still skeletal, and this preserved a sense
that opportunities for future work were still available.\textsuperscript{43}

Ostwald’s predilection to express grand hopes that physical chemistry would provide a
new and superior foundation for chemical education could well have attracted the
student seeking academic credentials. Claiming that manufacturers had found
chemists trained strictly in organic chemistry unfit to cope with general problems,
Ostwald boasted:

Among the many proposals which have been made to escape ... the pressing
danger of chemical one-sidedness, none appears to me more suitable than
the encouragement of ... general and physical chemistry. It deals with
questions which lie at the base of organic and inorganic, of pure and applied
chemistry; it forms a foundation for all real chemical education, and must be
regarded as lying at the root of all chemical teaching.\textsuperscript{44}

Noyes quickly came to understand research as probing for areas that taxed the
applicability of chemical theory and finding the conceptual and empirical techniques
that would bring such areas within the reach of theory. His first published paper in

\textsuperscript{42}Hiebert, "Ostwald," 456.

\textsuperscript{43}Servos, Physical Chemistry, 60. Dolby, "Case of Physical Chemistry," stresses that there was no
quantitative increase in the physical chemistry literature stemming from these theoretical breakthroughs.
That fact is not necessarily inconsistent with Servos’ assertion of the enthusiasm generated by the new
theories. The important point here is that physical chemistry exercised an enduring hold on Noyes
once he joined Ostwald’s circle; a spurt in the physical chemistry literature would be expected only if
thermodynamics’ advocates could attract and handle large numbers of students or promptly convince
their peers to reeducate themselves in the new chemistry.

\textsuperscript{44}Wilhelm Ostwald, Conversations on Chemistry, Part I, General Chemistry (New York: John
Wiley and Sons, 1905) iv.
physical chemistry was a "purely theoretical" contribution that proposed mathematical means to treat solutions of salts, strong acids, and strong bases whose behavior, experiments had shown, deviated from the gas laws.\textsuperscript{45} For his doctoral dissertation, Noyes performed the necessary experiments to produce a systematic comparison of solubility values as determined by experiment and as calculated from data on a solute's dissociativity.\textsuperscript{46} This line of research enmeshed Noyes in an enduring problem for physical chemistry. For strong electrolytes, such as those whose behavior had inspired his first paper, the "constants" that were supposed to mediate the relationships among chemical and physical properties varied widely with the concentrations of the electrolytes in solution.\textsuperscript{47}

On returning to MIT with his Ph.D. in 1890, Noyes, unlike Sedgwick, did not seek a creative adaptation to the local circumstances and collegial opportunities at MIT; rather he sought to create his own version of Leipzig at MIT. Where Sedgwick flexibly let slide his doctoral research in mammalian physiology and generated biological research opportunities through his involvement Massachusetts Board of Health, Noyes continued to delve into the anomaly of the strong electrolytes, pressing a claim that unreliability in determining dissociativity by conductivity was behind the apparent anomaly.\textsuperscript{48} Where Sedgwick collaborated with colleagues in other disciplines, and sought to spin a new field out of the collaboration, Noyes took over the established course in "Theoretical Chemistry," turned it into a course largely on physical chemistry including experimental techniques in physical chemistry, and


\textsuperscript{46}Idem., "Über die gegenseitige Beeinflussung der Löschlichkeit von dissocierten Körpern," ibid., 6 (1890), 241-267; Walter Nernst, Ostwald's assistant, had demonstrated how the two properties were theoretically linked.

\textsuperscript{47}Further discussions on the problem of strong electrolytes and Noyes' work, see John H. Wolfe, "The Anomaly of Strong Electrolytes," Ambix 69 (1972) 175-196; and Dolby, "Debates over the Theory of Solution," 300-509.

\textsuperscript{48}Servos, Physical Chemistry, 124-125.
sought collaborators among the course's top students. Finally, where Sedgwick published much of his research in the annual reports of the Board of Health to an audience of scientists, local policy-makers, and public interest groups concerned with public health, Noyes generally wrote his papers in German and published them in Ostwald's *Zeitschrift für Physikalische Chemie* to an audience of the internationally scattered band of chemists versed in thermodynamic concepts and ionic theory.  

Noyes soon craved better working conditions to perform more sophisticated research, but unlike Sedgwick, Noyes looked neither laterally to other departments within MIT nor to local agencies with interests in recent chemistry. Instead, he petitioned MIT's president and executive committee to establish a Department of Chemical Research at MIT. He offered to put up $5,000 per year for five years of his own money for the Department on the condition that MIT matched his contribution and provided adequate space and the permanent equipment. The purpose of the Department would be to conduct "research in pure science, not advanced instruction," and its staff members "should ... devote themselves almost exclusively to scientific investigation." However, the researchers would give one advanced course in the Chemistry Department throughout the year in the interest of finding talented students to recruit for doctoral work. "Thus, in my own case, I should hope to be able to continue ... the lectures in theoretical chemistry and the charge of

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49 Arthur A. Noyes, "Instruction in Theoretical Chemistry," *Technology Quarterly* 9 (1896) 323-325. In Leipzig, the terms "theoretische Chemie" and "allgemeine Chemie" were used interchangeably with "physikalische Chemie"—a terminology which bespoke Ostwald's ambitions for physical chemistry's significance; see Servos, *Physical Chemistry,* 4-5.


51 Noyes to (William?) Pritchett, 25 November 1901; copy attached to Noyes to George Ellery Hale, 19 December 1901, Hale Papers, Series 1, Box 27, microfilm edition at American Institute of Physics.

undergraduate thesis work in that subject and yet be able to give as much time as was necessary to the Research Department." 

Noyes insisted that the Department would have to be self-governing to be effective. While formal appointments to positions would come from the MIT corporation, they would be made "upon informal recommendation by the Department." As for daily administration, "The Department should be administered by a Department Council consisting of the professors of the Department." The Department would have an internal hierarchy and salary scale so that a young researcher who impressed his seniors could aspire to a career in research rather than a sojourn in research prior to taking a job with advancement possibilities. 

What Noyes wanted was for MIT to grant him and his associates a fiefdom which would be independent of corporate and presidential control, except for the macro-budgetary concerns that could affect MIT’s contribution to this fiefdom. Such a department would not prove its worth by teaching elementary chemistry to large numbers of engineering students, stimulating research opportunities for members of other departments, or providing data necessary to improve the technological sophistication of Massachusetts’ government and industry but rather by becoming a leader in developing thermodynamic interpretations (and interpreters) of chemical change. Noyes wanted MIT to see itself as part of an international community of research institutions dedicated to helping its staff compete with like-minded researchers at other institutions in the exploration of conceptually novel territory.

MIT did not accept Noyes’ proposal, but two years later, in 1903, when Noyes had in hand a $2,000 grant for research from the Carnegie Institution of Washington, MIT accepted a similarly argued proposal for a research laboratory rather than a full-scale department. Despite the need for external recognition to induce MIT’s administration to grant him his wishes, Noyes celebrated the founding of the Research Laboratory in Physical Chemistry as MIT’s acknowledgement of a

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53Ibid., 6.

54Ibid., 3.
greater than local interest in physical chemistry and MIT's commitment to compete
with other universities which encouraged such endeavors:

It is extremely gratifying to feel that ... its [MIT's] government has taken a
determined stand in support of the principle that the needs of the
undergraduate instruction must not be allowed to check the development of
advanced study and research, by which alone the Institute can maintain its
prestige among similar institutions.55

While insisting that his new laboratory be oriented to his reading of the state
of ionic theory,56 Noyes was not prudish about industrial service. The money he
offered to support the RLPC apparently came from an agreement stemming from his
design of a process for the recovery of alcohol and ether vapors for the American
Aristotype Company.57 But consulting and invention were not, for Noyes, the
principal manner by which an educational institution could support industry. Noyes
offered two arguments for why investigating the properties of extremely dilute
electrolytic solutions, though unrelated to any apparent technological process, was his
best contribution to engineering and technology. First, Ostwald's disciples in the
United States generally assumed that physical chemistry would be a "donor
specialty"—that once well worked out, it would donate a host of insights to many
fields. Because many reactions, both natural and human-induced, take place in
solution, a sound theory of solutions, they reasoned, would be applicable in many

55Arthur A. Noyes. "The New Research Laboratories at the Institute: The Research Laboratory in
Physical Chemistry," Technology Review 5 (1903), 305, my emphasis.

56With mechanically gifted students, especially William Coolidge, Noyes built apparatus to
investigate the conductivity of electrolytes at varying concentrations and untried extremes of
temperature as both a new approach to the anomaly of strong electrolytes and a source of fresh data in
which to search for regularities and relationships between chemical and physical factors. See Servos,
Physical Chemistry, 125-130, and Arthur A. Noyes, The Electrical Conductivity of Aqueous Solutions,

57Noyes did not patent the process but kept it secret; the effectiveness of this business strategy may
explain why he did not want industrial support for an educational research laboratory. Informed
hearsay set Noyes' income from the operation of this process at $1,000 per month for several years.
See Servos, Physical Chemistry, 109 and George Wise, Willis R. Whitney, General Electric, and the
contexts. Second, and more importantly, Noyes thought physical chemistry possessed superior pedagogical virtues for the training of engineers. He argued that the point of engineering education was to produce powerful, not knowledgeable men: "It is power that counts, and not knowledge. The ultimate test is what a man can do, not what he knows." To guard against the temptation "to ply the student with more than he can possibly assimilate," Noyes urged faculty to organize their courses around problems that forced students to think their way back to basic principles and reason forward to the problems' solutions. The work and thought habits acquired in solving problems constituted, for Noyes, the scientific spirit and the true object of technical education:

The teacher of any science who says it is not his business to attend to these things [problem solving and good work habits] does not, in my opinion, understand his business, which is not so much to teach the subject-matter of the science as it is to teach scientific method and to cultivate the scientific spirit.

Physical chemistry, for Noyes, was a large part of the solution to engineering education's vices. He took seriously Ostwald's ambitions for a physical chemistry that would reform and reformulate chemistry in general. In 1902 Noyes published a prolegomena to his ideal chemistry text: The General Principles of Physical Science: An Introduction to the Study of the General Principles of Chemistry. Noyes openly embraced Ostwald's and Mach's philosophy that the object of science is to achieve economy of thought, and in 160 pages he presented 18 general principles

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58See Servos, ibid., 66-69 for a discussion of physical chemists' arguments for the utility of their specialty.


60Ibid., 663, Noyes' emphasis.


62Ibid., 3-9. Noyes' references for his views on the aim and method of science were works of Ernst Mach, Nernst, and Ostwald; see 164.
relating to matter and 16 relating to energy to form the foundation for an expanded work discussing chemical change and equilibrium, the relationship of physical properties to chemical composition, and energy changes attending chemical reactions. The expanded text came out in 1914: The General Principles of Chemistry. As he explained on presenting a copy to his friend George Ellery Hale:

The subject is presented largely through problems. These form the main purpose of the book instead of being merely incidental. ... I wish someone would write a textbook of physics on the same basis. So few of our textbooks are really written as courses of instruction, this object being often defeated by the feeling that many things must be included for purposes of reference, or to protect the author against the charge that he has omitted something important.

This book became known in physical chemistry circles as the "textless textbook." Its deliberately spare offerings represented Noyes' hope for an MIT that would support conceptually oriented research as the generator of pedagogy that would produce "powerful" men trained to find the general principles that underlie problematic phenomena.

For Noyes, graduate education at Leipzig under Ostwald was a model he hoped to recreate at MIT. With the leverage provided by his extra-academic income and by the willingness of philanthropists to contribute to his internationally recognized efforts to elaborate chemical theory, he won from MIT a research laboratory that he dedicated to examining the explanatory power of thermodynamic concepts. (By contrast, Sedgwick linked his research ambitions to the Massachusetts Board of Health's need for biological assessment of the efficacy of sewage treatments). In his teaching and textbook writing, Noyes adopted Ostwald's positivist philosophy and sought to pare down the empirical material students learned in order to stress the power of using chemistry's "general principles." (By contrast, Sedgwick stimulated his colleagues to produce a string of topical texts that reflected the professional

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43Ibid., v.

44Noyes to Hale, 3 October 1914, Hale Papers, Roll 27.

45Pauling, "Noyes," 324.
concerns of health officers.) Whereas Sedgwick's ultimate objectives became to create a new, hybrid discipline, to secure acceptance for the hybrid discipline within the university, and to produce a cadre of health officers better able to identify the sources of and design solutions for a community's health problems, Noyes' ultimate objectives became to create an expanding intellectual structure out of thermodynamic concepts and experimental ingenuity, to promote the new specialty as a strategic "donor" to a student's mastery of any part of chemistry, and to produce a cadre of chemists who could find new domains in which thermodynamic concepts would prove intellectually powerful. He did not need an administrative champion to help hold the loyalties of those over whom he had no authority but a president who would grant him a jurisdiction and maintain its boundaries.

**William Walker and the Taxonomy of Industrial Practice**

Noyes' views on the relationship of research, teaching, and utility were a more sophisticated version of Osborne's. Where Osborne raised the idea of scientific research being an investment without specifying how scientific knowledge would pay dividends, Noyes pushed physical chemistry as a donor specialty to a number of fields. Where Osborne viewed the training of professionals as the duty autonomous researchers assumed for research support, Noyes produced arguments for why the students of such researchers would become the most effective professionals. Nevertheless, Noyes' conception was still open to a more sophisticated version of the criticism that Howard leveled at Osborne. A donor specialty could be accused of absorbing more in personnel than it donated in useful ideas. A course of study which instilled an appreciation only for economy of thought and problem-solving power could be accused of blinding students to the challenges and benefits of understanding industrial practices. William Walker developed an alternative to Noyes' vision of chemistry around these concerns.

Walker's ambitions at MIT were driven by the economic power he was certain could flow from close relations between universities and industrial firms. Like Noyes and Sedgwick, Walker held a doctorate from a research-oriented university. But
unlike Noyes, Walker did not promote himself by contributing to a new chemical theory but by seeking intellectual order in industrial practice; and unlike Sedgwick, Walker did not seek multi-departmental cooperation with a local agency but departmental affiliation with a nationally representative selection of firms from an industrial sector. Walker wanted MIT to become a national clearinghouse for the industrial research problems of chemical firms and the national standard for the education of chemical engineers. He sought to establish research traditions by categorizing industrial practices and matching them with scientific skills.

Walker graduated from Pennsylvania State University in 1890. Like Noyes, Walker went to Germany for graduate study, but Walker took his Ph.D. in Göttingen under the organic chemist Otto Wallach in 1892. Since the 1870s, academic organic chemists generally in Germany, and at Göttingen in particular, had succeeded at synthesizing compounds of interest to dye manufacturers, and become the object of competition among industrialists seeking relations that would bolster their firm’s patent position and provide a ready source of talent for their laboratories.66 From this environment where industrial respect for academics and academic interest in industrial relevance prevailed, Walker returned to Pennsylvania State as an instructor in chemistry, and in 1894 moved to MIT.

Chemical engineering existed only on paper when Walker joined MIT’s Chemistry Department.67 That situation did not disturb him, for German universities and technical institutes happily maintained separate chemistry and mechanical engineering departments, and German corporations successfully used committees of chemists and mechanical engineers to work out the problems of managing chemical

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reactions on an industrial scale. But in 1900, Walker left MIT to form a consulting firm with Arthur D. Little and learned that American manufacturers did not mirror the German in their use of chemists and mechanical engineers. Although chemical manufacturing was growing lustily, research chemists in industry were scarce and not well understood by their corporate superiors; most "chemists" in industry were little-respected and poorly paid performers of routine analyses; and mechanical engineers were claiming the right to design and manage chemical plants by virtue of their expertise in the intricacies of heavy machinery.

Five years of consulting convinced Walker that he should reform relations between American universities and chemical industries in order to put further growth on the sustainable foundation of progress in chemical research. He was contemptuous of "men today who pass under the name of chemists who are little more than testing machines." He predicted the downfall of the manufacturer who hires such a "so-called chemist ... and expects this chemist to improve his process and keep his business in the skirmish line of the industrial battle." And he called on technical schools which offered specialized curricula for a plethora of individual manufacturers

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66Contributing to growth were a mass demand for some inorganic substances, an abundance of raw materials, and falling production costs that followed from the introduction of improved machinery built from mass-produced steel and from the availability of hydroelectric power. See Martha Moore Trescott, The Rise of the American Electrochemicals Industry, 1880-1910 (Westport, Ct.: Greenwood Press, 1981), 115-129.


to recognize that their policies were "wrong in principle and disappointing in practice."\textsuperscript{73}

Germany had gained international leadership in chemical production, in Walker's view, because "most of her [Germany's] problems in technical chemistry are first ... studied in accordance with recognized methods of modern research by men fully trained in pure science."\textsuperscript{74} However, Walker was not so sanguine about American chemical firms constructing research laboratories, hiring Ph.D.s, and designing factories on the basis of laboratory results. Instead, he sought academic reforms that would orient chemistry departments to producing the chemical engineer, "a thoroughly trained chemist ... [who] has a sound knowledge of those related mechanical and electrical subjects which are necessary in order that his efforts may be industrially successful."\textsuperscript{75}

Chemical engineers were producible, Walker believed, if American technical universities would balance and harmonize the love of knowledge and methodological rigor common to German universities with the ambitions and ultimate destinations of their students:

In America, ... most men enter our technical schools with the intention of fitting themselves as rapidly as possible for some useful calling in life .... The presence of too much of this spirit is to be regretted; but it is a power to be turned to service, not to be opposed.\textsuperscript{76}

In 1905, Walker returned to MIT as a full professor of chemical engineering and set to work at creating research programs that gave credence to his belief that the unity of chemical training could be retained while introducing the breadth of factors relevant to industrial operations. A noteworthy early research success of Walker's seemed almost calculated to embarrass Noyes by showing how a professor broadly familiar

\textsuperscript{73}William Walker, "What Constitutes a Chemical Engineer," \textit{The Chemical Engineer}, 2 (1905), 3.

\textsuperscript{74}Walker, "Technical Chemistry," 27.

\textsuperscript{75}Walker, "Chemical Engineer," 2.

\textsuperscript{76}Walker, "Technical Problems," 62.
with industrial problems could find more significance in physical chemistry than a physical chemist.

Around 1900, Willis Whitney, who earned his Ph.D. from Noyes and had stayed on as instructor in chemistry at MIT, took up the question of why iron corrodes after helping a Boston hospital rid itself of rusty radiator pipes. Whitney realized that ionic theory implied that pure water alone should be sufficient to dissolve pure iron.77 His lone paper on this topic reported experiments demonstrating the dissolution of iron ions into pure water and argued that carbonic acid, whose elimination from radiator pipes coincided with the end of the rusting, merely accelerated corrosion in a closed heating system.78 There he let the matter stand, even though his claims were challenged and even though corrosion was of technological interest in more contexts than heating systems.

After returning to MIT, Walker began to compare the resistances of iron and steel to corrosion as a matter of widespread technological significance. As a first step, he repeated Whitney’s experiments in a more refined fashion that helped to identify experimental and interpretive flaws in Whitney’s critics. But then Walker pressed on to consider other factors which should, according to ionic and electrochemical theory, affect the rate at which corrosion proceeds.79 Walker’s additional work added little to ionic theory’s explanatory power, but by giving

77For more than 50 years, corrosion had been suspected of being an electrochemical process, but debate existed over the necessity and roles of oxygen, carbonic acid, and iron impurities in the process; prior to the work of Arrhenius, nobody suspected that water dissociated sufficiently to be an electrolyte and that a single piece of iron could serve as both anode and cathode in an electrochemical reaction when some regions of the iron became coated with hydrogen ions. See George Wise, Whitney, 54-58.

78Willis Whitney, "The Corrosion of Iron," Journal of the American Chemical Society, 25 (1903), 394-406. Because of several chemical and physical factors, carbonic acid, Whitney reasoned, was recycled through heating systems so that small amounts in the system would continuously accelerate corrosion. A student of Arrhenius had published similar results before Whitney, but Whitney’s paper introduced American chemists to the ionic theory of corrosion.

79William Walker, Anna Cederholm, and Leavitt N. Bent, "The Corrosion of Iron and Steel," Journal of the American Chemical Society, 29 (1907), 1251-1264, quote from 1251. Walker investigated the process by which oxygen liberated the hydrogen ions which coated a region of iron so that more iron could dissolve and the affect which differences in electrical potential at different spots on the surface of iron was associated with rusting.
systematic attention to the conditions under which corrosion occurred, Walker found the intellectual means to suggest a host of ways to prevent corrosion in many settings in which iron and steel were used. His devotion to publicizing the ionic theory of corrosion and its applications won him the American Chemical Society's Nichols gold medal in 1908; in the medal's five-year history, this was the first time it had been awarded for work in physical chemistry.

As noteworthy as Walker's research and writing were to his peers, they were nevertheless ad hoc from an institutional standpoint. Walker's entrepreneurship and familiarity with American industry, not any organized means of disseminating industrial problems, led him to the settings in which corrosion was a problem and the firms that might appreciate his guidance in finding a technically and economically viable means of halting or limiting corrosion. Walker's ability to attract students with conventional chemical educations, not a curriculum that drew students to subjects of importance to many industrial settings, earned him research assistants. In 1908, Walker set forth his solution to the first problem and started towards his solution of the second when, with Little's support, he organized the Research Laboratory of Applied Chemistry.

Little and Walker envisioned the RLAC as a national laboratory that would prod America's chemical manufacturers to compete in research with Germany's. Their proposal encouraged chemical manufacturers and trade associations to bring their problems to the MIT laboratory where advanced students, under faculty supervision, would conduct research. The sponsoring firm would pay for the students' time, exotic equipment, and overhead; for an additional payment, which the

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80The effects of paints and varnishes on corrosion in metallic cans, the integrity of iron reinforcement in concrete buildings, the effect of stress on the ease with which iron corrodes, and the effects of manufacturing techniques for annealing other metals to iron on the potential for future corrosion were all among areas Walker found illuminated by the electrolytic theory of corrosion. See his articles in Electrochemical and Metallurgical Industry, 5 (1907), 270-272; Journal of Industrial and Engineering Chemistry, 1 (1909), 754-758; Journal of the Iron and Steel Institute, 79 (1909), 69-80.

81For the Nichols medal, see Proceedings of the American Chemical Society for the Year 1908, 62-63.
laboratory would use to fund self-generated research, the firm could keep the
sponsored research results confidential. A Research Laboratory of Applied Chemistry
would thus prove the value of research to businesses without research facilities, serve
as a national clearinghouse for problems in applied chemistry, and set a standard and
model for other universities to emulate.\textsuperscript{82}

Except for the substitution of "Applied" for "Physical," the name of Walker's
laboratory was identical to Noyes', and these semantics indicate deeper similarities.
Walker was as internationalist and competitive as Noyes; Germany's accomplishments
were Walker's standard of excellence, and he hoped to thrust MIT and American
manufacturers into competition with German universities and manufacturers for the
discovery of industrially useful chemicals and chemical processes. Walker's
institutional tactics were similar to Noyes'. Like the RLPC, the RLAC functioned as
a semi-autonomous division of the Chemistry Department.\textsuperscript{83} Thus rather than
involve an exchange among researchers from several fields, as did Sedgwick's
laboratory, the Research Laboratory of Applied Chemistry, like the RLPC, would
guide advanced students in chemistry into an intra-disciplinary research specialty.

Where the contrast between Walker's and Noyes' titles for their laboratories
proved significant were in their relations to patrons and their scope of coverage.
Where Noyes' "physical" laboratory attracted philanthropic support based on his
ongoing ability to publish research in international journals, Walker's "applied"
laboratory would attract industrialists based on his ongoing ability to find university-
based skills of relevance to their problems. Where Noyes and his students specialized
in studies of ionic dissociations in solution because of the challenge this area posed to
the elucidation of chemical theory, Walker's laboratory would potentially be as broad


\textsuperscript{83} Servos, \textit{ibid.}, 536. The similarity between the two laboratories perhaps reflects the fact that both Noyes and Walker were products of German Ph.D. programs.
as the industrial conditions under which chemical reactions helped or hindered economic objectives (though practically bound by the pressure to fragment when demand for its services grew too large for one university to handle.)

A different set of similarities and contrasts characterize the relationship between Walker’s RLAC and Sedgwick’s Sanitary Research Laboratory and Sewage Experiment Station. Both men justified university research on the utilitarian grounds, and both men were comfortable with a direct relationship between their laboratories and the expected users of the knowledge generated in the laboratory. But their intellectual ranges and political horizons were constructed differently. The RLAC sought patrons nationally and was to serve as an example of what all Chemistry Departments could do; in effect, the laboratory was less MIT’s than the nation’s, and Little anticipated and looked forward to the day when businessmen, appreciative of the laboratory’s contributions, would insist on its division into specialized laboratories conveniently situated to specific clienteles.44 By contrast Sedgwick focused on the biology of bacteria in all environments, natural and technological, in order to satisfy the needs of the Massachusetts Board of Health. Insofar as research into other biological objects promised utilitarian benefits, other universities could organize laboratories and fashion links between the laboratory and the users of the new knowledge; insofar as other localities had public health problems similar to Massachusetts’, they could hire Sedgwick’s students to run their health departments. But Sedgwick would never have proposed that his laboratory fragment into specialized parts spread all over the country or that biologists everywhere concentrate on bacteriology and build working relations with chemists, sanitary engineers, statistician, and municipal officials.

Had Walker’s reform interest been limited to the laboratory (and had his personality differences with Noyes not added fuel to their substantive

44Ibid., 536-7; Little, "A Laboratory for Public Service," 16-24.
disagreements\textsuperscript{55}, perhaps each would have accepted the other as a valuable complement to his MIT-based interests.\textsuperscript{66} But Walker, like Noyes, wanted his laboratory to feed back into curricular reform, where they proved hopelessly at odds. For Walker, the economy of thought was a virtue but not the one true goal of a scientific curriculum. He would not shun the diversity of industrially relevant chemical reactions and of the uses of industrial equipment by letting chemical theory define the boundaries of the curriculum and the value of experiments. Rather, academic chemists should make industrial chemistry intellectually manageable by devising classification schemes for industrial practices. Universities’ resources, he argued,

should be applied to the exhaustive study of a few processes … considered as types, rather than be devoted to a large number of scattered experiments, simply because the reaction by which each is accomplished be different. If the reaction alone is to be studied, it can be better done in the laboratory of general organic or inorganic chemistry. On the other hand, the details of factory practice may best be learned in the factory itself.\textsuperscript{57}

Walker posed himself a taxonomical problem—to figure out a way to divide industrial practices into "types" that could be studied in a university. The RLAC, by attracting

\textsuperscript{55}In addition to their substantive differences, Noyes and Walker had personality clashes as well, according to MIT lore. See H. C. Weber, "The Improbable Achievement: Chemical Engineering at MIT," in History of Chemical Engineering, Furrer ed., 79-80.

\textsuperscript{66}At the national level, Noyes had urged the American Chemical Society to form specialized subdivisions, and in June 1908, Walker and Little both contributed their talents to the formation of a Division of Industrial Chemists and Chemical Engineers that published the Journal of Industrial and Engineering Chemistry. Walker and Little were simultaneously also helping to form the American Institute of Chemical Engineers. But they avoided conflict with the ACS by adopting a restrictive membership policy, by concentrating on setting educational criteria for chemical engineers, and by publishing nothing beyond its own Transactions. See Charles A. Browne and Mary E. Weeks, A History of the American Chemical Society (Washington: American Chemical Society, 1952), 68-85; Herman Skolnik and Kenneth M. Reese, eds., A Century of Chemistry: The Role of Chemists and the American Chemical Society (Washington: American Chemical Society, 1976), 12-15; Terry S. Reynolds, 75 Years of Progress: A History of the American Institute of Chemical Engineers (New York: American Institute of Chemical Engineers, 1983), 5-17.

\textsuperscript{57}William H. Walker, "A Laboratory Course in Industrial Chemistry," Technology Review 6 (1904) 165, my emphasis.
many and varied industrial problems, would provide its directors with ample opportunities to try out classification schemes.

In 1915, Walker and Little moved to specify the types of processes around which chemical engineering curricula should be constructed. In the report of the Chemistry Department's Visiting Committee, Little coined the term "unit operations" for the conditions that engineers could adjust to bring about a desired reaction.

Any chemical process ... may be resolved into a coordinate series of what may be termed "Unit Operations," as pulverizing, drying, roasting, crystallizing, filtering, evaporating, electrolyzing, and so on. The number of these basic unit operations is not large .... The complexity of chemical engineering results from the variety of conditions as to temperature, pressure, etc. under which the unit operations must be carried out in different processes, and from the limitations as to materials of construction and design of apparatus imposed by the physical and chemical character of the reacting substances.88

Within a year, Walker was publicizing a MIT School of Chemical Engineering Practice, which he had constructed around the concept of unit operations, to grant a combined bachelors and masters degree in chemical engineering. Students would start in the usual curriculum, but the second half of the fourth year and the succeeding summer would be spent in six-week stints at each of five manufacturing sites, selected to illustrate the range of unit operations in industrial practice. The fifth year would be spent back at MIT pursuing elective courses and a research project.89 Nor were Walker and Little content with defining educational criteria for chemical engineering at MIT. Through the selective accreditation policies of the American Institute of Chemical Engineers, they sought to make their educational policy the standard for the nation.90

89 Ibid., 747-748.
The advantages of this curriculum, according to Walker, were pedagogic, economic, and intellectual. Pedagogically, the course allowed a student to "translate for himself the fundamental principles ... as exemplified in the laboratory, into the application of these same principles in units and processes of commercial size and value." Economically, the course would train students to handle industrial machinery without either requiring MIT to buy and operate factory-scale equipment or burdening industrial firms with the creation of their own training programs. Intellectually, the course provided for "taking advantage of the directness of purpose and enthusiasm for further scientific study which the factory work will create,"91 with the students' fifth year dedicated to specializing along the lines in which he had found himself most adept.

The prospect of Noyes' and Walker's differences erupting into confrontation received a public demonstration at the 1916 meeting of the American Electrochemical Society. The "Symposium Upon Cooperation in Industrial Research" turned into a more vituperative airing of views similar to those expressed at the American Society of Naturalists' discussion 15 years earlier. Walker was especially testy, complaining how the government only helped agriculture, how university professors failed to pay attention to affairs outside the university, and how industrialists were secret mongers. He took an especially transparent jab at Noyes:

One difficulty in New England [in getting physical chemists involved with paint manufacturers] is that some of our professors take no interest in industrial affairs; they attend the meetings of the National Academy of Sciences with a religious fervor, and believe that when that is done their whole duty is accomplished.92

Fortunately, for the sake of reasoned discussion, the consulting metallurgist and MIT alumnus Lawrence Addicks calmly represented a perspective similar to Walker's. Addicks argued that professional societies should serve as clearinghouses for the transmission of industrial problems to universities. Students would work on

91All quotes in Walker, "Master's Course, 746, 747, and 748 respectively.

92The symposium was published in Transactions of the American Electrochemical Society, 29 (1916), 39.
these problems as their thesis work to become more employable upon graduation.\textsuperscript{93} Addicks went on to recount an experience he had on a consulting job in an electrolytic refinery where nobody at the plant knew if Ohm’s law applied to a current passing through a copper sulfate solution, and he could not readily find any information on this question in the scientific literature. Although he found ignorance among industrialists contemptible, he concluded, "Until we get real engineers in charge of these industries, I think it is up to the universities, first, to make a showing."\textsuperscript{94}

Noyes did not participate in the symposium, but his point of view was well represented by his fellow alumnus of Ostwald’s laboratory, Wilder Bancroft, professor of physical chemistry at Cornell.\textsuperscript{95} Bancroft responded directly to Addick’s anecdote:

Mr. Addicks also said that the universities have been making conductivity measurements on dilute solutions instead of upon commercial copper sulfate solutions. He thinks that is a pity. I think it is much better it should be as it has been. We [in the universities] have worked out something in regard to the theory of electrolytic dissociation, while Mr. Addicks himself could have measured the conductivity of his own solution.\textsuperscript{96}

While Bancroft thought it was fine for professors to be consultants to industry—he, himself consulted for over 15 firms\textsuperscript{97}—he called for a rigid distinction between consulting and university activities. When a consultant, a professor was an expert and obliged to secrecy; when a university researcher, he was a teacher and obliged to

\textsuperscript{93}Ibid., 26-7.

\textsuperscript{94}Ibid., 42.

\textsuperscript{95}Bancroft and Noyes disagreed strongly on which aspects of Ostwald’s thought formed the best foundation for further research, but “although Bancroft differed with many of his colleagues over the most promising avenue for the growth of physical chemistry, … Bancroft was interested in finding more effective ways to approach the study of chemical change; his work fell within a framework shared with those studying dilute solutions.” Servos, Physical Chemistry, 66. In the context of this symposium, the shared framework between Bancroft and Noyes overshadowed their tactical differences.

\textsuperscript{96}“Symposium,” Electrochemical Society, 51.

\textsuperscript{97}Servos, Physical Chemistry, 69.
publish. Besides, in Bancroft's view, the university did not have all that much to offer industry:

What the college can do best is to clear up the theoretical side of a [industrial] problem; but if the process is actually running successfully on a commercial scale, the question of why it works is relatively unimportant, except in so far as it may lead to new developments ... but there is no certainty of this latter occurring.  

From Bancroft's point of view, universities and industries should maintain a division of labor which left them going their separate ways.

While Addicks and Bancroft took positions similar, respectively, to Howard and Osborne in the American Naturalist's Society discussion, what is peculiar to the Electrochemical Society symposium is the lack of an academic exponent of Sedgwick's position. The person who came closest to Sedgwick's position was Willis Whitney, who had left MIT to become director of research at General Electric. G.E. was gambling that scientific investigations of commercially successful processes would lead to new developments, and Whitney was building a group of "ionists" to investigate light bulbs and vacuum tubes.  

At the symposium, Whitney argued that laboratories incorporating diverse fields were best suited to make industrial progress. While this view corresponds with Sedgwick's, Whitney saw such work as best housed in a corporation's research laboratory rather than a university's: "The universities are, properly, so far out of touch with technical practice that they do not realize the needs of industry." He worried less about university-industry relations than political forces whose anti-trust policies would make it impossible to create corporations large enough to support research laboratories.

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100 "Symposium," Electrochemical Society, 36.

101 Ibid., 37. Though not mentioned by name, Woodrow Wilson's New Freedom campaign platform must have weighed heavily on Whitney's mind.
With the establishment of the RLAC and the School for Chemical Engineering Practice, Walker, like Noyes was petitioning the MIT presidency for a departmental fiefdom within which he could use research to reform the structure and content of the Chemistry Department’s curriculum. But for Walker, the intellectual utility of such a fiefdom was not to promote and explore a theory that potentially seemed able to economize on a discipline’s theoretical foundations but to promote and refine a classification scheme that captured the variety of operations employed in the industrial exploitation of chemical reactions. Like Sedgwick, Walker was urging academic researchers to seek intellectual stimulation in technological contexts, but unlike Sedgwick, Walker did not build on a particular point of contact between a scientific novelty and engineering practice, but sought to embrace an entire industrial sector that needed factory-scale control of phenomena customarily studied by a particular discipline. To Walker, MIT was a site for aggregating industrial operations at a national scale, creating taxonomies that defined boundaries of engineering practices, and conducting research within the taxonomic framework of engineering practices. MIT’s president’s job, implicitly, was to campaign for industrial contributions of money and problems for MIT’s faculty and to define and enforce MIT’s organizational divisions.

For Walker, taxonomical thinking healthily blended the potentially conflicting needs to economize on thought and to expose students to industrial practice.

Conclusion

By the eve of World War I, MIT had become the site of three independent research programs. While all were the product of the Americans’ exposure to German practices or the urge to institutionalize research in American universities, their social and intellectual histories varied significantly. Each combined aspects of scientific practice, curricular reform, and financial patronage in ways that placed different burdens on the MIT presidency.

William Sedgwick’s campaign for public health science originated at the intersection of recent enthusiasm for experimental, as opposed to morphological,
biology and problems with Massachusetts' drinking water. By emphasizing the study of living organisms of relevance to the Massachusetts Board of Health, researching bacteria in conjunction with the Board's efforts to control the water supply, and forming lateral ties with other academic departments with an interest in public health, he hoped to create a new, hybrid specialty that could be nourished by ferment in any of its sources. General biology, as a body of knowledge that developed ways of thinking about life, receded in importance to a standard introductory course for students approaching public health science through biology. Sedgwick needed a president with the power and will to reward faculty for forming lateral ties across departments and the stature to represent MIT-based specialties effectively in national forums that considered the organization of research.

Arthur Noyes' campaign for physical chemistry originated in the research vistas opened by the use of thermodynamic concepts to interpret chemical kinetics and the infectious enthusiasm and ambition of Noyes' teacher, Wilhelm Ostwald, for an intellectually economic approach to chemical phenomena. By modifying extant courses to reflect his concerns and by tapping philanthropic enthusiasm for academic researchers with the ambition and talent to compete in international science, Noyes was able to launch and staff a research laboratory that he could control and dedicate to experimenting in areas that were eluding the grasp of theoretical explanation. His desire to use his research to fulfill Ostwald's ambition to reform chemistry through a superior elaboration of general principles remained prominent in his teaching and textbook writing. Noyes needed a president willing to grant fiefdoms within which professors could explore theoretically novel domains in isolation from other concerns and a president able to attract philanthropic patrons who would not insist that theoretically-minded scientists form lateral relations.

William Walker's campaign for chemical engineering originated in the poor use American industrialists had been making of academically trained chemists as compared to German industrialists. By demonstrating how modern laboratory results could be worked into a technological context, and by using the relationships and reputation he developed with industrialists while a professional consultant with Arthur
Little, Walker was able to create a MIT laboratory for the investigation of manufacturers' chemical problems and use the laboratory as a central clearinghouse of information from which he and Little could abstract a classification of industrial operations to serve as the basis of a curriculum. The taxonomy of unit operations, rather than a chemical theory or a group of chemical reactions, would guide the development of both research and curricula in science. Walker needed a president who would grant fiefdoms to faculty with ideas for embracing the problems of an industrial sector and who would attract industrial patrons by appealing to their enlightened self-interest in a supply of better trained engineers to help them be competitive with German manufacturers.

Prior to World War I, MIT's presidents felt no need for an MIT-wide research policy that would impose a consistent style of administration across departments. Faculty with the urge to pursue research were left to their own devices, and they produced a variety of approaches to combining their desire to pursue research with the resources, traditions, and organizational possibilities at MIT. However, forces within both MIT and the nation forced an end to this laissez faire attitude of MIT's presidents. The next chapter analyzes the circumstances that caused research policy to become an ongoing concern for MIT's presidents.
CHAPTER 3

THE EVOLUTION OF PRESIDENTIAL RESPONSIBILITIES AND THE DEMISE OF INITIAL RESEARCH

The preceding chapter contains almost no references to the MIT presidency. Once Sedgwick, Noyes, and Walker had been hired, and once they had secured laboratory space, their dependence on MIT's executives ended. Each defined his research ambitions, sought patronage, and developed curricula without help or permission from higher officials; and when, as was the case with Sedgwick, cooperation with another department was desirable, he forged his own lateral relations with chemists and civil engineers.

This independence of researchers from Institute-wide policy did not last. Three developments drove the entry of the MIT presidency into research policy. First, MIT's needs for capital improvement (and at one point, operating revenue) pressed MIT's president to raise funds from a wealthier base than MIT alumni. Under these conditions, the president was bound to judge researchers on the basis of who could hold the interest of potential donors, especially when researchers solicited the same donors as the president. Second, large-scale philanthropy became increasingly institutionalized and professionalized in endowed foundations whose independent staffs exercised considerable power to define philanthropic goals and the foundations' methods of achieving them. When a foundation's staff insisted that the character and facilities of universities, not just those of particular researchers, be part of the criteria on which grants were given for academic purposes, researchers were bound to lobby the president for amelioration of intra-university conditions that the foundation's staff found objectionable. Third, the quantitative growth of industrial
research laboratories expanded career opportunities for those with industrially-relevant research skills. The president was called on to help fashion academic-industrial relations that would create student training for industrial research without disrupting or violating intra-university policies and traditions.

Noyes, Sedgwick, and Walker each faced the challenge of securing relationships and generating results that would indicate to the president that his approach to organizing research best served the president's needs. Hanging in the balance were support for their programs and opportunities for successors with the intellectual ambitions to exploit and/or supersede their accomplishments. Had one of them succeeded, the MIT presidency would have been defined from below and hardly an office to catapult its occupants to greater prominence. However, Noyes and Sedgwick overtly failed to grasp constructive roles for MIT's president; they did not realize the task at hand until it was too late, and their programs fell into tailspins shortly after World War I. Walker initially stumbled into success and tried to capitalize on his good fortune; but in the later 1920s, his success was making MIT less internally governable without generating satisfying outlets for presidential activism.

By 1929, the Executive Committee of the MIT Corporation, frustrated by the failure of President Samuel Stratton to find administrative and financial foundations for research at MIT and responsive to the burgeoning power in the Electrical Engineering Department (to be discussed in the next chapter) was searching for a new president. The failures of the decade following World War I created vague, criteria for a successful presidency and thus an opportunity for a new president to build his reputation by impressing his experiences on the office. MIT's president would have to revive the precedents that matched his sensibilities and use them to deal with the national philanthropic foundations and the industrial patrons that continued to control the financial means for a university's development.

The body of this chapter is dedicated to analyses of the unproductive interactions among MIT's presidents and the initiators of research programs in the years after World War I. These interactions are indicative of the sensitivity of
research programs to higher administrative structures and conversely the ability of executives to affect the direction of research. By revealing pitfalls in the administration of research, they form the standard for Compton's ingenuity and decisiveness in reforming MIT. Thus they decisively altered the character and qualifications for the MIT presidency.

Sedgwick never interested a MIT president in giving "public health science" institutional status within MIT (beyond renaming the Biology Department the Biology and Public Health Department) and in promoting public health science nationally as a field that other universities could adapt to fit their local circumstances. The aggregation of bacteriology, civil engineering, vital statistics, and preventive medicine over which Sedgwick, Rosenau, and Whipple presided in the MIT-Harvard School of Public Health appeared administratively and intellectually improvisatory to officials at the Rockefeller charities who were seeking to define advanced training for full-time public health officers. When they chose to promote public health as a medical specialty (to be administered in a way that Noyes would have found comfortable), Sedgwick found he had no recourse other than to petition Maclaurin to raise an unrealistic amount of money for public health at MIT. Sedgwick died unexpectedly in 1920, but his successors saw MIT's administration abandon public health outright rather than compete with foundation-supported schools.

Noyes never tried to interest a MIT president in eliciting philanthropic support for creating departmental fiefdoms for theoretically motivated research outside of chemistry, and no MIT president tried that tack on his own. Thus when President Richard Maclaurin hit upon a successful fund-raising plea, it clashed with Noyes' insistence on his departmental fiefdom that built both its research and its curriculum around the direction of scientific theory. Though Maclaurin may have respected Noyes' accomplishments, he would not pander to Noyes' jurisdictional sensibilities to the point of overtly violating his fund-raising tactics. With his influence waning, Noyes resigned in 1919 in favor of making a fresh start at a younger, malleable California school, Throop College, where he associated himself with prominent program-builders in physics, astronomy, and biology. Back at MIT, the Research
Laboratory of Physical Chemistry withered in comparison to the Research Laboratory of Applied Chemistry.

Walker's organization of academic research around categories of industrial practice temporarily provided both an outlet for presidential activism and a means for attracting financial support from corporations. But Walker's initial success steadily made MIT more difficult to govern: laboratories where researchers worked on industrial projects for academic pay could not hold their staffs; faculty members who could boost their salaries by using MIT facilities for consulting purposes were beyond presidential control and enjoyed greater rewards than those who dedicated their after-teaching time to non-proprietary research; and worries that MIT faculty would end up in competition with their former students for commissioned studies of industrial practice could not be addressed. Stratton's efforts to deal with these difficulties pleased nobody, and the pursuit of philanthropic foundations over corporations became the policy of Stratton's successor.

MIT's Capital Needs and the Demise of Noyes' Influence

The need for more space, which had annoyed Sedgwick in the 1890s, was a recognized Institute-wide problem at the turn of the century. While nationally, universities more than quadrupled their enrollments between 1870 and 1900 (and quadrupled them again between 1900 and 1930) on the basis of the power of their degrees to launch careers in an increasingly industrial society,¹ MIT had run out of room to expand its campus in Boston's Back Bay.² MIT's administrators could not simply sell the campus and purchase a larger site, unless they could raise the money to construct new buildings.


In 1905, the plans of MIT's president William Pritchett to break this deadlock fell apart, and he began looking for his successor.\textsuperscript{3} When Arthur Noyes, then chairman of the faculty, first learned, in the fall of 1906, that MIT's executives were having difficulty in finding a new president, he did not offer his own services but instead sought to convince his friend and MIT classmate, the astronomer George Ellery Hale, to take the job. With a site change still necessary, Noyes wrote Hale, MIT's next president would have the chance "to create a new institution with more fully developed ideals of education."\textsuperscript{4} Hale declined to consider the job, citing responsibilities he had already incurred, but he offered to make himself available to consult on the design of a new campus if Noyes heeded his own assessment and took the presidency.\textsuperscript{5} Noyes did become MIT's acting president in the fall of 1907.

This boost from faculty chairman to president is ample testimony to the respect, among both faculty and corporation members, that Noyes had accrued from his international prestige in theoretical chemistry, and Noyes was willing and able to draw general conclusions on educational policy from his experiences.\textsuperscript{6} Nevertheless, Noyes did not seize the presidency as an opportunity to enlarge his influence at MIT. He persisted through the winter of 1907 in trying to get Hale to take the presidency, and when that hope was definitively quashed,\textsuperscript{7} Noyes just despaired the dearth of attractive presidential prospects and concentrated on "securing the best possible man to act upon the financial and public sides, and enlarging the duties and responsibilities

\textsuperscript{3}Pritchett's plan was to sell the Back Bay campus, move to the site now occupied by the Harvard Business School, merge MIT with Harvard, and use the bequest of Gordon McKay for a Harvard engineering school to build the new buildings. Alumni opposed to the merger found legal grounds to block the sale and thus scuttle the whole plan. See \textit{ibid}.

\textsuperscript{4}Noyes to Hale, 15 September 1906, Hale Papers, microfilm edition, Roll 27.

\textsuperscript{5}Hale to Noyes, 24 September 1906, \textit{ibid}.

\textsuperscript{6}See Section 2.2, above.

\textsuperscript{7}Noyes to Hale, 5 October 1907; Hale to Noyes, 15 November 1907; Noyes to Hale, 20 December 1907, all in \textit{ibid}. Noyes relented when Hale reported that his daughter had a condition that made a move to Boston medically inadvisable.
of the Chairman of the Faculty, so that he may be responsible for the educational
development." From this decidedly lukewarm perspective, Noyes welcomed
Richard Maclaurin, a New-Zealand-born, Cambridge-trained physicist who had been
teaching at Columbia, as MIT's new president.9

The ideal president for Noyes would have recognized and appreciated how
conceptual developments had provided Noyes with guidance for research and
foundations for a curriculum, would have encouraged the formation of similarly
constructed specialties in other disciplines, and would have enjoyed both: the
promotion of MIT to employers, patrons, and alumni and the management of MIT's
budget and physical plant. If Noyes was disappointed by the lack of candidates with
that combination of qualities, he was deluding himself in assuming either that the
president's financial or public duties could be separated from educational development
or that no challenges to the respect he then held could be mounted from within the
MIT faculty. In order to raise funds for a new campus, Maclaurin would have to
interest potential patrons with statements about MIT's educational character, and by
discouraging Maclaurin's involvement in educational policy, Noyes was creating an
opportunity for some other faculty member to gain influence.

Maclaurin did begin his presidency by earnestly soliciting MIT's alumni for
funds and seeking a site for a new campus. The issue so absorbed his efforts that he
could not but note the repetitiveness of his annual reports:

There are some problems of prime importance that must be attacked in the
immediate future. One of these is the problem of a site, a problem that has
occupied a considerable portion of presidential reports for years .... [I]n the
crowded conditions of the Institute today, an early removal is inevitable.10

Maclaurin was able to raise money through alumni channels to purchase the land that
is now the nucleus of the MIT campus, but he had to cast his net more widely in the

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8Noyes to Hale, 20 December 1907, ibid.

9Noyes to Hale, 6 November 1908, ibid.

10Bulletin, of the Massachusetts Institute of Technology: Reports of the President and Treasurer,
January 1911, 22-23.
search of funds for construction. Maclaurin needed to enlist one of the nation’s supremely wealthy industrialists to the cause of MIT, and he struck gold (or at least silver nitrate) in 1912 when he wrote to George Eastman.

Maclaurin’s appeal to Eastman stressed the familiarity with and readiness of MIT’s graduates to cope with industrial practice and played to the sense of grandeur and self-importance that came from shouldering a "national" responsibility. As evidence of the good that would come from MIT’s expansion, Maclaurin quoted Thomas Edison as stating that MIT graduates "have a better, more practical, more useful knowledge as a class than graduates of any other school in the country."\textsuperscript{11} Eastman, Maclaurin urged, should consider contributing to MIT because he wished to count himself among a select group: "Fortunately, there are in the country [the United States] men of large vision who appreciate the national importance of such institutions [as MIT], and are ready to help where they are convinced that encouragement is deserved."\textsuperscript{12} Conspicuous by their absence from Maclaurin’s letter were any mention of the power that Noyes attributed to students trained through the study of theoretically-troubling problems or the service that Sedgwick attributed to an academic laboratory and curriculum constructed around the natural processes that were strategic to the technology of a local government agency. Though not consciously designed to do so, Maclaurin’s letter promoted MIT on William Walker’s terms. Eastman anonymously donated $3.5 million, which, when combined with the smaller donations it stimulated and the proceeds from a legally successful sale of the Back Bay property, enabled MIT to construct its new campus.

Maclaurin relied on his own contacts and sensibilities rather than MIT networks and traditions when it came to planning the new campus;\textsuperscript{13} Noyes found himself excluded from the planning, and he had misgivings about some of the results, even though he had no complaints about his space:

\textsuperscript{11}Maclaurin to George Eastman, 29 February 1912, MIT Archives, AC 13, 25/1851.

\textsuperscript{12}Ibid., my emphasis.

\textsuperscript{13}Sinclair, "Genteel Tradition," 12-14.
The buildings are making rapid progress .... The Chemistry Department has come out, I think, on the whole very satisfactorily. I am extremely sorry that the Physics Department should be so cramped. I don't fully understand how it came about.\textsuperscript{14} Noyes' estrangement from Institute-wide policy deepened as Walker's program in chemical engineering developed. At the beginning of 1916, his sense that MIT's "science work is being more and more subordinated to its engineering work" was sufficiently strong that he threatened to resign if Maclaurin did not adopt practices that would halt MIT's slide towards "becoming a technological institute in the narrow sense." Noyes presented a host of suggestions, but primary among them was that Maclaurin vest the authority currently held by presidially appointed department heads in departmental councils, comprised of each department's senior faculty, that would elect their own chairmen, directors of laboratories, secretaries, and directors or "promoters" of research.\textsuperscript{15} In positive terms, this proposal, a generalization on Noyes' original conception of the RLPC as an autonomous Department of Chemical Research,\textsuperscript{16} was aimed at creating departmental authorities who would be respectful of faculty with high reputations within the discipline. But it was also a slap at presidential authority, and if adopted, would have limited the president to deciding on proper budgetary allocations among departments and excluded him from coordinating departmental activities and instigating and supporting departmental reforms.

If Maclaurin had any inclination to limit his authority for Noyes' benefit, he would have lost it quickly. Shortly after receiving Noyes' letter, Maclaurin learned of two developments. First, Walker and Arthur Little, who were ignorant of George

\textsuperscript{14}Noyes to Hale, 3 October 1914, Hale Papers, Microfilm Edition, Roll 27.

\textsuperscript{15}Noyes to Maclaurin, 31 January 1916, MIT Archives, AC 13, 15/437. Among Noyes' other requests were that Maclaurin grant junior staff the option to spend half their time on research (with promotion contingent on successful research as well as excellent teaching) and cope with any shortfall in teachers by limiting contingent enrollments; and that Maclaurin both grant more money and space to the Research Laboratory in Physical Chemistry and permit the Laboratory's merger with the Division of Theoretical Chemistry to bring about better management of resources for instruction and research.

\textsuperscript{16}See Chapter 2.2, above.
Eastman's contribution to the new campus, had solicited Eastman to contribute to the creation of the School of Chemical Engineering Practice, and Eastman, rather than being annoyed at the solicitation, not only agreed to give $300,000, -- a sufficient quantity to give the school a trial of several years -- but also indicated a willingness to consider another proposal for a major donation. And second, Walker complained about subversive resistance from Noyes as the Chemistry Department considered the appropriate requirements for admission to the School of Chemical Engineering Practice. Eastman's interest in a departmental reform that was generating intra-departmental controversy could hardly have failed to impress Maclaurin with his own need for involvement in educational development to insure the continued good will of a major donor.

To his credit, Maclaurin did try to create common ground among Noyes, Walker, and Eastman. Maclaurin recommended that Eastman boost his contribution to the School of Chemical Engineering Practice by $200,000 in order to provide an endowed income for the Research Laboratory of Physical Chemistry. Maclaurin argued that to make the school successful:

We must, if we are to profit by the experience of Germany, carry a number of them [the students] a good way along lines of chemical investigation .... I have hoped ... that we could do something to strengthen our existing laboratories of physical chemical research. Nothing came of this suggestion, and had the matter been pursued, Walker and Little would surely have argued the Research Laboratory of Applied Chemistry, properly fulfilled the need.

During World War I, Noyes and Maclaurin went further in contrasting directions. Noyes helped Hale in his efforts to invigorate the National Academy of Sciences by making it the organizer and administrator of a national defense research

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17Maclaurin to Eastman, 21 February and 27 May 1916, MIT Archives, AC 13, 25/1851.

18Walker to Maclaurin, 16 May 1916, MIT Archives, AC 13, 21/615; Servos, "Industrial Relations" 538.

19Maclaurin to Eastman, 27 May 1916, ibid.
program for academic scientists;\textsuperscript{20} Maclaurin lobbied the War Department to use MIT to train engineers for the Officers Reserve Corps and ended up in Washington as Educational Director of the Student Army Training Corps.\textsuperscript{21} Still, in February 1919, Noyes, perhaps believing his standing had been strengthened by the Academy's accomplishments in creating and obtaining a peace-time existence for the National Research Council, began "a great fight to get a much more thorough, and at the same time more cultural, physics course included in our curriculum; also to get much more time allotted to humanistic and broad natural science subjects (astronomy, geology, organic evolution, etc.)."\textsuperscript{22} Noyes believed success was likely because he had Maclaurin's backing. But either Maclaurin reversed himself or Noyes never understood Maclaurin's stand in the first place, for in April Noyes was plying Hale with "ammunition" for Hale to use in a "fight to convince Maclaurin of the need for aggressive action if the science departments and research work are to be properly developed."\textsuperscript{23} At the same time, Noyes was petitioning Maclaurin to ignore Walker's latest appeals to effect changes in the content and/or staffing of chemistry courses.\textsuperscript{24}

Despite Noyes' efforts, Maclaurin's primary concern had to be the prospects for matching a conditional gift of $4,000,000 from Eastman to build MIT's endowment.\textsuperscript{25} Among the last things Maclaurin needed were the responsibility to explain a major curricular reform and the need to curb the ambitions of a faculty


\textsuperscript{21} Maclaurin to Eastman, 30 March 1917, 2 October 1917, and 21 October 1918, all in MIT Archives, AC 13, 25/186l.

\textsuperscript{22} Noyes to Hale, 20 February 1919, Hale Papers, Microfilm Edition, Roll 27.

\textsuperscript{23} Noyes to Hale, 9 April 1919, \textit{ibid}.

\textsuperscript{24} Noyes to Maclaurin, 5 April and 7 April 1919, copies in \textit{ibid}.

\textsuperscript{25} Maclaurin to Eastman, 5 June 1918, MIT Archives, AC 13, 25/186l; Charles Stone to Frank Vanderlip, 16 August 1919, MIT Archives, AC 13, 19/550.
member with strong industrial contacts. This time Noyes did make good on his threat to resign,\textsuperscript{26} though he waited until November in order to complete some MIT-based projects. He left a protege at MIT and the research laboratory itself, but what he took with him—his reputation and abilities in teaching and research and his contacts with the Carnegie Institution—proved more valuable. The protege, in Walker’s opinion, was "but the reflection of yourself [Noyes] without the clearness of thought and the originality in method which you possess."\textsuperscript{27} And the laboratory’s budget stagnated and its equipment depreciated without the injection of philanthropic funds. Soon it became evident that MIT was losing its reputation as a school that launched and supported careers in academic chemistry.\textsuperscript{28}

Noyes’ demise was due to the interrelationship of two factors. First, he insisted on departmental self-management and the autonomy of the individual professor within his courses when MIT’s president needed to maintain harmony between MIT’s internal development and his appeals for financial support from outside MIT circles. And second, Noyes aimed for a curriculum that culminated in research that sought limits to the power of physical chemistry’s conceptual framework when an industrialist like George Eastman supported MIT for the understanding of industrial practice that it imparted to its students. Given the choice of fitting physical chemistry into Walker’s taxonomy of industrial practice or starting anew in California, the not-so-young Noyes went west.

National Philanthropic Foundations and the Demise of Public Health Science

William Sedgwick and his successors never even had that choice. Their problems did not stem from a desire to keep MIT’s president out of educational issues, but from a failure to cultivate and use MIT’s president. Sedgwick built his relations within MIT and to the Massachusetts Board of Health and Harvard Medical

\textsuperscript{26}Noyes to Hale, 21 April 1919, Hale Papers, Roll 27.

\textsuperscript{27}Walker to Noyes, 20 January 1919, MIT Archives, AC 13, 21/616.

\textsuperscript{28}Servos, "Industrial Relations," 534.
School with neither help nor hindrance from MIT's presidents; and in turn, he neither offered "public health science" as an innovation that a president could use to enhance MIT's reputation among schools of science and patrons of universities nor sought reform of the presidency to improve the standing and security of his innovation. When the nationally oriented Rockefeller charities took up the issue of the appropriate training for public health officers and the appropriate institutions for providing that training, there was no way to represent Sedgwick's blend of bacteriology, civil engineering and vital statistics as the model for what other combinations of scientific schools, medical schools, and local governments were attempting rather than a local peculiarity of MIT and its environs. When the Rockefeller charities decided on a medically based and medical-school-affiliated approach to public health training, MIT's biologists and engineers withered away.

By the standards of the early 20th century, George Eastman was one of a vanishing breed with respect to his practice of personally choosing the objects of his philanthropy and directly negotiating the terms of his gifts with his beneficiaries. For the men who made fortunes late in the 19th century by successfully coordinating a commodity's price and supply to fit the conditions of an increasingly national economy, the distribution of philanthropy likewise came to be treated as a responsibility to be professionally administered according to a reading of national and international needs.29 Andrew Carnegie and John D. Rockefeller most notably, in the first decade of the 20th century, began to establish philanthropic foundations, whose full-time staffs could specialize in identifying and administering programs for areas where philanthropy might instigate social reform. While numerous others emulated their example, for higher education, the Rockefeller and Carnegie foundations remained the overwhelmingly dominant source of funds.30 And because


30Ernest V. Hollis, Philanthropic Foundations and Higher Education, (New York: Columbia University Press, 1938), 115-126. Higher education was particularly appealing to Rockefeller and Carnegie charities because the policies of educational institutions could be discretely influenced in
Carnegie's foundations disbursed smaller grants more widely to institutions that exceeded a base-line standard for eligibility, the Rockefeller boards were the only source of concentrated financing for major innovations at individual schools.\textsuperscript{31}

In establishing foundations, philanthropists ceded most discretionary authority to the foundations' professional staffs,\textsuperscript{32} which had far greater capabilities than their patrons to assess how the recipients of philanthropy should use bequests and thus far greater potential to redirect the efforts of recipients and the policies of those who aspired to become recipients. George Eastman was in no position to address issues in the planning of the new MIT campus, and while his support of the School of Chemical Engineering Practice did favor one branch of the Chemistry Department over another, Maclaurin neither sought nor received his advice on how to handle Noyes' and Walker's feuds. But when the Rockefeller Foundation moved towards support of public health schools in 1914, its officials were already accustomed to setting standards and guidelines for the internal policies of academic recipients. Local traditions either had to be in line with the foundation staff's thinking, or had to be reformed, or had to survive without foundation support.

Public health education stood at the intersection of two Rockefeller philanthropies. Since 1909, the Rockefeller Sanitary Commission, under Wickliffe Rose's direction, had been fighting for the eradication of hookworm in the southern United States. After four years campaigning to convince physicians in the field to aggregate their findings and coordinate their efforts, Rose saw his superiors' ambitions for an international program as contingent upon the creation of a cadre of professionals dedicated to careers in public health.\textsuperscript{33} For help in creating such a

\textsuperscript{31}Berliner, System, 25.


cadre, Rose, in 1914, turned to colleagues at an older Rockefeller charity, the General Education Board, incorporated in 1902.\textsuperscript{34}

The GEB, whose first guiding personality, Frederick Gates, was enamored of medical research that sought for microbial causes of diseases with bacteriological methods, had shifted from an exclusive concern with supporting a free-standing research institute to a broader concern with reforming medical schools so that their faculties would devote themselves to laboratory research over the commercial interests of private practice and the sectarian disputes over therapeutic methods.\textsuperscript{35} Between 1911 and 1913, Gates and his protege, Abraham Flexner, found they could make an exemplar of Johns Hopkins Medical School, whose dean, William Welch, was willing, with Rockefeller support, to make clinical professorships full-time, salaried positions. In October, 1913, the GEB gave Hopkins $1.5 million for the William Welch Endowment for Clinical Teaching and Research with the contractual stipulation that the principal be forfeited if the interest were not used to pay the salaries of full-time clinical professors.\textsuperscript{36}

Flexner and Rose pursued Rockefeller support for public health education as a continuation in style and substance of the reform of medical school professorships—they sought a university that was willing to adopt the foundation's ideals and able to serve as a standard-bearer for others. MIT never figured seriously in their calculations. In October 1914, they convened a conference of foundation officials, New York public health officials, and professors from Pennsylvania, Columbia, Chicago, Johns Hopkins (Welch), and Harvard (Rosenau and Whipple) to outline the type of university institution that would meet Flexner's and Rose's criteria. In this setting, the Harvard-MIT school, with its administration by a three-man committee representing biological, medical, and engineering departments, came across


\textsuperscript{35}Berliner, System, 53-75; Wheatley, Philanthropy, 25-39.

\textsuperscript{36}Wheatley, Philanthropy, 60-73.
as an ad hoc arrangement tailored to its participants obligations and loyalties to other divisions of Harvard and MIT. To create a model that others could emulate and that would familiarize future public health officials with medical practice, the conference called not for a freestanding School of Public Health but for an Institute of Hygiene with access to a teaching hospital.\textsuperscript{37} The name, which came from Max von Pettenkoffer’s institute at the University of Munich where Welch had pursued postgraduate studies, and link to a teaching hospital implied the Rockefeller charities would support public health as a medical specialty organized along the lines that was familiar to MIT from Noyes’ use of Ostwald’s institute as the model for the Research Laboratory of Physical Chemistry.

Not until one year after the conference did Sedgwick get the opportunity to comment on its findings.\textsuperscript{38} He stressed to Flexner the importance of “an almost absolute independence of the new establishment” in opposition to the ties that the conferees had implied to medical practice and went on to state (negatively) his belief in administratively porous research laboratories for American universities:

\begin{quote}
I firmly believe that the new school should be built upon and around research and charged with its life-giving spirit. I am not convinced that its “nucleus” should be an Institute of Hygiene of the continental European type. The conditions actually existing in America, and the problems to be met in the American democracy, are so different from those of the continent of Europe that there is grave danger, in following too closely European models, of failure to articulate effectively American ideals and American life.\textsuperscript{39}
\end{quote}

Success in both educational and policy terms had stemmed, for Sedgwick, from collaborative arrangements among MIT faculty from several departments and the state and municipal laboratories and experiment stations. From his perspective, to create

\textsuperscript{37}Fee, \textit{Disease}, 35-42. The official view from the conference was enshrined in a document named the “Welch-Rose Report” although it was actually Welch’s substantive reorientation of Rose’s conception.

\textsuperscript{38}Flexner invited Sedgwick’s comments after snubbing Sedgwick during the Foundation’s visit to Boston to evaluate sites for the new Institute; Fee, \textit{Disease}, 45.

\textsuperscript{39}Quoted in Curran, \textit{Founders}, 14, from notes on a November 1915 conference between Flexner and Sedgwick in the Rockefeller Foundation Archives.
an academic fiefdom within a medical framework was to ignore the state's need for sophisticated engineers to manage public works and to inhibit academics from experimenting with their own social relations in response to shifts in the demands placed on public health officials.

By the time Sedgwick made this pitch, however, Flexner had probably already decided to place the Institute at Johns Hopkins, even though Baltimore's elected officials had been unwilling to make public health appointments non-partisan, and even though Hopkins' engineering school was modest in size and achievements. Everyone in Cambridge and Boston may have been equally dismayed when the Rockefeller Foundation chose Johns Hopkins for its first venture into public health education and research. But not everyone was equally able to recoup. During the site visit to Boston, Flexner had paid more attention to Harvard Medical School's lack of control over its teaching hospitals than the Harvard/MIT School for Health Officers. Although Johns Hopkins' School of Hygiene and Public Health was administratively independent of the medical school, the structure of Hopkins' medical school (including full academic control over the University Hospital) had apparently paved the way for the School of Hygiene and Public Health. Harvard could set about reforming its medical school to meet the GEB's standards; MIT had no medical school to reform.

In the wake of the Rockefeller Foundation's decision, Sedgwick alerted Maclaurin that the regional foundations on which Sedgwick had carved out a prominent niche for MIT in public health education were being undermined: "If Massachusetts and New England are to continue to occupy their traditional position as leaders in the promotion of public health in the United States, they must very seriously bestir themselves." He recommended that Maclaurin seek the means to establish an Institute of Public Health that would expand on the extant School for

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40 Rose went along with Flexner, even though Welch's revisions of Rose's initial proposal undermined important aspects of that proposal, because Hopkins' salaried clinical faculty seemed least likely to sabotage public health as a threat to physicians' incomes. Fee, Disease, 50-52.

41 Fee, Disease, 45.
Public Health Offices by adding divisions of Testing and Research, Publicity and Education, and Library and Museum. The Testing and Research Divisions coordinated seven sub-divisions that placed aspects of biology, engineering, and social science on an equal footing with medicine. But when Sedgwick estimated start-up costs and the need for an endowment to provide for part of the annual operating expenses, he still came to the conclusion that "not less than a million dollars would be absolutely necessary for its [the Institute's] proper inception." That was more than Maclaurin had raised for a new MIT campus before Eastman's intervention. Although Maclaurin plugged Sedgwick's proposal in the next annual report, Sedgwick's institute never materialized; indeed a dozen years later, and seven years after Sedgwick's death, his successor was still looking for someone to finance construction of a Sedgwick Memorial Laboratory at MIT.

The Harvard/MIT School for Public Health Officers continued to grow, and growing correspondingly were its organizers' belief in the school's intellectual soundness and their frustration at the financial and administrative restrictions on its development. In December, 1920, they resolved to press for a new overture to the Rockefeller Foundation and a search for a "substantial and satisfactory basis and organization," vaguely concluding that a satisfactory organization would involve a "closer relation with Harvard University." To some, Sedgwick seemed to concede

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42The seven sub-divisions were Laboratories (ideally including chemical, bacteriological, biochemical, diagnostic, serum, and vaccine), sanitary engineering, vital and social statistics, personal hygiene (including applied physiology), epidemiology, industrial hygiene, and sanitary law. Only personal hygiene was plainly the physician's realm; biologists, engineers, or social scientists could all claim leadership for one or more of the other divisions.

43Sedgwick to Maclaurin, 8 December 1916, MIT Archives, AC 13, 18/350. Final costs would be still higher, since Sedgwick assumed the Institute would not initially need a director of the library and museum division and facilities for the care of laboratory animals.

44Bulletin of the Massachusetts Institute of Technology: Reports of the President and Treasurer, 1917, 23.

45Samuel Prescott to W. T. S. White, 13 March 1928, MIT Archives, AC 13, 16/457.

46Curran, Founders, 19, quoting Administrative Board Minutes of 19 December 1920.
the necessity of organizing public health education and research within a medical environment.\textsuperscript{47} But whatever his attitude, it quickly became moot; he died unexpectedly a month later.

Sedgwick's successor at MIT did not remember him as conceding primacy to Harvard Medical School. In preparing notes on the Biology Department's accomplishments for the first post-Sedgwick review by the Visiting Committee, Samuel Prescott, the acting department chairman, wrote:

Technology [at Sedgwick's instigation] was the first school to recognize public health as a distinct profession and has trained many men of national and international reputation.... A very considerable group of non-medical specialists have entered the public health field with such success as ... to produce a statement from Dr. Victor Vaughan, Dean of the University of Michigan Medical School, that the sanitary engineer is in many respects better equipped for public health work than the physician.\textsuperscript{48}

That view of Sedgwick's accomplishments became the basis on which MIT's public health faculty would judge innovations in public health education and research. What transpired left them bitter and isolated.

The failures of Harvard Medical School to attract Rockefeller support inspired the importation, in 1918, of David Edsall from the University of Pennsylvania to serve as the dean.\textsuperscript{49} Edsall enjoyed prompt success in attracting Rockefeller support for improvements in Harvard-affiliated hospitals.\textsuperscript{50} In March 1920, six months before the administrators of the School for Health Officers moved to reopen discussion of their status, Edsall was lobbying Wickliffe Rose for a Harvard Public Health School that would be less independent from the medical school than Johns Hopkins' and retain a "nominally maintained" affiliation with MIT. In December,

\begin{itemize}
\item \textsuperscript{47}Curran, \textit{Founders}, 25, and Fee, \textit{Disease}, 216 so interpret Sedgwick's attitude.
\item \textsuperscript{48}"Report of the Committee on Biology and Public Health," 4 January 1922, MIT Archives, AC 13, 5/124.
\item \textsuperscript{49}Wheatley, \textit{Philanthropy}, 73-82. In addition to not winning the first Rockefeller-endowed venture into public health education, the medical school had twice failed to elicit support for incremental reforms towards a plan of full-time faculty appointments.
\item \textsuperscript{50}Curran, \textit{Founders}, 22-24.
\end{itemize}
Rose urged Edsall to file a formal application, and that preempted any petition from Rosenau, Whipple, and Sedgwick. Edsall proposed that the new school consist of four departments (preventive medicine, industrial medicine, tropical medicine, and comparative pathology) carved out of the medical school, one (sanitary engineering) import from the university, and three (bacteriology, immunology, and vital statistics) created de novo with Rockefeller financing. While the names and number of departments shifted, the overall arrangement was sufficiently to the Foundation’s liking that by February 1921, financial terms were the only outstanding issue.\textsuperscript{51}

The waning importance of MIT and its supporters at Harvard became apparent when Harvard’s president, in November 1921, appointed a committee to work out the details of staffing new departments and defining curricula from the expanded offerings. He not only bypassed the MIT professor who was filling Sedgwick’s slot on the administrative board of the joint school, but also George Whipple, his own professor of sanitary engineering. Of the original triumvirate, only Rosenau served on the new school’s organizing committee, which reported to Edsall, who became dean of both the medical and public health schools. "Cooperation" with MIT was reduced to a statement in the catalogue notifying students that (unspecified) MIT courses could be taken for credit.\textsuperscript{52} When Whipple, citing his commitment to teach municipal engineering administration in the Government Department and his belief that the public health school should be as oriented towards the university as the medical school, declined to join the public health school’s faculty, sanitary engineering’s courses were also not listed in the public health school’s catalogue.\textsuperscript{53}

For Prescott and Claire Turner, MIT’s specialist in public health education, the development of Harvard’s School of Public Health as an offshoot of the medical school, was morally offensive as well as intellectually misguided. MIT, by their reckoning, had provided three fourths of the instruction in the joint school, and

\textsuperscript{51}Ibid., 24-30.

\textsuperscript{52}Ibid., 26-31.

\textsuperscript{53}Ibid., 228-232.
Sedgwick, in their memories, had agreed to Harvard administration of new monies with the understanding that the cooperative school would continue. The GEB’s policies, they concluded, were driven by "the influence of important individuals with the directors of foundations" as well as a commitment to medically-trained public health officials. Harvard’s administrators appeared deceptive about their intentions and too greedily self-interested to be cognizant of the advantages of cooperation with MIT.

This combination of intellectual differences and personal animosities could have become the stuff of a productive, if unpleasant, competition, except that MIT lacked the means to mount a powerful alternative to Harvard and Hopkins. The GEB would not reconsider its policies. To Flexner, the GEB "had a responsibility to promulgate authoritative, universally applicable institutional models ... [and to] mandate a strengthening of those local hierarchies and leaderships which supported the national agenda." Sedgwick’s use of MIT to pull together a multi-disciplinary approach to local public health issues could not possibly serve as a universally applicable institutional model in the absence of a MIT president who could forcefully argue that such practices were the best policy for the advancement of American sciences generally. But MIT had no place else to turn. There was no other foundation in the business of granting endowments for programs in advanced education and research; MIT was not going to raise such an endowment from the regular contributions of its alumni; and no independently wealthy benefactor was found to contribute an attention-grabbing gift for Biology and Public Health.

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54Turner to Prescott, 18 January 1932; Turner to Prescott, 9 March 1934 attached to Prescott to Compton, 3 April 1934; "Extracts from Memoranda Prepared by Dean Prescott for Dr. Compton at His Request in 1932 and 1934;" all in MIT Archives, AC 4, 1/17.

55At one point Turner, armed with an endorsement from Welch, inquired about support for one of his studies, but was told the GEB did not consider educational research a branch of public health research, and if it did, would probably make grants to Harvard. Turner to Prescott, 18 January 1932, MIT Archives, AC 4, 1/17.

56Wheatley, Philanthropy, 50.

57Samuel Prescott to W. T. S. White, 13 March 1928, MIT Archives, AC 13, 16/457.
While Noyes’ and physical chemistry’s demise at MIT lay in his insistence on departmental self-management and his rejection of a presidential role in educational and research policy, Sedgwick’s and public health science’s demise lay in the insufficiency of MIT’s institutional stature and power. Sedgwick had managed locally to knit together parts of biology, chemistry, civil engineering, and statistics into a state-affiliated program of teaching and research at MIT. But the power of the purse rested with the philanthropic foundations, whose officials believed their goals were better advanced by working through the traditional structures of the older American universities and the newer imitators of German universities rather than through the scientific schools established following the Civil War. Between Rose’s concern with securing public health officers who could gain the cooperation of an area’s physicians, Flexner’s dedication to inspiring reforms in medical schools by financing innovations at exemplary institutions, and Welch’s ambitions for administering a continental-style research institute for post-doctoral MD’s, the only chance for Sedgwick’s local innovation to receive support was to argue that the Foundation should reevaluate its criteria for judging educational innovations. But Sedgwick was not even invited to the meeting that laid out the philosophy for Rockefeller-endowed public health schools, and Maclaurin had cultivated industrial magnates, not the staffs of philanthropic foundations. When the precedent-setting Rockefeller endowments went to Johns Hopkins and Harvard, Sedgwick and his successors found that neither they nor MIT’s president could command the means either to compete with or to emulate the Rockefeller-backed schools. While Noyes went west, Sedgwick’s approach to public health became the road not taken.

Administrative Needs and the Regulation of Corporate-Sponsored Research

Following World War I, William Walker’s approach to organizing research stood preeminent among the three early efforts to institutionalize research at MIT. His taxonomy of chemical techniques employed in manufacturing was the national standard for accrediting schools that offered degrees in chemical engineering; he had acquired the patronage of George Eastman, which would have won him the respect of
MIT's president even if Eastman had not been secretly bankrolling MIT's endowment; and he had interested a spectrum of firms in participating in his research and teaching program.

A series of fund-raising challenges and presidential misfortunes allowed Walker to turn his departmental success into Institute-wide influence. In 1919, a court ruling voided MIT's arrangement with Harvard, under which MIT received 60% of the operating income generated by Gordon McKay's bequest. That same year, George Eastman offered MIT another four million dollars in endowment on condition that MIT raise an equal sum on its own. On the heels of these financial challenges came presidential turmoil. In January 1920, Richard Maclaurin contracted pneumonia and died; his successor resigned for medical reasons before taking office. Not until January 1923 was the Executive Committee of the MIT Corporation, recruiting from applicants who knew they were third choice at best, able to install Samuel Stratton, a long-time director of the Bureau of Standards, as the full-time president. In the interim, a faculty Administrative Committee managed MIT's daily affairs.

To raise the funds to meet Eastman's challenge while coping with the immediate drop in operating income, Maclaurin enlisted Walker to launch a campus-centered drive to boost MIT's non-tuition income.\(^{58}\) What emerged was the "Technology Plan," a truncated, Institute-wide version of the School of Chemical Engineering Practice. Where the School bartered the research of faculty and thesis-writing masters students into a firm's processes and products for the pedagogic use of the firm's plant, the Technology Plan sold firms easy access to the former for a contribution to MIT—the contribution serving to tide MIT over its loss of income and to count towards the funds needed to match Eastman's offer.

In narrow terms, the Plan was thoroughly successful. Walker formed a Division of Industrial Cooperation and Research (DICR) to administer the Plan; it

drummed up over a million dollars in contracts; and Eastman’s challenge was met.\textsuperscript{59}

But for Walker, the Plan was an embodiment of his longstanding, broad ambitions to forge an American industry-university nexus to rival Germany’s:

[The plan demonstrates] that close cooperation between the industrial interests and the educational institutions of the country, which in Germany was made so effective by the domination of both by the state, can, in America, be brought about only by a voluntary personal relationship between the executives of the companies and the instructing staffs of the institutions.\textsuperscript{60}

Without a full-time president and with Walker, for a time, chairman of the Administrative Committee, there was no authoritative figure to press Walker for a justification of how an expedient technique to raise a large fund quickly could support a reform of national significance.

As an effort to create an Institute-wide research policy that would serve as an enduring exemplar, the Technology Plan failed by quickly falling apart. The proximate causes of deterioration lay in administrative bungling.\textsuperscript{61} Nevertheless, from its inception, the effect of the Plan on the governability of MIT was a concern. "To sell its services beyond the confines of its own walls," one observer noted to a member of the MIT Corporation’s Executive Committee in December 1919, "the


\textsuperscript{61}Walker failed to address the character and quantity of work the Plan would generate, to define relations between the Research Laboratory of Applied Chemistry and the Division of Industrial Cooperation and Research (beyond his personal ties to both), and to spell out whether Technology Plan contributors were to pay additional charges for any research they commissioned. With faculty protests promising to embroil Walker in a new round of polemics, he resigned. Charles Norton, a professor of physics with industrial interests, took over the DICR, and the DICR and the RLAC became competitive. As their competition for research sponsors led the RLAC to solicit Technology Plan contributors and the DICR to solicit non-contributors, the question of benefits to contributors became further muddled. The DICR staff member who contacted firms in 1924 about renewing their Technology Plan contract "took quite a beating...and in most places was told to get out none too politely." See Servos, "Industrial," 540; Karl Compton, "Memo of Conversation with Prof. Millard regarding the DICR," 23 September 1931, MIT Archives, AC4, 1/6.
Institute [would have to] compete with its own graduates after sending them into the field." The committee member confessed that "when one thinks it over, it appears that there is considerable force in that argument." He promised to communicate his "grave concern" to Maclaurin, but then went on to cite the importance of the Technology Plan to raising the funds to match Eastman's offer.62

Maclaurin died a few weeks later, but concern over the effect of the Technology Plan on the relationship between MIT and its alumni did not die with him. Edwin Wilson, an Administrative Committee member and chairman of the Physics Department, was alarmed at the prospect of a battle between alumni, intent on limiting the faculty's on-campus activities to teaching, and the Executive Committee, intent on building MIT's endowment through industrial service.63 Preferring that power reside in the Executive Committee, which attracted higher quality people than the leaders of the Alumni Associations, Wilson warned another Executive Committee member: "If the fight comes at Tec between Corporation and Alumni ... the Executive Committee would do well, I believe, to have the faculty firmly on their [sic] side." Such an alliance, in Wilson's view, was hardly a given because the faculty was inbred and influential in alumni affairs and because the president's and Executive Committee's initiatives in securing a better campus, larger endowment, and accommodation with Harvard had left the faculty feeling unconsulted and ignored. Wilson recommended that the Executive Committee choose the next president for his ability to restore good relations between the faculty and the Corporation.64


63A second possible source for Wilson's familiarity with the problem was Dugald Jackson, chairman of the Electrical Engineering Department, who encountered skepticism and fears among engineering consultants who had heard about the Plan; see Noble, Design, 142-143.

64Edwin Wilson to Edwin Webster, 4 April 1921, MIT Archives, AC 13, 22/622.
Neither Wilson’s hopes nor fears were realized. Samuel Stratton, whose tenure at the Bureau of Standards featured government-industry cooperation in research, was hardly the "faculty man" Wilson hoped for, but he did not pursue corporate support for on-campus activities to the point of inspiring formation of a faculty-alumni coalition dedicated to keeping MIT’s activities self-contained. Stratton simply lacked the intellectual energy and discipline to generate hopes and fears. Arthur Noyes, hardly a detached observer, nevertheless complained about Stratton’s appointment in terms agreeable to all partisans in the struggle over research at MIT:

I feel much disappointed in the appointment …. For one thing, Stratton is altogether too old for the position, being now sixty-one years old …. [His appointment] is due to the fact that Frederick Fish [a member of the Executive Committee] … was determined to have a man of outstanding position, with only secondary reference to his real qualities. Stratton’s presidency offered partisanship the opportunity to muddle through, but that turned out to serve nobody’s interests, including Stratton’s.

Walker’s protegé, Warren Lewis, in the 1920s, inherited an autonomous department of chemical engineering and enjoyed unfettered freedom to pursue corporate patrons for the RLAC. The result was a roughly twenty-fold expansion in the RLAC’s budget. This quantitative change brought managerial difficulties. Walker and Arthur Little had envisioned the RLAC as inculcating industrially-relevant research skills in students who would go on to design and operate the factories of firms that had paid little heed to chemists. But in its expansion, the RLAC netted large corporations with the means and interest in maintaining "in-house" research.

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65Wilson himself was not part of MIT’s future; he accepted the professorship of vital statistics at Harvard’s School of Public Health.

66Noyes to George E. Hale, 20 October 1922, Hale Papers, Microfilm Edition, Reel 27. Noyes also complained about Stratton’s background in government over academic administration and his presumed preference for industrial over basic research.

67Servcs., “Industrial Relations,” 541.

68Recent clients, as of January 1924, included Goodyear Tire and Rubber, Standard Oil, Humble Oil, and three sub-divisions of General Motors. See W. K. Lewis, to Stratton, 20 January 1924, MIT Archives, AC 13, 25/176f.
Such firms were as interested in the RLAC's staff as its students, and staff members were frequently delighted to exchange supervision of industrial research at university pay for supervision of industrial research at corporate pay. To Lewis' consternation, a string of nine directors and assistant directors left the laboratory during the 1920s for "responsible positions in industrial research" at salaries the department could not hope to match. By 1931, Lewis was recommending the Laboratory be disbanded if continuity in supervision could not be maintained.\(^6\)

In the fall of 1925, Lewis alerted MIT's administration of the Laboratory's difficulty in holding a director and in providing the staff with opportunities to pursue "scientific work" along side of the sponsored research. Stratton responded by blaming the RLAC's problems on its separation from the Chemistry Department and on the RLAC's preference for contracting with individual firms instead of trade associations.\(^7\) He clearly had not grasped what Lewis meant by "science" and the means for its pursuit. Science, for Lewis, was the imposition of intellectual order on industrial practice:

The ideal problems [for the RLAC] are the theoretical analysis of reactions and processes, search for new methods and products, and the attempt to replace empiricism by scientifically sound bases of analysis. Factory trouble hunting ... is outside the legitimate function of the Laboratory ... [as is] the determination of fundamental physical and chemical constants.

He valued contracts with individual firms as allowing the RLAC "intimate contact with chemical industry, thus assuring the Laboratory problems of vital interest and a knowledge of the most pressing needs of industry." He found contracts with trade associations "operate chiefly to the detriment of the Laboratory's aim of training men in the methods of applied chemistry," because the associations' members feared that

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\(^7\) W. K. Lewis to F. P. Fish, 27 October 1925, and Stratton to Fish, 9 November 1925, MIT Archives, AC 13, 26/2131.
technical intimacy with the association would spread proprietary information to competitors. 71

The "scientific work" Lewis wanted the RLAC to initiate did not require different sponsors but fewer sponsors:

Work for clients is always under pressure. This possesses obvious advantages, particularly in the education of men for industrial work. However, too much pressure kills intellectual initiative .... For best results, a larger proportion of the work of the Laboratory should be under its own control. 72

Lewis wanted more time for the laboratory staff to search for, experiment with, and codify for teaching the unit operations common to the sponsored problems the RLAC attracted. Such opportunities, he assumed, were not available in industrial laboratories and would thus help MIT to hold RLAC staff despite the comparatively poor academic pay. Stratton's assumption that the RLAC needed better relations with the Chemistry Department and trade associations indicated that he better understood his former supervisor, Secretary of Commerce Herbert Hoover, who believed trade associations could serve as organs of industrial self-government, than the MIT faculty. 73

Stratton did not do any better understanding the staff of the DICR, even though he assembled a Visiting Committee for the DICR for the first time early in 1927. The Committee broadly interpreted its responsibility to evaluate "the general subject of research and cooperation with those individuals and organizations throughout our social structure who are dealing in any large way with matters of Applied Science." 74 In eliciting opinions within MIT and asking about the practices of other schools, the committee succeeded in highlighting three conditions. First, the

71 Lewis and Marek, "Research Laboratory."
72 Ibid.
entire faculty, even those inclined to MIT's mission strictly in instructional terms, supported research and extra-curricular consulting as activities that enlivened and updated the faculty's teaching. Second, it was commonplace for faculty members to use sponsored research and consulting to boost their incomes to a satisfying level, even when they would have preferred to pursue self-generated studies. Third, other schools were moving towards the elimination of research of immediate commercial relevance and successfully attracting support for problems of "major importance" from trade associations, engineering societies, research foundations, and/or government bureaus.

The committee also identified several relevant questions to the administration of sponsored research: should the DICR be made a part of the President's office; should such a reorganized Division have the authority to solicit research sponsors and to direct projects to the appropriate departments; how should fees for sponsored research be determined; how should financial recognition be granted to successful researchers; should there be an Institute-wide policy for sponsored research or should each department work out its own policy in cooperation with the Division? But the Committee could not come close to answering its own questions and simply recommended that department heads, the Division's director, and the president jointly address the questions the Committee had raised.

While hardly a prescription for action, the Visiting Committee's report evoked enough bad memories to arouse Charles Norton, Physics Department chairman and DICR director. When Walker resigned amidst faculty resentment towards the Technology Plan, Norton took over the DICR "with a bias against it ... [and]
studiously avoided any interference with earlier connections [between faculty members and corporations]." Norton continued to want no part of regulating sponsored work that did not originate through the DICR and pressed on Stratton the thesis that the less quantifiable benefits to MIT of unregulated, on-campus industrial service—a better, more accessible faculty and good will for MIT in corporate circles—outweighed the costs of foregoing centralized administration.

But Stratton, when he focussed his attention on the issue, did not embrace Norton’s utilitarian calculation but thought primarily in terms of administrative rationality. Stratton scribbled on Norton’s letter:

Whatever may be said for and against this question of outside work, no one familiar with it would hold that it should not be handled in a business like way. The Institute cannot help being an interested party. It should know and administer the work.

Haltingly, Norton came to the conclusion that Stratton wanted to use the DICR for MIT-wide regulation of sponsored research. He drafted a set of rules under which the Division’s approval would be needed before any sponsored work were undertaken, the Division would process and add a 50% to 100% overhead charge to all bills for sponsored services, the Division’s permission would be needed before any sponsored work were submitted for publication, and the faculty would be precluded from devoting more than 20% of their time to sponsored work.

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79 Ibid., Appendix F-II, emphasis added.

80 Norton to Stratton, 26 April 1927, MIT Archives, AC 13, 25/1761.

81 Ibid. Stratton appears to have had troubles in understanding and applying himself to his work. On 12 May 1927, Norton sent Stratton two letters: one, a summary of the 26 April letter as though Stratton had not succeeded in abstracting for himself the original letter’s salient points. The other, a follow-up to a meeting asking Stratton to address six questions, "answers to which in a large part would depend the future work of the Division," as though Stratton had not addressed them during the meeting. While Stratton’s substantive opinions may have induced Norton to hope to gain a reconsideration under guise of providing summaries and requesting clarification, there are other instances of Stratton failing to make timely, pointed responses to the issues of his office.

82 Norton to Stratton, 8 November 1928 and 18 December 1928, in ibid.
Norton limited his editorializing to Stratton to noting that some of his (Norton's) proposals "are manifestly impracticable" and that "it would seem that these changes are proper matters for discussion before the Faculty Council, and perhaps in the Faculty itself before they are carried into effect."\textsuperscript{83} Norton's assistant, Edward Millard, had no compunctions about predicting to Norton that a regulatory DICR would not reward honest and energetic faculty, not raise money for MIT, not eliminate abuses of MIT's equipment and students, nor stimulate stale faculty to greater effort. Instead, energetic and honest faculty would conduct their industrial affairs off-campus, MIT would thereby probably lose as many opportunities to collect overhead as it would hope to gain, abuses would continue since the DICR would be out of position to know what happened in departmental laboratories, and faculty content with a career of rote teaching would not even be discomfited by seeing colleagues using their positions to generate better personal incomes. The end-result would be a Prohibition-like environment—the healthy majority would be inconvenienced by regulations that would fail to stop the abuses of a minority. Instead of vesting new powers and prominence in the DICR, Stratton, in Millard's view, should have been pressing the department heads to stop any abuses of faculty privileges. Millard implored Norton (to implore Stratton) to "begin with registration [of sponsored work] alone and endeavor to adjust the overhead matter by friendly tact in the individual cases requiring it."\textsuperscript{84}

Stratton never followed through on the reform of the DICR. By September 1929, when Millard was writing his letter, Stratton was under pressure to step down as president. He had blamed the RLAC for its problems without pursuing the measures its leaders believed would improve conditions, and he had pressed DICR staff to plan for measures the staff considered unwise. Having failed to build on the legacy that had enabled Maclaurin to build a new campus and deal with Harvard, Stratton had no claims on the loyalty of the MIT Corporation and was vulnerable to

\textsuperscript{83}Norton to Stratton, 13 February 1929, \textit{ibid}.

\textsuperscript{84}Millard to Norton, 4 September 1929, copy in MIT Archives, AC 4, 66/11.
the creation of new sources of power within the faculty. Frank Jewett, the president of Bell Telephone Laboratories and one of the members of MIT's Board of Trustees representing the increasing strength of the Electrical Engineering Department, told a foundation official that September:

Some time ago, S[tratton] was requested to make plans for securing a temporary successor for himself, pending the selection of a new president. Nothing has been done by him, but the Trustees expect to act very soon in the matter. 85

With neither corporate nor faculty support, Stratton's presidency had disintegrated.

Had someone with Stratton's experience in organizing cooperative relations with industry, but in the midst of building rather than capping a career, been tapped as MIT's new president in 1920, could he have made sponsored investigations of industrial practice the institutional foundation for generating research programs at MIT? In retrospect, it is clear that the differential between MIT and industrial salaries caused grief for both the RLAC and the DICR. The lower MIT salaries induced RLAC directors to take industrial research positions and inhibited DICR administrators from acting on any Institute-wide concerns that might disrupt existing means for augmenting faculty incomes. Hypothetically, a more vigorous president could have taken the case for better faculty salaries to corporate sponsors of research at MIT. Such a program, however, would have been a "tough sell" for anyone holding MIT's presidency in the 1920s. When the National Academy of Sciences, with the public sponsorship of Secretary of Commerce Herbert Hoover, tried to tap industrial wealth for a scientific research fund, the drive foundered on the unwillingness of firms in competitive markets to finance endeavors that did not promise a competitive advantage. 86 While the RLAC and the DICR cooperated with firms seeking competitive advantage, regular contributions to boost MIT's faculty

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86Kevles, The Physicists, 148-154, and 185-188. The Academy largely had to settle for establishing national research fellowships which gave recent Ph.D. recipients the opportunity to devote themselves to research under internationally-acclaimed leaders of their fields.
salaries would seem no more in a firm's business interest than open, scientific research; and if a popular Secretary of Commerce could not inspire regular corporate contributions to academic activities that did not coincide with a business' self-interest, nobody in the MIT presidency was likely to enjoy better success.

Conclusion

While it was easy enough for members of the MIT Corporation to see that Stratton was failing to turn Walker's innovations into a tradition of constructive presidential activism, the more profound, perplexing issue was to define a feasible success. Arthur Noyes was the most prominent symbol of a road not taken. His new institutional base—Throop College, provocatively renamed, at his behest, the California Institute of Technology—had attracted productive, internationally recognized scientists to its faculty as George Hale, the director of the nearby, Carnegie-supported Mount Wilson Observatory, made Throop the cornerstone for building "cooperative research programs" around the needs of astrophysics.87 When CIT's officials, in 1924, worried that the capital provided by southern Californians was not generating enough income to pay top salaries for faculty in all the fields they were determined to cover, the officials applied for support from the General Education Board and promptly received $450,000 on condition that the school raise another $900,000 on its own.88 If CIT could tap organized philanthropy for help with retaining and attracting faculty, those interested in MIT had to ask why not MIT?

Individuals in Pasadena were not bashful about claiming that MIT's troubles stemmed from its failure to support research programs on the basis of their challenge to scientific theorizing and that MIT's salvation lay in emulation of CIT. George


Hale told his fellow MIT alumnus Gerard Swope, the president of General Electric,

If Tech [MIT] could turn out results like those of Faraday, it would not only attract graduate students but benefit industry far more than in any other way .... [Robert] Millikan has found it an easy matter to build up here a physics department which draws the best graduate students from all parts of the country and also from some of the leading laboratories of Europe. I wish Tech would do the same.\(^8\)

The crude appeal of emulation would have been enhanced had MIT executives known that CIT's application for Rockefeller support was built partly around Noyes' failed attempt to get a "more cultural physics course" into the MIT curriculum.\(^9\) CIT's "emphasis from the first upon cultural values," according to its proposal, not only enabled it to make an "an important contribution to engineering education but to the whole vexed, world-wide problem of shifting education itself from the linguistic basis bequeathed to us from the Middle Ages to the scientific basis apparently demanded by the world's present and future needs."\(^9\)

Straightforward emulation of CIT, however, was not realistic for MIT. Part of what made CIT appealing to the GEB was based on Noyes' hindsight with respect to developments at MIT and could no longer be created at MIT. Whereas Noyes' drive to create his own fiefdom for chemical research had led to prickly relations with MIT presidents, at CIT he so carefully allied physical chemistry with the interests of astronomers and physicists in atomic theory that he could seem self-effacing with regard to chemistry.\(^2\) Whereas the expansion of MIT's undergraduate enrollments had burdened the science faculty with large numbers of elementary students, cramped the campus, and oriented the administration towards pleasing Eastman, at CIT Noyes

\(^8\)Hale to Swope, 19 October 1925, copy in MIT Archives, AC 13, 19/564.

\(^9\)See Section 2.2, above.

\(^9\)"Application to the General Education Board from the California Institute of Technology," 19 January 1925, Rockefeller Archives, G.E.B. Series 1, Subseries IV, 1103.2, Folder 6476, emphasis in original.

used his influence as an insider to insist that CIT limit its undergraduate classes to 150, even though southern California's need for trained personnel was a major argument in Hale's efforts to raise funds from local sources. CIT's small size was not lost on the GEB, which saw "unusual promise" in CIT's program building, "particularly when these developments are in no way hindered or complicated by problems of additional undergraduate work of collegiate grade."

MIT's Executive Committee, unlike Throop's, did not have the option to remake the school by opening it to the ambitions of a director of a nearby, foundation-supported research institute. And MIT could not shrink its undergraduate enrollments and pretend that it had never been committed to training its students to design and supervise industrial production. While the support of organized philanthropy would have to be tapped for MIT to attract and hold researchers, the substantive programs would have to be grounded on extant MIT strengths and not a mythical tradition. Fortunately, the issue of how next to try to institutionalize research at MIT was not explicitly resolved through assessments of MIT's history and comparisons with the policies of others, but implicitly addressed through the efforts of newly powerful departments and personalities to have their needs satisfied. The electrical engineering department in the 1920s had gained the power to make its strengths the basis for a reform of Institute policy, and the vision of its leaders owed far more to Sedgwick and public health science than to either Noyes and theoretical chemistry or Walker and chemical engineering.

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93 Pauling, "Noyes," 337. See also, Noyes to Hale, 22 September 1928; Noyes to Millikan, 13 September 1928, and Noyes to William Munro, 21 November 1928, all in Hale Papers, microfilm edition, reel 27, for Noyes' views of Caltech's and MIT's development.

CHAPTER 4

FROM DEPARTMENTAL RESEARCH TO INSTITUTE REFORM: ELECTRICAL ENGINEERING, THE REVIVAL OF SEDGWICK'S IDEALS AND THE MIT PRESIDENCY

While Sedgwick's, Noyes', and Walker's ambitions all created demands that they could neither satisfy themselves nor arrange for others to satisfy, Sedgwick-style programs were being organized in the 1920s. Dugald C. Jackson, who entered university teaching without an advanced degree, became chairman of MIT's Electrical Engineering Department in 1907 and set out to develop his department's equivalent of the Sanitary Research Laboratory and Sewage Experiment Station. His efforts and the MIT-wide consequences of his successes are the substance of this chapter.

Like Sedgwick, Jackson believed in constructing local, university-based research communities around poorly understood processes that were embedded in engineering practice. And like Sedgwick, Jackson faced the problem of institutionalizing university research that was not structured along conventional educational lines. But unlike Sedgwick, Jackson, as an engineer, did not initially have the ideological burden of justifying his programs as "science." And unlike Sedgwick, Jackson presided over a department that grew to be the largest constituency within MIT. Thus when Jackson's research-minded proteges felt constrained by the character of collaborators they could draw on from the Physics Department, Jackson and his department's supporters possessed a capability that Sedgwick lacked: they could insist that MIT's president, in the interest of MIT's vitality as an engineering school, be responsible for interesting physicists and their patrons in research that originated in engineering difficulties.
Jackson's successes, mounting, ironically, as the fortunes of MIT's Biology Department declined, fundamentally altered MIT. They created a new locus of power within MIT, opened opportunities for presidential activism in the coordination and funding of multi-departmental research programs, and fueled the MIT Corporation's search for a president with the skills and desire to make such administration his primary professional activity. Instead of a presidency that took no active responsibility for faculty research, a presidency that opportunistically exploited researchers whose ambitions ran parallel to the prevailing patronage winds, or a presidency that was buffeted by conflicts between a president's administrative sensibilities and faculty traditions, Jackson and his supporters demanded and received principled presidential interest in building local research communities around points of mutual concern between scientists and engineers.

The first section of this chapter analyzes Jackson's views on the proper relationship among universities, industry, and researchers and recounts his initial, pre-World War I efforts to build an MIT research program that satisfied his social sensibilities. He was largely unsuccessful because the individual he recruited to provide research leadership, Harold Pender, first was attracted to Walker-style studies of industrial practice and then to the challenge of being chairman of his own department. With Pender disinterested and Jackson himself not possessing the talent for sustained, original research, the institutional achievements of creating an intra-departmental Research Division and receiving regular corporate contributions for research outweighed intellectual achievements.

The second section charts the rise to prominence, following World War I, of two "home-grown" academic engineers, Vannevar Bush and Edward Bowles, who sensed how to use their intellectual and organization talents to produce programs that fit Jackson's desire for locally organized research that would draw scientifically trained people into electrical engineering at MIT. Bush, starting from the mathematical problems of analyzing the behavior of power networks, realized that the computational techniques which would serve the power engineer could also be the basis for a specialty that would create a common interest among mathematicians,
physicists, and electrical engineers. Bowles, starting from the educational need to develop a curriculum in radio engineering, realized that the engineering challenge to create and use directed radio beams would provide a unifying framework within which physicists and engineers could jointly study the generation, propagation, and reception of electromagnetic radiation.

By the late 1920s, both Bush and Bowles were encountering either financial or personnel obstacles to a full blossoming of their ambitions. The last section discusses the pressures that the department’s external supporters brought to bear on Samuel Stratton, Stratton’s efforts to combine reform and fund raising to meet the electrical engineers’ needs, and the external supporters’ recruitment of a new president when Stratton’s efforts came to naught. The new president, Karl Compton, appeared to possess both the inclination to promote physical research that complemented electrical engineering interests and the reputation to make such research appear deserving to philanthropic supporters of education.

**Dugald Jackson and the University’s Role in Engineering Research**

Jackson came to MIT with ideas for relations among research, curricula, and engineering practice. After taking advanced courses in electricity and magnetism (but not an advanced degree) in 1886 and 1887 under the physicist William Anthony at Cornell, Jackson managed lighting and street railways projects in 13 states, before leaving corporate employment in 1891 to chair the University of Wisconsin’s new electrical engineering department and to form his own consulting firm. At Wisconsin he developed a curriculum that included liberal doses of social science in order to give an engineering student the skills to become a "leader, manager, and executive" in private industry, as opposed to a technical specialist whose duties would be

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determined by those who controlled the budget and managed the work force. In 1903 his innovations drew the ire of Charles Steinmetz, the German-born and -trained mathematical engineer, who had enjoyed wonderful successes formulating mathematical expressions to guide the design of alternating-current machinery from his position in General Electric’s Engineering Division, and who was concerned that industry-bound university graduates were not prepared to turn his successes into an enduring tradition.

After moving to MIT in 1907, Jackson continued to refine a curriculum that would prepare students for managerial positions. However, he concurrently worked to find, hold, and promote research engineers who would turn MIT into a local center of technological innovation. His dispute with Steinmetz was not over the relative importance of technical creativity and business acumen for engineers, but rather over the proper institutional setting for technical creativity. Jackson was determined to create academic careers in which technically creative engineers could investigate technological possibilities in partnership with academic scientists.

In 1909, Jackson had his first opportunity to make the kind of major appointment that would set the tone for his administration. His choice was Harold Pender, whose career had been a more scientifically intense version of Jackson’s.

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5This was evident in the effort Jackson invested in creating a cooperative course between MIT and the General Electric Co. See Carlson, "Academic Entrepreneurship," 547-565; McMahon, Making of Profession, 72-77.

Pender, in 1901, had completed a doctorate in physics at Johns Hopkins under Henry Rowland, and then worked as an industrial engineer and consultant. During the summer before Pender’s arrival, Jackson solicited Pender’s input for “a brief setting forth the objects and advantages and appurtenance of research carried on as part of the work of the electrical engineering department … to present to several gentlemen the color of whose money we would like to see supporting the expenses of such research assistance and apparatus.”

Pender’s suggestions were mostly inarticulate and inappropriate. However, one—"study of the design of high tension insulators; also experimental investigation of the mechanical and electrical properties of porcelain, glass, and other insulating materials"—intersected with Jackson’s sensibilities and his reading of the lay of MIT’s land. In his brief, "Advanced Instruction and Research in the Electrical Engineering Sciences at the Massachusetts Institute of Technology," Jackson divided engineering research into "problems closely related to the commercial affairs of the day" and "advanced research in scientific directions which the industries may be approaching but have not yet reached." Noting that MIT’s chemists had split these

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3Rowland was one of the few American physicists with an international reputation, and he had become one of the United States’ leading advocates for the elevation of scientific over industrial pursuits. But Rowland’s students frequently attempted to parlay their skills into a career in the electric power or communications industry. See McMahon, Making of a Profession, 2-6, 49-51; Kevles, The Physicists, 25-27, 38-44.

8Wildes and Lindgren, Century, 46-47.

9Jackson to Pender, 13 July 1909, MIT Archives, MC5, 5/351.

10Pender to Jackson, 23 July 1909, ibid. Pender did not provide Jackson with an essay on what needed research and why MIT’s department should do the research, but a list of “general problems.” Occasionally a “general problem” was nothing more than the name of a device with known uses, such as “the mechanical and open arc rectifiers for changing alternating current to direct;” more frequently, a “general problem” was the design of an electrical machine or system for defined purposes, such as “speed control of three-phase and single-phase induction motors with reference to the use of such motors for traction purposes.”

11MIT Archives, MC5, 5/351 and AC13, 11/324. Jackson probably sent this brief to President Maclaurin -- thus its appearance in the AC13 collection -- because he argued the importance of research and advanced education to MIT’s future reputation as other schools obtained the staff and equipment for high-quality undergraduate education.
branches into independent research laboratories of physical and applied chemistry, while Sedgwick’s Sanitary Research Laboratory and Sewage Experiment Station was "exercising activities of both these branches in connection with sanitary matters." Jackson resolved that "The electrical engineering department ought to likewise carry on activities in both of these categories." The Sanitary Research Laboratory’s successful prosecution of both branches rested on Sedgwick’s ability to discern under-researched biological processes in water filtration systems and in epidemics. Pender’s recommendation to experiment with insulators, while likely born of the limitations that insulators placed on the service-ranges of power stations, was expressed in Jackson’s brief as a challenge to experimental skill and theoretical understanding:

One of the most important researches which can be carried out...is a comprehensive study of the characteristics of insulating materials, the causes of the differences between insulating and conducting materials, and the effects of high voltages upon insulating materials. This problem...can only yield to extended experimental research planned and carried on with a high-minded view of the advancement of knowledge....This is a subject in which both Professor Pender and myself are particularly interested...and seems to us to hold forth the promise of being one of the greatest subjects of experimental investigation which exist in the electrical sciences.\(^\text{12}\)

In bringing Pender to MIT, Jackson hoped he had found someone who, in Sedgwick’s style, would exploit the areas where issues of physical theory and measurement were embedded in the industrial use of electromagnetism. However, raising funds and holding Pender’s enthusiasm for this research proved more than Jackson could manage. He considered the research to be "the kind that ought to be supported by privately contributed funds and the results should then become public property."\(^\text{13}\) To that end, he first sought entry, through his midwest contacts, to Samuel Insull, Edison’s former personal secretary who in 1892 left a comfortable vice-presidency at General Electric to make an even grander fortune by turning the fledgling Chicago Edison Company into the city’s mass provider of an all-embracing

\(^\text{12}\)Ibid.

\(^\text{13}\)Ibid.
system of electric light and power.\textsuperscript{14} Such a man, Jackson reasoned, "might become greatly interested in the support of scientific research," especially when that research held prospects of "pointing the way to enlarge the radii which may be served by great concentrated central stations." and when that research would be "under the direction of a man like Dr. Pender, who is not only an able engineer, but also is notable among men of science."\textsuperscript{15}

Jackson's plea fell on deaf ears, and Pender resorted to using his extracurricular time to teaching extension courses at the Lowell Institute for Industrial Foremen.\textsuperscript{16} When the department did attract some research funds in the spring of 1911, it was "under circumstances similar to those which are carried on by the Research Laboratory of Applied Chemistry;" the Edison Electric Co. of Boston commissioned a study of the relative costs and advantages of electric, gasoline, and horse-drawn vehicles.\textsuperscript{17} Though a far cry from Jackson's ambitions, Jackson recommended to Maclaurin that Pender should direct the research because "Professor Pender came to the Institute with the understanding that he should be given every opportunity to develop a research career, and he is interested in this research and would like to take up its prosecution." Jackson would continue his own efforts to raise more research funds, "particularly funds for pure research in electrical engineering which at present are not immediately associated with commercial problems."\textsuperscript{18}

\textsuperscript{14}Thomas Hughes, \textit{Networks of Power}, (Baltimore: Johns Hopkins University Press, 1983), 202-204.

\textsuperscript{15}Jackson to Louis Ferguson, 15 February 1910, MIT Archives, AC13, 11/324. Jackson also noted that MIT alumni and east-coast philanthropists would be solicited to contribute to the improvement of MIT's site, while Insull was unlikely to be approached for those purposes.

\textsuperscript{16}Jackson to Maclaurin, 30 November 1911, MIT Archives, AC13, 13/393.

\textsuperscript{17}Jackson to Maclaurin, 28 March 1911; "Memorandum of Proposed Procedures in Investigation of Electric Vehicles for Service in Boston and Its Vicinity." 23 March 1911, both in MIT Archives, MC5, 4/281.

\textsuperscript{18}Jackson to Maclaurin, 7 April 1911, MIT Archives, MC5, 4/281.
Once Maclaurin had secured funding for MIT's new site and buildings, Jackson pressed MIT's administrators and patrons to finance "pure research in electrical engineering." Progress was indirect and slow. In the spring of 1912, he sent a version of his Brief to the Department's Visiting Committee in the hopes of finding a route to a research endowment.\(^{19}\) That tactic yielded no money but did garner the rhetorical and organizational support to form an Electrical Engineering Research Division, to which professors could belong and from which they could draw extra salary for directing the work of research assistants.\(^{20}\) Jackson never obtained a research endowment, but with the Research Division's existence as evidence of intra-MIT support for faculty research, he was able to convince John J. Carty, chief engineer at American Telephone and Telegraph, and Charles Coffin, president of General Electric to commit to contributing $10,000 per year for five years for electrical research, pending submission of acceptable research plans. Pender and Jackson put together a prospectus describing the structure and operation of the Research Division and suggesting three projects, two with specific industrial relevance and the third being "a study of the nature of electric puncture in insulation."\(^{21}\)

Unfortunately for Jackson, Pender had become thoroughly engrossed in investigations of electrical vehicles and their utility. As early as the spring of 1912, after Pender had spent one year directing cost comparisons of electric-, horse-, and gasoline-powered vehicles, Jackson recognized that "Personally, I am particularly interested in the [proposed] endowed research while Pender is particularly interested in the industrial research."\(^{22}\) A taste for administration was added to Pender's

\(^{19}\)Jackson to Edwin Webster, 23 March 1912, MIT Archives, MC5, 5/404.

\(^{20}\)Edwin Webster and Lucius Tuttle to the MIT Corporation, 4 June 1912, MIT Archives, AC13, 5/128. An Electrical Engineering Research Division was, within a year, authorized by the MIT Corporation. For evidence of the Division's existence, see Pender to John J. Carty, 18 June 1913, MIT Archives, MC5, 4/283.

\(^{21}\)For Coffin, see Jackson to Edwin Webster, 23 March 1912, MIT Archives, MC5, 5/404; for Carty, see Maclaurin to Jackson, 16 May 1913, and Pender to Carty, 18 June 1913, both in MIT Archives, MC5, 4/283; a draft of Pender to Carty with Jackson's emendations is in MC5, 2/97.

\(^{22}\)Ibid.
interest in industrial practice when, during Jackson’s leave-of-absence in the academic year 1912-1913, he easily pulled together funding for additional studies of railroad operations and streetcar fares. He left MIT in 1914 to chair the University of Pennsylvania’s Electrical Engineering Department.

With Pender building his research career in William Walker’s style, Jackson could not hold to his vision of combining research into matter’s electrical properties with the transmission of high voltages to create his department’s version of the Sanitary Research Laboratory. He was not himself a creative researcher who could offer physical insights to guide students’ research, and he must have recognized that while he had been spinning his wheels, one of his classmates at Cornell in the 1880s, Harris Ryan, had obtained a high-voltage research laboratory at Stanford and was well on his way to a useful understanding of the more troubling effects bedeviling the insulation of high-voltage transmission lines. By the spring of 1914, the third project in the Electrical Engineering Research Division was no longer an investigation of electric punctures in dielectrics but the construction of an artificial transmission line to simulate the transient effects produced by short circuits and thrown switches.

Bush, Bowles, and the Emergence of "Home-grown" Local Research at MIT

Pender’s interest in industrial practice at MIT and his departure for Pennsylvania did not end Jackson’s hopes for building a Sedgwick-styled research community around poorly understood aspects of electrical technologies. But Pender’s departure did lead Jackson to abandon the tactic of recruiting a scientifically trained research director who shared his ambitions for building an intra-MIT research

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23Pender to Jackson, 22 October 1912, MIT Archives, MC5, 5/351.


25Hughes, *Networks*, 58-60. In Hughes’ estimation, Ryan’s work was the most important academic contribution to the spread of alternating-current power systems.

26Jackson to Webster, 1 May 1914, MIT Archives, MC5, 5/404.
community. He allowed an institutional windfall to take care of Pender’s departure. The agreement between Harvard and MIT over the administration of the Gordon McKay bequest brought Jackson the services of Harvard’s Arthur Kennelly, whom Jackson placed in charge of the Research Division. Kennelly had made a major impact on electrical engineering by adapting the algebra and calculus of imaginary exponentials to the analysis of alternating-current circuits. This work made the analysis of steady-state alternating-current circuits nearly as straightforward as that of direct-current circuits.\(^{27}\) He had also earned a reputation in physical circles by attributing the long-distance propagation of radio waves to the refractive powers of an atmospheric belt of charged particles.\(^{28}\)

Kennelly possessed no ambition to build MIT into a center for creative research. In his view, technical institutes, whose intellectual mission Kennelly deemed "narrower" than the university’s, should focus on industrial research projects that could prepare their students for industrial careers, and the American Institute of Electrical Engineers was the proper forum for identifying scientific projects that universities would carry out.\(^ {29}\) However, Kennelly’s contentment with MIT and the


opportunities it afforded was overshadowed by his impact on two MIT graduate students. Kennelly's work in alternating current analysis and radio wave propagation suggested to Vannevar Bush and Edward Bowles that the most scientifically intriguing problems in power transmission lay in finding the mathematical techniques for solving the differential equations that governed power networks and that the most technologically significant physical processes were the generation and propagation of electromagnetic waves. With Jackson willing to achieve his goals on the backs of "home-grown" talent with independent ideas on the best tactics for achieving those goals, these topics, rather than the physical properties of insulators, became central to the development of Sedgwick-style local research communities.

Both Bush and Bowles, in the 1920s, found that satisfaction of their research ambitions required a reworking of relations with other departments at MIT, and especially the physics department. Bush took up the search for mathematical techniques that would enable engineers to anticipate and cope with the complexities of power transmission; eventually, the means became a potential end as Bush recognized that the computational prowess developed for power transmission analysis could itself be a specialty that would boost the importance of electrical engineering at MIT to the academic community. Bowles took up the use of radio waves as a form of communication; for him the end became a potential means as he realized that the components of radio, when understood as objects of physical analysis, could be adapted for other uses than broadcasting. Taking advantage of the opportunities exposed by Bush and Bowles presented administrative challenges that effectively led to a reconsideration of the role and qualifications for MIT's presidency.

Bush entered MIT as a graduate student in 1915. He was so intent on completing his doctorate in one year\(^3\)\(^0\) that there was no opportunity for the faculty to recruit him to carry out any externally supported research on a part of industrial practice. Instead Bush wrote a dissertation that extended Kennelly's approach to the

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teaching of circuit analysis. Kennelly had laid out the use of imaginary exponentials \( e^{\alpha t} \) for analyzing steady-state alternating current. Bush attacked "oscillating currents"—currents that diminished in amplitude in the absence of an external force—that power and radio engineers encountered in a variety of circumstances. He found that by using a complex exponential, \( e^{\alpha + i\Omega t} \), instead of a purely imaginary one, he could carry through an analysis; he dubbed this technique "the method of generalized angular velocities" because his use of a complex exponential was a generalization on Kennelly's imaginary one.\(^{31}\)

Bush received a mixed response when he presented his work to the American Institute of Electrical Engineers. Some criticized Bush's approach as a short-cut that could promote an inadequate understanding of differentiation and integration and thus lead engineers to dangerously shoddy mathematical practices.\(^{32}\) But at least one reader regretted that Bush did not further elaborate on his approach in order to simplify the mathematical analysis of larger classes of transmission phenomena.\(^{33}\) Those responses were sufficient challenge and encouragement for Bush to take up the mathematics of power transmission when he returned to MIT, after World War I, as associate professor of power transmission.

The intellectual and business frontiers of power engineering neatly coincided in the 1920s. Intellectually, alternating-current systems were analyzable so long as the system was in a steady state; however, transients (i.e., traveling waves induced by sudden changes in the input or demand on a transmission line), while qualitatively understood, were quantitatively unpredictable and pedagogically indigestible.\(^{34}\) In 

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\(^{32}\)See comments of C. Fortescue and Thornton Fry in ibid., 222 and 223, respectively.

\(^{33}\)See comments of A. Press, ibid., 224-236.

\(^{34}\)For example, Arthur E. Kennelly, in Artificial Electrical Lines: Their Theory, Mode of Construction, and Uses, (New York: McGraw Hill, 1917), 1, stated: "The behavior investigated [on an artificial electric line] may relate either to the steady state of electric flow over the line or to the
business, utility executives were seeking to offer regional services by consolidating utility companies and "networking" together transmission lines and generators; however, networks were more susceptible to transients and the damage they could do because of branches coming on and off line in response to demand. Soon Bush had fostered a three-prong program to bring the analysis of transients within the purview of electrical engineering's teachable skills. Much of the work was multi-departmental from inception and parts became difficult to pursue in the absence of better external funding and internal administration than the Electrical Engineering Department could itself provide.

The first prong of Bush's program was to develop operational calculus to cope with the mathematical difficulties of solving the equations governing circuits when transient conditions were assumed. Knowing that the rigor of operational methods would be scrutinized, Bush may well have been on the "look-out" for mathematical help among his peers in MIT's Mathematics Department. However, the assistant professor of mathematics who also began teaching at MIT in 1919, Norbert Wiener, turned out to be a collaborator of genius, not just a helpful consultant. By the end of the 1920s, Bush was sufficiently confident not only to publish a textbook advocating operational analysis of circuitry, but to introduce it by first conceding the validity of the criticisms leveled ten years earlier:

It is entirely possible to utilize the operational method on specific problems without the slightest idea of why and when it does or does not work....After all, a little mathematical knowledge can be a dangerous thing, and the use of operational methods except for pure computation should be accompanied by an appreciation of the logic of more than simple algebra.

transient states of electric flow, during disturbances. Since, however, the steady state is much the easier to examine, and is much more thoroughly understood in the present state of engineering knowledge, we shall confine ourselves, in the main, to the study of the steady states of artificial and real lines." The oscillating currents of Bush's doctoral thesis were one type of transient.

Hughes, Networks, 324-362.

Wiener benefitted from Bush because he found mathematical stimulus in engineering (and a variety of other fields) and preferred to skirt research areas that would bring him into competition with other mathematicians. Steve J. Heims, John von Neumann and Norbert Wiener: From Mathematics to the Technologies of Life and Death, (Cambridge: MIT Press, 1980), 20, 23-24.
And then claiming to have met them:

It is necessary to have at least some grasp of the classic mathematics for which it [operational analysis] is the working tool. This means primarily a grasp of Fourier analysis and of its treatment in terms of the complex variable... I have therefore included a brief survey of certain essential parts of the classic treatment ... to show their [operational analysis' features] dependence upon the classic process which constitute their background.\textsuperscript{37}

To bolster that claim Bush included an appendix by Wiener outlining the strategy by which one could prove that Fourier analysis of large classes of functions yields expressions whose differentials and integrals were legitimately replaced by operators.\textsuperscript{38}

The second prong of Bush's efforts in the 1920s was to develop "smooth" artificial transmission lines—lines whose electromagnetic parameters were continuously distributed along their lengths—on which to test calculations of transient phenomena.\textsuperscript{39} In 1920, MIT's Electrical Engineering Department dedicated a string of research assistants to overcoming the material obstacles to smooth lines. At the 1923 annual meeting of the American Institute of Electrical Engineering, MIT's researchers reported success,\textsuperscript{40} and learned that General Electric had been combining more conventional artificial lines into artificial networks that successfully simulated the current division of real networks for normal conditions and for certain kinds of


\textsuperscript{38}Norbert Wiener, "Fourier Analysis and Asymptotic Series," in \textit{ibid.}, 366-379.

\textsuperscript{39} "Lumpy" artificial lines, composed of discrete "lumps" of capacitance, inductance, etc. had been used to guide designers of all sorts of electromagnetic systems since the 1860s, but lumpy lines distorted traveling waves as the waves passed over the spatially distinct parameters. Kennelly, \textit{Artificial}, v-vi and 1-5.

\textsuperscript{40}F. S. Dellenbaugh, "Artificial Transmission Lines with Distributed Constants," \textit{Transactions of the American Institute of Electrical Engineers}, 42 (19-3), 803-819; Bush, "Transmission Line Transients," \textit{ibid.}, 878-891. The two backbones of MIT's smooth line were a variation of a coil that Michael Pupin, an electrical engineer at Columbia, had designed for use in a smooth line, and the use of vacuum tube amplifiers in the circuits leading to the oscillographs and meters. The vacuum tubes lowered the power level at which measurements could be made while the coil's improved design and materials raised the level of power that the coil could reliably handle.
short circuits. The two groups set aside their urges to doubt each other's accomplishments in favor of the technical allure of creating an artificial network with smooth lines and the social allure of building industry-university cooperation. One Bush student, Harold Hazen, divided his time between MIT and GE and ended up, in 1925-1926, supervising construction of the joint MIT-G.E. "network analyzer," which could simulate actual or contemplated power systems. When Yuk Wing Lee, another Bush student, began speculating on whether determining the characteristics of electrical networks through "analysis" had a "synthetic" counterpart in which mathematical logic would guide the construction of networks with specified characteristics, Wiener again proved instrumental by advising Lee's doctoral dissertation, "Synthesis of Electrical Networks by Means of the Fourier Transforms of Laguerre's Functions."

The third prong of Bush's research concerned the evaluation of the solutions to circuit equations. Operational calculus may have shortened the mathematical distance between the initial equations and solutions that expressed current or voltage as functions of space and time, but once those solutions were in hand, the operators had to be replaced by their calculus counterparts and the resulting integrals evaluated for the appropriate boundary conditions. When formal means failed—the need to integrate the product of two functions was particularly common and vexing—the

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42 At the conference, GE and MIT researchers did tussle over the reliability of MIT's vacuum-tube measurement circuits. See *ibid.*, 820-821 and 839-840.


44 Wildes and Lindgren, *Century*, 157; Heims, *Wiener and von Neumann*, 173. Lee's work found less application in power than communications engineering, where the desire for selective transmission of frequencies was long-standing.
functions had to be plotted graphically, the area under the resulting curve measured, and the result returned to the original equation for further graphical analysis. 45

By early 1925, Bush was urging graduate students to devise machines that would ease the burden of calculation. His delight with incremental progress46 turned to explosive creativity when he realized that Harold Hazen, by using an electrical and a mechanical integrator in series to calculate solutions to certain second-order differential equations that governed the vacuum-tube circuits in the network analyzer, had in hand the rudiments of a general-purpose machine.47 Bush knew full well that "[classical] physics revolved about the second-order differential equation,"48 and Wiener, who had worked briefly and successfully towards formalizing quantum physics,49 could well have told Bush that atomic physicists were courting new computational difficulties with Schrödinger's second-order partial differential equations. A machine that could be adapted for several first- and second-order equations would not only be logically intriguing and useful to electrical engineers, but could potentially expand the relevance of electrical engineering to a host of other fields. However, the ability cf Bush to achieve that potential at MIT depended not


47The night after seeing Hazen's sketch for the use of a mechanical integrator, Bush produced a 20-page memorandum demonstrating the generality of Hazen's step and outlining a new machine to take advantage of it. For Bush, who learned from Kennelly how the mathematics for analyzing mechanical vibrations could be reinterpreted for a-c circuit analysis, the prospect of a mechanical device for evaluating the solutions to the equations of a-c circuits must have seemed like the natural completion of a grand loop. In a more general sense, the disk integrator and the graphs it produced embodied the mathematical pedagogy common to engineering schools in the early 20th century. See Owens, "Bush," 69 and 85-95; Wildes and Lindgren, Century, 90.


only on improving components but also on the availability of collaborators to guide designs for non-engineering contexts. The latter condition was beyond both Bush’s and Jackson’s direct control.

At the end of the 1920s, Bush’s horizons had overgrown the context in which he had begun the decade. In *Operational Circuit Analysis* he asserted “I write as an engineer, and...I do not pretend to be a mathematician.” That was certainly true insofar as the book’s purpose was not to build a cumulative structure of theorems and proofs. However, Bush was hardly writing as an electrical engineer concerned with power system transients. He defined circuit "as a physical entity in which varying magnitudes can be sufficiently specified in terms of time and a single dimension," and throughout the book, electrical circuits were simply one (albeit prominent) example.\(^{50}\) Bush’s projects all had their origins in the search for teachable methods of coping with transients in power systems, but their mathematical content piqued Wiener’s interests, and in the case of calculating machines, enabled Bush to imagine that electrical engineers would construct a machine that could attract collaborators from a number of fields. Throughout his life Bush designated himself an engineer, not a scientist,\(^{51}\) but he had become a mathematical engineer who appreciated, welcomed, and supported those who explored or extended to other domains the mathematical formalisms he developed in his duties as a teacher of power transmission.

Through a somewhat different course, Edward Bowles reached a position analogous to Bush. Bowles came to MIT in 1920 to do a master’s degree before entering industrial employment. However, Jackson, sensing academic talent, induced Bowles to stay a second year with an appointment as instructor. Bowles’ master’s thesis, supervised by Bush as part of Bush’s responsibility for teaching advanced alternating-current circuits, was an effort to use vacuum tube circuits to measure


\(^{51}\)Kevles, *The Physicists*, 293.
hysteresis loops that limited the efficiency of magnetic circuits.52 But in contrast to Hazen, who was able to use GE's vacuum tubes to amplify the weak signals from smooth artificial transmission lines, Bowles' work required that he design and build his own. His results did not greatly advance the measurement of hysteresis effects, but he became, at a propitious moment, more familiar with the vacuum tube than anyone else at MIT.53

In the years preceding World War I, the vacuum tube's workings had been shown to be more novel and its uses more varied than envisaged by any of the several claimants to its invention. Several competing electrical firms ended up owning the conflicting patent rights that had been granted as the tube's properties were discovered, and only after the war did these firms drop litigation in favor of a cross-licensing agreement to permit the development of a radio industry unfettered by intercorporate legal disputes.54 Thus as Bowles struggled to fashion a tube with the proper characteristics for his experiments, popular interest was booming in radio broadcasts that used tubes as oscillators in transmission and amplifiers in reception.55

Kennelly was already teaching a course (which Bowles had taken and then assisted in) on the Electrical Communication of Intelligence, but Jackson saw Bowles as someone whose ambitions Jackson could harness to develop a locally organized communications program that could equal what Bush was building around power transmission. During Kennelly's leave-of-absence in 1921-1922, Jackson, Bowles, and Bush put together a distinct "Communications Option" that they inserted into the established curriculum.56 Kennelly returned to find himself leading an Option he

52 Wildes and Lindgren, Century, 110.

53 McMahon, Profession, 184-185.


56 McMahon, Profession, 184-185; Wildes and Lindgren, Century, 111-112.
had not shaped. Sensing his waning influence, he resigned from MIT in 1925 to return to full-time teaching at Harvard.

Kennelly's resignation seemed more of an opportunity than a loss to Jackson. He wanted the faculty's research locally organized around points of contact between science and engineering so that the students could readily cultivate a first-hand appreciation for "the methods of research and their importance." Such policies, Jackson explained to Frank Jewett of Bell Telephone Laboratories, "have given us an apparent preeminence in the electric power transmission field as far as the colleges go." Kennelly, in Jackson's estimation, was simply not oriented to Jackson's image of MIT's progressive development:

I am personally of the belief that this [MIT's policy in power transmission] is the best educational process for developing notable men in considerable numbers and I think it may serve better than the policy of individual leadership that Mr. Kennelly and numerous other scientifically trained men adopt for their method. Consequently, much as we regret to have Dr. Kennelly retire, I believe the change of method may result in more notable accomplishment at the Institute on the communication side.  

With Kennelly's departure, Jackson promoted Bowles to head of the Communications Option.

In contrast to his abortive efforts to find a patron for research into dieléctrics, Jackson had a local candidate for patronage of radio research, the railroad magnate Edward H. R. Green. Jackson apparently had Bowles prepare a "Memorandum on Possibilities of Research in Radio Communication at the Massachusetts Institute of Technology" to convince Green of MIT's potential to "produce results which would give eminence to his name." Bowles suggested the study of static as a problem

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57 Jackson to Jewett, 15 April 1925, MIT Archives, AC 13, 11/327.

58 Green had fruitlessly tried to use a radio system for train dispatching, but retained an amateur's interest in radio and established, in 1923, a radio station and laboratory on his estate in S. Dartmouth, Massachusetts. From there, he began intermittently communicating with Jackson, Bush, and President Stratton about research work. See Wildes and Lindgren, Century, 114.

59 Jackson to Stratton, 31 March 1925, MIT Archives, AC 13, 11/324; the memorandum was separated from the cover letter and is in AC 13, 11/327. An author's name does not appear on the memorandum, but the writing style is neither Jackson's nor Bush's, leaving Bowles the likely...
"adapted to investigation in an educational institution" where there resided "mathematicians, physicists, and electrical engineers intensely interested in electrical communication." Such a community, they reasoned, could cover "all promising angles" to static alleviation: the responses of antennae and receiving circuits to mechanical oscillations, multiple-frequency waves, and irregularly modulated signals; the effect of different antenna forms on the reception of static; the use of very short wave lengths for transmission; the effect of transmitter forms and locations on the creation of static; and the effect of atmospheric conditions on the propagation of radio waves.60 They hoped that Green would equip a laboratory at MIT (presumably for research into radio circuitry), complete his laboratory at his estate, Round Hill, (presumably for research into wave propagation and the spatial distribution of fields), and pay for the salaries of research assistants.61

Bowles could just as well have said that research into static required coordinated research into all the components of radio communication. By making the study of static the object of an externally funded, departmental program, MIT's communications researchers would possess a common objective that was sufficiently definite to generate criteria for research priorities yet sufficiently broad to inspire collaborations among various specialists. A faculty that might otherwise be a collection of independent specialists, who would have to collaborate on nothing more than working out a division of the teaching labor, would thus become a research community where individual interests would find their full expression in achieving the common objective.

Green did not jump into the arrangement. He appears to have needed a more distinctive technological objective than the minimization of static, evidence that effective research was possible, and evidence that MIT's curriculum would produce effective researchers. The last two points were addressed as two of Bowles' first

60"Memorandum on Possibilities," ibid.

61Jackson to Stratton, 31 March 1925, AC 13, 11/324.
students, using the modest support Green initially offered, designed apparatus that improved measurements of the parameters of radio-frequence circuits and radio waves. The problem of a distinctive technological objective was inadvertently solved by Charles Lindbergh. Green responded to Lindbergh’s famous 1927 flight by constructing a runway and experimental flying field on his estate. Amidst the foggy weather of the area, the study of aeronautical communication and navigation in fog replaced the alleviation of static as the unifying technological purpose for MIT’s radio engineers.

If Bowles or Jackson felt miffed at having to conform to Green’s predilections, they wisely did not show it. Green’s support lasted until the Depression took its toll on his finances, and his interests were neither disruptive nor meddlesome. The use of directed radio beams to aid aeronautics was a well-recognized engineering challenge, even if it did not have static reduction’s potential for popular impact. And the basics of Bowles’ plans for static were readily adapted to ground-air communication: studies of the techniques for and hindrances to the transmission and reception of radio waves would still be relevant, though the generation of shorter waves and the study of their propagation would take on greater urgency. Bowles’ most profound problems lay within MIT; with Green’s facilities expanding and his purse strings loosening, he needed students with the taste for experimental design or theoretical analysis associated with science education.

From its inception, the Communications Option impressed on its developers the need for coordination with the Physics Department. Bowles and Bush adapted

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63 Wildes and Lindgren, Century, 117-119.

64 See Ross Gunn, “Aircraft Radio and Navigation,” Journal of the Franklin Institute, 205 (1928), 849-864. Since switching to dictionary format in 1920, The Engineering Index made “Direction Finding” and “Radiogoniometry” categories in which to index articles.
circuit theory and electrical machinery laboratories used in teaching power
transmission to explain and illustrate the components of electrical communication,65
but they were not prepared to teach the physical theory (and attendant four-
dimensional mathematics) of electromagnetic fields and radiation.66 In 1923, Bowles
audited the Mathematics Department’s course on vector analysis and judged it suitable
for communications students. But he found nothing comparable in the Physics
Department and resorted to inducing one of the few graduate students in physics,
Manuel Vallarta, to teach a two-term cycle on electromagnetic theory and wave
propagation.67

Vallarta looked like a plausible prospect to cultivate an interest among physics
students in the generation and propagation of radio waves. In 1921 he had performed
experiments on the skin effect in the Electrical Engineering Department’s Research
Division,68 and Bush later drew him to consider the mathematical justification for an
operational approach to Maxwell’s equations.69 But Vallarta was also pursuing
relationships at Harvard, and his choice of dissertation topics—the incorporation of
general relativistic effects between electron and nucleons into Bohr’s model of the
atom—displayed a primary concern for an intellectual integration of the new physical
theories of the time.70 Two years of postgraduate study at German centers of

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65See "Memorandum for Dr. Jewett," attached to Jackson to Jewett, 15 April 1925, MIT Archives,
AC 13, 11/327.

66McMahon, Profession, 124-132 argues that radio engineers formed an independent Institute of
Radio Engineering because radio engineering had a technical and social characteristics that were neither
well represented nor promoted in the power-transmission dominated American Institute of Electrical
Engineering.

67Wildes and Lindgren, Century, 113.

68See Vallarta’s comments on Waldo V. Lyon, “Heat Losses in Stranded Armature Conductors,”
Transactions of the American Institute of Electrical Engineers, 41 (1922), 199-214, 212-213 for
Vallarta’s comments.


70Manuel S. Vallarta, “Sommerfeld’s Theory of Fine Structure from the Standpoint of General
Relativity,” Journal of Mathematics and Physics, 4 (1925), 65-83. Sommerfeld had used the presumed
special-relativistic variations of the orbiting electron’s mass to explain the fine structure of the hydrogen
theoretical physics strengthened Vallarta’s commitment to seeking coherence among physical theories. After returning to MIT his work on electromagnetic field theory aimed to help physicists find unifying connections between that theory and relativistic\textsuperscript{71} and quantum mechanics,\textsuperscript{72} not to help engineers fathom radio waves.

Even had Vallarta continued to work within a framework relevant to radio engineers, the fact that a graduate student had to be recruited to cover an important course for radio engineers was itself unacceptable to those who wanted MIT to provide advance training in communications. In 1923, the Electrical Engineering Department’s Visiting Committee (with Frank Jewett contributing as an advisor) recommended:

Electrical Engineering is based upon physics and may undergo sudden and radical changes with advances in our knowledge of physical phenomena. (As an illustration of this, consider the field of electronics and the important role which the vacuum tube has suddenly come to play in electrical engineering). It therefore seems highly desirable that there should be available for graduate students in electrical engineering advanced courses in physics. It would also be helpful if such advanced students could come in contact with research work of a fundamental character in physics. It would seem to the Committee that these ends could best be accomplished first, by adding some enthusiastic research workers to the teaching staff of the physics department and second, by the closest possible cooperation between the electrical engineering and physics department.\textsuperscript{73}

While this recommendation was only one of eleven, it was prominently placed, the


\textsuperscript{72}Manuel Vallarta, "Note on the Statistical Interpretation of Maxwell's Equations." Journal of Mathematics and Physics, 8 (1929), 155-161.

\textsuperscript{73}Report of the Visiting Committee of the Department of Electrical Engineering, 6 June 1923, MIT Archives, AC 13, 5/128.
Committee singled it out as a high-priority item in its 1924 report, and the Committee in 1926 met jointly with the Physics Department’s Visiting Committee with the joint report "respectfully urg[ing] that greater emphasis be put upon securing men of high attainments in the Department of Physics for advanced work in pure science."  

When Stratton failed to produce quick change (for reasons discussed in the next section), Bowles and Jackson worked around rather than through the Physics Department. They helped their students with research talents to pursue doctoral studies at a Germanic technische Hochschule, not a classical university, and then wooed them back to MIT. They succeeded with Julius Stratton, who attended Zurich’s Eidgenössische Technische Hochschule in 1927, and Wilmer Barrow, who attended the Munich Technische Hochschule in 1929. Both became mainstays of the radio research program.

Though Bowles could not match Bush in technical creativity—a fact that Bowles retrospectively admitted and that is "anti-documented" in Bowles’ absence from the bylines of research papers—by 1930, Bowles’ horizons, like Bush’s, had overgrown the context of his initial departmental responsibilities. As Bush began by adapting mathematical techniques to suit the needs of power engineers who would be facing transients in power networks, Bowles began by adapting circuit theory to the needs of radio engineers who would need to be more familiar with vacuum tubes and higher frequency phenomena. And as Bush realized the computational prowess he was developing could attract the attention of researchers in multiple disciplines, so Bowles, in order to take advantage of the opportunity to develop techniques for

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75 The Visiting Committees of the Departments of Electrical Engineering and Physics..., 9 March 1926, MIT Archives, AC 13, 5/129.

76 Wildes and Lindgren, Century, 110; between 1928, when it first included an author index, and 1939, the Engineering Index lists only one paper by Bowles, a guide to buying amplifiers in Popular Radio.
guiding airplanes through fog, found he needed to administer a research community that included individuals trained in specialties that lay beyond MIT's electrical engineering curriculum. Bowles retained the title of communications engineer and sometimes expressed a (perhaps too) zealous pride in the jurisdictional responsibility and intellectual initiative of the Electrical Engineering Department for the Round Hill research, but he had become a research administrator who selectively welded parts of other disciplines to research aimed ultimately at ground-to-air communication through fog.

At the level of personal self-interest, Bush and Bowles were building their careers by developing expertise in areas of burgeoning industrial interest. At the departmental level, they were exploiting the potential implicit in Arthur Kennelly's work to organize local research communities in a fashion that led to advancement in Dugald Jackson's administration. But at the level of MIT-wide history, they were reinventing, in an engineering department, William Sedgwick's research policy. Power networks and directed radio communication served Bush and Bowles as sewage filtration systems and epidemics served Sedgwick. They were junctures at which, following Sedgwick's metaphor for the creation of scientific specialties, streams of knowledge could be combined into a river that could make a forceful contribution to the ocean of accumulated human knowledge. And what made them junctures were their simultaneous value as challenges to intellectual creativity and as material for advancing technological capabilities.

Bowles and Bush probably never knew that Jackson strove to emulate Sedgwick, who died before they were prominent within MIT. And as engineers, they were spared Sedgwick's burden of arguing the scientific character of striving to merge parts of several fields rather than sub-divide a single discipline. But their successes within MIT raised the same question on which public health science ultimately foundered: could MIT attract the personnel and external patronage that would turn

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"Edward Bowles. "Science at Round Hill." Technology Review. 37 (1934-1935) 18-42 contains only one paragraph at the end of the article on the Physics Department projects that were entrusted to Round Hill and Bowles' administration."
these local innovations in the organization of knowledge into nationally recognized, firmly institutionalized traditions of teaching and research? Personnel was not a grave problem for Bush; in Norbert Wiener, he had a first-rate collaborator who shared ideas, advised students, and possessed wide-ranging intellectual contacts. But Bush had no immediate access to funds for a research community that could expand from its historical foundations in the analysis of transients in power networks. Bowles had the mirror image problem. He enjoyed an enthusiastic patron in Edward Green but was developing personnel by working around rather than through other MIT departments. Both men's problems were elevated to presidential responsibility on the strength of the Electrical Engineering Department's power within MIT.

**Electrical Engineering's Patrons and the Drive for Institute-wide Reform**

Bush and Bowles achieved their broadened horizons with little help from an MIT administrator higher than Jackson, and in the last half of the 1920s, it was not clear whether their ambitions would become sources of institutional development or personal frustration. In 1923, Jackson enthusiastically explained the prominence of his staff's contributions to an AIEE meeting by citing his department's support for engineering research that was educational rather than industrial, yet technological rather than physical:

> What I want to emphasize is this: that while the sort of work described by Dr. Bush and Professor Dellenbaugh in their papers...is directly serviceable to the art, the fact is that that work...was actually planned for educational purposes and was carried out for those purposes: that the philosophy of the thing is to bring the senior students and graduate students...into contact with the most advanced philosophy of engineering structures that we have....That is not what the physicist would call fundamental research, but it is definite and effective engineering research.\(^7^8\)

But in the last half of the decade, fulfilling Bush's and Bowles' ambitions came to depend on acquiring philanthropic research funds and invigorating the physics

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\(^7^8\) *Transactions of the American Institute of Electrical Engineers*, 42 (1923), 821.
department. The distinction between academic engineering and "what the physicist would call fundamental research" lost its utility.

Since the advent of the Communications Option in 1922, the Electrical Engineering Department had been lobbying the MIT Corporation through its Visiting Committee for reform of the Physics Department. In 1926, Bush's first calculating machine inspired similar lobbying aimed at increasing the space and money for his programs. To a greater extent than in 1912, when Jackson and Pender used the Visiting Committee to press for research support, the request came laden with a promise of responsibility and opportunity:

With the increasing interest of power engineers in line surges it is imperative that this study [of traveling waves on power transmission lines] be extended. While there are several distinguished European engineers who have contributed to this problem, there is no other center in this country where a theoretical study of this matter is being made and checked by experiment. 79

The initial monetary request, $15,000 per year for three years with only $6,000 for "a computing machine of a radically new sort," was quickly superseded as Bush, two years later, had plans for a machine with six disc integrators at an estimated price of $25,000. 80

With the character of the Physics Department and the quantity of the research budget both in doubt, the attention of the Electrical Engineering Department's patrons was pulled to the role and quality of MIT's president, Samuel Stratton. Stratton could not have ignored this scrutiny, even if he had wanted to, for the department had plainly become the most powerful constituency within MIT. Over 20% of MIT's students between academic years 1920-21 and 1928-29 were registered in the Electrical Engineering Department, making it the most popular on campus, and the

79Bush, "Memorandum on Research Program," 4 October 1926, MIT Archives, AC 13, 3/68, my emphasis. Research dealing with transients dominated the memorandum. In addition to the integrating machine, the department requested support for a laboratory to study transients in machinery and the construction of the network analyzer (at this time known as an a.c. computing table). The only project not dealing with transients was the least expensive.

department had 28% of the graduate students between the years 1926-27 and 1928-29.  
81 When positions opened up on the Executive Committee of MIT's Corporation in 1919 and 1926, both times a member of the Electrical Engineering Department's Visiting Committee was appointed: Charles Main in 1919 and Gerard Swope, the president of General Electric, in 1926.

Swope was especially active and demanding. He had GE contribute $15,000 annually to MIT to hire additional instructors in Physics and Electrical Engineering so that faculty could devote more time to research.  
82 Through Willis Whitney, Arthur Noyes' former student and the director of the General Electric Research Laboratory, Swope learned of and kept Stratton informed about the opinions of MIT alumni at the California Institute of Technology.  
83 Stratton had no choice but to try to reform the Physics Department and raise funds for research from the Rockefeller charities, which had become the outstanding source of large gifts for education.

Stratton faced formidable obstacles to any reform, appropriate or inappropriate, that would promote research within the Physics Department. The leadership of the Physics Department had not been touched by the changed expectations concerning faculty rights and responsibilities for doing research. Charles R. Cross became chairman in 1877 and held the post for the next 40 years. His energies went to elementary teaching and industrial consulting.  
84 Neither research

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81 Figures computed from MIT President's Report for 1929, 84-85.

82 Swope to Stratton, 16 May 1925; see also William Wickenden to Swope, 10 May 1926, both in MIT Archives, AC 13, 19/564.

83 (George E. Hale?) to Swope, 19 October 1925, copy annotated by Stratton in ibid. Stratton to Swope, 31 December 1926 expresses amazement at report that CIT's founders did not consider MIT a worthy object of comparison in their assessment of CIT's development.

84 On Cross' contributions, see Tenney L. Davis and Harry M. Goodwin, A History of the Departments of Chemistry and Physics at MIT, 1865-1933, (Cambridge: MIT Press, 1933); Wildes and Lindgren, Century, 21; and Wise, Whitney, 26-27.

85 After a string of papers he co-authored with students in 1893, Cross published nothing more, even though, the establishment of the Physical Review that year increased publishing opportunities. See Cross and Harry Goodwin, "Some Considerations regarding Helmholtz's Theory of Consonance," Proceedings of the American Academy of Arts and Sciences, 27 (1893). 1-12; Cross and Margaret
nor departmental expansion attracted him. Cross' staff, burdened with the duty of teaching elementary physics to all the engineering students, had little time to pursue research except as a byproduct of these teaching responsibilities. One of Cross' chief assistants, Silas Holman, who was known as a "research type," found time for extended reflections on physical theory only when illness prevented him from teaching.

When Cross retired in 1917, Richard Maclaurin, did not seize the opportunity to import new leadership, despite no lack of advice urging him to take that course. Frank Jewett warned Maclaurin not to promote Cross' protege, Charles Norton, who had been teaching physics at MIT ever since he had received his B.S. from that department in 1893, to department head. Jewett argued that the position should be filled by "a broad gauge physicist with a thorough appreciation of the necessity of making clear... the principles of physical science without which knowledge [students] will become not engineers but mere artisans in the engineering field." He went on to warn that he was finding university-trained physicists better for "high-grade" engineering work because "the more thorough training [students] receive in


Through 1910, only 54 students graduated with degrees in physics, "owing to its [physics'] non-professional character," although virtually all students took a physics course as part of an engineering curriculum. Charles R. Cross, "Department of Physics," undated memorandum but apparently Cross' contribution to Maclaurin's circulaires of 1 and 25 November 1910, MIT Archives, AC 13, 13/393; cf. Jackson to Maclaurin, 30 November 1910, in same.

Wildes and Lindgren, Century, 25. Holman's lecture notes, Discussion of the Precision of Measurement, were privately published in 1888, then expanded into a full-scale textbook (New York: John Wiley, 1892) which was reprinted in 1894, and revised in 1897 for a second edition which was also reprinted (posthumously) in 1904.

fundamental science and in the *methods* of research renders them better able to cope with problems of their later engineering life." Arthur Noyes stressed to Maclaurin "it is to be borne in mind that only a man with the research point of view can develop educational methods adapted to a higher institution." He urged that Maclaurin not interpret the department head's job as concerned with administration and teaching as opposed to research, and he offered his own and Holman's frustrations as examples of how faculty with research ambitions do not fulfill their promise under unappreciative administrators.  

Maclaurin neither promoted Norton nor recruited an outsider with a reputation for promoting physical research. Instead, he moved the statistician Edwin B. Wilson over from the Mathematics Department. Wilson was well versed in the uses of statistics in physical theories relating atomic behavior to macroscopic phenomena, but he remained primarily concerned with finding new realms that statistical thinking could illuminate. He left MIT in 1922 to take the first professorship of vital statistics at the Harvard School of Public Health. As MIT then had no effective president, Norton, by default, became chairman.  

Norton proved to be a stodgy, unaggressive chairman. When Jackson doubted the qualities of the physicist he knew Norton would assign to teach an acoustics course needed for the Communications Option, Norton insisted his man be given "a chance to try it out, especially since he has spent considerable time fitting himself for it." Norton responded to the enthusiasm for advanced research in his junior staff by requesting Stratton's consent to the establishment of a Laboratory of Theoretical  

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99Jewett to Maclaurin, 14 April 1917, MIT Archives, AC 13, Box 11, Folder 330.  
98Noyes to Maclaurin, 26 March 1917, copy in Hale papers, microfilm edition, Roll 27.  
97Curran, *Founders*, 265. Wilson had studied under J. Willard Gibbs, one of the first to construct a statistical mechanics, while pursuing his doctorate at Yale.  
96Stratton reluctantly conceded the point: "If Dr. Barss can give the kind of lectures that we have in view, I shall be greatly pleased .... Thus far I have neither seen nor heard anything to indicate that he is working up the underlying physical principles of sound." Norton to Stratton, 25 October 1926 and Stratton to Norton, 27 October 1926, both in MIT Archives, AC 13, 15/434.
Physics "to enable us to carry out experimental work with graduate students." But instead of seeking additional money or space for the laboratory, Norton claimed the laboratory could be launched by reshuffling space and carefully administering the department's research funds. Stratton tried to shake up the Physics Department by importing a series of prestigious European physicists to lecture at MIT. Norton was courteously mystified. He would help see the lectures through publication by the MIT Press, but when Théophile De Donder's lectures were published as The Mathematical Theory of Relativity, Norton sent a copy to Stratton with the assessment: "Frankly it does not look very interesting to me, but some of our mathematical friends seem to get quite excited over it."

Beyond the direct stimulation from the lectures proper, Stratton also sought the lecturers' advice on how to improve MIT's department, and hoped one would himself be interested in implementing his advice. He found himself in the midst of a vicious circle: the industrial orientation of the department repulsed the visiting theorists, but the leadership of a theorist was what they prescribed. Norbert Wiener, writing on Stratton's behalf, offered the position of department chair to Max Born. Wiener conveyed Stratton's promise to divide the Physics Department, "in accordance with your suggestion," into autonomous departments of industrial and pure physics with Norton taking the industrial side, and intimated that Stratton would form a third department of experimental physics. This offer, although restructuring MIT along the lines of the University of Göttingen, still did not convince Born that he would fit

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93 Norton to Stratton, 1 March 1924, MIT Archives, AC 13, 15/443.


95 Norton to Stratton, 1 June 1927, MIT Archives, AC 13, 15/434.

96 Born, one of the architects of quantum mechanics and professor of theoretical physics at Göttingen, had lectured at MIT in 1925 and collaborated productively with Wiener.

97 Wiener to Born, 14 February 1926, MIT Archives, AC 13, 2/48.
comfortably into MIT.98 William L. Bragg, after working at MIT in 1927, told Stratton that the x-ray laboratory "needs direction by a man with knowledge in theoretical physics as well as with a research spirit." But Bragg noted that John Norton, Charles' son and the assistant professor in charge of the x-ray work, was an obstacle to the importation of such a physicist:

I find it most difficult to frame my suggestions because of John Norton's present position. He has great practical ability and common sense and is a thoroughly nice fellow to work with, so pleasant that I find it hard to be critical. Yet I think he would be the first to realise the need for more direction than he can give if you wish to develop the laboratory to its fullest extent.99

The most Bragg would further do for MIT was to have its most promising junior researcher, Charles Warren, work in Bragg's laboratory in Manchester. That was no quick source of new leadership for the Physics Department.

Despite his lack of success in finding new leadership for the Physics Department, Stratton proceeded to petition the General Education Board in the spring of 1927 for $2,500,000 to endow research in and build a laboratory for physics and chemistry.100 In a follow-up visit to Wickliffe Rose, the Board's chairman, Stratton must have stressed his difficulties in obtaining fresh leadership in the Physics Department, for he left Rose with the impression that at MIT there was "neglect of pure science and research. Everything thought of in terms of technology."101 Stressing the magnitude of change that the Board could effect at MIT was not the approach to take with Rose, whose policy had been to support international leaders in the natural sciences, especially European scientists whose institutions had been

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98 Born to Stratton, 13 February 1927, ibid. Born recommended Stratton consult James Franck about MIT's needs because "he is much better informed about practical questions than I."

99 Bragg to Stratton, 23 April 1928, MIT Archives, AC 13, 2/57.

100 Stratton to General Education Board, 23 March 1927, Rockefeller Foundation Archives, RG 1.1, Series 224D, Box 4, Folder 38. This initial request consisted of a two-page letter with an invitation to the Board to have its representative visit MIT.

101 Excerpt from Rose's Diary, 25 March 1927, Rockefeller Foundation Archives, RG 1.1, Series 224D, Box 4, Folder 38.
weakened as a consequence of World War I. Rose simply let MIT's request sit in the General Education Board's docket without taking any action. Stratton concluded he needed to establish a School of Pure Science, with its own faculty and salary scale, alongside the School of Industrial Science "to dispel the illusions of those who believe that work in research and pure science is neglected at the Institute." When the Rockefeller charities were reorganized in 1928, jurisdictional responsibility for MIT's request shifted to the Rockefeller Foundation, and in January, 1929, Max Mason, the president, pressed Stratton for details of MIT's plans. Stratton solicited memoranda from the Physics and Chemistry Departments and forwarded them to Mason with a fresh introduction. Individually, neither memorandum cast MIT in a good light; together, under Stratton's covering letter, they were an embarrassment.

The Physics Department argued it needed new capital because the enlarged interest in advanced training in physics had been unpredictable. The planners of the new MIT campus, Norton claimed, had not anticipated the expanded range of physics courses demanded by engineering (especially electrical engineering), the increased demand for physicists in industrial laboratories, and the academic growth stimulated by theoretical developments. This admission that MIT's past leaders viewed physics as a satisfier rather than a creator of demands was accompanied by a research prospectus that displayed MIT's current leaders' inability to integrate advanced research into a coherent program. The prospectus consisted of a list of faculty interested in "pure research"—of the five cited as devoting most of their time to graduate teaching and research, none held rank above assistant professor—with titles


103Francis Hart to Stratton, 30 June 1928 and Stratton to Hart, 5 July 1928, MIT Archives, AC 13, 26/2791. Hart, was a member of the MIT Corporation Executive Committee. Stratton was even willing to seek an amendment to MIT's charter, which only mentioned a school of industrial science, should the charter prove a legal obstacle.

104Kohler, "Policy for the Advancement of Science," 502-513.
of their recent papers and project. There was no rationale for the importance of the particular specialties because nobody had both knowledge and authority to create a reasoned statement.

By contrast, the Chemistry Department, under Arthur Noyes' former student Frederick Keyes, provided cogent summaries of recent researches in inorganic, organic, and physical chemistry, and an assessment of prospects for cryogenic research, which the department had the personnel but not the space and equipment to pursue. Unfortunately for Stratton, the chemists' interpretation of MIT's administration was equally contrasting:

The quarters set aside for graduate work in Physical Chemistry consist of a residue of badly suited attic rooms which have proved seriously inadequate for carrying on work of a fundamental type.

Laboratory space in the Mining Department was set aside in 1920 for [organic] chemistry. This space, though ill adapted for the purpose, has had to be returned .... The organic group is now installed in wretched quarters of approximately half the floor area formerly available.105

For the chemists, the memorandum was a vehicle to vent their anger over the decline of Chemistry relative to Chemical Engineering at the time the new campus was built.

The only impression these memoranda could have conveyed to Mason was that senior faculty who were sympathetic to MIT's administration had little aptitude for organizing research while senior faculty with cogent research ambitions had poor relations with the administration. To make matters worse, Stratton, in his introduction, stated that MIT could not rely on its alumni to provide major capital support for research while the General Education Board had traditionally offered conditional grants requiring the recipient at least to match the grant.106 Little wonder that Mason responded to this application by soliciting Frank Jewett's opinion "from the inside, of the Tech situation and the advisability of participating in their


program." Jewett told him the executive committee of the MIT corporation, and especially Gerard Swope, was dissatisfied with Stratton and searching for a successor. Jewett counseled Mason to await the appointment of a new president.107

In an interview with Stratton in October, Mason stated openly what Rose had left tacit: "I explained to S[траттон] that it had not been the policy of the Board to aid technical colleges." Stratton was indignant but impotent. He had not understood the distinction between Sedgwick's and Noyes' forms of specialization, and thus not spotted Jackson's dedication to the former. In taking the advice of Europeans to separate industrial and pure science, and then failing to obtain leadership for pure science, Stratton's petitions invited the foundation to repair the mistakes of MIT's past administrators rather than to build on MIT's extant strengths to achieve the foundation's goals. This approach created a vicious circle of failure rather than a reinforcing link between innovation and fund-raising.

The electrical engineers and their allies on the corporation's executive committee wanted a president who would foster intra-MIT specialties that, like Sedgwick's public health science, were built on the shared interest of scientists and engineer, in the study of particular natural objects or processes. The Rockefeller Foundation's attitude towards "technical colleges" indicated that the president would have to possess the scientific credentials to convince the Foundation that supporting such specialties was its best contribution to the sciences. As president of GE, Swope probably solicited the advice of Willis Whitney, director of GE's Research Laboratory and also an alumnus of MIT, who recommended Karl Compton, the chairman of Princeton's Physics Department and a consultant to GE.108

107 Excerpt from Mason Diary, September 1929, Rockefeller Foundation Archives, RG 1.1, Series 224D, 4/38.

108 Mason Diary, 16 October 1929, Ibid.

Like Sedgwick, Compton pursued doctoral studies at an American university under an Englishman concerned with the conceptual development of his discipline. Compton graduated in 1912 from Princeton where Oliver Richardson advised Compton in experimental studies of the photoelectric effect, which Einstein had explained in 1905 by attributing particulate characteristics to light. Richardson had developed a theory that was similar phenomenologically to Einstein's without recourse to the troubling hypothesis of light quanta. Compton developed improved apparatus for measuring the velocities of photo-electrically liberated electrons, and together he and Richardson sought to test Richardson's and Einstein's theories. In this fashion, Compton was introduced to the international fascination with radiant energy and the structure of matter at the start of a period in which the American physics community as a whole would boost itself through increasing contacts with European researchers and indigenous efforts to become involved with the subtleties of atomic physics.

After a brief stint teaching at Reed College, Compton returned to Princeton as a faculty member in 1915. He set himself the task of developing the statistical

\[\text{\footnotesize 110} \text{Sopka, Quantum Physics 1.26-7 points out that bringing European physicists over to the United States for brief or extended stays was a common feature of the period.}\]


mechanics of collisions between electrons and gases, and then used his skills at controlling and measuring electron velocities to study such collisions experimentally in order to determine the "critical potentials" at which an atom or molecule would radiate, ionize, or dissociate. To Compton, the essential problem with atomic physics circa 1920 was that two different atomic models had been proposed with varying strengths and weaknesses: Niels Bohr's "dynamical model" explained radiation and ionization of the hydrogen atom but did not account for these phenomena in more complex systems and did not address questions of chemical properties; Gilbert Lewis' and Irving Langmuir's "static model," on the other hand, qualitatively accounted for chemical combinations without addressing questions of radiation and structural stability. By generating data on phenomena that fell between the successes of these models, Compton was attempting to lay the empirical foundations for an integrated viewpoint.

In retrospect, Compton's research was not strategic to finding an internally consistent atomic theory. But before the momentous conceptual insights that ushered in quantum mechanics in 1925 and 1926, many physicists expected that theoretical enlightenment would be won from an extended period of experimentation. Compton's skill was so highly regarded that his review article on critical potentials was one of the few American publications in atomic physics in the 1920s translated into German.

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117 Compton and F. L. Mohler, "Critical Potentials," Bulletin of the National Research Council, vol. 9, part 1 (September 1924); this was one of the few works of the American physics of the period to have the distinction of being translated into German. See Sopka, Quantum Physics, 2.78.
Yet while Compton's research can be readily portrayed as his variation on Richardson's effort to find experiments that illuminated the current problems of theorists, Compton also learned, like Sedgwick, to exploit other aspects of his work. The General Electric Research Laboratory studied the interaction of electrons with gases on the grounds that such phenomena were always occurring in light bulbs. From its founding in 1900, GE's Laboratory offered researchers an alternative to academic or entrepreneurial careers, and it attracted a group of "ionists" who found scientific challenges in understanding the light bulb's workings. GE's Laboratory, and others like it could seek what Leonard Reich aptly labels "the natural aspects of the artificial in...attempts to understand the natural laws of man-made devices." In the case of GE's Irving Langmuir, such an approach led to both valuable patents and a Nobel Prize.

By the early 1920s, GE's scientists were confident they had exhausted the incandescent lamp as an object of research—and thereby assured the company that no competitor could undermine GE's patent position—and they began studying the radiation of excited gases as a means of producing light. This was one of the properties of gases Compton was measuring, and he became one of the few consultants GE hired. Consulting had a broadening effect on Compton. Prior to 1922 virtually all of his publications appeared in the Physical Review or in Philosophical Magazine (and Physical Review continued to be his primary outlet), but from 1922 on he also presented his work in more technological journals.

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118Wise, "New Role," and "Ionists in Industry."


121Ibid., 84.

122Compton published pieces in Electrical World, General Electric Review, Journal of the American Institute of Electrical Engineers, Journal of the Franklin Institute, and Transactions of the American Electrochemical Society, especially on electric arcs. For a full bibliography of his writings, see
This broadened audience for electron physics sustained Compton as the conceptual relevance of his research was undercut by European theorists, who in 1925 and 1926 found starting points for internally consistent treatments of atomic structure and dynamics without analyzing data for critical potentials. When theorists implied that further research into molecular and electronic phenomena was academically unimportant since only mathematical complexity lay between known laws and a derivation of the phenomena, Compton responded by pointing to the technological value of the data and by arguing that mathematical derivations would not reveal all that is worth knowing:

But if any ambitious young scientists be discouraged lest there be little left to do, let him consider...the fact that every attempt to apply these laws, which look so satisfactory to us now, discloses new realms of knowledge still unexplored.\textsuperscript{123}

Like Sedgwick, for whom the study of bacteria in natural, laboratory, and engineering environments possessed value independently of its relevance to debates over the mechanisms and chronology of biological evolution, Compton continued to pursue studies of electron-gas interactions independently of their relevance to quantum mechanics. By the end of the 1920s, his and Langmuir's intellectual interests were so close that they collaborated on a massive pair of review articles summarizing and analyzing 30 years of work on ionized gases.\textsuperscript{124}

Compton did not know that he was being considered for the MIT presidency. He later recalled that Swope requested they meet to discuss GE's research program with the result that Swope asked Compton to write up his ideas on problems of organization and personnel. When Compton returned to discuss his report, Swope

\textsuperscript{123}Karl Compton, "The Electron: Its Intellectual and Social Significance." \textit{Annual Report of the Board of Regents of the Smithsonian Institution}, (1937), 220.

informed him that the prior meeting had been a pretense for Swope to assess Compton as a possible president of MIT, and he offered Compton the job on the spot.

Compton accepted after a discussion with Frank Jewett, president of Bell Laboratories and a member of the MIT Corporation. Jewett portrayed MIT's role as no longer to train students in shop practice, since advanced firms had specialized to the point of providing that training, but to provide students with a foundation in fundamental science and engineering principles:

On this basis, Dr. Jewett maintained that the leading institution of technology had a great obligation and a great opportunity to break away from the traditional [engineering] program and introduce a much more powerful element of fundamental science....[It appeared to me....that the job at the Massachusetts Institute of Technology would not be an abandonment of my professional career, but would be an opportunity to draw on this background of experience and my scientific contacts in order to enlarge the scope of their value and influence in the educational and research fields generally. 125

Compton joked about his lack of preparation for the MIT presidency: "While I knew something about the difference between an electron and a proton, I knew much less about the difference between a stock and a bond." 126 However, his recruitment and MIT's problems in maintaining research traditions in the sciences indicate that Compton was not being hired to oversee the management of MIT's endowment but to find an appropriate way to manage research at MIT.

The relationship of research to university administration was a subject to which Compton had given thought in the 1920s, partly as a response to his GE experiences. Responding to the ubiquity of specialization in the sciences, Compton argued "with this tendency [towards specialization] comes the necessity of a balancing movement ...: Research must become more and more cooperative." 127 University

125"Memorandum by Karl T. Compton on the subject of his invitation to become President of the Massachusetts Institute of Technology in 1930." 12 June 1945, MIT Archives, AC 4, Box 57, Folder 32.

126Ibid.

administrators should assume responsibility for keeping specialization from becoming intellectually distorting and socially irresponsible, and he recommended two strategies. First, was the promotion of border-line fields in order to compensate for the distortions caused by the extant system of specialization:

Nature herself is not divided into a physical world, a chemical world, a biological world; she is a unit. These artificial distinctions have been introduced for convenience. ... They have resulted in rapid development in the particular direction and by the particular method of each of the sciences, whereas work in the border-line fields has lagged behind.\footnote{Ibid., 441.}

Second, university administrators should cease to treat all departments equally and instead promote a select few on the basis of pre-existing strengths, the school's traditions and the needs of the employers of the school's graduates:

If these favored departments are chosen in a coordinated group, then the university becomes an active center for the development of that field and the promotion of cooperative effort....Through concentration of effort in a coordinated group of departments, a university has the opportunity not only to correct the dangers of over-specialization, but also to take a strategic position in fulfilling its obligations to society.\footnote{Ibid., 441.}

Industrial laboratories, Compton knew from his consulting to GE, had successfully followed this course, and while university laboratories could not match their scale, yet Compton thought universities could find ways to incorporate industrial laboratories' wisdom. He imagined "departments of a somewhat more flexible nature than those to which we are accustomed...built around one or two outstanding men [who would have] an opportunity for organization and concentration of effort."\footnote{Ibid., 442.} Compton's succession to the MIT presidency gave him the opportunity to put his ideas into practice, and in this sense his new job was indeed an extension rather than an abandonment of his research career.

In April, 1930, after agreeing to take the MIT presidency but well before his inauguration, Compton began corresponding with Rockefeller representatives about
MIT. Despite the manifest deficiencies in Stratton's proposal, Compton sought to salvage it because it contained an overarching virtue: it placed a consideration of MIT's character in the foreground of deliberations. Like Stratton, Compton wanted philanthropic support to back efforts to reform MIT. He pointed out to Max Mason,

It is of course known that my interest has been primarily in the fundamental sciences and that my selection [as MIT's president] indicates a wish on the part of the [MIT] corporation to emphasize this line of development. Immediate substantial evidence of approval by the Rockefeller Foundation would have a psychological affect in putting this program on a firm and accepted basis which would be of perhaps even more value than the actual financial support itself. 131

Compton's reworked application interpreted MIT's charter as consistent with a stress on scientific research:

It is the conviction of the Administration of the Institute that the greatest forward step which should now be taken in the direction of "aiding ... the advancement, development, and practical application of science" is a great increase in the emphasis placed upon research by the fundamental sciences in the work of the Institute. 132

But unlike Stratton, Compton understood the importance of conditional grants. Compton stated as his objective a $4,000,000 fund with the first million taken from MIT's endowment for construction of a new physics and chemistry laboratory, the GEB contributing the second million to a research endowment, and MIT raising the final two million as additional endowment over the course of the next two years.

With MIT the recipient and focus of the gift, Compton proposed to administer the endowment in a fashion that would foster the development of coordinated specialties. Appropriations from the endowment would be made on the recommendation of a Research Committee consisting of the president, the Dean of the

131Compton to Mason, 7 April 1930, enclosed with Compton to Executive Committee of MIT Corporation, 6 October 1930, MIT Archives, AC 13, Box 17, Folder 487.

Application to the Rockefeller Foundation for Assistance in Securing a Science Research Fund for Massachusetts Institute of Technology," MIT Archives, AC 13, Box 17, Folder 487. Application is undated, but judging from the absence of mention of new appointments to the MIT faculty, this must be the "outline of a specific program" mentioned in Compton's 7 April letter to Mason. John Slater agreed to become head of MIT's Physics Department in May, 1930.
Graduate School, and representatives of the mathematics, physics, chemistry, biology, and geology departments. He expected such administration to be of value "through coordinating the general research program and outlook of the various departments, through stimulating constructive thought and planning for the future, and through bringing about group effort and solidarity." Under such a regime, it would be easy to identify specialties that would be of interest to more than one department, and projects that were of interest to more than one department would obviously be favored.

Like Stratton, Compton followed up his letter with a visit to Mason, who forthrightly acquainted Compton with the Rockefeller charities' hesitancy to support a technical school. In response, Compton could initially only retreat from his argument that support of fundamental science would make MIT a better engineering school towards a view that science and engineering were independent with foundation support intended only for science:

"In connection with the general policy of your Board not to enter into the field of the technical schools, I should like to emphasize ... [that] MIT is not just an engineering or a technical school, but is just as truly a scientific school .... The fundamental sciences have exactly the same status as the engineering applications .... Study and research in the fundamental sciences are therefore in no way essentially different at MIT from their situation at Princeton, or Chicago, or any of the universities." Despite the frustration in Compton's response, Mason's attitude must have helped Compton to understand the task he faced. He needed MIT's science departments to advocate research programs that could be justified within MIT as complementing engineering interests and justified to the philanthropic foundations as a significant contribution to scientific knowledge. Compton's first appeal on MIT's behalf, written before he had conferred with those already at MIT, recruited new faculty, and addressed issues of intra-MIT policy, could not possibly fulfill such criteria.

\[133\text{Ibid., 7.}\]

\[134\text{Compton to Mason, 11 April 1930, enclosed with Compton to Executive Committee of MIT Corporation, 6 October 1930, MIT Archives, AC 13, Box 17, Folder 487.}\]
Conclusion

Since before the turn of the century, European and American trained faculty had pressed MIT’s presidents for time and materials to perform research. And since the turn of the century, philanthropic foundations had included research and post-graduate education among their targets for charitable disbursement. However, MIT had not become home to a confluence of these trends.

Though Sedgwick, Walker, and Noyes all believed in research as part of a university professor’s privileges and duties, their varied intellectual developments and ambitions led them to seek contrasting, sometimes contradictory frameworks for the administration of research. Sedgwick’s creation of public health science through a selective recombination of aspects of several fields created the need for a MIT president who could champion local innovations to philanthropies of national scope. His failure to cultivate Maclaurin (or conversely, Maclaurin’s failure to seize this role) left MIT without a seat at the table when the Rockefeller Foundation convened deliberations to set its policy for the support of public health education. Noyes’ insistence on using chemistry’s conceptual foundations to define an intra-departmental research laboratory attracted philanthropic support, but divorced him from Institute-wide issues of development and fund-raising. After refusing to accept the MIT presidency and to dedicate himself to working for MIT-wide developments that were compatible with his preference for intra-departmental research laboratories, Noyes found he lacked the power to compete with faculty that could connect their research ambitions with presidential initiatives. Walker’s construction of an intra-departmental research laboratory around a taxonomy of industrial practice did intersect with Maclaurin’s fund-raising efforts, but Walker left MIT’s president with the need to reduce inequities between academic and industrial salaries and between the earning potential of faculty engaged in consulting and faculty pursuing academic research. When Stratton refused to take on this challenge, Walker’s protégés found that corporate patrons could too easily disrupt MIT’s institutional integrity and ignore research aimed at the intellectual ordering of industrial practice.
Though Maclaurin and Stratton both rhetorically supported research, they neither made principled choices concerning the proper nexus between research and administration nor insisted that departments limit elementary teaching burdens for faculty with research ambitions. Intra-institutional developments depended more on the social connections of the researchers and the immediate burdens on the presidency than a reading of longer-term social developments and MIT's role in them. MIT's presidents could not maintain this "anti-tradition" of laissez faire and opportunism when the university's most powerful department wished to export research initiatives. Through perseverance and good fortune, Dugald Jackson did find two researchers whose programs held promise of fulfilling Jackson's desire to make the Electrical Engineering Department home to research laboratories modeled on Sedgwick's. When their ambitions became limited by the indifference to research in the Physics Department's leadership, his department's external patrons held the power to elicit a presidential response. When Stratton's efforts did not yield results, these patrons were prepared to hire a new kind of president for MIT.

Compton's recruitment and arrival at MIT signaled the Corporation Executive Committee's desire both for foundation support of research at MIT and for MIT research traditions that, like Sedgwick's public health science, would exploit local overlaps in scientific and engineering interests to create distinctive research and educational programs. To succeed, Compton quickly learned, he would have to establish suitable relationships among researchers, his office, and external patrons. The Rockefeller boards were not going to support intra-MIT reforms for the sake of improving MIT. To gain the sympathy and trust of the philanthropists for his need to produce reforms within MIT, Compton would have to demonstrate that intra-MIT reforms would attract or promote researchers that philanthropists could comfortably support. The next section explores Compton's administration of research at MIT: the priorities and accomplishments it favored, and the division of powers among philanthropists, administrators, and faculty it produced.
PART II: KARL COMPTON AND THE ESTABLISHMENT OF A LOCAL CONTEXT FOR RESEARCH POLICY
CHAPTER 5

INTRA-MIT ADMINISTRATIVE REFORMS AND SPONSORED RESEARCH

As Karl Compton assumed the presidency of MIT in September, 1930, the conflicting demands of his office were clear to him. On the one hand, the people directly responsible for hiring him, Gerard Swope and Frank Jewett, wanted MIT’s faculty to train researchers who would bring academic standards for intellectual rigor to industrial research laboratories. From Swope and Jewett thus came pressure to reform MIT to attract academically accomplished faculty whose ambitions would not undermine MIT’s reputation as the starting point to an industrial career. On the other hand, the people whose imprimatur would signal MIT’s suitability as a home for the intellectually ambitious, the officials at the Rockefeller Foundation and General Education Board, had made plain that their practice of philanthropy precluded support for reforming an engineering school that had not, on its own, been striving to reward its faculty and train its students for research that the foundation considered worthy. From the foundation thus came pressure either to frame MIT’s plans in line with foundation goals or to seek industrial support.

Equally clear was Compton’s theory for resolving this conflict. He took the presidency of MIT as an opportunity to act on his belief that a university administrator should organize his faculty’s research in response to institutional opportunities, interests, and traditions and not let his faculty specialize indefinitely on disciplinary criteria. This part of the dissertation is dedicated to Compton’s efforts to make Sedgwick’s philosophy of specialization into Institute-wide policy. Compton resolved the tensions of his office by promoting MIT as the site for creating new research fields through the local re-channeling of "streams of knowledge" into
knowledge "rivers" of industrial and educational utility. From an MIT-centered perspective, this policy was an agenda for local reform. It required that new faculty be recruited to cover the spectrum of "streams" within the purview of the envisioned "river," and it required that new and old faculty frame their ambitions in response to local needs and opportunities as well as to competition from like-minded specialists at other institutions. But to the foundations Compton could argue that his proposals merited support because they would enlarge the scope of scientific research and teachable knowledge, and he could discreetly de-emphasize the significance of his proposals to MIT's structure and the serviceability of its students for industrial research.

Success for Compton depended on symbiosis between his intra-MIT reforms and external campaigns. But for analytic clarity, the two will be separately discussed. The rest of this chapter will deal with the powers and people Compton pulled into his administration and their in-house policies for industrial support of research. Nobody, as has been seen, was happy with Stratton's handling of the intra-MIT impact of corporate patronage. Compton, without significant resistance, expanded presidential powers to regulate the faculty so that faculty members' industrial contacts would serve the goal of integrating industrially relevant phenomena into university research but without restructuring university teaching around industrial practice. No advocates of Noyes' or Walker's traditions rose to oppose his measures, and had any tried, they would have been hard-put to find external leverage during the Great Depression.

Compton's external campaigns to secure philanthropic support take up the rest, and bulk, of this part, because they were essential to making Compton's local reforms of more-than-local significance. To succeed in his external campaigns, Compton had to politic on two fronts. First, he had to convince the foundations that their proper role (at least with respect to MIT) was to make university administrators the arbiters of their faculties' intellectual ambitions and innovations. If foundation officials—from their own reading of intellectual opportunities or from consultations with a sampling of leading researchers—decided what were and were not the promising avenues for academic research, then MIT, as an administrative unit, would be cut out of the
formulation of research policy, and Compton would have no means to resist "overspecialization" of research within disciplinary boundaries. Second, to convince foundation officials that his proposals to reorganize fields within MIT created academically worthy research opportunities, Compton had to convince nationally known scientists to direct his proposed programs. If scientists of accomplishment or obvious potential would not take charge of MIT's intellectual reorganizations—even though that work often equated scientific merit with the extension of academic breadth rather than the deepening of theoretical foundations—Compton's claims that MIT was incubating desirable new fields would be undercut. Compton's recruitment drives were thus effectively petitions to the national scientific community to broaden its scope and welcome locally constructed specializations.

The next chapters present Compton's efforts to spur or assist the reforms he thought desirable for MIT. His pursuit of funds and personnel for locally constructed research fields established a pattern of relationships among his office, his faculty, and the philanthropic foundations. The MIT presidency became an active broker between researchers and research patrons. Compton personally supported faculty initiatives that he believed would fruitfully recombine intra-disciplinary specialties into novel fields of research and teaching. And he pressed foundations to grant his office the power to distribute their contributions to MIT's research so that he could compel faculty attention to local needs. Compton's activist presidency both encouraged his faculty to consider how their interests might be combined with others' within the greater MIT community and restrained intra-departmental specializations that could lead to fragmentation and competition within MIT. His administration was not a vindication of Arthur Noyes in response to the failings of William Walker's followers, but a repudiation of the departmental framework, conceptual concerns, and nationalist orientations that had fueled Walker's and Noyes' dispute.1 Compton drew his strength from Sedgwick's legacy, which most closely mirrored his own

1Cf., Servos, "The Industrial Relations of Science," 548; Geiger, To Advance Knowledge, 181.
experiences, and which had blossomed in a vibrant, expansive electrical engineering programs, whose patrons were responsible for his hiring.

Administrative Personnel and MIT Traditions

Within MIT, Compton's predilections were most baldly evident in his choice of faculty for MIT-wide administrative positions: Samuel Prescott as dean of science and Vannevar Bush as vice president and dean of engineering. Prescott, who turned 60 when he was appointed dean in 1932, had been chairman of the Department of Biology and Public Health since Sedgwick's death in 1921. His promotion, effectively a salute to what might have been in the life sciences at MIT, signaled adherents to Noyes' ideals that Compton would not promote intra-departmental laboratories built around concerns with conceptual problems.

Prescott's background ideally qualified him to present the new administration's research policy to the MIT community as a strengthening of extant MIT traditions. With Compton's explicit support, he wrote a Technology Review article that presented MIT's biology department as a model for how Compton sought common interests among departments.

Institutionally, however, there appear to have been relatively few attempts ... to build up strong, well organized programs of cooperation in diverse departments or members of different staffs in the study of research problems with multiple scientific interests. ... It is gratifying that recently, as a result of administratively activated committee work, research has been undertaken in special fields in which the needs of industry or the demands of special knowledge have been so apparent and so compelling as to bring about official action. ... It is reasonably evident that there are many problems classed as pure science which are in reality composites, and in which there would be obvious advantages in the association of workers from the contributing fields. ... To illustrate this point it may be stated that research in most subdivisions of our own field of applied biology ... must have recourse to special knowledge of physics and of chemistry if the results are to be of highest value and stand the most severe tests as to their worth.²

²Samuel C. Prescott, "Pooling Efforts in Research: Scientific Provincialism Must Give Way to Concerted Action," Technology Review, 37 (1934-1935), 15. Compton appended a note to Prescott's article, "The Road to Achievement in Research," in which he declared: "The day of academic isolation of the research worker is past. Research in scientific fields, as in all others, must progress through the
The message to partisans for pure science at MIT was clear: the route to administrative favor lay in finding pure research that was composite in character.

The appointment of Bush, with his ambition for developing differential analyzers that transcended the mathematical needs of power engineers, was likewise an indication to adherents to Walker's ideals that Compton would not promote departmental laboratories built around categories of industrial practice. Engineers, to find administrative favor, would have to find topics that were relevant to the experimental or mathematical needs of scientists. However, Bush's appointment also indicated Compton's willingness to promote the career of MIT faculty whose accomplishments matched Compton's ideals, regardless of departmental proprieties. Unlike Prescott, Bush had never been the chairman of his department, and at age 42 in 1932, Bush's new position was plainly not to be the last of his career. Compton ended up entrusting to Bush the problem of determining an intra-MIT policy for industrially sponsored research.

**Presidential Powers and Faculty Privileges**

Samuel Stratton had failed to find a unified approach to the problems of sponsored research at MIT. He laid responsibility for the problems of the Research Laboratory of Applied Chemistry on the predominance of sponsorship by individual corporations over sponsorship by trade associations; he laid responsibility for the problems of the Division of Industrial Cooperation and Research on its administrators' bureaucratic timidity; and he ignored pleas that better salaries or research opportunities were essential to keep MIT competitive with other universities. Swope and Jewett probably described Stratton's failings when recruiting Compton, for even before Compton took office, he began to consider how sponsored research and consulting could be used to further his reading of MIT's educational mission. Upon taking office, he promptly conferred with selected individuals about possible policies.

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pooling of information and effort in a common cause. ... Departmental organization necessary for administrative purposes must not hinder contact and cooperation.
Compton arrived at MIT with both ideas and some means for changing policies. For inspiration, he had not only his own experiences with General Electric, which provided a model for how academic and industrial science could be mutually enriching, but also conversations with administrators of the University of Chicago, who stressed to Compton that they expected their faculty to work full time for their salaries and to turn over all consulting fees to the university.\(^3\) For means, Compton enjoyed authority over an operating budget enlarged by a tuition hike made possible by the new Technology Loan Fund, which Gerard Swope had raised to help MIT undergraduates finance their educations.\(^4\) Within months of his arrival at MIT, Compton formulated a "New Plan of Faculty Appointments" that blended his own experiences, the academic model of the University of Chicago, and MIT's traditions.

The outlines for Compton's new plan first emerged in a discussion of 11 September 1930 with Dugald Jackson, the chairman of the Electrical Engineering Department. Jackson came prepared to plead for raising the salary scale and found himself preaching to the converted. Compton took the opportunity to discuss the "suitable handling of the outside consulting activities of members of the staff" as a means to raise more money for the salary scale. For purposes of analysis, they divided the staff's outside activities into three categories: 1) private business activities; 2) research, testing, and consulting for private companies; and 3) research for trade associations.

Samuel Stratton, as Compton and Jackson understood him, considered only the last a contribution to MIT because the results could usually be published and the faculty member usually assessed no personal fee for doing the research. But Compton saw no reason to elevate work for trade associations over work for private firms "provided such work [for private firms] can be carried on in such a way as to

\(^3\)Compton spent the summer between his departure from Princeton and arrival at MIT in Chicago, where he had the opportunity to speak with university administrators. See Compton, "Problems of Outside Paid Employment," Speech File, 77-60, MIT Archives.

aid rather than injure the work of the Institute and the professional attainments of its staff." Compton was inclined to charge a 100% overhead on all outside work, as did the Electrical Engineering Department on its work for the National Electric Light Association. When a faculty member charged a consultant's fee to an outside organization, Compton wanted half the fee turned over to MIT. "On this general plan," Compton concluded

the value to the Institute of any member of the staff might be estimated on any one of several grounds, such as 1, his ability as a teacher; 2, his value as a research man; 3, his value to industry and to the Institute as indicated by the returns from his consulting practice.5

The major barrier Compton and Jackson anticipated to their ideas was to secure the allegiance of the "great majority" of the faculty over the potential leadership of those senior members making so much money in consulting fees that a salary increase would not compensate a 50% loss in those fees. To soften the impact of their reforms to the resistant, Compton and Jackson envisioned offering faculty the option of freezing their current salary and allowing them to keep their full fees.6 Younger faculty would then have to decide whether their interests lay in climbing MIT's salary scale while foregoing half their future consulting fees or maximizing their consulting income. Given the uncertain prospects of the latter in the Depression and the new administration's preference for the former, Compton and Jackson had reason to hope their reforms would take root.

Compton found a trustworthy scout of faculty views in Charles Norton, the longstanding faculty member whom Compton quickly shifted from chairman of the physics department to full-time director of the Division of Industrial Cooperation and Research (DICR) in order to make space for new leadership in physics.7 Norton

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5Compton, "Memorandum of Conversation with Professor Jackson." 11 September 1930, MIT Archives, AC4 66-12.

6Ibid.

7See Section 6.2, below.
impressed Compton with both his good will and administrative sense. About two weeks after the discussion with Jackson, Norton brought Compton the news that several faculty members to whom Norton had talked would "without question" accept instructions to return to MIT portions of their salaries to compensate for time spent consulting. Although Compton wanted the inverse, he was heartened to hear that he could expect a respectful reaction to his efforts to define just recompense for the faculty's varied activities. He immediately shared with Norton his own ideas for changing the terms on which faculty were employed. Ten days later, Compton waxed enthusiastic to Swope:

I have been working gradually towards some suggestions in regard to the policy for handling outside consulting work by members of the staff, and have had some very helpful discussions with Professors Jackson and Norton....I have high hopes that some definite steps may be taken next year without arousing very serious opposition.

Some dissenting advice did reach Compton. At the suggestion of University of Chicago officials, Compton conferred with Trevor Arnett, the General Education Board's expert on university finance. Arnett argued that outside work usually created a divided interest in the minds of the faculty and that the ideal way was for the full time of the faculty to be controlled by the Institute....As a matter of principle, it would not seem well for the Institute to divide the [consulting] fee with the members of the faculty.

Compton left the meeting toying with the idea that he might selectively arrange as

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8Norton called Compton's attention to the lack of policy on when MIT stationary should be used in reporting research results to external sponsors and recommended the course Compton preferred—that consultants' reports to private firms be written on personal stationary while reports to technical journals be written on MIT stationary. Norton to Compton, 22 September 1930, and Compton to Norton, 1 October 1930, MIT Archives, AC4 1-15.

9Compton, "Memorandum of Conversation with C. L. Norton," 24 September 1930, MIT Archives, AC4 66-12. See also Compton, "Tentative Draft (confidential): To Members of the Instructing and Research Staff," undated, MIT Archives, AC4 66-8. This is probably the "preliminary draft of a letter to the faculty regarding the question of outside consulting and research work" to which Compton refers in his report of his conversation with Norton.

10Compton to Gerard Swope, 3 October 1930, MIT Archives, AC4 1-19.

11Trevor Arnett Diaries, 1929-1930, entry for 5 November 1930, page 305, Rockefeller Foundation Archives.
many high-salary/no-consulting-income appointments as MIT's budget and the preferences of individual staff members permitted. But he quickly rejected this idea when he heard it again in a different context. The day after meeting with Arnett, Compton met with Professor William Ryan of the Chemical Engineering Department. Ryan and his assistant, Warren Lewis, advocated the establishment of two types of faculty appointments—one with low salaries and the privilege to consult and the other with high salaries and no consulting privileges—as a way to narrow income differences among staff members and to provide employment terms that would give the department a fighting chance to hold a full-time, non-consulting director of the Research Laboratory of Applied Chemistry. Although identical to his transmutation of Arnett's advice, Compton judged Ryan's suggestion to be unworkably rigid because the particular expertise of staff members could, over time, fall in or out of fashion with industry. Furthermore, Compton doubted that two salary scales could make a significant dent in income differences among staff of equal rank.

Instead of seeking to turn all or part of MIT into the University of Chicago, Compton accepted faculty involvement in industrial affairs as legitimate and potentially virtuous. His most comprehensive private formulation of his ideas on research for industry set forth a single set of terms for employment based on the presupposition that research and consulting work on practical problems for industrial interests is a legitimate public service by the Institute and may be a source of inspiration and benefit to the members of the staff who undertake it.

What he claimed for his office were the powers to safeguard MIT's reputation on the grounds that the ability of some faculty members to build lucrative consulting practices depended on the ability of all faculty and administrators to make an MIT

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12Compton, "Memorandum of Conversation with Mr. Arnett," 5 November 1930, MIT Archives, AC4 66-12.


faculty appointment a prestigious position. Thus Compton justified requiring faculty to turn over half their consulting fees to MIT for salary increases that rewarded all who contributed to MIT's prestige, justified ordering department chairmen to give priority in use of facilities to prestige-enhancing faculty research, and justified placing a 50% surcharge on the overhead charged to external sponsors to raise the capital MIT's administration needed for future innovations and renovations. To implement these measures, Compton wanted all contracts between MIT staff and outsiders to be "made through or with the collaboration of the DICR," which would compute the overhead fee, inform the bursar that the staff member owed MIT a share of his personal fee, and assist in making patent arrangements for inventions by the staff.\(^{15}\)

In practice, Compton moved gradually to implement his ideas. In January 1931, he announced a "New Plan of Faculty Appointments," which promised "salaries substantially above present salaries" but made promotions and new appointments contingent on the individual agreeing to turn over half of any consulting fees he received to a "professor's fund." In the announcement, Compton made clear his contempt for "outside work of a routine 'pot-boiling' type which does not advance the art or develop the professor" and his approval of "a high type of consulting work or industrial research ... as a service to society, as an advancement of the art and as a factor in quickening and broadening the teacher." But he also explicitly declined to address the role and policies of the DICR, the office through which he could most readily exercise power.\(^{16}\)

Compton's acceptance of industrially sponsored research as a potential benefit to MIT and its faculty brought no backlash from Noyes' former compatriots in the Chemistry Department. The chairman, Frederick Keyes, recognized (with regret) that consulting had become a way of financial life and recommended that MIT tax this activity. He reported that 17 of 39 chemistry staff members worked as consultants

\(^{15}\)Ibid.

\(^{16}\)Compton to Members of the Faculty, "New Plan of Faculty Appointments," MIT Archives, AC 13, 6-138.
and had, on average, increased their income by 43.5%; when he heard that the
administration was considering a salary increase, he proposed a more complicated
version of the Compton-Jackson plan for administering outside research, not a
removal of all incentives to consult.\footnote{Keyes to Charles Norton, 17 December 1930, MIT Archives, AC 4, 66-12; Keyes,
"Memorandum Regarding Outside Work, Chemistry Department," 21 January 1931, MIT Archives,
AC 4, 66-13. Ideally, Keyes would have preferred to require faculty to take their industrial work off-campus, but recognized that was no longer possible. He suggested the administration set the financial "needs" of faculty members, allow the faculty unfettered collection of consulting fees to make up the difference between their needs and salaries, and progressively tax consulting fees that boosted income over needs. The fact that he sent this memorandum one day before Compton’s announcement of his new plans implies that Keyes, for this issue, was not among Compton’s confidants.}

Nor did Compton’s desire for regulatory powers inspire a backlash from
Walker’s supporters. In March 1931, the Visiting Committee to the DICR’s annual
report contained the usual justification for keeping close contacts between the MIT
staff and industry:

Obviously, therefore, insofar as there are problems of common interest to
Technology and industry, particularly problems in Pure or Applied Science,
through the solution of which industry would be helped and in the solving of
which our instructing staff would get the necessary or desirable teaching
stimulus and the possibility of increased income, thereby our relations with
industry should be so developed that we would be in intimate touch with
these problems.\footnote{Report of the Massachusetts Institute of Technology Visiting Committee of the Corporation on
the Division of Industrial Cooperation and Research," 11 March 1931, MIT Archives, AC 13, 6-138
and AC 4, 66-9.}

But the committee declined to speculate on how the new plan of appointment would
affect this philosophy in the long run, expressed confidence in Compton’s ability to
make judgements as experience with the new plan accumulated, and actively
supported, from a different perspective, Compton’s desire to exclude industrial
support for routine analyses at MIT. The committee was not worried that such work
was beneath the dignity of MIT faculty, but rather, as had been noted at the
Technology Plan’s inception, that it could turn MIT into a cut-rate competitor with
the commercial laboratories in which MIT alumni often made their living.\textsuperscript{19} No less a figure than Arthur D. Little, one of the original architects of the Research Laboratory of Applied Chemistry and School of Chemical Engineering Practice, contributed a letter to the committee articulating this view.\textsuperscript{20}

Compton's major effort to define an Institute-wide role for the DICR came the following spring when the visiting committee reconvened. In the interim Compton had learned the troubled histories of the Technology Plan and the RLAC through discussions with Norton's chief assistant, E. B. Millard.\textsuperscript{21} And the press of events was making decisions necessary. Compton was expecting a visit in May from members of the New England Council, a trade association that had been considering a proposal to underwrite construction of an MIT industrial research laboratory, which would rent research space to small member firms that could not afford their own laboratories, research staffs, and libraries.\textsuperscript{22} Since the proposal had the DICR managing the laboratory and making the contracts, it behooved Compton to settle on a definite policy for the DICR before negotiations with the Council came to a head.

The visiting committee served as a sounding board for the administration's proposals for operating the DICR. Compton, Bush (now vice president of MIT), and the DICR staff joined the members of the committee for discussions of MIT's options. Compton quickly gained unanimous assent to the propositions that "the

\textsuperscript{19}Ibid. See Section 3.3, above.

\textsuperscript{20}Little to A. Farwell Bemis, 11 February 1931, attached to ibid. in AC 13. The prevailing economic conditions may well have brought home the force of this argument to Little.

\textsuperscript{21}Compton, "Memorandum of Conversation with Prof. Millard regarding the DICR," 22 September 1931, MIT Archives, AC 4, 1-6; Millard to Compton, 15 October 1931, MIT Archives, AC 4, 66-13; Millard to Norton, 4 September 1929, MIT Archives, AC 4, 66-11. Millard suggested that corporate relations be administered from the President's office in order to supersede the competition between the DICR and the RLAC.

\textsuperscript{22}Compton to Ray Hudson, Technical Advisor of the New England Council, 12 December 1930, MIT Archives, AC 4, 66-12; "Proposal of the Massachusetts Institute of Technology to the New England Council for the establishment of an Institute of Industrial Cooperation and Research," MIT Archives, AC 4, 66-8; Maurice Holland, Director of Division of Engineering and Industrial Research of the National Research Council, to Ray Hudson, 1 December 1930, copy in MIT Archives, AC 4, 66-12.
Institute should make an organized effort...particularly directed to the service of industry" and that "all work of industrial cooperation in the Institute should be unified."23 The problem, however, was how to fit the variety of activities deemed "industrial cooperation" into a unified administration.

Compton distinguished industrial work that individual staff members arranged through their own contacts from work that would come to MIT through the DICR. The latter, he believed, should become more prevalent because industrialists would insist on institutionalized over individual arrangements for research:

President Compton emphasized the need of extending the activities of the Division and better organizing its work because of the changing attitude of industry in general toward research. He said industry formerly perhaps tolerated but was fast realizing that it must depend on research.24

There was no controversy over placing the Division fully in charge of managing institutional arrangements for sponsored research and declaring that the Division’s research would be for publication in all but exceptional circumstances. The Division would thus become a reaction vessel in which MIT’s administration could combine industrial demands for research with academic skills under conditions appropriate to the creation of educationally relevant specialties.

More troubling to the Visiting Committee was the role of the DICR in dealing with consulting or closed research for individual firms, whereby staff members boosted their incomes and provided research assistants with both the social contacts that led to attractive positions with the contracting firm. Bush was particularly sensitive to the problems inherent in the joint administration of industrial requests for Institute help and staff members’ use of Institute facilities for individually arranged, confidential research. In a formal presentation, used as a starting point for the visiting committee’s discussion, Bush stated, "The Institute is justified in carrying on closed research...as a means of increasing income, serving industry, and giving

23"Meeting of Corporation Visiting Committee and Staff of the Division of Industrial Cooperation and Research," 10 March 1932, MIT Archives, AC 4, 66-10, emphasis in original.

24Ibid., emphasis in original.
research training to younger staff members." But during the discussion,

Dr. Bush said he was not yet convinced that acceptance by the Institute of such [closed] research is desirable....Dr. Bush suggested that acceptance of closed research almost necessarily led to our working with and for only one concern in any field of industry.

A solution, Bush pointed out in his formal presentation, might rest in fashioning a division of powers between the DICR and other units within MIT: "In carrying on closed research, there should be a central organization supervising such work for the entire Institute, but with considerable decentralization from a technical standpoint." If DICR personnel could do their jobs without learning the technical specifics of closed research, then competing firms could sponsor research in MIT's other units without fear of DICR personnel spreading trade secrets.

Rather than push for a definitive statement in a single day, the committee decided to meet again in the hope that Arthur Little and William Coolidge of General Electric could attend. In the interim, Bush met with Lewis and Ryan of the Research Laboratory of Applied Chemistry, who provided valuable insights into the prospects and problems of relating the Division to their laboratory. While they wanted sponsored research to be "primarily educational in aim," they insisted that closed contracts were critical to "secure frankness on the part of client." They had a valuable suggestion for how these contracts could be centrally administered without identifying the entire Institute with the RlAC's clients:

To avoid contract tying entire Institute to a single client exclusively [through a closed contract], suggestion is that departments formulate their own contracts, subject to central approval, contract, however, tying up the work of a single laboratory only. Contracting thus decentralized would avoid one argument against the exclusive contract.26

If department chairmen, in Lewis' and Ryan's estimation, had control over contract

25Ibid.

26Bush, "Conference with Professors Lewis and Ryan," 18 March 1932, MIT Archives, AC 4, 66-14. While they agreed that centralized control over industrial contracts was needed, Lewis and Ryan also wanted the central authorities to spend profits from sponsored research in departments that attracted outside contracts rather than redistribute the profits.
formulation, they could match projects and laboratories so that competing firms could comfortably sponsor independent, closed research at MIT.

With Bush reporting Lewis' and Ryan's perspective, the second meeting of the DICR visiting committee reached collective confidence that a central authority would have enough information, power, and options to administer all forms of industrial cooperation without alienating either the faculty or sponsors. The information would come from the requirement that "all outside work using Institute facilities should be reported to the Division of Industrial Cooperation and Research." The power would come from the requirement that "all work involving charges in excess of $50.00...must have approval of the Division, as regards type of work, form of contract, relation to the Institute, and charges for the service." The options would come from the DICR's ability either to negotiate the contract and organize the work itself or to have a department take on these responsibilities subject to the Division's review. Through his control of the Division's policies, Compton could stop work that put MIT in competition with commercial laboratories, collect a profit for MIT from sponsored research, throw his own weight behind projects that promised publishable results of curricular import, and delegate to departmental administrators the responsibility for negotiating and managing educationally valuable closed contracts with competing firms.28

While these issues could potentially put Compton into delicate situations, he was willing to accept such burdens in exchange for the ability to set a unified system of charges and chain of command that reflected his own values for the worth to MIT of various types of sponsored research. A "very adequate" financial return—Bush had recommended a 100% surcharge on the overhead—would be a prerequisite to

27 "Meeting of Corporation Visiting Committee and Staff of the Division of Industrial Cooperation and Research," 21 March 1932, MIT Archives, AC 4, 66-10.

28 Ibid. While not fully resolving the issue of how to distribute profits from sponsored research, the committee nevertheless noted "that central control and pooling of receipts might be of distinct advantage in equalizing interest in bad times...It seemed the general feeling that on the whole central control was desirable and that the departments generally could be satisfied that adequate consideration of their interests would be arranged."
undertaking closed research, and MIT would not compete for such work by lowering its charges. 29 Under these conditions, Compton was happy to tell department chairmen who worried that sponsored research was taking time and apparatus from departmental research, that their judgements would enjoy "moral backing by the DICR and the Administration." 30

The following autumn, Compton presented the deliberations of the visiting committee to the faculty, along with a restatement of the logic behind the new plan of appointments. Compton wished the faculty to take two points from the meeting:

First, is the necessity of a proper coordination and control of the work of industrial cooperation. ... Second, is the fact that the work of industrial cooperation is considered not entirely as a personal matter with each staff member, but that it is essentially a part of the Institute's program and should be recognized as such and carried on in that spirit. 31

In administrative terms, these two points collapsed into one; both functions could only be performed in the president's office, to which all staff would report their industrial activities.

Conclusion:

Compton's efforts to subject industrial research at MIT to Institute-wide controls were largely successful. Faculty members accepted Compton's mediation when their industrially oriented pursuits brought them into conflict. For example, when two faculty members collaborated on developing germicides from organic peroxides, but disagreed over whether they should share any patent royalties for non-germicidal uses of the organic peroxides, Compton arbitrated to the satisfaction of

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29Compton, "Memorandum of Meeting with the Visiting Committee of the Division of Industrial Cooperation and Research," 21 March 1932, MIT Archives, AC 4, 66-10.


31Compton, "Presentation of Work of the Division to Faculty," 9 November 1932, MIT Archives, AC 4, 1-6, emphasis added.
both.\textsuperscript{32} Industrialists accepted Compton's judgement on whether their pursuit of consultants disrupted MIT's integrity as an educational institution. For example, when the General Electric Research Laboratory wanted the consulting services of a faculty member on terms that would make patents stemming from the member's past, Institute-sponsored research assignable to General Electric, Compton bluntly told William Coolidge that "we would be in an indefensible position if we should permit this [Institute-supported research] to be tied up exclusively with one company."\textsuperscript{33} General Electric did try to modify its proposed consulting contract to take Compton's objections into account, though no relationship was consummated because General Electric came to doubt that the staff member had the time to do the job General Electric wanted.\textsuperscript{34} Finally, Compton continued to solicit industrial sponsorship of research at MIT on the terms he thought appropriate. For example, when a friend told Compton that an executive in a New England textile firm was interested in research, Compton promptly wrote to invite the executive to consider sponsoring research at MIT and to explain the DICR's policies.\textsuperscript{35}

In revising intra-MIT policies, Compton's primary task was to push the faculty to view their industrial contacts from a scholarly and educational perspective. He sought changes in three areas. First, he wished to raise MIT's operating budget and faculty salaries so that faculty members would feel financially able to exercise discretion in taking on outside work. Second, he sought to tax the consulting fees of faculty members and use the taxed funds for more salary increases, thereby reducing the income differential between staff members who consulted and those who

\textsuperscript{32}Compton, "Memorandum of Conference with Milas and Proctor," 21 August 1931, MIT Archives, AC 4, 1-14.

\textsuperscript{33}Compton to Coolidge, 5 May 1939, MIT Archives, AC 4, 1-41; Servos, "Industrial Relations," 547-548.

\textsuperscript{34}See "Retainer Agreement," attached to Ernest Hauser to Compton, 2 June 1939, MIT Archives, AC 4, 1-41, especially paragraphs 2 and 6; Coolidge to Hauser, 6 June 1939, copy in same.

\textsuperscript{35}Compton to William S. Nutter, vice president of Goodall Worsted Company, 3 November 1934, MIT Archives, AC 4, 1-15.
contributed to MIT's reputation by publishing research, teaching, or carrying administrative burdens. Third, he centralized the administration of consulting and sponsored research in order to judge the appropriateness of the incoming problems.

By establishing criteria and administrative procedures for judging the suitability of and adjusting the rewards for sponsored research, Compton had gained for his office the power to set Institute-wide research policy. In negative terms, he used this power to preclude work that could be performed in commercial laboratories and discouraged industrialists, through the surcharge on the overhead, from soliciting members of MIT's staff to perform such work. But there was a positive side to Compton's use of administrative powers, as he explained to a potential industrial sponsor:

our particular opportunity lies in the handling of problems which require a cooperative attack by men in different related fields, or which require equipment and facilities not usually found in smaller laboratories or which are of a peculiarly pioneering nature.\textsuperscript{36}

This view of MIT's virtues applied equally, in Compton's estimation, to research that philanthropic foundations should sponsor. The powers that allowed Compton to adjudicate conflicts involving industrial sponsorship of research also potentially allowed him to cultivate projects that piqued his sense of scientific values. However, convincing foundations to use MIT in the fashion that Compton believed most valuable entailed entrepreneurial demands that took Compton beyond dealings with his faculty and MIT's boosters.

\textsuperscript{36}Ibid.
CHAPTER 6

FOUNDATION RELATIONS AND THE REFORM OF PHYSICS

The reform of MIT's Physics Department became the crucible in which Compton sought to forge his preferred relations among researchers, university administrators, and philanthropic patrons. The Physics Department was an obvious choice of sites for several reasons. First, the department's chairmen had never promoted research and graduate education, so presidential initiatives were not going to enliven intra-departmental conflicts over the tactics and directions for research. Second, it behooved Compton to move promptly to satisfy the Electrical Engineering Department and its powerful supporters, who had hired Compton because they believed the lack of a physics research program was damping the expansion of electrical engineering research interests. Third, the philanthropic foundations had been well disposed in the 1920s to support institutions featuring physical research. And fourth, Compton knew the physics community so well that he could keep his own council on MIT's needs and physicists' qualities. With the need for improvement manifest, powerful intra-MIT support assured, and philanthropic precedents established, conditions were highly favorable for a knowledgeable administrator to effect "top-down" reform.

The Opportunity for Reform and the State of Physics

Two factors peculiar to physics circa 1930 made intra-MIT reform of physics a challenge to Compton's desire to use the powers of his office to regulate specialization in his faculty: first, the principles of quantum mechanics had been worked out; and second, demographic expansion in physics was forcing physicists to
reconsider the structure of their professional societies. In the absence of a satisfying understanding of atomic structure, all experiments and theories involving atomic particles constituted a single literature.¹ But with quantum mechanics providing guidelines for distinguishing between what was, in principle, comprehensible and mysterious, the various skills of physicists no longer seemed united under a common cognitive goal. At the same time, the American Physical Society, which had comfortably accommodated the needs of a small, largely academic physics community, was struggling with how and whether to deal with the significant and increasing number of physicists in industrial research. In the late 1920s, researchers in several industrially important areas of physics were forming their own societies rather than pursuing their interests within the American Physical Society.²

These circumstances potentially placed Compton on the horns of a dilemma. If physics per se became unified around and limited only to specialties, like radioactivity studies and nuclear physics, in which the adequacy of quantum mechanics was tenuous in principle, then he would be hard-pressed to develop a physics department that would find common cause with other departments whose researchers confronted physical problems. On the other hand, if MIT’s physics department became merely a holding company for a group of specialties with no common sense of purpose, then he could end up renewing what MIT already had: a collection of limited experts whose inclination would be to take advantage of the industrial value of their expertise. Compton’s challenge was to create a physics department that covered a breadth of specialties yet held the specialists’ loyalties through their common interests in the development of mathematical or experimental techniques.

¹Studies of general relativity were the exception to this rule. For a summary of the results of a cocitation analysis of the physics literature for this period, see Weart, "Solid Community."

²Weart, "Physics Business," 321. Compton’s own research on critical potentials and electron impact phenomena is a fine example of research whose standing seemed uncertain given that the research had been undertaken to aid the development of atomic physics but had neither contributed fundamentally to the formulation of quantum mechanics nor challenged physicists to add to the known principles of quantum mechanics.
In intellectual terms, Compton had an implicit alternative to those who preferred to divide the physics community by whether or not its members’ work seemed potentially to require an expansion of theoretical horizons (as opposed to greater cleverness in the use of established theoretical principles). For speech-making purposes, he considered the internal structure of physics in 1931 and divided the field broadly into "theoretical" and "applied" physics. However, he did not use "theoretical" to mean a mathematical system for explaining or describing the behavior of physical systems. His examples of "theoretical physics" consisted of "the atomic nucleus," "spectroscopy," "electricity in metals," "electrical insulators," and "electrical discharges through gases."³ The message in this manner of structuring physics was that "theoretical" physicists were not to be defined by the concepts they used in explaining phenomena but by the objects which they studied. What made these topics "theoretical" to Compton was that they held, in his estimation, potential for further growth through the elaboration of experimental and mathematical technique. From his perspective, physicists could address topics appropriate to their varied professional circumstances without risking their self-identities as scientists who create new knowledge. So long as more was to be learned about an object or wrung from an experimental technique, Compton would include the specialty within theoretical physics, even if the research did not strain the fabric of recent conceptual developments or were pursued most aggressively in industrial settings.⁴

Making this view of physics an institutional reality required more than speech-making on Compton’s part. He fought social fragmentation of the national discipline by serving on American Physical Society committees that sought to make the society


⁴Consistent with this attitude, Compton was more inclined to classify highly conceptual work as outside of physics proper. Thus when asked to comment on Albert Einstein’s research, Compton responded: "It has always seemed to me that the theory of general relativity is a ‘tour de force’ of mathematics rather than a helpful contribution to the interpretation of nature .... [Einstein’s] interpretation of the phenomenon of photoelectricity has been tremendously more important in interpreting the phenomena of nature than has the general theory of relativity." Compton to David Morris, 30 October 1931, MIT Archives, AC 4, 1-14.
more welcoming to non-quantum physicists. These committees, in 1931, successfully promoted the creation of the American Institute of Physics, a federation of the American Physical Society and the more industrial Optical Society of America, Acoustical Society of America, and Society of Rheology, for the purposes of financing journal publication and promoting physics to the general public. But that success assured, at best, that Compton’s localized efforts to create a physics department appropriate to MIT would not be undercut by widespread, public feuding over which subjects merited the limited space in the Physical Review or which careers merited the greatest public approbation. To secure a physics department that accepted responsiveness to other departments as a virtue that could be cultivated without undercutting the department’s unity of purpose, Compton needed to combine research funding procedures that would enable him to coordinate faculty research with a staff that shared his sympathies about locally coordinated research. Since the Rockefeller charities would not support MIT on the grounds that MIT ought to be a recognized home to scientific research, Compton had to build a staff that philanthropies would wish to support collectively.

The Search for Physics Leadership

Compton began by searching for a new physics department chairman shortly after accepting the MIT presidency. He offered the post to William L. Bragg, since 1919 a professor at the University of Manchester, Albert Hull, since 1913 a member of the General Electric Research Laboratory, George Thomson, since 1922 a professor at the University of Aberdeen, and William Houston, since 1929 associate professor at the California Institute of Technology—all of whom declined—before

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landing John Slater, since 1926 an associate professor at Harvard.⁶ The careers of these individuals exemplify what Compton considered the proper scientific perspective and accomplishments to qualify a scientist for administrative authority.

Bragg, Hull, and Thomson had completed their training before or immediately following World War I, and all three had gained reputations through diffraction studies. Bragg, with his father, had won the Nobel Prize in 1915 for their 1913 work showing that the diffraction of x-rays by crystals could be explained by, and thus used to elucidate, the geometric arrangement of atoms in crystals.⁷ The elder Bragg challenged Hull to produce and analyze a diffraction pattern for iron. Hull developed a "powder method," which he used to diffract x-rays through iron and several other substances that do not form sizable crystals, before abandoning the hope of finding a technologically relevant explanation for iron's magnetic properties.⁸ Thomson, a friend and peer of the younger Bragg, eventually won the Nobel Prize for his experiments in 1925 showing that electrons could be diffracted like x-rays and thus possessed wave properties as Louis de Broglie had postulated in 1924.⁹

Compton's preference for a champion of diffraction studies fit with his desire for a physics department whose members would seek, find, and pursue significant aspects of physics in the interests of other departments, thereby enabling Compton to

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⁷The first experiments analyzing the scattering of x-rays by a crystal were not performed by Bragg but conceived by Max von Laue and performed in Arnold Sommerfeld’s Institute for Theoretical Physics at Munich. The reasons why von Laue's original interpretation failed to convince his peers is a matter of historical controversy, as is the extent to which physicists were knowledgeable of and had confidence in the theory that crystals consisted of lattices. See Paul P. Ewald, ed., *Fifty Years of X-ray Diffraction*, (Utrecht: Oosthoek’s Uitgeversmaatschappij, 1962), 5-101; David Phillips, “William Lawrence Bragg, 1890-1971,” *Biographical Memoirs of Fellows of the Royal Society*, 25 (1979), 88-89. Paul Forman, “The Discovery of the Diffraction of X-rays by Crystals: A Critique of the Mythos,” *Archive for History of the Exact Sciences*, 6 (1969-70), 38-71.


tout MIT as deserving patronage because of the novel research program its particular combination of personnel could generate. As Bragg later characterized x-ray analysis:

    It is a branch of research with a discipline of its own... At the same time, it is a typical border-line science which has had a great influence on many other sciences, and has used their bodies of knowledge as a basis for its discoveries. ¹⁰

This feature of x-ray diffraction was apparent in the 1920s. Bragg owed his professorship at Manchester to a mineralogist whose open ambition was to establish "a department of pure crystallographic research," not to build up the physics department.¹¹ Metallurgists in the 1920s used Hull’s powder method of x-ray diffraction to analyze alloys of several industrially important metals.¹² Still others were beginning in the 1920s to use x-ray diffraction to analyze organic compounds and biologically important substances.¹³

Electron diffraction, a more recent discovery than x-ray diffraction, had not acquired such a track record in 1930. Nevertheless, Compton, especially given his friendship with Jewett, would have been intrigued that Thomson’s experiments duplicated results that C.J. Davisson and Lewis Germer of Bell Telephone Laboratories had achieved in a series of experiments dating back to 1919. Davisson’s and Germer’s duties had been to examine the processes occurring in a vacuum tube,
not to test an atomic theory.\textsuperscript{14} The convergence of theirs and Thomson's work would have reminded Compton of the convergence of his and Langmuir's interests and made electron diffraction seem a cornerstone for building an industrial audience for quantum physics.

With Bragg, Thomson, or Hull as chairman, Compton could look forward to a physics department that, while maintaining an identifiable core of experimental and mathematical techniques, would expand its range to meet the interests of other departments and turn MIT into the center for cooperative research that Compton believed universities ought to become. Compton could not, however, induce any of these men to come to MIT. Both Englishmen were professionally restless in 1930, and Compton knew how to tailor his offers to fit each man's ambition. He stressed to Bragg that Bragg would control "excellent facilities [that] will be forthcoming to build a really great department of Physics"\textsuperscript{15} at a time Bragg feared he was earning a reputation as a crystallographer who analyzed structures rather than as a physicist who produced a teachable body of theory and technique.\textsuperscript{16} Compton approached Thomson with the offer of a "research professorship," having surmised that that title encompassed "all the essential features which would appeal to Thomson"\textsuperscript{17} at a time Aberdeen forced Thomson to obtain grants to purchase his apparatus.\textsuperscript{18} Unfortunately for Compton, Imperial College at London University was also

\textsuperscript{14}Russo, "Fundamental," 125-138. Their first priority was to examine emissions from filaments bombarded with positive ions, but bombarding surfaces with electrons was an active, attractive "sideline" because their equipment was easily converted to experimenting with electrons and because electron experiments might reveal ways to minimize secondary electron emissions from vacuum tube components.

\textsuperscript{15}Compton to Bragg, 1 April 1930, MIT Archives, AC 13, 2-57.

\textsuperscript{16}In 1929 Bragg unsuccessfully petitioned Ernest Rutherford, director of the Cavendish Laboratory at Cambridge, to divide the laboratory in half, with Bragg coming to Cambridge to be in charge of physics of the solid state while Rutherford continued to direct nuclear investigations. Phillips, "Bragg," 104-105.

\textsuperscript{17}Compton to Stratton, 23 April 1930, AC 13, 4-105.

\textsuperscript{18}Russo, "Fundamental," 152-153.
searching for a chairman for its physics department. Bragg, when confronted with concrete offers to leave Manchester, suffered a nervous breakdown and turned down all offers.\textsuperscript{19} Thomson took the London job.

Hull was probably disinterested in chairing MIT's physics department because, outside of introductory teaching, the post held no new challenges. Within GE, he already had responsibility for overseeing younger researchers and advising the laboratory director on the initiation and conclusion of laboratory projects.\textsuperscript{20} GE's insistence that his work contribute to corporate interests\textsuperscript{21} had not been incompatible with earning a scientific reputation. He won election to the National Academy of Sciences in 1929 on the strength of his publications on the motion of electrons in the vacuum tubes he invented and on the research uses to which these tubes could be put.\textsuperscript{22}

Having failed to secure a diffraction physicist close to his own age, Compton turned to younger men whose educations were coterminous with the advent of quantum mechanics. However, in recruiting William Houston and John Slater, Compton was careful to consider quantum physicists who treated theory as a tool rather than an object of research. As was common among their generational peers, both men pursued post-doctoral training with European theorists without following their hosts into celebrating or worrying over the conceptual peculiarities of the new

\textsuperscript{18}Phillips, "Bragg," 105.


\textsuperscript{21}GE researchers sometimes took on development work that took them away from research, had to secure patent department approval before disseminating results, and had to abandon, as Hull did with x-ray diffraction, fertile research areas that did not impress colleagues as leading to useful results for GE. See Hull, "Autobiography," in Ewald, ed., Fifty, 584-585; Wise, Whitney, 106, 271-272; and Reich, American Industrial Research, 110.

formalism. However, Houston and Slater evinced not only such a philosophical pragmatism but also a preference to using quantum mechanics to continue their research into electronic properties of matter.

After completing his doctorate at Ohio State in 1925, Houston obtained post-doctoral support at Caltech on the strength of his use of the Fabry-Perot interferometer to resolve the complex structure of spectral lines. When he began this work in the early 1920s, two theories for the origin of spectral lines' complex structure had each had partial success, and many physicists anticipated that a consistent quantum theory of the atom would emerge from an accumulation of more accurate spectral data and more experience in the applicability of the two theories.

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24Interferometers split incident light into separate beams and recombine the beams to produce an interference pattern with distinct maximal fringes for each of the incident light's constituent wavelengths. Fabry's and Perot's design split the incident light by reflecting the ray between two parallel crystal plates covered with reflecting films. The technique was problematic because light was "wasted" by absorption in and transmission through the films, and many researchers preferred Lummer and Gehrcke's design, which split the incident light by internal reflections within a single crystal plate. Houston tried various films for the Fabry-Perot design and demonstrated that a series of two Fabry-Perot interferometers, in which the spacing between the latter's plates was an integral multiple of the former's, would better resolve the structure of spectral lines than either individual instrument. See Houston and W. G. Moore, "Transmission and Reflection of Metal Films," Journal of the Optical Society of America, 16 (1928), 174-176; Houston, "Compound Interferometer for Fine Structure Work," Physical Review, 29 (1927), 478-484. As reflective films improved, most researchers came to prefer the Fabry-Perot interferometer because of the adjustability of the spacing between the plates. For a discussion of interferometers contemporary with Houston's research, see W. Ewart Williams, Applications of Interferometry, (Brooklyn: Chemical Publishing Co., 1930), esp. 91-93; for a more current discussion, see S. Tolansky, An Introduction to Interferometry, (New York: Longmans, 1955), esp. 141-143.

But by the time Houston published data on three lines of the hydrogen spectrum,26 this justification for experimental spectroscopy had been undercut. In the summer of 1925, Werner Heisenberg showed that a consistent quantum formalism could be built by redefining the kinematic terms with which the motion of an individual electron was described,27 and shortly thereafter George Uhlenbeck and Samuel Goudsmit realized that fine spectral structure could be explained by adding spin to Heisenberg’s kinematics.28

Houston did not react to these rapid conceptual developments either by learning a new experimental technique that might yield more interesting results to theorists or by becoming a full-time theorist. Instead, during the academic year 1927-1928, which he spent in Germany on a Guggenheim Fellowship, Houston learned to use quantum mechanics as a tool for interpreting older experimental results and the spectroscopic results he knew how to generate. In Munich Houston thrived under the direction of Arnold Sommerfeld, the most philosophically pragmatic of continental theorists,29 who had seized on using quantum mechanics to rework the classical electron theory of metals.30 Houston first used Sommerfeld’s approach to interpret

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26 Houston, "Fine Structure and Wave Lengths of the Balmer Lines," Astrophysical Journal, 64 (1926), 81-92. The hydrogen spectrum merited close analysis because it was the one spectrum which the relativistic theory appeared to explain comprehensively.


experiments of Robert Millikan, Houston’s boss at Caltech, who had shown that
electrons behave as though they do not share in the thermal energy of a metal’s
atoms. Houston then used wave mechanics to create an explanation for one of the
best-known difficulties for classical theories of the behavior of electrons in metals: the
dependence of metals’ conductivity on temperature. In Leipzig with Heisenberg,
Houston concentrated on working out a perturbation technique for approximating the
energy levels and spectral fine structure of two electron systems. Under neither
theorist did Houston demonstrate an interest in dealing conceptually with the wave-
particle duality or with phenomena that were outside his experimental expertise.

Like Houston, John Slater, from Harvard, had spent a post-doctoral year in
Europe. But unlike Houston, Slater’s European training came in 1923-1924, before
the development of quantum mechanics, and Slater had been based in Copenhagen
with Niels Bohr. Bohr did draw Slater into considerations of the conceptual
underpinnings of quantum theory. Slater’s reaction to this experience was not to
emulate Bohr’s dedication to seeking an intellectual framework that would resolve the
paradoxes of quantum theory but to align himself with experimentalists in seeking an
accurate accounting of spectral phenomena. By the late 1920s, Slater had bumpyly

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31 Houston, "Die Electronemission Kalter Metalle," Zeitschrift für Physik, 47 (1928), 33-37; R. A.
Millikan and Carl F. Eyring, "Laws Governing the Pulling of Electrons out of Metals by Intense
Electrical Fields," Physical Review, 27 (1926), 51-67. Millikan and Eyring found a 700°C
temperature range in which electron emissions from tungsten depended only on the field’s
characteristics.

32 Houston, "Elektrische Leitfähigkeit auf Grund der Wellenmechanik," Zeitschrift für Physik, 48
(1928), 449-468; H. E. Rohrschach, Jr., "The Contributions of Felix Bloch and W. V. Houston to the
which treated electrons in metals like gas particles and defined conductivity as the mean free path of an
electron through a lattice of metallic ions, had no organic means of explaining why the mean free path
changed with temperature. Houston derived a relationship between conductivity and temperature by
treating electrons as waves and interpreting metallic conductivity as a function of the distance over
which electron waves would encounter no interference effects from thermally oscillating ions.

33 Houston, "Some Relationships between Singlets and Triplets in the Spectra of Two Electron
electrons.
established a publication record that was similar to the one Houston had smoothly created.

Slater arrived in Copenhagen with the idea that electrons orbiting atomic nuclei might emit "virtual electromagnetic waves," which would not carry energy but determine the probability of finding a light quantum at a given point in space when the electron jumped between stationary states. His idea appealed to Bohr because it provided an assumption that could help obviate the need for light quanta and restore conceptual coherence to atomic physics.\textsuperscript{34} Bohr included Slater as co-author in a paper arguing that quantum theory should combine virtual waves with statistical rather than strict laws of conservation of energy and momentum in order to preserve the universal applicability of the wave theory of light.\textsuperscript{35}

However thrilled Slater may have felt at the beginning of his fellowship,\textsuperscript{36} he became disillusioned with Bohr and institutes of theoretical physics when experiments promptly demonstrated that atomic emission and absorption of radiation strictly obeyed conservation laws. He published his own note to establish what his original ideas had been and to argue their viability despite the new evidence.\textsuperscript{37} Back at Harvard, he independently published his efforts to recast his Copenhagen work in a form that could be applied to spectroscopic data,\textsuperscript{38} and he collaborated with George Harrison, a recently minted Stanford spectroscopist who spent the 1924-1925

\textsuperscript{34}Slater, \textit{A Scientific Biography}, 9-12; B. L. Van der Waerden, "Introduction," in \textit{Sources of Quantum Mechanics}, B. L. Van der Waerden, ed., (New York: Dover, 1967), 12-14. In his recollections, Slater portrayed himself as doubting the viability of Bohr's ideas from the outset. Whether or not Slater's memory was accurate on that point is irrelevant here, where the object is to characterize how Slater positioned himself professionally after his year in Europe.


academic year at Harvard, in efforts to measure and account for the transition probabilities between stationary states of the sodium atom.\(^39\) Slater came to feel personally estranged from Bohr and viewed Bohr’s institute as an exemplar of how not to organize physics.\(^40\)

Compton’s desire that Houston or Slater become chairman of MIT’s physics department showed a preference for American physicists who had learned quantum theory’s intricacies from European theorists without becoming enthralled with duplicating European theoretical institutes within American universities. Both Houston and Slater treated quantum mechanics as a tool to understand electronic phenomena and not itself an object of research, and not as a criterion for judging the value of experimental topics. With either man as physics department chairman, Compton could reasonably hope he was not creating another Arthur Noyes who would insist on an autonomous fiefdom that would compete with engineering interests but another William Sedgwick who was sufficiently \textit{au courant} scientifically to appeal to the philanthropic foundations yet sufficiently parochial in outlook to respond to the local needs of the MIT community.

Compton approached Houston first, thus initially avoiding competition with Harvard. However, Houston was put off by Compton’s terminology for internal divisions in physics. Compton’s original offer was for Houston to be professor of "theoretical physics." But Houston, who wished to continue experimental research, assumed Compton used the term in the European sense and expressed dissatisfaction with a position that could exclude him from organizing experimental work. Compton tried to reassure him with the explanation: "it is you we want and not any specific title or prescribed duties. As a matter of fact we use term theoretical as describing a


\(^{40}\)Slater, \textit{Solid State}, 240.
physicist intermediate between experimental and mathematical and partaking of both.\textsuperscript{41} But Houston elected to remain at CIT, where he was made full professor in 1931, rather than accept the greater risks and promotion of the MIT offer.

Unlike Houston, Slater responded to Compton’s overtures not with worries over the position’s precise duties but with three questions that probed the viability of the position. First, he asked whether his acceptance of the MIT job would undermine Compton by antagonizing Harvard. Second, he asked whether a young outsider who managed a favored department would arouse such resentment and jealousy within MIT to make failure likely. And third, and most intriguing, Slater asked whether by moving to MIT he would be foolishly gambling his own career on Compton’s ability to establish solid research traditions in the sciences at MIT. Slater told Compton, "I am, as far as I know, the only person at Harvard who has been enthusiastic about your appointment, who has not merely thought that you were crazy and that there was another good physicist gone wrong."\textsuperscript{42} After all, Slater pointed out, in the long run the reputation of MIT’s president and physics department chairman would not attract and hold research-oriented students and faculty unless MIT could raise money for graduate student fellowships, laboratory apparatus, student and faculty housing, and other amenities of university life.

Compton’s response paid the most attention to relations with Harvard, but he did not dodge Slater’s worries about his career. He frankly acknowledged that taking the job would be a gamble for Slater, and then argued that the gamble was worthwhile. Compton had essentially taken the same gamble himself, and he used his own decision as evidence that there were genuine prospects for success:

I may say that the Corporation, particularly Mr. Swope, have [sic] impressed me very much with their earnestness and active interest in the Institute and their desire really to work for it \textit{provided there is a real

\textsuperscript{41}See Stratton to Compton, 21 April 1930 and Compton to Stratton, 23 April 1930, MIT Archives, AC 13, 4-105, where they discuss Houston’s objections to the terms of Compton’s offer. Compton’s letter to Stratton quotes from one he sent to Houston.

\textsuperscript{42}Slater to Compton, 23 May 1930, Slater Papers, American Philosophical Society, microfilm copy in MIT Archives.
program to strengthen it. I may say that this attitude was the chief thing which led me to undertake the job.43

Compton was offering Slater the chance to work out "a real program" for physics at MIT. Just as Jewett had induced Compton to view the MIT presidency as an opportunity to enlarge the influence of his experiences and contacts, Compton was offering Slater an opportunity for Slater to turn his views on physics into an institutional reality. Slater quickly wired Compton his acceptance.

Departmental Reorganization and the Renewal of Rockefeller Foundation Petitions

By the time Compton and Slater settled into their MIT offices in September 1930, they had largely finished the selection of personnel for the Physics Department. Slater had convinced his former collaborator, the spectroscopist George Harrison to come to MIT as Director of the physics half of the new physics and chemistry laboratory,44 whose construction Compton would finance with the remains of Eastman's capital gifts. Compton had obtained commitments from two of his former students at Princeton, the electron physicist Wayne Nottingham and the theorist Philip Morse, to shift to MIT in 1931. Finally, Compton and Slater had determined who among the current faculty belonged in the Physics Department, who belonged in engineering departments, and who should leave MIT altogether. Most significant in this respect was the change in affiliation of Julius Stratton, Edward Bowles' protege, to the physics department. In an exchange of letters, Compton, Slater, and Dugald Jackson had agreed that Stratton would continue to guide research work at Round Hill

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43"Compton to Slater, 28 May 1930, in ibid. Compton's emphasis. With respect to Harvard, Compton stressed that an MIT offer that involved a promotion for the Harvard recipient should not be viewed as a hostile act towards Harvard. As for the resentment Slater's appointment might arouse, Compton assured Slater, "the entire Institute has been so conscious of the need for strenuous action in regard to the physics department, that I feel confident of support in any good move to that end."

44"Harrison had been planning to move to Ohio State, and because Stanford was not inclined to continue spectroscopic research, he was free to take his instruments with him."
while teaching in the physics department. Thus Stratton was positioned to attract physics students to the study of phenomena encountered in the use of directed radio beams.

These changes in personnel and leadership, however, changed the structure of the department far more than the content of the curriculum. Slater organized the course catalogue to group advanced course in five specialties: optics and photography, electronics, x-rays and the structure of matter, atomic structure and spectroscopy, and theoretical physics. While any structure was going to be new, since none previously existed, courses in all these areas were being taught at MIT before Compton's and Slater's arrival, and only a few courses that were obviously designed to expose students to industrial practice were transferred to engineering departments. Compton and Slater were less interested in drawing boundaries between physics and engineering than in using administrative authority to impose intellectual order on industrially relevant physics.

The significance of these moves for Compton's vision of MIT's relations to the broader scientific community became apparent in the new version of the MIT proposal to the Rockefeller Foundation and the General Education Board. In October 1930, Compton asked selected department chairmen for briefs on their proposed research programs to be used as exhibits in the grant application. Slater's response made explicit his "real program" for physics at MIT. Quantum mechanics, in Slater's view, was most significant for its potential to provide a basis for calculating

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45See Dugald Jackson to Samuel Stratton, 15 August 1930, AC 13, 11/328, Compton to Slater, 30 July 1930, Slater Papers, American Philosophical Society, microfilm copy at MIT. Apparently, Samuel Stratton acted on the transfer of Julius Stratton without obtaining Dugald Jackson's final assent, and Jackson was upset because no prior provisions were made for a successor to J. Stratton for direction of the scientific research at Round Hill. Compton alerted Slater that Jackson could want to discuss J. Stratton's responsibilities, and Compton advised Slater that "I am sure you will find Professor Jackson very reasonable and I will be glad for him to meet you because I think he is the man of all others on the faculty whose interest in the Department of Physics is keenest and most likely to be helpful." Stratton's salary ended up being shared, with two-thirds coming from the Physics Department and one-third from the Round Hill research budget; see Edward Bowies to Compton, 19 September 1932, AC 4, 76/52. See Section 7.1, below for an account of Stratton's activities.
properties of objects that had previously been subject to only empirical and qualitative analysis:

This being the situation, we must design a research program to take advantage of it [quantum mechanics], for it is evident that a new field, deductive atomic theory, has just opened, and one may anticipate an even more important development from it, on account of its connection with chemistry, metallurgy, electrical engineering, and many other branches, than even electricity and mechanics have had, and a combined scientific and engineering school, as the Institute is, seems the ideal place to carry out such a program.\[46\]

He predicted that the specialties defined as parts of the physics department would provide data for and be unified by deductive atomic theory. Spectroscopy under Harrison would be a central pivot.

This [spectroscopy] fits in particularly well with the probable course of development of physics. In the first place, intensities [of spectral lines] give very direct information about the processes going on in discharge tubes ... leading very naturally to the fields of photochemistry and chemical problems on the one side, to discharges and arcs on the other. And in the second place, the study of intensities leads very naturally to problems of the optical properties of solids.\[47\]

In each area, Slater claimed the services of productive researchers. Nottingham, upon arriving, would lead the work on discharges and arcs, simultaneously providing knowledge of atoms and molecules in excited states and collaborating with electrical engineers interested in applying discharge techniques. Two physicists Slater inherited, Charles Warren, the x-ray crystallographer Bragg had labeled promising, and Hans Müller, a former assistant to the well reputed Dutch theorist Peter Debye, would initially cover the properties of solids, though Slater anticipated wanting more appointments "because in the development that physics is having at present, the properties of matter in bulk are becoming more and more important." Slater himself would press for cooperation with chemistry because in theory, "chemistry and physics

\[46\] Slater to Compton, 1 November 1930, Slater Papers, American Philosophical Society, microfilm copy at MIT Archives.

\[47\] Ibid.
are becoming indistinguishable," and he pointed out that MIT's plans to house chemistry and physics together in a new laboratory embodied that vision.

Compton's covering brief to the application contained the same financial request as the previous application, but he reorganized his arguments to shift the stress from MIT's needs to MIT's potential. Leading the list of reasons Compton gave for granting the application was that "The program of research is very important, in some respects unique, and its support should be justified primarily by this fact alone, as an effective contribution to the advancement of fundamental science."48 Relegated to the rear was his contention that favorable action by the Rockefeller Boards would strengthen the proponents of an increased emphasis on fundamental science at MIT.49 Between these claims were arguments that portrayed fundamental science not as the weak link in the advancement of MIT's technical mission but as endemic to an advanced institute of technology. The more rapid growth of the graduate school over the undergraduate college, Compton maintained, indicated MIT was becoming a "super-technological school" that would provide "the best final stages of training" to those who desire and are capable of benefiting from "contact with the spirit of research." Consequently,

There is no reason for hesitating to support fundamental science at MIT because it is by name a technical school; on the contrary there is an excellent reason for supporting it in that science is so essentially the root of all the work of the school.50

Rockefeller support, in this scenario, would be going to science and not engineering, but to science that would be distinctive to, benefit from, and contribute towards a technological education.

48 "Brief of Application to the Rockefeller Foundation or the General Education Board for Support of Fundamental Science at the Massachusetts Institute of Technology," attached to Compton to Stratton, Thomson, Swope, Webster, Main, Hart, and Morss, 19 November 1930, MIT Archives, AC 13, Box 17, Folder 487, 1.

49 Ibid., 5.

50 Ibid., 3-4.
What Compton and Slater wanted was for the Rockefeller charities to adopt a certain type of science policy (at least with respect to MIT). Funding would be granted to MIT itself, rather than selected researchers at MIT, and Compton, with his committee of department chairmen and top aides, would use the money to spawn new fields, like "deductive atomic theory," through the coordinated pursuit of topics that appealed to members of multiple departments. In intellectual terms, such a strategy for forming new fields of study would enable Compton both to inhibit specialization within disciplinary boundaries and to encourage alliances among faculty from engineering and science departments. In political terms, such a strategy for forming new fields would empower MIT to be a local institutional arbiter of intellectual innovation, because only where an appropriate groups of specialists could be brought together under unified authority would a field like deductive atomic theory thrive.

The job for the foundation, in this scenario, was not to assemble a general consensus on what were (and were not) the worthy, achievable goals for research and then to evaluate whose projects could best contribute to those goals. Rather the Foundation should support those institutions whose locally generated innovations held promise of developing into greater-than-local significance. The foundation would be an investment banker to universities capable of assembling a group of investigators whose interactions promised to give rise to new fields.

The Rockefeller charities only partially embraced the role Compton laid out for them. As the title of MIT's proposal implied -- "Brief of Application to the Rockefeller Foundation or the General Education Board..." -- the proposal straddled the jurisdictional line that the Rockefeller charities' administrators were attempting to draw between research (Rockefeller Foundation) and teaching (General Education Board). The General Educational Board did not accept Compton's contention that MIT was or would become a "super-technological school." At its 7 November 1930 conference, the board ruled MIT's application to be outside its jurisdiction because "MIT is essentially a technical school," and at the same meeting the board voted to

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51Kohler, "Policy."
contribute to a CIT drive to raise $4,000,000 to endow work in chemistry, geology, and biology. Compton fared somewhat better with the Rockefeller Foundation. He wrote to Max Mason and Herman Spoehr, the director of the Natural Sciences Division, to supplement further his claim that the precedent of supporting CIT should make MIT eligible for support. In a meeting with Compton, Spoehr stated that "the officers did not feel a case could be made for having the R[ockefeller] F[oundation] undertake the endowment of a research program in N[atural] S[cience]." The stumbling block, however, was less the character of MIT than doubts over the desirability of giving out endowments. The discussion switched to the possibility of the Foundation starting the research program so that MIT would later have a basis for raising a research endowment from its own backers. Compton changed his request to a "fluid research fund" of $130,000 to be spent in six years on physics and chemistry, later increasing the request to $170,000 and including biology and geology. This form of support retained the centrality of MIT as the controller of the funds and would permit Compton to distribute them through the deliberations of his Research Committee of deans and department chairmen.

The Rockefeller Foundation accepted this proposal, citing as its reasons Compton's rise to the presidency, the "group of noteworthy investigators" assembled in the physics department, and "the importance of stressing research on fundamental scientific problems in order to develop a more progressive and effective attitude in

52 "Officers' Conferences General Education Board," vol. 3, 1 July '930 - 30 June 1934, 28, Rockefeller Archives. The award to CIT was reduced to 1/4 of the total at the 25 November 1930 conference; see 36.

53 Compton to Mason and Spoehr, 21 January 1931, Rockefeller Archives, RG 1.1, Series 224D, Box 4, Folder 40. Compton presented data showing the ratio of science to engineering students in each class. For MIT the ratios were .44 for juniors, .34 for seniors, and 1.05 for graduate students. For CIT they were .56, .39, and 4.22 respectively. Compton claimed the ratios were not notably different, but the ratio for graduate school, where research would be done, belie that claim.

54 "Excerpt from Herman A. Spoehr Diary," 27 January 1931, in Ibid.

55 Compton to Spoehr, 30 January 1931, in Ibid.
engineering and industry." In retrospect, Compton was lucky that the Rockefeller Foundation acted when it did. Herman Spoebr did not last as director of the Natural Sciences Division, and his successor, Warren Weaver, disliked quantum mechanics and took to funding individual researchers whose projects fit his vision of a reductionist biology. It seems doubtful that MIT’s application, which placed programmatic control in the hands of an MIT research committee led by atomic physicists, would have appealed to Weaver as he sought to set policy and create precedents for his tenure.

Slater’s and Compton’s policies plus Rockefeller support created a lively physics department whose members used their mathematical and instrumental skills to expand the utility of physics within MIT. The major recruits found overlaps with each other, with the retained members of the department, and with other departments. The result was a culture in which faculty and students sensed they were supposed to explore the depth and significance of their technical overlaps and not concentrate exclusively on matters of relevance to their disciplinary peers.

Philip Morse, whose ambitions as a theorist included contributing to an understanding of nuclear dynamics, used his mastery of the mathematics of waves to analyze regions where the distinction between waves and particles did not break down. He assumed responsibility for the acoustics classes, which the electrical engineers had found poorly taught under Norton’s administration, and supervised the thesis of C.S. Draper on problems of vibrations in internal combustion engines. When he and Julius Stratton found they used the same mathematical functions in their individual work on quantum theory and electromagnetic propagation, Compton raised

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56"Grant Action 31050," 15 April 1931, Rockefeller Archives, RG 1.1, Series 224D, Box 4, Folder 38.


58See, for example, "Collision of Neutron and Proton," Physical Review, 50 (1936) 748-754, and 51 (1937) 706-710.

59See Section 4.3, above.
funds so that they could oversee the development of tables for the values of those functions.\textsuperscript{60}

George Harrison, in addition to measuring the complex spectra that would be the empirical core of "deductive atomic theory," also promoted spectroscopy within MIT on the basis of its multi-disciplinary utility. He organized MIT summer programs in spectroscopy, which combined opportunities for students outside physics to become versed in spectroscopic techniques with a conference where researchers in biology, medicine, engineering, chemistry, and astronomy as well as atomic physics presented spectroscopically-achieved results and advances in spectroscopic technique.\textsuperscript{61}

The department's culture soon produced benefits for its graduate program. For several years, Mervin Kelly, as director of the vacuum tube department at Bell Laboratories, had been agitating for building up the laboratory's sophistication in recent atomic physics out of a sense that the vacuum tube was not good enough to be the last word in amplification and that a better in-house grasp of matter's electronic properties would consequently serve the company's interests. In 1936, when he became director of research and his superiors lifted a Depression-induced hiring freeze, he was able to recruit on this basis.\textsuperscript{62} Kelly's demands were a boon for physicists open to inspiration from the problems of an engineering environment and willing to specialize on the basis of the phenomena they studied rather than the mathematical or experimental tools they used. Slater found that students who

\textsuperscript{60}Compton to American Philosophical Society Committee on Grants, 17 August 1939, AC4, 1/35. See Philip Morse, "Waves, Waves, Everywhere," \textit{Technology Review}, 39 (November 1936), 9-11 for his description of the intra-MIT significance of his skills.

\textsuperscript{61}See the announcements of Special Summer Program on Spectroscopy and its Applications in AC4, 1/9. At one point, Compton and Slater toyed with the idea of making Harrison director of a program in applied physics, but a distinctive curriculum did not emerge. See Compton to Slater, 23 October 1935, AC4, 1/30 and Slater, "History of the MIT Physics Department, 1930-1948," typescript in MIT Archives, 22-26.

combined studies with him and Wayne Nottingham, Compton’s former student, or the physicists retained from Norton’s administration, developed both theoretical and experimental skills for examining the electronic properties of materials and found fruitful employment at Bell Telephone Laboratories.63

Slater’s sense that quantum mechanics could be used to reorganize previously distinct specialties into a mutually enriching unit did prove well-founded, although his sense of how many specialties should be housed under quantum mechanics was, in retrospect, broader than practical.64 By the end of the decade, Slater was one of a handful of physicists who were seeking to interest metallurgists in quantum mechanics65 and to fit a quantum theory of solids into the format of an advanced textbook.66 However, his academic peers did not share his assumption that the extension of quantum mechanics’ breadth was intellectually valuable work. In the 1940s, efforts to form a Division of Solid State Physics within the American Physical Society provoked years of debate over the proper size, role, and effects of divisions that embraced broad segments of both industrial and academic interests.67 To its industrially oriented proponents, the Division would keep industrial physicists from abandoning the American Physical Society in favor of forming splinter societies. But to the Division’s opponents, a physical society’s members should be united in their concern for areas where the explanatory power of theories seemed inadequate in principle, and the proposed division seemed a step towards the "Balkanization" of physics. While eventually resolved in favor of creating a Division of Solid State Physics, the affair illustrates that good policy for physics within MIT did not necessarily make sense to academic physicists in a national context.

63Slater, Scientific Biography, 169;
64Weart, “Solid Community;”
65See Slater to Compton, 1 August 1939, AC 4, 1/57, where Slater describes the experiences of quantum theorists giving summer courses at the University of Pittsburgh.
67Ibid., 629-640.
Local Structure and the Administration of Nuclear Physics

Compton's drive to obtain philanthropic resources to support scientific research programs that integrated the range of interests within MIT fit perfectly with Slater's ambition to expand the breadth of quantum mechanics to meet the subject matters of experimentalists, chemists, and technologists. But Compton's administration of relations between MIT's faculty and the Rockefeller Foundation did not suit all specialties in physical research. Physicists more concerned with contributing to the cognitive concerns that united academic physicists than with broadening the appeal of physics to practitioners in other disciplines, and university administrators more concerned with demonstrating the competitiveness than the distinctiveness of their institutions, stressed different subject matters than did Compton and Slater at MIT. MIT physicists who wanted to compete in creating specialties that would deepen the foundations of physical theory found their opportunities limited in comparison to colleagues at other universities.

Developments at the University of Rome highlight how Compton's insistence on using university-wide administration of research to make MIT appealing by making it distinctive led to a de-emphasis of possible research directions. As was the case at MIT, at the University of Rome an accomplished physicist who had left the laboratory for administrative positions was in a position of power and determined to use his power to reinvigorate its research. However, Orso Corbino brought a much different political sensibility strategy to Rome than did Compton to MIT, and Corbino recruited a physicist with different intellectual ambitions for a different sort of position than did Compton.

In contrast to Compton, Corbino had not moved from success within a university department into university-wide administration but into national politics and policy; throughout the 1920s, Corbino served as a senator of the Kingdom and occasionally held ministerial posts. Though he was not a member of the Fascist Party, nationalist sentiments propelled his reformist activism at the University of Rome. Weighing on Corbino's mind were the absence of Italians since Volta from any list of "great" physicists, the lack of any Italian Nobel Prize winner in physics
(except for Marconi, whose work on telegraphy was not supported in Italy), and the lack of an obvious successor to Augusto Righi, who at his death in 1920 was considered Italy's leading physicist. His goal was to "recapture Italy's place in physics;" the University of Rome was his vehicle by virtue of the influence he could wield there rather than any particular character or tradition.\footnote{Gerald Holton, "Striking Gold in Science: Fermi's Group and the Recapture of Italy's Place in Physics," \textit{Minerva}, 12 (1974), 177-184.}

This nationalist conception led Corbino to characterize the internal organization of physics differently than Compton. Corbino did not need an intellectual framework that would help identify common ground among physicists with different types of employers but a framework that would help identify a future area of competition among physicists of leading nations. For Italian physics to make a "place" for itself without duplicating the infrastructure of other nations, Corbino needed Italian physicists to establish a new field of research that would branch off from existing specialties and attract an international following.

In contrast to Compton's division of physics into theoretical and applied branches, Corbino asserted that physicists engaged in three types of activities: "the discovery of new and unforseen phenomena which cannot be explained through extant theories;" "the qualitative and quantitative verification of results, derived from the prevailing theories of the period;" and "the determination of mechanical, thermal, electrical and magnetic constants of substances." The third Corbino considered well ensconced within "specially equipped laboratories" and thus not a domain in which a university could lead a nation to prominence. He placed all of what Slater encompassed under "deductive atomic theory" in his second category. While conceding that the atomic structures of liquids and solids is "a field of study whose theory is behind the times," reorganizing technical specialties to serve the expansion of theory would not make a secure place for Italian physics because, Corbino predicted, "just as acoustics (except for practical application) is now a depleted branch of science, so thermal physics, optics and the study of electricity are likewise destined
to become depleted." He concluded that "new branches of physics will not arise until one obtains possible artificial modifications of atomic nuclei" and declared nuclear experimentation the only currently possible activity that deserved placement in "the highest category of physics research." ⁶⁹

To establish this highest form of physics activity in Italy, Corbino adopted the tactics Arthur Noyes had urged on MIT's administrators. He created and protected from external incursions a new chair in physics at the University of Rome so that the promising physicist he picked to hold it, Enrico Fermi, would have a fiefdom for the pursuit of nuclear experimentation.⁷⁰ Initially, Fermi had to send his best students to foreign centers of nuclear studies and bring them back to Rome.⁷¹ But after the discovery of the neutron in 1932, Fermi "struck gold" with the discovery that a relatively inexpensive nuclear probe could be made from the copious neutrons released when paraffin was irradiated. In a "riot of discovery," Fermi led an international competition to probe each element in the periodic table, garnering a Nobe Prize for himself while contributing to a catalogue of reactions that a theory of nuclear structure should account for.⁷² Although Fermi himself left Rome after Fascist policies and Corbino's death deprived him of personal and professional security, an enduring tradition of centralized strategic investment in physical research has continued to keep Italy's "place" in physics.⁷³

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By contrast, MIT’s physicists, faced with Compton’s categorization of the atomic nucleus as just one of several theoretical specialties, could not claim the moral high ground in arguing for support of sub-atomic research. Slater later recalled

Compton and I never felt that we wanted to have the department go overboard in the direction of nuclear and high-energy physics, as so many university departments were doing. We felt that at a great institution like MIT, in which engineering and technology were associated closely with science, we should develop all the branches of physics together, emphasizing the way in which a development in pure science soon takes its place in technology. 74

And MIT’s physicists, faced with Compton’s policy of supporting specialties when they appealed to a breadth of interests, could not investigate nuclear phenomena as a specialty in its own right. When Compton brought his Princeton colleague, Robert J. Van de Graaff, to MIT in 1931 to work on his electrostatic generator as a tool for nuclear research, Van de Graaff came as a "Research Associate," not a teacher, and was given quarters not in the Physics Department but at the Round Hill estate of Colonel Green where Edward Bowles’ group pursued radio research. Bush introduced John Trump, an electrical engineering doctoral student, to Van de Graaff. Their collaboration made the generator itself an object of research in high-voltage electronics and its applications as well as a particle accelerator that could pry into atomic nuclei. 75 Between Van de Graaff’s lack of teaching, the geographic separation of his work from the main campus, and his collaboration with the electrical engineers, his presence could not make nuclear research prominent in the physics department.

After nuclear physics’ "miraculous year" in 1932, 76 Compton recruited Robely Evans to cover radioactivity in the physics department, 77 but Evans’ arrival

74 Slater, Scientific Biography, 170.

75 Wildes, Century, 160-165; Slater, Scientific Biography, 169-170.

76 1932 saw the discovery of the neutron, the positron, heavy water, and the invention of the cyclotron.

77 Slater, Scientific Biography, 171.
did not launch MIT into a full-fledged program. Evans joined three other members of the physics staff to petition Compton to involve MIT in cosmic ray research:

Up to the present time, those of us who have worked in this field have done so under the sponsorship of other Institutions, taking our ideas elsewhere for test or application. We feel that it would be more satisfactory to us and to the Institute if we could carry out these ideas as Institute projects. 78

Insofar as their proposal contained a few modest projects for the improvement of apparatus and its use in the immediate vicinity of Cambridge, Compton promised to keep the proposal in mind as the year progressed and he learned how accurate his budget planning had been. But Compton's response to their principal objectives, to make measurements of cosmic ray activity at different latitudes and altitudes, and to construct a permanent laboratory atop Mt. Evans near Denver, Colorado, was to insist they raise their own funds from outside MIT. 79 He did not even suggest the possibility of, let alone offer, support from the fluid research fund that the Rockefeller Foundation had given MIT. Compton did not discourage the proposed research on the grounds of scientific flaws, but sending physicists off to distant points to do research that would not interest others at MIT was clearly not his idea of an "Institute project."

Evans found himself the object of Compton's favor after Saul Hertz, a physician at the Massachusetts General Hospital, approached Compton in 1937 with the idea of using radioactive iodine to study the physiology of the thyroid gland. 80 Compton promptly wrote to Hertz's superior, James Means, to get Means and Hertz to meet with Evans and J. W. Horton, an electrical engineer who had been drawn into designing bio-medical instruments. Compton also contacted Frank Jewett, who was

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78 Vallarta, Bennett, Evans, and Boyce to Compton, 27 November 1934, MIT Archives, AC 4, Box 1, Folder 5.
79 Compton to Vallarta, Bennett, Evans, and Boyce, 28 November 1934, in ibid.
80 The history is given in J. H. Means to Archie Woods, 19 July 1938, copy in MIT Archives, AC 4, Box 1, Folder 33.
chairman of the Milton Fund, for help in obtaining funds to get the research started.\textsuperscript{81} Eventually, MIT successfully solicited the Markle Foundation for funds to build a cyclotron to be used exclusively for "cooperative bio-medical research."\textsuperscript{82}

In the 1930s laboratories for the investigation and use of nuclear processes usually took up work in neighboring specialties to satisfy the interests of its researchers or to broaden appeals for financial support.\textsuperscript{83} But at MIT, prospects for cooperative work with neighboring fields was a statutory pre-requisite to any work in nuclear physics. On this basis, Evans did build up a Radioactivity Center around the MIT cyclotron, but he had to do so in Sedgwick's rather than Noyes' or Fermi's spirit. Instead of concentrating on problems deemed intellectually strategic by a scientific leader responding to the efforts of international peers, the Radioactivity Center expanded to respond to cover the interests of diverse local research groups:

there are groups whose primary interest lie in radiochemistry, cyclotron [improvement], radioactivity detection, geophysics, nuclear spectroscopy, medical applications, etc. The existence of these groups working as a single cooperative unit has time and again been demonstrated to be the only method of solving complex problems initiated in one or another of the groups.\textsuperscript{84}

One member likened the Center to an amoeba. A "functional but not directional" nucleus could coordinate the laboratory's parts to send out a pseudopod. The pseudopod

may be of an exploratory nature or may carry the whole organization in the direction of stimulation. Sufficient intensity of stimulation causes a growth of the central body which reabsorbs the pseudopod .... After completion of a

\textsuperscript{81}See Compton to James Means, 7 June 1937 and Means to Compton, 14 July 1937, MIT Archives, AC4, 1/28.

\textsuperscript{82}"Cyclotron," \textit{Technology Review}, 40 (1938), 421.


\textsuperscript{84}Robely Evans, "Radioactivity Center, 1934-1945," Evans Papers, MIT Archives, MC 80, Box 1.
project this amoeba forms a vacuole, fills it with reports, and vacuates this residue of progress.85

As had been Sedgwick’s policy in building sanitary science, Evans reached out to include subjects that the Center’s nucleus could coordinate. Research on its own nucleus (i.e. atomic nuclei) was not precluded but would arise from the need to satisfy the scientists whose areas it coordinated. Thus nuclear physics we welcomed by Compton and Slater at MIT as an agent for the cooperative unification of local interests and specialties, but not as a specialty to be independently cultivated.

Conclusion

Compton and Slater operated on the premise that university administrators should manage their researchers primarily on the basis of intra-university criteria. When those criteria conflicted with or were tangential to the inclination of researchers to use their universities as a base for competing with like-minded specialists, that competitiveness was to be resisted or, more happily, encouraged as a secondary activity that could improve a researcher’s ability to contribute to the intra-university programs that were Compton’s primary priority. Thus physicists such as Harrison, Nottingham, and Warren, whose experimental specialties may not have been exciting international competition for the production of results that stretched the conceptual fabric of quantum theory, were recruited and encouraged because their specialties did seem relevant to building an intellectually manageable body of knowledge that would further the industrial ambitions of MIT’s students. Thus physicists such as Morse, Van de Graaff, and Evans, whose interests in nuclear phenomena placed them in an internationally competitive specialty of physics, were recruited and encouraged because they dedicated themselves to using the mathematical and experimental skills of nuclear physics to advance other specialties of concern within MIT.

Within MIT, Compton’s and Slater’s reform of physics was a resounding success. The department that, under Cross and Norton, had graduated few students

85John W. Irvine, in ibid., Appendix III.
and burdened faculty with heavy teaching loads in the elementary courses that other fields required, became one of the university’s more popular majors and capable of supporting several research-oriented faculty who taught advanced courses that were close to their research interests. The early 1930s saw a lull in industry’s hiring of physicists, but once that passed, MIT’s administrators could well claim that their industry-bound students were well suited to fulfilling the Rockefeller Foundation’s desiderata of stimulating a more progressive attitude in industry towards research on fundamental problems.

As important as these achievements were for the creation of solid state physics as a recognized specialty within academic physics and for the entry of more sophisticated physicists into industrial research, their significance must also be stressed as a type of research policy. For Compton, the reform of MIT’s physics department was exemplary of the proper relations among university administrators, researchers, and patrons. Throughout the 1930s, he encouraged and sought faculty whose ambitions were consonant with his desire to use the powers of his office to spur research programs that appealed to practitioners of multiple disciplines. The programs of Bush and Bowles blossomed under Compton’s abilities to attract patrons and his willingness to reward engineers who made instruments and phenomena of technological relevance the objects of scientific study and use. However, the limits to the utility of Compton’s policies are apparent in the difficulties he encountered in revitalizing MIT’s biology department. Here the kinds of tensions that he was able to accommodate in the reform of physics undermined his aspirations.
CHAPTER 7

POLAR OPPOSITES: THE EXPANSION OF ELECTRICAL ENGINEERING AND THE DEMISE OF BIOLOGICAL ENGINEERING

By the end of the 1930s, Compton could look back with satisfaction at the changes in MIT’s Physics Department and his role in them. He had made space for a new chairman while finding a useful niche for the old in the centralized administration of industrially sponsored research. For chairman, he had found a young quantum theorist with European training but intellectual horizons and social sensibilities that melded with Compton’s vision of the university as a locally organized center for cooperative research. He had convinced the preeminent philanthropic supporter of university research to grant MIT a fluid research fund that he and select administrators could dispense on the basis of local possibilities. New faculty members included some of his former Princeton associates, whom Compton sensed would find intellectual and career satisfaction within the boundaries of locally constructed research policy. The Department had also expanded into the new specialty of nuclear physics without becoming consumed and thus narrowed by the challenge nuclear phenomena posed to the principles of quantum theory.

Well before the close of the decade, Compton supported or spawned initiatives from the Electrical Engineering Department, which had provided the impetus for his recruitment as MIT’s president, and the Biology and Public Health Department, which had been MIT’s originator of Compton’s form of research policy. His tactics were variations on his approach to the Physics Department. He sought a synergy among philanthropic patrons, mid-career researchers, and himself to generate an intra-MIT reform that would differentiate MIT from other universities, provide leadership.
opportunities for the mid-career researchers, and send messages to early-career researchers about the expectations of the MIT administration and the types of accomplishments it was prepared to reward. The result was supposed to be a culture in which researchers would downplay the importance of deepening disciplinary foundations in favor of using specialization in experimental techniques, mathematical techniques, or the study of a class of phenomena to develop multi-departmental cooperative research programs.

Compton's vision smoothly turned into a reality in Electrical Engineering at MIT, but was a blind alley for Biology. The contras illuminate the limits on the appeal and practicality of his belief in locally defined research policy. In electrical engineering, the mid-career research leaders found an intellectually manageable number of interesting technical points of contact between themselves and physicists or mathematicians, and they encountered no detractors questioning whether the research was appropriate for electrical engineers. In biology, Compton and his supporters never did produce an intellectually manageable number of points of contact between biologists and other disciplines, and they never found a mid-career researcher willing to assume leadership of research that was not oriented to deepening disciplinary foundations.

Multidepartmental Centers and the Support of Bush and Bowles

The horizons for research opened up by Bush and Bowles in the later 1920s had stimulated the Department's patrons to recruit Compton for MIT's presidency. Nobody, it appears, outright told Compton to promote Bush's and Bowles's careers. He readily figured out for himself that their ambitions conformed to his ideal of cooperative research. Both wanted to exploit instrumentation with many possible uses or the scientific elucidation of technologically relevant phenomena to build local, multi-disciplinary research centers. Thus Compton neither needed nor attempted any large-scale reforms. He let Bush and Bowles proceed, either helping them to raise funds or to attract and promote talent as their situations seemed to indicate. They, in
turn, benefitted directly from Compton's policies while requiring less intervention from him on their behalf.

Bush's ambitions did not require Compton's help to attract personnel. Between the electrical engineers concerned with calculating the behavior of power networks and the mathematicians intrigued by the electro-mechanical embodiment of differential equations, Bush had talented people interested in developing the differential analyzer. However, Bush had no patron to back his efforts. From Compton, Bush learned how to frame his ambitions, which originated in problems of industrial practice, so as to appeal to the philanthropic foundations supporting university scientists.

Bowles did not need Compton's help to explain to foundations how his ambitions advanced science. The passions of Colonel Green and the interests of several federal agencies maintained funding for Bowles' efforts to expand physical theory and instrumentation to make possible directed radio beams of obvious utility for aviation. However, Bowles capitalized on Compton's conviction that research faculty be rewarded for contributions to the local community. Bowles's European-trained proteges adapted their skills to pursue research relevant to directed radio beams and were professionally rewarded when they were successful.

The advantages to Bush of Compton's presence was obvious in Bush's promotion to vice-president and the entry into research policy circles that came with Compton's imprimatur. For Bush, the arrival of Compton, Slater and the physicists they recruited was an oppo. unity, which he seized well, to find new clients for the differential analyzer and more challenges for himself and his assistants.¹ As a longstanding faculty member who found common cause with the reform-minded scientists, Bush was an obvious and excellent candidate for Compton to promote to a MIT-wide administrative position.

Shortly after Bush became Vice-President and Dean of Engineering in 1932, he and his colleagues began considering the problems and costs of a new differential

¹Owens, "Bush and the Differential Analyzer," 76.
analyzer that could be reconfigured more easily for different calculating tasks. The costs were beyond what could be obtained from adjustments in MIT’s budget. For the needed support, Bush might have solicited the producers of electrical power, whose problems originally inspired his research, or other industrial interests that the differential analyzer could serve. But as Kennelly’s student, a program builder in Jackson’s department, and Compton’s vice president, Bush had the intellectual disposition, institutional ambition, and social contacts to make development of the analyzer an object of philanthropic support to further science. In 1932, when Warren Weaver of the Rockefeller Foundation visited MIT, Bush took the opportunity to demonstrate the analyzer for Weaver. Weaver, himself a physicist and applied mathematician by training, was impressed and well understood that his impression would likely encourage a future proposal.²

Within a year, Bush informed Weaver that Sven Rosseland, one of Bush’s colleagues, was leaving MIT with the ability to duplicate Bush’s machine and with ideas for “something of a radical development.” Bush requested

that you [Weaver] and I...discuss this entire matter of the further development of this type of machine....With all due modesty, I think that this radical development should be in my own hands....[T]he best all-round procedure...seems to be for one to pioneer in a field which is expensive with the best of economy.³

Weaver, though enthusiastic about the analyzer, was initially pessimistic about the response of the Foundation’s trustees to a proposal from Bush. An award to Bush would prima facia violate the Foundation’s new policy to focus on individual researchers using physical and chemical techniques to investigate biological phenomena.⁴ As an individual researcher, Bush could not possibly qualify as an investigator of biological phenomena; and a well-argued proposal for how the

² “Excerpt from Warren Weaver’s Diary,” 21 November 1932, Rockefeller Archives, RG 1.1, Series 224D, Box 2, Folder 22.

³ Bush to Weaver, 7 July 1933, ibid.

⁴ Kohler, “Management.”
development of a new differential analyzer would advance biology could not possibly come from an individual investigator. Still, Weaver held open the hope that "there are always some loopholes in any reasonable program and I am perfectly frank to say that I am very much interested in what you are doing."\(^5\)

With the Depression eroding the Foundation's income, nearly two years elapsed before Weaver thought the time opportune for Bush to submit an anomalous proposal. Then Bush submitted a proposal unabashedly oriented to the MIT traditions he represented and with minimal regard to the Foundation's recent programs. Like Slater in his contribution to the request for a fluid research fund, Bush argued that a new field of knowledge could best be developed by establishing a center for its pursuit at MIT, because MIT possessed the personnel and the institutional flexibility to demonstrate the field's viability. Only in Bush's case, the field was analysis rather than "deductive atomic theory," and the indigenous strengths the new center would combine lay in MIT's Departments of Mathematics and Electrical Engineering rather than Physics and Chemistry.\(^6\)

Bush's proposal neatly encapsulated his MIT career. He understood that his prominence within the Electrical Engineering Department had been due to Jackson's respect for those who could build a local research community and win philanthropic support by abstracting scientific problems from engineering practice. Noteworthy by its absence from the proposal was any mention of the power-engineering problems that had initially impelled Bush to look at the circuit components his students used in measurements as potentially generalizable calculation devices. And Bush understood that his advancement to MIT-wide administrative responsibilities had been due to Compton's respect for those who could find multi-disciplinary significance in the problems and developments of their particular specialties. Noteworthy by its presence was a separate letter from Compton to Weaver expressing Compton's support for the

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\(^5\) Weaver to Bush, 10 July 1933, Rockefeller Archives, RG 1.1, Series 224D, Box 2, Folder 22.

\(^6\) Bush to Weaver, 22 April 1935, Rockefeller Archives, RG 1.1, series 224D, Box 2, Folder 22.
proposal and making clear the proposal was for a MIT center and not for Bush as an individual researcher. 7

The lone place where Bush recognized the Foundation's interests was to point out, probably at Norbert Wiener's suggestion, 8 that a new differential analyzer could support "mathematical biology as illustrated in the work of [the population geneticist, J.B.S.] Haldane." However, Sedgwick had de-emphasized evolution, genetics, and their conceptual reconciliation in his construction of "public health science" as a division of biology at MIT. 9 Nothing reminiscent of Haldane's work appeared in the proposal appendix listing the problems on which the first differential analyzer had been used.

Bush's proposal forced the Foundation to examine its framework for research policy. Compton's stress on the institutional character of the proposal called attention to the importance of a local environment for innovations that expanded the breadth of a field and the skills of its practitioners. By implication, that endorsement called into question the wisdom of supporting individual researchers whose projects were competitive within a centrally defined field of research. Weaver presented the proposal to the Foundation board "as an item of exceptional value and significance outside the Natural Sciences Program." He played up the breadth of intellectual utility that a more powerful and flexible differential analyzer would have, stressing that successful development of the analyzer "may result in striking advances in fields well within [the Foundation's] concentrated program" as well as "creates means whereby analysis in scientific fields may be extensively furthered." 10

Weaver succeeded in selling Bush's proposal as a worthy exception to Foundation policy. The Foundation granted MIT $10,000 for design and development

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7 Compton to Weaver, 22 April 1935, in ibid.

8 Wiener had struck a friendship with Haldane; see Heims, Von Neumann and Wiener, 173.

9 See Section 2.1, above.

10 "Grant Action 35098," 17 May 1935, Rockefeller Archives, RG 1.1, Series 224D, Box 2, Folder 22.
studies; when they were deemed to have gone well, the Foundation granted another $85,000 for construction of the new analyzer. In justifying the latter grant, the Foundation recognized MIT’s success in making itself into a center for analytic machinery:

There has never at any previous time been gathered in one place the variety and excellence of abilities which are necessary for the development, design, construction, and operation of such a device.\textsuperscript{11}

But the Foundation also explicitly noted the award was an exception to its established policy.

From the perspective of MIT’s history, however, proposing a Center for Analysis was anything but anomalous. The Foundation’s grant was the fulfillment of Dugald Jackson’s vision in 1910. As Jackson had hoped Pender would do, Bush had concentrated on "advanced research in scientific directions which the industries may be approaching but have not yet reached" and found colleagues in science and engineering departments with common interests in a particular direction. As Jackson had hoped he would do on the basis of the possibilities Pender would spot, Bush and Compton had won financial backing from a patron of science on the basis of the scientific merit that a MIT research center could achieve. Back in 1925, when Arthur Kennelly left MIT, Jackson had implied that MIT would be better off with a research leader who looked for topics of mutual interest to engineers and scientists rather than problems that exercised his analytic strengths.\textsuperscript{12} At least in retrospect, Jackson was not engaging in bravura; ten years later, his first home-grown faculty researcher was winning grants from a philanthropic foundation during an economic depression.

Bush’s entrepreneurship was, as Jackson had believed, a boon for Bush’s colleagues and MIT. Faculty and students concentrated on using the old analyzer, building the new, and developing other computational aids that occurred to them. Bush’s administrative duties prevented him from daily involvement in the technical

\textsuperscript{11} "Grant Action 36071," 15 April 1936, in \textit{ibid}.

\textsuperscript{12} See Section 4.2, above.
work, but his administrative position also helped him to secure for The Center for Analysis additional funding for activities that were not directly related to the Rockefeller Foundation grants. His detachment from the daily work and his decision to leave MIT altogether also provided opportunities for upward mobility based on technical accomplishment and usefulness. Harold Hazen, who had first seen the possibility of coupling integrators to solve second-order differential equations, eventually succeeded Dugald Jackson as chairman of the Electrical Engineering Department; several other of Bush's early colleagues went on to hold important positions in research and administration within MIT.\(^\text{13}\)

These successes occurred even though the differential analyzer did not become a broadly emulated instrument and the MIT Center for Analysis failed to become a self-perpetuating part of MIT's organization. The problems of making a more flexible differential analyzer proved thornier than expected, and staff members' investigations of other computational devices started to rival the development of the new analyzer.\(^\text{14}\) World War II accelerated these developments. Improving such military functions as fire-control focused researchers on the problems of quickly making many simple calculations (as opposed to evaluating algebraically intractable integrals), and government funding provided budgets that enabled researchers to develop electronic, digital computers, (as opposed to the analyzer's electro-mechanical, analog methods).\(^\text{15}\) Bush's rise to prominence and the MIT careers enjoyed by the colleagues he promoted are testament to how highly Compton and MIT as an institution valued those who could find research opportunities in engineering practice and cultivate multi-departmental relations around the several contexts to which the research opportunities seemed related.

\(^{13}\text{Owens, "Bush and the Analyzer," 76.}\)

\(^{14}\text{See ibid., 81-85.}\)

\(^{15}\text{David F. Noble, Forces of Production: A Social History of Industrial Automation (New York: Oxford University Press, 1986), 106-113.}\)
Edward Bowles had fewer initial funding problems than Bush but more personnel problems because MIT's Physics Department, before its reform by Compton and Slater, was not staffed or disposed to support the opportunities for physical research that Bowles viewed as implicit in a radio laboratory at Round Hill. Consequently, in the late 1920s, he encouraged his more promising masters-degree students, Julius Stratton and Wilmer Barrow, to take doctorates in physics at German technische Hochschule and return to the MIT staff. However, working around rather than through the Physics Department was no way, in the long run, to secure Stratton's and Barrow's careers. They prospered because under Compton, MIT's departments rewarded faculty who helped to make MIT a center for cooperative research and education. The Physics Department promoted a theorist for turning his original interests in engineering phenomena into a rigorous, advanced course in physics, and the Electrical Engineering promoted an experimentalist for turning his doctoral training in physics towards phenomena that lay within the department's technological interests.

Stratton took his Ph.D. from the Zurich Technische Hochschule in 1927, where he was guided into the study of the wave mechanics Erwin Schrödinger had proposed as the foundation for atomic physics. Stratton's dissertation used wave mechanics to calculate how the hydrogen atom ought to disperse \(x\)- and gamma rays.\(^{16}\) From the perspective of atomic physics, Stratton's work had at least three merits. First, nobody had yet done the calculation—Schrödinger having only presented a theory for the dispersion of visible light. Second, the calculation turned the (Arthur) Compton effect, a major experimental stimuli to developing a quantum theory of radiation,\(^{17}\) into a derivable phenomena. Third, the project illustrated the

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\(^{17}\)Compton's experiments showed that electrons scatter x-rays as though x-rays were discrete particles obeying conservation of energy and momentum. For the impact of Compton's experiments on the course of quantum physics, see Roger Stuewer, The Compton Effect: Turning Point in Physics, (New York: Science History Publications, 1975), 287-333.
problems of redefining such classical conceptions as particles with an electric dipole of definite dimension for analysis in wave mechanics.

Upon his return to MIT, however, Stratton did not use quantum theory's conundrums to generate research, but rather used his skills in the mathematics of wave equations. His duties were to teach the theory of electricity and magnetism and to supervise research at Round Hill. With Green showing interest in aeronautic navigation through fog, Stratton posed himself the problem of determining the effects of rain and fog on radio waves, especially at the short wavelengths that could be reflected into beams by apparatus of manageable size. Although radio waves and water droplets were many orders of magnitude too large to exhibit quantum effects that required statistical interpretation, Stratton still capitalized on the mathematics of his dissertation: both the Round Hill and his thesis work were scattering problems in which the wavelength of the incident radiation approached the size of the scattering particle. He concluded that fog and moderate rain would not affect the propagation of radio waves with wavelengths over four to five centimeters.\(^\text{18}\)

Nobody in 1930 had a way to generate radio waves of useful power below a meter in wavelength, so Stratton's paper was a theory for experiments that nobody could yet perform. Nevertheless, Stratton's work set a limit on the "lengths" to which researchers should go in the pursuit of shorter wavelengths. And Bowles had masters-degree students and research assistants working on experiments to which Stratton's skills in the mathematics of dispersion were useful. For Henry Houghton, who was studying the transmission of light through fog, Stratton worked out the theoretically-expected results of interactions between electromagnetic radiation and scattering particles of appropriate wavelengths and sizes.\(^\text{19}\) With Howard Chinn, who was using Green's coastal airfield and a donated dirigible to measure the field


intensities about a vertical antenna mounted over sea water, Stratton worked on ways to calculate the expected intensity distribution without resorting to simplifying assumptions about the conductivity of the surface under the antenna.\footnote{J. A. Stratton, and H. A. Chinn, "The Radiation Characteristics of a Vertical Half-wave Antenna," Proceedings of the Institute of Radio Engineers, 20 (1932), 1892-1913.}

To secure professional advancement within the Physics Department and to fulfill the anticipated potential of his appointment, Stratton set out to establish that his research specialty could be codified in a text whose style was based on the disciplined development of physical concepts, and whose content familiarized students with technologically relevant phenomena and formulae. For style, Stratton called on his continental European training, and used potential theory, rather than lines of force, as his starting point because potential theory could be elaborated with more mathematical rigor.\footnote{Stratton shifted his departmental affiliation from Electrical Engineering to Physics shortly after Compton and Slater took office. See Section 6.3, above.} He paid a price for his rigor in that the discussion of mathematical techniques that his approach necessitated cramped his discussion of topics of engineering importance. However, he could still claim that the text would "satisfy the needs of those who are unwilling to accept engineering formulas without knowledge of their origin and limitations." For content, Stratton called on his Round Hill experiences, concentrating on large-scale phenomena of relevance to communications technology and abandoning discussions of the atomic-level properties of matter that create electromagnetism. He had, however, an excellent justification for compromising on rigor and removing himself from one of his decade's major theoretical challenges: "Whatever form the equations of quantum electrodynamics ultimately assume, their statistical average over large numbers of atoms must lead to

Maxwell's equations." Stratton's compromises seemed most reasonable to his peers; his textbook was published in 1941, and MIT promoted him to full professor that year.

Wilmer Barrow roughly followed in Stratton's footsteps. After earning his masters in electrical engineering from MIT in 1929, Barrow attended the Munich Technische Hochschule for doctoral studies in physics. Barrow's dissertation, supervised by the acoustician J. Zenneck, involved building, investigating, and using a novel "howler"—a sound generator of constant amplitude and periodically varying frequency. He not only used his howler to demonstrate acoustical principals, but also to test and extend a theory put forth by John R. Carson, a mathematician at Bell Laboratories, for the spectrum generated by frequency-modulated waves.

Barrow's doctoral research made electro-acoustical analogies a pillar of his scientific imagination, imbued him with a short-term ambition to build up the physics of circuitry with varying parameters, and a long-term ambition to explore ultra short radio waves where "the ordinary assumptions of circuit theory based on the quasistationary conditions no longer hold. Indeed our usual conceptions of capacitance, inductance, etc. have lost their meaning." The long-term ambition fit with Bowles' goal of exploring directed radio beams. Barrow returned to MIT, and Bowles included his research program in the Round Hill budget.

Barrow promptly began experimental and theoretical investigations of circuits with varying parameters. A string of papers ensued in which Barrow presented a

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24Conventional electromagnetic howlers varied the frequency of a sound-generating circuit by mechanically rotating the plate of a variable condenser. Barrow inserted iron cores in his induction coils and varied the frequency much faster by using alternating current to magnetize and demagnetize the cores. In addition to the acoustical uses to which Barrow put his howler, he noted that circuits with periodically varying frequencies were important to wireless telegraphy and ionospheric investigations.


26Barrow to Jackson, May 1931, quoted in Wildes and Lindgren, Century, 121.
technique for analyzing the harmonic composition of his howler's output, set forth a general theory for determining the behavior of a circuit with a periodically varying capacitance, and reported experiments that tested his theory in unexplored wavelengths. Whatever the merits of this work in terms of its contribution to the physics of circuitry, they did not appear to be drawing him into studies of oscillations of interest to his Round Hill peers, they did not appear to point the way towards other programmatic interests, and he had not yet been promoted above instructor.

Under Stratton's "council," Barrow moved from the creation and study of circuits per se to the adaptation of circuit theory and experimentation for the technology with which Bowles sensed could be used for radio beams. Barrow worked towards an antenna-specific circuit theory that would improve Stratton's and Chinn's calculations for the field-intensities emitted by an antenna under real rather than ideal conditions. This work, though novel for Barrow in terms of the breadth of topics

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28Barrow, "Frequency Modulation and the Effects of Periodic Capacity Variations in a Nondissipative Oscillatory Circuit," ibid., 21 (1933), 1182-1202. Previous treatments of circuits with periodically varying capacitance had imposed simplifying restrictions on the relationship between the frequency of the condenser's variations and the natural frequency of the circuit.

29Barrow, "On the Oscillations of a Circuit Having a Periodically Varying Capacitance," ibid., 22 (1934), 201-212.

30Barrow's line of research could conceivably have launched him into a consideration of frequency-modulated broadcasting. But in the early 1930s, Carson's theory for the spectrum generated by a frequency-modulated wave showed that frequency-modulated broadcasting, at frequencies that could then be generated, would only further clog rather than decongest the broadcast spectrum. See, John R. Carson, "Notes on the Theory of Modulation," ibid., 10 (1922), 57-64. Only as improvements in vacuum-tube oscillators extended the technically feasible broadcast spectrum to higher frequencies, where there was more "spectral space," did researchers investigate and find the advantages of frequency-modulated broadcasting. See John R. Carson and Thornton C. Fry, "Frequency Modulation," *Bell System Technical Journal*, 16 (1937), 513-540.

on which it impinged,\textsuperscript{32} still revealed technical continuity with his experimentation in circuitry. He recognized that he could calculate the impedance of an antenna on the basis of properly spaced measurements along the antenna's transmission line when the applied voltage came from a howler.\textsuperscript{33}

By getting Barrow to define circuit-theory problems in the context of Round Hill technology, Stratton inadvertently but felicitously also revived Barrow's interest in shorter radio waves. The proper spacing for the measurements from which Barrow could calculate antenna impedance was proportional to the wavelength of the applied, howling voltage; only at the short wave-lengths relevant to directed radio waves, Barrow realized, could the measurements be spaced closely enough to be frequently practical.\textsuperscript{34}

With Barrow's attention drawn to the generation and transmission of shorter waves, his doctoral training in physics enabled him to spot the practical significance of an obscure theoretical calculation. He was generally familiar with the work of the English physicist Lord Rayleigh, who had been among the first to use acoustics as a guide to deriving the properties of electromagnetic waves.\textsuperscript{35} Rayleigh had shown that a dielectric-filled cylinder carved out of a conducting block would perfectly transmit waves shorter than a cut-off wavelength proportional to the cylinder's radius. For the wavelengths and uses that the Round Hill researchers were contemplating, Barrow realized, the size of cylindrical wave guides would be practical. Using air ducts and the latest in vacuum-tube oscillators, from which he obtained waves as short as 38 centimeters, Barrow excited waves in the ducts, confirmed their nearly

\textsuperscript{32}His bibliography, in contrast to all his previous articles, included the caveat "no pretense of completeness."

\textsuperscript{33}Barrow, "Measurement of Radio-Frequency Impedance with Network Simulating Lines," \textit{ibid.}, 807-826.

\textsuperscript{34}Barrow did work out how an artificial transmission line could be designed so that measurements could be made on a compressed spatial scale.

\textsuperscript{35}R. B. Lindsay, "John William Strutt, Third Baron Rayleigh," \textit{Dictionary of Scientific Biography}, vol. 13, 100-107, especially 101-102. Barrow's publications include several references to Rayleigh's work.
attenuation-free transmission, and, following the acoustical analogy, terminated the cylinders in "horns" that "blared" the radiation in a desired direction.\textsuperscript{36}

In 1936, the year of his discovery of microwave transmission through hollow pipes, Barrow was promoted to assistant professor. 1936 was also the year Bowles' long-time benefactor, Edward Green, died. Green's declining fortunes in the Depression had already prompted Bowles to interest government agencies in supporting MIT's work to develop systems for alleviating the dangers of flight in fog. Green's intestate death made Barrow's work all the more timely and valuable. In 1937, Barrow's transmission system became the foundation for Bowles' successful petition to the Sperry Corporation and the Bureau of Air Commerce to support the development of a blind-landing system; Barrow himself led the research and development effort.\textsuperscript{37} That support held the Round Hill group together until Karl Compton found a new source of philanthropic support.\textsuperscript{38}

Bush and Bowles, starting from an engineering department, had created the conditions that had been the stuff of William Sedgwick's dreams. They each spotted strategic sites at which scientific and engineering streams could converge—in Bush's case, computation of algebraically intractable integrals in engineering and scientific contexts; in Bowles's case, the elucidation and use of the optical character of radio waves. They each won philanthropic support for a laboratory to develop the hybrid specialty under university administration and thus as a branch of knowledge. They each attracted people from more than one disciplinary department to carve out teaching and research niches within the laboratory's structure. And they each obtained sufficient social space to advance the careers of talented staff who came to understand what kind of accomplishments were sought. Furthermore, Bush and


\textsuperscript{38}See Section 8.2, below.
Bowles never felt obliged to justify the research they fostered as "engineering" in the way Sedgwick felt he had to promote "public health science" as deserving the name science. While their researches were not seeking to develop a taxonomy of industrial practice in electrical manufacture in analogy to William Walker's model of chemical engineering, their creation of new specialties by combining scientific and engineering interests within a university center was sufficiently distinct from Arthur Noyes' stress on departmental research laboratories focusing on problems with the most cognitively powerful theories that they did not fear appearing too scientific for engineering tastes.

Underlying Bush's, Bowles's and Slater's successes were Compton's conception of cooperative research as essential to a university's social mission and the health of national scientific communities, the authority he claimed for the presidency within MIT, and the relationships he sought with philanthropic patrons of MIT. Bush, Bowles, and Slater all understood that the support they enjoyed from Compton was based on the congruence of their ambitions with Compton's understanding of cooperative research. Anyone, such as the physicists desirous of a cosmic ray observatory or Wilmer Barrow as he explored esoteric circuits in isolation from Round Hill interests, desirous to do research of interest only to disciplinary peers learned that Compton would not use his persuasive powers or administrative authority on their behalf. The grants that supported the intra-MIT organizational experiments of Bush, Bowles, and Slater were for the development of MIT, not for the way their individual research projects fit a patron's national program. All three individuals and their programs are testaments to the kinds of accomplishments that Compton wished to stimulate and reward. Their research centers made MIT a distinctive university that Compton could promote on the basis of its local character.

Conceptual Incoherence and the Demise of Biological Engineering

Compton's choice of Samuel Prescott as Dean of Science indicated Compton's appreciation of Sedgwick's approach to the formation of research specialties and professional training. However, Compton's appreciation did not change the fact that Sedgwick and his successors had been unable to tap philanthropic enthusiasm for
public health science. With the Rockefeller Foundation directing its research funds to individuals pursuing philosophically reductionist approaches to biological topics, Compton would have been remiss had he not tried to enlist the Foundation in a reassessment and recapitalization of MIT's Biology Department. However, in launching a program that both held to his and the Biology Department's faith in the value of locally designed and administered cooperative research and appealed programmatically to the Rockefeller Foundation, Compton encountered difficulties that exposed intrinsic weaknesses in his approach to research policy.

Compton's attempt to revitalize the Biology Department was an awkward hybrid of his successes with physics and electrical engineering. As was the case with physics, Compton wanted to bring in fresh leadership, and Prescott's plan to retire at age 70 in 1942 created an automatic opportunity for redirecting the department. However, the department's legacy of innovation in research and curricula encouraged Compton to tap the extant staff, as he had in electrical engineering, to help plot a new departmental direction. Compton ended up seeking outside leadership for a program that was outlined in-house without the buffer, as he had had in physics, of being able to recruit new leadership on the strength of his reputation. This situation created a more direct confrontation between local initiatives and national norms than had occurred in reforming the Physics Department or building up the Electrical Engineering Department's research program.

To interest the Rockefeller Foundation in MIT's Biology Department, Compton needed to address the unpleasant history over the endowment and policies of the Harvard School of Public Health. For a time, Compton toyed with making a direct assault on the public health issue. In 1935, some of MIT's alumni were eager to organize a study of public health training in the hope of defining and calling attention to a niche that MIT could well fill.39 Compton wondered whether the enthusiasm for such a study could become the basis for a joint consideration of the

39See Allan W. Rowe to Compton, 19 February 1934; Prescott to Compton, 15 March 1934; Compton to Rowe, 20 March 1934; and Rowe to Compton, 21 March 1934. All in MIT Archives, AC4, 1/18.
proper roles of the two schools in public health training and research. However, the level of invective that the subject of MIT-Harvard relations inspired in MIT's public health circles convinced him that he could find no way around "the difficulty of eliminating collegiate political influences from coloring either the report or its subsequent administration."\textsuperscript{40} In 1937, when he broached the idea of a joint study with Harvard's president, James Conant, he also recommended waiting "until those individuals most actively concerned in establishing the programs in the present schools shall have retired."\textsuperscript{41} By then, he had prospects for reinvigorating biology at MIT while sidestepping older public health issues.

Concurrent with learning of his Biology Department's resentment of Harvard and the Rockefeller Foundation, Compton also found that the Department had been groping towards ways to reconfigure its teaching and research. Prescott had long plugged for developing biochemistry in the context of assisting the technology of food preparation.\textsuperscript{42} In 1926 he tried (unsuccessfully) to spur Samuel Stratton into efforts on the department's behalf by arguing that "one of the agencies most necessary for developing a broader acceptance of the importance of the technological aspect of biological training is the extension of research."\textsuperscript{43} In the later 1920s, the Department hatched a "Proposed Option in Sanitation or Public Health Engineering" that would increase the amount of biology relative to civil engineering in a sanitary engineer's training and would make engineering, as well as the "scientific and

\textsuperscript{40}See Compton, "Notes on a Conference with Dean Prescott," 2 March 1936, MIT Archives, AC4, 74/34.

\textsuperscript{41}Compton, "Memorandum: Discussion with President Conant," 1 April 1937, MIT Archives, AC4, 1/24.

\textsuperscript{42}See, for example, Prescott to the Administrative Committee, 5 October 1922, MIT Archives, AC13, 16/456.

\textsuperscript{43}Prescott to Stratton, "A Sketch of the Work of the Department of Biology and Public Health," circa 1926, MIT Archives, AC13 16/457.
administrative aspects of public health" a professional course for the department's students.44

A beneficiary of the Department's attempts to diversify was John Bunker, who joined the department as an assistant professor in 1921. Bunker had taught sanitary biology at Harvard from 1911-1915, during the height of Sedgwick's influence over the MIT-Harvard School of Public Health, and had directed the bacteriology laboratory for the Digestive Ferments Company.45 His appointment at MIT, however, was in physiology and biochemistry, not bacteriology. He took his new teaching responsibilities and institutional affiliations as an opportunity to learn and develop a new research technique. With the cooperation of Donald Stockbarger, a physicist interested in optics, Bunker explored the use of light as an experimental probe and therapeutic tool.46

By the time Compton took office in 1930 and began soliciting research plans from department chairmen for the proposal that won fluid research funds from the Rockefeller Foundation,47 Bunker's research merited its own explanatory memorandum in the report Prescott submitted for the Biology and Public Health Department.48 The following spring, Bunker was recommending through Prescott

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45 His research before joining MIT had been focused on the diphtheria bacillus. See his string of reports in the Scientific Proceedings of the Society of American Bacteriologists for 1916, 1917, and 1918.


47 See Section 6.3, above.

48 Bunker, "Memo in Explanation of Item 1, preceding page," attached to Prescott to Compton, 28 October 1930, MIT Archives, AC4 1/17. No other biologist attached an explanatory memo. Bunker was especially hopeful of piquing Compton's interest in developing a quartz monochromator of unprecedented size to be used both for irradiating animals with ultra-violet light and for physical
the establishment of a Division of Research in Biology with liaison responsibilities assigned to members of the Physics and Chemistry Departments.\textsuperscript{49}

The Biology Department was not Compton's first priority in 1930, and he took no action on Bunker's request for a departmental research division. However, the existence of a biologist who had directed industrial research and built cooperative relations with physicists and chemists lodged in Compton's mind as he contemplated the Biology Department's prospects. In 1936, Compton enlisted Bunker in the creation of another proposal that asked the Rockefeller Foundation to provide $2.75 million to endow an intra-MIT reform centered on the Biology Department.\textsuperscript{50}

In this proposal, Compton had three desiderata. First, he sought to reconfigure the Biology Department's strengths in a rubric that outflanked the earlier debate over the relative importance of medical and technological training for public health professionals. Second, he sought to expand the sphere and influence of his views on the value of cooperative research and activist local administration for universities. And third, he sought to shift the Rockefeller Foundation's attention from cognitive and methodological to organizational issues in the development of biology.

Compton took care of his first desiderata in the proposal's stated title and objective: "to establish a new art of Biological Engineering...developed jointly through programs of education and research."\textsuperscript{51} The title was an obvious generalization on "public health engineering," and indeed the field was deemed to exist "in fact, if not in name," in several areas that MIT was already covering,

\textsuperscript{49}Bunker to Compton and Bunker to Prescott, 6 May 1931, MIT Archives, AC4, 1/17. Bunker had also begun working with the organic chemist Nicholas Milas, who came to MIT in 1928.

\textsuperscript{50}Compton to Rockefeller Foundation, 11 February 1937, MIT Archives, AC4, 75/44. $750,000 was for construction of a new laboratory, and $2 million for a research endowment. Bunker's name does not appear on the proposal, but his contributions are obvious, and he was co-author of a published article that presented the proposal.

\textsuperscript{51}Ibid.
including sanitary science, food processing, nutrition, and therapeutics. By stressing how the Department had diversified since Sedgwick’s death, yet claiming the Department retained an underlying unity that ought to be built on and upheld as an example to others, Compton could hope to undercut any preference to use comparison with Harvard’s public health school as the context in which to judge the value of MIT’s Biology Department.

The proposal potentially expanded Compton’s influence by applying his views on specialization to new areas of science (and thus seeking to convince another disciplinary community of their value). It called more pointedly for what Slater, Bush and Bowles were practicing:

first, [for] the continual attention of a very wide group of creative scientists and engineers to the possible biological and medical applications of the products of their researches...; second, [for] sustained cooperative research between biologists and other scientists; third, [for] cooperation with medical and industrial agencies...; fourth, [for] a course of training...to give students the preliminary knowledge, viewpoint and techniques which will render them most useful in this borderline field.

Their justification for a biology program that was built around local cooperative possibilities were the views Compton had staked out a decade earlier—specialization had proceeded far enough in biology to make convergence through cooperation with physics, chemistry, and engineering both possible and desirable.

Compton invited the Rockefeller Foundation to focus on organizational over cognitive issues by touting the proposal as a "third avenue" to progress to bringing physical and chemical techniques into biological research. He was careful not to

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52Ibid. Exhibit A, 5.

53The title also had administrative significance; though Prescott was Dean of Science, Bush was Dean of Engineering. It was Bush, not Prescott, who participated in the important discussions with Warren Weaver and James Conant that subsequently took place, though Prescott was sent carbon copies of most correspondence.

54Compton to Rockefeller Foundation, 11 February 1937, MIT Archives, AC4, 75/44.

denigrate the first two, which he labeled the "direct approach through biology, physiology, and medicine" and the "second avenue through biophysics and biochemistry as tools operated and controlled by medical or biological groups,"56 because that would have implied criticism of prior Rockefeller Foundation policy. He argued that the reductionist epistemology underlying the Foundation's policies were compatible with the sociological novelty of pooling disciplines within in Biological Engineering:

Living matter and non-living matter have in common a structure of matter, molecules and energetics....No boundaries can be drawn to delimit sharply the fields of science; ...the names which man has attached to the centers of interest in each field are for convenience in description and reference, and do not represent individual factual entities.57

But a third avenue implied the need for a different standard of judgement of value. Compton was again inviting the Foundation to stop asking which scientists had the best research projects for furthering a shared cognitive goal and instead to ask which university had developed a local capability for reorganizing research specialties in a way that held promise of greater-than-local significance.58

On the assumption that the Foundation would accept the implied criteria for judging the proposal, Compton claimed MIT was the best place to begin Biological Engineering because "the necessary beginning elements are already at hand" and could readily be programmatically brought together because "the barriers of lack of mutual interests which too often separate experts in different specialties have largely broken down."59 To substantiate these assertions, Bunker pulled together a list of research projects—ranging from the use of artificial isotopes as a research tool in physiology, to means of controlling various pests, to the development of instruments

56Compton to Rockefeller Foundation, 11 February 1937.
58See Section 6.3, above.
59Compton to Rockefeller Foundation, 11 February 1937, Exhibit A, 1, 6-7 for each quote.
useful to biological measurements—\(^{60}\) that purportedly combined challenges to biological, physical, and engineering insights. And Bunker sketched out a five-year biological engineering curriculum that featured a full load of courses in basic mathematics, physics, chemistry, and engineering along with four semesters of traditional biology in the first three years and divided the last two years equally between specialized course in particular biological and physical phenomena.\(^{61}\) To demonstrate that these ideas were not the concoction of physical scientists who were naive when venturing into biology, Compton solicited letters of support from three leading biologists: Edward Conklin of Princeton, Walter Cannon of Harvard Medical School, and Frank Lillie of the University of Chicago.

Weaver had become familiar enough with Compton’s and MIT’s proposal style to provide constructive criticisms to improve its prospects. He understood that Compton and Bush wanted the Rockefeller Foundation to create a permanent, local center from which biological engineering would spread, and warned Compton, in October 1937, that the proposal could not succeed if judged by comparing the appeal of its suggested research projects to those proposed to the Foundation from all over the nation. To convince the Foundation to fund a local center to demonstrate the value of an intra-university reorganization, Weaver thought Compton had to boost the impressiveness of a Cambridge center by including Harvard.\(^{62}\)

Compton and Bush had already opened discussions of MIT-Harvard relations in biology with Harvard’s president James Conant.\(^{63}\) Conant was sympathetic and open to cooperation, but found his faculty willing only if MIT narrowed its definition

\(^{60}\)Ibid., 14-30.

\(^{61}\)Ibid., 33-34.


\(^{63}\)Compton, "Memo of Discussion with President Conant," MIT Archives, AC 4, Box 1, Folder 24.
of biological engineering. With Weaver's encouragement as a prod, Compton pursued the matter, but to no avail. When Harvard's draft of a joint application restricted biological engineering at MIT to "instruction and research in the training of engineers to design and operate industrial plants and machinery involving biological processes," negotiations fell through.

Harvard's definition of biological engineering echoed William Walker's views on chemical engineering, implying that MIT should seek out the "unit operations" of biological manufacture and leave laboratory research into biological principles to Harvard. To boost MIT's credibility as an incubator of curricular innovations, Compton had invoked Walker and chemical engineering in the proposal to the Rockefeller Foundation. But Compton had no taste for seeking funds by abandoning his vision of MIT as a "super-technological school" to which basic research was endemic, undercutting his claim that cooperative research was an antidote to over-specialization within disciplines, and denying himself the use of his administrative powers to create working relations among faculty with contrasting cognitive interests but common technical needs. In March 1938, Compton told Weaver there were no foundation for a joint MIT-Harvard proposal, and Weaver, responded that the absence of cooperation with Harvard, further erosion of Foundation income, and a decision to act favorably on a longstanding application for capital support for the

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64 Conant to Compton, 18 June 1937, MIT Archives, AC 4, Box 57, Folder 34.

65 Compton to Conant, 18 October 1937, in ibid.

66 "Proposed Joint Application," 9 November 1937, marked informal draft and initialed by Conant, MIT Archives, AC 4, Box 1, Folder 24, and Box 57, Folder 34.

67 See Section 2.3, above

68 "Excerpt from Warren Weaver's Diary," 4 March 1938, Rockefeller Archives, RG 1.1, Series 224D, Box 1, Folder 14 contains Weaver's recounting of what Compton told him of the MIT-Harvard negotiations.
University of Chicago’s biology program, together made MIT’s request for capital support inviable.  

As he had when the Foundation rebuffed his initial request for an endowment for the cooperative research possibilities opened by the reform of MIT’s Physics Department, Compton did not let the matter die. Instead, he scaled back the request to $30,000 per year for five years to support yet-to-be-named appointees in biophysics and biochemistry. As with Bush’s unorthodox request for funds to develop an MIT Center for Analysis, Weaver remained alert for an opportunity to make another exception to standard Foundation policy. The opportunity did not come in 1939, as Rockefeller Foundation income was anticipated to hit an all-time low, but in February 1940 Weaver informed Compton that he would push the application. The problem, Weaver stated, would be "to convince my colleagues that this is not simply a proposal to strengthen a biology department...[but] a long-term program of steady and close coordination of many resources of the Institute...under an able biological research group."  

That announcement sent Bunker and Compton scurrying to recruit an "able biological research group." The task turned out to be anything but straightforward because of the social and philosophical significance of "under." Weaver sensed sociological danger and warned Compton to appoint scientists of "unquestioned standing among their biological colleagues" in order to minimize the danger that the biological fraternity treat the development of 'biological engineering' in a technological institute as something possibly a

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69 Weaver to Compton. 9 May 1938, MIT Archives, AC4, 57/34.

70 See Section 6.3, above.


72 Weaver to Compton. 11 April 1939, MIT Archives, AC4, 57/35.

73 Weaver to Compton. 2 February 1940, Rockefeller Archives, RG 1.1, Series 224D, Box 1, Folder 16. My emphasis.
little queer, a little strangely outside of their own historical framework of ideas and organization."\textsuperscript{74}

From Compton’s perspective, however, the virtue of intra-institutional cooperative was to broaden and reconfigure disciplinary boundaries to embrace a more socially useful range of phenomena. He conceded to Weaver the importance of appointing highly reputed biologists, but insisted that MIT’s program had to go beyond biologists’ historical framework: "If we did not do this to some extent, we would have nothing of particular value to contribute."\textsuperscript{75}

Frank Lillie, one of the biologists from whom Compton solicited an endorsement for the original proposal, sensed philosophical insensitivities in Compton’s conception. While granting Compton’s point that "living and non-living matter have in common a structure of matter, molecules, and energetics," he dismissed reductionism and the implication that scientists without a background in biology could be equal to biologists in research into biological phenomena:

\[ \text{[T]he functional interrelations of all the mechanisms that we discover [in living beings] are difficult to formulate in terms of matter, molecules, and energetics, so that biology has laws of its own that require to be studied at their own level, and this should never be left out of account in the organization of Biological Engineering. This will be almost automatically taken care of if the personnel is properly selected.}\textsuperscript{76}

Lillie’s criticism was intended to be constructive, but he had put the cart before the horse. The issue was not whether a biologist would add a cognitive perspective to the Biological Engineering program, but whether the MIT program could attract a leading biologist.

Compton was comfortable with the challenge of finding highly-reputed biologists to develop Biological Engineering through cooperative research that would more stretch than deepen biology’s framework. He had obtained Slater and others to reform the Physics Department on such a basis, and found the Electrical Engineering

\textsuperscript{74}Weaver to Compton, 11 April 1939, MIT Archives, AC 4, Box 57, Folder 35.

\textsuperscript{75}Compton to Weaver, 14 April 1939, in \textit{ibid}.

\textsuperscript{76}Lillie to Compton, 4 February 1937, MIT Archives, AC 4, Box 57, Folder 34.
Department was promoting faculty interested in building research programs with physicists. However, in reforming the Physics Department, Compton and Slater could point to quantum mechanics and spectroscopy as providing a foundation for cooperative research that would unite specialists dealing with the properties of solids and molecules. Compton and Bunker had nothing so specific to back up the truism about living and non-living matter having a common structure.\(^77\)

Compton and Bunker first tried to interest two major figures: the Hungarian Nobel Prize winner Albert Szent-Györgyi and Detlev Bronk of the University of Pennsylvania. Conditions in Europe made Szent-Györgyi ripe for recruitment in 1940. Fascism had already decimated the remarkable intellectual community that had developed in Budapest in the early twentieth century,\(^78\) and war was obviously going to make scientific activity even more difficult. Still, Szent-Györgyi did not snap at MIT's offer but indulged in a glut of soul-searching over whether he should move to continue his scientific work or stay to uphold the scientific traditions he had helped to set in Hungary. At one point, Szent-Györgyi wired acceptance of an MIT offer, but reneged when he could not leave Hungary as planned.\(^79\) He weathered the war in Hungary and used MIT's standing invitation to lecture to obtain an American entry visa. In 1947 he did emigrate to the United States but never joined MIT or any other university, preferring to try to recreate the research institute he had commanded in Hungary.\(^80\)

\(^77\)The lack of cognitive unity from biological laws for MIT's plans was especially noticeable in Compton, "Possibilities in Biological Engineering," which degenerated into a list of physical techniques that could prove useful in a specialized biological investigation.


\(^79\)See Bunker, "Memo of Conversation with Compton and Weaver," 10 February 1940, and Bunker's correspondence with Szent Györgyi from February to April 1940, all in MIT Archives, AC4, 57/36.

Compton and Bunker were no more successful in enticing Bronk, who was the ideal person for MIT. Bronk’s own education included graduate work in electrical engineering, physics, and biology.\textsuperscript{81} His research on conduction of impulses through nerves valued both precision in physical technique and awareness of biological functions.\textsuperscript{82} And in the name of the unity of knowledge, he had been urging the institutionalization of biophysics so that physicists would have career incentives to undertake cooperative research with biologists and so that students could simultaneously pursue training in biology and physics.\textsuperscript{83} Bunker sounded out Bronk in Philadelphia, and after Bronk visited MIT Compton offered him a full professorship plus a broad hint that Bronk would become chairman of the department after Prescott’s retirement in 1942.\textsuperscript{84}

Prospects may have looked good to Compton and Bunker at this point, but they quickly soured. Bronk wanted to hold together his research group and to teach physiology to medical students; he also had offers from Cornell’s and the University of Chicago’s medical schools, and Pennsylvania was trying to retain him.\textsuperscript{85} Compton wanted Bronk to lead MIT’s extant staff; and MIT had no medical school. Furthermore, Compton could not assure Bronk that MIT would have Rockefeller

\textsuperscript{81}Frank Brink, Jr., "Detlev Wulf Bronk." \textit{Biographical Memoirs of the National Academy of Sciences}, 50 (1979), 7-21, details Bronk’s education.

\textsuperscript{82}Ibid., 21-6.

\textsuperscript{83}Bronk, "The Relation of Physics to the Biological Sciences," \textit{Journal of Applied Physics}, 9 (March 1938), 139-142. Compton and Bunker approvingly cited Bronk’s paper in "Genesis," 9. Bronk also dismissed Lillie’s concern for biological laws governing biological organization and adopt a strictly reductionist view. Bronk saw only three branches of biology: anatomy, biochemistry, and biophysics. "On first thought it may appear necessary to add another category to include problems of organization .... One can readily show, however, that all of these problems ultimately depend on anatomical arrangements ... and upon physical and chemical processes." Quote from p. 140.

\textsuperscript{84}Bunker to Weaver, 21 March 1940, Exhibit G: "Data Concerning our Interviews with Dr. Bronk," Rockefeller Archives, RG 1.1, Series 224D, Box 1, Folder 16; "Excerpt from Warren Weaver’s Diary," 27 February 1940, same place.

\textsuperscript{85}Brink, "Bronk," 26-30.
Foundation support because Weaver refused to act in advance of Bronk's decision. Bronk opted to fit into an established framework rather than assume the risks and potential of leading a department in transition.

Compton may have been disappointed at failing to land Bronk or Szent-Györgyi, but the well-established physicists he had wanted to lead MIT's Physics Department in 1930 had also refused. What distinguishes Biological Engineering was Compton's inability to convince a rising star—the equivalent of John Slater—to view an MIT program as the way to make a mark on science. The younger scientist Weaver most highly recommended to Compton was Glenn Millikan (b. 1906), son of the physicist, who had the imprimatur of the leading British physiologists. Millikan was an assistant to Bronk, and Bronk's decision "cleared the field" for Millikan, who visited MIT in March 1940. Compton wrote Weaver that Millikan's reaction was

entirely favorable with one possible reservation as follows. He wonders whether association with a large and active group of men in biology and in other departments, who are interested in practical applications of science, will...wean away his interest in theoretical biology.... Millikan raised the above point at luncheon with a group of men like [Robely] Evans, [John] Trump, and [George] Harrison from other departments, who were all unanimous and enthusiastic in their testimony that they had found contact between theoretical and practical science to be mutually beneficial to both groups.

Compton stated that Millikan would likely take the MIT job.

Two days after writing Weaver, however, Millikan indicated that what Compton called an "entirely favorable reaction" had qualifications and that what

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86Weaver to Compton, 8 March 1940, Rockefeller Archives, RG 1.1, Series 224D, Box 1, Folder 16. Weaver refused to force the trustees to vote on MIT's proposal because Bronk's administrative superior at the University of Pennsylvania, A. Newton Richards, was a trustee.

87See Section 6.2, above.

88See Bunker's notes on a meeting with Compton and Weaver, 10 February 1940, MIT Archives, AC 4, Box 57, Folder 36.

89Compton to Weaver, 12 March 1940, in ibid.
Compton called "one possible reservation" was a grave doubt. For Millikan, the professional advance the MIT job presented was tempered by questions concerning his ability to create "a biological atmosphere I would like to work in." The virtues of cooperative research with science and engineering departments were tempered by his desire to establish "that from my point of view as a physiologist...I would necessarily want to regard them [physicists, chemists, and engineers] as servants, not masters." Millikan balked at association with MIT staff investigating applications because he wanted to work within administrative boundaries that coincided with his intuitive sense of the field's cognitive development.

I very much need the stimulus of a group about me working along lines...which are going to overlap [with mine] in their more distant ramifications. Now it just so happens that of the examples of cooperations between the physical and chemical sciences on the one hand and the biological sciences on the other which were given in your prospectus, there didn't happen to be a single one which gave me the feeling of leading into an overlapping field.\footnote{Millikan to Compton, undated but received 14 March 1940, in \textit{ibid}.}

Cooperative research, in Compton's view, revealed the unity of knowledge obscured by specialization within disciplines. Millikan viewed the cooperation envisioned in biological engineering as leading to an eclecticism that would dissipate his efforts in a series of cognitively unrelated projects. He never did overcome his misgivings and eventually turned down MIT.

Weaver, in the meantime was pushing forward MIT's proposal on the basis of Compton having opened negotiations with Szent-Györgyi and Millikan. He warned Raymond Fosdick, president of the Foundation, that a "somewhat unorthodox" proposal "to develop a new discipline and a new profession" would be receiving the endorsement of the Natural Sciences Division. Weaver's enthusiasm was based on two premises. Weaver touted the novelty of MIT's proposal, noting that at the California Institute of Technology—the only other American institution that Weaver considered capable of such a proposal—biology, though distinguished, "has developed along entirely classical lines, and almost wholly without reference to the setting in a
technological institute." And Weaver touted MIT's proposal as the first recognition of the need to create departmental career paths for the scientists being produced as a result of the Foundation's patronage of individuals interested in the borders between biology and other fields: "If it succeeds—and there is every reason to think that it will—it will undoubtedly stimulate and inspire further recognition of biophysics elsewhere." Lost in Weaver's reading of the proposal and his predilection to substitute "biophysics" for "biological engineering" was Compton's claim that a programmatic pooling of biologists with engineers and physicists would open "a third avenue" to progress in biology.

In April 1940, the Foundation made an even larger award than MIT had requested ($200,000 to be spent over 7 years). The justifications followed the case Weaver made to Fosdick. The Foundation's research policy "has not been fully or satisfactorily reflected in biological training nor in the academic organization which so intimately affects the careers of biologists." MIT's program looked like a step in the right direction.

Compton no doubt found satisfaction in having buried the unpleasant history between MIT's biologists and the Rockefeller Foundation by expanding the department's steps towards diversification into a revival of Sedgwick's ideals for forming new specialties by recombining extant ones with recent research results. He could also have congratulated himself for getting the Foundation to make room for an intra-university innovation in a program to support researchers working within the Foundation-mandated fields of interest. However, there remained unresolved tension

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91 "Weaver to Fosdick, 23 February 1940, in ibid. In The Molecular Vision of Life: Caltech, the Rockefeller Foundation and the Rise of the New Biology, (NY: Oxford University Press, 1993), Lily Kay stresses the importance of the "cooperative ideal at CIT (15-16 and 68-70). But she also points out that the appointment of Morgan to lead CIT's Biology Division was "paradoxical" given CIT's leaders' stress on biology's service roles to medicine and agriculture. She stresses Morgan's rhetorical embrace of physical and chemical approaches to biology, but also the shallowness of his knowledge (77-99). Most tellingly, for the limited scope and meaning for cooperation at CIT, CIT launched no collaborative projects involving its chemical and biological laboratories, despite the prominence and biological interests of Linus Pauling (147).

92 "Grant Action 40039," 3 April 1940, in ibid., Folder 13.
over whether the purpose of MIT's program was to recast biology's boundaries by reorganizing relations between biologists and physical scientists and engineers (Compton's third avenue to progress) or to provide a university department for scientists hoping to recast biology's conceptual foundations by introducing physical techniques. That issue remained alive in Compton's quest for a leader and administrator for Biological Engineering.

When the Rockefeller Foundation acted in April 1940, Compton believed Szent-Györgyi and Millikan would join the faculty.\(^93\) When neither showed, MIT had a grant in hand but nobody to spend it on.\(^94\) Not until November, 1940 did Compton find someone to relieve Bunker, who had become dean of the graduate school, and to absorb the Rockefeller money: the Washington University physiologist Francis Schmitt. To land Schmitt, Compton had to offer a salary of $10,000 (25% more than he offered Szent-Györgyi, the Nobel laureate) had to promise to find a position for Schmitt's younger brother in the physics or electrical engineering departments, and to end MIT's involvement in public health.\(^95\) Compton had been independently considering dropping public health.\(^96\) But because Biological Engineering was the product of the philosophy that had led to the emphasis on public health in MIT's Biology Department program and a product of the administrative structure that Compton had built for launching Sedgwick-style initiatives, Schmitt's insistence on excluding public health did not bode well for Schmitt assuming Compton's views on academic leadership.

Schmitt's tenure at MIT started smoothly enough. Compton terminated MIT's public health training in 1942, the year Prescott retired, but retained Prescott's Food

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\(^93\) Compton, "Memorandum of Conversation with Dean Prescott, Professor Bunker, and Mr. Killian re Administrative aspects of Biological Engineering," 8 April 1940, MIT Archives, AC 4, Box 57, Folder 37.

\(^94\) A condition of the grant was that it not be spent on people or resources to which MIT was already committed.

\(^95\) Compton to Schmitt, 4 November 1940, ibid., Box 38.

\(^96\) See Section 3.2, above.
Technology program, which even critics of MIT's involvement in public health had singled out for praise. Schmitt expressed satisfaction with Compton's arrangements to a Rockefeller Foundation program officer and started into research that neatly bridged wartime needs, theoretical concerns, and physical techniques—by studying the protein which makes up connective tissue through x-ray diffraction and electron microscopy, Schmitt hoped to produce insights useful in the treatment of wounds and crucial to theories of protein structure. The Rockefeller trustees were happy that MIT's biology students would complete their degrees with training in physical techniques and would not have to learn such techniques haphazardly as the need arose.

However, it was not long before Schmitt bridled against the stamp that Compton and MIT had placed on his department. Late in 1944, Compton told a Rockefeller Foundation officer that Schmitt was dissatisfied with the prominence of applied work in Biological Engineering and with the administrative burden of directing the department. Compton stated that he had done all he could to accommodate Schmitt, and that Schmitt would likely accept an offer to join the Rockefeller Institute. Schmitt stayed at MIT, and after the war, Food Technology was set up as its own department and the Department of Biological Engineering changed its name to the Biology Department. In contrast to John Slater, whose policy for the Physics Department was to reorganize the extant courses, Schmitt dramatically pared down offerings to a set of introductory courses covering

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97See "Excerpt from Warren Weaver's Diary," 16 October 1937, Rockefeller Archives, RG 1.1, Box 1, Folder 13; Compton to Henry Vaughn, 12 November 1941, MIT Archives, AC4, 74/37; Compton, "Memorandum of Conversation with H. Vaughn, 5 December 1941, AC4, 74/38; Alan Gregg to Compton, 26 November 1941, AC4, 74/37.

98Schmitt to Frank Hansen, 14 January 1942, Rockefeller Archives, RG 1.1, Series 224D, Box 2, Folder 18.


100Frank Hansen, "Lunch at the University Club, New York," 20 November 1944, in ibid.

101See Section 6.3, above.
conventional biological topics and advanced work in physical and chemical techniques in biological research.\textsuperscript{102}

Schmitt had succeeded in making instruction in physical techniques the end point of biological training. Even where there were obvious overlaps with Food Technology, as in the biochemistry of microorganisms, Schmitt rid the department's offerings of any mention of applied subjects. Instead of a Department of Biological Engineering generating cooperative research, there was a Department of Biology setting the agenda for the use of physical techniques in research—much as Millikan would have liked. Compton's third avenue to progress in biology—the departmental unification of biologists, physical scientists, and engineers around phenomena and techniques of common interest; and the creation of a curriculum that reflected the professional opportunities for students with a new form of specialization—had hit a dead end.\textsuperscript{103}

In retrospect, Schmitt's policies were good for MIT. As Watson's and Crick's famous work convinced biologists that the chemical structure of DNA held the key to the transfer of genetic information, the structure Schmitt had imposed on MIT's department attracted the influential biologists who made MIT outstanding in what is now called genetic engineering.\textsuperscript{104} Such a possibility never crossed the imagination of Compton, Bunker, and Bush; the words "genetics," "heredity," and "evolution" never appeared in their proposals. When someone suggested H. J. Muller as a candidate for Biological Engineering, Bush warned Compton "I have comments that would persuade me that it would not be wise to have very much to do with him."\textsuperscript{105} Muller certainly had many objectionable traits, but if Compton, Bush, and Bunker had

\textsuperscript{102}The 1936 Catalogue listed 44 course in the Biology Department; in the 1947 Catalogue, there were 17.

\textsuperscript{103}Cf., Kohler, \textit{Partners in Science}, 318-321.

\textsuperscript{104}Salvador Luria came to MIT in 1959; David Baltimore in 1968.

\textsuperscript{105}Bush to Compton, 7 March 1940, Bush Papers, Library of Congress, Box 26, K. Compton file. Bush did not elaborate on the character of his objections to Muller.
wanted a biologist to direct the use of physical techniques to investigate phenomena that were strategic to biology's conceptual structure, they could hardly have expected to find anyone so accomplished, so intellectually inclined towards such a program, so well connected to the Rockefeller Foundation, and available for hire in 1940.\footnote{Elof A. Carlson, \textit{Genes, Radiation, and Society: The Life and Work of H. J. Muller}, (Ithaca: Cornell University Press, 1981), 274-288. Muller had a history of bitter relations with collaborators-turned-competitors, involvement with left-wing politics, and nervous breakdowns.}

Compton was less able to leave his mark on MIT's biology department than he was in physics and electrical engineering. The latter two, within the framework of university-patron relations pressed for by Compton, served as the origins for locally distinctive research programs that downplayed the deepening of disciplinary foundations in favor of reorganizing disciplinary boundaries through systematic pursuit of topics of shared interest to multiple specialties. Yet in spite of serious efforts to duplicate those successes, the biology department ended up being structured around disciplinary issues.

In part, this result reflected idiosyncratic circumstances. Compton had no background in biology and ended up with a department chairman who did not, like John Slater, share his desire to organize academic research locally around shared concerns of multiple departments. Compton reached into the departmental ranks, as he had in electrical engineering, for an MIT-wide administrator to help promote an intra-MIT innovation. But MIT's Biology and Public Health Department, after being rebuffed by the Rockefeller Foundation and Harvard Medical School, was hardly a dynamic, growing department that could attract and hold junior faculty of Bush's and Bowles' caliber.

Nevertheless, the collapse of Biological Engineering into the Department of Biology signifies more than Compton's lack of savvy in evaluating biologists and the department's unhappy history since Sedgwick's death. From the time Compton and Bunker first circulated a proposal, they provoked suspicions that cognitive unity was not adequately stressed as a guide to and goal for biological engineering. Their rhetorical contention—"Nature herself is not divided into a physical work, a chemical
world, a biological world; she is a unit"—did not reveal which specialties were ripe for coordinated pursuit given the state of experimental techniques and theoretical capabilities. The terms Schmitt demanded, the further changes he requested, and Compton’s acquiescence bespeaks a discipline-wide preference to deepen biology’s foundations and eschew its expansion.

Conclusion

Compton’s efforts to invigorate MIT had their foundations in his determination to make research policy locally where he, as MIT’s president, would be an active broker between patrons and researchers. Instead of being titular heads of collections of autonomous departments whose diverse specialists would conduct their research in competition with similarly trained specialists at other institutions, Compton wanted university administrators empowered to stimulate recombinations of specialists within their faculties when they spotted possibilities for curricular and research novelties that would differentiate and advance their universities’ interests. Instead of setting cognitive goals and using those goals as a framework within which individuals’ research projects could be competitively judged, Compton wanted research patrons to assess the breadth of research opportunities and judge universities by whether their programs were extending the breadth of learning in interesting and socially important ways. Extension of phenomenological breadth, in Compton’s views, was as meritorious an academic goal as plumbing theoretical depths.

From an MIT-centered perspective, Compton’s presidency was a positive watershed. During the 1920s, MIT had been frustrating promising researchers with a penchant for teaching, losing other promising researchers to industry once they had demonstrated their capabilities, and failing to attract patronage that gave its personnel the discretion to balance the demands of cognitive goals and technological relevance in their activities. Compton’s intra-MIT policies and his fund-raising successes guaranteed that MIT would promote those faculty who could define a research area that expanded their specialties’ breadth and that MIT could attract outside faculty with first-rate credentials when its in-house faculty lacked the qualities needed for
launching and administering such programs. MIT as known today—an elite undergraduate engineering school with a distinguished faculty that offers innovative as well as classic graduate programs—is built upon Compton’s success. In the eyes of one of his successors, "It is no exaggeration to say that Compton’s feat in transforming the Institute into a world-class university was an achievement of educational leadership that should place him in the pantheon of America’s greatest university presidents."107

Compton’s pursuit of institutional grants from foundations for research programs involving multiple departments turned the battle of Noyes and Walker for the support of Maclaurin into ancient history and made MIT a lively, invigorating place amidst the somber economic background and anti-science rhetoric of the Depression.108 His accomplishments awed his successors109 and so impressed his peers that he moved readily into publicly prominent positions.110 Most importantly for the future, by the end of the decade he could himself play the patron; his vice-president and dean of engineering, Vannevar Bush, became president of the Carnegie Institution of Washington in 1938, and from that office gained access to the White House to organize the National Defense Research Committee. Yet while Compton’s efforts provided one model for how academic specialization could be managed to the benefit of both education and industrial development, they also sowed the seeds for an enduring debate over the particular aims of academic science and the institutions that should structure scientific research. The histories of Compton’s efforts reveal that not all scientists wanted research boundaries defined by the local politics of universities. Of course, so long as his powers directly affected only MIT, there was little call for widespread scrutiny. But when Compton became a deputy to his former deputy Bush

107Kilian, Education, 161.


109Kilian, Education, 158-162.

in controlling the lone major source of governmental funds for university research, characterizing his policies and evaluating their impact on particular research fields and their partisans became far more politically and intellectually pressing.

Yet when viewed from a national perspective that takes cognizance of the growth of scientific specialties, Compton's "successes" are particular measures that, while well-suited to MIT's particular conditions, were not an unambiguous basis for generalization. Compton's insistence that research policy be made locally—so that he (and other university presidents) could favor programs that would reorganize disciplinary boundaries and highlight MIT's institutional distinctiveness—entailed potential risks and costs. Risks were involved in picking specialties from several disciplines to constitute a new program and a specialist to lead the new program. As the history of biological engineering shows, if the specialties were too intellectually disparate and the leader too insistent on making administrative boundaries coincide with conceptual coherence, the entire program could be subverted and transformed. Costs were involved in the lack of emphasis on programs that were widely considered important within a discipline. The stability of most atomic nuclei under most conditions and their transformations under some conditions were patently phenomena that lay beyond the scope of extant physical theory during the first third of this century. Yet Compton's and Slater's reform of the physics department required that those interested in experimental nuclear physics adapt their techniques to and share their instrumentation with biologists, physicians, and engineers who did not share their conceptual concerns. The difficulties in encompassing both evolutionary dynamics and genetic stability in a single conceptual framework had fueled both experimental and theoretical ferment in biology in the first third of the twentieth century. Yet Compton's and Bunker's proposals for biological engineering did not include projects that posed appealing challenges to those who had gained reputations by contributing to that ferment. Only Bush's and Bowles' programs, which were centered in an engineering rather than a science department, broadened their fields without provoking dissent about the importance of deepening foundations. But then, deepening the foundations of engineering was arguably not something that engineers
had to worry about since scientists could be viewed as responsible for providing the insights that deepened engineering.

In the 1930s, however, there was little need to think about research policy from a national perspective. In the 1920s, the efforts of the National Academy of Sciences to raise a national research fund from industrial sources had come to naught and were not revived. The Rockefeller Foundation’s reorganization and retrenchment circa 1930 had largely removed it from issues of university organization and the competing claims of diverse specialists. And no federal agency was dedicated to supporting university researchers or soliciting the advice of university researchers, and all proposals to create any such agency in the 1930s were politically stillborn.\footnote{Joel Gennuth, "Groping Towards Science Policy in the United States in the 1930s," \textit{Minerva}, 25 (1987) 238-268.}

Nevertheless, in 1940, world events made the idea of national research policy far more than an intellectual abstraction, and more local developments made Compton’s practices the starting point for a national policy and eventually national debate about science policy. Vannevar Bush was too young and ambitious to await Compton’s retirement in order to acquire more responsibility, and Compton was not one to hold back a valuable staff member. In 1939 Bush became president of the Carnegie Institution of Washington. His appointment affected far more than the CIW’s policies and directions. At that time, the CIW president was the only full-time administrator of a major civilian, private scientific institution in the capital. With the outbreak of war in Europe making it possible for President Roosevelt to support an agency to mobilize private civilian scientists for national defense, Bush took full advantage of his propitious position to define and organize the program. In remarkably short order, he and Compton shifted from university administrators who had to convince private patrons to make the kinds of grants they thought appropriate to public patrons who could establish the framework within which university administrators and researchers would make proposals. Their use of that power for initiating and administering research programs are the subjects of the next chapters.
PART III: THE EXERCISE OF NATIONAL POWER FOR LOCAL RESEARCH ADMINISTRATION IN WORLD WAR II
CHAPTER 8

THE POLITICS OF WAR RESEARCH I:
ADMINISTRATIVE STRUCTURE AND THE INITIATION
OF RESEARCH PROJECTS

The role Compton carved out for himself at MIT, the relationships he pursued between MIT and the Rockefeller Foundation, and the substance of the policies he and his appointees carried out were not a matter of direct concern outside MIT.¹ Not even Harvard's biologists cared to adapt to a Compton initiative when offered the chance. However, when Bush left MIT in 1939 to take the presidency of the Carnegie Institution of Washington, his background soon became nationally relevant.

The outbreak of war in Europe raised interest in civilian contributions to military preparedness, and Bush was in the "right spot at the right time" to develop a structure for government funding of civilian research. He was the chief administrator of Washington's most prestigious private research institution, he had access to the White House through Frederic Delano, the president's uncle and Carnegie Institution trustee, and he had a working familiarity with the policies of a government research laboratory through service on the National Advisory Committee on Aeronautics. In June 1940, Bush secured an executive order from President Roosevelt to form a National Defense Research Committee (NDRC), with Bush as chairman, under the White House's Council of National Defense. A year later Bush secured Roosevelt's approval to subsume NDRC and the Health and Medical Committee, another research

¹A major exception to this statement is Compton's efforts, which failed to unite the scientific community nationally, to obtain a government-funded program for academic research within the New Deal. See Genth, "Groping," 238-268.
committee under the Council of National Defense, within a new Office of Scientific
Research and Development (OSRD), with Bush as director, within the Office of
Emergency Management of the Executive Office of the President.

By setting up NDRC and then OSRD under executive order without
Congressional sanction, Bush sacrificed long-term legitimacy for short-term
discretionary power. With no legislative history or judicial review as constraints, he
could follow his own instincts and those of his chosen colleagues so long as he had
the president's support. Bush’s plans fit Roosevelt’s desire to prepare for war in an
ad hoc, politically discrete manner that did not provoke isolationists, who were still
willing to press their cause after the Nazi blitzkrieg in the spring of 1940.3

Bush’s chosen colleagues were the administrators he had dealt with,
cooperatively or competitively, at MIT: Karl Compton, Frank Jewett—the president of
Bell Telephone Laboratories, one of Compton’s backers for the MIT presidency, and
a prominent employer of the graduates of Compton’s reformed Physics
Department—James Conant, the Harvard president who would have liked to have
cooperated in MIT’s Biological Engineering proposal had his faculty been willing, and
Richard Tolman, Dean of Science at the California Institute of Technology, which
MIT’s administrators envied and against which they sought to differentiate MIT.
These men made NDRC the type of patron Compton had wanted the Rockefeller
Foundation to be. Their strategy was to support local research centers that had
brought together the relevant specialists to investigate phenomena or instruments that
could broaden scientific disciplines while generating engineering-relevant results.
They cultivated White House connections and adopted an internal structure suitable
for that strategy. They avoided dealing with intra-administration advocates of
centrally managed government programs and isolated researchers seeking an agency to
define objectives and judge individual research proposals whose proponents competed

1985), 32-42; Wayne S. Cole, Roosevelt and the Isolationists, 1932-1945, (Lincoln: University of
Nebraska Press, 1983), 363-422. NDRC was one of several ad hoc agencies, though it was
exceptional in its ability to begin concrete work, as opposed to planning and advising, without a profile
that attracted public attention.
to generate results that brought their community closer to the objectives. Thus
NDRC's selection of contractors should not be viewed as placing money where the
best researchers were but rather as using the elite universities to channel the talents of
the best researchers into the working relations they needed to address military needs.

The two developments that most famously demonstrated the efficacy of
researchers to support national defense, radar and the atomic bomb, embody the
strengths and weaknesses of NDRC's approach to research policy. Radar was among
the first areas NDRC pursued, grew to be its largest project, remained under its
management throughout the war, and thus represents the best case of what NDRC's
leaders considered the basic business of research policy. Its origins and development
were reminiscent of MIT's research programs in physics and electrical engineering
and built substantively on the links Bowles had promoted between the departments.
By contrast, a nuclear fission program was not pushed until 18 months after NDRC's
establishment, and NDRC transferred its management to the Army Corps of
Engineers when the program needed industrial resources to progress. Its origins and
development reflected the tensions that Arthur Noyes and William Walkcr had
generated within MIT, and its advocates practiced the steady insistence on changing
administrative boundaries that characterized Francis Schmitt's turning Biological
Engineering into a Department of Biology.

By the time the war's end seemed in sight, researchers involved in the radar
and fission programs were hatching two distinct forms of planning for continuing the
substance or style of their wartime work. Their desire for continuing government
patronage of research in private institutions combined with suspicions over which
lines of research would be best served by an agency of particular design forced an
open legislative debate. Ironically, by avoiding political debate at NDRC's inception,
NDRC's founders obtained the power to achieve so many successes that legislation to
create a postwar agency became partisan in politics and divisive within the scientific
community.
Committee Management, White House Relations, and Internal Organization

After taking the position at the Carnegie Institution, Bush continued to meet regularly with Compton and Conant for meetings of the National Academy's Committee on Scientific Learning Aids. Despite its title, this committee provided occasions for discussing international affairs and the United States' defense preparedness. Conant, in October 1939, had spoken publicly against isolationism and steadily made himself a leading advocate of a pro-British policy. These discussion made an interventionist of Bush and emboldened him to attempt to obtain governmental funds to support academic research for national defense independently of either the military services or the National Academy of Sciences. For help in the mechanics of setting up such an organization, Bush turned to John Victory, NACA's long-time executive secretary, who wrote the first draft of an executive order to set up a National Defense Research Committee.3

Bush's use of NACA as the source of bureaucratic wisdom for NDRC reflected well-founded comfort with NACA's style. NACA's success was based on the kind of research policy that had served Bush and Compton so well at MIT. NACA's researchers had developed and mastered a complex, expensive instrument, the variable-pressure wind tunnel, and used it for studies that appealed to a broad range of interests—from university professors concerned with aerodynamic theories to military officers concerned with specifications for new aircraft.4 NACA defined this work as "fundamental science" and successfully used this designation to argue against subsuming its work under any cabinet department concerned with regulating or using

3Bush, Pieces, 32.


aeronautics.\textsuperscript{7} The wind tunnel at Langley Laboratory served NACA much as the differential analyzer and Center for Analysis served MIT. In both cases, common interest in an instrument and its improvement bound researchers with utilitarian and cognitive concerns into a local community. And in both cases, the community insisted that its institutional setting be independent from commercial or regulatory concerns and that its patrons support its activities as a contribution to science.

Bush's proposal for a NDRC extended committee-management of government research from aeronautics to all other problems of national defense. Whereas NACA was chartered "to supervise and direct the scientific study of the problems of flight," Victory's draft of an executive order called for a National Defense Research Committee "to coordinate, supervise, and conduct scientific research on the problems underlying the development, production, and use of mechanisms and devices of warfare."\textsuperscript{8} Placing a committee in charge of defense research had the virtue of insuring breadth in perspective for decisions on the establishment and evolution of laboratories that would appeal to the variety of specialists needed to address the underlying problems of military technology. A single administrator would be inclined, given the limitations imposed by his own specialization, to seek balance among competing interests rather than to seize the opportunity to combine interests.\textsuperscript{9}

Yet while the idea of an independent research agency under committee management held strong appeal to Bush, his agency departed decidedly from NACA in one respect. NACA, in seeking to establish its indispensability as a government agency, had striven to distinguish its "fundamental" research from the universities' "theoretical research" lest Congress conclude that federal taxes were being used to

\textsuperscript{7}Tid., 135-142.


\textsuperscript{9}It is noteworthy, in this respect, that only one change occurred in the composition of the NDRC during its life. When Bush became director of the Office of Scientific Research and Development, Conant became chairman of NDRC, and Roger Adams, a University of Illinois chemist, was added to fill Conant's old position.
duplicate privately- and state-supported research. Bush came to prominence in Compton’s MIT because distinctions between theoretical and fundamental were neither intellectually nor organizationally respected.\textsuperscript{10} Instead of creating new government laboratories for the “fundamental” research appropriate to the military technology of 1940, Bush proposed that NDRC contract such research to non-governmental institutions:

There appears to be a distinct need for a body to correlate governmental and civil fundamental research in fields of military importance.\ldots\textsuperscript{11} It should form a definite link between the military services and the National Academy [of Sciences].\ldots\textsuperscript{11} It could perform a very valuable function\ldots in stimulating, extending, and correlating fundamental research which is basic to modern warfare.

It should not be created unless it would be welcomed, and hence supported by the three bodies primarily concerned, the War and Navy Departments, and the National Academy of Sciences. If it has their support, it will also be able to enlist the support of scientific and educational institutions and organizations, and of individual scientists and engineers throughout the country.\textsuperscript{11}

Bush was creating in the federal government, and for the limited objectives of defense preparedness, the same function that Karl Compton had pushed the Rockefeller Foundation to assume. Bush’s committee would principally patronize universities and secondarily individuals; and principally those universities whose administrations had organized their staffs in ways appropriate to extend and correlate the fundamental research basic to warfare.\textsuperscript{12} But where MIT’s leaders in the 1930s had to cajole the Rockefeller Foundation into supporting proposals to extend and

\textsuperscript{10}Roland, Model Research, 130-135 and 167-171. As a member of NACA, Bush made it possible for Jerome Hunsaker, chairman of MIT’s Department of Aeronautical Engineering, to prod NACA into more systematic contacts between itself and both industry and the universities.

\textsuperscript{11}Quoted in Dupree, "Great Instauration," 450-451. Dupree interprets Bush’s use of the term "fundamental research" as something of a ruse to get academic scientists involved in projects they would not otherwise touch. This is perhaps the only point on which I disagree with Dupree; Bush’s use of the term seems consistent with the organization of research at MIT.

\textsuperscript{12}Bush, Pieces, 38 makes the point that contracting with universities, rather than individuals, was a departure from standard practice, though he emphasized how such contracting allowed NDRC to pay overhead costs and free researchers from handling business affairs, not how such contracting affected researchers’ working relations.
correlate research basic to broad technological interests, Bush and Compton through
NDRC could reward universities that defined research fields in the style they
preferred.

Bush transmitted his plans to Frederic Delano, who arranged a meeting
between Bush and Harry Hopkins in early June, 1940. Bush recalled being
apprehensive because he disagreed with the New Deal’s use of government powers to
achieve social reforms. He later considered it a "minor miracle" that he and Hopkins
"hit it off" and found "a common to language to speak." Bush really just needed a
more nuanced view of Hopkins’ efforts and how they differed from those of other
Roosevelt administration officials. Hopkins had formed a federal program to provide
mass employment on civil works projects by "deputizing" state relief officials to
manage projects and by scrambling to involve engineers and accountants in the state
operations. Hopkins and his aides issued guidelines for the projects and determined
the level of federal expenditure in a state, but the power to define and carry out the
projects rested with the local officials. Arthur Schlesinger found Hopkins:

   profoundly convinced of the values of decentralization. He kept his
   Washington staff small and gave great responsibility to state administrators,
   while at the same time holding them as well as he could to the national
   mark.

   Hopkins’ program was the public-works equivalent of the fluid research funds
   that MIT obtained from the Rockefeller Foundation. In both cases, federal support

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14Bush. Pieces, 35.

that Hopkins pressed for the participation of engineers and accountants even though they tended to be
Bull-Moose Republicans or independents and even where their presence upset local Democrats. This
non-partisanship is also consistent with Hopkins’ willingness to work with Bush. See also Kenneth S.
Davis, Franklin Delano Roosevelt: The New Deal Years, 1933-1937 (New York: Random House,

went to a more local institution whose officials decided on the support’s precise use. Frank Jewett’s advice to Bush on how to run NDRC perfectly matches Schlesinger’s description of Hopkins:

[W]e are bound to find ourselves driven with urgent problems which demand our personal attention as members of the Committee.... I think, therefore, that we should firmly resolve to delegate everything we can to existing agencies and hold them responsible for results. If special groups have to be assembled under these agencies, let them struggle with the griefs.... Let us use the existing machinery of colleges, technical schools, industrial laboratories, and the N[ational] R[esearch] C[ouncil] to the limit.

Thus whatever disagreements Bush and Hopkins may have had over fiscal policy or the ability of the government to ameliorate social problems, they agreed on the proper administration of federal support and could of course agree on the goal of preparedness for a potential war.

Throughout the war, Bush avoided entanglements with members of the Roosevelt administration who had opposed Hopkins’ methods during the New Deal. Secretary of Interior Harold Ickes and Secretary of Agriculture Henry Wallace had both striven to increase their departments’ power to evaluate and coordinate the policies of local officials; indeed, Ickes’ differences with Hopkins led to a running feud over bureaucratic control of public works funds. Bush kept NDRC out of controversies over critical materials and factory production that involved Ickes. And Bush flatly ignored Wallace’s offer to arrange for cooperation between USDA

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17Indeed, the largest, single source of outside funds for the MIT Physics Department during the Depression was a WPA grant. George Harrison had invented a partially automated method for measuring the spectra of a class of elements. The WPA was used to hire unemployed scientists to tend the instruments in a wholesale effort to analyze these spectra. Slater, Scientific Biography, (New York: John Wiley, 1975), 167-8.

18Jewett to Bush, 21 June 1940, National Archives, RG 227, Item 13, Box 58, Folder 2.

laboratories and NDRC. Thus Ickes and Wallace never had cause to question Bush’s administrative methods.

The executive order that created NDRC followed Bush’s wishes virtually to the letter: Compton, Conant, Jewett, and himself (plus the Commissioner of Patents) became the civilian members of the NDRC. The NDRC was placed in the Executive Office of the President and thus could receive an appropriation to spend as it saw fit. And it was authorized to “correlate and support scientific research,” not to conduct research itself. This last point assured that NDRC would operate through local, private institutions rather than build government laboratories or assemble a large staff to administer a myriad of individual projects. Further encouragement came from the military services, especially the Army, on the principle that problems of quality control and procurement would inundate their laboratories in the event of war. Karl Compton’s first assignment, which he readily dispatched, was to find out what projects the military laboratories would curtail or never launch should war come. This information was raw material for NDRC.

NDRC’s first key decision was to set its own internal organization. As the organizers of an independent corps of scientists, NDRC had three possible arrangements to consider: arrange scientists by discipline, by military problem to be solved, or according to the possibilities presented by the state of science. Bush’s impulse, he informed his fellow civilian members, was to use Compton’s survey as a

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21Dupree, “Great Instauration,” 452.

22Baxter, Scientists, Appendix A, 451, is the executive order establishing NDRC.

23Ibid., 16-17.

basis for creating divisions within NDRC:

The organization of our internal affairs requires careful consideration. From the research work now in existence, details on which will be available today or tomorrow, it will become fairly evident what our general subdivisions will be.\(^{25}\)

The next day he proposed that Jewett lead a Transportation and Communication Division, Compton an Aircraft, Submarine, and Mine Detection Division, and Tolman a Guns, Projectiles, Armor, and Materials Division.\(^{26}\)

Some dissent must have been registered, perhaps from Conant,\(^{27}\) for the notes from the 25 June meeting show internal organization to be mixed on a disciplinary and problem basis: Jewett retained a Communications Division, but Compton was charged with Physics, Conant with Chemistry, and Tolman with "proposals not falling in one of the preceding categories."\(^{28}\) However, at the next meeting on 2 July, which the first official meeting since the executive order was dated 27 June, the Committee opted for Bush’s original proposal: Compton again had Airplane, Submarine, and Mine Detection; Tolman had Guns, Projectiles, Armor and Materials; and Conant was assigned to lead an Explosives, Fuels and Power, and Gases Division. NDRC retained a problem-oriented organization throughout the war. Disciplines, when represented, were reduced to "catch-alls" as Compton explained to a British audience:

These divisions and sections [of NDRC] are each built around a specific functional concept, such as fire control .... However, there are two divisions which are in the nature of ‘catch-alls.’ For example, the Division of Physics and the Division of Chemistry can be defined as handling everything

\(^{25}\)Bush to Compton, Conant, and Tolman, 20 June 1940, National Archives, RG 227, Item 13, Box 58, Folder 2.

\(^{26}\)Bush to Compton, Conant, and Tolman, 21 June 1940, in ibid. Coe was given responsibility for unsolicited inventions; Conant’s job was left unspecified.

\(^{27}\)In retrospect, at least, Conant viewed his, Compton’s and Jewett’s initial roles as organizing chemists, physicists, and engineers, respectively; Several Lives, 238.

\(^{28}\)"Notes from the 25 June 1940 Meeting," in ibid.
in these respective fields which does not fall under any one of the more
sharply defined divisions.²⁹

These early decisions made NDRC a governmental version of the modern
multi-divisional, decentralized corporation. The trademarks of such corporations were
that divisions were formed around products with each division chief responsible for
coordinating all the tasks that were daily necessary to bring the product to market,
while corporate headquarters, staffed by generalists and professional administrators,
concentrated on entrepreneurship, long-range planning, and the evaluating the
divisions' performances.³⁰ NDRC's practice of creating research divisions around
military functions was analogous to creating product divisions in corporations. The
university administrators whose laboratories received contracts were responsible for
managing the researchers needed to produce knowledge in a form the military would
"buy." NDRC proper functioned as the executive committee of a multi-divisional
corporation. The four civilians on NDRC were scientific generalists who had left
research for full-time administrative positions.

Initially, unlike the corporate model, each member was personally in charge of
organizing a division and report on the division's activities to the committee as a
whole. However, NDRC adopted corporate wisdom when, 18 months after opening
business, the four divisions had spawned 60 sections,³¹ making a member's job of
reporting his own division's work and voting knowledgeable on other divisions'
recommendations too onerous a task. The lone major reorganization of NDRC
consisted of grouping the 60 sections in 20 divisions, which individual committee
members ceased to oversee personally. Instead, the committee appointed sub-com-

²⁹Karl Compton, "Organization of American Scientists for the War," Science, 98 (23 and 30 July
1943), 71-76 and 93-98, quote from 95. The articles were taken from Compton's Pilgrim Trust
Lecture to the Royal Society on 20 May 1943.

³⁰Chandler, Strategy and Structure, 1-17.

³¹Irvin Stewart, Organizing Scientific Research for War: The Administrative History of the Office
mittees of itself to review bi-annually the program and budget of each division, and met as a whole every Friday to consider the divisions' proposed contracts.\textsuperscript{32}

The decision to organize research around military problems gave NDRC's contractors a task that was most familiar to MIT's administrators—the assembly of teams of talented researchers from multiple disciplines into local research centers devoted to areas deemed fundamental to military needs. To effect this form of intellectual organization, NDRC practiced what MIT habitually requested from the Rockefeller Foundation; contracts were made with universities for the program of an entire university unit or set of units rather than with individual researchers with ideas for specific projects.\textsuperscript{33}

Since shortly after the end of the war, observers who noted that NDRC had concentrated its funds in prestigious institutions have viewed the politics of post-war science policy as a battle between scientific "haves" and "have-nots" or between "elitist" scientists pushing for the "best science" and politicians emphasizing the responsibility of science policy to promote social welfare.\textsuperscript{34} However, equally, if not more important, is the fact that NDRC concentrated its money on specific projects. Of the approximately $336,000,000 OSRD spent in its top 25 non-industrial contractors, 77\% was spent in its top four: MIT, CIT, Harvard, and Columbia. Yet these four institutions administered only 33\% of the 822 contracts NDRC let to its top 25 contractors.\textsuperscript{35} Each of these universities housed substantial units devoted to research on specific "instruments of war:" MIT for radar, CIT for rockets, Harvard for radar counter-measures and underwater sound detection, and Columbia for smoke screens, underwater sound detection, and radar. The ability of universities as administrative units to focus the attention of combinations of researchers on areas of

\textsuperscript{32}Ibid., 60-5.

\textsuperscript{33}On the innovating character of such contracting, see Bush, Pieces, 38-39.


\textsuperscript{35}Figures calculated from information in Baxter, Scientists Against Time, Appendix C, 456-457.
relevance to military technology was as important to NDRC's success as gaining the services of the most highly reputed scientists.

These organizational arrangements produced complementary successes and problems for NDRC in the inauguration of research projects. The policy of placing research into military functions in specific university laboratories permitted NDRC to build on an extant university program when that program, by design or coincidence, focused on phenomena of relevance to military technology. However, it left NDRC ill equipped to deal with demands from widely dispersed scientists for their organization into a unified program. This distinction accounts for the different priorities initially assigned to investigations of microwave and nuclear phenomena. While NDRC comfortably delegated research in the former to a single university with a tradition of encouraging such work, the latter begged for centralized coordination of scattered efforts.

**Local Traditions and NDRC Action: The Support of Radar Research at MIT**

The most outstanding outcome of NDRC's research policy was is the Radiation Laboratory at MIT. In Bush's earliest thoughts on organization, Compton was to handle a division for detection. Bush placed this area under Compton's direction knowing full well that Compton was supporting Bowles' efforts to achieve directed radio-beam systems at microwave wavelengths. After the Depression forced Col. Green to suspend his support for Round Hill in 1934, Bowles held together at least the engineering half of his group by obtaining a contract from the Civil Aeronautics Administration to develop a blind landing system. Compton sought more support for physical aspects of the research, and he found a potential sponsor in early 1939 in the banker and independent scientist Alfred Loomis.

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36See Bowles to Green, 10 March 1934, MIT Archives, AC 4, Box 76, Folder 53.

37See Section 7.1, above.
Loomis was between projects in 1938 when Compton arranged for him to meet with Bowles to consider a joint ultrahigh frequency research program. The prospects appeared "unusually promising" to Bowles "because we have several members of the Physics Department who can help us materially on the pure physics aspects of our work and who are at the same time very much interested and eager to participate." Bowles proposed a division of labor to Compton. Julius Stratton and his assistants would guide research on propagation of microwaves at Loomis' laboratory on his estate in Tuxedo Park, New York; work at MIT would focus on the effects of microwaves on materials and dielectrics and on the development of a variable-frequency power source of microwaves. (The Sperry Corporation, which owned patent rights to the klystron, the most promising generator, would support the MIT-based research.)

Compton kept Bush posted of the Bowles-Loomis negotiations. Bush's response was that the only further need was to see that the Army "is properly stirred up," and he reasoned that Sperry could be relied on to do most of the stirring "since they own the rights." However, as MIT's researchers, after a summer's work, turned their initial plans and surveys into more concrete programs, and as Bush became more concerned with the United States' military preparedness, his attitude turned from passivity to a decided willingness to involve the Carnegie Institution in the matter. He visited Bowles at MIT in January 1940 and reported to Compton:

I told Bowles that if there was some point in his microwave program where he needed further support, to let me know. I judge that the situation is that the group at MIT now have [sic] just about all they can handle ....

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38See Henry Guerlac, "Notes on an Interview with Karl Compton," 20 August 1943, National Archives - Waltham Branch, RG 227, Historian's Office, Box 43, Microwave Committee folder.

39Bowles to Compton, 12 May 1939, MIT Archives, AC 4, Box 3, Folder 3.


41Bush to Compton, 4 May 1939, in ibid.

42See Bowles to Compton, 31 October 1939, MIT Archives, AC 4, Box 3, Folder 3 (new numbers).
Nevertheless, this is a very important thing, and if it can be expedited in any way I am anxious to help to increase its speed.⁴³

Compton took Bush up on that offer. In May 1940, Compton requested a special appropriation from MIT's Executive Committee "to insure the prosecution of the scientific and theoretical aspects of this work [on ultra-high frequency radiation]"⁴⁴ while Bush arranged for a Carnegie Institution grant to Bowles.⁴⁵ When Bush and Compton met next month to structure NDRC, placing detection under Compton was an extension of extant relations among Bush, Compton, and MIT.

When Compton, in his survey of military research which war would preempt, found several projects related to microwaves, "the decision to limit the detection work to the field of microwaves was made almost at once. Such a program fulfilled to a fault the general policy requirements that [NDRC] should undertake long-range projects too speculative for the Service laboratories in time of war."⁴⁶ Compton tapped Alfred Loomis to chair a Microwave Committee, and Loomis formed a committee of industrial scientists from the major electronics firms in order to stimulate the interest of their employers in devices that did not appear to offer competitive commercial advantages.⁴⁷ For the committee's secretary, Loomis took

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⁴⁴Compton to Executive Committee, 20 May 1940, MIT Archives, AC 4, Box 6, Folder 2 (new numbers).


⁴⁶Ibid., 247. The military projects relating to microwaves included fog penetration, bombing through overcast, navigational aids, and tracking and attacking waterborne targets.

⁴⁷Henry Guerlac, "Interview with Alfred Loomis." 21 May 1943, National Archives - Waltham Branch, RG 227, Historian's Office, Box 59. Although Albert Hull of General Electric had begun research in 1916 into magnetron-controlled vacuum tubes (magnetrons), the conclusion, in 1921, of cross-licensing agreements among the major manufacturers of electrostatically-controlled tubes had reduced the commercial imperative for such research. Only in 1928 did Americans become aware, from European and Japanese investigations, that magnetrons generated microwaves, and only in 1933 were magnetrons shown conclusively to be more promising generators of microwaves than electrostatically-controlled tubes. See James E. Brittain, "The Magnetron and the Beginnings of the Microwave Age," Physics Today, 38 (July 1985), 60-67.
Bowles, his major collaborator at MIT. At Bush’s urging, E. O. Lawrence also served in order to have an academic physicist on the committee.  

The Microwave Committee first undertook a disappointing survey of microwave research in the United States. The klystron had not turned out to be a sufficiently powerful and reliable generator of radiation, but nobody was found to have anything better. "At the end of the summer [of 1940]," Bowles recalled, "we decided to write a report—a sign that we didn’t know what to do next." That the lack of a generator should leave the Microwave Committee stymied is indicative of the sensibilities that its members brought to its duties. Industrial laboratories had learned to organize fundamental research around the processes going on in their firms’ products in order to learn "the natural laws of man-made devices," and the MIT affiliates atop the NDRC hierarchy had been accustomed to building academically inclined versions of such laboratories. Without a generator capable of producing powerful, high frequency waves, there was no man-made device to learn the natural laws of.

The Microwave Committee could have used its members’ contacts and meetings of scientific and engineering societies to let researchers know that the government was prepared to support proposals to work on the development of a microwave generator. Had such a policy been followed, the Microwave Committee would have a combination peer-review and prize-award panel. The committee never tried to assume such a role. Even had there been no security concerns over the broad dissemination of one of its research needs and the industrialists’ desire to make research-based inventions the patented property of their firms, Bowles knew full well that recommending a policy of stimulating competition among individual researchers

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49Ibid., 249-250.

50Henry Guerlac, "Interview with Edward Bowles," 21 August 1943, National Archives - Waltham Branch, Historian’s Office, Box 58.

51Reich, "Irving Langmuir," 201.
was no way to elicit Compton’s enthusiastic support. MIT’s applications to the Rockefeller Foundation sought to tap a national source of wealth for the support of locally generated intellectual reorganizations; Compton’s willingness to promote Bowles’ proteges and broker arrangements between Bowles and Loomis were due to Bowles’ creation of a multi-disciplinary community around the possibility of directed radio beams. Had the Microwave Committee actually tried to function as a peer-review panel, NDRC would probably have viewed its organization as indicating that its work was too visionary for this war.

The Microwave Committee was spared this fate because in the fall of 1940, the British technical mission to the United States in the fall of 1940 brought the answer to its problems: the resonant cavity magnetron, which generated amply powerful radiation around 10 cm in wavelength. For the Microwave Committee, the magnetron was a unifying rallying point: "There was general agreement that a central laboratory under civilian direction should be set up at once, staffed as much as possible by research physicists from the universities."52 Where to place the laboratory presented remarkably few problems. The need to be near a coast and to have access to airport facilities eliminated much of the country. Initially the committee looked into setting up a laboratory at the Army’s Bolling Air Field near Washington, but the plans were abandoned because NDRC lacked authority to establish its own laboratories, and the Army reneged on its original offer of space.53

Given the need to embed the laboratory in an extant institution and near a sea coast, Loomis and Bowles preferred MIT. Stanford, where the klystron was developed, was a possibility, but it lacked representation in NDRC’s upper ranks, its work with microwaves was more recent in origin, and the work was oriented to developing an electron accelerator as a less expensive research tool than Berkeley-

52 Guerlac. Radar, 257.

53 Ibid., 258. The Army’s shift was a reminder that NDRC could not depend on the services for logistical support when, by definition, NDRC was working on projects that the services had shelved to prepare for war.
model cyclotrons, not to investigating radio beams.\textsuperscript{54} Loomis' and Bowles' only real fear was that Frank Jewett would make a determined plea to place the project in the Bell Laboratories. But Loomis and Bowles, with Bush's help, succeeded in extracting Jewett's endorsement for a MIT laboratory at a private meeting on 16 October 1940.\textsuperscript{55}

With all the pieces in place, the Microwave Committee on 18 October unanimously recommended the establishment of a laboratory at MIT, and NDRC approved the recommendation at its 25 October meeting.\textsuperscript{56} An account of the decision to place what became known as the Radiation Laboratory at MIT, prepared when NDRC's procedures were coming under public scrutiny, does not mention the individuals involved, but is noteworthy for its accuracy and persuasiveness:

There was then [once Bolling Field was ruled out] consideration as to what university or academic institution had a background in the field of microwaves .... There were two institutions which had some background in this field. One was Stanford University and the other was MIT....[T]he interest of people at Stanford in this field was of quite recent origin and included only Dr. Hansen, Dr. Webster, and one or two others. At MIT there was at that time 18 members of the staff who were engaged in research in this general field. Work at MIT dated from before 1930 and had been aggressively pushed in the later years of the '30s.\textsuperscript{57}

MIT proved to be a most accommodating host. NDRC initially appropriated $455,000 for the Radiation Laboratory on the assumption that its staff would number 50. But by July, 1941, the staff had grown to 225, including 141 scientists and engineers. While NDRC could obtain increases in its appropriations to pay and equip


\textsuperscript{55}Guerlac, "Brief Second Interview with E. L. Bowles, 21 August 1943," National Archives - Waltham Branch, RG 227, Historian's Office, Box 58, E. L. Bowles folder.

\textsuperscript{56}Guerlac, \textit{Radar}, 259.

\textsuperscript{57}Carroll Wilson to Oscar Ruebhausen, "Selection of MIT as the Site for the Radiation Laboratory," 10 September 1945, National Archives, RG 227, Item 13, Box 54, Contract Operations File.
the added staff, MIT had to build one new building and buy another in order to provide adequate space.58

The Physics Discipline and NDRC Hesitancy: The Resistance to Uranium Fission

The distinctiveness of NDRC's approach to research policy is well illustrated by the contrast between the launching of programs in microwave radiation and nuclear fission. Whereas the former quickly received aggressive and enthusiastic support, the latter provoked caution and discomfort; in blunt budgetary terms, through November 1941, NDRC approved $300,000 for uranium studies and at least ten times that for the Radiation Laboratory at MIT. The channels through which the prospects of investigating nuclear fission came to governmental attention were not ones that aroused Bush's instincts for program-building, and the decisions that a program in nuclear fission foisted upon its supervisors were not ones that NDRC wished to confront.

NDRC had backed a microwave program at MIT in the knowledge that the military considered the investigation of detection devices worthy of its laboratories' peace-time attention, that MIT had successfully fostered interest among its physicists in working with electrical engineers on the generation and propagation of radio beams, and that the resonant cavity magnetron could serve as a common point of departure for a multi-disciplinary laboratory of physicists and engineers. None of these conditions existed for nuclear fission. The military had not been experimenting with radioactive materials. The major centers for the study of atomic nuclei—the Radium Institute in Paris, the Kaiser Wilhelm Institute near Berlin, the Radiation Laboratory in Berkeley, the Institute for Theoretical Physics in Copenhagen, the Cavendish Laboratory in Cambridge—tended to be autonomous institutes, shaped by the research interests of their prestigious and competing directors. These directors' attentions were drawn, on occasion, to matters of utilitarian import, but their

laboratories were not designed to integrate the interests of scientists and engineers. Finally, nuclear fission offered not a device, like the magnetron, whose workings and uses could be jointly explored, but a stimulus to determine whether (and if so under what conditions) a chain reaction could be created and a hunger for the resources to create those conditions.

Research in uranium fission came up for governmental consideration throughout the scientifically advanced world because its discovery by Otto Hahn, Liese Meitner, and Fritz Strassman in January, 1939 caused a sensation among physical scientists. Two features contributed to the sensation. First, fission, as a concept, departed from conventional interpretations of neutron-nucleus interactions while explaining the puzzling results of bombarding uranium with neutrons. Second, scientists had speculated on tapping the energy in radioactive substances since

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59 The Kaiser Wilhelm Institute was perhaps the closest to an integrated laboratory; Otto Hahn, with corporate support, used radioactive tracers to study crystallization of commercially important substances. However, he gave up this research in favor of uranium studies in order to bolster his international reputation when his anti-Nazi views rendered his domestic reputation suspect. See Weart, "Discovery of Fission," 95-96 and Fritz Kraft, "Internal and External Conditions for the Discovery of Nuclear Fission by the Berlin Team," also in William R. Shea, ed., Otto Hahn and the Rise of Nuclear Physics (Dordrecht: D. Reidel, 1983), 141-145. The Radiation Laboratory at Berkeley became involved in radiation medicine—in part because E. O. Lawrence's brother was a physician interested in radiation therapies and in part because Lawrence hoped to tap medical foundations for research funds—but the physical science staff found this involvement a distraction; see Martin Kamen, Radiant Science, Dark Politics (Berkeley: University of California Press, 1985), 73-74. Ernest Rutherford resisted pressure to de-emphasize nuclear research at the Cavendish Laboratory to the point of refusing to solicit industrial support for expansion in the Cavendish's space or equipment; see Charles Weiner, "Institutional Settings," 195; and J. G. Crowther, The Cavendish Laboratory, 1874-1974 (New York: Science History Publications, 1974), 186, 193-194, 205-206, and 230. Niels Bohr's support of biological research in Copenhagen, but he was the only physicist actively interested in biology, and the biology and physics research programs did not interact. Aaserud, Redirecting Science, 220-235.

60 On the relationship of fission to conventional interpretations of nuclear phenomena, and on circumstances that enabled Hahn, Meitner, and Strassman to discover fission, see papers of Weart and Kraft in Shea (ed.), ibid. Both Irene Joliot-Curie and Enrico Fermi had bombarded uranium with neutrons and not found fission because they assumed uranium, like all other previously bombarded elements, would absorb neutrons and decay from an unstable isotopic state. For the work of Joliot-Curie and her husband, Pierre, see Spencer Weart, Scientists in Power (Cambridge: Harvard University Press, 1979), 37-59; for Fermi, see Emilio Segrè, Enrico Fermi, Physicist (Chicago: University of Chicago Press, 1970), 75-76.
the discovery, in 1903, that radium was always warmer than its environment.\textsuperscript{61} Uranium fission offered the first evidence of a physical process through which such ambitions could be achieved.

News of Hahn's discovery inspired an international flurry of scientific research and spasmodic efforts at political organizing among refugee scientists in the United States. The scientific activity was so intense that one year after the appearance of Hahn's paper, the author of a review article on nuclear fission cited over 100 studies.\textsuperscript{62} The political activity was more circumscribed. A meeting in March, 1939 between Enrico Fermi and Naval Research Laboratory personnel, who viewed uranium fission as a possible source of power for submarines, resulted in nothing more than a promise by the Navy personnel to maintain contact.\textsuperscript{63} That same spring, the more agitated Leo Szilard tried to organize a publication ban on fission research among scientists in the United States, England, France, and Denmark, but his efforts broke down when Pierre Joliot-Curie declined to participate.\textsuperscript{64} Szilard had a bit more success in October when he and Eugene Wigner convinced Albert Einstein to call President Roosevelt's attention to the potential of uranium fission. Roosevelt appointed a Uranium Committee under Lyman Briggs, director of the National Bureau of Standards. Briggs succeeded in obtaining $6,000 from the military to purchase materials for experiments on the moderating properties of

\textsuperscript{61}Weart, \textit{ibid.}, 9-10 and 37-38.

\textsuperscript{62}L. A. Turner, "Nuclear Fission," \textit{Reviews of Modern Physics}, 12 (1940), 1-29. Almost all the cited studies appeared in the \textit{Physical Review}, \textit{Nature}, \textit{Comptes Rendus}, \textit{Die Naturwissenschaften}, or \textit{Zeitschrift fur Physik}, but authors included scientists working in the Netherlands, Denmark, the Soviet Union, and Japan as well as scientists active in the nations in which the journals were published.


\textsuperscript{64}Weart, \textit{Scientists}, 87-91.
graphite, but he did not inspire Roosevelt to make a sustained commitment to a
government-supported program.  

As president of the Carnegie Institution of Washington, Bush was an obvious,
prominent target for lobbying by nuclear physicists who feared that government
officials would not appreciate their hopes and fears for uranium fission. Under
Bush’s predecessor, John Merriam, the CIW had provided centralized coordination of
discipline-wide physics activities. In 1932, Merriam appointed a Committee on
Coordination of Cosmic Ray Investigations that effectively determined the placement
of observing stations and standardized the design of recording instruments for this
field of research. In 1935 the Institution inaugurated yearly, invitation-only
conferences in theoretical physics, "devoted solely to the clarification of the current
status of the [selected] subject and to discovering the profitable directions for
immediate attack," in imitation of the annual gatherings Niels Bohr hosted in
Copenhagen. Furthermore, CIW itself was home to enthusiastic fission
researchers. Since 1926, its Department of Terrestrial Magnetism contained a sub-
division for Magnetism and Atomic Physics, which under the direction of Merle Tuve
had built high-voltage accelerators and launched an experimental program to probe
how protons and electrons could form stable nuclei in apparent violation of Maxwell’s

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conferences of 1935, 1937, and 1938 were devoted to nuclear physics.

68 Carnegie Institution of Washington, *Yearbook*, 36 (1936-1937), 263. For Bohr’s and his
conferences’ social contributions to the formation of nuclear physics, see Weiner, "Institutional
Settings," 187-212.

Tuve, the leaders of this work, also hoped the high voltage could be used to simulate cosmic rays,
which were then believed to be very penetrating electromagnetic radiation.
Tuve and his colleagues were attracted to fission research because accounting for fission would be essential for any theory of nuclear dynamics as well as a possible means to harness atomic power. The pressure on Bush as CIW president peaked following the American Physical Society Meeting in April, 1940. John Dunning, another of Fermi’s colleagues at Columbia, had just published experimental results indicating that the rare isotope of uranium, U-235, was responsible for fission. Several of the convening scientists, including Tuve, agreed on the desirability of separating U-235 in kilogram quantities. To perform his experiments, Dunning had the help of Alfred Nier of the University of Minnesota, whose skill at mass spectrometry enabled him to supply small quantities of uranium enriched in isotope 235. While Dunning and Nier had readily made their own arrangements, an enlarged project would require both more funds and a centralized administration to support, coordinate, and divide the labor among scattered individuals taking control of various aspects of the project. As a step in this direction, Tuve urged Bush to make a grant to J. W. Beams, a physicist

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at the University of Virginia, who had constructed a centrifuge that seemed potentially able to separate the uranium isotopes in larger quantities.\footnote{Ibid., 24.}

Where Bush, four months earlier, had told Karl Compton he was eager to do anything he could to speed the Bowles-Loomis research into microwaves at MIT, he balked at supporting the pleas of fission’s advocates. As Bush understood the matter, the physicists could neither rule out the possibility of producing a chain reaction in uranium nor calculate whether or how a chain reaction in uranium would proceed. And even if a chain reaction were produced, the physicists could not predict whether the reaction would occur at rates that would be useful for explosives or power generators. Such uncertainties may have made an organized program compelling to those who had specialized in exploring the properties of atomic nuclei. But Bush felt "decidedly puzzled," he told Frank Jewett, about a situation in which researchers, if they succeeded in obtaining the data, performing the calculations, and perfecting the operations they believed necessary, might end up demonstrating that their fears and hopes were unfounded.\footnote{Bush to Jewett, 2 May 1940, Bush Papers, Library of Congress, Box 55, Jewett file.}

For Jewett’s consideration, Bush sketched four possible lines of action: 1) hurl the Carnegie Institution into the fray with a grant to Beams; 2) encourage the Naval Research Laboratory to make the grant to Beams and organize the enthusiastic physical scientists as an advisory committee to the Naval Research Laboratory; 3) do nothing; and 4) convene a meeting of the specialists in hopes of achieving a better estimate of the prospects for achieving and using chain reactions. The fourth option, though consistent with Carnegie Institution traditions, Bush reported, "I rather dislike ... because it may get people stirred up unduly, and they are stirred up enough now." Doing nothing "would be the way that I would be inclined to move if this were a peaceful and reasonable world. Since it isn’t, the difficulty with doing nothing is that one is not likely to know what others are doing."\footnote{ Ibid.} Convincing the Naval Research
Laboratory to act would relieve him of responsibility, but he had no statutory leverage with which to pressure naval officials and insure continuous support. Finally, if the first option were appealing, Bush would not have needed to write Jewett in the first place.

The advocates of a fission program were looking to Bush to be their Warren Weaver—that is, someone in a position to approve of their goals, acquire the money they needed, and support the creation of institutes within which they could specialize and acquire administrative authority (as had Francis Schmitt). But Bush was not eager to be in a position to judge the ambitions and hunches of specialists and to fashion and coordinate a division of labor among their institutes. He temporarily held off fission’s enthusiasts by combining his first two options: he agreed to consider a grant to Beams to cover expenses for which the Uranium Committee could not find government funds. This solution set well with Briggs, who was finding, without Bush’s help, interest in the Naval Research Laboratory.77

Six weeks later, Bush was presiding over the first meetings of the NDRC, to which the Uranium Committee was assigned to report. This change in jurisdiction did not initially lead to an ambitious uranium program. On 15 June 1940, shortly before its transfer to NDRC, the Uranium Committee had decided it needed $100,000 for isotope separation experiments, $40,000 for experiments to determine nuclear constants, and $100,000 to purchase uranium and graphite in quantities suitable for an "intermediate" experiment in the construction of a nuclear pile.78 The military provided for the first of these recommendations, but the latter two were passed on to NDRC. Although NDRC approved this plan "in principle" in July, only $40,000 for the work on the moderators was forthcoming in September.79


NDRC was not designed to consider the issues that a fission program raised. Fission's advocates needed an administrative authority to make a scientific judgement in favor of trying to produce a useable chain reaction and then to back that decision with funds and authority for procuring materials and dividing labor. However, NDRC was constituted as a committee of generalists to consider the planning, administrative, and entrepreneurial problems associated with reorganizing academic scientists for research into military functions. It was not a useful "sounding board" for Briggs, whose presentations to NDRC provoked "an uneasy feeling that the normal procedures of the committee were not suitable for what was, at least historically, a special case."^80

Though hardly enthusiastic, Bush and NDRC did not quash fission research. When E. O. Lawrence became a fission enthusiast,^81 Bush commissioned the National Academy of Sciences to review the uranium program in April 1941. Lawrence landed on the review panel, as well as the University of Chicago's Dean of Physics Sciences, Arthur Compton, who had already requested that members of his physics department investigate the possibility of achieving a chain reaction. Two Academy studies failed to address the feasibility of making a bomb while still urging an aggressive research program.^83 But these failures were offset by Bush's and Conant's exposure to the thinking of refugee scientists in Britain. Among the world's physicists, only Rudolf Peierls and Otto Frisch in England used Niels Bohr's and John Wheeler's published theory of uranium fission to predict that fast neutrons would fission U-235 and to estimate the critical mass for a bomb.^84 When their findings

^80Conant, My Several Lives, 274.

^81Lawrence's interest was piqued by his laboratory's isolation of plutonium, which fissioned readily like U-235. See ibid., 33-37.

^82Arthur Compton, Atomic Quest (New York: Oxford University Press, 1956), 40-45. They were joined by W. D. Coolidge, John Slater, and J. H. Van Vleck who were accomplished in modern physics without having dedicated themselves to radioactivity studies in the 1930s.


reached Washington in July, 1941. Bush and Conant moved towards serious action. In October, Bush sought and received Roosevelt’s permission to make an all-out effort to investigate the possibility of an atomic bomb; funds were available in early December. 85

Conclusion

The contrasting beginnings of the Radiation Laboratory and the Manhattan Project illustrate the range of issues around which Bush believed NDRC should construct a research policy. The attraction of specialists in nuclear studies to the pursuit of uranium fission was, for Bush, no cause for administrative action in a peaceful world and initially cause only for wary, limited action in a war-threatening atmosphere. NDRC was set up to learn and assess the military services’ needs; then to use chosen universities to organize academic researchers so that their interactions would naturally produce militarily relevant knowledge and instruments. From this perspective, a burgeoning investigation of microwave radiation at MIT made perfect sense. NDRC was not intended as a court of judgement on the plausibility of specialists’ speculations or the coordinator of dispersed researchers working on interdependent projects. When the advocates of a fission program pressed such issues on NDRC, the committee balked at accepting responsibility, commissioned studies from the National Academy of Sciences, and agreed to an ambitious program only after learning of the seriousness with which scientists in war-ravaged England viewed fission.

This distinction between the type of issues raised by the two programs was undoubtedly not so sharp in the minds of NDRC’s individual members as it can be drawn here. As successful and respected administrators, they understood that their self-confidence had to be combined with a willingness to consider exceptions to their usual manner of operating. Nevertheless, NDRC’s structure and its bureaucratic

85Hewlett and Anderson, New World, 44-49. Bush did commission a third Academy report to confirm the British findings.
relations in Washington did make it easier to inaugurate radar over fission research. In the context of defense preparedness amidst signs of impending war, the contrast was obscured by the secrecy of policy deliberations and the political will to fund prestigious researchers with hunches whose infeasibility could not be demonstrated. By the end of the war, the contrast was further obscured by both research programs' successes. But as a matter of general principle in the context of enduring government patronage of academic research, the relationship of structure to strategy in science policy had to be addressed.
CHAPTER 9

WARTIME RESEARCH ADMINISTRATION II:
NDRC STRATEGY AND THE CREATION
OF POSTWAR ALLIES AND FOES

The manner in which wartime laboratories were formed had an enduring impact on the evolution of the laboratories, the political sensibilities of scientists inside and outside the laboratories, and the ability of NDRC to manage dissent and plan for the future. These experiences became the foundation for differences among scientists over how to structure a permanent government agency for the support of non-governmental research. Researchers in the MIT Radiation Laboratory experienced different kinds of frustration and satisfaction from researchers in the Manhattan Project, and those researchers not mobilized at all by NDRC experienced nothing but frustration whose cause, under wartime secrecy, could not be altogether directly discussed.

For the scientists recruited for the Radiation Laboratory at MIT, NDRC's decentralized management and willingness to delegate intellectual initiative enabled the scientists to feel a sense of scientific freedom through local self-government of the laboratory's affairs. However, these scientists had to learn to work with the full spectrum of specialists and specialties needed to produce a militarily useful device. Dissent came from individuals who feared that these unfamiliar research boundaries would undercut their effectiveness. But within NDRC's decentralized structure, such dissent could be accommodated through debate, negotiation, and compromise without fundamentally altering NDRC's structure. These scientists tended to be allies of Bush in the postwar political debate over science policy.
The scientists left out of the war program were embittered and insulted. They naturally preferred to blame NDRC's policies for their alienation rather than question their own qualities as scientists. And they considered how a different governmental structure could make use of and encourage their style of specialization. During the war, NDRC was largely able to ignore these outsiders because of its secure standing within the Roosevelt administration. However, well before the war's end, the un-mobilized scientists began to reach out to potential allies in Congress to gain a political voice in postwar policy.

The scientists in the Manhattan Project found war work a mixed blessing. On the one hand, they were able to create a centralized authority to divide the research labor in conformance with their sense of scientific specialization. But on the other hand, they found that their military managers could use their organizational framework to compartmentalize their activities. They resented this intrusion into normal laboratory operations and doubted that any benefits in security offset delays resulting from the limited dissemination of broadly relevant information. Although these scientists had no cause to feel underemployed, as did their unmobilized colleagues, they developed a similarly jaundiced view of research administrators in Washington and focused their political thought on how to keep control of their working relations so as not be exploited again by outsiders.

Local Orientation, Decentralized Management, and Scientific Freedom

Retrospective pieces about MIT emphasize that the Radiation Laboratory was a laboratory at MIT and not an MIT laboratory. As a statement expressing that the staff ultimately came from many institutions and that MIT's reputation received a windfall benefit from the contributions of these outsiders, it is correct and appropriately modest. Yet while the laboratory was not MIT-staffed, it was, in origins, organization, and denouement, an MIT laboratory—just one that benefitted

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from the wartime conditions that made national recruitment possible. The laboratory was initiated in much the way Compton had pursued his intra-MIT reforms, it was based on substantive research that MIT had considered appropriate for an academic setting, and it left MIT stronger in what had already been one of its local specialties.

Bush's insistence that E. O. Lawrence serve on the Microwave Committee was an insightful extension of MIT's institutional history. When the Committee, after learning of the resonant cavity magnetron, united behind establishing a MIT laboratory with a staff of academic physicists, Lawrence became the new laboratory's chief recruiter. Just as MIT in 1930 needed a president of Compton's stature and achievement in the physics of the 1920s to convince physicists that MIT's traditions could support a properly structured physics program, so NDRC in 1940 needed a nationally recognized leader in the physics of the 1930s to convince his colleagues that a new, secret laboratory scheme at MIT would be structured to make the best use of their scientific skills. Lawrence's involvement persuaded Lee DuBridge, a professor at Rochester who had done research in both electron and nuclear physics, to accept appointment as the laboratory's director. Even Bowles, who had every reason to be proud of and call attention to his success at making Round Hill a home to physical research, saw Lawrence's usefulness: "No other scheme would have brought in such a great bunch. They [physicists] needed the leadership of a nuclear physicist to get them recruited."

Lawrence's effectiveness notwithstanding, Bowles' advocacy of microwave research at MIT in the 1930s was present at the Radiation Laboratory's inception. Of the 40 initial staff members of the Radiation Laboratory, eleven were MIT affiliates; in addition to Bowles' service on the Coordination Section and Microwave Committees, the electrical engineers Wilmer Barrow and William Hall each served on two sections for research into components, and four MIT physicists—Slater, Stratton,

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Morse, and William Allis—and one electrical engineer, L. J. Chu, were on the Radiation/Theoretical Problems section.\(^4\)

The concentration of MIT physicists in the Theory Section was in keeping with the department’s use of technological phenomena to challenge theoretical capabilities.\(^5\) When Compton, just before Roosevelt approved the formation of NDRC, asked his department chairmen for recommendations on how their departments could aid national defense preparedness should the request for aid arise,\(^6\) Slater passed the letter around and found his staff urging that the theoreticians remain together as a group to avoid being wasted and scattered on routine calculations.\(^7\) Slater recommended to Compton "operating the department a little as a firm of consulting physicists would be run."\(^8\) That was how theorists initially operated in the Radiation Laboratory—as a panel of consultants to the (often nuclear) physicists who were struggling to develop their intuitive sense of microwave work while piecing together a workable radar system.\(^9\) Slater, typically, sought to provide an educational background suited to the experimental research. He lectured (as did Edward Condon) regularly to the staff in the early going\(^10\) and quickly turned these

\(^{4}\) "MIT Microwave Laboratory Staff, 11 December 1940," National Archives - Waltham Branch, RG 227, Historian’s Office, Box 25, 1940 Folder. Barrow was on the Transmitter Tubes and Receivers Sections; Hall on the Parabola and Cathode Ray Tubes Sections. Of the other three MIT affiliates, two were graduate students in electrical engineering and the third was a recent graduate who had started his own electronics firm.

\(^{5}\) See Sections 6.2 and 6.3, above.

\(^{6}\) Compton to Department Heads, 29 May 1940, John Slater Papers, American Philosophical Society, microfilm copy at MIT Archives.

\(^{7}\) Julius Stratton to Slater, 3 June 1940; Philip Morse to Slater, undated, concurs with Stratton and reports Manuel Vallarta also in agreement. Both letters in ibid.

\(^{8}\) Slater to Compton, 5 June 1940, in ibid.

\(^{9}\) The Laboratory did not even form formal groups to develop theories for magnetron operation and microwave propagation until the winter of 1942. See Guerlac, Radar, 625 and 633.

\(^{10}\) Ernest C. Pollard, Radiation: One Story of the MIT Radiation Laboratory, (Durham: Woodburn Press, 1982), 46.
lectures into a textbook "sufficiently fundamental so that it could be published as an unclassified book, and yet which I felt would help along the research effort."\(^{11}\)

The rapid growth of the Radiation Laboratory swamped the concentration of MIT affiliates, who tended to leave the laboratory for other responsibilities as they arose.\(^{12}\) Slater spent most of the war at the Bell Laboratories working on magnetron theory and construction.\(^{13}\) Bowles accepted a post as special advisor to Secretary of War Stimson and involved MIT colleagues in studies of the military use of radar.\(^{14}\) Barrow organized and directed the MIT Radar School for military officers.\(^{15}\)

The departure of MIT affiliates, however, did not reduce the influence of the MIT-style origins that NDRC stamped on the Radiation Laboratory. The Laboratory’s success was due to its ability to internalize paradoxical, if not antagonistic, characteristics. On the one hand, the laboratory’s historian has described it as ruthlessly utilitarian:

> The conditions and objectives of research [in the Radiation Laboratory] were widely different from what most of the men had been accustomed to. It was applied science; and it was also wartime science .... The wartime urgency of the work meant that a wholly logical, planned attack on a problem as in peacetime, was almost never feasible.\(^{16}\)

Yet on the other hand, the laboratory inspired loyalty from its scientists as scientists:

> One of its [the Radiation Laboratory’s] outstanding merits, in the eyes of its own management, was that it was a physicist’s world, run for, and as completely as possible by, physicists. Everything was subordinated to

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\(^{12}\)By March 1941, the staff numbered 140 with 95 physicists and engineers; by July the figures were 225 and 140, and the biggest rate of growth came after Pearl Harbor. Guerlac, "Radar," B-II-56-8.

\(^{13}\)Slater, *Scientific Biography*, 211-214.

\(^{14}\)For an account of Bowles’ activities in Washington, see Kevles, *The Physicists*, 309-312. On the personnel Bowles took with him, see Wildes *Century*, 199-207.


\(^{16}\)Guerlac, *Radar*, 265.
producing an environment for research as free and untrammeled as in a university .... The Radiation Laboratory came close to realizing a scientist’s dream of a scientific republic, whose only limitation was the supply of scientists.  

Devotion to questions of warfare is not what one customarily associates with scientific utopias. However, Compton believed (and at MIT had demonstrated) that physicists could exercise their skills and satisfy their curiosities while working in a university research environment that encouraged scientists to work with engineers in the investigation of technologically relevant phenomena. The Radiation Laboratory, though larger in staff, narrower in technological focus, and more insular in dissemination than anything MIT assembled for research in peace-time, still was based on a Bush-like stress on the multi-departmental interests in a piece of instrumentation, a Bowles-like stress on researching all aspects of relevance to an engineering system, and a Slater-like stress on expanding physical theory into engineering realms. In the 1930s, Bush and Compton had proposed the Rockefeller Foundation fund their local rearrangements of researchers’ working relations; as leaders of NDRC, they supported war-relevant versions of the kinds of proposals they had submitted to the Foundation.

The Radiation Laboratory was able both to be utilitarian and to inspire scientific loyalty because of NDRC’s decentralized management. On paper there was a clear chain of command from Bush and Compton to Loomis and the Microwave Committee to DuBridge and his steering committee to the leaders of the various Laboratory sections. However, once NDRC leadership established the Laboratory, it quickly left the business of setting the Laboratory’s technical agenda. Initially the

\[17\text{Ibid.}, 297.\] Ernest Pollard caught the same paradox from his perspective as a researcher: "It [the Radiation Laboratory] had an urgent, extremely practical mission and so it could not, for very much of the time, be dealing with the very research frontier of science .... All the same, the laboratory did generate something like five Nobel Laureates, so it can not have been too bad a breeding ground for first rate science and first rate scientists."  \textit{Pollard, Radiation, xix.}  

\[18\text{NDRC nearly killed the Laboratory at its inception by ordering it to develop an airborne intercept system to aid Britain in the air warfare over its territory. Not until March 1941 did the laboratory produce a testable system, which did not work well. In the interim, the British had succeeded in turning the tide in the air war with the use of their longer wavelength radar. Prospects for the} \]
Microwave Committee supervised the budget and military and industrial contacts with DuBridge and his steering committee responsible for "scientific decisions." However, as researchers and visiting officers from the armed services generated suggestions even faster than space and personnel could grow, the need to set priorities on the basis of the technical feasibility and engineering impact of the suggestions made a mockery of this division of authority. The Microwave Committee steadily retreated, following NDRC's decentralizing example. By early 1942,

the Laboratory Steering Committee took the lead in formulating policy ... [including] deciding what priority a project should have .... As time went on, the Microwave Committee took on more and more the character of a Board of Trustees ... willing to delegate to the Laboratory administration the effective direction of the NDRC radar program.¹⁹

Decentralization did not stop at the laboratory director's level. DuBridge and the steering committee did not treat the laboratory's sub-divisions as bureaucratically rigid categories and themselves judge when the needs identified by a division or the opportunities created by a division should carry over to another. When the laboratory was small, "Men drifted across them [sub-divisions] freely to aid one another in a tight spot, even sometimes to trespass to good effect upon someone else's preserve."²⁰ As the laboratory grew, necessitating more formality, DuBridge made a policy of keeping researchers on components and the developers of systems interdependent and in constant communication. While this policy occasionally resulted in points of friction and disagreement ... [n]evertheless the interaction of the two sorts of groups upon each other was an invaluable source of stimulation and mutual education .... [B]etween the two constant

¹⁹Ibid., 295-296.

²⁰Ibid., 265.
cooperation was unescapable; and by the participation of components men in the work of various systems divisions a new improvement in shipboard radar might be carried over, if applicable, to the airborne projects.\textsuperscript{21}

Having received control of outside contacts from the Microwave Committee, the Steering Committee turned that power over to the Laboratory's Divisions to foster collegiality between researchers and members of the armed services.

The Divisions ... [were] nearly autonomous in their relations with the Armed Services and with industrial concerns. Some of the most fertile discussions with the Armed Services leading to the design of new equipment took place in direct exchange between Division Heads, Group Leaders, and ordinary staff members and Army and Navy representatives .... This administrative decentralization prevented the Director's Office from becoming a single bottleneck .... The Director was frequently consulted, but he was called on to intervene only ... when basic policy considerations were involved.\textsuperscript{22}

With intellectual initiative resting entirely in the Radiation Laboratory, Bush's job was entrepreneurial—to see to it that the centralized armed services knew and took advantage of the fruits of his decentralized research organization. This task required that Bush be able to participate in discussions of military tactics and be able plausibly to threaten military leaders with an appeal to their political superiors when he thought insufficient importance was being attached to laboratory developments. Roosevelt made plain that Bush was entitled to argue military tactics with career officers and to raise his concerns with Roosevelt himself when Roosevelt agreed to subsume NDRC in the Office of Scientific Research and Development, to appoint Bush director, and to make the OSRD director the chairman of a Joint Committee on New Weapons and Equipment, which reported to the Joint Chiefs of Staff. Bush found these privileges especially valuable when the incorporation of a new weapon into military tactics threatened to alter jurisdictional boundaries between the armed services—as when the use of air-to-surface-vessel radar in an offensive campaign against German submarines implied bringing the Army Air Force into a theater that

\textsuperscript{21}Ibid., 311-312.

\textsuperscript{22}Ibid., 313.
had been the Navy's preserve. But in no sense, as others have rightly noted, should Bush's access to political and administrative authority be interpreted as a centralization of control over the research agenda.

The sense of scientific freedom that the Radiation Laboratory enjoyed stemmed from NDRC and the Microwave Committee taking to heart Jewett's advice and Harry Hopkins' approval of delegating authority to existing agencies and holding them accountable for results. Once the NDRC and the Microwave Committee had set the parameters of the intellectual environment through its own internal organization, recruitment of leaders, and placement of the laboratory, the laboratory was able to create its own agenda and make its own outside contacts. The parameters, however, reflected the character and aesthetics of NDRC's leaders. The Radiation Laboratory was an intensely local organization, which brought physicists and electrical engineers together to explore properties of the electromagnetic spectrum and to develop detection devices. Ideas and projects were generated through the interactions of laboratory members with different jobs and perspectives and through an understanding of the combat conditions in which the radar units would have to prove useful. Under the conditions of secrecy that enveloped war work, there was no way for anybody from outside the Radiation Laboratory to participate. Yet even if secrecy had not existed, it would have been difficult for researchers geographically separated from the daily exchanges among scientists, engineers, and military personnel to help or compete with such a localized concentration of appropriately selected experts.

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24 Dupree, "Great Instauration," 453 dismantles the perception of Bush during the war as "a science czar."
Localism and the Complaints of the Unmobilized

The minutes for the 25 October 1940 NDRC meeting note that Compton stated that he took no part in the decision to place the Radiation Laboratory at MIT.\textsuperscript{25} This acknowledgement of the need to avoid conflicts of interest seems half-hearted insofar as Compton’s absence left responsibility for the decision largely in the hands of Bowles, Loomis, and Bush, who all had ample reasons for feeling a sense of pride in and loyalty towards MIT. Yet it is entirely unnecessary to impute crass motives to NDRC’s leaders in order to explain their decisions. Roosevelt had vested power in Bush, because he had clear ideas on how to handle a pressing situation, and Bush relied on his own, previously acquired knowledge, instincts, and contacts to help implement his ideas. The problems NDRC’s leaders encountered were not due to moral failings but structural conditions. In creating the Radiation Laboratory at MIT, NDRC’s leaders were doing what they knew how best to do. But the development of local, multi-disciplinary research centers as government-funded national research policy was not the same thing as their development as MIT’s strategy for institutional development and differentiation.

During the 1930s, Compton had experienced some difficulties in finding a prestigious outsider who viewed the development of a physics program appropriate to MIT as a career opportunity, and he landed and held a prestigious biologist only by giving up on the idea of using MIT to incubate a new field of Biological Engineering. However, in wartime there was no quibbling among recruited scientists over whether intellectual unity could be forged among the selected specialties at the local centers. Whether out of patriotic fervor, gratefulness at being spared more dangerous duty, or both, the recruited scientists undoubtedly felt inspired to do the best they could with matters as NDRC’s leaders had organized them.

The emergency conditions that made nearly any asked scientists willing to serve also created the presumption that everyone with specialized scientific training ought to be using his expertise for the war effort. Back in 1927, when Compton

\textsuperscript{25}Guerlac, \textit{Radar}, 259 and note 13 on 301.
advocated that universities concentrate on developing coordinated groups of specialties, he noted that one advantage of a policy of supporting all departments across the board would be to save the administration the embarrassment of dealing with specialists left outside the concentrated effort.\textsuperscript{26} At MIT he was rarely plagued with such complaints, in part because scientists whose ambitions did not fit with MIT did not come and in part because Compton was interested in extending his approach to forming specialties to all scientific disciplines.\textsuperscript{27} However, working at the national, rather than the local MIT level, his 1927 insight came true.

In May 1941, Thorndike Saville, Dean of Engineering at New York University, addressed the Middle Atlantic States Section of the Society for the Promotion of Engineering Education and pointed out that NDRC contracts were going predominantly to NDRC’s members’ home institutions while several leading engineering schools had few or no contracts.\textsuperscript{28} Saville’s complaints filtered back to Compton, who privately presented three arguments to Saville: first, contract recommendations to NDRC came from its divisions’ committees (like the Microwave Committee) and against that broader base of personnel the charge of self-interest on the part of NDRC leadership was less serious; second, maximizing the speed of NDRC’s response to military requests had to be the prime criterion in placing a contract with an institution; and third, "Most of them [the contracts] have been in the fields of chemistry and physics" while "relatively few … are in the field of engineering."\textsuperscript{29}

Compton’s use of the terms "physics," "chemistry," and "engineering" was duplicitous. He knew better than anyone that disciplines were not the organizing

\textsuperscript{26}See Section 4.3, above.

\textsuperscript{27}The exception to this statement is Compton’s reluctance to make cosmic ray research a MIT program. See Section 6.4, above.


\textsuperscript{29}Compton to Saville, 26 June 1941, attached to Compton to Bush and James Conant, 26 June 1941, Library of Congress, Bush Papers, Box 26, K. Compton file.
principle that NDRC followed. However, Compton did not wish to become embroiled in a dispute with engineers whose work he was dedicated to seeing reorganized through cooperative arrangements with scientists. To Bush and Conant, Compton recommended the expedient of coopting Saville (and giving the headache of managing Saville to someone else): "I wonder whether it would not be a good plan to take Saville into camp by appointing him ... [to] Jewett's Division, probably under Transportation."\(^{30}\)

Compton did attend the 1942 meeting of the Society for the Promotion of Engineering Education. He made a general address on OSRD's work and organization to the conferees and explained to the society's administrative officers that the placement of the Radiation Laboratory at MIT was due exclusively to the availability of facilities at the Boston airport and that the staffing of the laboratory with physicists was due to the technical similarity of cyclotron operation with short-wave radio work.\(^{31}\) Both points were partial truths. MIT's research into directed radio beams, in addition to its location, were important to its selection as the site for the Radiation Laboratory, and the integration of components into durable radar systems, not just the development of better ways to generate and detect microwaves, was the criterion on which NDRC judged the Radiation Laboratory's performance. Nevertheless, Compton's partial truths evidently mollified the SPEE, for it did not again become a forum for complaints about NDRC policies.

Having used the argument that NDRC was concerned with science rather than engineering to assuage the concerns of Saville and the SPEE, NDRC had thornier problems dealing with scientists who were not among the groups of specialists that NDRC chose to mobilize and coordinate. Harry Grundfest, a biologist at Columbia and an officer of the New York branch of the American Association of Scientific Workers, badgered the National Academy of Sciences in late July 1942 with the

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*\(^{30}\)Ibid.*

contention that
most biologists, physiologists, botanists, geologists and a considerable number of geochemists, chemists and bacteriologists have not yet engaged in any scientific work related to the war. Despite the channels that have been established ... many willing scientists ... have been unable to participate in war work. They lack knowledge of the subjects of military significance, even though they are familiar with the major peacetime problems in their special fields.¹²

Answering Grundfest's letter proved to be a tricky operation, because it elicited sympathy within the Academy. The letter landed on the desk of Joe Morris, Director of the Office of Scientific Personnel at the National Research Council, who solicited the help of Carroll Wilson, a close aide to Bush, who bumped the letter to Frederick Hovde, another aide. Hovde provided a memorandum listing 11 itemized points whose essence was that imbalances in OSRD's personnel policies were due to its specific job to serve the military and the need to concentrate research in special laboratories. Hovde admitted there were untackled problems of civilian defense and war production, but in keeping with OSRD's determination to steer clear of jurisdictional conflict with members of Roosevelt's cabinet, Hovde recommended Grundfest be directed to work with the state government on such matters.

Hovde had accurately reflected OSRD views; however, when Grundfest's letter worked its way up through the Academy to Frank Jewett, then president, he asked Bush to comment.¹³ Wilson intercepted the letter and added his own memorandum. Wilson noted that NDRC's organization along military functions failed to create a niche for these scientists: "Moreover, it is their biologists ... who are not yet utilized in the war effort to any great degree. I suppose the real answer is that the kinds of problems which require solution are not those which people of these


¹³Hovde to Wilson, 7 August 1942, attached to Wilson to Morris, 7 August 1942, ibid.

¹⁴Jewett to Bush, 24 August 1942, in ibid.
backgrounds are especially qualified." Bush evidently drafted a response advising Grundfest to contact the Inventors’ Council in the Patent Office—Bush’s standard technique for getting rid of people with whom he did not wish to be bothered—but Jewett blocked the letter. In the interim, Ross Harrison, chairman of the National Research Council, had informed Jewett that he found "many of the things which they [Grundfest and co-authors] suggest are good and they are undoubtedly an earnest lot of people." Jewett did not want Bush’s letter to undercut Harrison’s efforts to mobilize biologists or to give the appearance that OSRD, NAS, and NRC were at cross purposes.

Bush’s final response did not recommend Grundfest contact the Inventors’ Council, but its tone was dismissive:

The term ‘technical war,’ which is frequently applied to the present conflict, might be assumed to embrace activities in all fields of science. To date, however, it has seemed neither profitable nor necessary to set up projects which would utilize the entire available manpower of those having scientific training.

Bush had set up NDRC as an active broker between the military’s generic technological needs and the academic community’s knowledge and expertise; from his perspective, scientists whose peacetime specialization left them out-of-touch with subjects of military significance had specialized poorly. The fact that such scientists were unmobilized did not constitute evidence that NDRC was doing a poor job.

Nor did one have to be an engineer, biologist, or officer in a scientific society with an openly political flavor to feel snubbed by Bush; it was sufficient to seek the use of national scientific societies in the organization of war research. At the August, 1944 meeting of the American Mathematical Society, M. H. Stone, the chairman of

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36 Jewett to Bush, 9 September 1942, in ibid. Jewett questioned the wisdom of telling Grundfest to contact the Inventors’ Council; by inference, that’s what Bush’s letter must have recommended.

37 Harrison to Jewett, 29 August 1942, in ibid.

38 Bush to Grundfest, 11 September 1942, in ibid.
Harvard's mathematics department and the society's president, bitterly recounted his frustrations in calling NDRC's attention to the society's efforts. According to Stone, the society formed a War Preparedness Committee (with sub-divisions for research, education, aeronautics, ballistics, computation, cryptography, and industry) in response to the German invasion of Poland, and offered the committee's services to NDRC after the fall of France. "Apart from receiving an acknowledgement of its offer, the committee made little progress towards a more specific understanding of its potential usefulness." The formation of OSRD one year later inspired the mathematicians to repeat their offer, but Bush and Conant directed them to the National Research Council, which was not in position to organize or initiate research. When OSRD did organize an Applied Mathematics Panel, it did not consult the NRC committee, and the panel itself "would display considerable reluctance to call on the leaders of our profession." In Stone's opinion, many of the best mathematicians did not work for OSRD but for one of the armed services.39

Grundfest persisted in seeking sympathetic listeners in the government. He complained in political terms to Leonard Carmichael, the president of Tufts University, who had set up a National Roster of Scientific Personnel in the Department of Interior:

'It still seems to many of us ... that the full potentialities of our scientists are not being called forth. Frankly, I cannot see the OSRD and the National Academy doing very much about or even recognizing the shortcomings of our scientific mobilization. They are acting like a collection of elder statesmen, holding control of everything tightly and unable to see that a more democratic organization of our war science would release the enthusiasm and capacities of the great majority of our scientists.40

The "elder statesmen" were not tightly holding everything. Rather they had no detailed a priori vision of what would be militarily valuable and scientifically possible. They were delegating intellectual responsibility to investigate broadly


40Grundfest to Carmichael, 10 April 1943, copy in National Archives, RG 227, Item 13, Box 27, Cooperation - AASW file.
construed military functions to local centers where the interactions of relevant specialists were expected to lead to devices and tactics that the "elder statesmen" peddled in strategy-making circles. What Grundfest meant by "more democratic organization of our war science" was a more nationalist regime in which the "elder statesmen" broke down military needs into problems suitable for dissemination among scientists generally. The more scientists competing to solve a war problem, the more likely a solution would be found for NDRC members to transmit back to its military contacts.

Managing Dissent Within OSRD

Bush seemed unjustifiably autocratic to the petitioners he rejected. However, what probably made Bush confident in dismissing his critics was that they were not telling him anything he was not already hearing from within NDRC and finding ways to manage through NDRC's extant structure. Industrialists in NDRC were sounding like Saville—questioning whether the support of researchers in university "science" laboratories overlooked the virtues of performing research within manufacturing organizations. Scientists accustomed to independently defining their research agenda in response to friendly competition with scattered colleagues with shared objectives were sounding like Grundfest—worrying that they were being pulled in too many directions in local centers with heterogeneous personnel. The threat that the ideals of Walker and Noyes could upset Bush's and Compton's government structure for Sedgwick-style research policy came to the fore with American entry into the war.

After Pearl Harbor, radar research could only be viewed as a relatively long-term venture that would receive increased support. NDRC had to confront the administrative and political advisability of concentrating expanding resources at the Radiation Laboratory, whose prewar growth had already raised some eyebrows. Pressure for expanding the Radiation Laboratory came from the laboratory's upper management, DuBridge, Loomis, and Compton, who viewed the Radiation Laboratory as a nucleus that could usefully coordinate expanding functions. Objections came from industrial representatives on the Microwave Committee, who had allies-of-
convenience among some laboratory researchers and a spokesman at the NDRC level in Frank Jewett.

The case for expanding the Radiation Laboratory rested on an enlarged laboratory's capabilities to address what its administrators viewed as its two major pitfalls. First, the Laboratory's standards for a convincing case for improved military equipment were not the same as the armed services: "Experience has shown that it is not sufficient to make a bread-board model of a new device in order to get it accepted by the services."41 Second, industrial producers with their own laboratories, particularly Bell Telephone Laboratories, had taken to re-engineering the Radiation Laboratory's designs, thereby recreating flaws that Radiation Laboratory researchers had already learned to avoid.42 DuBridge complained to Compton: "Experience has shown that in cases where there is no official recognition of NDRC in connection with a contract [between a military service and industrial producer] neither the Radiation Laboratory nor the producing company feels authorized to proceed with complete exchange of information."43 To ameliorate the first problem, the Laboratory had already established a Model Shop in October, 1941 to produce small numbers of prototypes that would make a more compelling case for acceptance by military officials.

Expanding the Model Shop was one way to deal with the second problem. By placing the design of a manufacturing process under the control of the Radiation Laboratory, the Laboratory's administrators could hope to eliminate re-engineering that diminished the improvements achieved in the laboratory. But in the eyes of NDRC's industrialists, that endowed the Model Shop with the potential to be a

41 "Memorandum on the Future of the Radar Research and Development in the United States," 3 February 1942, anonymous rough draft but I suspect DuBridge to be the author, National Archives, RG 227, Division 14, Contracts OEMsr -- 262, Box 58, Radiation Laboratory Steering Committee Minutes File for 1942.

42 Guerlac, "Interview with Karl Compton," 20 August 1943, National Archives - Waltham Branch, RG 227, Historian's Office, Box 43, Microwave Committee folder, 7-9.

43 DuBridge to Compton, 4 February 1942, National Archives - Waltham Branch, RG 227, Director's Office, Box 4.
government-owned factory that would compete with private firms in radar production. To assuage these fears, Compton and Loomis appointed an oversight committee consisting of DuBridge, representatives of six firms concerned with radar production, and a neutral industrialist as chairman, H. A. Poillon, president of Research Corporation. Furthermore, as a general rule, the Model Shop was not to accept orders for over $100,000. In the short term, the industrialists fears were well founded, for the Model Shop was used to fill small orders of new radar sets when it appeared that industrial firms, clogged with larger orders for older equipment, could not speedily respond. When the Microwave Committee discussed the first production contract for the Model Shop, its members made clear that such contracts had to be exceptional:

Several [members] realized that it [the contract] conflicted with the concept under which the Model Shop was established, namely the construction of only a few items of any given type. That the contract was made upon special request of Dr. Bush and as an emergency was also stated. This background, not previously known to some members of the Committee, clarified the situation and eased any anxiety over possible conflict with industry.

In the long run, the Model Shop turned out to be a strictly wartime institution; MIT’s desire to build on the legacy of the Radiation Laboratory following the war did not extend to running a factory for the production of new radars.

The second route to keeping the manufacturers from recreating problems that the Laboratory had solved was to increase the information flow between the Laboratory and manufacturers. Compton obtained appropriate letters stating that as a consultant to the military services, NDRC was authorized to engage in information

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44Guerlac, Radar, 287-288. The first crash production order came 17 December 1941.

45"Informal Evening Microwave Committee Meeting," 19 February 1942, National Archives, RG 227, Division 14, General Correspondence, Box 5.

46See Section 9.5, below.
exchanges with producers. But the manufacturers were structurally inhibited from reciprocating. Restricting the scope of the university staff at the Radiation Laboratory was a point of pride among industrial researchers, who did not wish to see their preeminence in development and production problems challenged. Furthermore, patent issues, which could be prickly enough in peacetime relations between universities and manufacturers, were potentially more difficult during war. In peace, manufacturers could negotiate a purchase or licensing agreement for patent rights granted to MIT or one of its faculty, but no such possibility existed for the Radiation Laboratory. MIT had no legitimate claim to patents coming from Radiation Laboratory work (since the staff came from a myriad of institutions), and individual Laboratory researchers were not allowed to patent inventions to prevent intra-Laboratory jealousies and suspicions. By default, all Radiation Laboratory patents were dedicated to the public.

Although upper laboratory management favored expansion, the industrialists' views were reported to the research leaders, and expansion did have an in-house critic in the Nobel Prize-winning physicist I. I. Rabi. However, Rabi's criticism of

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47 The latter point was implied in DuBridge to Compton, 4 February 1942, National Archives, RG 227, Division 14, General Correspondence, Box 5.

48 Compton stressed this point in his interview with Guerlac.

49 In 1939, when planning research with Loomis, Bowles found Jewett preferred not to allow MIT and Bell researchers to confer on ultra-high frequency radiation lest "some individual concerned felt that his ideas discharged at a conference had resulted in a patent application being filed quite innocently to the advantage of some one else." Bowles to Jewett, 9 November 1939, and Jewett to Bowles, 21 November 1939, MIT Archives, AC 4, Box 3, Folder 3.

50 Bush, Pieces, 83. OSRD did offer universities the option of a contract that left the university with commercial rights to patents developed under OSRD sponsorship while giving the government a royalty-free license to use the patent for defense purposes. See "Academic Contractors," circa October 1945, National Archives, RG 227, Item 13, Box 54, Contract Operations File. This form of contract was referred to as "the standard long form" in contrast to "the standard short form," which dedicated all patents developed under OSRD funding to the public. 65 of OSRD's 80 contracts with MIT were standard long form; by contrast, only 15 of 72 contracts with Columbia were standard long form.

51 DuBridge communicated the industrialists' views to Radiation Laboratory members. See the Minutes of the Radiation Laboratory Coordinating Committee, 14 January 1942, 21 January 1942, and 12 March 1942, National Archives - Waltham Branch, RG 227, MIT Laboratories, Box 2.
expansion had different premises from those of the industrialists. Rabi argued that expansion would create too many subdivisions reporting to DuBridge, would be overburdened with supervising too many people who covered too diverse a spectrum of functions. As an alternative, Rabi suggested collapsing the research program into two divisions for systems (Airborne and Ship & Ground) and two for components (Research and Special Developments). His premise was that such an organization reflected the intellectual parameters with which the laboratory staff was comfortable:

This proposal is designed to group under one leadership those systems or projects which are parallel in their objectives and requirements .... This procedure leads, in turn, to a higher degree of flexibility. Experience has shown that little difficulty occurs in shifting manpower within a group between projects which are fairly similar in nature.52

Rabi's scheme would have explicitly divided labor between research scientists working on components and engineers working on systems. Rabi's scheme had the endorsement of the systems engineer L. C. Marshall,53 who was frustrated with the propensity of the components researchers to keep refining components while he and his colleagues were struggling to create integrated systems for production and use.54 From Marshall's perspective, Rabi's division of labor would have enabled systems researchers to work in peace themselves with components that laboratory management had judged good enough for integration into systems. However, Rabi's proposal did not receive much high-level attention because prevailing sentiment in the Laboratory was that "so much of the stimulus for research and development work comes from seeing results of the work go directly into practical use."55 Having successfully created an interacting, cooperative community that blurred distinctions between

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52I. I. Rabi and L. C. Marshall to DuBridge, 16 December 1941, National Archives, Record Group 227, Division 14 Contract OEMsr - 262, Box 58, folder labeled "Steering Committee Minutes, 1941-1942."

53Marshall was chair of the "Roof Group" which was charged with assembling and trying out radar systems atop the MIT laboratories.

54Guerlac, "Interview with L. C. Marshall," 6 December 1945, National Archives - Waltham Branch, Historian's Office, Box 46.

55Guerlac, Radar, 293.
science and engineering, the Laboratory's administration was not inclined to sunder it.

With industrial members of the Microwave Committee suspicious that increasing Model Shop activities or increasing dissemination of manufacturing processes to Laboratory staff would threaten their interests, and with the Laboratory's most prestigious scientist suggesting that a reorganization along lines favored by the industrialists could create a better research environment, Loomis and Bowles in the winter of 1941-1942 resorted to subterfuge to broaden Radiation Laboratory responsibilities. They passed their recommendations directly to Compton for action at the NDRC level without consulting the Microwave Committee.\(^\text{56}\) They could not have considered this tactic anything but a temporary ploy because of Frank Jewett, who as Bell Laboratories president supervised a Microwave Committee member. In February Jewett realized he was voting as an NDRC member on matters the Microwave Committee had not reviewed. His objections forced Compton to take the case for Radiation Laboratory expansion at MIT to Conant and Bush for a definitive decision.

Compton argued that while the Radiation Laboratory's expansion at MIT would be extraordinary, yet it still represented a continuous, incremental development of basic NDRC policy. "From the beginning," he noted, "it has been the policy of Section D-1 to enlist and build up centers of microwave activity," and MIT had been singled out, in part, because its "Electrical Engineering and Physics laboratories were a great asset." Although the Laboratory's growth to date had drowned out that virtue, the availability of living quarters and airport and harbor facilities in the Cambridge area still made MIT a superior locale for a central laboratory. Sub-dividing the Radiation Laboratory into independent units was inadvisable, for it would defy the "fundamentally similar" character of microwave systems and their constituent components. In a statement obviously aimed at Jewett, Compton opined:

The situation is very analogous to that of the telephone industry which has found it advantageous and efficient to concentrate research in the whole field of telephone systems into one laboratory .... We believe that this intrinsic

\(^{56}\text{Guerlac, "Interview with Compton," 7-8.}\)
characteristic of the main stem of microwave research requires continuation of this central laboratory and that its dispersal will be just as improper as a dispersal of the Bell Telephone Laboratories.\textsuperscript{57}

Furthermore, Compton pointed to the armed services' use of the Radiation Laboratory as a clearinghouse for information on microwave developments and as an advisor to the military's procurement officers who had to set production standards.

The major concession Compton was willing to make was to seek out projects that did not have to be done within the Radiation Laboratory and spread them among other universities. However, this concession did not change the character of the program. The Microwave Committee scrutinized the "farming out" process for security breaches that could jeopardize relations with the armed services.\textsuperscript{58} The major result of the effort was not a general dissemination of research projects but the creation of two new and smaller versions of the Radiation Laboratory: the Radar Counter-Measures Laboratory set up at Harvard and the Tube and Circuit Laboratory set up at Rabi's home institution, Columbia, for the investigation of components for producing microwaves in the one centimeter wavelength region.\textsuperscript{59}

Even theoretical work did not get far beyond the control of the Theory Group at the Radiation Laboratory. A contract for theoretical studies—issued in March 1942 to Cornell University and involving physicists at Cornell, the University of Rochester, and Purdue—did not result in autonomous physicists scattered across the country reporting to a central authority. Rather, the Cornell work, from the start, "was done in close collaboration with various staff members and groups of the Radiation Laboratory," and the major result of the contract was to funnel yet more people into the Radiation Laboratory: "Most of the members of the Cornell projects have become in the course of time members of the Group 43 [the Theory Group] and have

\textsuperscript{57}Notes to accompany letter to Drs. Vannevar Bush and James B. Conant from Karl T. Compton, February 26, 1942," copy in National Archives - Waltham Branch, Historian's Office, Box 43, General file.

\textsuperscript{58}Executive Session, Microwave Committee Meeting, 20 February 1942, National Archives, RG 227, Division 14, General Correspondence, Box 5.

\textsuperscript{59}Guerlac, \textit{Radar}, 289-291.
continued work at the Radiation Laboratory.\footnote{Mark Kac and Harold Levine, "A Terminal Report -- July 1, 1945, Cornell University, Contract OEMsr - 429" and G. E. Uhlenbeck, "Evaluation of Contract OEMsr - 429, Cornell University," respectively for the two quotes. Both documents in National Archives - Waltham Branch, RG 227, Records of Group 43, Box 876.} The Radiation Laboratory thus retained its character as the preeminent center for microwave research and radar development.

The outcome of the insiders’ debate largely favored Compton’s petition to Bush and Conant and reaffirmed the policy of favoring local academic centers where diverse specialists worked interdependently on all technical matters relevant to a military function. The Radiation Laboratory remained a single institution at MIT, and its staff was divided into 10 units (seven of them substantive and three administrative) that all reported to DuBridge. These units covered the full range of microwave issues from fundamental research, headed by Rabi, to systems engineering and production, headed by Marshall.

The industrialists’ suspicions and Rabi’s reorganization proposal were insiders’ versions of the objections of Grundfest and Saville. Like Grundfest, who complained that NDRC was not breaking down military needs into problems that could be worked on by the nation’s corps of scientists, Rabi feared that expanding Radiation Laboratory responsibilities for production would distract scientists from specialized research into components. He resembled Francis Schmitt, who kept paring down the scope of Biological Engineering until it matched the cognitive dimensions he felt researchers could confidently roam. Like Saville, who thought NDRC was overlooking the engineering colleges, the industrialists worried that their competence was being slighted and their interests threatened. Marshall, their in-house champion, resembled William Walker, seeking independence from scientists in order to be able to select scientific accomplishments with which to work.

However, unlike the outsiders, who were either rebuffed or coopted, the insiders were able to reach a livable compromise without changing the basic character of NDRC or the Radiation Laboratory. Rabi was later reported as upset over the
influx of engineers into the Radiation Laboratory, as required by the "follow-through" policy of involving the Model Shop in production. But as head of the Laboratory's Research Division, he could influence the amount of attention his staff was giving to matters he preferred others handle; and he indirectly supervised the Tube and Circuit Laboratory at Columbia, his home institution. Jewett and the other industrialists were surely displeased with the continued preeminence of an academic over an industrial laboratory in an area of governmental interest. But they could limit the Model Shop's production to emergency crash runs, and use, for work in industrial laboratories, an OSRD contract form that granted the contractor the commercial rights to patents created in the course of research. Whatever displeasure lingered over the insistence of Bush, Compton, Loomis, and DuBridge to making a university the home to the integrated study of a military function, that displeasure did not degenerate into a debilitating resentment. Nobody resigned or was demoted; and nobody aired grievances outside of NDRC.

This amicable resolution of policy issues was due to the quality and breadth of debate that NDRC's decentralized structure made possible. With the Radiation Laboratory delegated full intellectual authority for the creation of a microwave program and NDRC concentrating on entrepreneurship and long-range planning, DuBridge could serve as a two-way channel between the laboratory staff and the oversight committees. The various proposals for defining and organizing the laboratory's responsibilities following Pearl Harbor were circulated as a matter of course. The disappointments over the decisions that NDRC made could not be attributed to repression of dissenters' ability to convey their views or ignorance on the part of decision-makers of the full range of concerns among the dissenters.

**National Science, Centralized Management, and Scientists' Disaffection**

The advocates of uranium fission studies were the one, nationally dispersed

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61Guerlac, "Interview with L. C. Marshall," 6 December 1945, National Archives - Waltham Branch, RG 227, Historian's Office, Box 46.
group to gain the attention of NDRC. But the "elder statesmen," as Grundfest
despairingly referred to NDRC members did indeed hold this project closely; in
giving permission, in October 1941, to launch an aggressive research program into
the atomic bomb, Roosevelt restricted the discussion of policy to himself, Vice-

president Wallace, Secretary of War Stimson, Bush, and Conant (plus their immediate
aides). Bush decided to separate the Uranium Committee from NDRC's
jurisdiction—a move that not only made admininistrative sense insofar as NDRC had
been useless in defining a fission program but also helped to limit the circle of policy
discussants among scientists.62 Even if Grundfest or Stone had known of or been
involved in the Manhattan Project, they probably would have been no happier, for the
combination of centralized management, military supervision, and secrecy created a
profound sense of estrangement between the researchers and their administrative
superiors.

Launching the bomb research initially created within OSRD a corps of
scientists who centrally organized their activities in independence of NDRC. The
Uranium Committee, comprised mostly of specialists in nuclear studies, did not
delegate research and design to an intra-university laboratory and leave the university
and laboratory director to recruit personnel, define the research program, and build
working relations with industrial and military officials. Instead, the committee opted,
in December 1941, to break the project into parts according to the conceivable routes
to the bomb and the special abilities of the native-born, Nobel-Prize-winning research
directors available for service. E. O. Lawrence at Berkeley, the inventor of the
cyclotron, was charged with the electromagnetic separation of uranium isotopes and
research into the properties of plutonium. Harold Urey at Columbia, the discoverer
of heavy hydrogen, was charged with separation of uranium isotopes by gaseous
diffusion and centrifuge methods and the prospects for using heavy water as a
moderator for a nuclear pile. Arthur Compton at Chicago, the leading investigator of
cosmic rays, was charged with research into chain reactions—both fast reactions that

62Hewlett and Anderson, New World, 51.
could be used in a bomb and slow reactions that could be used in a plutonium-producing pile.\textsuperscript{53}

Roosevelt's sanctioning of bomb research and the Uranium Committee's willingness to devise a division of labor provided nuclear physicists with the funds to procure materials for large-scale experiments and the authority to deploy personnel through administrative fiat as well as through courteous cooperation. The latter point was especially significant for Arthur Compton. Whereas Lawrence and Urey were expanding and intensifying projects that were peculiar to their campuses,\textsuperscript{64} the news of uranium fission had inspired experiments aimed at chain reactions in a variety of locales. Compton had to concentrate this work geographically, but not, as had the Radiation Laboratory, for the purpose of creating working relationships that would focus researchers on a military function. Compton did not need to interest researchers in a chain reaction; his challenge was to accelerate the work already underway. Under the time and secrecy pressures of war, scientific differences had to be settled quickly and with minimal circulation of paper. While Compton left research on fast neutron reactions dispersed among several universities (eventually, this work was concentrated at Los Alamos), he brought together Columbia and Princeton groups with University of Chicago scientists in a "Metallurgical Laboratory" to consider jointly the design of a nuclear pile.\textsuperscript{65}

These arrangements placed Compton's, Lawrence's and Urey's groups in competition to show which process was the quickest route to the production of pure, fissile material. For the scientists in the laboratories, such arrangements were socially normal. Specialized research performed under the direction of a prestigious leader and in competition with other laboratories was a trademark of nuclear physics in the

\textsuperscript{53}Ibid., 50-51.

\textsuperscript{64}The major exception to this statement is that Jesse Beams' efforts to separate U-235 by centrifuge were located at the University of Virginia and under Urey's jurisdiction.

\textsuperscript{65}Hewlett and Anderson, New World, 54.
1930s.\textsuperscript{a}\textsuperscript{a} The audience for wartime research was not the scientific public, but to reach that public in peacetime, researchers needed the approval of journal editors and referees. So long as the Uranium committee did not use its power to close laboratories or redefine their missions, researchers could view it as a "super editorial board" that would decide the merits of the laboratories' efforts.

Strains developed within the bomb project because decisions entrusted to the central authorities went against the judgement of laboratory researchers. Dividing the research labor among autonomous laboratories left three issues in the hands of the central authorities: selection of the best process for producing fissile materials, the integration of research results with engineering plans to create production programs, and the regulation of inter-laboratory communication. The first of these aroused the least antagonism by virtue of being the least soluble. Bush had Conant try to pierce the enthusiasm of Lawrence, Compton, and Urey for their pet projects in order to decide which process merited further investment.\textsuperscript{b}\textsuperscript{b} When Conant was unable to reach a confident decision,\textsuperscript{c}\textsuperscript{c} he and Bush were able to persuade Roosevelt and the Army to accept a shot-gun approach to producing fissile material\textsuperscript{d}\textsuperscript{d} and thus avoided a major bloodletting in the scientific community.

Integrating research with engineering, in the context of a centrally organized division of research, became a greater source of friction than determining the best route to the bomb. Responsibility for this task initially fell to Bush, but he showed no inclination to hold it himself or to keep it within OSRD and the purview of university officials. Instead, Bush urged Secretary of War Stimson to assign an Army

\textsuperscript{a}\textsuperscript{a}See Section 6.4, above.

\textsuperscript{b}\textsuperscript{b}Conant, Several Lives, 284-287.

\textsuperscript{c}\textsuperscript{c}The inability of corporations to reach centrally rationalized decisions in the face of competition among functionally distinctive subdivisions was responsible for the adoption of the decentralized, multi-departmental structure. Chandler, Strategy and Structure, 295-296.

\textsuperscript{d}\textsuperscript{d}Hewlett and Anderson, New World, 114-115. Only centrifuge methods for separating isotopes was abandoned in 1942, though a review committee had also recommended stopping electromagnetic isotope separation as well.
engineer to manage any production programs that might later be authorized. As
Conant and Bush gained confidence in General Leslie Groves, the Army engineer
assigned to direct the Army's part of the project, they turned all management,
including the contracts with the various university laboratories, over to the Army
Corps of Engineers and themselves retreated to form the Military Policy
Committee—the supervisory body to which Groves could turn for advice and
sympathetic discussion of his problems. Groves moved promptly to enlist several
large corporations to construct, supply, and operate the production plants. In all
cases, he placed responsibility for design and planning with the corporate engineers
who would be responsible for meeting his ambitious schedules.

Reactions within the laboratories to Groves' policy varied. For E. O.
Lawrence and scientists at Berkeley, who were accustomed to the problems of
constructing and operating successive generations of cyclotrons, the transition to
supporting the construction and production efforts of Tennessee Eastman caused the
fewest difficulties. But to work with Columbia scientists, whose leader, Harold
Urey, had turned pessimistic on the near-term utility of gaseous diffusion for
separating isotopes, Groves had to ease Urey out of responsibility for the diffusion
project in favor of the Princeton physical chemist Hugh Taylor and the Union Carbide

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8Leslie R. Groves, Now It Can Be Told: The Story of the Manhattan Project (New York: Da Capo

9Bush, Pieces, 60-61; Groves, ibid., 23-25.

10Ibid., 121-123, 148-149, 186-188.

7Good relations between Lawrence and Groves were due to Lawrence's willingness to let intuition
and experiment outstrip well articulated theory when providing Tennessee Eastman with data of use for
construction and planning. When Groves authorized the construction of a second stage to the
electromagnetic separation plant (in order to guard against the initial stage not yielding sufficiently
enriched uranium), Lawrence's staff had the satisfaction of putting into practice at least some of the
improvements that had occurred to them as they pondered the workings of the initial stage. Ibid., 141-
147, 155-159.
scientist Lauchlin Currie.\textsuperscript{74} However, the strongest reaction came from Chicago. Arthur Compton favored using an industrial firm to design and operate a plutonium-producing pile and separation plant. Yet when he first broached the subject to his nuclear physics staff, he found them assuming that they would direct the design of a functioning pile on the strength of their superior knowledge of nuclear physics.\textsuperscript{75}

Any chance the Met Lab staff had of making its views prevail were squelched when Groves took office. Less than a month after his first meeting with the Met Lab staff, Groves lined up the E. I. du Pont Company to take the entire responsibility for plutonium production—designing, constructing, and operating the piles as well as the chemical separation plants.\textsuperscript{76} Du Pont officials treated the Metallurgical Laboratory as they did their own research division. They required that their engineering division design the piles in Wilmington (with data provided by the Met Lab) and that the Met Lab operate a pilot plant of the engineers’ design. Having lost the initiative in directing design work, Chicago researchers had little enthusiasm for using their expertise to operate du Pont’s pilot plant.\textsuperscript{77}

The Chicago scientists, unlike the scientists suspicious of the Radiation Laboratory’s expansion, went outside channels to try to force an alteration in policy. They surreptitiously tried to petition Roosevelt to provide political oversight of du Pont’s activities (on the dubious grounds that du Pont would otherwise abuse its

\textsuperscript{74}Urey had lost hope for designing a porous barrier to separate uranium isotopes in a diffusion process. A barrier had to withstand both pressure differentials and the corrosiveness of uranium hexafluoride, the only uranium compound which becomes gaseous at earthly temperatures. \textit{Ibid.}, 132-135.

\textsuperscript{75}\textit{Ibid.}, 90-91.

\textsuperscript{76}Groves, 38-46.

\textsuperscript{77}Hewlett and Anderson, 190-192. The Chicago chemists working on plutonium separation processes had fewer difficulties in working with du Pont because Chicago and du Pont chemists had space in the university’s laboratories to work out jointly the equivalent of a pilot plant. There could be no such trust-building operation in the construction of internally-cooled piles, especially after the Argonne site was deemed too small and too close to a population center for the job. See \textit{Ibid.}, 204-205.
privileged position\textsuperscript{78}, and they demanded salaries equal to their industrial counterparts.\textsuperscript{79} Their efforts evoked no sympathy from Bush or Conant, who retrospectively dismissed the physicists' desire to direct planning and construction of the piles as the delusion of European research directors and exuberant young Americans who were ignorant of the difficulties of industrial operations.\textsuperscript{80}

By the winter of 1942-1943, when Groves was ready to form a bomb-fabrication laboratory at Los Alamos under Robert Oppenheimer's direction, the Army's reputation for determining the role of scientists made recruitment difficult for Oppenheimer. At the MIT Radiation Laboratory, he found the physicists unwilling to accept military commissions and suspicious of working under anything but a NDRC contract.\textsuperscript{81} Oppenheimer turned to Bush for support in what Oppenheimer described to Rabi as a "strong and extremely painful attempt to have our project transferred to OSRD." But all Oppenheimer received was Bush's steadfast refusal either to make OSRD responsible for a centrally organized project or to use his double role as OSRD director and chairman of the Military Policy Committee as a route through which academic scientists could indirectly pressure General Groves.\textsuperscript{82}

Oppenheimer, who had already agreed to accept a commission in the Army,\textsuperscript{83} had to negotiate with Groves for terms that he (Oppenheimer) had not personally

\textsuperscript{78}Du Pont's contract with the Army called for du Pont to receive its costs plus a fixed profit of one dollar and for the government to receive all patents developed in the course of the contracted work. Hewlett and Anderson, 186-187.

\textsuperscript{79}Alice K. Smith, A Peril and a Hope: The Scientists' Movement in America, 1945-1947 (Chicago: University of Chicago Press, 1965) 15-17. The petition probably would not have been favorably received had it reached the Oval Office. When a Met Lab scientist complained about policy in a personal interview, obtained through social connections, with Roosevelt, Roosevelt immediately turned the matter over to Conant. Hewlett and Anderson, 203.

\textsuperscript{80}Groves, Now It Can, 44-45; Conant, Several Lives, 295-296.


\textsuperscript{82}Oppenheimer to Rabi, 26 February 1943, in ibid., 250.

\textsuperscript{83}Smith and Weiner, Oppenheimer, 247.
deemed necessary. Groves was willing, within limits, to accommodate scientists’ self-organization of their research. He agreed to divide the work of Los Alamos into two time periods. During the first, scientists would remain civilians because the work would consist of "experimental studies in science, engineering and ordnance." But during the second, when the work would consist of "large-scale experiments involving difficult ordnance procedures," scientists, to continue on the project, would have to accept commissions as officers. This concession was not sufficient in principle to induce Rabi to join the Los Alamos staff, but he became a consultant and did not discourage others in Cambridge from joining for the first period. In practice, Groves never invoked the "second period," so the scientists at Los Alamos never had the choice between finishing bomb work and their institutional status.

While the Army’s management of research-engineering relations created pockets of discontent within the Manhattan Project, its management of security produced near universal dissent. Again, the scientists had laid the foundation for their problems by centrally dividing the research labor for the bomb. These divisions formed a ready-made basis on which Groves could "compartmentalize" knowledge in order to limit the number of people who posed a security risk by virtue of their breadth of knowledge. From having scientists work on specialized topics under the direction of their prestigious research directors, it was a small step for Groves, though not the scientists, to have the scientists report all findings to and receive all information from these directors. Besides helping security, Groves believed compartmentalization helped intellectual efficiency by preventing individual researchers from tittering away their efforts on too many issues. Scientists complained that compartmentalization prevented low-level, inter-laboratory

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85 Ibid., 16-17.

86 Groves, Now It Can, 140.
communication that could speed the project through useful brainstorming on common problems. Yet, as Martin Kamen learned, a scientist who openly speculated about work taking place at other sites and maintained friendships with Russians could be summarily fired.\textsuperscript{87}

Whether or not compartmentalization slowed or speeded the project, it certainly strained relations between research directors, especially in Chicago, where Compton and his staff had already clashed over the Met Lab’s relationship to du Pont. Sensing the declining interest in their work among Washington policy-makers focused on producing bombs, Chicago researchers were susceptible to rumors about the laboratory’s demise and prone to speculate on how the use of the bomb would affect international relations and the future of nuclear research. Because of the combination of secrecy and the centralized division of labor of the bomb project, the Chicago researchers could not act on their concerns. Without information on the programs of other laboratories, they could not shape their laboratory’s agenda to improve its usefulness. Without a two-way dialogue with project authorities, they could not force consideration of their collective assessment of the dangers and benefits of the bomb’s uses. At war’s end, when they realized none of their recommendations had become policy, they instigated a broadly based scientists’ lobby to articulate scientists’ interests in national affairs.\textsuperscript{88}

Even at Los Alamos, whose isolation minimized the possibility of information leaks, secrecy policy provoked anxiety and friction. Edward Condon, one of the United States’ first quantum theorists, joined Los Alamos as Oppenheimer’s associate director but left within weeks of his arrival because he felt the restrictions on inter-laboratory communication rendered the project hopeless.\textsuperscript{89} His departure did not inspire others, but criticism of restrictions on inter-laboratory communication was

\textsuperscript{87}Kamen, \textit{Radiant Science}, 150-168.

\textsuperscript{88}Smith, \textit{Peril}, 17-72.

\textsuperscript{89}Condon to Oppenheimer, 26 April 1943, Oppenheimer Papers, Library of Congress, Box 27, Condon file.
endemic among the scientists.\textsuperscript{90} Hans Bethe's suggestion to hold occasional laboratory-wide colloquia raised the hackles of Groves because of the suggestion's repudiation of compartmentalization. But Groves relented with only minor caveats,\textsuperscript{91} probably because he realized the colloquia would be less significant as a technique for exchanging information than as a ritual for reminding the staff of their common purpose and responsibilities.\textsuperscript{92} Secrecy made procurement an enduring problem for the scientists. The University of California purchasing agents, who handled Los Alamos' requests from Los Angeles, were kept ignorant of Los Alamos' mission and thus could never intelligently rank requests or decipher ambiguous directions.\textsuperscript{93} Towards the end of the project, the scientists at Los Alamos, no less than their colleagues in Chicago, were prone to discuss politics and organize for a post-project expression of a collective consensus on policy issues.\textsuperscript{91}

**The Radiation Laboratory, the Manhattan Project, and Postwar Planning**

Between industrial firms desiring production rights and universities desiring the return of faculty members officially on extended leaves-of-absence to MIT, the Radiation Laboratory could not survive the war in its wartime form. However, MIT's leaders, to whom the Radiation Laboratory could be seen as an outgrowth from and justification for a peace-time research policy stressing locally administered cooperative research centers, determined to use the Laboratory to build on those traditions. The problem for MIT was twofold: first, to draw back to the Radiation Laboratory the individuals who had made MIT a desirable locale for the Laboratory but who had moved into other responsibilities during the war; and second to retain

\textsuperscript{90}Hewlett and Anderson, *New World*, 239.

\textsuperscript{91}Ibid., 238.

\textsuperscript{92}Hawkins, *Manhattan District History*, 32-33.

\textsuperscript{93}Ibid., 51-57.

\textsuperscript{94}Smith, *Peril*, 60-63.
enough of the Laboratory's staff to maintain continuity in research. Failure to solve these two problems would likely result in renewed potential for the fragmentation of the physicists into academic and industrial communities—the former MIT faculty working in industry and government would organize postwar microwave research while the former Radiation Laboratory staff members, returning to their original campuses, would return to their disciplinary research. On the other hand, if MIT could secure a strong group to continue the Laboratory's work, MIT could compete for students and funds with an ongoing program with a history of success.

The impetus to plan a postwar future for the Radiation Laboratory came when Bell Laboratories offered Julius Stratton a full-time position with the company in the summer of 1944. Once Stratton told Slater of the offer, Slater wrote Compton that Stratton could be induced to stay at MIT at personal financial sacrifice "if he could be fitted into a place at the Institute which would really appeal to him."95 That place, Slater figured, would be Lee DuBridge's job in a postwar version of the Radiation Laboratory. Slater suggested establishing a Research Laboratory of Electronics, to be started as the Radiation Laboratory closed, with Stratton made director. The new laboratory, in Slater's conception, would pursue "fundamental research both in the physics of electronics, microwaves and related fields, in which I want to start work myself ... and in the electrical engineering aspects of the field as applied to communications and other obvious applications."96 The laboratory's staff would be members of either the Physics or Electrical Engineering Departments for administrative and teaching purposes, but "would simultaneously be in Stratton's group for purposes of planning research and coordinating their work."97

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95Slater to Compton, 23 August 1944, Slater Papers, American Philosophical Society, microfilm copy at MIT Archives.

96Ibid. Slater followed up this letter with a more formal and less informative proposal, "Memorandum Regarding the Establishment of an Electronics Laboratory in the Departments of Physics and Electrical Engineering, MIT," 28 August 1944, in ibid.

97Slater to Compton, 23 August 1944, ibid. Slater also hoped that Stratton would take responsibility for coordinating the teaching of Physics and Electrical Engineering courses.
Slater was confident that a new, Stratton-led laboratory would renew the enthusiasm of MIT's old staff for the Institute. In addition to his own desire to continue microwave work, Slater believed that the physicists he and Compton had recruited in the early 1930s for MIT (Wayne Nottingham, Philip Morse, and Arthur von Hippel) would gladly fit into the organization. Although Slater could not claim any authority for the Electrical Engineering Department, he speculated that even if Edward Bowles decided not to return to teaching, Wilmer Barrow, the electrical engineer in charge of MIT's radar school, would work well with Stratton and be an effective head of the Electrical Engineering Department's Communications Program. "Taking everything together and coordinating them, it would make a very impressive program" and "would give [Stratton] a situation as nearly as possible like the one which he would have here at the Bell Laboratories."98 MIT's top administrators promptly met and approved Slater's scheme, and at its September 1944 meeting, the Executive Committee of the MIT Corporation earmarked seed money for the venture.99

As always, it proved difficult to convince outsiders that an intra-MIT innovation amounted to "a very impressive program." However, perseverance and administrative flexibility did bring results. After several recruitment failures, Albert Hill and Jerrold Zacharias, two senior scientists at the Radiation Laboratory, agreed to join MIT's Physics Department, though only Hill, himself an MIT alumnus, continued to do research in microwaves.100 However, the money advanced by the Executive Committee proved invaluable in retaining younger staff members, who typically had either interrupted or deferred doctoral programs to join the Radiation

98Ibid.

99Killian, Education, 46. Stratton did stay at MIT to direct the laboratory and later became MIT's president. Jerome Wiesner, rather than Wilmer Barrow, became the Electrical Engineering Department's chief representative in the laboratory, and Wiesner succeeded Stratton as both laboratory director and president. The practice of taking a researcher turned research administrator as president of MIT did not end with the selection of Karl Compton.

100Slater, Scientific Biography, 219-220.
Laboratory. MIT used the seed money to establish "Research Associate" positions that paid Radiation Laboratory veterans 75% of their prior salary, required them to continue research in the Research Laboratory of Electronics, and allowed them to take the necessary courses to complete their degrees. These terms attracted researchers torn by the desire for further education and for work at the salary to which they had grown accustomed and thereby insured that the laboratory would open with experienced personnel.

From an MIT-centered perspective, the establishment of the Research Laboratory of Electronics was the culmination of Compton's career. Back in 1927, he had argued that university administrators should find ways to emulate the organization of industrial laboratories in order to prevent overspecialization of academic research within disciplinary boundaries and to place their schools in a strategic position for fulfilling social obligations. Here in 1944, Compton's chairman of the Physics Department was requesting the merger of research programs in the Physics and Electrical Engineering Departments in order to create the closest thing possible to an industrial research directorship in an academic setting. What Compton could not have envisioned in 1927, however, was that the social obligation such institutional and intellectual innovation would discharge would be so closely linked to military strategy and thus to government operations. Gaining patronage in the form he preferred following the war would require that political means be devised.

While postwar planning for a successor to the Radiation Laboratory hinged on MIT taking measures to retain researchers attracted either to continuing microwave research under non-academic auspices or to using academic auspices to study more esoteric particles than electrons, a different constellation of pressures shaped considerations for the future of nuclear research. In the first place, industrial opportunities did not appear imminent for Manhattan Project veterans. Even at the Metallurgical Laboratory, whose staff suspected du Pont's motives, mature reflection

\[101\text{Ibid, 220-222. The Research Associates were also responsible for helping to teach the introductory courses likely to be swollen by returning servicemen.}\]
led to the realization that the nuclear research had been almost entirely academic and that

the first problem of industrial participation in nucleonics development will be not how to exclude undesirable industrial concerns, but how to induce any concern at all to step into a field which, at least at present, offers little prospect of early profit. 102

In the second place, since the research labor of the Manhattan Project had been divided and much ... been done at the production centers created at Hanford, Oak Ridge, and Los Alamos, no university could claim to have championed nuclear research as MIT could for microwave research. In short, while MIT sought a laboratory that would attract government funds by keeping universities in research in regions of industrial and military interest, the Manhattan Project researchers needed a boundary between governmental and academic research in order to disperse and compete as they previously had.

During the war, the Manhattan Project's academic research managers acted as though nuclear research would revert to a competitive enterprise whose participants would be scattered among several universities. The concentration of physicists in a few laboratories in order to achieve quick and confidential resolutions of scientific uncertainties provided unparalleled opportunities for younger scientists to display their skills to their seniors and for senior scientists to display their leadership qualities to both their juniors and their administrative superiors. While wartime concerns were obviously a first priority, competition among universities for the postwar services of project personnel was nonetheless a feature of the Manhattan Project.

The University of Chicago, by virtue of hosting a laboratory that attracted scientists from other universities, was superficially in a position like MIT. But where MIT struggled to hold key faculty recruited by Bell Laboratories, Chicago had no such competition from du Pont. And where MIT struggled to convince some of its visitors to make a permanent switch into microwave research, Chicago could sell

itself as the best place for its visitors to continue nuclear research. The university treated the Metallurgical Laboratory as part of its structure and encouraged the staff, insofar as security regulations permitted, to participate in university life.\textsuperscript{103} This policy not only softened the psychological impact of the security regulations, it also made it easy for visiting staff to consider Chicago as a potential home. In the summer of 1944, the university offered Enrico Fermi, then on leave from Columbia, a professorship and assured him that he could combine university duties with directing a postwar national laboratory at Argonne. Columbia complained to Bush, who warned A. Compton not to make assumptions about Argonne's future in his efforts to enhance Chicago's appeal.\textsuperscript{104} Nevertheless, the offer to Fermi stood and was accepted.

Los Alamos was not a site where a university dean like Compton could hope to entice a Nobel laureate like Fermi into making a permanent move.\textsuperscript{105} However, the youthfulness of the Los Alamos staff made it fertile territory for tenured scientists to recruit junior staff members to strengthen university departments following the war. In November 1943, amidst considerations of how best to integrate British scientists into the Manhattan Project, Oppenheimer took time, in a letter to Raymond Birge, chairman of Berkeley's physics department, "to think constructively about the peace that is to follow ... [and] the welfare of our department."\textsuperscript{106} Specifically, Oppenheimer urged Birge immediately to offer Richard Feynman a position at Berkeley. Birge declined to act, citing an unwillingness to make decisions with much of his tenured staff on leave and an insecurity over the future largesse of the university's administration and the California legislature. When Feynman

\textsuperscript{103}Hewlett and Anderson, \textit{New World}, 199-200.

\textsuperscript{104}Ibid., 323-324.

\textsuperscript{105}Not only was Oppenheimer not a dean, but his involvement in the procurement difficulties between Los Alamos and the University of California left him wondering whether he was welcome back at Berkeley. Oppenheimer to E. O. Lawrence, 30 August 1945, and Oppenheimer to Robert Sproul, 29 September 1945, in Smith and Weiner, \textit{Oppenheimer}, 301-302 and 306, respectively.

\textsuperscript{106}Oppenheimer to Birge, 4 November 1943, in ibid., 268-269.
subsequently accepted an offer from Cornell, which was well represented at Los Alamos by Hans Bethe, Oppenheimer chided Birge by citing several instances in which universities had lined up young scientists for postwar service,\textsuperscript{107} and continued to offer to use his "rather good position to find out about personnel."\textsuperscript{108}

When Manhattan Project scientists, at war's end, dispersed to various universities with hopes of continuing nuclear research, they needed an understanding (preferably favorable) of the relationship of academic structure to military interests. The most acute and widely publicized problem was the incompatibility of the military's restrictions on inter-laboratory communication with the prosecution of nuclear research from scattered academic sites. To scientists distressed at how the Army had taken command of their research organization, it became a cardinal principle to restore academic norms. James Franck could not conceive of further progress in nuclear science if competitive research and open teaching were not restored.\textsuperscript{109} Enrico Fermi succinctly made the case for the necessity of lifting military regulations in order to pursue competitive academic research:

Secrecy on the scientific phases of the development [of the atomic bomb] not only would be of little effect [in slowing proliferation] but soon would hamper the progress of nuclear physics in this country to such an extent as to even make it exceedingly difficult to grasp the importance of new discoveries made elsewhere in the field.\textsuperscript{110}

To assert the political and intellectual benefits of academic hegemony over nuclear research, a determined "atomic scientists' lobby" formed to press the Truman administration to make open, competitive research a foundation for an international regime to control atomic weaponry and to press Congress to place any national agency for the support and exploitation of nuclear energy under civilian control.\textsuperscript{111}

\begin{itemize}
  \item \textsuperscript{107}Oppenheimer to Birge, 26 May 1944, in \textit{ibid.}, 275-276.
  \item \textsuperscript{108}Oppenheimer to Birge, 27 September 1944 and 5 October 1944, in \textit{ibid.}, 283-285.
  \item \textsuperscript{109}Smith, \textit{Peril and Hope}, 95.
  \item \textsuperscript{110}Fermi to Hutchins, 14 September 1945, quoted in \textit{ibid.}, 96.
  \item \textsuperscript{111}Smith, \textit{Peril and Hope}, 187-189.
\end{itemize}
Less publicized than the secrecy issue, but equally essential for the revival of the academic careers of project scientists was the design of means by which nuclear scientists could claim fresh "turf" and gain support for its investigation. This subject received less attention from the project members working on an international agency to control developments in nuclear energy. However, in the summer of 1945, as word filtered down to Metallurgical Laboratory staff members that their superiors had been appointed to a committee that would consider postwar plans, John Wheeler urged them to consider what academic physicists might do beyond experimenting with the interactions among nucleons and how they might define and obtain support for their ambitions.

Wheeler called for a program in "ultranucleonic research," by which he meant the science that he hoped would emerge from reducing neutrons and protons to "lower entities" as opposed to the prewar and wartime "nucleonic research," which treated protons and neutrons as indestructibles. He justified pursuit of this area—at least to what he believed would be the satisfaction of the Manhattan Project's research directors—not by arguing its relevance to any governmental or industrial interest, but by speculating that ultranucleonic research might reveal a better way to convert mass to energy than nuclear fission. Such a discovery, Wheeler suggested, "might completely alter our economy and the basis of our military security." He looked forward to a postwar era in which the government would support academic science as too potentially lucrative to be allowed to languish because of its expense or speculative character.

\[112\] Even the "Prospectus on Nucleonics," which solicited the speculations of Metallurgical Laboratory scientists on what peacetime directions nuclear research could take, failed to address administrative issues beyond wishing for a return of nuclear research to academic settings and the creation of a private "nucleonics industry." Jeffries, et. al., "Nucleonics," in Smith, ibid., 555-559.

\[113\] Wheeler's work with Niels Bohr in 1939 had laid the foundation for a theoretical understanding of fission.

To work out a program of research for the government to fund, Wheeler envisioned that some unnamed government agency would appoint a committee of physicists "of national standing" to survey the ultranucleons field. The committee would discharge that responsibility by commissioning reports from the "good men" in the field and then organizing a general conference for the discussion of these reports. The reports themselves would give either "bird's eye views of selected fields" or an "analysis of present and proposed techniques of investigation." The ensuing discussions of what was unknown and what could be researched, Wheeler reasoned, would yield a consensus on what ought to be done. The committee would then be able to draft a formal statement that the conference as a whole would ratify. The conference proceedings would be published; the committee would submit statements on budgetary needs, possible military applications, and the importance to the country of fundamental research in physics to the government agency.¹¹⁵

One purpose of Wheeler's proposal was to show "that scientists in free association can show more vision and judgement on research planning than any centralized governmental authority." Not a scientist in the country would have disagreed. Yet in both rhetoric and administrative framework, Slater's proposal for a Research Laboratory in Electronics contrasts sharply with Wheeler's proposal for research in ultranucleons. Where Slater believed that his scientific ambitions were best pursued and would be highly valued in a laboratory partly dedicated to creating technological innovations, Wheeler stressed that research pursued in independence of technological concerns (except for the design of scientific instruments) could create theoretical novelty that might inspire technological thinking. For Slater, the appropriate institutional framework for planning his research was his university. The Research Laboratory in Electronics rested on MIT's activist administrators, who organized inter-departmental laboratories to exploit research in areas of interest to multiple departments, and the willingness of MIT faculty members from different departments to make their different concerns a source of mutual stimulation. For

¹¹⁵ibid.
Wheeler, however, the appropriate institution for planning research was the discipline—i.e., the "good men in the field." His proposal for ultranucleonic research rested on the willingness of similarly trained specialists from multiple institutions to define a research program that identified their shared interest, and on the physicists "of national standing," who would have the wisdom, experience, and contacts to convey the program's credibility to the appropriate government officials. For Slater, senior administrative authorities within a university should guide the research interests of faculty members, while for Wheeler, seniority and prestige in a discipline brought only the privilege of being a mouthpiece, and budget calculator for the discipline, and university administrators play no role.

One point that Slater and Wheeler had in common was that neither considered how the government agency they wished to petition would be organized, where it would fit into the structure of American politics, and what influence these factors would have on the agency's general policies and its likelihood of favoring one type of petition over another. However, Slater's proposal did have an advantage over Wheeler's in that it spurred Karl Compton promptly to think about the features that MIT would consider desirable in government funding of academic research. For the October 1944 meeting of the Executive Committee of the MIT Corporation, which was the first meeting following the one that approved the Research Laboratory of Electronics, Compton outlined his thoughts for an "MIT Policy re Government Contracts."

Moving beyond the homilies that MIT should reassert the primacy of its educational mission, Compton argued that should any government agency wish to contract research work to MIT, the agency must respect MIT's character, since that was presumably what made MIT the best site for the research in the first place. To Compton MIT's character stemmed from the combination of its administrative flexibility and the ambitions of its staff to excel in teaching and research, for these factors enabled MIT to seek fruitful redefinitions of the social boundaries of fundamental research. The upshot for the government was that its contracts must not restrict university researchers' publication rights, must not infringe on their patent
rights, and must include, in addition to immediate research expenses and MIT's operating overhead, "a provision toward those indirect Institute expenditures which will be incurred to strengthen and develop scholarship and fundamental research."\textsuperscript{116} Compton did not flesh out what he meant by that last provision, but because MIT's Executive Committee had just appropriated $250,00 as seed money to start the Research Laboratory of Electronics, he presumably wanted the government to contribute towards MIT's administrative capacities to foster more such innovations in the future.

\textbf{Conclusion}

As a statement of what MIT sought in a government-university nexus, Compton's views were unassailable. But then, the proposals he sponsored to the Rockefeller Foundation were equally unassailable as a statement of what MIT sought in a foundation-university nexus. After five years in which university research was nationally administered by men who had been linked by MIT before the war, what Compton thought best for MIT had more than local significance.

NDRC or its individual members had been principals in the two enormously successful projects that made World War II known as a physicist's war. But such a generalization papers over the differences in the roles they played, the relationships they developed, and the accomplishments they fostered. By war's end, researchers equally devoted to ideals of academic freedom and excellence were proposing research agendas that assumed different frameworks for planning research. In insisting on publication privileges as central to peace-time government support of university research, Compton was certainly on firm ground with all academic scientists. But as Wheeler's prospectus makes clear, not all academic scientists were touting patent rights and university administrators as foundations for operating research frontiers.

By design, NDRC existed to designate military functions that non-governmental laboratories would seek to address by creating multi-disciplinary university laboratories that included all specialists relevant to the creation of an instrument of war. Its immediate model was NACA, but the origins of its members’ predilection for non-disciplinary research laboratories goes back to Dugald Jackson’s successful emulation of William Sedgwick’s style for creating new research specialties and to the intra-MIT problems that Jackson’s proteges had in achieving their full potential. The radar program, which took MIT’s coordinated peace-time research into the physics and engineering of directed beams of microwaves as its initial nucleus, exemplifies the benefits of NDRC’s design. The radar program’s centerpiece, the Radiation Laboratory, brought together physicists and engineers with complementary skills, created an environment in which differences in the staff’s training and perspectives were more a source of learning and stimulation than friction and annoyance, provided a clearinghouse in which researchers and military officers could learn what each supplied and demanded while maintaining the self-governance researchers required to satisfy their passions for technical challenges, excellence, and exploration. NDRC’s collective and Compton’s individual judgements were critical to the radar program’s success. NDRC gave the Laboratory the power of the first draft in determining the research agenda; and when the Laboratory’s assessment of its needs sparked conflict, NDRC was an effective forum for discussing the value and applicability of Compton’s principles in supporting the Laboratory and an acceptable decision-making body to the researchers in the Laboratory and the other organizations with a stake in the radar program.

By tradition, nuclear scientists worked in competing, freestanding research institutes that they controlled. Their attempts to have NDRC support and supervise their structure met with collective discomfort, and eventually outright rejection. But individually NDRC members were open-mindedly skeptical, and when they lost faith in finding a sound reason for why fission could not be useful during the war, they actively supported what they considered a more appropriate organization’s efforts to manage the nuclear scientists. For the nuclear scientists, NDRC was a mixed
blessing. Without NDRC they would have never acquired an administrator with the personality and the authority to mobilize the massive industrial resources needed to turn the theory of an explosive chain reaction into a bomb. But with NDRC that administrator was a general who placed far more authority in the corporations that provided the resources and worried far more about the dangers of security breaches than the nuclear scientists thought reasonable or productive. Many ended up feeling as Arthur Noyes had in the face of William Walker’s ascendancy within MIT—intellectually constrained, politically exploited, and alienated from the administrative superiors they had wanted to look after their interests. But unlike Noyes, the nuclear scientists in World War II had no Throop Institute to which they could flee and regroup.

By expectation, biologists considered themselves scientists on a par with physicists. NDRC did nothing for them, and except for those who found work in a project of the Committee on Medical Research (which cries out for its historian), neither did OSRD. They could do little more than seek sympathetic ears to listen to their efforts to understand why their efforts were unwanted.

From a political perspective, NDRC’s successes and problems were founded in its adherence to delegation of authority to local officials. The radar program blossomed because interdependent physicists and engineers within a local research center found they could develop excellent military radar sets. The Manhattan Project acquired less support because NDRC resisted pressures to make centralized scientific judgements and to coordinate specialists desiring a multi-center project. Symbolized and cemented by Bush’s ongoing friendship with Harry Hopkins, and maintained by the distance OSRD kept from areas of interest to advocates of centralized government planning, NDRC’s use of local institutions to generate and carry out projects evoked longstanding political conflicts and traditions in the United States.117 Turning the indisputable fact of wartime successes into a compelling argument for a political

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consensus on a postwar research agency and policy became Bush's passion as the end of World War II turned from a wish into an imaginable military reality.
PART IV: PUBLIC DEBATE, GOVERNMENT SCIENCE POLICY
AND AMERICAN FEDERALISM
CHAPTER 10

THE ORGANIZATION OF RESEARCH AND THE RHETORIC OF DEMOCRATIC IDEOLOGY

The formation of NDRC, as Bush frankly remembered it, "was an end run, a grab by which a small company of scientists and engineers, acting outside established channels, got hold of the authority and money."¹ That process and the subsequent secrecy of its projects inhibited direct public discussions of its structure and operations. Nevertheless, intellectuals and legislators began more generic examinations of possible relationships between science and the state in response to two conditions. First, before the war, the emigration of scientists from the fascist to democratic nations prompted speculations on why democracies seemed more supportive of scientific enterprises than other nations. And second, early in the war, the federal government's difficulties in redirecting the nation's industrial capacity to war-related uses prompted Senator Harley Kilgore (D.-W.Va.) to propose an Office of Scientific and Technological Mobilization that would have broadened the public role of non-governmental scientists.

Because of wartime secrecy and the pre-war lack of direct, widespread government funding of university and industrial research, these discussions were richer in rhetoric than specifics. However, the participants did succeed in calling attention to some important concepts, in exploring the relations among them, and in uncovering some common interests. In conferences involving a broad array of intellectuals, and in scientific journals (most notably Science) that made space for

¹Bush, Pieces, 31-32.
discussions of science and public affairs, scientists expressed preferences that differed from NDRC practices, echoed the tensions within the secret war program, and alerted NDRC leadership that the personal and institutional histories that underlay their policies were not universally shared.

Late in 1943, NDRC scientists made their first serious effort to explain themselves to outside scientists concerned with NDRC’s policies and their implications for the future. Around the same time, Kilgore dropped efforts to create a useful agency for the war and began considering the basis for a long-term, peacetime agency. By war’s end, Bush and Kilgore were independently preparing reports that each hoped would be the basis for a permanent agency that supported scientific research in private institutions. To claim the rhetorical high ground for their contrasting perspectives, both resorted, in the reports, to generalities so as not to alienate those potentially suspicious of the policy instincts of the authors. However, the authors’ instincts are discernible in their proposed agencies when the political rhetoric is invested with historical content.

Science and the Popular Front

For several reasons, anti-fascism found no mass constituency in the United States in the 1930s. Against this background, the concern among American academics for the difficulties their overseas colleagues were encountering made them seem pointedly anti-Fascist. With the Depression slowing American academic careers, even the American Communist Party attracted radicalized academics.

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2Among the reasons were that student activism was pacifist and isolationist, no domestic group with fascist sympathies was sufficiently prominent to arouse the anxiety of organized labor, and Marxist groups were as inclined to battle each other as to recruit new members. See Larry Ceplair, *Under the Shadow of War: Fascism, Anti-Fascism, and Marxists, 1918-1939* (New York: Columbia University Press, 1987), pp. 181-201.


With the signing of the Nazi-Soviet non-aggression pact, Communists ceased to support anti-fascist activities. But even so, at the outbreak of the European war, several New-York-based intellectuals, led by literary critic Van Wyck Brooks and theologian Louis Finkelstein, successfully organized the Conferences on Science, Philosophy, and Religion in their Relation to the Democratic Way of Life. Here anti-fascists from many fields could discuss the proposition, offered by Brooks, "that our failure to integrate science, philosophy, and religion, in relation to traditional ethical values and the democratic way of life, has been catastrophic for civilization." By encouraging conferees to represent their disciplines and to reflect on the conditions that made their studies possible, Finkelstein hoped to produce arguments for "why certain ethical, philosophical, and religious values transcend tentative scientific and historical constructions." The conferees would then be better able to instill a love of democracy among their peers.

In this setting, the natural scientists were committed to explaining to the humanist majority what aspect(s) of science contributed to a healthy political culture and a just public administration. The scientists' audience ranged from suspicious to downright hostile. The putative overdevelopment of the natural sciences in comparison to the social was a commonly asserted cause for the Depression and its social effects. At this conference, two highly-placed scholars even suggested natural science had not just outstripped but perverted other fields to the point that their...
practitioners could no longer instill the cultural prerequisites to democratic practice in their students.9

With one exception, the natural scientists at the conference accepted that intellectuals bore responsibility for creating a culture in which democracy would thrive, and argued that the sciences, properly understood, contributed to a capacity for moral reasoning and provided a healthy example of social solidarity. The basis for these claims was an image of science as a mode of thought that imbued in its devotees the proper techniques of reasoning and a unity of purpose based on shared suppositions. With such an image, the participating scientists could assert their importance for shaping character on the humanists' terms. However, this "intellectualist" image left them without any sense of science as a complex of skills whose practitioners benefitted from the kind of administrative engineering that Compton worked at MIT and NDRC was performing nationally.

Philipp Frank, the emigre Austrian physicist and philosopher of physics, most bluntly made the case for the cultural benevolence of a science education that stressed the philosophical disposition needed to pursue scientific research. He claimed that the Austrian students

least accessible to such [totalitarian] propaganda are the students of the exact sciences, mathematics, physics, astronomy, etc. The same goes for the professors.... [Yet] the percentage of convinced nationalists and chauvinists...is particularly large among the students of those fields having to do with the practical application of these sciences....I have encountered the most uncritical of all the adherents of totalitarianism among the students of the engineering sciences, even more than among the students of history and modern philosophy.10

From these observations, Frank concluded that scientists' democratic habits should not

9Pitrim Sorokin, a Harvard sociologist, and Mortimer Adler, professor of philosophy at the University of Chicago, both blamed the United States' cultural problems on its professorate's adherence to empiricist philosophies, which supported the development of natural science, and the professorate's denigration of moral reasoning. Adler went so far as to suggest that totalitarianism could be a blessing for the American professorate—serving as the scourge that forced a people to retrieve its true sources of virtue as the Babylonian and Assyrian tyrants had done for the ancient Israelites. See Brooks and Finkelstein, Science, Philosophy, and Religion (1941), 90-119 for Sorokin and 120-138 for Adler.

10Ibid., 218.
be attributed to scientific information, "for the engineer and physicist are acquainted with exactly the same facts," but rather to "the manner in which they approach their subjects." Frank postulated that stressing the "scientific spirit, that is, the spiritual attitude inculcated in a person who has to occupy himself with science ... can contribute towards an ethical education and especially to an education for democracy." Just as Arthur Noyes had, when arguing that MIT needed to put departmental science laboratories alone at the apex of its institutional values, or the Manhattan Project scientists had, when resisting Groves' vesting of managerial authority for the production of fissile material in corporate engineers, Frank was insisting that scientists, understood as those who contributed to a progressive deepening of a conceptual system for coping with nature, deserved to be leaders of the institutions to which they contributed.

Frank believed that the scientific spirit taught scientists to treat theories pragmatically—that is, to adopt as their working code the philosophical position that theories, regardless of the private aesthetic virtues that scientists find in them, have no collective meaning beyond the facts for which they account. This habit of placing priority on the consequences over the beauty of a theory was what protected scientists from totalitarian ideologies, which in Frank's view demanded total adherence to general principles. Even nationalistic German scientists, he maintained, rejected Nazism on the grounds of its logically inconsistent premises and morally offensive consequences.

Whatever their views on pragmatism as a philosophy, the other participating natural scientists affirmed Frank's generic claim that scientists' practice was defined by their treatment of theories and that improvements in political culture would follow if scientists and non-scientists (among whom Frank included engineers) could carry that treatment over to other spheres of life and study. Albert Einstein suggested that

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11Ibid., 219, my emphasis.

12See Section 2.2, above.

religious leaders would more frequently inspire decency in human behavior if they stopped using the notion of a personal God who rewards and punishes and instead "avail[ed] themselves of those forces [i.e. scientific theorizing] which are capable of cultivating the Good, the True, and the Beautiful in humanity itself."\textsuperscript{14} Caryl P. Haskins, who split time between his independent laboratory and a research professorship in biophysics at Union College, extolled the scientific method,

\begin{quote}
  a method of thinking which [not only] is definite and essentially alike among large groups of highly intellectual individuals [but also] proposes a rigidity of discipline and offers a set of tangible standards of achievement which greatly reduce the natural variability in methods of work of good minds.\textsuperscript{15}
\end{quote}

Herein Haskins perceived an object lesson for democratic political life. If humanists studied more science more thoroughly, the realization would become widespread "that, while there may be an infinitude of subject matters among religions, philosophies, and sciences, there is a unifying spiritual and intellectual approach which underlie them all."\textsuperscript{16} Just as scientists used adherence to scientific method to align their individual satisfaction with community goals, so a widespread commitment to a unity of approach among all forms of thought could be the basis on which democracies could achieve national solidarity without resort to exclusionary concepts of national identity.

The lone contrasting perspective among the natural scientists came from the lone representative of an industrial research organization, Karl Darrow of Bell Telephone Laboratories. In his paper, "The Interplay of Theory and Experiment in Modern Physics," he discussed physics not as the product of individuals with the proper disposition, but as the product of properly organized communities of experimentalists and theorists, who sometimes formed inter-community alliances but

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\textsuperscript{14}\textit{Ibid.}, 209-214, quote on 213.
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\textsuperscript{15}\textit{Ibid.}, 232.
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\textsuperscript{16}\textit{Ibid.}, 234.
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often worked independently.17 Critical to further advances in Darrow’s view were mutual respect among scientists for their varied skills and their various motives for pursuing research, and the will of scientists’ fellow citizens to provide the material resources and institutional flexibility needed to find and take advantage of research opportunities. In its social content, Darrow’s talk could have been assembled by rewriting William Sedgwick’s arguments for granting space in academia for "public health science" and continued public support for research into the natural processes exploited in sanitary engineering and the epidemiology of contagious diseases. But where Sedgwick viewed his students as the best civil servants for a democratic government Darrow, when he asked in the spirit of the conference "What has all this to do with the democratic way of life?", responded: "Here I am afraid I must disappoint the reader by giving him no clear lead."18 While totalitarian states had intruded most frequently and destructively into the business of scientific institutions, Darrow saw no reason in principle why such dangers were limited to totalitarian nations or why democratic nations would always supply adequate material support for research.19

When the second conference convened in September 1941, the backdrop of Nazi battlefield successes heightened the political implications of Haskins’ and Darrow’s conceptions of science. Haskins, who viewed science as the disciplined application of scientific method, attributed Nazi military successes to "the scientific thoroughness of preparation with which this war was undertaken."20 Having previously upheld scientists’ adherence to method as a model for citizenship, Haskins could not attribute German war preparedness to the superior morality of German

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17Ibid., 193-207. Darrow cited the discoveries of x-rays, radioactivity, and the electron as experimental inspirations to theorists and relativity and quantum theory as theoretical inspirations to experimentalists.

18Ibid., 207.

19Ibid., 207-208.

scientists. Instead he argued that scientists in totalitarian states had become "tools for another group of people whose wishes they have served" via a Faustian bargain in which scientists turned their discoveries to military uses in exchange for material support for research. Scientists in democracies, Haskins concluded, needed to become politically engaged. Like philosophers and clerics who became political managers of the enthusiasms their ideas unleashed, Haskins wanted scientists to manage the technological enthusiasms that their research unleashed.

In his comments on Haskins' paper, Karl Darrow posed two sociological questions that deflated Haskins' dream of a scientific community unified by adherence to scientific method and disposed to manage the technological fruits of science according to democratic principles. How, Darrow asked, were scientists to build research careers and pursue political work when the former was already a full-time occupation; and what would be the effects of scientists' activity in the political arena on their deliberations over scientific issues? Darrow asserted that political work required far too much time for a young scientist to pursue. While he conceded there would be benefits "if a greater proportion of successful scientists of middle age were to emulate K. T. Compton, R. A. Millikan and Sir William Bragg in taking part in public affairs," he doubted the benefits would be intrinsic to their scientific training:

I begin to wonder whether we should regard the scientist as a scientist when he enters into the field of public affairs. Should we not rather regard him as an intelligent man, who most laudably sets aside a part or all of his time from his prime or his former interests, in order to devote himself to something entirely different?

Any attempt to sidestep the problems of individual time commitment by creating and staffing a political organization of scientists was anathema to Darrow. He argued that organizations like the British and American Associations of Scientific Workers posed dangers to progress in science:

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21Ibid., 4.

22Ibid., 15 and 16 respectively.
I have the impression that these [associations of scientific workers] are mainly of a strong leftist tinge. Now you may reply that I ought to go out and form an organization of a strong rightist tinge. Assume for the sake of the argument that somebody does so: we then have a fight, instead of a unification of scientists seeking ends on which all are in agreement. If organizations of scientists for other than strictly scientific purposes are formed, they will get all tangled up in the battle between socialism and capitalism, leftism and rightism, new-deal-ism and old-deal-ism or whatever you choose to call it. If you reply that the clear vision of the scientist will abolish this battle, I must answer that in my view you are much too optimistic.23

Rather than solve problems of statecraft, a political organization of scientists, in Darrow's view, would divide scientific communities that needed organizational flexibility to get the most from the many talents and motivations that inspired scientific research.

No suggestions for public policy with respect to research emerged from the Conferences on Science, Religion, and Philosophy. Yet even in the absence of concrete proposals, tensions were apparent over the aspects of science that ought to define its practitioners' relationship to political power. University scientists like Frank and Haskins, concerned with the standing of their fields vis a vis the humanities and social sciences, craved public recognition for the sciences' cognitive elements, because those elements made training in science seem part of a liberal education. In contrast, Darrow, an industrial scientist, feared that public responsibilities for scientists would inject political partisanship into the already delicate art of exploiting common interests among individuals of varied talents and motives.

Since June 1940, NDRC had been rewarding flexibility in scientific institutions without becoming embroiled in partisanship. The attack on Pearl Harbor eliminated discretion as a tactic for avoiding political attention. With entry into the war came demand for involvement from unmobilized scientists and widespread interest in war mobilization in all forms. Finding NDRC unwilling to break down military needs

23Ibid., 15.
into isolated technical problems that dispersed scientists could work on competitively, unmobilized scientists found Congressional concerns on which to hitch their ambitions. What began as a partnership of convenience evolved into a reasoned linkage between Haskins' image of science as method and political concerns over the adequacy of government powers to administer war programs.

**Congressional Activism and Disgruntled Scientists**

In the spring of 1941, Congressional interest in defense preparedness was high enough that the Senate formed a Special Committee to Investigate the National Defense Program with Sen. Harry Truman (D.-Mo.) as chairman. But the committee's work initially seemed so dry that Truman induced only first-term senators to serve. But after Pearl Harbor the Committee's charge went from peripheral to central, and the quantity of targets to investigate grew with the influx of business executives (so-called "dollar-a-year men") into government service. In the spring of 1942, after the Japanese Army had cut off the U. S.'s major source of natural rubber and government officials had become hyper-sensitive to imputations of blame for rubber shortages, the Truman Committee chose to investigate what had been done before the war about synthetic rubber production.

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The non-development during the 1930s of a domestic capability to produce buna-s, the best synthetic rubber for tire production, was caused, the Committee concluded, by corporate patent agreements that did not serve the national interest. In 1929, Standard Oil of New Jersey and I. G. Farben, the German chemical firm, signed a deal in which Farben retained supremacy in the chemicals field all over the world, including the United States and turned over to Standard its patent rights in the oil field for use anywhere in the world except in Germany. Patents that did not neatly fit the distinction between the oil and chemical fields (like Farben's patents for buna-s) were assigned to a jointly-owned company, Jasco, but with the stipulation that the patent's creator controlled its development. Under these conditions, the Nazis pressured Farben into preventing Jasco from offering acceptable licensing terms to U.S. firms interested in buna-s. Standard could not retaliate because it did not control Farben's use of oil patents in Germany. But Standard did use the potential of Jasco licenses to discourage other U.S. companies from investing in the development of alternatives to buna-s. Only when the U.S. government threatened to impound foreign patents did Farben allow Jasco to license the buna-s patents to Standard.

Even after Standard had control of buna-s patents, the Truman Committee found that the separation of government financing for factory construction from government research into war production caused misunderstandings that slowed buna-s production. Most glaring was the failure of anybody in the public or private sector to investigate the availability of raw materials for buna-s production. After Pearl

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27In May 1941, the government planned to produce 40,000 tons of synthetic rubber even though imports of natural rubber in 1939 totaled 600,000 tons. Robert Solo, Across the High Technology Threshold: The Case of Synthetic Rubber (Norwood, Penn.: Norwood Editions, 1980), 23; Congress, Senate, Special Committee Investigating the National Defense Program, Additional Report, Rubber, 77th Cong., 2d sess., 1942, report no. 480, part 7, 4.

28Congress, Senate, Special Committee Investigating the National Defense Program, Rubber, 1942, 29, emphasis in original.
Harbor, executives united on the need to produce large quantities of buna-s only to learn that butadiene, an essential raw material, was not being mass-produced.²⁹

What was lacking in the short run, according to the Truman Committee, was an "integrated program," and the committee was certain of the administrative solution:

From the start our rubber program suffered because it was administered by too many people .... The committee believes that some one person should exercise full responsibility, and accordingly, full power to take all necessary action to provide such rubber as is necessary to the war program.³⁰

The proper placement of this person, according to the Committee, would be in the War Production Board, just below the level of director. To prevent a recurrence with other critical materials, the Committee recommended a legislative reconsideration of the relationship of research to the national interest.³¹ The Committee’s charter precluded it from proposing legislation, but individual committee members were free to propose legislation under other auspices.

Publicly, NDRC was conspicuous only by its absence from the rubber controversy,³² but Bush had recommended that the White House commission a National Academy of Sciences report.³³ No request was ever made to the Academy, but Roosevelt, in August 1942, formed a Rubber Survey Committee, consisting of James Conant and Karl Compton—who served as distinguished, technically literate

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²⁹Ibid., 42-50, quote on 44. The Truman Committee’s views appear largely shaped by the testimony of William Batt, a former member of the National Defense Advisory Council; see Senate Special Committee, Hearings, Rubber, 4283-4306.

³⁰Ibid., 56-57.

³¹Ibid., 28-29.

³²NDRC appears only once in the index of the rubber hearings, and the reference is to governmental users of synthetic rubber, not governmental sources of expertise about synthetic rubber. See Senate Special Committee, Hearings, 1942, 4760.

individuals—and the industrialist Bernard Baruch, to report on the rubber program.\textsuperscript{34} The Rubber Survey Committee, with its greater prestige and intellectual firepower, reached conclusions that were virtually identical to the Truman Committee’s: an outrageous lack of systematic solicitation of scientific advice caused butadiene production to be ignored; proposals for producing butadiene from oil should be "bullied through" while alcohol should be stockpiled as an alternative raw material for producing butadiene; and all rubber programs should be consolidated under one person accountable to the director of the War Production Board.\textsuperscript{35}

Synthetic rubber production ceased to be a bone of contention following the publication of the Rubber Survey Committee’s report in September.\textsuperscript{36} However, Senator Harley Kilgore (D.- W. Va.) took to heart the Truman Committee’s recommendation to assess the relationship of research to the national interest. Kilgore proposed an Office of Scientific and Technological Mobilization be formed to link researchers and their findings to presidential authority. The OSTM would inform the president of research with implications for technologies the government needed and would create public programs for developing the needed technology by directing private institutions with appropriate expertise to address relevant topics. Thus the national government could override corporate inhibitions about developing publicly

\textsuperscript{34}Roosevelt wished to head off further Congressional ferment. Midwest senators, inspired by the possibility of producing butadiene from grain alcohol, held hearings and proposed legislation through the Senate Agriculture Committee. Congress eventually passed legislation, which Roosevelt vetoed, creating a Rubber Supply Agency beyond the control of the president and the War Production Board. See Tuttle, "Synthetic Rubber,” 49-53; Solo, \textit{High Technology}, 30-32.

\textsuperscript{35}Congress, House, "Message from the President of the United States Transmitting a Digest and Report of the Special Committee to Study the Rubber Situation and to Recommend Action," 77th Cong., 2d sess., .942, Document 836.

\textsuperscript{36}Rubber did again threaten to become problematic in 1943 when the combined demand for butadiene and aviation fuel, which were mutually-exclusive products of the same oil-refining process, exceeded processing capacity. But by then large quantities of alcohol were available for butadiene production, so rubber interests were able to concede priority to aviation interests. See Tuttle, "Synthetic Rubber," 61-63.
important technologies over which they might not be able to obtain patent rights or reasonable licenses.\textsuperscript{37}

When Kilgore convened hearings on his legislation in October 1942, NDRC's critics had a forum for airing their concerns and finding their common interests. They presented two distinguishable lines of thought, though both had their foundations in the difficulties of some scientists to enter war-related research. First, some critics opined that OSRD's restriction to problems of weaponry and medicine had caused it to neglect a variety of other governmental concerns to which a broader range of academic and industrial researchers could contribute. The thrust of these critics was that scientists would be most useful if there were an agency with a government-wide perspective on research comparable to the Budget Bureau's perspective on fiscal policy.\textsuperscript{38} Second, some critics asserted that even within its field of operation, OSRD was failing to involve all useful organizations. Though one noted the legitimacy of security concerns, he suspected that by properly breaking down research projects, OSRD could have distributed the component parts widely with minimal risk of giving away the projects' overall purposes.\textsuperscript{39} To both those who would expand and reform OSRD, a single, national agency, which took research as a whole as its domain, was a step towards improving the usefulness of scientists in the war effort.

These criticisms were based on misconceptions of how NDRC's architects had made it work. The critics assumed that OSRD was a more focused version of the Rockefeller Foundation. Where the Foundation declared the importance of a type of research and judged the proposals its declaration stimulated, OSRD was translating military needs into scientific problems and assigning the problems to favored researchers at favored institutions. It did not occur to the critics that NDRC


\textsuperscript{38}See testimony of Lyman Chalkley, and Waldemar Kaempffert, in \textit{ibid.}, especially 14-15 and 71-72, and statement of Harry Grundfest, 218-219.

\textsuperscript{39}See testimony of Robert Brown in \textit{ibid.}, especially 49-50.
functioned as a more focused version of the role that MIT had sought to thrust on the Rockefeller Foundation. Where MIT's proposals to the Foundation claimed that MIT could link the researchers and equipment needed to make knowledge of broader-than-disciplinary significance and urged the Foundation to act like a banker willing to invest in universities with credible claims of having identified a new niche in the research market, NDRC functioned as a broker matching military functions with laboratories that could recruit researchers with the variety of skills needed to produce militarily relevant insights.

NDRC's representative at these hearings was Frank Jewett, who was less interested in explaining how NDRC operated than in blunting Congressional enthusiasm for changing the environment in which NDRC operated. He did not speak to NDRC's role as an active broker and the advantages of such a framework over a reformed or enlarged OSRD. Instead Jewett stressed the administrative rationality of separating secret activities from publicized ones in order to justify both OSRD's restriction to military weapons and medicine and the desirability of leaving OSRD independent from any new agency Congress might create.40

Kilgore's most immediate problem, however, was not to learn how NDRC operated in order to engage its leaders in constructive dialogue but to learn how not to threaten scientists' sense of scientific freedom. Section 4(b) of Kilgore's first bill was particularly inflammatory:

The Office [of Technological Mobilization] is authorized and directed to appraise the current use being made of scientific and technical personnel and facilities, both public and private, and to draft all such personnel and

40Ibid., 315-319. Jewett's argument carried the implication that OSRD should be strictly a wartime phenomenon since university research cannot be secret if academic careers were to be built through publication. His view is thus consistent with and foreshadows his disapproval of any post-war government agency to support university research. By contrast, Bush and Lee DuBridge, director of the Radiation Laboratory, thought peacetime research in academia (and under normal academic rules) on issues of military relevance would be desirable for both the military and universities. See Section 11.1, below.
facilities failing to submit or to accept plans for immediate conversion of their efforts to work deemed more essential by the Office of Technological Mobilization. 41

Kilgore’s second bill toned down such language and placed conditions on the Office’s use of its powers.42 Still, it provoked a mutually uncomprehending exchange between Robert Elliott, representing the Chemical Arts Forum, and Kilgore:

Elliott: "In discussing this bill, it is apparent that its provisions could only be detrimental to the technological progress of our Nation. History has shown, and our own experience has confirmed, that regimentation is particularly harmful to technical progress. Untrammelled freedom of action and individual initiative best provide the fertility for rapid and steady technical advancement."

Kilgore, after quoting back to Elliott the last two sentences of the section quoted above: "We therefore seem to have exactly the same ideas on this subject and testimony before the committee has curiously brought out the fact that this bill may liberate many scientists from the regimentation that now exists not alone in our major monopolistic industries, often circumscribed by cartel and other agreements, but also in some of our technical societies. Fortunately for the development of technology in this country, there are many independent scientists and technicians whose efforts might well be stimulated by an such as is proposed in bill S.702."43

For Elliott, freedom of action meant that researchers must select their lines of research; the Kilgore bill threatened to inject governmental supervision into a realm properly reserved for individual judgements. For Kilgore, freedom of action meant researchers should pursue the technological fruits of their work over the vested interests of private organizations; the current system threatened to suppress technologies that would serve the public interest.

Harry Grundfest, the Columbia biologist and member of the American Association of Scientific Workers, urged his colleagues to view Kilgore’s OSTM as a

41Ibid., 2.

42The text of the bill is in "The Mobilization of Science," Science, 97 (May 7, 1943), 408.

route to influence:

[T]hrough the scientific representatives on the Board of OSTM and through
the Administrator of the Office, scientists and technologists for the first time
will be given an opportunity to take part in the decisions of national policies
and will have a freer hand than at present in the support and conduct of
scientific and technical activities.\textsuperscript{44}

But the possibility that OSTM's powers might be used on rather than by scientists
inspired only the optimistic analogy that research supported by OSTM would be no
more "dictatorial" than research supported by the Public Health Service. The
Princeton astronomer John Stewart compared the proposed powers of the OSTM to
the Public Health Service and felt comfortable dismissing Grundfest as "naive."\textsuperscript{45}

The American Association for the Advancement of Science agreed with
Stewart. Kilgore, AAAS critics noted, had defined a scientist as anyone who had
completed a college course of study in science or who had worked six months in a
scientific or technical vocation.\textsuperscript{46} Such a definition, when applied to the criteria for
staffing the OSTM, led AAAS to conclude: "Instead of insuring that the Board [of
OSTM] shall have these essential qualities [of integrity, ability, and experience in
science] the act provides for a primarily political Board."\textsuperscript{47} With the issue defined
as whether there should be partisan or professional control of a science agency,
Kilgore could make little headway with the scientific community.

Nevertheless, Kilgore's demand for an executive-branch capacity to develop
technologies for public use provided a government function around which unmobilized
scientists could start to rally. He needed to invite scientists into the exercise of power
without threatening or offending their pride in the sophistication that research
required. The unmobilized scientists had to clarify why scientists who viewed their

\textsuperscript{44}K.A.C. Elliott and Harry Grundfest, "The Science Mobilization Bill," \textit{Science}, 97 (April 23, 1943), 376 for both quotes.

\textsuperscript{45}John Q. Stewart, "The 'Science Mobilization Bill,'" \textit{Science}, 97 (May 28, 1943), 487.

\textsuperscript{46}"The Mobilization of Science," 408.

\textsuperscript{47}American Association for the Advancement of Science, "Resolution of the Council on the Science
Mobilization Bill (S. 702)," \textit{Science}, 98 (1943), 136.
research as a way to deepen their science's cognitive foundations should support creation of an agency concerned with administration of the executive branch. In contrast to NDRC's Sedgwick-style matching of technological functions with academic laboratories, the unmobilized scientists needed a variation on Arthur Noyes' combination of research to solve cognitive problems in university laboratories with separate and proprietary applications of science for industrial clients.

The Development of a Scientists' Rationale for the Kilgore Bill

Perhaps the best sign that Kilgore's political concerns and unmobilized scientists disaffection posed a threat to the operations of NDRC lay in the willingness of NDRC researchers and administrators to take time to publicize their approach to research organization. The American Philosophical Society, in November 1943, sponsored a symposium on "The Organization, Direction, and Support of Research." Two participants in war research from the Philadelphia area, Hugh Taylor, a physical chemist at Princeton, and Detlev Bronk, a bio-physicist at the University of Pennsylvania, joined James Conant in presenting their understanding of the historical foundations for successful war research.

Conant began the discussion by bringing the meaning of scientific freedom into clearest focus. Reflecting on the historical debate over the importance of social conditions to Isaac Newton's scientific interests, Conant argued:

It would be my contention that certain types of strong social forces must play upon the world of scholarship if the spirit of learning is to live and flourish. Paradoxically, free inquiry must be powerfully polarized [in the sense of polarized light, not polarized politics] if inquiry is to prosper....For

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if free inquiry is but an aimless, leisurely ramble amidst delightful scenery, it is likely to become an occupation only for the old and intellectually infirm. Conant did not specify the appropriate "polarizers" for free inquiry, but his co-panelists derided disciplines as obstacles to effective research.

Taylor, who was taking on the stressful research into barriers for the gaseous diffusion process of producing U-235, endorsed the policies Compton had proposed while at Princeton and implemented at MIT. He proclaimed research institutes to be the social structure that had prepared the physical scientists for World War II and would meet the nation's future need to maintain scientific leadership. He defined research institutes as "centers of research devoted to a group of connected problems" and characterized them as "'convergent' in distinction from the more usual 'divergent' laboratories, in which problems of many kinds are studied." As American examples of research institutes, Taylor cited the Geophysical Laboratory at Washington and the Mt. Wilson Observatory (both set up by the Carnegie Institution), the Institute of Paper Chemistry at Appleton, Wisconsin, the Institute of Gas Technology at Chicago, the American Petroleum Institute headquarters at Ohio State, and the research laboratories of Eastman-Kodak, General Electric, Bell Telephone, and Westinghouse (even though they did not include the word "institute" in their names). Taylor predicted the development of research institutes for textiles and cellulose chemistry and that the University of California's cyclotron would evolve into a research institute.

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49James Conant, "The Advancement of Learning in the Post-War World," Proceeding of the American Philosophical Society, 87 (1944), 293; also in Science, 99 (February 4, 1944), 87-94.


51Ibid., p. 300.

52Ibid., pp. 300-1.
With the exception of the University of California's cyclotron laboratory, whose use for medical therapeutics annoyed its abler physical scientists, none of Taylor's examples of research institutes bore close identification with an academic department, while none of his examples among industrial laboratories had responsibility for quality control or production problems. Research institutes, in his view, were best part of universities, where they could draw on faculty and students to supervise and carry out research. But wherever located research institutes were all organized around technological themes or research instruments rather than disciplines or applications. They represented Compton's commitment to cooperative research, echoed the success of the Radiation Laboratory, and presented the perspective that Taylor hoped would revive morale in gaseous diffusion research, which had slipped into crisis as its leader, Harold Urey, became convinced that no application of diffusion theory could be realized in time to produce U-235 for the war effort.

While for Taylor, the recent history of physical science had been the success story of research institutes, D-tlev Bronk, whom Compton had wanted to lead MIT's Biological Engineering Department and who directed OSRD's program in aviation medicine, reported, "It has been a disappointment to many biologists that ... there have been few obvious ways in which biologists as a group ... could make immediate and vital contributions to the winning of the war." To the obvious question of what had biologists been doing while physical scientists had been building research institutes, Bronk answered that biologists had been living within the false freedom of disciplinary structure:

The formal organization of research reflects the pattern of teaching departments....[T]he boundaries of these departments have often defined the scope of the instructor's research [and] insensibly created artificial barriers to the free range of inquiry....

It may be desirable to retain certain of these compartments for the purpose of administrative convenience; but there is need for clear thinking about the impediments they offer to effective research and the limitations

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39Heilbron and Seidel, Lawrence, 238-239.
they impose on the character of the training we give our future investigators.\textsuperscript{54}

Bronk's alternative to "departmentalization" was the development of Compton-Taylor styled research institutes in biology. He called for bringing together researchers from multiple disciplines "into powerful organizations for biological exploration," and for "the creation of university institutes for the study of both the fundamental and applied aspects of subjects such as physiology, biochemistry and biophysics, of anatomy and bacteriology and entomology."\textsuperscript{55} Biologists' style of specialization, not OSRD policies, struck Bronk as the chief cause of their impotence, though that did not deter Francis Schmitt from beating back MIT's attempt at Biological Engineering into a Department of Biology.

The virtues of research institutes as "polarizers" of academic research, however, did not become a foundation on which Kilgore's critics in the wider scientific community attacked his bill. Instead, the critics displayed a variety of readings of the bill and assumptions about the proper organizing principles for university research. In 1943, Percy Bridgman, the Harvard physicist, used his address as the outgoing president of the American Physical Society to rail against the concept of scientific planning, as expositied by J. D. Bernal, the intellectual leader of the British Association of Scientific Workers,\textsuperscript{56} and the loose use of the term "science" to include technology.\textsuperscript{57} He also perceived a "growing tendency in this country to overemphasize the socially utilitarian aspects of science, with resulting efforts to control and regiment all scientific activity, as exemplified most strikingly


\textsuperscript{55}\textit{Ibid.}, 310-311.


perhaps in the original Kilgore bill."58 His conclusion—"It is society as a whole that is in a position to provide the mechanism of control [over applications of science] rather than the individual discoverer"—begged the questions raised by the Kilgore bill. Did society have a good mechanism for controlling applications of science; and if not, what sort of agency would improve matters and where might scientists fit into the new organization?

Bridgman's arguments contained paradoxes that others promptly pointed out. One observer urged scientists to consider the significance of the political alliances they were forming before opposing the Kilgore bill for assuming an organic link between the putatively distinct realms of science and industry:

While opposed to the Kilgore Bill on the grounds of too great externally applied control of science, pure scientists found themselves in the company of others who were opposed to it for reasons totally different: whose freely admitted motives are 'public good and corporation profit.'...The negative attitude of industry towards Government-sponsored research has been pointed out....[However] most pure scientists would yet encourage the scientific advance that its expenditure would create.59

Another pointed out that physicists' employment patterns undercut claims of their independence from corporate interests:

It is the ever-increasing employment and importance of physicists in industry—the professionalization of physics—that will ultimately destroy freedom in science...[and] is inviting government control.60

If Kilgore wished to impose governmental controls on science because of the connection between research and industrial practice, and if physicists were working in increasing numbers in industrial research, then Bridgman, in attacking the Kilgore bill, was treating the symptom rather than the disease. "Pure" scientists should either work to make the proposed agency a source of funding for their research or withdraw


59Science, 100 (September 8, 1944) 217-218.

60Alexander Stern, "The Threat to Pure Science," Science, 100 (October 20, 1944), 356. Stern was correct about physicists’ employment opportunities. See Weart, "The Physics Business."
from affiliating with industrial researchers in order to reduce the invitation to
government control.

Where scientists publicly expressed support of NDRC research policies, they
did so without engaging political issues and with due regard for scientists they feared
they were insulting. At the 1944 meeting of the American Association for the
Advancement of Science, Robert Griggs elaborated on Bronk's message, arguing that
biologists had to organize themselves more as a profession and pursue research that
was relevant to the needs of a clientele (whether or not the clients knew they needed
the research).\textsuperscript{61} He judged mycologists intellectually prepared to contribute to war
research but lacking "a competent war committee ... to establish the necessary
contacts and confidence in the competence of their 'profession.'\textsuperscript{62} He judged
ecologists to have wrongly specialized along traditional taxonomic lines for public
service: "at present...we have plant ecologists...and we have animal ecologists...but
we have no adequate and comprehensive knowledge of the total ecology of
pastures.\textsuperscript{63}

Bronk, in his American Philosophical Society talk, did not even acknowledge
the possibility of dissenters to his views, and Taylor only feared that industrial
researchers might view university research institutes as supplanting rather than
complementing industrial laboratories. However Griggs, before the AAAS audience
admitted that in imagining biology as a profession, "I am not talking about biology as
they [academics] know it." And Griggs did not know what to do about academic
biologists. He suggested they support professionalization as the way to achieve
growth, so "even for the men, therefore, who love pure science,... opportunities will

\textsuperscript{61}Robert F. Griggs, "Biology and Agriculture in the Postwar World," \textit{Science}, 101 (March 9,
1945), 236. Note that Griggs directly contradicted Alexander Stern. \textit{Ibid.}, in precisely the language
Stern used.

\textsuperscript{62}\textit{Ibid.}, p. 236.

\textsuperscript{63}\textit{Ibid.}, p. 237.
greatly increase." He advocated the formation of "umbrella societies" of biological specialties to organize professionally oriented research projects that were "more than some of our scientific specialties could undertake." He missed the contradictory character of his suggestions: if academic biologists wanted to be more professional, they could reform their specialized societies or create different ones; and if they did not, an umbrella society dependent on its constituent societies for money and power could not be an agent for reform.

While Kilgore's detractors either embraced a view of science that was at odds with NDRC policy or barely addressed the need to push unmobilized or disaffected scientists to accept the preeminence of NDRC-style research institutes, Kilgore's supporters explored possible models for drawing scientists devoted to research for deepening conceptual foundations into Kilgore's camp. The 1945 Conference on the Scientific Spirit and Democratic Faith, which revived the efforts of New York-based intellectuals to find a pan-disciplinary framework for discussing political life, Harry Grundfest addressed the question "Does Private Industry Threat Freedom of Scientific Research?" He viewed the Truman Committee hearings as having established, beyond need of further discussion, that cartel and monopoly arrangements suppress entire lines of research. But he took a nuanced approach to whether industrial laboratories produced inferior research. He objected to research institutions that focused its researchers on particular subjects—what Taylor would have deemed healthily convergent research institutes—for inhibiting researchers from following conceptual concerns into novel empirical realms and thus dulling their curiosity about "the broader fields of fundamental science." But he did not reject research

64Ibid., p. 239.
65Ibid., 238.
67Ibid., p. 60.
68Ibid., p. 59.
institutes altogether. Rather with passage of the Kilgore bill, he hoped opportunities would be created to reform American research institutes so that they would resemble "the great laboratories of the Soviet Union."69

The appeal to the Soviet model, before the advent of Cold War and full recognition of the deadly depravity with which intellectual debate was conducted in Stalin’s regime, was consonant with the ideals of noteworthy American scientists and science historians. Soviet leaders in the 1920's created research institutes in the image of western models,70 but unlike the independent, locally organized American research institutes extolled by Taylor, yearned for by Bronk, and operating at MIT, Soviet research institutes were under the aegis of the Academy of Sciences, which was the institutional center of theoretical research in pre- and post-revolutionary Russia.71 With the Academy providing the expertise to produce the five-year plans that seemed so attractive to westerners frustrated at their governments' ineffectiveness in the face of the Depression,72 the Academy received funding to support an expanding range of research institutes.73

The Columbia zoologist L. C. Dunn, when he reported on Soviet biology to the National Council of American-Soviet Friendship in 1943, drew a lesson for American politics from the advances in population genetics he knew first-hand had

69Ibid., pp. 59-60.


come from the Soviet Union.\textsuperscript{74}

The progress of biological research in the Soviet Union has taught us a very valuable lesson. It is that control and organization of science by and for the whole community does not kill the scientific spirit or initiative nor submerge the individual scientist in a dead level of anonymity.\textsuperscript{75}

When Dunn directly considered the organizational needs of American science in the post-war world, he downplayed the virtue of casting research in forms that would serve the needs of "clients" as Griggs had advocated, in favor of a more Bridgman-like stress on science's philosophical powers:

In discussing the material means which have to be provided to scientific research, it is often forgotten that the great and lasting changes wrought by science are in men's minds, and that, in the end, science is to be supported for the same reason that education is to be supported. The products of science are primarily increase and diffusion of knowledge and increase in the number of trained minds, and secondarily increase of the technical facilities and production of goods.\textsuperscript{76}

The government agency that could institutionalize such a philosophy, in Dunn's view, was a full-fledged cabinet Department of Science headed by a Secretary of Science, for "only through the political power which attaches to cabinet rank can it [the Department of Science] gain the means and facilities with which to support the study


\textsuperscript{76}L. C. Dunn, "The Organization and Support of Science in the United States," \textit{Science}, 102 (November 30, 1945), 549.
of both immediate and long-term problems [of the nation]."\textsuperscript{77} NDRC, by implication, was too dependent on its military clients to promote properly academic interests.

Dunn’s proposal was an Americanized version of the Soviet system of the Soviet system as Dunn idealized it from his experiences in the latter 1920s. Academic science would unite with political power in the interests of furthering the increase and diffusion of scientific thought and bringing scientifically-trained minds to bear on the technical problems of public policy. The institutional vehicle was not, for Dunn, the American National Academy of Sciences, whose organizational weaknesses were proven by its failure to take charge of war work,\textsuperscript{78} and not a committee of scientists and engineers exercising power under obscure legislative authority but a new cabinet department that would support the efforts of academic scientists to increase their intellectual powers. And the department’s end product would not be five-year plans but an increase in number and sophistication of specialists, who could advise the government about and directly administer its technological interests. Such roles placed scientists, as scientists, in the governmental arena of conflicting interests, but where Darrow saw danger is such roles, Dunn saw a necessity over which it was counterproductive to be squeamish: "There is a politics concerned with policy, and...only through such a political channel can science come to occupy its rightful and necessary place in the state."\textsuperscript{79}

The Intellectual Foundations for Political Conflict

The outpouring of wartime products from private industry induced Kilgore and his staff to concede that a new agency would not be a short-term improvement to the war effort. But they continued to develop ideas for an agency that would, on a long term basis, both address their concerns over the ability of the government to mobilize

\textsuperscript{77}Ibid., 552.

\textsuperscript{78}Ibid., 553.

\textsuperscript{79}Ibid., 554.
the nation's resources and appeal to scientists' desire for financial support for research. However, Bush, whose success as NDRC chairman and OSRD director made him the lone established authority on research policy, chose to develop his own proposal for a postwar science agency rather than press Kilgore to incorporate NDRC-based wisdom. Thus the war's denouement saw the publication of two reports that purported to crystallize the lessons of government support for science in World War II: OSRD's famous *Science, The Endless Frontier*, which enjoyed the prestige and publicity of a presidentially commissioned report, and the Subcommittee on War Mobilization's *The Government's Wartime Research and Development, 1940-1944*, which did not even have a catchy title to attract attention. And thus the war's denouement saw the submission of two bills to establish a permanent science agency: Warren Magnuson's (D-Wash) S. 1285, calling for the National Research Foundation proposed in *Science, The Endless Frontier*, and Kilgore's S. 1297, calling for a National Science Foundation that embodied the perspective of his committee's report.

By eschewing negotiation with Kilgore, it became all the more important that Bush obscure dissension over wartime policies among scientists. The pursuit of consensus among scientists required a retreat from the punchy rhetoric of the American Philosophical Society symposium, and stressing the idea that OSRD's proposal was for a scientists' agency (with the implication that anyone else's was a politicians' agency). With Bush an opponent, it became all the more important to Kilgore to obscure Congressional dissension over wartime expansion of federal powers. The pursuit of consensus among politicians required a retreat from the regulatory powers Kilgore called for in his first bill, and stressing the idea that the subcommittee's proposal was for a public program (with the implication that anyone else's served private interests). The elevation in the moral rectitude with which Bush and Kilgore presented their proposals both obscured their objectives and elicited equally derogatory responses from their critics. However, by reinterpreting the two

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*On Bush's decision to oppose Kilgore and the maneuvers that enabled him to draft a report and legislation, see Daniel Kevles, "The National Science Foundation and the Debate over Postwar Research Policy, 1942-1945," *Isis*, 68 (1977), 5-26.*
reports in light of the pre-war and wartime controversies over the development of research institutions, it becomes possible to understand what was desired from a government science agency and what was lost when the harsh rhetoric led to a political stalemate rather than victory or consensus.

The OSRD report depicted "basic research" in its introduction as the contribution of university researchers to a partnership among academia, industry, and the military:

The publicly and privately supported colleges, universities, and research institutes are the centers of basic research….As long as they are vigorous and healthy and their scientists are free to pursue the truth wherever it may lead, there will be a flow of new scientific knowledge to those who can apply it to practical problems.81

Left implicit was the premise that university administrators would build harmonious institutional relations with the corporate sector and the military. Both the Medical Advisory Committee and the Committee on Science and the Public Welfare worried that federal support would result in centralization that would rob university administrators of the power they needed to cultivate common interests between university and industrial researchers. They feared the image of successful centralized planning during war, when all was judged by the single short-term goal of military advantage, would apply to peacetime basic research, whose goals should be multiple, long-term, and open to competitive interpretation by research universities.82

By contrast, the Subcommittee on War Mobilization depicted basic research as essential to insuring the ability of government officials to make fully informed decisions:


82Ibid., especially 50 for the Medical Advisory Committee and 94 for the Committee on Science and the Public Welfare.
Because science is decisive, both in war and peace, we must provide for it systematically within the regular framework of the Government....Our people must support basic research instead of relying on basic research evolving from the laboratories of other nations.83

Left implicit was the premise that government officials should be deciding the technological directions that represented the public interest. Instead, the subcommittee pointed out each war had inspired the creation of new research agencies, but the agencies of one war never seemed adequate to the needs of the next. World War II had fostered 12 new agencies more or less directly involved in war research, and the biggest spender among these, OSRD, was slated to close.84 They feared the image of peacetime normalcy would justify a return to the one-step-behind syndrome that had plagued the government's use of science.

To justify the different fears acknowledged in the two reports, their authors cited two different sets of statistics. For OSRD, the point was to demonstrate that basic research was in danger of becoming the weak link in the chain of institutions contributing to technological improvement. Thus Bush turned to figures on research expenditures for the peace-time period 1930-1940 to show that the growth rate for research in industry and government was nearly double that of research in universities and research institutes.85 In this context, European events were extra evidence for the OSRD cause because Europe's destruction had bankrupted the United States' source of borrowed basic science.86 For the Subcommittee on War Mobilization, the point was to demonstrate that reversion to pre-war practices and levels of support would deprive the nation of a necessary power for the postwar world. Thus the subcommittee compared research expenditures during war-time to peace-time to show


84Ibid., pp. 22-24.

85Bush, Science, 19-20. By OSRD calculations, the ratio of industry-government to academic-institute research in 1930 had been approximately 6:1; in 1940 it was 10:1.

86Ibid., 78.
that research expenditures nationally had more than doubled during the war, and that the government's share had gone from 1/6 to 3/4 of the total. In this context, the message of European events was that Germany had used state-supported science to build its military capabilities, and that the U.S. should have been doing likewise.

The differing views on the primary purpose for government support of research led to different views on the relationship of science to technology. Bush again stressed the importance of universities to technological development:

If the colleges, universities, and research institutes are to meet the rapidly increasing demands of industry and Government for new scientific knowledge, their basic research should be strengthened by use of public funds.

Behind the notion that technology placed demands on scientific knowledge was a definite view on what sort of research strategy would best advance science:

In the 19th century, Yankee mechanical ingenuity, building upon the basic discoveries of European science, could greatly advance the technical arts. Today the situation is different....In the next generation, technological advance and basic scientific research will be inseparable; a nation which borrows its basic knowledge will be helplessly handicapped in the race for innovation.

Left implicit (or secret) was Bush's personal success in making the calculation problems of power engineers an inspiration for mathematical analysis in general, Compton's imposition of Sedgwick's ideals as MIT-wide policy over the legacy of the Noyes-Walker conflict, and the blending of nuclear physicists with electrical engineers in the MIT Radiation Laboratory.

The subcommittee reversed OSRD's direction of demands between science and technology:

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87Subcommittee on War Mobilization, Government's, 5-11.

88Ibid., p. 16.

89Bush, Science, 7.

90Ibid., 78-79.
The technological accomplishments of the nineteenth century could not measure up to demands of the science of the twentieth century. We have had to grow with the times.91

By combining the ideas that "basic research is the foundation of all applied science"92 and that "free, open, and widespread exchange of information is basic to scientific and technical advance," the subcommittee hoped to gain some leverage over the "major problem in the economic development of this country before the war [of] the increasing concentration of industrial research resources in a few large industrial corporations."93 Left implicit was the Truman Committee’s finding that patent agreements between Standard Oil and I.G. Farben were a cause of the initially inadequate production of synthetic rubber, and the idea that the public’s interest lay in supporting research that would not result in patents that large firms could buy and trade in their own interests.

Implicit in OSRD’s fondness for what Hugh Taylor called university-affiliated, convergent research institutes lay a political framework that ran counter to the aims of the subcommittee. A foundation for convergent research institutes was powerful university administrators to induce faculty members from multiple departments to organize their research interests around their common interest in some phenomena or instrumentation rather than their distinct conceptual concerns. Compton at MIT in the 1930s had convinced the Rockefeller Foundation to make occasional room for such an approach, and NDRC had structured itself to favor that approach. To insure that convergent research institutes remained prominent in universities, a government research agency would have to eschew the use of central authority for setting and enforcing national standards in favor of empowering local officials to pursue the best arrangements they could make. Within the framework of American politics and government, such administrative terms implicitly stressed the values of regional

91Subcommittee on War Mobilization, Government’s, 12.

92Ibid., p. 15.

93Ibid., p. 18.
traditions, local government, and the ties between local officials and their representatives in Washington over the values of national identity, federal power, and centralized rationality in the management of the federal government. The OSRD report recognized that small businesses rarely supported research laboratories, so that regions with large firms that recruited research staff members from universities with advanced research programs were better situated to spawn convergent research institutes. But the solution to such inequity, in the OSRD view, was not for government research that made up for the inadequacies of small business, but to fund research cooperatives of small firms when they have displayed the will and ability to undertake research "sufficiently basic to achieve the most significant results." In short, small business should compete with large firms by collectively behaving like large firms.

Implicit in the Subcommittee's fondness for an economic policy that stressed widespread competition lay a research framework that ran counter to OSRD's. To insure that the activities of a government science agency did not lead to patent positions that conferred advantages on large firms implied that agency-supported research be oriented towards educational objectives or technical objectives that served government responsibilities:

Military and medical research...should have more Federal support than before the war. Basic science research...should have substantial consideration.... Low cost housing has been recognized as a governmental responsibility and research in this field would enable the Government to discharge its responsibility..."95

The Subcommittee described the "basic science research" universities should pursue as being "of a general character."96 Though not much of a description, it potentially appealed to scientists intent on research that deepened a discipline's foundations, for


95Subcommittee on War Mobilization, Government's, 13.

96Ibid., p. 14.
nothing could be more general in character than an improved vantage point from which to write textbooks.

Even the very name of the agency proposed in the two reports implied differences in orientations. Bush preferred National Research Foundation, while Kilgore suggested National Science Foundation. "Research" did not carry connotations of exclusivity; it was an activity carried on across university and industrial laboratories. But "science" implied a type or style of research to be distinguished from technological or engineering research; educational institutions claimed science as a cultural legacy they would perpetuate by teaching.

Bush thought of a NRF as a broadened version of NDRC. A board of nine (rather than five) individuals, appointed by the president to staggered four-year terms, would choose a director to oversee an agency with divisions for medical research, the physical and natural sciences, and national defense (rather than problems underlying devices of warfare). Bush preferred the board members be otherwise unconnected to the government or "any special interest" and members of the military services and the Public Health Service were to serve on advisory committees to the relevant NRF programs in strictly a liaison capacity. Thus the NRF board, like NDRC, would have the right to support convergent research institutes in any area and at any institution without regard to other agencies interested in using the phenomena on which the institutes were converging; but the NRF board, like NDRC, had no basis for entering any jurisdictional disputes among those other programs. Indeed, in listing the functions of the NRF, Bush left out coordinating government research and recommended creation of a separate Science Advisory Board to provide advice on the policies of government agencies performing research. NRF's patent policy would be the same as NDRC's had been for contracts to institutions that were not temporarily employing members of other institutions. On a case-by-case basis, NRF

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98 Ibid., 18 with respect to the military, 62 for the Public Health Services.

99 Ibid., 20-21.
would decide what would "leave the cooperating [research] organizations with adequate freedom and incentive to conduct scientific research;" in general, the public's interest in any resulting patents would be satisfied by a royalty-free license for government use the patent.100

The Subcommittee viewed NSF as expanding on the public administration that had worked for OSRD:

This [the war record of advances and improvements] is partly because of the necessity that impelled it, but it is also in part a factor of responsibility with each of the three major agencies in research [War, Navy, and OSRD] being directly accountable to the Commander in Chief. The time has passed for purely advisory performance, for piecemeal research agencies which were adequate at one time but are unsuited to the demands of all-out technological warfare.101

Thus the Subcommittee recommended that the agency be headed by a presidentially appointed director, who would consult on all matters of major policy with a National Science Board, which would consist of representatives of eight cabinet departments and eight members appointed at large by the president. This structure would draw the NSF director into coordination of government research agencies and into evaluations of their missions in light of the results achieved by private researchers funded by NSF and inhibit the NSF from initiating university research programs that trod too closely on areas of interest to other government agencies. In order to insure that NSF-supported research would not lead to further concentrations of economic power, the Subcommittee recommended that the agency-creating legislation require that any patents stemming from NSF-supported research be the property of the government with the NSF empowered to grant non-exclusive licenses to anyone wishing to use the patent.102 Amidst the diversity of patent policies the Subcommittee found in the agencies contracting for research in World War II, it found the policy of the Rubber

100 Ibid., 38.

101 Subcommittee on War Mobilization, Government's, 12.

102 Ibid., 27-29.
Reserve Company, which had been created to handle the synthetic rubber program most compelling:

[I]t has been the definite policy of RRC to require that all research work authorized and paid for by RRC shall be subject to arrangements which permit RRC to make the results of such research as widely known as possible, both from the technical and patent standpoints. This policy was deemed necessary to avoid the creation of private monopolies or unjust enrichment of private corporations as the result of research expenditures which are ultimately borne by the taxpayers.¹⁰³

Conclusion

World War II did cause a discontinuous shift in the level of political interest in research. Both the executive and legislative branches in 1945 were considering how, not whether, to build a permanent system of public support for scientific research into the federal government.¹⁰⁴ However, the increased interest in research policy did not eliminate the need to determine the principles on which a research agency should operate and to identify a correspondingly appropriate position for the agency in the government. The lofty language with which the authors of Science, the Endless Frontier and The Government's Wartime Research and Development presented proposals for a new agency still disclosed significant differences over means and ends.

Bush wished to continue, on a broader scale, NDRC's practice of supporting intra-university, multi-departmental research institutes that converged on phenomena or techniques of relevance to both scientists and engineers. By vesting authority over the National Research Foundation in a board of private citizens who would choose a director, he both echoed the structure of NDRC and isolated the NRF from bureaucratic politics in the executive branch. By declining to stipulate a patent policy for the NRF, he made sure the NRF would not centralize power over the development of technologies. Kilgore wished to improve on the national administration of public resources that had eventually succeeded in churning out the materials for war. By

¹⁰³Ibid., vol 1, p. 53.

¹⁰⁴Genuth, "Groping."
vesting authority over the National Science Foundation in a presidentially appointed
director who would be advised by a board including representatives of cabinet
departments, he both echoed the structure of OSRD and invited the NSF into
questions of coordination and jurisdiction over government research and the
development of technologies for the public interest. By insisting that the NSF own
patents stemming from the research it supported and license them royalty free to any
interested party, he made sure the NSF would not increase inequality in the
concentration of economic power.

Much hinged on how scientists would interpret the difficulties in wartime
research and how public officials would interpret the successes of wartime research.
Could Bush hold the loyalties of scientists who were not fully comfortable within the
locally administered laboratories in which NDRC had matched military functions with
scientific and engineering skills or who had been put under military administration
because their ambitions and preferred form of organization did not match NDRC’s
structure? Could public officials uncomfortable with decentralized delegations of
power because of the problems the national government incurred in mobilizing
industrial resources for war accept that the political criteria for a national agency to
support scientific research were different from other areas of governmental activity?
The answer to both questions were negative. Rather than escaping tensions in the
practice of federalism, research policy reinvented them.
CHAPTER 11

THE CONTENT OF SCIENCE POLICY AND THE STRUCTURE OF A GOVERNMENT SCIENCE AGENCY

Through 1943 and 1944, Bush himself was publicly silent on the Kilgore bill in particular and postwar needs for research in general, but did comment on Kilgore's bills to Kilgore in correspondence. Kilgore found Bush's criticisms sufficiently constructive that he asked Bush to look over a new draft of his legislation late in 1944. However, on the same day that *Science, the Endless Frontier* was issued with fanfare and to mostly rave reviews, Senator Warren Magnuson, a Democrat from Washington, introduced legislation (S. 1285) for a National Research Foundation following the design stipulated in Bush's report. Kilgore quickly introduced legislation for a National Science Foundation (S. 1297) so as not to be left behind.

In feeding Magnuson legislation for a NRF, Bush had committed OSRD to at least a period of political warfare with Kilgore. Magnuson's bill threatened to turn Kilgore from a legislative pioneer in a new area of government policy into an under-informed meddler pursuing a legislative dead end, and it turned Bush, from Kilgore's perspective, from a constructive critic into a less-than-trustworthy foe. Both Magnuson and Kilgore were Democrats, so partisan pressures did not harden their positions, but the joint hearings they held prompted some witnesses to endorse one

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1Kevles, "National," 14-16.

2Although a member of the Commerce Committee, Magnuson did not get to hold hearings independently of Kilgore.
bill or the other and added an edge of implied preference even to generic philosophizing on the need for a foundation.

The hearings also brought government-wide issues of public administration directly into the scientists' debate over the best organization for their preferred research strategies. These issues were partisan and a source of tension within the Democratic Party because President Roosevelt, during his second term, had inspired many in Congress to be suspicious of the presidency by trying to pack the Supreme Court and by seeking the power to reorganize the executive branch. The latter was especially relevant for the design of a science foundation, which could supplement the government's research agencies the way NDRC took on the investigation of radar at shorter wavelengths than the military services' laboratories judged immediately relevant, or could set the agenda for government agencies the way uranium fission's advocates obtained presidential orders for the Army Corp of Engineers to build the needed factories and laboratories to make atomic bombs.

The combination of personal rivalries between Bush, Kilgore, and their respective supporters plus resonance with longer standing governmental issues with partisan associations proved toxic for all concerned. When the 79th Congress failed to pass either Kilgore's, Magnuson's, or a compromise bill in the spring of 1946, the possibility of creating a science agency without the obstacles of political partisanship passed. The 1946 Congressional elections brought Republican majorities to both houses of Congress, and the Magnuson bill became the Republicans' political property. To the surprise of Bush, President Truman vetoed the science-agency legislation the Republicans passed, and to the surprise of many more, Truman won the presidential election of 1948. When Congress finally passed science-agency legislation Truman found acceptable, neither Kilgore's nor Bush's original vision was feasible.

Conflicts Among Scientists Over Research Policy

The scientists testifying before Kilgore and Magnuson addressed the bills in terms of their understanding of the state of American science before World War II, of
the relationship between their wartime and pre-war research, and of the importance of
administrative power for creating the working relationships they needed to satisfy
their research ambitions. The scientists more closely involved with NDRC research
or more collegial with NDRC's members viewed the pre-war years as triumphant for
American science, found continuity between their pre-war and war-time research, and
stressed the appropriateness of the Magnuson bill's structure for providing the
administrative oversight scientists needed to use their skills to best effect. The
scientists at the fringes of or excluded from NDRC research had jaded views of the
pre-war years, found discontinuity in pre-war and post-war research, and saw no use
for administrators as managers of their intellectual relationships. At stake from the
scientists' perspective was whether a government science agency would empower
administrators of research institutions to rearrange specialties in the interest of
differentiating their institutions and responding to the demands for expertise from the
employers of their graduates, as MIT's administrators had wished to do with
Rockefeller funding in the 1930s. Or whether a government science agency would
strengthen scientists' efforts to find research paths that would deepen the foundations
of the disciplines in the interest of improving the philosophical power of a science
education and spreading such habits of thinking into political and governmental
activities.

The NDRC's view of the past was presented at the beginning of the hearings
by Isaiah Bowman, the president of Johns Hopkins University, to whom NDRC had
turned for an administrative foundation for a laboratory to investigate proximity
fuses. Bowman stressed the importance of freedom for progress in science and then
asserted:

> I have described a condition that has prevailed in the United States, notably
> between the world wars, when we made such rapid progress in practically
> all fields of science, and particularly in the application of science.
> However, we should remember that it was not Government money...that
> produced these effects...and enabled American science during the Second

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World War to achieve its amazing results....I confess at once that I deplore the tragic circumstances [the need for continued military preparedness] that have forced us to consider the problem of Federal support for scientific research. I wish that we could return to the good old days.\footnote{United States Senate, Committee on Military Affairs, \textit{Hearings on Science Legislation}, (Washington: Government Printing Office, 1945), 11-12.}

Even if the good old days did not return, something better, from Bowman's perspective, could result from government support for the multi-departmental, convergent research institutes that Hugh Taylor had touted as the organizational feature that had put physical scientists in the 1930s in position to contribute to war work in the 1940s. Bowman looked forward to a NRF that would enable university administrators to organize their faculties to respond as effectively to a vibrant military as they had to vibrant industrial research laboratories before the war.

Bush and his OSRD allies built on Bowman's historical perspective by stressing the continuity from prewar to wartime research and the continuity they hoped to build from wartime to postwar. In the shift from wartime to peacetime research, Bush argued, production responsibilities should be removed from research laboratories:

\begin{quote}
During the war,...it has been necessary in some cases to build up large, highly integrated developmental laboratories which could not only develop new weapons but often actually produce them....This is the way to win a scientific war, but this is not the way to advance the frontiers of knowledge.
\end{quote}

But a NRF was necessary to keep militarily relevant research within the purview of "basic science" in scientists' eyes:

\begin{quote}
Our objective [in peacetime] should be to finance genuine basic scientific research wherever it may be found... Unless Federal fundamental funds are brought to bear ... there will be certain fields of basic research which will fail to receive due attention....I am thinking particularly of basic research on military and naval problems as well as research in medicine and the related sciences.\footnote{\textit{Ibid.}, 202-203.}
\end{quote}

Lee DuBridge, the director of the Radiation Laboratory, echoed Bush's sentiment in arguing that the NRF's National Defense Division be a "powerful instrument" that
would tap academic resources for defense and "expand these resources while it taps them." Detlev Bronk exuded enthusiasm over the benefits of wartime research for biologists:

Biologists were employed [in the war] ... to guide the design of weapons so that they would best satisfy the biological requirements of the fighter. ... Their accomplishments now suggest an important role for biological science in making our technological civilization more suitable for human life....The cooperation of biologists and physicists that made such a program successful is an example of the advantages that come from the pooling of scientific disciplines, and the dissolution of boundaries between sciences.

Karl Compton envisioned a NRF that would recapitulate on a grander scale his initial survey of the armed services' laboratories, which led to NDRC use of military functions for its internal structure, and the decision to place the Radiation Laboratory at MIT.

[The NSF program should be] aimed...at focusing on important scientific or technical objectives. Such a program, for example, would consider one after another of the most important industrial or agricultural or economic problems of the country and support a constructive attack on these problems at the places and with the personnel which show specially good promise of bringing about the desired results.

By rewarding those universities that united the specialists needed for coordinated investigations of important objectives, Compton's NRF would encourage others to differentiate in order to position themselves to be a prime candidate for research into other important objectives.

The importance of an authoritative board of proper composition to fulfill the ambitions of NDRC supporters to build continuity in research policy came through in

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*See Section 8.2, above.

*Hearings*, 626.
an exchange between Kilgore and Edward Bowles, the MIT electrical engineer who had organized MIT's research in directed radio waves and served on the Microwave Committee:

Bowles: We are interested here, I believe, in science and technology. I don't want to get into a conflict of definitions, but I believe perhaps you are using the term "scientist" to include...those people who are interested in the application of science.

Kilgore: No. The bill largely goes to what is called basic or pure science.

Bowles: I believe we should include [in the NSF board] people who have some appreciation of the entire gamut of thinking. I can't believe that we want only what we would call the ivory tower or cloistered scientists guiding these problems. I have in mind men with the breadth of Bush and Compton, for example, whose points of view are very catholic.10

Bronk endorsed the Magnuson bill because it recognized "the necessity for setting up adequate safeguards against the control of research by scientifically incompetent administrators."11 His willingness to ignore the history of the sciences in universities and corporations with non-scientists as administrators was testament to his commitment to a science agency as a vehicle to bring about beneficial poolings of disciplines within universities. Who else but such a board could be expected to identify fruitful reshufflings of disciplinary relations?12

The wartime experience of all these men had been that academic scientists, when organized in research institutes concerned with military functions, would produce militarily useful knowledge and techniques. The ingredients for success had been entrepreneurial university administrators, who before the war had organized their researchers in multi-specialty groups more dedicated to broadening than deepening a discipline's foundation, a supervisory committee with the collective wisdom to

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10Hearings, pp. 284-5. Bowles' recommendation of Bush seems especially strong when one considers that he and Bush did not like each other personally. See Kevles, The Physicists, 310-311.

11Ibid., 563-564.

12Robert Griggs, who before the publication of Science the Endless Frontier had echoed Bronk's complaints about biologists' social relations without arriving at a plausible route to reform (see Section 10.3, above) also endorsed the Magnuson bill as having the better administrative structure. Ibid., 602.
mediate conflicts that arose when the disciplinary diversity threatened to pull the
groups in too many directions, and the committee’s privilege to organize itself and
spend its funding independently of the interests or wishes of other agencies. The
commitment of these men to the administrative structure proposed in Science the
Endless Frontier reflected their desire to continue NDRC traditions. With a policy-
making board of scientists with the "catholic points of view" of Bush and Compton
overseeing a small number of broadly defined internal divisions, the science agency
would be structured to identify problems of wide significance and the best places to
investigate them.

Opposing the NDRC scientists on every point were accomplished scientists
either little involved in war work or Manhattan Project scientists who had worked
under the Army Corp of Engineers. Harlow Shapley, the director of the Harvard
Observatory, who had concentrated on building a graduate program in astronomy at
Harvard between the wars, directly contested Bowman’s view of the "good old days:"

It should be somewhat humiliating to us to realize that the revolutionary
sulfa drugs had their beginning in German research laboratories; that atom
splitting was discovered in Berlin; that the basic pioneer work that has led to
radio and radar and the enormous American electronics industries was that
of a German professor…. That situation has been well stated in the Kilgore
report [The Government’s Wartime Research and Development] and the
proper conclusion has been drawn; namely, hereafter we must rely on
ourselves for basic research.¹³

Harold Urey, the Nobel laureate who had been eased out of leadership in the
development of gaseous diffusion for the Manhattan Project, later echoed Shapley’s
humiliation by comparing the number of Nobel Prizes awarded to Europeans
(especially Germans) and Americans before the war.¹⁴

The firmest statements of discontinuity between prewar and wartime research
came from a panel of Manhattan Project scientists, who had been alertly lined up to
testify by Kilgore’s aides at a time when they were denied a forum by the House

¹³Ibid., 49.

¹⁴Ibid., 658-661.
Military Affairs Committee, which was then considering the War Department's version of atomic energy legislation.\textsuperscript{15} Robert Oppenheimer characterized the Manhattan Project as scientifically hollow:

In the special field of the development of atomic weapons I think it is fair to say that no fundamental discoveries, no real increase in our understanding of nature, and not even any great scientific questions for the future to answer, resulted from the wartime work.\textsuperscript{16}

When asked about how a science agency would affect scientific freedom, Oppenheimer seemed to take direct aim at the advocates of continuity between wartime and peacetime: "I have heard it said by people who ought to know better that the purpose of the National Science Foundation was to see that we attack the problems of the peace as we had attacked the problems of the war. The trouble is that they are not the same problems."\textsuperscript{17} Robert Wilson sought to capture the difference between his wartime and peacetime pursuits by drawing a distinction between fundamental and programmatic research, the former aiming to reveal and elucidate unexpected facets of nature and the latter aiming to characterize more precisely what was already assumed to exist. Though Wilson noted his distinction could, in principle, be breached,\textsuperscript{18} he thought they were best separated administratively. Programmatic research, he maintained, could be planned on the basis of technological need for the information, but fundamental research required "freedom of discussion not only among our national colleagues, but also among our

\textsuperscript{15}Alice Smith, \textit{Peril and Hope}, 152.

\textsuperscript{16}\textit{Hearings} (1945), 300.

\textsuperscript{17}\textit{Hearings} (1945), 308, my emphasis. Oppenheimer accepted Army administration of Los Alamos on the basis of this perspective. See Oppenheimer to Rabi, 26 February 1943, in Smith and Weiner, eds., \textit{Robert Oppenheimer: Letters and Recollections}, 250.

\textsuperscript{18}\textit{Ibid.}, 330-331. Programmatic research at Los Alamos, he averred, had revealed an unexpected result that inspired major consequences for bomb design. NDRC scientists would have designated Wilson's loophole a principle: the attention of different specialists to a technical objective generates fruitful material for further research.
international ones." The disappearance of geographically bounded institutions in his consideration of the needs of fundamental research echoed John Wheeler's prospectus for ultranucleonic research. The success of Kilgore's aides in providing a forum for the Manhattan Project scientists worried Magnuson's aide, who reported to OSRD's leaders: "Schimmel [Kilgore's aide] is so agreeable these days I suspect no good. I believe he is working on organizing the atomic physicists. I see a lot of those boys hanging around his shop." His fear of an alliance between Kilgore and Manhattan Project scientists proved unfounded. Congress treated the establishment of an Atomic Energy Commission as a topic on which action had to be taken promptly, and the organization that the Manhattan Project scientists established, the Federation of Atomic Scientists, labored with a "myopia in regard to atomic energy" that precluded vigorous participation in the NSF debate. Even on the day set aside for their testimony before Kilgore and Magnuson, the Manhattan Project scientists did not address whether they would be better served by either bill.

It fell to the unmobilized biologists to make an explicit case for Kilgore's proposed administrative structure. L. C. Dunn realized that Kilgore's bill came closest to Dunn's proposal for a cabinet-level Department of Science. When Magnuson noted that a board-directed agency conformed to the precedents set by the National Advisory Committee on Aeronautics, the National Cancer Institute, and proposals for an Atomic Energy Commission, Dunn responded:

We are dealing here, I take it, with basic science as a whole. The others deal with individual segments and applications, not primarily with the

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19Tbid., 331.

20See Section 9.5, above.

21John Teeter to Carroll Wilson, et. al., October 24, 1945, Records of the Administrative Office, OSRD, National Archives, Record Group 227, Item 13, Box 53.

22Smith, Peril and Hope, 325-327.

23Hearings, p. 553; see Section 10.3, above.
discovery of principles…. I think in an organization … attempting to cover
the whole of natural science, a public responsibility and a permanent
character is imposed, which seems to me to have the same importance as
any existing Government departments which do follow the other
pattern…[of] a responsible cabinet officer at the head. 24

The desire for a permanent, public commitment to "the discovery of principles," as
opposed to their pursuit as necessary for research into technologies of governmental
significance, eliminated for Dunn the virtues of a panel of sophisticated scientists at
the head of the agency. When Magnuson asked Dunn whether research decisions
should be made at the agency's top level or at the intermediate level of the internal
divisions, Dunn responded, "Even further down. By the scientists in the colleges and
research institutes who are applying for the funds." 25 This scenario implied, in
contrast to Bronk's enthusiasm for pooling disciplines, that biologists' perception of
the relevance of another field's techniques to biological questions was a pre-condition
to collaboration and that university-based specialties legitimately formed only when
individual scientists were independently attracted to studies that shed light on shared
theoretical principles. 26

The scientists' testimony reveals that they differed over the health of pre-war
American science, the degree to which war research built on their prewar work and
laid a foundation for postwar work, and the administrative apparatus that would best
assist their causes. For Bush and his supporters, the pre-war years had been ones of

24Ibid., 556.

25Ibid., 558.

26Ibid., 551-552. The bills received an even more pointedly pro-Kilgore interpretation from Dunn's
coopanelist, the Washington University zoologist H. B. Steinbach. Steinbach suggested that the
Magnuson bill's "active language"—calling for the agency to "initiate and support basic scientific
research,"—combined with the autonomy of its board of scientists would lead to a "board that might
well feel itself charged with the true initiation of scientific research projects, allocating funds to those
who will work on them." In contrast, Kilgore's passive call "to provide adequate support for, and to
otherwise encourage scientific research," combined with a presidential appointee as director, would lead
to a "responsible public official, … [who] would certainly hesitate before attempting to specify new
projects which some other individual should carry out." The conclusion turned Percy Bridgman on his
head: "If it is agreed that basic research should be planned generally at the individual-scientist level,
then it would seem to follow that the form of administration provided by S. 1297 [the Kilgore bill] is
desirable." See 586.
successful institution building as universities developed research institutes around new instruments or particular phenomena of interest to faculty from multiple disciplines. The war provided Bush and his associates with the authority and funds to showcase the power of their approach to the organization of university research while narrowing their focus to the militarily relevant and drawing them into policy-making circles they had no previous ambition to join. The point of a national science agency should be to retain NDRC’s organizational approach to research, to broaden the base of organizational foci to include economically and medically relevant instruments and phenomena, and to separate the support of research from further participation by university researchers in high-level, executive-branch discussions of military or economic strategy. This perspective was manifest in the MIT proposal for a Research Laboratory for Electronics, which would extend the Radiation Laboratory’s traditions into peace-time university science, maintain the interest of researchers trained in experimental nuclear physics in the character of microwaves and the techniques for working with them, and create a public space for industrial researchers and military officers to learn from academic efforts and educate academics about their needs. The administrative structure of the Magnuson bill favored Compton’s style of proposal.

Bush’s antagonists within the scientific community viewed the prewar years as a time when American scientists had, metaphorically speaking, learned to play all the notes but not how to make music. The war, while bringing government support for some university scientists, had interrupted the efforts of America’s few scientific maestros to instill aesthetic sensibilities in their technically sophisticated students. Even the Manhattan Project scientists, whose work was made possible by conceptually stimulating pre-war research, found little of intellectual significance in their war work. The point of a national science agency should be to strengthen the quest of university scientists in all departments for new and improved principles to serve as the foundation for science educations and technological imaginations, to expand the influence of university research scientists over colleagues at universities that were less supportive of faculty research, and to secure for university researchers a share of public funds commensurate to their public contributions. This perspective was
manifest in Wheeler's call for a conference "of all good men" in the field to plan investigations into "ultra-nucleonics" as the area that offered the best challenge to physical theory and the greatest inspiration to technological imaginations. The administrative structure of the Kilgore bill provided the means to organize such a conference and acquire support for the actions it recommended.

Ultimately, the dispute among scientists over a national science agency reduced to which of two distinctions was the most important. Bush and his allies were at pains to distinguish between wartime and peacetime science agencies. NDRC had started by matching military functions and scientists' skills in universities that had cultivated a capability for such research. As the war progressed, NDRC had to assume ownership of patents for the inventions of scientists working outside their home institutions, had to accept production orders for new equipment developed under its sponsorship, and had to subsume itself under OSRD so that one of its members would have the bureaucratic right to debate the implications of new equipment for military strategy with the military's leaders. A peacetime agency would revert to being solely a matchmaker—and heed non-military functions as well—in a grander manner than MIT had tried to use the Rockefeller Foundation before the war. Thus Bush's NSF would not be directly responsive or useful to the president but would stake its fortunes on convincing Congress that its expenditures improved military and other government functions.

For Bush's antagonists, on the other hand, the critical distinction was not between wartime and peacetime science agencies but between wartime and peacetime science. OSRD's virtue lay in bringing the intellectual power of academic scientists to the notice of public officials. As the war progressed, scientists had increasingly shelved their normal standards for worthwhile research in order to focus on the accomplishments that could have a short-term military impact. A peacetime agency would retain the wartime structure that had made university scientists prominent, but support the efforts of scientists in all disciplines to deepen conceptual foundations rather than concentrate on limited areas, as had the Rockefeller Foundation. Thus Dunn's and Urey's NSF would be accountable to the president in order to have
opportunities to promote science to the government and the power to balance the competing claims of the sciences for public funds.

While the testifying scientists were largely debating the needs of and best framework for academic research, their arguments ended with implications for the position and responsibilities of the a science agency within the government. The logical structure of their arguments opened the possibility for the formation of coalitions between factions of scientists and politicians, who were bound to judge science-agency proposals on the basis of whether they contributed to their sense of a healthy government. The prospect for politicians dividing over the structure of a new agency were as apparent as divisions among scientists.

Public Administration and the Design of a Science Agency

Public officials, like academic scientists, also judged science-agency proposals by whether they captured those aspects of wartime experience that were healthy for peacetime. But the context for public officials was their views on what had been beneficial or counter-productive in the New Deal. The proposals for a science agency fit the contours of an unresolved debate over the structure of power within the executive branch. Following the raft of new federal responsibilities that Roosevelt requested and received legislation for to combat the Depression in his first term, managing the organizations carrying out those responsibilities became problematic in his second. But his proposals for authority to reorganize the executive branch provoked controversy over both the goal and the means for exercising such power: should executive reorganization promote economy in government operations or managerial efficiency in presidential administration; should inter-departmental councils work out reorganizations or an independent research staff reporting to the president?27

In 1937, Roosevelt, acting on advice from leading social scientists with ties to the Natural Academy of Sciences, endorsed the goal of managerial efficiency and the means of an independent research staff. However, a panoply of interest groups and the cabinet departments that administered the policies the planners would coordinate opposed the proposal because it disrupted the relationships through which they exercised influence, and their concerns resonated in Congress, especially in the House, where many Democrats had become more concerned with maintaining Congressional prerogatives than party loyalty when considering presidential initiatives. Only in 1939 did Congress pass a weakened Executive Reorganization Act, which it further diluted in 1942 when the House refused to fund the National Resources Planning Board, the president’s research agency for planning.

Public officials testifying before Kilgore and Magnuson fell into an alignment that recapitulated the executive reorganization debate. The closer they were to the presidency, the more strongly they supported the Kilgore bill; the more concerned they were with individual government agencies or the more closely aligned with interests disaffected altogether from the Roosevelt administration, the more they favored the Magnuson bill. Kilgore’s two staunchest supporters among government officials, Harold Smith, the director of the Bureau of the Budget, and Henry Wallace, Roosevelt’s vice president during his third term, Secretary of Agriculture during his first two, and Secretary of Commerce in 1945, exemplified the appeal of his bill to advocates of presidential authority. Both had decided in the 1930s that presidential management of the executive branch was essential to discharge enlarged federal responsibilities; both thought the Kilgore bill better represented their experiences.


28Genuth, "Groping."

29Polenberg, Executive, 7 and 20-27.

30Karl, Merriam, 241.

31Polenberg, Executive, 162-180.
Wallace had come to favor increased presidential management of the executive branch even though, as Secretary of Agriculture, he had unambiguous jurisdiction over the Agricultural Adjustment Act. He found he still needed a decision from Roosevelt to resolve an intra-departmental dispute with broad political implications.\textsuperscript{32} Even with intra-departmental unity at headquarters and an improved legislative mandate,\textsuperscript{33} he was still limited in the policies he could implement to assist rural citizens by the lack of a federal field staff and the opposition of the states’ agricultural extension services to federal efforts to reform local conditions.\textsuperscript{34} Thus when Wallace, in December 1939, publicly addressed social scientists "Emerging Problems of Public Administration," he felt obliged to bracket his discussion of policy issues with a consideration of "the necessity for improved techniques [in administration]."\textsuperscript{35} Wallace strove to imagine a structure in which policy determinations would be increasingly centralized but the administration of policy would be decentralized. He called for stronger staffs for high-level government executives, reasoning that executives with such staffs would concentrate on explaining their policies to the public, coordinating their policies among themselves, and improving the

\textsuperscript{32}The dispute was over how to increase prices for farmers’ products—by governmental control of production or by government-sanctioned marketing agreements. See Van L. Perkins, Crisis in Agriculture: The Agricultural Adjustment Administration and the New Deal, 1933, (Berkeley: University of California Press, 1969), 83-89 and 179-184.

\textsuperscript{33}The Supreme Court ruled the original Agricultural Adjustment Act unconstitutional, but Congress reestablished the AAA while abandoning its industrial counterpart, the National Recovery Administration. For a comparison of the political administration of the two agencies, see Theda Skocpol and Kenneth Finegold, "State Capacity and Economic Intervention in the Early New Deal," Political Science Quarterly, 97 (1982), 255-278.


\textsuperscript{35}Henry Wallace, "Emerging Problems in Public Administration," address to the American Political Science Association and the Society for Public Administration, (1939), bound copies in University of Chicago Library, 2.
organizational frameworks of their departments and leave policy implementation to subordinates in position to work directly with the affected citizens.\textsuperscript{36}

From Harold Smith's perspective, the Bureau of the Budget was the proper agency for transmitting proposals and orders between political executives and the president. He viewed the Bureau's creation in 1921 as wisely ending presidential detachment from public administration. The Bureau's move from the Treasury Department to the Executive Office of the President established that "The main function of the Bureau is to serve as an agent of the President in coordinating operations and in improving the administrative management of the government."\textsuperscript{37}

Both Wallace and Smith supported "planning" as presidential assessments of the executive branch's structure in light of the political and intellectual character of policy needs, but neither man was well disposed towards the National Resources Planning Board. For Wallace, the Board was too isolated from the organizations it was assessing,\textsuperscript{38} and Harold Smith agreed. Shortly after the Board's Congressional foes had liquidated the Board, Wallace favorably recorded his impressions of a meeting with Smith:

Harold Smith wanted me to know that he thought the planning function of government should rest in the Bureau of the Budget. He felt that the National Resources Planning Board had rather made a mess of things and that planning was not something that could be done in a closet off to one side but was a daily operation of the government. He would have planning conducted in each of the departments running along with the operations of the department and the whole thing finally heading up in the Budget Bureau.\textsuperscript{39}

For Wallace and Smith, presidential planning should not evade the cabinet; instead

\textsuperscript{36}Ibid., 15-18.


\textsuperscript{38}Several of Roosevelt's cabinet members and top administrators, including Wallace, objected to placing a planning board that did not have the power to carry out its plans in the Executive Office of the President. Karl, \textit{Merriam}, 247.

political executives should have sufficient staff help to plan, and the president should have sufficient authority to force a meeting of minds when differences became manifest.

When Bush attempted to use his connections with the armed services to insure continuous funding for academic research by arranging for military funds to be transferred to the National Academy of Sciences, Smith's staff began to consider the proper scope of the Bureau's concerns over post-war research policy. Internal discussions convinced the staff that "the question of the Government's post-war organization and activities in relation to scientific and technological research is of such importance" that the Bureau was behooved to "focus on the administrative and organizational implications," even though in so doing the Bureau "cannot avoid the ideological issue as to the extent of socialization of scientists and inventors." Given Smith's dedication to using the Bureau for presidential management of the executive branch, the staff rightly sensed he would protest the lateral transfer of funds from agencies to a private organization.

When his staff's findings reached Smith, he petitioned President Truman to order the Secretaries of War and Navy to halt their arrangements with the Academy. Even after Truman so ordered, Smith fumed about Bush to Wallace:

Harold Smith was thoroughly alarmed by the fascist-minded proposal of Vannevar Bush, spearheading for the War Department and the Navy to set aside $100 million for research which would not go through regular Congressional channels. The more Smith talked about this, the more alarmed he became and the more I shared his alarm. I like Vannevar Bush, but he really knows nothing about genuine democratic government. He believes in government for scientific aristocracy.

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41 L. W. Hoelscher to Donald Stone, 11 December 1944, in ibid.

42 Kevles, The Physicists, 353. See also "Draft Memorandum to the President," 27 April 1945, in ibid.

43 Wallace Diary, 1 May 1945, 438, my emphasis.
"Government for scientific aristocracy," both Wallace and Smith well knew, hardly characterized Italian and German policy in the 1930s. Their resort to purple prose in private indicated the depth of their fear that the introduction of science policy into government activities could increase executive-branch fragmentation and detract from the president's managerial authority when problems of demobilization and reconversion from military to civilian production could conceivably plunge the nation into another period of widespread unemployment.44

In the hearings before Kilgore and Magnuson, Smith and Wallace were at pains to argue that their views on public administration were consistent with scientific freedom. Smith stressed that a science agency be prepared to address dislocations occasioned by the introduction of the new technologies to follow from the new knowledge:

Such [technological] changes, while bringing general progress, have often disrupted the lives of people... in ways that could have been prevented if the country had been paying attention to the social effects of its material improvements. If the agency responsible for furthering research in the physical sciences ... is also concerned with research in the biological and social sciences, we may be better able to make sure that our material progress is reflected in improvements in the health and welfare of the Nation as a whole.45

For Smith, this combination of duties required an agency with a presidentially appointed administrator, because the president was the official whose electoral interests came closest to "the welfare of the Nation as a whole." But that arrangement, Smith contended, would have no more effect on scientists' freedom in research than Magnuson's:

This issue [of appointing the administrator] has little or nothing to do with how much voice scientists will have in decisions on scientific problems. In

44See Henry A. Wallace, Sixty Million Jobs, (New York: Simon and Schuster, 1945), especially 48-49, for the importance he attached to an appropriately organized science agency for economic policy.

45Hearings, 1945, 97-98.
an agency as large as the proposed foundation, most scientific problems
must be solved by divisions and subdivisions, not by the head. 46

Wallace also underscored the argument, which in Dunn's hands became an appeal to
the cognitively oriented scientists, that an agency under a politically appointed
director would be less likely than an agency under an independent board to interject
itself into laboratory-level affairs. The desire to insulate science from politics was
balderdash to Wallace:

If "politics" ... is understood to mean public administration directed and
guided by the policies established through the elected representatives of the
people, in accordance with our democratic traditions and constitutional
procedures, no agency with public powers and which spends public funds
should be insulated from politics. 47

He concluded by pointing out that the integrity of the research performed in the
regular Federal departments ought to dispel fears that American public administration
would infringe upon "scientific freedom." 48

From Wallace's and Smith's perspective, Kilgore's science agency satisfied
their criteria for an agency that would improve American public policy. Its
administrator, with advice from a board comprised of representatives of other
government agencies that conduct research and leading scientists in private
institutions, would settle jurisdictional issues over where particular research programs
were best pursued and commission studies of how best to cope with particular
technological changes. Whatever the agency administrator could not settle would be
passed upwards to the White House. The agency would thus be a source of ferment
over the proper organization of the executive branch and the powers the executive
should have, but one that was in better touch with the realities of government bureaus
in cabinet departments than the National Resources Planning Board.

46Ibid., 100.

47Hearings, 1945, 143.

48Hearings, 1945, 145.
Magnuson's support from outside the academic science community came from business interests disaffected from the New Deal and government scientists in departmental bureaus. For business executives, opposing the growth of presidential authority over the executive branch kept open the hope that "associative relationships," which Herbert Hoover fostered between the government and private interests and which the Roosevelt administration first over-extended and then abandoned as a way to revive economy, could again be made to work. For officials in or responsible for government research bureaus, keeping the new science agency as distant as possible from the presidency would keep them more safely in charge of bureau affairs. The Magnuson bill recognized the necessity of continued federal support for research but structured the agency to supplement the ongoing activities of businesses and government agencies without creating powers to reform those activities.

The testimony of Richard Dearborn, representing the National Association of Manufacturers (NAM), illustrated how obstructionists to governmental expansion could accept a research agency that had few inter-agency implications. NAM had already denigrated Kilgore’s second bill in language that evoked the executive reorganization debate:

it would concentrate in the hands of a single Government official unprecedented authority .... The effect of the measure is to authorize the complete socialization of personnel devoted to scientific and technical effort.

But Dearborn did not oppose any science agency as unwarranted growth of

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50 Senate Committee on Military Affairs, Scientific Mobilization, includes as an exhibit, 309-311, the text of the National Association of Manufacturers pamphlet "Shall Research be Socialized?"
government; rather, he called for an agency that left the business ecology undisturbed. He declared Magnuson's administrative structure essential because a board of scientists would not represent any "special interests;" Kilgore's patent clause was fatally flawed because "by accepting research funds from the Government they [small businesses] would sacrifice the rights which would enable them to become established and expand." 51 Though Kilgore successfully forced Dearborn to retreat from both these assertions, 52 Dearborn, by equating the dangers of special interests with presidential administration of the executive branch, and by equating public ownership of patents based on government-supported research with denigration of incentives for private entrepreneurship, had tied Kilgore's bill to partisan political sensibilities.

Irving Langmuir, who had a highly successful scientific career at General Electric, 53 endorsed the Magnuson bill on similar grounds to Dearborn. Within General Electric, Kilgore's wartime efforts to influence research policy had evoked partisan rejection—one researcher described Kilgore's patent policy as "a strange extension of the philosophy of the New Deal ... [whereby] everyone unable or unwilling to support himself ... is entitled to support by the industrious, efficient and thrifty majority." 54 Yet Langmuir, amazed at the success of the Manhattan Project 55 and fearful that the ties of Soviet scientists to the Soviet state would prove

51Hearings, 1945, 176.

52Kilgore pointed out the impossibility of avoiding special interests when a science agency funded intra-university programs rather than faculty projects: "I know if I were the head of a big educational institution, I would want to get all the grants for my institution I could, because I would have more confidence in my institution than any other ... or I wouldn't want to be head of it." Kilgore also pointed out that his patent provision could not hurt small businesses without research laboratories capable of winning government grants. Magnuson concurred with Kilgore on the impossibility of insulating a board from special interests. See Ibid., 172-177.


54L. A. Hawkins, Executive Engineer in the Research Laboratory, to E. A. Adler, Law Department, 3 June 1943, copy in OSRD Papers, National Archives, RG 227, Item 13, Box 31.

55GE, in the 1920s and 1930s, had not delved into quantum phenomena or nuclear physics except in conjunction with its work in medical instrumentation. See Wise, Whitney, 250-281.
an effective nexus,\textsuperscript{56} accepted the need to debate the terms of "our own American ways of obtaining equally good results."\textsuperscript{57}

Those ways involved structuring the agency to confine it to supporting work that respected or complemented corporate research traditions. To contract for research with industrial research laboratories, which Langmuir was certain a National Defense Division would want to do because of industrial laboratories' access to heavy equipment and special materials, a science agency would have to respect industrial laboratories' origins and purposes:

Industrial laboratories ... are built and maintained to serve the industries of which they are a part. They are designed to earn a profit and insure a continued existence to the industry .... These laboratories are not organized to hire themselves out to anyone who wants a job done .... [W]e [industrial scientists] do not want to give all advantages to our competitors by giving all [patent] rights to the Government and then having them [sic] make nonexclusive licenses to everyone.\textsuperscript{58}

The price to universities of having government support, Langmuir assumed, would be the end of universities' pursuit of patents as a way to finance research, and an agency controlled by a board that included prominent industrial researchers could support technologically relevant research in universities and keep university activities from slipping into industrial activities. Though not presented with florid ideological language, Langmuir's message was clear. The price of business support for a government programs to fund research was to guarantee that industrial laboratories be the linchpin between new knowledge and new products, regardless of the political needs of the president.

Representatives of government research agencies and cabinet departments—and even interest groups that had been friendly to Roosevelt—echoed the industrialists' concerns with keeping a science agency from expanding the presidency. For

\textsuperscript{56}Langmuir and several other Americans had recently been to the Soviet Union to celebrate the 220th anniversary of the Russian Academy of Sciences. See \textit{Science}, 101 (15 June 1945), 603.

\textsuperscript{57}\textit{Hearings}, 1945, 32.

\textsuperscript{58}\textit{Ibid.}, 34.
government officials from research agencies with close ties to industry, those concerns added up to outright endorsement of the Magnuson bill. Government officials whose agencies did not have such ties and representatives of Roosevelt-friendly interest groups preferred Kilgore’s patent policy, but wanted a board to direct the agency.

Jerome Hunsaker, the chairman of the National Advisory Committee on Aeronautics, praised Magnuson’s administrative structure for emulating NACA’s. NACA’s mission to research the problems of flight, Hunsaker pointed out, "imply all that is known in metallurgy, in chemistry, in technology, in electricity, and aerodynamics." Committee management, he maintained, had balanced and unified the many disciplinary specialists contributing to or seeking support from NACA, and flexibility in patent management enabled NACA’s researchers to prosecute their studies without "patent-consciousness" causing either intra-laboratory competition or excessive occupation with extra-laboratory business affairs. A general science agency, in his view, would do well both to emulate NACA and leave it alone.

H. H. Arnold, the Army Air Force’s Commanding General, likewise commended Magnuson’s administrative structure, though Arnold suggested it be amended "to provide for more appropriate Air Force representation" on the board. And Arnold brought along the Air Force’s patent specialist, who agreed with Langmuir that industrial contractors would not accept government research contracts if the patent rights for any inventions had to be dedicated to the public. The military’s civilian leaders, perhaps wary of committing themselves to a position at a time when much was in flux, avoided comparing the bills. Nevertheless, Secretary of War Patterson suggested that NDRC’s leaders should be trusted to design a permanent science agency:

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59Ibid., 115.
60Ibid., 348.
61Ibid., 344.
I lean pretty heavily on the view of people who are leaders in scientific thought themselves, and I would go to Dr. Bush and I would go to Dr. [Karl] Compton ... and say to them, ‘What form of organization will the scientists themselves best respond to?’ ... [P]resumptively, at any rate, I think they would have the correct solution.\(^62\)

While hardly a principled endorsement of Magnuson’s bill, Patterson had aligned the Army with the bill that had the support of industrial researchers and manufacturers. Standing between the advocates of executive authority and of associationist relationships between government agencies and private institutions were government officials who oversaw research bureaus but did not have strong ties with private firms and interest groups that supported Roosevelt more than the presidency. Like the advocates of executive reorganization, they wanted a science agency to address inequities created by technological change, but like the associationists, they wished to protect the autonomy of departments from presidential oversight. They argued for melding provisions from the two bills to bring about an agency which would influence the relationship between research and economic power but not the structure of American government.

Secretary of Interior Harold Ickes’ testimony before Kilgore and Magnuson reflected a determination to keep non-governmental advisors subordinate to cabinet members and to keep public resources in governmental hands. Unlike Wallace, whose frustration with implementing policies as Secretary of Agriculture led him to broader policy considerations, the vice-presidency, and a desire to run for president, Ickes tried to expand the Interior Department in order to break dependence on local officials for implementation of his department’s policies.\(^63\) Ickes offered vigorous support for Kilgore’s patent provision on the grounds that Magnuson’s opened the

\(^62\)Hearings, 1945, 237. In referring to Bush and Compton as “leaders of scientific thought,” Patterson had mistakenly equated attainment of administrative authority with intellectual accomplishment. This type of mistake was especially irritating to Manhattan Project scientists. See Smith, Peril, 32-33.

way to corrupt dealings between private interests and government officials.\textsuperscript{64}
However, he preferred that a science agency be controlled by a board, but one whose composition was closer to Kilgore’s specifications:

I believe that at least one half and preferably two thirds of the members of the board should be Government officials of Cabinet rank or its approximate equivalent .... The remaining members of the board should be outstanding scientists from outside of the Government.\textsuperscript{65}

The scientists on the board might unite behind pursuing what they considered important research, but should the agency’s administrator perceive an opportunity for presidential initiatives in the results or organization of research, he would first have to submit to the scrutiny of cabinet officials on the board before alerting the White House.

Rolla E. Dyer, the Public Health Service’s representative, echoed Ickes’ concern that a science agency not threaten another agency’s work. The Public Health Service and its fledgling National Institute of Health had been negotiating as early as August 1944 to take over OSRD’s contracts for medical research at war’s end.\textsuperscript{66} Dyer argued for “preserving the scientific integrity and independence” of NIH and opined that NIH already had the statutory authority to pursue the program in medical research that Bush proposed the NRF pursue in \textit{Science the Endless Frontier}.\textsuperscript{67} The Public Health Service’s external supporters explicitly endorsed Ickes’ sentiments:

1. That proposed legislation should be amended to include statements to the effect that autonomy in the development and conduct of research should be maintained by those governmental agencies now engaged in such activities.
2. That there should be governmental representation on such boards and

\textsuperscript{64}Hearings, 1945, 344.

\textsuperscript{65}Ibid.


\textsuperscript{67}Hearings, 1945, 514-515.
advisory committees as may be set up in connection with the proposed National Research Foundation.\textsuperscript{68}

Lewis Hines of the American Federation of Labor and Russell Smith of the National Farmers Union also supported Kilgore's patent clause as the way to make patents created in the course of government-financed research serve to promote competition in the economy.\textsuperscript{69} Yet while both groups had been supporters of Roosevelt and both shared Wallace's and Smith's desire to use a science agency to address the social problems of technological innovation, both had opposed the executive reorganization bills out of fear that their favored departments would be hurt or their favored bureaus would be transferred to departments where they would face new pressures.\textsuperscript{70} Like Ickes, they preferred a powerful board with representation that reflected their interests to a powerful administrator who would someday be appointed by a president they did not support.\textsuperscript{71}

Within the overall consensus that university science should continue to receive government support lurked tensions over a science agency's place in the executive branch. Henry Wallace and Harold Smith believed that the president would increasingly need to decide issues that transcended departmental and regional boundaries. They supported a science agency where a presidential appointee would report to the Executive Office of the President on the basis of advice from a board of prestigious scientists and representatives of cabinet departments. Such an agency would arm the presidency with information on the economic, social, and military implications of new knowledge and would generate initiatives for government programs to cope with these implications. Irving Langmuir, Richard Dearborn, and

\textsuperscript{68}National Advisory Health Council and National Advisory Cancer Council of the Public Health Service, "Proposals for a National Research Foundation," \textit{Science}, 102 (23 November 1945), 541.

\textsuperscript{69}Hearings, 1945, 117-121 for Hines, 122-123 for Smith.

\textsuperscript{70}Polenberg, Reorganizing, 84-87, 105, 112.

\textsuperscript{71}Hines stressed the need to appoint heads of government research to the board; Smith argued that a "fruitful relationship between laymen and scientists would be consummated if the board included two representatives each from business, labor, and agriculture along with six scientists. 118-119. Hearings, 1945, 125 for Smith.
Henry Arnold looked forward to more university research that would better complement the efforts of the military and industry to develop weapons and products. They supported a science agency where a board of leading university and industrial researchers would set research strategy, the Congress and presidency would determine its appropriation, and a non-partisan administrator would keep track of the money. Such an agency would favor institutions with the best proposals to organize researchers to meet the board’s emphases, and the agency would deal case-by-case with problems created when differences in the professional interests of academic and industrial scientists threatened their collegial pursuit of their common scientific interests. Harold Ickes, Rolla Dyer, and labor and agricultural interest groups wanted the cabinet departments to expand their its resources and improve their policies. They supported a science agency where a board of cabinet representatives and non-governmental scientists would set research strategy for a non-partisan administrator to carry out. Such an agency would regulate the private use of publicly financed research and empower departments to work out any policy initiatives inspired by new knowledge.

The question facing Kilgore and Magnuson at the end of the hearings was whether the hearings had laid a foundation for compromise or further dispute. Two factors favored compromise. First, they would more easily get Congress to act on science-agency legislation while memories of wartime research successes were fresh. Second, both senators were Democrats, which freed them from partisan posturing as a motive for hardening their positions. Third, with Democratic majorities in both houses of Congress and a Democratic president, there would be no partisan obstacle to a consensus Kilgore-Magnuson bill. It remained an open issue, however, whether prominent scientists would continue to debate the proper structure for the new agency in a way that undermined Congressional confidence in Magnuson’s and Kilgore’s drafting skills. It likewise remained open whether the Democrats would unite rather than fragment as they had following their landslide victory in 1936.
Political Stalemate and the Demise of Initial Ideals

Well before the end of the legislative hearings, the scientists began polemical public exchanges that added a partisan character to their differences the principles for organizing research and forming specialties. On November 15, 43 individuals calling themselves the Committee Supporting the Bush Report wrote an open letter to President Truman to persuade him to endorse the Magnuson bill.\(^1\) The composition of the committee speaks to the core of Bush's support. There were five university presidents plus Warren Weaver, director of the Rockefeller Foundation's Natural Science Division; eleven medical school faculty or administrators, but only three biologists outside of medicine, two of whom (Detlev Bronk and Robert Griggs) were published critics of biology's relationship to applied science; six engineers but only three physicists, two of whom (Lee DuBridge and John Tate) had worked on radar and none on the Manhattan Project.\(^2\) Even granting that the precise make-up of the committee would have depended on who happened to be easily reached, the preponderance of academic administrators, engineers, and physicians over research scientists with disciplinary loyalties was unmistakable.

The letter backfired on its authors for two reasons. First, its content contained nothing that had not already been well articulated, and Truman responded by referring the committee to Harold Smith's testimony for Truman's views on science legislation.\(^3\) Second, the letter alienated scientists who gave any credence to Kilgore's efforts. An anonymous introduction to the letter's publication characterized


\(^2\) The letter was published as "Pending Legislation for Federal Aid to Science," Science, 102 (November 30, 1945), 545-548. Appended to the letter was a statement from Robert Chambers and J. S. Nicholas, representing the Union of American Biologists and the American Biological Society, approving the letter contingent on "having realized the contemplated plans to include the biological sciences in a division of basic science separate from medical research."

\(^3\) Truman's response was published in Howard Meyerhoff, "Science Legislation and the Holiday Recess," Science, 103 (January 4, 1946), 10-11.
the two bills as "start[ing] from wholly different premises with respect to the purposes of such a foundation and therefore they present widely divergent points of view and completely different organizations." The letter proper then argued:

Most of the scientists called to testify ... stated that they were in favor of the form of organization and other features provided in the Magnuson Bill. ... We ... bespeak your further, favorable consideration of the Bush-Magnuson plan which the scientists desire and will support.76

By implication, any scientist who considered the Kilgore bill just a workable starting point for the creation of a science agency lacked loyalty to his calling.

Harlow Shapley and Harold Urey, the most prestigious of the scientists to endorse provisions of the Kilgore bill, took up the cause for producing a timely compromise that would take advantage of science's popularity and take into account legitimate political concerns. Toning down sentiment for a dramatic statement of opposition to the Committee Supporting the Bush Report,77 Shapley and Urey recruited scientists for a Committee for a National Science Foundation with a letter, circulated under their names, calling for "research in all fields of fundamental scientific inquiry relevant to the national interest without arbitrary exclusion of any area," a policy that "scientific findings from federally financed research should receive publication and should be dedicated to the welfare of the public," and an agency administered by an unspecified compromise between Kilgore's and Magnuson's proposals. The letter received 200 endorsements including such luminaries who did not sign the Bush Committee letter as Oppenheimer, Fermi, Einstein, and the biologist Theodor Dobzhansky.

The Shapley-Urey letter's language played on the fears of biologists and others that their minimal involvement in war research would carry over to peacetime under a NDRC-style agency and turned Kilgore's patent clause into a generic statement of

75 Committee Supporting the Bush Report, "Pending," 545.

76 Ibid., 545-546.

77 For a proposal to make a dramatic statement, see Paul Webbink to Harlow Shapley, November 29, 1945, Shapley Papers, Harvard University, HUG 4773.10 Box 11-E.
widely shared values among academic scientists. However, the letter endorsed no bill and did not mention the letter from the Committee Supporting the Bush Report. It thus demonstrated, at a general level, the attractiveness of the Kilgore bill to scientists, especially to scientists of renown within particular disciplines, when bill's philosophy was presented in pleasing words to scientists and without Kilgore's name.\(^7\)

Lee DuBridge, however, had no trouble perceiving the Shapley-Urey letter's political purposes. He declined to sign, he told Shapley, because "the purpose of your Committee is to oppose the Committee Supporting the Bush Report." DuBridge went on to express his regret and confusion over Shapley's actions.

I think it is most unfortunate that scientists are splitting in this way. ... Our committee stated certain general principles which we believe are quite important and which are worth fighting for. It is certainly not the thought of our committee that a compromise wording of the Magnuson or Kilgore bill ... would be quite acceptable. Why would it not be better for your proposed committee and the Committee Supporting the Bush Report to get together and talk this matter over before one tries to line the scientists up in opposing camps? This, it seems to me, is the surest way of destroying the possibility of having a reasonable science foundation bill passed in the near future.\(^7\)

Shapley also had well known convictions for which he would fight. When OSRD staff realized that Shapley would aid Kilgore, they had no trouble assessing his position:

[Shapley] fears industrial research laboratories ... will starve the colleges and universities of scientific talent by competitive bidding. He feels these laboratories will work for profitable applications of scientific effort rather than basic research.\(^8\)

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\(^7\)Hodes, Precedents, 137 speculates that the Shapley-Urey letter's vague language attracted signatories who did not recognize its polemical uses. Even if true, this argument misses the point that the letter's value lay in showing that Kilgore's ideas were appealing when separated from his name.

\(^7\)DuBridge to Shapley, December 14, 1945, Shapley Papers, Harvard University Archives, HUG 4773.10, Box 11-E.

\(^8\)Memorandum from John Teeter to Carroll Wilson, August 23, 1945, General Records OSRD, National Archives, RG 227, Records of the Administrative Office, Item 13, Box 53. See also Memorandum from Wilson to Bush, August 3, 1945 in the same place. They apparently believed that
To DuBridge, Shapley tried to turn the tables on who was saving and who endangering NSF legislation: "Urey and I seek only to save the National Science Foundation which seemed to be seriously jeopardized by the (to us and many others) astonishing statement which you signed." 81

DuBridge and Shapley had reached the bedrock of disagreement implied by the perspectives of Darrow and Haskins towards the relationship of scientists to political power. DuBridge, following Darrow's statement, felt that the Committee Supporting the Bush Report was stating principles that were universal to the practice of science and on which all scientists should agree in order to keep their politics out of scientific affairs. Shapley, in DuBridge's view, was committing the error envisioned by Darrow of dividing scientists over the political means to agreed-on scientific ends. But Shapley saw himself, following Haskins, as constructively engaging politics in order to create the best conditions for supporting the most conceptually significant research. DuBridge, in Shapley's view, was working to keep control of research in private hands to the detriment of the best, most publicly significant research.

Shapley and DuBridge did agree that face-to-face discussions were in order and that a united scientific community would help get a bill passed. Negotiations did take place in late December among Magnuson, Kilgore, their staffs, members of the Committee Supporting the Bush Report and the Committee for a National Science Foundation, and some Senatorial mediators. The new bill expanded the number of programmatic internal divisions from three to six to quell the fears over the status of

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Shapley would be a healthy influence on Kilgore and no threat to Magnuson because Shapley would not care enough about the patent clause to risk a political stalemate. Their view may have been different had they known Shapley had spoke of the NSF as "the new attack against entrenched industry" to Kirtley Mather, a geologist at Harvard and president of the American Association of Scientific Workers. Shapley to Mather, August 17, 1946, Mather Papers, Harvard University Archives, HUG 4559.500.4, Correspondence with Societies and Organizations, Box A-AAAS, AAAS Executive Committee Minutes and Letter Folder.

81Shapley to DuBridge, December 19, 1945, Shapley Papers, Box 11-E.
disciplines whose scientists were less involved in war research. The new bill accommodated both Kilgore's desire for better management of the executive branch and Bush's for broadening NDRC's approach to research strategy by creating two boards with different members and powers under a presidentially appointed director. One board was identical in composition to Magnuson's and explicitly entitled to meet, discuss, and report on its own initiative so that the director could not ignore the research strategy it recommended. The second was an Interdepartmental Committee on Science, consisting of representatives of all government agencies involved in scientific research with the NSF director as chairman, for presidentially sanctioned considerations of science policy within the government. The patent clause was Kilgore's with an enormous loophole—all patents stemming from agency-supported research would be dedicated to the public except in the case of for-profit contractors that brought prior experience in a field to agency-supported research. Since no for-profit firm would seem to want agency support (or even be competitive in pursuing agency support) in areas in which it had no prior experience, the loophole kept open prospects for the agency to support programs built on common interests among university and industrial researchers.

Kilgore and Magnuson jointly introduced their new bill, S. 1850, in February 1946. Both the Committee on Military Affairs, on which Kilgore sat, and the Commerce Committee, on which Magnuson sat, favorably reported S. 1850 out to the full Senate for action, and it came up for action on July 1. However, the Committee Supporting the Bush Report gave S. 1850 only a grudging endorsement:

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82Section 3(b). S. 1850 had Divisions of Mathematical and Physical Sciences, Biological Sciences, Health and Medical Sciences, National Defense, Engineering and Technology, and Social Sciences. Social Sciences was restricted to studies of the social impact of science and technology until it produced a report setting forth its needs in general.

83Hodes, 139-40. The text of the bill is in Science, 103 (February 22, 1946), 225-230. The losers in this compromise were the advocates of expanded powers for cabinet departments. They were neither insulated from the presidency nor enabled to regulate the private use of publicly supported research results.

84Harvey Flaumenhaft, The Struggle to Create a National Science Foundation, Master's thesis, Political Science Department, University of Chicago, 1962, 45-46.
"protracted delay or failure to enact this legislation would be far more prejudicial to the public interest than the inclusion of the provisions objected to." 85 And the Republican members of the Military Affairs Committee (with the exception of Princeton alumnus H. Alexander Smith (NJ)) issued a minority report that denounced S. 1850 in the inflammatory language last heard in the debates over executive reorganization in Roosevelt’s second term:

Obviously, the Administrator [of the NSF] … will become one of the most powerful men in the Government and in the country. The bill proposes to add another large agency to the Government structure. Another large sector of our national economy would come under the centralization, control, and supervision of Washington. Another field of State responsibility, education and learning, would be brought under the domination of the Federal Government. Another huge expenditure of two hundred to three hundred million dollars per year would be added to our already dangerously unbalanced budget.

The bill is a link in the chain to bind us into the totalitarian society of the planned state.86

While the Republicans supported this thesis by assuming that worst-case scenarios and abusive interpretations of various clauses were both certainties,87 the rhetoric yet drew attention to genuine continuities between a science agency and the debate over a presidential planning board. Was an agency director empowered to report to the president on the government’s research activities more likely to promote better ways to structure presidential decision-making, or intrusive paper work that at best slowed agency work and at worst led to misguided reforms? Did the more traditional and acceptable rationale for executive reorganization—to economize on

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86 United States Senate Committee on Military Affairs, National Science Foundation: Report Pursuant to S. 1850, 79th Congress, report 1136, part 2, (Washington: GPO, 1946), 1 and 3, respectively.

87 At one point, the report is self-contradictory; in successive paragraphs the Republicans complain about the administrator possessing excessive discretionary power and a lack of flexibility in the bill’s provisions. Ibid., 4.
government operations—apply to research, where, as The Committee Supporting the Bush Report argued, "proliferation of interest and activity, together with a high degree of institutional and individual freedom and responsibility, is desirable." Would the government, by claiming ownership and then issuing non-exclusive licenses for the patents produced by not-for-profit institutions in the course of agency-supported research, liberate small businesses to adopt new processes and manufacture new products or discourage the introduction of technical novelties into the economy by reducing the rewards to a firm that had to undertake significant, ancillary development work to make use of the patent?

The effect of equating a science agency with executive reform was telling. The Senate soundly voted down a package of amendments that amounted to substituting the original Magnuson bill for S. 1850. The vote was 39-24 with Democrats opposing the amendment 33-5 and Republicans supporting it 19-5. But when the amendments’ proposer, H. Alexander Smith (R, NJ), broke his amendment into separate parts for individual votes, he exposed the fault lines in the Democratic Party. An amendment to place a board in charge of the agency was barely defeated 35-34, and an amendment barring the social sciences, which had been most prominent in the New Deal planning board, from the science agency passed 46-26.

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Science. 102 (November 30, 1945) p. 547.

Committee on Military Affairs, National, part 2, 8.

Robert La Follette, the Wisconsin Progressive, was the 39th opponent. The maverick Democrats were four southerners, Byrd (Va), McClellan (Ark), O’Daniel (Tex), and Swift (Ala), plus Walsh (Mass). The Republican mavericks showed no geographical bias. They were Aiken (Vi), Capper (Ka), Ferguson (Mich), Morse (Ore), and Young (ND).

Democrats plus La Follette opposed the amendment 29-11; joining the five Democratic dissenters to the first amendment were Eastland (Miss), George (Ga), Hoey (NC), Russell (Ga), and Stewart (Tenn), plus Gerry (RI). Altogether, voters from Confederate states supported the amendment 9-7.

Democrats plus La Follette opposed the amendment 22-21 with senators from the old Confederacy supporting the amendment 13-2. The Republican dissenters varied little in number and composition throughout the amendment votes. Ferguson, Knowland (Cal), Langer (ND), and Young voted with the Democrats on some occasions, while Aiken and Morse consistently supported the Kilgore-Magnison bill. Capper never again did so after the first amendment vote and even voted against the final bill; perhaps he did not understand the issue on the first vote.
Only Smith's effort to undo S. 1850's patent clause unified the Democrats. The bill thus amended passed 48-18 with Democrats favoring the measure 37-2 and Republicans opposing it 16-10. In the House of Representatives, which had been more hostile than the Senate to executive reorganization, just the presence of a House bill similar to Magnuson's original was enough to paralyze the House committee to which S.1850 was referred after it passed the Senate. Science agency legislation thus died in the 79th Congress for want of House action.

Shapley thought the demise of legislation in the 79th Congress was due to a lack of organized discussion among scientists about a science agency. Cornell president E.E. Day told him:

It seems to me quite clear that unless we are more successful at effecting a meeting of minds this year than we were last, there is virtually no chance at all of getting Science Foundation legislation through the Congress. I know from contacts I had with some of the senators and representatives last year that it was the failure of the scientists to come to agreement that ultimately prevented the passage of any bill.

Right after the Congressional elections, Shapley wrote Isaiah Bowman, who had chaired the Committee Supporting the Bush Report, to argue that a renewed attempt to effect a meeting of minds was essential in light of the Navy's move to support basic science and the specter that "those who were worried about domination of

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93The vote was 41-31 with Democrats plus La Follette opposing the amendment 37-6 and Republicans favoring it 25-4.

94La Follette was the 48th supporter. Wiley (Wis) and Donnell (Missouri) joined Smith and Hart, whose states contained Ivy League schools, as the extra Republican mavericks. The Democratic dissenters were McKellar (Tenn) and O'Daniel (Tex).

95Polenberg, Reorganizing, 162-180.

96At the behest of John Teeter, Bush's ally on Magnuson's staff, Rep. Wilbur Milis introduced a bill like Magnuson's original and held two days of hearings at which the only witnesses were representatives of the Public Health Services, NACA, the War, Navy, and Commerce Departments, the National Association of Manufacturers, and six members of the Committee Supporting the Bush Report. All, except for the representative of the Commerce Department, spoke approvingly of Mills' bill; several explicitly compared it favorably to S. 1850. See Flaumenhaft, 54-58.

97Day to Shapley, December 13, 1946, Shapley Papers, Box 11-E.
freedom in American science by the great industries, can now worry about domination by the military." Shapley proposed forming "a small steering committee composed of Bowman, Urey, Shapley, [and] E. E. Day of Cornell, to employ some effective executive officer who would do the work [of lobbying Congress]."\textsuperscript{98}

Bowman's response was anemic. He approved in principle of a steering committee, but he declined to participate, and he spoke of the Navy's support of research as adding complexity, not urgency, to the creation of a science agency.

Shapley did not even receive enthusiastic responses from those he could expect to be allies. Talcott Parsons, the Harvard sociologist, noted the Republican majorities in both houses of Congress and informed Shapley.

The consensus [of the SSRC committee on science legislation] was that it would be politically inadvisable at this time to press for a separate social science division in the Foundation, but it was most important to try to get a formula which would not close the door on support of social science work. The approach which got most favor was in terms of problems such as public health and national defense on the understanding that the Foundation would be free to mobilize any disciplines which could contribute knowledge to the solution of these ranges of problems.\textsuperscript{99}

If the social scientists were preparing to justify inclusion in a science agency on the basis of their technical relevance rather than their intellectual disposition, Shapley might have realized he was in danger of being obstructionist by pushing for the old S. 1850.

By the end of November, Shapley could tick off an impressive list of people he had contacted, "but in spite of all this chatter, I don't see exactly who is going to be the leader or organizer or financier."\textsuperscript{100} Despite these polite brush-offs from potential allies and foes, Shapley persisted in seeking a scientists' lobby through the American Association for the Advancement of Science, where he was to become

\textsuperscript{98}Shapley to Bowman, November 6, 1946, attached to Bowman to Bush, November 13, 1946, Bush Papers, Box 13, Bowman file.

\textsuperscript{99}Parsons to Shapley, November 20, 1946, Shapley Papers Box 11-E. Parson's emphasis.

\textsuperscript{100}Shapley to Mather, November 26, 1946. Mather Papers, Correspondence with Societies and Organization. Box A-AAAS, Folder on Inter-society Committee on Science Legislation.
president and where he had allies in important positions: Howard Meyerhoff, a professor of geology at Smith who reported on the legislative debate in Science, and Kirtley Mather, the president of the American Association of Scientific Workers, a professor of geology at Harvard, and longstanding member of the executive board of the AAAS. In informal meetings during the AAAS annual convention, they were unable to get agreement to a AAAS-appointed committee to manage the lobbying effort, but consensus was reached on forming an Inter-society Committee for a National Science Foundation with the AAAS responsible only for initiating the necessary meetings. Mather was appointed to make the necessary arrangements, the committee was formed, it took straw polls showing a majority of scientists favored an agency with a presidentially-appointed director and a division for social sciences, and presented the results to the relevant Congressional committees. None of which mattered because the committee was born irrelevant.

None of this activity mattered. Senator Smith of NJ had introduced a new version of the original Magnuson bill. In declaring himself unable to attend the committee's organizational meeting, Smith told Mather: "In introducing the revised bill ... we have consulted with Drs. Bush, Conant, Smythe, and other scientists, as well as with representatives of the Army and Navy and the Research Department heads of some of our principal Eastern industrial concerns." Bush knew from the difficulties of establishing the Atomic Energy Commission that Shapley's fear of

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101 "Memorandum," from W. Parker Anslow to CSBR and others, January  , 1947. Copy in Mather Papers. Correspondence with Societies and Organization. Box A-AAA , Folder on Inter-society Committee on Science Legislation. Conant's role in the AAAS positioning on science legislation is murky. As AAAS and Harvard president, he had the power to limit or reprimand the use that Shapley, Mather, and Meyerhoff were making of the AAAS and Science, but did nothing. Shapley told Robert Chamber, a NYU biologist, "The general feeling here is that Conant is not at all passionate about the National Science Foundation legislation. I believe he is a bit complicated because of his close associations with Bush, Jewett, Bowman and others. In the AAAS he seems to be leaving matters to Mather. Shapley to Chamber, December 12, 1946, Shapley Papers, Box 11-E.


103 Smith to Mather, February 20, 1947, Mather Papers. Correspondence with Societies and Organization. Box A-A-AAS, Folder on Inter-society Committee on Science Legislation.
military domination of science was potent,\footnote{Bowman had sent Bush copies of his correspondence with Shapley. Bowman to Bush, November 13, 1946, Bush Papers, Box 13, Bowman file.} and he was preparing to defuse it. He had requested the Secretary of the Navy Forrestal to issue a statement of its willingness to transfer its program in basic research to a new science agency.\footnote{Bush to Forrestal, December 11, 1946, OSRD, RG 227, Records of the Administrative Office, Item 13, Box 55.} Even though his letter aroused resistance within the Navy, Bush believed he would get his way by making "some strong statements later on in Congress" that would resonate with Congressional "reluctance to leave things in the hands of the military that are not their direct concern."\footnote{Bush to K. Compton, December 30, 1946, Bush Papers, Box 26, Compton file.} The Inter-society Committee, in the absence of a credible claim that Bush's science agency was an endorsement of continued military patronage, could do no more than endorse a bill with different provisions than a majority of its members wanted.

Bush did get the bill he wanted from the 80th Congress, but not with a veto-proof majority. Truman vetoed the bill at the urging of Bureau of Budget personnel, and when Truman won the 1948 presidential election, Bush had lost his last chance for a foundation structured to support local initiatives in the reorganization of university research to meet technological opportunities that served the public welfare, broadly construed. Truman's rationale for vetoing the 80th Congress' bill stressed the impropriety of allowing government funds to be spent without direct presidential oversight of the responsible officials,\footnote{Hodes, Precedents, 151.} not a presidential need for a public official charged with acquiring a government-wide view of research and advising the president on needs, strengths, and the possibility of national initiatives. In the interim, Truman had created by executive order the President's Scientific Research Board, with John Steelman as chairman, and the board had set out to produce a report on government-
wide science activities. His activities served the function that Kilgore envisioned the NSF administrator carrying out within the executive branch; for Truman, the creation of a NSF was thus no longer a matter of political urgency (so long as he felt he had adequate oversight of the military's support of research). The legislation Truman signed into law to create a National Science Foundation in 1950 provided for agency administration by a presidentially appointed, Congressionally approved director, but the first director, Alan Waterman, with the board's support, resisted or deflected what demand there was to use the NSF to evaluate and create government-wide policies.  

Conclusion

Through a series of incorrect estimates of influence and intentions and electoral contingencies that nobody could have foreseen, the National Science Foundation ended up nothing like the agency either Bush or Kilgore wanted. Bush had over-estimated the relevance of his experiences as researcher and research administration for national scientific communities. Scientists generally would not concede him the authority to design a postwar, long-term science agency on their behalf. A foundation that would operate like NDRC on a broader scale seemed natural to Bush, given the continuity between his prewar and wartime approach to developing university research institutes, but struck scientists accustomed to pursuing cognitive challenges as extending into peacetime a framework that was appropriate for war. The efforts to portray his proposed NRF as the scientists' bill backfired when scientists of the stature of Dunn, Shapley, and Urey questioned (with varying degrees of explicitness) where their sciences and their approach to research strategy would stand in such an agency. Kilgore had over-estimated the relevance of his frustration at the pace of industrial war mobilization. Not even his fellow Democrats would

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\textsuperscript{10a}President's Scientific Research Board, Science and Public Policy, (Washington: GPO, 1947).

grant him the authority to draft legislation for a postwar science agency on their behalf. His assumption that NDRC officials were negotiating towards compromise legislation backfired when Magnuson sponsored legislation that matched Bush’s original ideals.

On two occasions, Bush elected not to compromise on his ideals to take advantage of politically propitious moments. In 1946, something close to the Kilgore-Magnuson compromise bill (S.1850) could have become law had Bush and his supporters not used the House of Representatives to revive the original Magnuson bill. S. 1850 did contain more of Kilgore’s politics than Magnuson’s—the director of the NSF would have been central to executive-branch considerations of government research and the definition of technologies that served the public interest—and significant concessions to Bush’s critics in the scientific community—in place of Bush’s three broad sub-divisions into defense research, medical research, and physical sciences were more and more specialized sub-divisions with titles that echoed the disciplinary structure of university science. But by establishing two advisory boards for the director, S. 1850 separated the NSF’s service to the presidency from its support of research in non-federal institutions and provided the NDRC-like board to advise on research patronage with the means to wage political war on a director that ignored its wishes. And by adding a major loophole to Kilgore’s patent provision, S-1850 left open the possibility of the NSF brokering constructive relationships among university and industrial researchers. In 1947, with Republican majorities in both houses of Congress, a foundation with no government-wide role and three broad sub-divisions could have been created if Bush and his political sponsors would have agreed to the S-1850 administrative structure to preserve nominal presidential prerogatives and managerial discipline in the event of agency improprieties.

After Truman’s veto, Bush ceased participating in (though not thinking about) governmental affairs. That was the proper personal decision—working towards a legislative framework that satisfied enough interests to be politically viable did not sit
well with his mental constitution—\textsuperscript{110} but never again has anyone acquired the stature to shake cabinet departments with statements on the scope and administration of basic research. The foundation that was established after the veto had to develop in the interstices of other government programs. The Public Health Service built on the grants it took over from OSRD’s Committee on Medical Research;\textsuperscript{111} today, any argument to subsume the National Institutes of Health under the NSF seems \textit{prima facia} absurd. The Atomic Energy Commission took over nuclear sciences, and today the Department of Energy remains the largest supporter of research using or into particle accelerators. The Office of Naval Research stepped into the funding of general academic science. Over the decades, the NSF has grown—in part because Navy advocates of more applied or in-house research narrowed ONR’s horizons long before Congress during the Vietnam War came to doubt the appropriateness of military funding for not essentially military activities.\textsuperscript{112} But the Defense Department continued to loom large as a supporter of scientific research,\textsuperscript{113} and the effect on NSF of taking responsibility for ONR activities was incremental, not defining, as it would have been had most of ONR work been transferred to a new agency in 1947. Given the level of support with which the NSF would start, debating the number and breadth of its internal divisions was an academic exercise in the pejorative sense.\textsuperscript{114}

\textsuperscript{110}Kevles, \textit{The Physicists}, 363.

\textsuperscript{111}Strickland, \textit{Politics}.


CHAPTER 12:

SCIENCE POLICY AND THE ENDURANCE OF FEDERALIST CONFLICTS

Everyone has local origins. But not everyone’s sensibilities in regard to the organization of research developed within the locale of a research university. And not even all American university scientists’ sensibilities in regard to the organization of research developed within the locale of a research university that made a virtue of differentiating itself at a university-wide level from others. Though more case studies would undoubtedly show they were not unique, the MIT-centered sensibilities that Bush and his NDRC colleagues brought to national policy embodied a distinctive set of experiences from which to fashion principles. In a political culture where carefully crafted laws, not reasonable public officials, are supposed to be the guarantors of enlightened government, the local origins of even deservedly influential research administrators could not be a dominating perspective in legislation to establish a science agency. Too many prominent scientists had different sensibilities, and too many powerful politicians disagreed over the source of governmental problems.

The political stalemate over creating a National Science Foundation established several "facts of life" for the subsequent practice of science policy. First, the stalemate resulted in the separation of science advice to the president from government support for scientific research in non-federal institutions. Truman independently proceeded to establish his own science advisory apparatus, and there was no way NSF could quickly build up a program that would provide its director with the authority to participate in policy discussions with officials in the Executive
Office of the President. Second, and partly stemming from the first, the stalemate left the world as safe as previously for cabinet-department research bureaus and independent agencies to enter into the support of extra-mural research. There would be no first-among-equals research agency with a presidentially appointed director whose job satisfaction would depend on feeding the president analyses of the influence of government research bureaus on the direction of research and the development of technology. Third, and partly stemming from the second, the stalemate left no principal arbiter of the relationship of non-federal research institutions to federal patronage. There would be multiple sources of funds for research, and scientists could shop for a patron whose needs fit with the intra-university working relations they desired.

Institutional fragmentation, intellectual divisions, and varying power relations between local or private and national authorities thus became characteristic of American science policy. Scientific insights certainly bring clarity, precision, and a durable consensus to human considerations of natural phenomena, but little was made clear, precise, and durable at the formal initiation of national science policy in the United States. However noteworthy the particular accomplishments or failings achieved through government use and support of science in the United States, I cannot become excited by suggestions that the end of political conflict as previously known, dystopian fears, or the creation of incompatible choices of nearly apocalyptic

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significance could be products of science policy. With so little established in 1945-1950 as to which agency should fund what kind of research, which institutions with what kind of relationships should be the employers of researchers carrying out government-funded programs, and who should use what means to turn research results or prospects into policy initiatives, there is only rhetorical value in claiming that American science policy has become lost at the frontier. We cannot lose what we never had, and we never had a timely, politically legitimate, and clear foundation for what a science agency should be and do.

A more fruitful line of inquiry is to ask how science policy has fit into American political traditions. More political scientists than I have likely read have produced analyses indicating that in American federalism, tangled webs or missing links among voters' behavior, partisan rhetoric, politicians' powers, and policy implementation have made and continue to make concerted government action like the motion of a massive object—difficult to initiate and difficult to stop once started. If

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*Deborah Shapley and Rustum Roy, *Lost at the Frontier: U.S. Science and Technology Policy Adrift*. (Philadelphia: ISI Press, 1985). But the rhetorical value of this work is real; I concur with their claim that *Science the Endless Frontier* has been more cited than read, and that Bush viewed science as part of an ecology of interacting parts.

one accepts that negotiation and compromise predominate over calculated fiat in the making of government policy at the national level, what was it about the activity of science, where calculation can lead to a consensus that becomes the truth of textbooks, that made science policy fit into the American framework of government? And what are the consequences for the health of American government?

Don Price has most clearly recognized and best addressed these questions. As an advocate of presidential authority, Price has framed the problem as a paradox based on well-grounded historiography. Between roughly 1880 and 1936 the sciences (natural and social) helped make it intellectually possible for humans to manage (and sometimes imagine) technologies that expanded markets or were advantageous in ever larger markets, and the expansion of markets beyond the authorities of local officials fueled a political demand for building the national government’s administrative powers. But the sciences did not then and have not since World War II brought an order to governmental affairs commensurate to corporate affairs, even though expansion in the scale and complexity of economic and social affairs kept up the political demand for national power.

The cause, in Price’s view, of science policy’s impotence is cultural. Scientists, in building their relationship to political power, have inherited and adapted a framework designed in the 18th and early 19th centuries for the relationship of clergy to political power. To achieve freedom of religious belief from political power while using theological justifications for the regulation of public behavior, the

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Bill Clinton, and all the smart people in their circles ignore or fail to recognize the structural elements to Reagan’s success).

Price worked in the Bureau of the Budget under Harold Smith and drafted the memoranda that Smith used to convince Truman not to accept science-agency legislation that did not provide for a presidentially appointed director. Kevles, The Physicists, 356-361.

architects of American government took to writing detailed legislation for specific public behaviors. At the price of the slow progress intrinsic to case-by-case legislation, the nation was able to tap the moral insights of the theologically trained without building executive power that could ever threaten religious thought. Scientists in the 20th century have shared the shoes of the clergy, substituting the notion of "pure science" for religious belief in freedom from political power and substituting "applied science" for theological justification in the regulation of public behavior. Prospects for improvement in the United States' ability to govern itself lie in downplaying fears of political intrusion into intellectual life, and lowering standards of presidential accountability to Congressional review of a president's broad policy principles. Then scientists will move effectively across the "spectrum from truth to power"\(^\text{5}\) to create an executive branch that is organized so that a president could exercise authority over diverging agencies.

The analogy between scientists and clerics with respect to their relationship to political power is elegant, but still an analogy without an empirical foundation. The upshot of the history presented here is that science policy enforces the problems of American governance because scientists in the United States have harbored socially divisive impulses that played into traditional political struggles over the separation of powers at the national level and between national and local levels. Better governance may lie in a more nuanced understanding of the differences among scientists and their research institutions and a differentiation in the structure and function of science agencies.

From the time research became part of the duty and calling of an academic career in an American university of stature, academic scientists and their administrative superiors have had to struggle with contrasting possibilities for harnessing research to institutional impulses. Research could be used to judge the quality of a university's specialists in comparison to similarly trained peers at other universities. So understood, research could be a tool for raising the standards for a

\(^{5}\)Price, Scientific Estate, 120-162.
university’s faculty but also a spur to intra-university competition for resources among faculty with different training and research interests. Alternatively, research could be used to build collegiality among specialists within a university. So understood, research could be a tool for strengthening solidarity and balancing breadth and unity in students’ educations, but could also pull a university’s scientists out of the competitive mainstream.

Cognitive structures, by making phenomena and experiments intellectually manageable (and thus teachable) through the manipulation of concepts, have been the component of science that specialists trained at different universities have held in common. Perceptiveness in demonstrating the strength of a cognitive structure, in discovering phenomena that could not be accounted for by extant cognitive structures, and in discovering new cognitive structures have been the major criteria for judging scientists. Arthur Noyes and William Walker both wished MIT to make a policy of boosting its competitive standing with European universities by building departmental laboratories that would pursue cognitively informed research (and promote the researchers.) Unfortunately for MIT President Richard Maclaurin and their MIT colleagues, Noyes and Walker had different criteria for judging the value of cognitive structures in chemistry and were unwilling to be less than the dominant intellect within a Chemistry Department that included the other.

The acquisition of technical skills—by requiring scientists to use instrumentation or mathematical techniques that were developed within and taught for another discipline or to use instrumentation also appropriate for investigating phenomena at the limits of the explanatory power of another discipline’s cognitive structure—have been the component of science that different specialists in the same university have shared. Appreciation of the cleverness, flexibility, and applicability of technical skills across cognitively defined fields have been the source of collegiality through research at a university-wide level. William Sedgwick represented this perspective at MIT, concentrating on the biology of bacteria and reaching out to civil and sanitary engineering, chemistry, preventive medicine, and statistics to form "public health science." Unfortunately for MIT, his efforts ended in pulling MIT out
of the mainstream of both public health and biology when the Rockefeller boards started financing public health institutes at medical schools and conceptual problems in accommodating both genetics and evolution focused biological interests.

Within MIT, Sedgwick’s approach to developing research programs and new fields of science achieved a resounding victory with the selection of Karl Compton as MIT’s president in 1930 and the activist presidency he succeeded in establishing. Compton’s own experience in research had produced a well articulated preference for organizing researchers around common elements across the disciplines and within the administrative structure of the university. His stature as one of the few American physicists to have mastered a means for obtaining data relevant to atomic structure, and his success at recruiting physicists, promoting electrical engineers with research interests that merged into the mathematical and physical, and obtaining philanthropic funding for distinctively MIT-based, multi-disciplinary research programs made his understanding of research organization a cultural norm for the faculty whose careers he helped to make.

The coincidence of war in Europe, Bush’s move to the Carnegie Institution of Washington, and the presence of the president’s uncle on the CIW board brought Bush and Compton, along with their competitors and collaborators, into national administration of research and into advisory relationships with political officials. But there was nothing accidental about the character they imposed on those responsibilities. NDRC was structured to operate as Compton and Bush had wanted the Rockefeller Foundation to operate during the 1930s. NDRC adopted an internal structure based on broadly construed military functions rather than specific technologies or academic disciplines; it favored proposals that reconfigured academic researchers from various disciplines around the development and use of militarily relevant instrumentation; and it provided an effective sounding board for intra-laboratory administrative problems stemming from the size and composition of the laboratories. These characteristics were evident in the wartime radar program. NDRC passed off to the Army Corps of Engineers the Manhattan Project, which it could neither fit into its structure nor dismiss as useless.
From the perspective of the history elaborated here, an appeal to the utility for scientists of older patterns of mediating relations between intellectuals and political power does not do as an explanation for science policy's evolution into an institutionally fragmented, intellectually disjointed function of an American government over which centralized administration has always been limited. Science policy could have had well articulated institutional and intellectual foundations if either Bush's or Kilgore's original bills had become law; science policy could have become the province of one agency charged with being all things to all parties if the compromise Kilgore-Magnuson bill had become law. The end result could only have pleased the research bureaus in the several cabinet departments and independent agencies, whose powers were a problem for the presidency in the eyes of Kilgore's political supporters, whose research interests were limited to applied subjects in the eyes of Kilgore's scientific supporters, and whose competence and scientific broad-mindedness were suspect in the eyes of Bush's political and scientific supporters.

The roots of the political difficulties for science policy lay in the parochial foundations of Bush's approach and the refusal of scientists with different perspectives on the war and the prior peace to accept and quietly politic for a niche within a science agency that would have been a broadened version of NDRC. The near-total victory of Sedgwick's approach at MIT was not universal in the academic community and was not even as successfully instituted in MIT's Biology Department as it had been in physics and the two major wings of electrical engineering research (though Bush in 1945 and 1946 could not have been worrying about the prospects of Francis Schmitt narrowing biological engineering into a customary biology program with advanced training in the use of physical techniques). During the war, biologists and engineers had been publicly critical of NDRC, and Bush well knew that prominent physicists inside and outside the Manhattan Project considered Army administration repulsive and unnecessary. They turned out not to be a fringe element that manifested itself only when academic scientists kept company with philosophers and social scientists at New York City gatherings. A genuine swathe of the American scientific community thought of scientific research, as had Arthur Noyes, as the
competitive pursuit of better cognitive foundations for the comprehension of nature, and they signed a petition that used Kilgore's language to express those ideals.

Kilgore accepted the distinction, advocated by his sympathizers among scientists, between wartime and peacetime science. What he did not accept was a distinction between wartime and peacetime science agencies. Agencies could be either effective or ineffectual depending on their ability to have implemented over private or vested interests what elected officials judged the public's interest. NDRC's need for presidential sanction to participate in discussions of military strategy and the use of new weapons was just a specific manifestation of a generally applicable lesson—a research agency needed bureaucratic leverage over operating agencies if its research were to bear fruit in policy initiatives. The assumption that the NSF's director and upper-level staff would busy themselves with issues of executive-branch organization and shorter-term policy issues that involved scientific judgements sat well with scientists whose principal concern was to turn back NDRC's pursuit of multi-disciplinary laboratories contoured to cover technological functions in favor of an agency that judged proposals that individual scientists generated. Harold Smith's claim that the scientific decisions in Kilgore's NSF would be made at levels well below the director resonated with scientists who wished to plan research in arrangements similar to what John Wheeler had envisioned for ultra-nucleonics, in which a conference of working research physicists would determine desirable and feasible experiments for its leaders to argue for with government budget makers.

Bush would not accept the distinction between wartime and peacetime science. Research could be either enlightening or dull depending on the interest it generated outside the circle of specialists devoted to its pursuit. NDRC's research successes were a specific manifestation of a generally applicable lesson—specialists operated best when working in inter-dependence with different specialists under local administration. The assumption that the NSF's governing board would consider whether the nation's research institutions were covering the best range of technological functions sat well with scientists who wished to plan research in arrangements similar to what John Slater had envisioned for electronics, in which a
university administrator used the career ambitions of a staff member of value to two departments to spur his university to create a novel research unit for the staff member to direct. The distinction Bush pushed was between wartime and peacetime science agencies. The need for NDRC to be placed within OSRD was an artifact of war, when quick decisions, even if wrong in retrospect, were preferable to taking the time to build a consensus among all the parties affected by the introduction of a new weapon. Bush's isolation of a research agency from presidential administrative authority resonated with businessmen and advocates of more local control over public policy, whose principal concerns were to identify and retain only the essential aspects of national governance amidst the federal government’s expansion during the New Deal and the war.

Granting more or less power to the federal government or the president within the federal government cannot alter scientists' contrasting predilections to pursue research that deepens disciplinary foundations and research that broadens disciplinary influences. A better issue for consideration is to revisit the question of whether there are conditions under which the two goals can coexist within a single policy framework. Bush doubted a single agency could so function; he preferred to continue criticizing the Kilgore-Magnuson bill in the House rather than push for its adoption. However, his perspective may well have had parochial foundations. Bush had achieved success in electrical engineering by adopting Dugald Jackson's values, which stressed the importance of faculty who could fit the fundamental insights of an Arthur Kennely into a broad intellectual and technological context, rather than following Kennely, whose specialized studies of machines that challenged his mathematical sensibilities left no long-term impact on MIT. Bush had cut his administrative teeth by inquiring into the problems of the Research Laboratory of Applied Chemistry as part of Compton's efforts to bring administrative order and university standards to the sponsored research that remained after Noyes left MIT and Samuel Stratton was unable to build on Walker's legacy. Small wonder that Bush undermined the Kilgore-Magnuson bill; he could hardly have lived with himself had he not done all he could
to keep a national science agency from having to suffer through its version of MIT's local history in order to reach the enlightened perspective he embodied.

Are scientific and engineering communities—organized respectively around deepening disciplinary foundations and codifying industrial practices—incapable of drawing resources from a common fund without falling into destructive competition? Must scientists accept secondary priority for the specialized study of phenomena that appear to hold promise of opening new conceptual horizons in order to enjoy productive social relations with engineers whose involvement with matters of near-term industrial relevance will always hold general appeal for support? The institutional foundations for US science policy developed when the local experiences of science policy's most prominent advocates led them to answer both questions affirmatively, and when scientists who did not want second-class status for research built around conceptual goals had barely begun to fathom the first. Perhaps that is not the case today. If national science policy is to help strike a balance between the virtues of centralized administrative rationality over the programs of the national government and respect for local autonomy over the affairs of universities, and if national science policy is to help strike a balance between the pursuit of topics of widespread interest to national and international disciplinary communities of scientists with support for local diversity in the pursuit of research within universities, then there seems no alternative to internalizing the struggle, as the Kilgore-Magnuson bill did, and pursuing historical studies that can contribute to an appreciation of the difficulties that would likely be encountered.
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