

CHAPTER TWELVE

PHYSICS BETWEEN WAR AND PEACE*

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Introduction

Three hundred and fifty years ago, Galileo introduced the notion of mechanical relativity by invoking the experience of sea travel:

Shut yourself up with some friend in the main cabin below decks on some large ship, and have with you there some flies, butterflies, and other small flying animals. Have a large bowl of water with some fish in it. . . . The fish swim indifferently in all directions; the drops fall into the vessel beneath; and, in throwing something to your friend, you need throw it no more strongly in one direction than another. . . . You will discover not the least change in all the effects named, nor could you tell from any of them whether the ship was moving or standing still.¹

The imagery and dynamics of ships permeate Galileo's works, at once tying the new physics rhetorically to the modern navigational achievements of early seventeenth century Italy, and providing an effective thought-experiment laboratory for the new "World System."

Several centuries later, when Albert Einstein was struggling to overthrow Galilean-Newtonian physics, he too chose an image of contemporary transport as the vehicle for his radically new *Gedanken-experimente*. Now the railroads, symbol of the success of German technology and industry, replaced the sailing vessel in the argument. As Einstein put it, no optical experiment conducted in a constantly moving train could be distinguished from one performed at rest.²

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¹ Galileo, *Dialogue Concerning the Two Chief World Systems*, trans. Stillman Drake (Berkeley, Calif., 1967), pp. 186–187.

² See, for example, Einstein's popularization of relativity first published in 1916: *Relativity: the special and general theory* (New York, 1961).

Yet a third technological image of transport appealed to the American physicist Richard Feynman as he assembled a synthetic picture of quantum mechanics and relativity in the years after World War II. The positron, as he saw it, could be viewed as an electron moving backwards in time. If so, then the simultaneous creation of an electron-positron pair (ordinarily seen as involving two quite distinct paths) could instead be viewed as a single, continuous track: the positron travels backwards in time until it reaches the moment of creation, whereupon it becomes an electron moving forward in time. Feynman conveyed his vision with a vivid metaphor: “It is as though a bombardier flying low over a road suddenly sees three roads and it is only when two of them come together and disappear again that he realizes that he has simply passed over a long switchback in a single road.”³

Every age has its cultural symbols, and Feynman’s was as telling as Galileo’s. Young American physicists of the 1940s and 1950s had seen their discipline recrystallize around the twin poles of radar and the atomic bomb. The venerated B-29 bomber carried both—an apt symbol, therefore, of the fruits of their labor—and served as a perfect vantage point from which to view the new theoretical and experimental physics.

Of course, the effect of the war on the development of physics in general, and of high-energy physics in particular, goes far beyond a passing metaphor. Indeed, the problem is that the effects are too great, and too varied, to be treated comprehensively in any one essay. For in a sense, the effects of the war permeate every aspect of postwar history. In the history of physics these effects include the thoroughgoing overhaul of the institutional structure of government-supported science. From the National Science Foundation to the Atomic Energy Commission, and the Office of Naval Research, no aspect of science funding remained unchanged.⁴ Among the war’s

³ R.P. Feynman, “The Theory of Positrons,” *Physical Review* 76 (1949): 749.

⁴ On the World War II scientific organizations and their effects on research, see Irvin Stewart, *Organizing Scientific Research for War* (Boston, 1948); D. Kevles, *The Physicists* (New York, 1978); B. Hevly, “Basic Research within a Military Context: The Naval Research Laboratory and the Foundations of Extreme Ultraviolet and X-Ray Astronomy 1923–1960” (Ph.D. dissertation, Johns Hopkins University, 1987), esp. chap. 2; and A. Hunter Dupree, “The *Great Instauration* of 1940: The Organization of Scientific Research for War,” in G. Holton (ed.), *The Twentieth-Century Sciences* (New York, 1972), pp. 443–467. On the postwar contributions of one important

consequences was a profound realignment of all relations between the academic, governmental, and corporate worlds, especially as physicists began contemplating the funding necessary for the construction of atomic piles, larger accelerators, and new particle detectors. Further, the war forged many collaborations and working groups among scientists that continued smoothly into the postwar epoch. And finally, the war provided astonishing quantities of surplus equipment that fed the rapidly expanding needs of postwar “nucleonics”—the study of a broadly construed nuclear physics, situated at the nodal point of research problems of cosmic rays, nuclear medicine, quantum electrodynamics, nuclear chemistry, and the practical imperatives of industry and defense.

Above all, one cannot ignore the new relation of university physics to military affairs that in a sense began, rather than ended, in the skies over Nagasaki. Suddenly academic physicists could negotiate with high-ranking officials from the Navy, the Air Force, and the Army to acquire new machines. At the same time, the military became an active participant in the shaping of postwar scientific research, through university contracts, the continuation of laboratories expanded during the war, and the establishment of new basic research programs under the aegis of individual armed services. Projects of joint civilian and military interest were lavishly funded, offering physicists the chance to think about exploring cosmic rays, not 3 or 4, but 100 miles above the earth’s surface. Where a handful of technicians had once been sufficient to aid the physicists as they constructed new instruments, now the physics community began a deep new alliance with the various branches of scientifically informed engineers.

The present essay, addressing the impact of wartime research on postwar experimental and theoretical physics, can only begin to sketch some of these effects, drawing a few of the lines along which such a history of physics between war and peace might advance. I will not treat, for example, the vicissitudes in the dramatic careers of scientist-politicians such as Vannevar Bush, James Conant, or Robert

agency see S.S. Schweber’s contribution to this volume, “The Mutual Embrace of Science and the Military: ONR and the Growth of Physics in the United States after World War II.” [*i.e.*, in Everett Mendelsohn, Merritt Roe Smith, and Peter Weingart (eds.), *Science, Technology and the Military*, Sociology of the Sciences 12 (Dordrecht, 1988)].

Oppenheimer; the establishment and internal politics of funding organizations; or the alterations in industrial physics research policy. Instead, my goal is to peer into the effects of wartime science on the quotidian proceedings of physics itself, and into the experience of physicists in their research capacity. To do this I have chosen to focus on four exemplary physics departments: those at Harvard, Princeton, Berkeley, and Stanford. Each had its own trajectory, shaped in part by different war experiences and earlier patterns of research. Yet the four had much in common: each had to confront a sudden expansion, search for a new relationship between theorists and experimentalists, and solve the difficulties that accompanied the move into the epoch of large-scale, centralized, and cooperative research.

Together these changes transfigured the physicists' approach to research. In the most visible and dramatic fashion the war provided concrete instances of scientific accomplishments, though it remains an open question whether or not the lessons drawn from that experience were actually the ones responsible for the physicists' success. But in a sense that I will develop further below, the major weapons systems—radar, atomic bombs, rockets, and proximity fuses—formed *guiding symbols* that inspired the strategy of much postwar research. Needless to say, one can find earlier instances of one aspect or another of large-scale research: the great philanthropically funded telescopes, Ernest Lawrence's growing array of cyclotrons in the 1930s, and institutes of physics in Europe come immediately to mind—monumental telescopes cost millions of dollars, cyclotrons took several people to operate, and at certain institutes state and scientific concerns shared a common roof. Indeed, at universities such as Stanford and Berkeley, the 1930s saw the establishment of joint endeavors involving physics and electrical engineering. But despite the importance of such successes as the cyclotron and the klystron, before the war there were no physics achievements born of such large physics/engineering efforts that made the continuation of centralized big research seem either inevitable or inarguable. Beginning in the war, however, the physicists' and engineers' large-scale collaborative work on electromagnetic and nuclear physics-based weapons systems provided just such exemplars. In part as a result of these successes, between 1943 and 1948 key segments of the American physics community came to accept a mutation in the ideal of the physicists' work and workplace. One after another, physics departments began to conceive of a style of orchestrated research that has

come to dominate the character of modern investigations in high energy physics, and increasingly in other domains as well.

*Expansion and the Repositioning of Physics in the University:
Teaching, Surplus, and the Relation of Science to Engineering*

Our first need is for a closer analysis of how the expansion affected physics departments, and to achieve this we need to dispose of the myth that changes in scientific planning began only after the guns of World War II had ceased firing. For it was *during* the period 1943–45 that physicists and administrators first debated and set in motion the coming boom. Across the country, from Berkeley to Harvard, pressure to think about postwar expansion began at the top.⁵

Harvard

At a meeting on 20 November 1944 the president and Fellows of Harvard College agreed to create a panel whose task was to direct the expansion of Physics, Chemical Physics, and Engineering. Selecting representatives from the various physical sciences at Harvard, President James Conant established a Committee on the Physical Sciences.⁶ As a driving member of the principal organizations shaping scientific war research, Conant had arrived at a clear conception of the shape of postwar science. It was an image shared by several of his Harvard colleagues, as was evident at that committee's very first assembly. Edwin C. Kemble lobbied for physics in these terms: "The war has given a great boost to physics. It has stressed the importance of physics to industry and national defense and has underscored the

⁵ On the postwar work at Berkeley see R. Seidel, "Accelerating Science," *Historical Studies in the Physical Sciences* 13 (1983): 375–400.

⁶ J. Conant to Kemble, 28 November 1944; summary of meeting of 20 November 1944, in Hiektaann Papers, Physics Department Historical Records, box 3, ca. 1930–65, Harvard University Archives (hereinafter abbreviated as HP). Already in the summer of 1943, Conant was deeply involved in postwar planning for military/scientific relations, though the early speculations did not lead directly to accepted policy. See the excellent book on the military's expectations for their postwar condition: M. Sherry, *Preparing for the Next War. America plans for postwar defense 1941–45* (New Haven, Conn., 1967), pp. 137–138.

usefulness of men trained in pure physics when emergency requires that they turn to applications.”⁷ Consequently, he argued, the university needed to expand to new fields, get the best personnel, and enlarge the group for instructional purposes. In nuclear physics, which was “the field of greatest interest,” Kemble conveyed the department’s desire to keep its existing staff, to add “top caliber” theoretical physicists—for example, Julian Schwinger, Hans Bethe, or Harvey Brooks—and, of course, to augment their total budget with funds for mechanical assistance and construction costs.

To justify the expansion of theoretical physics, Kemble composed a memorandum on 9 December 1944 to the Physical Sciences Panel which began, under the rubric “presuppositions,” by contending that there would be a “nation-wide acceleration in the growth of the science of Physics as a result of war emphasis.” As Kemble saw it, it was a growth that would take place in two areas. *Solid-state physics* (“the field of properties of matter in bulk”) demanded the efforts of a new, more powerful contingent of theoretical physicists, who would be masters of quantum mechanics, statistical mechanics, and chemical thermodynamics. For Kemble, the need for more theory was illustrated by his colleague. P.W. Bridgman, whose superb investigation into the high-pressure domain had nonetheless “undoubtedly fallen short of its maximum potentialities since, to date, he has worked without steady and effective collaboration from theoretical physicists.”⁸

Beyond solid-state physics lay *nuclear physics*, which was, by Kemble’s lights, “the most spectacular field in physics today.” It was there that “the riddle of the physicist’s universe is found,” amid the cosmic rays, mass spectrographs, cyclotrons, and forms of radioactivity. And as the war was making perfectly clear, medical and chemical applications appeared “manifold,” and were accompanied by the even more tantalizing “possibility of unlocking stores of atomic energy [which added] urgent significance to the investigations.” Exactly this combination of the purely intellectual and of hoped-for practical consequences characterized the physics community’s justification of the needed expansion. In brief, the intellectual argument positioned atomic physics as a stepping-stone to nuclear physics.⁹

⁷ File cards in handwriting of E.C. Kemble, HP (note 6).

⁸ Kemble, “Panel: Physical Sciences. Memorandum on Proposals for the Development of the Department of Physics,” 9 December 1944, HP (note 6).

⁹ *Ibid.*

Speaking for his department, Kemble argued that since World War I the focus of physicists' concern had altered from understanding atomic structure and the structure of simple molecules. Previously, Harvard's own efforts had to a large extent been devoted to spectroscopy of all kinds; but now problems of this type "have largely been solved," and "work in this field operates against a law of diminishing returns," to be replaced by the more alluring problems of nuclear and solid-state physics. Both of these new domains demanded a new, deeper cooperation between experimental and theoretical workers to handle

the increasingly abstract and complex character of present-day physical theories. This quality is the result of the intensive search for more powerful means of attack on problems of a more and more difficult character. As one consequence of the complexity of the theory, the most brilliant experimental physicist is in need of theoretical collaboration to an extent previously unknown.

At the same time, an increasing number of the "more gifted young men" were choosing theoretical physics. Oppenheimer's presence at the Berkeley Radiation Laboratory was an example for all to see of the theorist's usefulness in joining the skills of experimental and theoretical physicists; the young theorist's contribution "has been of crucial importance in the meteoric rise of that laboratory to its place as principal center for nuclear investigations in this country." Unspoken—but undoubtedly understood—was Oppenheimer's masterful guidance of the Manhattan Project. The Harvard physicists hoped Schwinger or Bethe could enliven theoretical life in Cambridge.¹⁰

Acquiring theorists was, however, only part of a much larger pattern of growth. By the end of December 1944 Kemble was sure enough of the expansion plans to write Conant:

My personal acquaintance with the state of engineering arts at the time of the last war and at the present time convinces me that it is imperative for our future national safety that the scientific bases of engineering practice shall have far more intensive study than heretofore. We must have a much increased number of analytical engineers with brains and advanced training if we are to hold our own in the technological race.¹¹

¹⁰ *Ibid.*

¹¹ Kemble to Conant, 22 December 1944, HP (note 6).

Kemble added that the universities could best aid this cause by prosecuting work in “pure science,” which would yield more “in relation to the investment” and attract the best “quality of the men . . . likely to execute it.”¹² Here the Harvard physicist embraced the central justifications for expansion that would be repeated over and over again during the following decades: connections between fundamental research and teaching, industrial spin-offs, and military preparedness.

The “modest” investment Kemble had in mind was hiring Julian Schwinger and Edward M. Purcell; adding \$25,000 for operating expenses, \$30,000 per year for operating the cyclotron, and \$30,000 for the physics of metals; a new electronics building at a cost of \$100,000; a new mechanical engineering building at \$500,000; an electronics research budget of \$10,000 per year; further appointments in electronics, mechanical engineering, and aeronautical engineering; construction of a wind tunnel for \$75,000; and building an interdisciplinary science center for between 1.5 and 2 million dollars.¹³ Clearly, the expansion envisioned by plans such as these extend far beyond nuclear or “fundamental” physics and embraced a picture of a much-enlarged program for both physics and engineering. A few days later Kemble contacted his physicist colleagues Jabez Curry Street and Kenneth Bainbridge, requesting them to consider how their respective research areas might participate in the expansion.¹⁴

A boosted physics budget made possible both the new machines and the increased role of theoretical physics. Throughout the American university system the growth of physics was also fueled by a spectacular jump in the number of students. During the war, the armed forces had called upon the physics departments to teach thousands of conscripts the elements of physics so they could cope with a new generation of technical war apparatus, especially radar, radio, rockets, and navigational equipment. After the war the G.I. Bill funded these and other students as they came back to the university in droves. Although providing instruction to so many students often taxed their already depleted resources, wartime instruction also presented universities with an unprecedented opportunity to expand their clientele.

¹² *Ibid.*

¹³ Kemble, “Tentative Summary of Proposals before Physical Sciences Panel,” HP (note 6).

¹⁴ Kemble to Street, 30 November 1944; Kemble to Bainbridge, 30 November 1944, HP (note 6).

Temporary physics programs had formed during the war at Harvard and Bowdoin in electronics and communication, at MIT in radar, at Los Alamos, and at other institutions as well. At Harvard alone, 5,000 students passed through the pre-radar electronics course. After demobilization the faculty expected that large numbers of veterans from that course would return to further their physics education. As E.L. Chaffee put it on 9 December 1944, the Officer War Training Courses put “Harvard in an advantageous position for attracting students after the war.”¹⁵

Aside from introducing promising students to the university, Chaffee remarked that “the war-training courses have provided us at no expense with a very considerable amount of laboratory equipment. There is insufficient space in the Cruft Laboratory even to store this equipment, say nothing of setting it up for instruction.”¹⁶ Instructional equipment could be supplemented by war surplus apparatus suitable for research. Chaffee noted that Harvard’s antiradar work and other war projects would offer

an unusual opportunity to purchase advantageously some very valuable equipment from the OSRO [Office of Scientific Research and Development] projects and perhaps some equipment from the war training courses. . . . We should be prepared to purchase a considerable amount of this equipment. There will [be] available machine tools, obtainable at much reduced prices from the same sources, and I believe we should purchase a considerable amount of this machinery both to increase our present shop facilities and to replace some outmoded and worn machine tools.¹⁷

With this new equipment Chaffee expected that physicists in the postwar period would be able to exploit their newly developed capability to generate microwave signals “by methods which have worked but which are not understood.” Such applications included detecting molecular resonances, pulse systems of communication, and high-frequency heating. As Purcell rather over modestly put it: you didn’t have to be too smart to design an experiment with the extraordinary resources offered by the new electronics.¹⁸

¹⁵ Chaffee, “Expansion of Research and Instruction in the Cruft Laboratory,” HP (note 6). We still have relatively few systematic data on the effects of this teaching: were there lasting effects on the physics curriculum? Where did the students go after the war: to academic pursuits? industrial positions? military assignments?

¹⁶ *Ibid.*

¹⁷ *Ibid.*, p. 8.

¹⁸ E.M. Purcell, interview 26 May 1987. Purcell discovered the 21-centimeter

Surplus equipment came from many war sources and went to a wide spectrum of users. Connections established between the scientific and defense communities grew, yielding benefits for scientists long after V-J Day. Since they were on contracts from the Office of Naval Research, as late as 1960 physicists at Brookhaven could acquire large armor plating, originally intended for cruisers, to use in neutrino experiments.¹⁹ On the West Coast, Robert Hofstadter collected Naval gun mounts on which he could perch his magnetic spectrometer.²⁰ Overseas, A. Gozzini's experiments with surplus pulse-generator circuits and microwave equipment led to his development with Marcello Conversi of the "flash tubes" that played such an important role in cosmic-ray physics and in subsequent work on the spark chamber.²¹ Phototubes, crucial for scintillation devices, had been much improved and exploited during the war as sources for noise generation in radar countermeasures. As we will see, this type of continuity between wartime and postwar work ran deep; beyond the formation and evolution of administrative organizations such as OSRD, an essential consequence of the war work was the carry-over of physicists' techniques, equipment, and collaborations into the late 1940s.

With the promise of new physicists, new students, and new equipment, departments could embark on major new research projects. Harvard—along with many other universities—was determined to restart its cyclotron program on a much-increased scale. Planning to accelerate both electrons and protons, the nuclear physics planning committee met for the first time in January 1946. They wrote: "From the point of view of physics this program represents a vigorous and progressive plan which should enable Harvard to compete favorably for financial support and, in addition, enhance its attractiveness as a center of research in the nuclear field."²²

line, and won the Nobel Prize for his co-invention of Nuclear Magnetic Resonance—both exploited Rad Lab techniques.

¹⁹ M. Schwartz, interview 20 October 1983.

²⁰ R. Hofstadter, H.R. Fechter, and R.H. Helm to Commandant, Mare Island Naval Shipyard, 9 February 1952, Hofstadter private papers, Stanford University.

²¹ See P. Galison, "Bubbles, Sparks, and the Postwar Laboratory," in L. Brown, M. Dresden, and L. Hoddeson (eds.), *From Pions to Quarks: elementary particle physics in the 1950's* (Cambridge, forthcoming). [the volume appeared in 1989]

²² "Proposal for Nuclear Physics Program at Harvard," submitted 2 February 1946 and enclosed with minutes of the first meeting (11 January 1946) of the

When the Harvard physicists turned from war work to accelerator work they brought with them experience from the Manhattan Project (Kenneth Bainbridge), from the Radar Project (Edward M. Purcell, Julian Schwinger), and from Radar Countermeasures (Roger Hickman, John Van Vleck). All served on Harvard's newly established Committee on Nuclear Sciences. After selling their old cyclotron to the government for \$200,000, and getting a commitment of \$590,000 from Harvard and \$425,000 from the Navy, the committee members could begin to plan an 84-inch cyclotron.²³ Their physics program included proposals to produce 25 MeV deuterons and 50 MeV alphas in order to explore the nature of the proton, to produce high-energy neutrons, and to extend the wartime fission experiments to elements lighter than uranium and thorium. With accelerated electrons on tap, the planning committee hoped that the cyclotron could also be exploited to pursue radiation therapy, the photodisintegration of nuclei, and the formation of electromagnetic showers.²⁴

Princeton

At Princeton, as at Harvard, planning for the postwar expansion began long before the euphoric crowds descended on Times Square. On 4 January 1944 a somewhat overoptimistic John Wheeler wrote to H.D. Smyth that he trusted the war would soon be over and he could return to physics shortly. He then went on to formulate a "Proposal for Research on Particle Transformations," of which the "Ultimate Purpose" was "to determine the number of elementary particles, the transformations between them, the combinations which they permit, the nature of their interactions, the relation between these particles and the existing theories of pair formation, electromagnetism, gravitation, quantum mechanics and relativity." As he was witnessing an immensely successful collaboration of theory and

Committee on Nuclear Physics [later Committee on Nuclear Sciences], file "Index and Records," box "Committee on Research and Nuclear Sciences, Records 1946–1951," Harvard University Archives.

²³ P.H. Buck to R. Hickman, 5 October 1946, file "Miscellaneous Financial Material and Correspondence," box "Committee on Research and Nuclear Sciences, Records 1946–1951," Harvard University Archives.

²⁴ "Proposal for Nuclear Physics Program at Harvard" (note 22).

experiment at the University of Chicago's Metallurgical Laboratory (Met Lab), culminating in the achievement of fission in December 1942, he clearly saw this as the wave of the future. Wheeler began: "*Plan*. Effective progress calls for the collaboration of experiment and theory. The interaction between the two will be most fruitful, I believe, when in addition both approaches are combined in a single institution, under the same leadership."²⁵ He argued that such collaboration ought to be the model for physics research at Princeton; the lesson he drew was one taken to heart at institutions across the United States.

Wheeler needed help to solve the main theoretical problems. These included the need to invoke action-at-a-distance theories to eliminate the self-energy difficulty in quantum electrodynamics, the necessity of classifying relativistic quantum field theories, and exploring the theory of positronium. There were also such "associated experimental" problems as nuclear meson capture, mass distribution of cosmic ray particles, and the gamma-ray production of mesons. Like Kemble at Harvard, Wheeler counseled building the theoretical side of the Princeton department, by hiring three theoretical assistants of "the type of Feynman or Jauch," along with some experimentalists of the "type of Luis Alvarez or Bob Wilson." Of course the thirty-three-year-old Wheeler would "[w]elcome collaboration of interested older members of staff," and, again grounding his recommendation on his Chicago experience, he suggested that experimentalists should call on experienced "electronics men" for the design and development of instruments. The program promised, as Wheeler continued to insist after the war, the possibility of sources of energy many times more powerful than all known nuclear reactions, with "obvious implications for the problem of national defense."²⁶

As ambitious as young Wheeler's plans may have sounded initially, even before the war's end the Physics Department had begun to address the administration in a new, more confident tone. With the successes of proximity fuses, radar, and then the atomic bombs, physicists—who for years had occupied a decidedly secondary place

²⁵ J.A. Wheeler to H.D. Smyth, 4 January 1944, file "Postwar Research," in Physics Department Departmental Records, Chairman, 1934–35, 1945–46, no. 1, Princeton University Archives (hereinafter, PUA).

²⁶ *Ibid.*

within the university—acquired a radically improved self-image, evident as they spoke to colleagues and administrators. As the Physics Department put it in a draft of their department report,

The end of the war finds the department in a praiseworthy but embarrassing condition. The record of the members of the department in war work is laudable, so much so that many of them, particularly in the younger group, are receiving very attractive offers from other institutions and from industry. Such offers are not only attractive in terms of salary but are usually backed by promises of large expenditures for apparatus and equipment. The university must choose between going ahead vigorously, capitalizing the fine record of this department during the war or letting its physicists drift away to such a degree that it may take a generation to restore the department. The first course will require money for men and for equipment, a great deal of money, but it offers a magnificent opportunity, completely in the tradition of the university. We have never been in a better position to push forward in the field of fundamental physical research.²⁷

Indeed, their position *was* entirely unprecedented.

Physicists everywhere were attracted by the promise of the new technology and science for advancing the physics of nucleons and mesons, but developments in “fundamental” experimental physics at Princeton took on a particular cast. In part this reflected an imaginative style of work that John Wheeler had developed before the war, but the echoes of what he had seen in the Metallurgical Laboratory can clearly be heard in his ideas for postwar research. As early as June 1945 Wheeler had penned a proposal on the future of physics research that gave three goals for the postwar epoch. First, though he voiced doubts about some of their features, he advocated the development of accelerator sources for particles. Among these, he mentioned Luis Alvarez’s latest plans to build a linear electron accelerator; Wheeler judged that one would want at least a 5 GeV proton accelerator in order to produce pairs of mesons.²⁸ Second, he wanted Princeton to establish a wide-ranging “ultranucleonics” program. Third—and here he saw the real payoff in physics—he

²⁷ “Department of Physics Report to the President 1944–45,” Physics Department Chairman’s Correspondence 1942–43, 1943–44, no. 16, PUA.

²⁸ J.A. Wheeler, “Three Proposals for the Promotion of Ultranucleonic Research #6: H.D.S.,” 15 June 1945, copy to Smyth, in Physics Departmental Records, Chairman 1934–35, 1945–46, no. 1, PUA.

hoped that the department would seize upon cosmic rays as the primary domain in which to search for answers to basic questions, because there and only there could one find the high-energy collisions needed to probe the subnuclear domain. For the cosmic-ray project, Wheeler suggested using Flying Fortresses to hoist experiments and experimenters into the upper atmosphere.

The idea of enlisting bombers for the study of high-altitude cosmic radiation had several appealing aspects. It would alleviate the costs of such exploration for the universities, while leaving control over research apparatus entirely in the hands of the physicists. As for its physics justification, Wheeler noted that only by reaching far into the sky could one study particles with something like 10^{17} electron volts, and therefore exhibit the multiple meson production process that interested him. "This plan calls for army transportation of equipment up to 10 tons to altitudes of the order of 40,000 feet. Research money would in this way be freed for research itself, and for research of a most effective kind."²⁹

Finally, Wheeler felt that a survey of the entire field of ultranucleonics was of the highest priority. From what had been learned by cosmic-ray studies, he suspected that it might be possible to transform matter directly and completely into energy on the model of protons being transformed into mesons in the upper atmosphere.

Discovery [of] how to release the untapped energy on a reasonable scale might completely alter our economy and the basis of our military security. For this reason we owe special attention to the branches of ultranucleonics—cosmic ray phenomena, meson physics, field theory, energy production in supernovae, and particle transformation physics—where a single development may produce such far-reaching changes.³⁰

To reach these dramatic goals, physicists had to make their needs known, and here the survey would play a vital role. It would offer workers in postwar physics "a prospectus of long-range objectives," and it would gain financial support for fundamental physics by making research public and by demonstrating "that scientists in free association can show more vision and judgment on research planning

²⁹ *Ibid.*

³⁰ *Ibid.*; and see J.A. Wheeler, "Elementary Particle Physics," *American Scientist* 35 (1947): 170, 172, 174, 177–193, 223.

than any centralized government authority.” Not least, the ultranuclear survey would “uncover lines of investigation of evident present or future value to the country’s war power.”³¹ Though these functions were already far beyond the typical prewar involvement of the government in basic research, within a few months Wheeler had in mind new even more active roles for the government to play in physics.

One such role came from Nazi Germany’s development of the dreaded “vengeance weapons,” the V-1 “buzz bomb” and the V-2 guided missile. This had been an engineering project of immense scope, costing over \$3 billion—fully one and a half times the resources put into the Manhattan Project. The V-2 was brought into the war relatively late, but starting in September 1944 the Germans successfully launched more than 3,000 V-2s, killing almost 10,000 people in England. Soon, however, the Allies began advancing across Europe, and Wernher von Braun retreated from his headquarters at Peenemunde with some 4,000 workers to the V-weapon production facility situated in the concentration-camp complex of Dora-Nordhausen in Thuringia. Installing themselves in the Harz mountains, the V-2 workers successfully evaded the approaching Russian army, eventually surrendering themselves to an American garrison. Under a secret mission code-named “Overcast,” the American army shipped the Nazi scientists to the United States to continue missile development work at several sites. The Germans arrived in October 1945, and soon the Peenemunde team was split into groups, working with American industry to produce a variety of rocket types.³²

From 16 April 1946 to 19 September 1952, 64 V-2s were launched from White Sands. The first failed three and a half miles into the air when a fin ripped off and the rocket was destroyed; the next launch, on 10 May 1946, successfully rose to 71 miles.³³ For physicists at Princeton, the capture and reinstallation of the German rocket team offered an immediate opportunity. In November 1945—just a month after von Braun and his associates were brought to White

³¹ Wheeler, “Three Proposals” (note 28).

³² F. Ordway III and M.R. Sharpe, *The Rocket Team. From the V-2 to the saturn moon rocket* (Cambridge, Mass., 1982); see also the important article by Linda Hunt, “U.S. Coverup of Nazi Scientists,” *Bulletin of the Atomic Scientists* 41.3 (April 1985): 16–24.

³³ Ordway and Sharpe, *The Rocket Team*, pp. 353–354.

Sands, M.H. Nichols jotted an interoffice memo to Smyth suggesting that the department ought to propose to study optical and electrical phenomena in the upper regions of the earth's atmosphere. At the same time the Princeton group could explore cosmic rays and neutron densities. All this was made possible by "new advances in rocket technique as well as progress here at Princeton and elsewhere in the field of radio telemetering from aircraft and missiles," which would "make possible an extension of present data to regions as high as 500,000 feet."³⁴

For some time Wheeler had seen cosmic-ray physics, not accelerator physics, as the primary vehicle for understanding elementary particles. In a memo of January 1946 he stated that "cosmic ray research will take on an even more important role in physics in the next few years," and he advocated immediately setting up a joint experimental and theoretical research group. "Inasmuch as the V-2 firings will not last indefinitely, and inasmuch as the experienced researchers are becoming scarcer every day, it appears that some action [should] be taken as soon as sound decisions can be made." Presumably addressing himself to the department chairman, Wheeler stressed that they would be needing four to six assistants with experience in experimental electronics, nuclear physics, or cosmic rays, as well as experienced cosmic-ray and nuclear physics experimentalists. "Research in physics is starting fresh, and . . . new techniques and new vehicles are now available"—and so it behooved the department to search out consultants from among the best universities, institutes, and weapons laboratories.³⁵

Wheeler himself headed a Navy-funded project that would handle the telemetric transmission of cosmic-ray data from the German team's missiles. Begun on 1 January 1945 for other purposes, the Navy grant had been extended in March 1946 and was to cover the development of telemetry equipment, while at the same time serving to study cosmic-ray showers and the properties of "mesotrons" through the design, fabrication, and operation of cloud chambers and Geiger counters to be mounted on the V-2s. In July 1946 D.J.

³⁴ M.H. Nichols to H.D. Smyth, 26 November 1945, file "Postwar Research," Physics Department Records, Chairman 1934–35, 1945–46, no. 1, PUA.

³⁵ No author listed [probably Wheeler], "Program in Cosmic Rays," January 1946, file "Postwar Research," Physics Department Records, Chairman, 1934–35, 1945–46, no. 1, PUA.

Montgomery reported to the Navy that the Princeton V-2 expedition had arrived at White Sands and was making final tests for what they hoped would be the 100-mile-high Princeton Shot on 6 August. At this point \$335 thousand had already been allotted, with \$250 thousand more to be shared over the next two years between chemistry and physics.³⁶

Cooperation with the military remained close. Military and elementary-particle problems were interspersed in planning and designing the mission. Both physicists and strategic planners needed a comparison of “Lark” and Naval Research Laboratory telemetering systems, especially with regard to the reliability, intensity of signals, and freedom from disturbances of each system. Both civilians and uniformed personnel had to study radio signal propagation in the ionosphere by transmitting and receiving signals from the missile. In addition, the physicists could use the high-altitude flight to measure cosmic-ray intensity, to distinguish primary cosmic-ray electrons from primary protons, and to measure the neutron productivity in the atmosphere as a function of altitude.³⁷ Such studies directly continued some of the Princeton group’s wartime accomplishments in telemetry. In fact, at least one member of the staff, Dr. Walter Roberts, wanted to continue this work on guided missiles at the Johns Hopkins Applied Physics Laboratory. As Wheeler assured his readers, this would ensure a “satisfactory liaison” between the Princeton and weapons laboratories.³⁸

During the period from 1945 to the early 1950s, the liaison between civilian and military nucleonics functioned well—from both parties’ perspectives. The Office of Naval Research (ONR) liberally funded civilian science, and in return the scientists moved easily back and forth between nuclear physics, cosmic rays, and weapons problems.

³⁶ D.J. Montgomery, “Annual Report of Project Assisted by Outside Funds,” 23 July 1946, file “A-475 Wheeler,” Laboratory and Research Files, 1929–54, box I of 5, PUA (see also “Elementary Particle Projects as of 6 May 1946,” in same file). Among physics goals listed in this report were: determination of total cosmic-ray intensity, meson production, neutron intensity, multiply charged particles at rocket altitudes; study of radio propagation in ionosphere; telemetry tests; pressure, temperature studies; and coordination with the Schein group’s ground cloud-chamber and balloon tests.

³⁷ Wheeler, “Appendix IV—General Survey of the Princeton Project Program—Cosmic Rays and Telemetering,” 28 August 1946, file “A-475 Wheeler,” Laboratory and Research Files, 1929–54, box I of 5, PUA.

³⁸ *Ibid.*, see under “Guided Missile Developmental Work.”

Princeton's nuclear physicist, Milton White, for example, was pleased to report on some recent Princeton instrumentation work that seemed perfectly suited for transfer to the military sector. The laboratory had perfected a new, simple, rugged, and reliable scintillation counter, and White lost no time in alerting ONR to possible defense applications of the new device:

If the U.S. Government has need of a-particle counters, either in connection with plutonium plants, or atomic bombs, there should be set in motion a program for further engineering and quantity manufacture. I can visualize an eventual need of many thousands of counters; if this is correct then our contract with the Navy will already have given the government more than a fair return on the money thus far allocated.³⁹

White added only that he hoped the Princeton researchers could be spared the engineering details.

Berkeley

On the West Coast, Stanford and Berkeley had no intention of being spared the engineering details. The style of research in the West was somewhat different from that in the East: it was more tightly bound to engineering, and it drew more liberally from philanthropic and industrial sources. Such entrepreneurial physics had brought Berkeley's E.O. Lawrence international fame for his big accelerators paid for from private coffers; as engineering accomplishments big accelerators were unrivaled, though Lawrence was less successful at drawing deep physics from them. As Robert Seidel has so nicely shown, World War II brought a substitution of federal for philanthropic funds, and the important assignment that Lawrence's laboratory direct the electromagnetic separation of U-235. Lawrence was as well prepared to begin large-scale research as anyone, and shortly was supervising a dramatically bigger laboratory with a wartime expenditure of \$692 thousand *per month*. By the middle of 1944 the Radiation Laboratory held a total working population of 1,200 scientists, engi-

³⁹ M.G. White to Uerner Liddel, Nuclear Physics Section, ONR, 11 July 1947, file "761," box V of 5, Laboratory and Research Files, 1929–54, PUA.

neers, and technicians; indeed, once their war work began in earnest the number of engineers at the laboratory never dipped below sixty.⁴⁰

After a few months of intense discouragement because of difficulties with his electromagnetic separation facility, Lawrence began escalating his expectations for the postwar period. During the summer of 1944 he began lobbying for ten new isotope separation facilities, leading General Leslie Groves (who was in charge of the Manhattan Project) to some cautious thinking about spending \$7–10 million. Just a year later Lawrence began arguing for the rapid expansion of nonweapons facilities, including Luis Alvarez's plans for a linear accelerator and Edwin McMillan's for a synchrotron. Within a few months of the end of the war, Groves authorized \$250 thousand in surplus radar sets for the linear accelerator, \$203 thousand in surplus capacitors for the synchrotron, \$630 thousand for construction in the laboratory, and \$1.6 million for six months of operating expenses. Building on earlier experience, engineering and physics had grown together at Berkeley, to make the university one of the models of postwar physical research. In fact, the Berkeley Radiation Laboratory became the pacesetter for the Atomic Energy Commission's development of regional laboratories.⁴¹

Stanford

Stanford, like its Berkeley neighbor, had successfully linked engineering and physics before the war. While Lawrence and his team were building ever-larger cyclotrons, the Stanford physicists were binding electrical engineering to physics as they learned to manipulate microwaves. William Hansen had set the character of that collaboration at Stanford with his stunning development of the "rhumbatron," which set electrons in an oscillatory dance by creating electromagnetic resonances within a copper cavity. Although the device was quickly superseded as a particle accelerator, it formed the core of the klystron, a powerful microwave tube that the Varian brothers

⁴⁰ Seidel, "Accelerating Science" (note 5).

⁴¹ *Ibid.* See also R. Seidel, "A Home for Big Science: The Atomic Energy Commission's Laboratory System," *Historical Studies in the Physical Sciences* 16 (1986): 135–175.

designed and deployed in airplane navigation and locating systems. Soon the Sperry Gyroscope Company was underwriting a good deal of the joint physics/electrical-engineering efforts. With the help of their electrical engineer leader, Frederick Terman, Stanford's electrical engineering department built a myriad of radio communications systems around their jewel, the klystron. Gradually, the Stanford engineers transformed the klystron from a fascinating, isolated tube to a standardized component within a whole gamut of microwave circuits.⁴²

On a technical level, the microwave klystron-based research continued unabated into the early years of World War II: weapon innovations included instrument landing systems and doppler radar. But dramatic changes quickly accompanied the increased pace, scope, and funding of laboratory work. Already in April 1942, Paul Davis (Stanford's general secretary) was writing Terman that "there are many things that could be done under the pressure of the present war situation that will be more difficult to achieve in peace times"—including ambitious plans for electrical engineering. In August 1942, Stanford issued its "Proposal to Organize the Stanford Resources for Public Service," focusing on how to organize "a vastly augmented program of service on a contractual basis."⁴³

After listing suggested projects (from surveys of mineral and industrial resources to the creation of psychological warfare tunes, such as "Marching Civilization"), the August proposal turned to the effects of war work on Stanford. A radical increase in contractual research would provide an opportunity to reorganize faculty administration and to improve the physical plant for more effective war and post-war work. Substantial contracts would bring federal war priority, keeping faculty on campus, creating interdisciplinary research, and engaging a new cadre of talented students who would stay on after the war. Moreover, working on contracts would make Stanford stu-

⁴² On Stanford's early combination of physics and engineering see the excellent article by S.W. Leslie and B. Hevly, "Steeple Building at Stanford: Electrical Engineering, Physics, and Microwave Research," *Proceedings of the IEEE* 73 (1985): 1169–80. [see more recently Stuart W. Leslie, *The Cold War and American Science* (New York, 1994).]

⁴³ P. Davis to F. Terman, 18 April 1942, Terman Papers, SC 160, 1:1:2, Stanford University Archives (hereinafter SUA); unsigned typescript, "A Proposal to Organize the Stanford Resources for Public Service," 24 August 1942, Terman Papers, SC 160, 1:1:2, SUA.

dents known to public and private agencies, and might contribute to the long-term development of the West. But above all, government-sponsored research would rocket Stanford to a position comparable to Harvard, Chicago, Caltech, the University of California, and Columbia by bringing in “substantial additional income.”⁴⁴

Ironically, while Stanford did greatly expand during the war, its great advocate, Frederick Terman, spent the war years on the East Coast as director of the Radio Research Laboratory in Cambridge (RRL), a facility built to produce radar countermeasures. As the new laboratory grew into a powerful organization, Terman became ever more conscious of the models that Harvard, MIT, and RRL itself presented for the postwar situation at Stanford. He was also deeply impressed with many of the administrators from Harvard—especially with his neighbor, William Henry Claffin, Jr., Harvard’s treasurer.⁴⁵ Terman was quite keen for Stanford’s general secretary to speak with Claffin, and the Stanford engineer-administrator soon sought and arranged a meeting between Donald Tresidder, Stanford’s president, and Claffin. By December 1943, Terman had concluded that

the years after the war are going to be very important and also *very critical ones* for Stanford. I believe that we will either consolidate our potential strength, and create a foundation for a position in the West somewhat analogous to that of Harvard in the East, or we will drop so a level somewhat similar to that of Dartmouth, a well thought of institution having about 2 per cent as much influence on national life as Harvard.⁴⁶

Terman went on to set out a plan to “lick” Caltech by equaling it in the physical sciences—since “after all they are only a specialized school, and Stanford is a complete university.” In part, Terman wanted a “technical institute” that would create a joint identity among scientific and engineering fields. This alliance would aid in attracting and placing students, raising money, and creating an identity based on special Western areas of strength. One such field was the characteristically Western oil industry, which would link geology, heat transfer, and chemical engineering with the radio industry and accompanying research. Moreover, Terman argued, the competition was

⁴⁴ *Ibid.*

⁴⁵ Terman to Davis, 23 August 1943, Terman Papers, SC 160, 1:1:2, SUA.

⁴⁶ Terman to Davis, 29 December 1943, SC 160, 1:1:2, SUA; partially cited in Leslie and Hevly, “Steeple Building” (note 42).

softening: Caltech had become “smug,” leaving “cracks in its armor” by not developing electrical engineering, and Harvard, Yale, Columbia, and Princeton had thus far slighted the applied sciences in favor of natural philosophy and the humanities.⁴⁷ The codevelopment of electrical engineering and physics was the hallmark of Stanford physics, as it passed from the Microwave Laboratory to the High Energy Physics Laboratory, and eventually to the two-mile-long Stanford Linear Accelerator.

The Continuity of Technique and Discontinuity of Results

On many levels, then, physics began to change *during*, not after, the war at institutions like Harvard, Princeton, Stanford, and Berkeley. Nonetheless, there is a natural tendency among physicists and historians to overlook the continuity between wartime weapons development work and postwar research, and to reach back before the war to points of common peacetime research. The difficulty may stem from an understandable focus solely on *results*, ignoring the techniques and practices of the discipline. Contributing to the physicists’ inclination to elide the effects of war on research is the preponderance of theorists among those who have narrated the discipline’s history.

It may also be that war/postwar continuities are slighted because after the war the physics community found itself divided over the opportunities and hazards of the links to weapons research. Physicists walked a tightrope, using government funds to build the machines and teams they needed, but at the same time trying to reestablish a domain of work free from the constraints of a too-closely directed and supervised research. The struggle to maintain that independence also contributed to a vision of the history of physics as, in a sense, skipping lightly over the war years.

But whatever the source of this hesitancy in tracing the continuity of wartime to postwar research, we have inherited a broken narrative. Let me illustrate this point by focusing on the work of the long-productive cosmic-ray physicist Bruno Rossi.

Rossi, an important contributor to cosmic-ray physics before the war, to the war effort itself, and subsequently to high-energy physics, offered the following recollection:

⁴⁷ *Ibid.*

In 1939, a systematic investigation of air showers was initiated by Auger and his collaborators. Their work, still carried out by means of Geiger-Muller counters, produced results of very great significance. However when, in the late 40's, air shower work was resumed, it became clear that, in order to substantially advance and refine these studies, more sophisticated kinds of detectors were needed.⁴⁸

If attention is paid only to the specific results of air-shower research, Rossi's comment makes perfect sense. But instead of halting our historical inquiry at that point, let us descend to a "lower level" of analysis—that is, let us focus on the instruments and techniques of the work in question.

Many of the instruments developed after 1945 to detect air showers were fundamentally linked to war work. In Rossi's particular case, this link was abundantly clear since he, with H. Staub, literally wrote the book on the subject. Their volume, *Ionization Chambers and Counters* (1949), was produced for the National Nuclear Energy Series, Manhattan Project Technical Section. It summarized the advances in electronics and detectors that issued from the radar and bomb projects. Starting in July and August of 1943 Staub had directed a Los Alamos team in charge of improving counters, and Rossi led a group to improve electronic techniques. In September 1943, the two groups were merged into a single Experimental Physics Division group P-6, the detector group, under Rossi.⁴⁹

Roughly speaking, their task was to design and implement detector systems that could determine the type, energy, and number of particles emerging from a variety of interactions. Their principal mission was to develop ionization counter systems that functioned in four stages: a first device detected the particle by producing a small current; a second amplified the current; a third separated the signal from unwanted noise; and a final instrument counted and recorded the total number of pulses. Physicists from the two big war projects had improved electronic instrumentation in all four areas—detection, amplification, discrimination, and counting.

An ionization counter works as follows: A charged particle passes through a gas that is contained between two parallel plates at different

⁴⁸ Bruno Rossi, "Development of the Cosmic Ray Techniques," in A. Berthelot (ed.), *Colloque International sur l'Histoire de la Physique des Particules*, in *Journal de Physique*, no. 12, vol. 43 (1982), pp. C8–82. [*sic*].

⁴⁹ D. Hawkins. "Part I: Inward Trinity," in D. Hawkins, E.C. Truslow, and R.C. Smith, *Project Y: the Los Alamos story* (Los Angeles, 1983), p. 90.

voltages. Along the particle's track it ionizes gas atoms; the electrons wander toward the positive plate, and the ions toward the negative plate. If the field is not too strong, the charge deposited on one of the plates is equal to the number of ions produced. When these charges arrive at the collecting plate, the current that they produce can be amplified; the shape and height of this current can then be used to determine the charge and energy of the incoming particle.

Rossi's immediate postwar contribution to physics involved the development of fast timing circuits for cosmic rays. His work of the late 1940s built directly on the wartime timing circuits that he had used to link ionization chambers in tests of the Los Alamos "Water Boiler" reactor. For that "Rossi Experiment," as it became known, the Italian physicist set a neutron detector to register the presence of a chain reaction inside the reactor. Using a fast coincidence circuit, the experimenter could count the number of other neutrons emitted during a brief period after the start of fission. In this way Rossi and his coworkers determined the period between the emission of prompt neutrons (those simultaneous with the fission event) and the delayed neutrons.⁵⁰

By 1947 military authorities had declassified not only Rossi's electronic contributions, but a compendious batch of 270 Los Alamos technical reports. Immediately, journals on instrumentation brimmed with the new information. Even a cursory perusal of the 1947 volume of *Reviews of Scientific Instruments* indicates the depth of interest in the instrumentation that had been developed in the weapons projects.

Consider just one example from each of the four stages of measurement mentioned above. One way to find a neutron's energy was to scatter it from a hydrogen nucleus inside an ionization chamber. The recoiling hydrogen nucleus, since it is charged, ionizes other particles in the gas; these, in turn, cascade toward the negative plate. The pulse is then proportional to the number of ions, which is proportional to the energy of the recoiling proton, which is proportional to the energy of the original fast neutron. A variety of such "proportional counters" issued from the Manhattan Project, including sensitive ones that could measure the energy of neutrons traveling in a particular direction.⁵¹ Signals from devices like these could then

⁵⁰ *Ibid.*, pp. 104–107.

⁵¹ J.H. Coon and R.A. Nobles, "Hydrogen Recoil Proportional Counter for Neutron Detection," *Reviews of Scientific Instruments* 18 (1947): 44–47.

be analyzed. The simplest possible device only registered a pulse if its height came above a certain set level. In more sophisticated instruments developed at other laboratories (e.g., by the Chalk River group), separate channels were activated by pulses of varying energy. Such “pulse-height analyzers” gave an immediate energy spectrum; they were (and are) essential instruments in postwar nuclear physics.⁵² Finally, once the pulses emerged from the discriminator they needed to be counted. Here too a great deal of progress was made during the war. One such device that was designed at Los Alamos to be used with a wide variety of detectors was the “Model 200 Pulse Counter,” which, like the other devices just described, was first made public in 1947.⁵³

Physicists from the Manhattan and Radar projects disseminated their work widely. One, William C. Elmore, prepared a series of Saturday lectures that he delivered at Princeton in the spring of 1947. The Department of Physics mailed nearly 300 copies of the lectures to physicists all over the United States, and many of the Princeton physicists made quick application of the techniques;⁵⁴ Robert Hofstadter, to offer one example, recalled that his own work on the inorganic scintillation detector was strongly shaped by Elmore’s talks. The next year Elmore published an expanded version of his lectures in the journal *Nucleonics* as a four-part article, “Electronics for the Nuclear Physicist.” According to the author, the series constituted in part a “commentary on electronic instruments designed at the Los Alamos Scientific Laboratory, and now employed extensively at various university laboratories.”⁵⁵

⁵² Before the war there were essentially two ways to obtain an energy distribution. One could record the pulses photographically with an oscillograph, which was cumbersome and required large amounts of film. Or one could employ a counting circuit with a discriminator that would record the number of counts N above an energy amplitude E ; the resulting “bias curve” (N versus E) then had to be reduced after the experiment by taking $N(E) = dN/dE$, and plotting this quantity against E . See H.F. Freundlich, E.P. Hincks, and W.J. Ozeroff, “A Pulse Analyser for Nuclear Research,” *Reviews of Scientific Instruments* 18 (1947): 90–100. As of February 1947, descriptions of the other devices still had not been published: e.g., E.A. Sayle, British Project Report, January 1944 (cited in Freundlich, *et al.*).

⁵³ W.A. Higinbotham, J. Gallagher, and M. Sands, “The Model 200 Pulse Counter,” *Reviews of Scientific Instruments* 18 (1947): 706–714.

⁵⁴ Committee on Project Research and Inventions, Princeton University, “Proposal for Continuation of Research Projects in Proton-Nuclear Reactions for the Year 1948–49,” 5 March 1948, loose in box V of 5, Princeton Physics Department Laboratory and Research Files, 1929–54, PUA.

⁵⁵ W.C. Elmore, “Electronics for the Nuclear Physicist,” Parts I–IV, *Nucleonics* 2

These particular examples are only a sample of the variety of ways in which wartime ionization detectors, fast electronics, discriminators, and scalars were pursued after the war. Experimentalists used their wartime expertise to design devices for experiments with X rays, electrons, positrons, neutrons, protons, gamma rays, and fission fragments. Perhaps more influential than any of these considerations were the microwave techniques that played crucial roles in accelerator technology after the war: wave guides, transmission lines, klystrons, molecular beams. In addition, there were the benefits of the radiation laboratory efforts—better low-noise amplifiers, lock-in amplifiers, microwave oscillators, which profoundly shaped nuclear magnetic resonance techniques, radio astronomy, and microwave spectroscopy. It is not possible here to speak of the other war-bred technologies that led to calculating machines, computers, and many aspects of programming. Suffice it to say that wartime research had transformed the material culture of the physical sciences.

Collaboration, Work Organization and the Definition of Research

Thus far our attention has been on skills and the instruments of physical research. But the war left another legacy, one not captured in the new research apparatus, or even in the surplus war material that formed such an important basis for experimental work. The war provided a lesson about the nature of research that left an indelible stamp on the physicists who participated in the massive programs at the Chicago Met Lab, the MIT Radiation Laboratory, Berkeley, Oak Ridge, Hanford, and Los Alamos. That lesson concerned large-scale research organized upon complex managerial lines. So it was that just a few weeks after D-Day, Henry Smyth sketched at Princeton a proposal for a new kind of physics laboratory, one that could duplicate the scientific/engineering successes already in hand from the various wartime enterprises.

Smyth titled his July 1944 effort “A Proposal for a Cooperative Laboratory of Experimental Science,” and the document reflected on the vast changes facing physics: “The war,” he wrote,

(nos. 2–4, 1948): 4–17, 16–36, 43–55, 50–58; quotation on p. 4 of Part I. Together Elmore and Matthew Sands wrote a book, *Electronics: experimental techniques* (New York, 1949), that was widely distributed and translated into several languages.

has now reached the stage where it is desirable to make plans for the postwar period and the period of transition. The complete disruption of the normal activities of the universities and, in particular, of the scientific groups in the universities leaves the whole condition of science in this country highly fluid.

Smyth's remark strikes at a central, and often ignored point: change was facilitated to a large extent because the traditional structures of research, leave-time, personnel, teaching, and interdepartmental boundaries had been radically altered. While "normal procedures" were suspended, deeper and faster mutations could be imposed on the system than would have been possible in peacetime. As Smyth noted, the direction of those mutations would shape the very definition of a physicist, and of physical research:

Forty years ago the physicist working on a research problem usually was largely self-sufficient. He had available a certain number of relatively cheap instruments and materials which he was able to assemble himself into an apparatus which he could operate alone. He then accumulated data and interpreted and published them by himself. Most of the special apparatus that he needed he himself constructed with his own hands. He was at once machinist, glassblower, electrician, theoretical physicist, and author. He instructed his students in the various techniques of mind and hand that were required, suggested a problem, and then let the student work in the same fashion under his general supervision.

But even before the war, Smyth pointed out, physics had been growing more complex. Large laboratories had begun adding specialized technicians to their staffs, including glassblowers and machinists. Even graduate students came to depend on these technicians. Devices such as grating spectrographs dwarfed in size and complexity the simple table-top devices that previously had been sufficient. Thus it was that "a certain amount of cooperation in the use of such installations had to be worked out. But even twenty years ago research problems were largely individual." Only between 1930 and 1945 had this predominantly individual research dwindled, as equipment grew larger, more expensive, and sufficiently hard to handle that it came to require a team of workers to run. Of all such devices, the cyclotron stood out as the most dramatic example, costing in some cases more than the prewar physics budget of fifty laboratories.⁵⁶

⁵⁶ H.D. Smyth, "A Proposal for a Cooperative Laboratory of Experimental

For Smyth such developments held dangers, as well as promises. If every university aspired to build cyclotrons or betatrons the costs could prove overwhelming. There was a danger that only a few elite institutions would be left in command of research and that consequently the “background of strength in science which [had] grown up so successfully in the country in the past twenty years” would be weakened. Only cooperative research, he felt, could salvage the situation by consolidating various universities and “other institutions” into centralized enterprises.⁵⁷ In a February 1945 revision of his document, Smyth added that such big projects should not “oppress the individual scientists. Such installations must be the servants, not the masters of the research man.”⁵⁸

The OSRD experience of physicists carried, Smyth argued, three lessons: First, the importance of “fundamental science” in solving problems that mere “specialists” could not handle. “The moral which is to be drawn from this experience is that the ultimate technological strength of the country, even for military purposes, rests on men trained in fundamental science and active in research on fundamental problems of science.”⁵⁹ Second, the war demonstrated the profits that could accrue from “large cooperative research enterprises,” cooperation that would extend not only to other scientific fields such as chemistry, biology, and medicine, but to the deep links between physicists and engineers.⁶⁰ The latter was an alliance with roots dating from before the war, but which bore fruit only in the wartime efforts. Although for security reasons Smyth passed over it, the obvious reference of this section of his proposal is to the Manhattan Project; at the time the revised proposal was written, in February 1945, Los Alamos was only a few months from detonating its first nuclear weapon.

The laboratory of Smyth’s dreams clearly echoed a reality that lay, still secret, in the New Mexican desert. He figured 300 square feet per physicist, about 100 physicists, and about 15 foot ceilings, along with 5,000 square feet for large installations. This yielded

Science,” 25 July 1944, file “Postwar Research 1945–46,” Physics Department Departmental Records, Chairman, 1934–35, 1945–46, box I, PUA.

⁵⁷ *Ibid.*

⁵⁸ H.D. Smyth, “A Proposal for a Cooperative Laboratory of Experimental Science,” revised version, 7 February 1945, file “Postwar Research 1945–46” (note 56).

⁵⁹ Smyth, revised version of “Proposal,” p. 3 (note 58).

⁶⁰ Smyth, “Proposal,” versions of 25 July 1944 and 7 February 1945 (notes 56, 58).

525,000 cubic feet, which at \$0.70 per cubic foot would have totaled \$367,500. Industrial production provided the architectural prototype: “The laboratory should be essentially of factory-type construction, capable of expansion and alteration. Partitions should be nonstructural.” And with a democratic flourish Smyth appended his intention that “paneled offices for the director or any one else should be avoided.”⁶¹

Soon Smyth found his thoughts echoed in his colleagues’ memoranda. One physicist was

genuinely concerned that in the Atlantic coast region we have some possibilities in this field [of cooperative nuclear research] and that all of the government support is not thrown to those other sites which have a prior claim, of course, because of existing facilities.⁶²

Wheeler also reacted with enthusiasm, in December 1945, to the idea of a multi-university collaborative enterprise, and had no doubt that universities were owed support by the federal government:

Anyone familiar with work on nuclear physics and its applications to military and peace time uses is aware that progress in this field in the United States has now dropped to a very low rate. Scientists are leaving to go to laboratories where they can have conditions of freedom appropriate for independent investigations. The country is losing out because it hasn’t been able to work out a system suitable to enlist the participation of the scientists. In addition to this problem of applying science in the country’s service, there is also the problem of what the country can do to replenish the scientific capital on which it drew so heavily during the war. The universities paid in years of peace for the fundamental research of which the government took advantage in time of war. The universities need and can rightly call for government support in the future.⁶³

Such support should come in the form of engineering assistance, and equipment, Wheeler argued. He preferred a system in which the government financed and ran a facility where university researchers could bring their cloud chambers or magnetic spectrographs, make some measurements, and return to their home institutions. A self-administering center would therefore not burden academics more

⁶¹ Smyth, revised version of “Proposal” (note 58).

⁶² William W. Watson to Smyth, 23 June 1945, file “Postwar Research 1945–46” (note 56).

⁶³ J.A. Wheeler, draft of “Proposal for Cooperative Laboratory,” 11 December 1945, file “Postwar Research 1945–46” (note 56).

than necessary. For support Wheeler looked to the men and institutions that had built such laboratories in the past: the Manhattan Engineer District, Vannevar Bush, and industrial concerns.

Over the next half year the Princeton physicists combined forces with others in the Northeast to draft a proposal for a nucleonics laboratory that would cost about \$2.5 million (it soon increased to \$15 million, then to \$22 million, and finally to \$25 million). Blessed with support from General Leslie Groves and the Manhattan Project, the planning staff of the budding Brookhaven National Laboratory recruited the building and management expertise of Hydrocarbon Research Inc. In addition, and to the consternation of Oak Ridge, the planners explicitly resolved to crib experience and information from the proven facilities at Oak Ridge.⁶⁴

Unlike reactors, which were obviously too big for most educational institutions, cyclotrons hovered for the next few years at the boundary between being too big for universities and too small to merit their own cooperative laboratories. Indeed, Brookhaven's initial attempts to get its accelerator division funded were unsuccessful. Later, when synchrocyclotrons appeared, the Brookhaven reactor laboratory served as a prototype for collective research at the accelerator, and the model was soon extended elsewhere in the United States and then to Europe as a template for CERN. War laboratories thus clearly provided the managerial models, the technical expertise, and even the personnel for the establishment of postwar collaborative laboratory work.

Concern about the impact of a big cyclotron on university physics is visible in the case of Princeton. Milton G. White reported to Smyth in December 1945 that many members of the department were keen to find a place for themselves at a cyclotron facility, but remained a bit apprehensive about the nature of the research that awaited them:

“Dicke is leaning heavily toward elementary particle physics, but not too anxious to press for high energy if the engineering must come out of his hide. He wants to help get the cyclotron under way and then work on some simple interactions.” Therefore, to get the

⁶⁴ For my discussion of the origin of BNL I have relied on the excellent article by Allan Needell, “Nuclear Reactors and the Founding of Brookhaven National Laboratory,” *Historical Studies in the Physical Sciences* 14 (1983): 93–122.

accelerator program “back on its feet,” White wanted “*very* much to acquire someone who would attend to moving, wiring, redesign problems.” In addition, he wanted one of those sought-after types, “a Los Alamos man.”⁶⁵

More generally, White foresaw the need to create new positions for the changed environment of the large-scale laboratory, ones outside the traditional academic hierarchy from assistant professor to tenured full professor. “On the one hand,” White reported,

we find physics research going in the direction of complex equipment requiring a supporting staff of highly competent, broadly trained physicists, engineers, chemists and administrative personnel; while on the other hand we have the customary university policy of regarding all scientific employees as likely candidates for academic positions.

Instead of hunting for the “well-rounded” man appropriate to academia, White advocated a Division of Research that would hire with soft money provided in part by endowment, but significantly supplemented by industrial and government funds.⁶⁶

White was concerned about on-campus accelerator physics, whatever might come of proposals for the cooperative laboratory. And expected costs for the cyclotron clearly were going to be high: from \$100,000 to \$300,000. Size would also be significant, since forecasts called for at least 75,000 square feet. Using Smyth’s estimate quoted earlier, a plant of this dimension would run at least another \$825,000. In all, White forecast expenses of around \$100,000 per year for the next five years, and even this sum was exclusive of the building and power requirements of the accelerator.

No crystal ball is required to outline the trend in high energy physics—the trend is up! In not more than six months it should be possible to settle on the part we wish to play in high energy physics, and having settled this we must pick some one accelerator scheme and back it for all we are worth.⁶⁷

Faced with such skyrocketing costs, White simultaneously advocated a cooperative nuclear physics laboratory, with funding in large part

⁶⁵ M.G. White to H.D. Smyth, 20 December 1945, file “Postwar Research 1945–46” (note 56).

⁶⁶ M.G. White to H.D. Smyth, 6 May 1946, file “Postwar Research 1945–46” (note 56).

⁶⁷ White to Smyth, 20 December 1945, file “Postwar Research 1945–46” (note 56).

to be provided by private industry—citing, for example, the Monsanto Company.⁶⁸ Unfortunately for White's plans, Monsanto declined.

Stanford, by contrast, had already been successful at linking physics, engineering, and private investment. As Stuart Leslie and Bruce Hevly have shown, such scientific entrepreneurship had begun several years before the war.⁶⁹ Building on Stanford's engineering/physics of the 1930s, in November 1942 William Hansen began advocating the establishment of a "Stanford Microwave Laboratory" to be headed by Karl Spangenberg, an electronics specialist, and drawing on the consulting expertise of H. Skilling and Felix Bloch. When peace came, Hansen wrote the physics department chairman, Paul Kirkpatrick, the microwave lab would draw further staff and equipment from Physics and Electrical Engineering. "At this point, this laboratory goes out and gets a government contract for some microwave job. There should be no difficulty in doing this. With this job will come a priority. . . . Then you start spending money and also, of course, doing the job." Hansen hoped to establish as many attractive fellowships as could be filled, and then to order "equipment—of a sort that will be useful after the war." This should include "machine tools, measuring equipment, books, and any other things that can be used to generate apparatus or research." Played well, the plan would guarantee the "even if we don't have a dime after the war, good physics can be done."⁷⁰

Almost exactly a year later, in November 1943, Hansen elaborated on his initial scheme. It had already been "obvious" before the war that physicists would play a crucial role in industry and that radio engineering (including electronics) was in for rapid expansion. But,

while these trends were obvious before the war, the war has both accelerated and called attention to them. This is especially easy to notice . . . in the field of radar . . . The result will be that, after the war, all major universities will be forced to offer strong instruction in these two branches of science.

⁶⁸ White to Charles Thomas, Monsanto Chemical Company, 18 September 1945, file "Postwar Research 1945–46" (note 56).

⁶⁹ See Leslie and Hevly, "Steeple Building" (note 42).

⁷⁰ Hansen to Kirkpatrick, 6 November 1942, Terman Papers, SC 160, 1:1:7, SUA (note 43).

Precisely because it could strengthen and link the two domains, the microwave laboratory would prove essential. Modeled on the Berkeley Radiation Laboratory, it would remain under the control of the physics department, although the director would control the budget. Indeed, by establishing the laboratory with university funds, the inevitable private support would not dictate the direction of work.⁷¹

Thus by the war's end in August 1945, the microwave laboratory had been in gestation for nearly three years. And with the atomic bomb project passing from top secret to a national obsession, the physics department scaled up its requests. Now, in October 1945, discussions of physicists and physics took on a new, assertive tone, unheard before the war. Of Norris Bradbury, whom Kirkpatrick wanted to lure back to Stanford: "He is thus the head of the group that changed all human history, a continuing group whose power to effect such changes is by no means exhausted."⁷² Even Bradbury, it had become clear, had to struggle to keep the staff together at Los Alamos—and the salaries across the country had simply skyrocketed. As an indication of the precipitous rise, Stanford offered one physicist a salary of \$3,750, which was met immediately with a \$6,000 counteroffer from the University of Chicago. In fact, of a sample of fourteen physicists (almost all between 31 and 41), the *average* salary was \$8,460, with a high of \$15,000 and a low of \$6,000. "Whether one likes it or not [this salary level] reflects the present pronounced bull market in physicists, which has naturally resulted from a short supply and a heavy demand."⁷³

Now the Physics Department could address a memo to the president of the university that bluntly asserted in its first paragraph: "It appears to be the manifest destiny of this department to expand." Unanimously the Stanford physicists brought forth several "considerations": (1) ROTC and NROTC students were going to be taking physics courses, as were soldiers in the Army's officers training program; (2) the Consulting Committee on Undergraduate Studies was considering requiring new physics courses; (3) the federal government was probably going to subsidize science students, and "physics

⁷¹ W. Hansen, "Proposed Micro-Wave Laboratory at Stanford," typescript, 17 November 1943, Terman Papers, SC 160, 1:1:8, SUA.

⁷² Kirkpatrick to Tresidder, 1 October 1945, Tresidder Papers, SC 158, 1:4, SUA.

⁷³ *Ibid.*

will be the first science affected”; and (4) the microwave program was going to draw students, and indeed the whole “war record of physics is causing students to plan careers in this science.” Whereas before the war about one freshman per year indicated he wanted to major in physics, the figure now stood at eleven, and would surely rise further with the influx of veterans. On the basis of these facts, the department welcomed “a chance to enlarge the department” from six, to eight or nine on its permanent staff.⁷⁴

Enlargement by 50 percent would cost money, and as Hansen had advocated during the war, the source was government-contracted research. Clearly this benefited the universities, but how so the government? Colonel O.C. Maier of the Air Technical Service command put it concisely to Stanford’s president, in January 1946. The Army Air Forces, Maier wrote, had two purposes in continuing close cooperation between universities and the military: “We would not only get a good deal of our work accomplished by capable personnel, but in addition build up a pool of trained engineers and scientists, who could be of assistance in War Department research in case of emergency.” The choice of tasks spanned the gamut of microwave research: propagation in low, high, ultra-high, very-high, and microwave frequencies; modulation systems, including accurate timing for radar applications; pulse modulators; broadbanding of antennas and circuit elements; magnetrons and klystrons; research on millimeter waves; three-dimensional radar data presentation, and beacon communication; moving-target indicator research; random polarization jammers; flight computers; navigation systems; loran research; and novel radar systems.⁷⁵ And this was just one prospective offer from, one branch of the armed services—supplemented significantly by the Atomic Energy Commission, which soon took over contracting from the Manhattan Project. Within just a few years, workers at Stanford would be moving easily between classified, applied research and the open domain of academic studies.

⁷⁴ Kirkpatrick to Tresidder, 6 November 1945, Tresidder Papers, SC 151, 25:1, SUA.

⁷⁵ Colonel Oscar C. Maier to Office of the President, Stanford University, 11 January 1946, Tresidder Papers, SC 158, B1:4, SUA.

Conclusion: War and the Culture of Physics

For years we have treated the history of physics as if it simply stopped between 1939 and 1945; only the movement of refugee scientists, bomb building, and the federal administration of a dramatically larger science budget have commanded attention. But if we are going to understand the deeper implications that the conflict had for modern physics and, by extension, for all of the modern sciences, we must look to the techniques and practices of the discipline. In this paper I have followed five lines of continuity: the transfer of technology, the transfer of support, the realignment of physics and engineering, the new relation between theoretical and experimental physics, and the reorganization of the scientific workplace.

Technological transfer consisted, in part, in the invention of new devices: novel accelerator technology, such as the klystron and strong focusing. The new electronic technology of counters, timers, amplifiers, and pulse-height analyzers (among others) all contributed to the post-war burgeoning of the physics of nuclei and particles. But beyond pure invention, the war increased the industrial production of high-performance components. In turn, this capacity made tools available to the experimenter that had previously necessitated custom manufacture. Finally, there was technological transfer at the most literal level—great storehouses of equipment, hundreds of millions of dollars' worth of machinery, that the federal government shipped directly from war-designated activities to the civilian sector as surplus. From the lone researcher picking up a microwave generator in Europe to the best universities of the United States, this infusion of tools and machining equipment transformed the scope and capacities of post-war research.

The financial support for the discipline of physics had also completely altered. The Manhattan Project continued to underwrite many activities, and when it finally closed shop, its sponsorship was quickly taken over by OSRD, ONR, and then by the AEC. Most importantly, at each of the universities discussed here (Princeton, Harvard, Berkeley, and Stanford) it is strikingly clear that the war had trained academic physicists to think about their research on a new scale, invoking a new organizational model. Not only did physicists envision larger experiments than ever before, they now saw themselves as *entitled* to continue the contractual research that both they and the government had seen function successfully during the war. This

way of thinking molded both the continuation of wartime projects and the planning for new accelerators and national laboratories. Across the country, with occasional strong dissent, and with different emphases in different regions, the physics community strove to reenact the trilateral collaboration among government, university, and private enterprise.

The expanded institutional base of research permitted more complex relations between theorists and experimentalists, and between physics and engineering. These collaborative relations were firmly established at the huge project centers for scientific warfare: in the Metallurgical Laboratory of Chicago, in the vastly augmented Berkeley Radiation Laboratory, at Harvard's Radio Research Laboratory, in the rocket plants of Caltech, at the MIT Rad Lab, at Oak Ridge, and at Hanford. So it was that when the war ended, it was altogether natural for a Brookhaven, a Stanford Microwave Laboratory, or a rejuvenated Berkeley Radiation Laboratory to assume—from the start—a style of physics that elevated the role of theorists in the shaping of research programs, while keeping scientific engineering front and central. Through these new alliances, the great particle physics laboratories could be built, and the new relationship with the military justified.

Finally, and perhaps most importantly, the war changed physicists' mode of work—in the process redefining what it meant to be a physicist. As Smyth put it so eloquently, forty years earlier the physicist had been at once machinist, glassblower, electrician, theoretical physicist, and author. Just a few years after the war all that had changed: consolidating a prewar trend, the new breed of high-energy physicists were no longer taught to be both theorists and experimentalists—they chose one path or the other. In the place of physicist-craftsmen arose a collaborative association among theoretical and experimental physicists and engineers of accelerator, structural, and electrical systems.

One consequence of these interrelated transformations was a marked shift in rhetoric. The new, often triumphalist tone contained elements of pride in the physicists' contribution; it also signaled a defensiveness, as the scientists struggled to legitimate government funding while avoiding tight restrictions on the prosecution and dissemination of their research.

But the changes in the material culture, organization, and goals of physics went far beyond new turns of phrase. Collectively, these

various factors gave rise to a new style of research in nuclear and particle physics. Schematically, it is useful to think of this “industrialization” of university accelerators and national laboratories as having occurred in two stages. The first stage, discussed here, involved an upheaval in the laboratory environment in which nuclear physicists worked. This transformation of the “outer laboratory” began with Lawrence’s prewar forays into big science, but became the norm of nuclear physics only in the years 1943–48. In the following decade, the scale change of physics would reach even further into the conduct of experimental high-energy physics. With Alvarez’s massive hydrogen bubble chambers, the “inner laboratory” itself, the micro-environment of the experimentalist’s measuring and calculating devices, grew, like the outer laboratory, to industrial size.⁷⁶

When Henry Smyth solicited a laboratory with “factory-type construction” using “non-structural partitions,” and a director’s office “without paneling,” he was advancing a straightforward architectural request. But in those plans were other, less visible architectures. From the war, physicists had inherited a new sense of mission-directed, team-executed research that required a new human architecture as well—specialization, and collaborations with well-defined leaders (as evident from remarks like Wheeler’s, as he speculated on postwar physics). These directors were to lead interdisciplinary nuclear physics programs that, with their movable partitions, could shift priorities as new instruments or questions arose. Gone were the days when Palmer Hall at Princeton or Jefferson Laboratory at Harvard could devote small rooms purely to acoustical or magnetic research. And of course the new research would find its natural place in the lavishly-funded regional laboratories where—as in the war projects—university, industry, and government would work as a triumvirate.

At the same time, one senses in the postwar architectural plans an apprehension about the new physics—a sense that the leaders should not isolate themselves or stifle the working physicist. From many of the postwar physics planners one feels a deep tension, socially

⁷⁶ For more on the “inner” and “outer” laboratories and an extended discussion of the transformation of the inner laboratory in the 1950s, see P. Galison, “Bubble Chambers and the Experimental Workplace,” in P. Achinstein and O. Hannaway (eds.), *Observation, Experiment, and Hypothesis in Modern Physical Science* (Cambridge, Mass, 1985), pp. 309–373; and Galison, “Bubbles, Sparks” (note 21).

and intellectually. between the power available through collaboration and the ideological commitment to individual research. It is a tension only incompletely resolved through the Los Alamos model of large-scale, hierarchical teamwork where the physicist could argue with those up the line. Even the director of the huge National Accelerator Laboratory would later write an autobiographical essay entitled “My Fight against Team Research.”⁷⁷ When Smyth sat down to plan a regional laboratory he assumed there had to be a director’s office—but without paneling.

Plans for physics after the war constituted more than a shift in research priorities; they were *simultaneously* reflective of the wartime projects and *determinative* of the future direction of big physics. Whether physicists turned for guidance to the Met Lab or to the Rad Lab they were constructing a new culture by supplanting the guiding symbols of research. No longer could the image of laboratory work come from the precision interferometry studies of an Albert Michelson. Now, the cultural symbol of physics would originate in a Los Alamos, a Brookhaven, or a National Accelerator Laboratory.

In speaking of the guiding role of cultural symbols, I have in mind something similar to the role Clifford Geertz accords symbols—agreed-upon programs for future action, not mere emblems such as flags.⁷⁸ It is in this more robust sense of the term that the weapons projects were symbols. In the context of America in the mid-1940s, the Manhattan Project was far more than an indicator of the usefulness of physics; it was seen as a prescription for the orchestration of research. As a representation of how technical, physical, military, and political activities could coalesce, the wartime laboratory became the site for a mutation in the culture of physics.

After the fact, it is hard to grasp how abruptly physics was transformed from one among many university activities, all roughly on a par, to a massive enterprise that was consulted on subjects from university decisions to foreign policy. Suddenly, physics had both wealth and power. And physicists looked to their wartime experience not only to legitimize continued funding, but also for the administrative and work relationships that would govern them and succeeding gen-

⁷⁷ R.R. Wilson, “My Fight against Team Research,” in G. Holton (ed.), *The Twentieth-Century Sciences* (New York, 1972), pp. 468–479.

⁷⁸ Clifford Geertz, *The Interpretation of Cultures* (New York, 1973), esp. pp. 44ff.

erations of scientists. As a whole, the physics community had constructed a new identity for itself in the turbulent years between war and peace.⁷⁹

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2004 Postscript to "Physics Between War and Peace"

One day historians will look back on World War II and the Cold War as a single conflagration, a planetary war lasting half a century. The destruction that it wrought is incalculable, its effects still all-too visible in both the physical and political worlds. Flashpoints of conflict in the early twenty-first century lie like a thin crust of frozen lava over fault lines set years ago. But for better or worse,

⁷⁹ While the organizational features of the World War II weapons projects endured into postwar "pure" science, the extraordinary political consensus that bound the civilian physics community to the defense physics establishment did not. During the 1950s, for many reasons, the two scientific groups began to bifurcate. Some of these reasons were institutional—the slow decline of the General Advisory Committee, the rising capability of weapons laboratories outside universities; some were political—splits over the hydrogen bomb, the ABM system, the role of secrecy, the cold war; and some were physical—high-energy physics decisively split from nuclear physics both theoretically (*e.g.*, current algebra, field theory), and experimentally with the exploitation of devices that were useful in one field but not in the other (*e.g.*, bubble chambers). This is not to say that the two communities should be seen as completely autonomous—links remained through advisory panels, students, funding sources, and, most of all, shared technologies. But in the decades following the atomic bomb, the nature of the connection between the civilian and military scientific establishments changed from one of joint enterprise to one of shared resources. I wish to pursue these issues elsewhere.

the infrastructure of science was put in place during this Long War. The government-funded, company- (or university-) operated contract system dates from the war—this may have begun in places like Oak Ridge, Hanford, and Los Alamos, but it now reigns too at Brookhaven, Sandia, Fermilab, and many smaller sites. Demobilization lasted but a heartbeat; permanent mobilization of a massive military beginning with the Korean War in 1950 set the scale and pace of American science, especially physics.

Since I wrote this essay, the Cold War ended. On one hand, that epochal event seemed to alter everything for physics. Sure, there were many contributing reasons for the cancellation of the Superconducting Supercollider. There were fiscal problems that led to a revision of the budget-upwards—at a particularly bad political moment. There were divisions within the physics community itself: long-simmering tensions between particle and condensed matter physics came to the fore. There were missteps by the physicists in presenting their case both on the floor of Congress and to the broader public. But below these arguments on the deck of the Titanic, the iceberg tearing a great rip through the side of the ship of physics was the Cold War's end. Symbolically and substantively, the termination of this accelerator of all accelerators marked a deep wound to the long march inwards that had taken molecular to atomic to nuclear to particle physics. The Manhattan Project had cast a long shadow, for even when the physics of quarks was self-evidently of little concern to weapons-makers, the tie between national defense and the prestige center of physics was powerful. Techniques, students, equipment, summer studies—long after Hiroshima, particle physicists saw themselves at the pinnacle of the discipline, with all that accompanied the role. Then, suddenly, with the cancellation of the SSC, the prestige center of physics was punctured.

At just about this moment (early 1990s), the surrounding domains of physics were growing enormously. Astrophysics and cosmology pulled in an ever-increasing number of physicists who could build similar detectors pointing upwards instead of along the beam-line. The National Institutes of Health budget flew upwards like a home-sick angel, while the physics budgets remained flat—at every intersection of physics and biology one could find physicists, newly-minted Ph.D.s and veterans from the 1970s moving their interests and labs. Nanotechnology beckoned—that intellectual Four Corners where physics, biology, chemistry and engineering made common cause.

All this—with a bit of perspicacity—could have been seen in the early 1990s. Less easy to discern was the continuation of the National Security State, at least in the forms it has taken.

In 1992, it seemed possible that the military budget of the United States might be on the verge of a steep downward spiral—with the force against which it had been positioned, defunct. Several major developments first froze and then reversed this trend. First, the Complete Test Ban Treaty effectively stopped nuclear weapons testing. But when the weapons laboratories were asked if they could guarantee the efficacy of the nuclear force absent testing, the labs replied that they could do only with a hugely augmented simulation program. Bottom line: the bottom line of weapons laboratories went up following the end of real detonations. Second, the shift from a Democratic to a Republican administration in 2000 already began a process of military re-armament; when the World Trade Center and Pentagon were hit in September 2001, the upward trend of funding for high-tech military acquisitions became a certainty.

If I were continuing the kind of analysis I began in “Physics Between War and Peace,” these are some of the considerations I’d have in view. That is, methodologically, I’d continue to look for conjoint efforts between the military and the civilian sector. But topically, I’d be looking now at nano-technological research in smart clothing, physico-bio-medical interventions, cryptography, and other trading zones between physics and matters of war, commerce, and bio-medicine. What I would not do is to resurrect the ever more obviously useless categories of “pure” and “applied” science. I would not search desperately, in idle oscillation, between the view that science was driven by snow-white abstractions and the view that knowledge was nothing but the surface effects of deep, dark, and nefarious applications of military ambition. Our world is too complex, too interesting, too dangerous for such reductionistic fantasies.

I am glad that the authors and editor have put together their thoughts for this volume. We need them: now more than ever.

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