Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940-1960
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Behind quantum electronics: National security as basis for physical research in the United States, 1940–1960.

> The tremendous military research and development program will continue as far as I can see, for a long time in the future. It furnishes the background and a large part of the stimulus for this rapidly rising truly scientific age. Asst. Secretary of Defense C.C. Furnas, 1956.

> Previously the primary question was: what pattern of civil-military relations is most compatible with American liberal democratic values? Now this has been supplanted by the more important issue: what pattern of civil-military relations will best maintain the security of the American nation? Harvard Professor Samuel P. Huntington, 1957.

> In order to acquire the venture capital for expansion, the control of science has been transferred from scientists themselves to people who want to use it. Geophysicist, Henry W. Menard, 1971.

TODAY THE LASER is the centerpiece and symbol of the largest, farthest-reaching scientific-technical initiative since the space program was launched 30 years ago. Not the Strategic Defense Initiative alone, but the pattern of priorities in America of the 1980s, within which "Star Wars" fits so naturally, thrusts our attention toward the national security contexts of quantum electronics. Judged by the conscience

*National Museum of American History, Science, Technology, and Culture, Smithsonian Institution, Washington, D.C., 20560. I thank F. Aaserud, D.K. Allison, J.L. Bromberg, P.K. Hoch, D.J. Kevles, A.Q. Morton, A. Roland, and especially R.W. Seidel, for critical, challenging comments on drafts of this essay. D.F. Noble, in his work and his person, has been my chief inspiration in its conception and prosecution. The documentation, especially the statistical documentation, in the abundance here presented would not have been possible without research assistance rendered by J.E. Beichler.

The following abbreviations are used: AEC, Atomic Energy Commission; *BAS*, *Bulletin of the atomic scientists*; BOB, Bureau of the Budget; DOD, Department of Defense; LC, Library of Congress; NAS-NRC, National Academy of Sciences, National Research Council; NSF, National Science Foundation; *PT*, *Physics today.*

and consciousness of the American nation, the decade from the outbreak of war in Korea to the advent of the laser forms an epoch more similar in several respects to the 1980s than to the two intervening decades. Especially in the role assigned physical research, America in the eighties is reminiscent of America in the fifties.[1] That at any rate is the conclusion of this assay of the shape and purpose of the U.S. research enterprise, particularly in physics and electronics, from 1940 to 1960. My thesis is that here American physics, accelerating its historic quantitative growth, underwent a qualitative change in its purposes and character, an enlistment and integration of the bulk of its practitioners and its practice in the nation's pursuit of security through ever more advanced military technologies.

The picture drawn here may be surprising to those of us who have viewed science with much the same ideological commitments as those most devoted to its pursuit. We, like them, saw the enormous expansion of basic physical research after World War II as a good in itself, a praiseworthy diversion of temporal resources to transcendental goals. We, like them, have been pleased to suppose that science was here using society to its own ends.[2] Those, on the other hand, who in this

1. D. Dickson, *The new politics of science* (New York, 1984); P. Stares, *The militarization of space: U.S. policy, 1945–1984* (Ithaca, 1985). Between FY '80 and FY '85, the national defense share of the Federal budget increased by more than 25%, and its share of Federal R&D funds by more than 40%. The director of the National Science Foundation recently observed: "In the past 5 years the balance has shifted strongly toward the defense side again. At present, only a little more than a quarter of the Federal R&D effort goes into civilian research." E. Bloch, "Basic research and economic health: The coming challenge," *Science, 232* (2 May 1986), 595–599. The parallel between the 1950s and 1980s is most striking in the graph of U.S. Government expenditures on military nuclear energy activities, 1940–1990, given by R.S. Norris et al., "History of the nuclear stockpile," *BAS, 41:7* (1985), 106–109. The parallel appears also in restrictions on the freedom of scientific intercourse: M.B. Wallerstein, "Scientific communication and national security in 1984," *Science, 224* (1984), 460–466; R.L. Park, "Intimidation leads to self-censorship in science," *BAS, 41:3* (1985), 22–25. The above manifestations, but especially this last, are considered by D.J. Kevles, "The remilitarization of American science: Historical reflections," MS, 1987.

2. "Under 'Research' and even 'Basic Research' we have encouraged and budgeted huge enterprises of essentially operational character, most of them promoted with some enthusiastic hope of great national prestige. Essentially these projects are based on the twin arguments that the U.S.A. must be first and biggest, and that tax money is not real money but just a voucher for directing the expenditure of national effort toward certain speculative goals because otherwise this effort would not be spent at all or would just be directed toward more personal goals." M.A. Tuve, "Basic research in private research institutes," in D. Wolfle, ed., *Symposium on basic research* (Washington, D.C., 1959), 169–184; apropos diversion of temporal resources, H. Orlans, "Developments in Federal policy toward university research," *Science, 155* (1967), 665–668, asked "What was it that, in 1963, led Philip Handler and Frederick Seitz [then future and present presidents, respectively, of the National Academy of Sciences], apparently independently...happily to envisage that, by the end of the century, the nation might

period provided the money for physical research understood, at least implicitly, all that I will here describe. As General Arthur Trudeau, the Army's Chief of Research and Development, explained within a year of the first halting laboratory demonstrations of lasing:[3]

> Basic research, being but the applied research of tomorrow, is the key to technological progress....There was a time when the Army could develop and produce a weapon or a piece of equipment and use it until the day it wore out. Today this era is but a memory....If yesterday's miracles are today's relics, what an age tomorrow will be, with science as the guide....Army Research and Development is maximizing the great scientific discoveries of today and continually pressing for more progress in all fields....Electronics...has seen a quantum jump in the last 10 years. Basic research already has led to the technique of microminiaturization of electronic components....Other marvels in the electronic field are the ruby maser, the tunnel diode, and the parametric amplifier promising to increase tenfold the sensitivity and range of many Army electronic systems. And now, the LASER and IRASER are in sight.

Evidently those guiding the research programs of the Department of Defense had quantum electronics clearly in sight, and felt quite confident of its pertinence to their purposes. Within another two years their funding of laser research and development would exceed 100 million 1987 dollars.[4] I aim to make that circumstance

devote to research and development as much as half of its gross national product?"

3. A.G. Trudeau, "Research for survival," American Society of Civil Engineers, *Transactions, 127:5* (1962), 20–39. Useful bibliographical introductions are A. Roland, "Science and war," *Osiris, 1* (1985), 247–272, and "Technology and war: A bibliographic essay," in M.R. Smith, ed., *Military enterprise and technological change* (Cambridge, 1985), 347–379. Pertinent also are the following papers prepared for a Conference on Science, Technology, and the Military, MIT, 8–10 Jan 1987, which have, however, come to hand too recently for due consideration here: P.K. Hoch, "The crystallization of a strategic alliance: American physics and the military in the 1940s;" D.J. Kevles, "R&D and the arms race: An analytical look;" S.S. Schweber, "The mutual embrace of science and the military: ONR and the growth of physics in the United States after World War II." H.M. Sapolsky, "Science, technology and military policy," in I. Spiegel-Rösing and D.J. de S. Price, eds., *Science, technology and society* (Beverly Hills, 1977), 443–471, has observed that while in the past it was common "to characterize military bureaucracies as being highly resistant to technological change..., today, however, the military, perhaps to a greater extent than any other bureaucratic institution, has internalized the maxim that there is a continuous need to promote and to adjust to technological change."

4. *Aviation week and space technology* (8 Apr 1963), 75. In the spring of 1962 a team of students from Harvard Business School found that in the two years since the first report of laser action the number of R&D groups in the U.S. with some involvement in this field had leapt from less than 50 to about 500. "There are some applications which appear more urgent, if not more feasible than others, and it is these applications that will dictate the main directions which laser development will take. There is,

intelligible, even obvious and natural, by bringing forward a range of interconnections between the pursuit of physics and the promotion of military technologies in the United States from the late 1940s to the early 1960s. For the scientists most intimately involved, like physicist Jerrold Zacharias, there has been no question where and how it all began: "World War II was in many ways a watershed for American science and scientists. It changed the nature of what it means to do science and radically altered the relationship between science and government...the military...and industry."[5]

Between 1940 and 1945 the convergence of science with engineering that characterizes our contemporary world was effectively launched in its primarily military direction with the mobilization of U.S. scientists, most especially physicists, by the Manhattan Project and by the OSRD, the Office of Scientific Research and Development. In FY '38 the total U.S. budget for military research and development was $23 million and represented only 30% of all Federal R&D; in fiscal 1945 the OSRD alone spent more than $100 million, the Army and Navy together more than $700 million, and the Manhattan Project more than $800 million—an increase in current dollars over seven years by a factor of more than seventy, or more than fifty in constant dollars. In the immediate postwar years total military expenditure slumped to a mere seven times its prewar constant-dollar level, while constant-dollar military R&D expenditure held at a full 30 times its prewar level, and comprised about 90% of all Federal R&D. In the early 1950s total military expenditure soared again, reaching 20 times its prewar constant-dollar level, while military R&D reattained, and before the end of the decade much surpassed, its World War II high (figure 1). These numbers reflect a radical change in attitude toward science, toward national security, and toward the relationship between them on the part of both the military and the civilian leadership of the United States.

In exploring this relationship I have followed to a great extent paths broken by Daniel Kevles ten years ago.[6] Such paths, by which

for instance, at the time of writing (April, 1962) a great deal of publicity being given to the laser as an instrument of defense....A great deal more interest and research is being devoted to the defense applications than may be commonly supposed." C.A. Hogg et al., *Masers and lasers* (Cambridge, 1963), 118, 146, 178–79. I thank Joan L. Bromberg for these references.

5. J.R. Zacharias, draft letter to A.L. Singer, 19 Dec 1984. This was recognized, and complained of, much earlier by others, notably by M.A. Tuve in ref. 2, "Technology and national research policy," *BAS, 9* (1953), 290–293, and *PT, 7:1* (1954), 6–9; and by Norbert Wiener, "Science: The megabuck era," *New republic* (27 Jan 1958), 10–11, reprinted in Wiener's *Collected works, 4* (Cambridge, 1985), 710–711.

6. D.J. Kevles, *The physicists: The history of a scientific community in modern America* (New York, 1978), 287–426, 457–464.

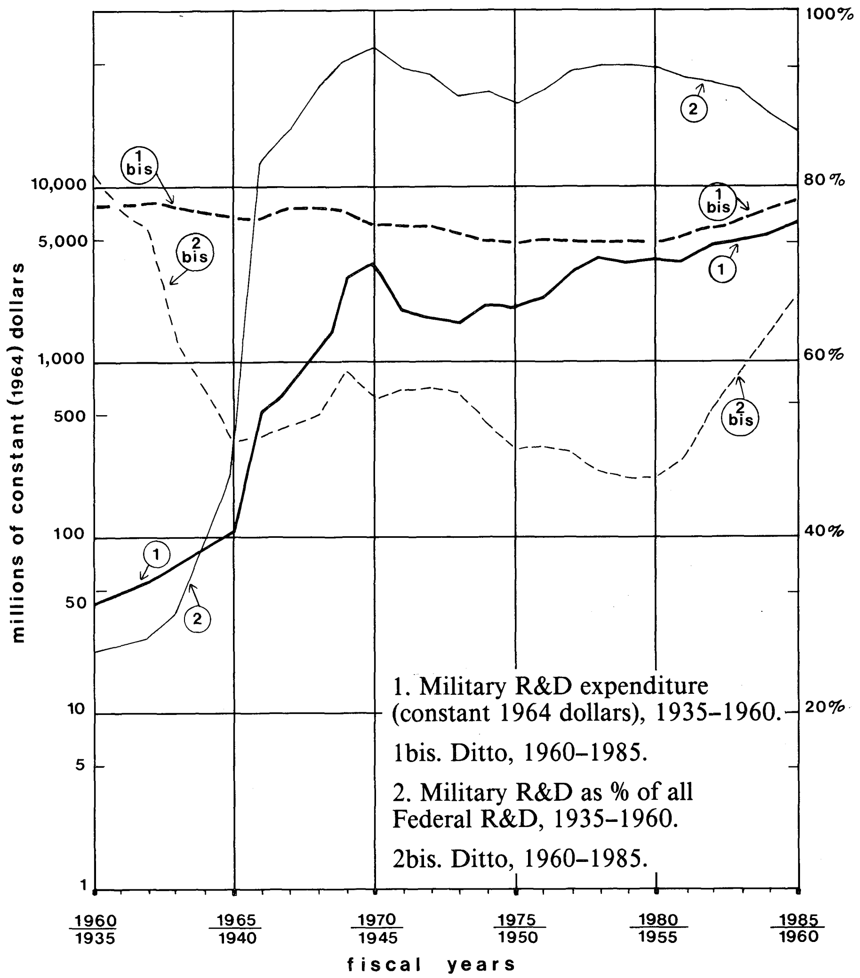

FIG. 1 U.S. MILITARY R&D EXPENDITURES, 1935–1985

NOTES FOR FIGURE 1

The figure is folded so as to facilitate a comparison between the fifties and the eighties. Included as "military R&D expenditure" FY '35–FY '60 are Department of Defense R&D outlays, all OSRD outlays, all Manhattan Project outlays, but only such AEC outlays as were declared to be R&D (ranging from a third in FY '50 to a seventh in FY '52 in the midst of the hydrogen bomb build-up), half of NACA's prewar R&D outlays, all NACA R&D outlays from FY '40 through FY '57, and in each of the following three years in which NACA became NASA and took off, an amount equal to the FY '57 figure. In 1955 DOD began to identify the development funds which were contained within procurement expenditures, namely "testing and engineering," thus laying the basis for a new definition of "development" closer to that which had been employed in industrial R&D. DOD and AEC employed this new definition from FY '58 forward, and in 1961, working with the BOB and NSF, they produced revised figures for their R&D expenditures from 1953 forward, resulting in an increase of roughly 50% in previous figures for FY '53–FY '57. C.V. Kidd's judgement that "it is impossible to correct these underestimates for earlier years" not withstanding, I have applied this 50% increment of military R&D figures also to FY '35–FY '52, and to the AEC's R&D, FY '47–FY '52. Sources: For FY '40–FY '60: NSF, *Federal funds for science, 13* (1965), 179–180, and, for revised definition of "development," ibid., *8* (1959), 70–71, and *10* (1960), 40–41, 62, 120–121. For FY '35, FY '37, and FY '38: A.H. Dupree, *Science in the Federal Government* (Cambridge, 1957), 332; NSF, *Scientific personnel resources: A summary of data* (Washington, D.C., 1965), 7; U.S. National Resources Committee, *Research–A national resource,* vol. 1: *Relation of the Federal Government to research* (Washington, D.C., 1938), 67. For FY '60–FY '84: U.S. Bureau of the Census, *Statistical abstract of the United States, 1985,* 754; NSF, Office of Special Studies, *Methodological aspects of statistics on research and development costs and manpower,* "NSF–59–36" (Washington, D.C., 1959), 7–15; and C.V. Kidd, *American universities and Federal research* (Cambridge, 1959), 254–255.

Here and throughout this paper monetary data have been translated into constant dollars using the price index of Federal Government purchases of goods and services as given by U.S. Bureau of the Census, *Statistical abstract of the United States, 1985,* 408, with 1964 as base year. The multipliers are:

1935	1940	1945	1950	1955	1960	1970	1980	1985
3.0	2.8	2.1	1.5	1.3	1.1	0.75	0.35	0.25

the history of our own times is brought straight home, are being retraced and realigned continually—not merely because more and more of mid-century America is being excavated, but equally because the present in which we stand is so close to this past that our standpoint shifts relatively rapidly. Nonetheless, scholarship of the last ten years has not only repeatedly confirmed the magnitude of the break with America's past brought by the second world war—seen so early and clearly by A. Hunter Dupree[7] —but has reaffirmed the

7. A. Hunter Dupree, *Science in the Federal Government: A history...to 1940* (Cam-

undiminished importance of the two central decades of this century as source and precedent for the present. It is my hope that as our daily experience of that legacy accumulates, we will find it more and more difficult to ignore.

Research for national security

In the waning phases of the war, each branch of the military, as well as the OSRD and various congressional committees, made assessments of the significance of scientific research for America's war effort and for the future. Each came to the same conclusion: that "that contact, that partnership," achieved during the war between civilian scientists and the military, "is, in our judgment, vital to the national defense;" that "to insure continued preparedness along farsighted technical lines, the research scientists of the country must be called upon to continue in peacetime some substantial portion of those types of contributions to national security which they have made so effectively during the stress of the present war;" and that consequently the military services must act promptly to assure themselves of "an uninterrupted continuation....into peacetime" of the services of scientists in academic institutions.[8]

bridge, 1957), 373; "Background of the alliance between the Federal Government and institutions of higher learning," in National Academy of Sciences, Committee on Science and Public Policy, *Federal support of basic research in institutions of higher learning* (Washington, D.C., 1964), 15–56; "The structure of the government-university partnership after World War II," *Bulletin of the history of medicine, 39* (1965), 249–251; "A new rationale for science," *Saturday review* (7 Feb 1970), 55–57; "The Great Instauration of 1940: The organization of scientific research for war," in G. Holton, ed., *The twentieth century sciences* (New York, 1972), 443–467; Dupree's testimony before a Congressional committee in 1970, reprinted in James L. Penick, Jr., et al., eds., *The politics of American science, 1939 to the present* (Cambridge, 1972), 435–443; his personal reminiscences, "A historian's view of advice to the President on science: Retrospect and prescription," in W. Golden, ed., *Science advice to the President* (Oxford, 1980), 175–190; and, his preface to the reissue of *Science in the Federal Government* (Baltimore, 1986), vii–xviii.

8. D.J. Kevles, "Scientists, the military, and the control of postwar defense research: The case of the Research Board for National Security, 1944–46," *Technology and culture, 16* (1975), 20–47; N.A. Komons, *Science and the Air Force: A history of the Air Force Office of Scientific Research* (Air Force Office of Aerospace Research, 1966), 1–7; H.M. Sapolsky, *The Polaris System development* (Cambridge, 1972), 4; M.S. Sherry, *Preparing for the next war: American plans for postwar defense, 1941–45* (New Haven, 1977), 126; C.R. Koppes, *JPL and the American space program: A history of the Jet Propulsion Laboratory* (New Haven, 1982), 25, 258. The quoted phrases are, in order, those of Representative Woodrum motivating a resolution authorizing funding of military R&D through the National Academy of Sciences' Research Board for National Security, in U.S. Congress, House, Select Committee on Postwar Military Policy, *Report 2: Research and development* (Washington, D.C., 1945), 1; the secretaries of War and

The atomic bomb did not generate but mightily confirmed and reinforced a conviction, already firmly established in the minds of newspaper editors and other opinion leaders by the summer of 1945, that "our national security rests upon superior science," and that governmental support of academic science would make "possible progressive development in military matters without maintenance of a large-scale military research establishment."[9] Following the conclusion of hostilities by two cataclysmic nuclear explosions, the Secretary of War was preaching to the converted when testifying in October 1945 before the Senate Committee on Military Affairs that, "since the laboratories of America have now become our first line of defense, I cannot make too strong or too emphatic, the interest of the War Department in the promotion of scientific research and development for new weapons." Thus the proposition that "the rapid extension of scientific knowledge...may reasonably be said to be a major factor in national survival," which opened the autumn 1947 multi-volume report of President Truman's specially appointed Scientific Research Board, was but a restrained restatement of a commonplace of the early years of cold war.[10]

To no inconsiderable degree these propositions were acted upon in the late 1940s. MIT, on the inside track, emerged from the war with a

Navy (Stimson and Forrestal) in a joint letter to the NAS, as quoted in V. Bush, *Science: The endless frontier. (Washington, D.C., 1945), 12, 27;* Chief of Naval Operations Ernest J. King's order, approved by Secretary Forrestal, 21 Aug 1945, for continuation of the Operations Research Group (ONR contract records, Washington National Records Center, WNRC, Access 63A2374, 9/NOD-6964, also MIT Archives, AC-4, 36/12). Similarly King's conviction as stated in his final report on the war to the Secretary of the Navy: "Only by continuing vigorous research and development can this country hope to be protected from any potential enemies." Quoted by D.K. Allison, "U.S. Navy research and development since World War II," in M.R. Smith, ed. (ref. 3), 289–328, on 290–291. I have refrained from quoting statements of this tenor by those who by the nature of their positions were advocates of military research, like V. Bush, E.L. Bowles, and admirals J. Furer and H.G. Bowen.

9. Quotations from Kenneth M. Jones, *Science, scientists, and Americans..., 1945–1950* (Ph.D. thesis, Cornell Univ., 1975; University Microfilms, 75-24202), 16–18. U.S. Senate, Committee on Military Affairs, Subcommittee on War Mobilization, *Legislation proposals for the promotion of science; the texts of five bills and excerpts from reports* (Senate Document Nr. 92, August 1945).

10. Secretary of War Patterson, as quoted by Clarence G. Lasby, *Project Paperclip* (New York, 1971), 92–93. In this pioneering study of a topic that is still one of the most inflammatory in the field of "science and the military," Lasby used the recruitment by the U.S. Army of German and Austrian scientists and engineers for work in its laboratories as a very practical measure of its commitment to "this hope for an endless application of science to war" (ibid., 61–63). John R. Steelman, Chairman, U.S. President's Scientific Research Board, *Science and public policy: A report to the President*, 5 vols. (Washington, D.C., 1947), *1*: 3.

staff twice as large as it had had before the war, a budget (in current dollars) four times as large, and a *research* budget ten times as large—85% from the military services and their nuclear weaponeer, the AEC.[11] Though a political consensus sufficient to launch a comprehensive National Research Foundation could not be found, Vannevar Bush, previously Director of the OSRD, and then Karl Compton, President of MIT, headed a strictly military Research and Development Board for the services. The RDB, whose structure followed that which Bush had proposed for the more pacific Foundation, employed a full-time staff of some 250 augmented by 1500 part-time civilian consultants impaneled on some 100 committees to coordinate several thousand military R&D projects, each expending, on the average, several hundred thousand dollars annually.[12]

Through the first five postwar years, despite the great disparity between Americans' deepening fears of communism and the nation's manifestly insufficient military strength to prevent its spread, President Truman insisted on a cap on military spending. Military research budgets remained flat, on a high plateau, and neither the military nor the academy counted on their climbing higher.[13] Even the

11. J.R. Killian, Jr., "M.I.T. redeploys for peace," in J.E. Burchard, *Q.E.D.: M.I.T. in World War II* (Cambridge, 1948), 313–326; F.L. Foster, "Sponsored research at M.I.T., 1900–1968," Typescript, 1984, photocopy in MIT Archives.

12. S.L. Rearden, *History of the Office of the Secretary of Defense*, vol. 1: *The formative years, 1947–1950* (Washington, D.C., 1984), 96–103; U.S. Navy Department, *Review of Navy R&D management, 1946–1973,* "Prepared by Booz, Allen & Hamilton, Inc." (Washington, D.C., 1976), 16–23; E. Speakman, "Research and development for national defense," IRE, *Proceedings, 40* (1952), 772–775: A.C. Cole et al., eds., *The Department of Defense: Documents on establishment and organization, 1944–1978* (Washington, D.C., 1978), 47, 72–73, 96–97, 110–111, 138–139, 153. Although scientific research and advanced technology had been given an important national-security role in the analysis of "American relations with the Soviet Union" in September 1946 by Truman's special assistant Clark Clifford, that was by no means generally the case in the planning documents of the Joint Chiefs of Staff and the National Security Council selected by T.H. Etzold and J.L. Gaddis, eds., *Containment: Documents on American policy and strategy, 1945–1950* (New York, 1978). Their culminating document, NSC 68, "U.S. objectives and programs for national security," April 1950, mentions scientific research or its application to advanced weaponry only in the case of "atomic armaments." An introduction to the literature: N.A. Graeber, ed., *The national security: Its theory and practice, 1945–1960* (Oxford, 1986).

13. The large literature on the DOD budget and strategic planning turns largely on the origins and alleged consequences of the planning document NSC 68. Rearden (ref. 12), 534, finds, however, that "the general feeling in the Defense Department before the outbreak of the Korean War seemed to be that NSC-68 would in all probability lead to little, if any, change in administration policy" and that there is no evidence that without bold military aggression by a communist regime "hard money" Truman would have altered the line he had followed since 1947. This too is the view of S.F. Wells, Jr., "Sounding the tocsin: NSC 68 and the soviet threat," *International security, 4* (Fall, 1979), 116–158, on 139: "Had [the Korean] war not intervened, there is strong evidence

detection of a Russian nuclear explosion in the autumn of 1949, though it led to the launching of a crash program to make a hydrogen bomb in the spring of 1950 and to an enormous jump in the AEC budget, brought no more general, and generally expensive, response.[14]

When, however, in the summer of 1950 Americans found themselves in a shooting war in Korea, the disparity between fright and might became greatly and generally alarming: "What was wanted was military strength—every and any kind of military strength—and it was wanted immediately." *Immediately* the funds to be overseen by the RDB for that fiscal year doubled, topping a billion dollars, and thereafter all areas of military R&D continued to grow rapidly.[15] Although the remobilization of science in the style of World War II was much discussed and even half-way initiated, very quickly the physicists, as American society generally, opted for guns *and* butter.[16]

that no major increase in defense spending would have won administration approval." Late in 1948, the Office of Naval Research expected a decline in funding for research in the next few years: J.E. Pfeiffer, "The Office of Naval Research," *Scientific American, 181* (Feb 1949), 11–15. Certainly the academic beneficiaries of military research funds were not bullish. At MIT it appeared, even as the first Berlin crisis was building in June 1948, that "a realistic appraisal of the prospects for continued military sponsorship [of the Research Laboratory of Electronics] allows no other alternative than to anticipate a drastic reduction in funds" beginning no later than "June 30, 1950." In an effort to stave off such anticipated reductions the RLE organized in November 1949 an elaborate show for representatives of the military services (MIT, Archives, IA, AC-12, 18/RLE, and AC-4, 204/7).

14. R.G. Hewlett and F. Duncan, *A history of the United States Atomic Energy Commission*, vol. 2: *Atomic shield, 1947-52* (University Park, 1969), 676–677, show the AEC's budget tripling from FY '50 to FY '52, while its allotments for physical research, remaining nearly flat at $30 million, dropped from 5% to 2% of the total. Even more striking is the AEC's *Tenth semi-annual report* (Washington, D.C., 1951), 52, which shows this tripling to have occurred in *one* year, from FY '50 to FY '51. See also R.W. Seidel, "A home for big science: The Atomic Energy Commission's laboratory system," *HSPS, 16:1* (1986), 135–176, on 151–156.

15. Quotation from W. Millis, *Arms and men* (New York, 1956), 330. Doubling of all military R&D: K.T. Compton to R.P. Russell and to F.W. Loomis, 26 Dec 1950 (MIT, Archives IA, AC-4, 245/17 and 246/2). The number of projects under the cognizance of the RDB had roughly doubled by the end of 1950: P.B. Taylor, "Military research and development," *Aeronautical engineering review, 10* (Apr 1951), 49–51; E.A. Walker, "Research and development: The nation's balance sheet in June 1951," *Journal of engineering education, 42* (Nov 1951), 126–133. At MIT's Research Laboratory of Electronics, "programs would double in the fall of 1950 under strong military support;" K.L. Wildes and N.A. Lindgren, *A century of electrical engineering and computer science at MIT, 1882-1982* (Cambridge, 1985), 247. At Stanford's Electronics Research Laboratories, work expanded by at least a factor of two with a half-million dollar a year ONR contract arranged nearly over night in the fall of 1950: S.W. Leslie and B. Hevly, "Steeple building at Stanford: Electrical engineering, physics, and microwave research," IEEE, *Proceedings, 73* (1985), 1169–1180. Likewise the budget of Caltech's Jet Propulsion Laboratory doubled between 1949 and 1951; Koppes (ref. 8), 47.

16. On mobilization of science: M.W. Rossiter, "Science and public policy since

Thus began the present era of the "permanent arms economy" based upon the practice of "military Keynesianism."[17] As the historian of the space age must allow, years before Sputnik "the U.S. Government had already forged what has become known as the National Security State, folding the intelligence agencies, the armed forces, corporations, research institutions and universities into a national complex for the provision of ever more sophisticated weapons systems."[18]

Electronics and the expansion of R&D

In 1941 fifty-five manufacturers of radio sets had total factory sales of $240 million. By 1944, the radio and radar industry had reached an annual peak production rate of $4.5 billion, with total deliveries of $7.5 billion from 1942 through 1946.[19] Postwar reconversion released pent-up consumer demand. For the electronic industry, America's

World War II," *Osiris, 1* (1985), 273–284, on 274–276; A.T. Waterman to V. Bush, 11 Jan 195[1], enclosing "Mobilization of scientific manpower," 6, and "Bibliography on utilization of scientific manpower," 2 (LC, Bush Papers, 117/2790). For guns and butter: H.G. Vatter, *The U.S. economy in the 1950s: An economic history* (Westport, 1963), 67, 89, 102; as Herbert Block observed, in *The Herblock book* (Boston, 1952), 182, "around here," in Washington, 1950/51, "you sometimes wondered whether it was a tocsin or a dinner bell that had been sounded." A strong statement in favor of butter is Waterman's own program for the Office of Naval Research, "wartime organization of science," Sep 1950: "In making these plans, it is assumed that something like the present degree of emergency will persist for an unforeseeable period of years....[Consequently] it is believed equally important to secure 'defense in depth,' as Dr. Piore puts it, on the research and development front by providing stable, continuous support to basic research" (LC, Waterman Papers, 31/ONR 1950).

17. The term "military Keynesianism" and an impressive argument for its operativity are offered by L. Griffin, J.A. Devine, and M. Wallace, "Monopoly capital, organized labor, and military expenditure in the US, 1949–1976," in M. Buraway and T. Skocpol, eds., *Marxist inquiries* (Chicago, 1982), S113–S153. *Fortune, 46* (Sep 1952), 128–136, took note of "a phenomenon new to American history—the influx of career admirals and generals into top jobs in U.S. business." For the situation some fifteen years later, see C.W. Pursell, Jr., ed., *The military-industrial complex* (New York, 1972), 253–270. For the integration of the U.S. space program into the pattern and practice of "military Keynesianism," see J.M. Logsdon, "The space shuttle program: A policy failure?" *Science, 232* (1986), 1099–1105.

18. W.A. McDougall, "NASA, prestige, and 'total cold war,'" paper presented at the History of Science Society meeting, Chicago, Dec 1985. Daniel Yergin, *Shattered peace: The origins of the Cold War and the national security state* (Boston, 1977), going only to 1948, neither defines the concept nor more than touches upon its characteristic manifestations. The national security state appears to have been launched by the Report of the March 1969 Congressional Conference on the Military Budget and National Priorities; *Progressive, 31* (Jun 1969), 18–20; Jul 1969, 5–6.

19. James D. Secrest, *Electronic Industries Association: The first fifty years* (Washington, D.C., 1974), 44, 49; EIA, *Fact book* [1955], 19, and [1956], 8.

headlong rush into television was an unprecedented boon. It brought the industry booming growth in the late 1940s, growth that continued through the 1950s at a rate four or five times that of U.S. industry in general. Total sales volume doubled twice between 1950 and 1960. Examined more closely, however, this overall growth is found to compound two very different trends: Between 1950 and 1960 annual factory sales of consumer electronics products showed no growth at all, while sales of electronic products to the military services increased 650%—in constant, uninflated dollars.[20]

The first big surge came with the Korean war. Boston's Raytheon, for example, barely holding its head above water in the late 1940s, grew explosively in the 1950s on defense contracts. At the decade's end the firm's annual sales—88% to the government—were exactly ten times those ten years earlier.[21] Already by early 1953 C.B. ("Tex") Thornton of Hughes Aircraft—another principal defense electronics firm—was urging that the industry's trade association, the Radio and Television Manufacturers Association, was "inadequate as now conceived and not organized to represent the new electronics industry that we have today," in which, Thornton was rightly certain, civil sales would never again equal sales to the military.[22]

20. W. B. Harris, "The electronic business," *Fortune, 55*, Apr 1957, 136–143, 216–226. The sudden spurt in consumer electronics following World War II and the equally rapid supersession of this market by military electronics after 1950 has been noted by Roger Godement in a very well-informed and stimulating survey, "Aux sources du modèle scientifique américain," *La pensée, 201* (1978), 33–69, *203* (1979), 95–122, and *204* (1979), 86–110. Although economists complained that "defining the electronics industry is next to impossible..., there is probably no more heterogeneous collection of products and firms" (Vatter (ref. 16), 167), they seem to have succeeded well enough in identifying 95% of these producers and products from the late 1940s onward: J.M. Hund, "Electronics," in Max Hall, ed., *Made in New York: Case studies in metropolitan manufacturing* (Cambridge, 1959), 241–326, 355–367; Battelle Memorial Institute, *The implications of reduced defense demand for the electronics industry* (Washington, D.C., 1965), 15 (Arms Control and Disarmament Agency, *Publication*, 28); A.E. Malito, Jr., "Characteristics of the electronics industry," in J.E. Ullmann, ed., *Potential civilian markets for the military-electronics industry* (New York, 1970), 11–43.

21. Otto J. Scott, *The creative ordeal: The story of Raytheon* (New York, 1974), 205–250. Raytheon had particularly close ties to MIT: Vannevar Bush had been one of its founders and after the war E.L. Bowles became the principal technical consultant to its president, as he had previously been to the Secretary of War. The substantial drop in both radio and TV sales in the early 1950s was evidently the result only of reduced consumer demand, not of shortages. But though sales of TV picture tubes were falling, sales of cathode-ray tubes to equipment manufacturers doubled and tripled. IRE, *Proceedings, 40* (1952): Aug, pp. 68A, 74A; Sep, 66A; Oct, 72A; Nov, 82A.

22. Secrest (ref. 19), 74–75. In the spring of 1956 Raytheon got out of the commercial radio and television business. *Annual report for the fiscal year ending May 31, 1956*, 3.

The "New Look" at U.S. military posture initiated by President-elect Eisenhower in the winter of 1952–53 sought to reduce defense costs through higher military technologies coupled with lower troop levels.[23] Between 1954 and 1959 the proportion of electronic equipment among all hard goods purchased by the U.S. military doubled (figure 2), thus helping the electronics industry sail through the recession of the mid-1950s and increasing the importance of the industry to the Department of Defense as DOD increased in importance to the industry.[24] This was especially true in New England, where a successful electronics industry was conspicuously connected with the region's strong institutions of higher learning, on the one hand, and conspicuously dependent on the military patron, on the other.[25]

What was true of production in the electronics industry was truer still of its research and development. Although company funds for R&D in American industry climbed steadily through the postwar period, they were overtaken in the early 1950s by the even more rapidly growing federal funds spent by private firms (figure 3). At least 90% of these federal funds derived from the budget category "National Security," and in the portions spent on or by the electronics

23. H.F. York and A.G. Greb, "Military research and development: A postwar history [to 1957]," *BAS, 33:1* (1977), 13–26. E.A. Kolodziej, *The uncommon defense and Congress, 1945–1963* (Columbus, 1966), 180–252, takes a long look at the "New Look." J.B. Gaddis, *Strategies of containment: A critical appraisal of postwar American national security policy* (Oxford, 1982), gives the term a much more comprehensive meaning.

24. "Plants expand for defense in 1954," *Electronics, 27:8* (1954), 18, 20, and "Defense business upswing continues," ibid., *31:12* (1954), 6, 8. In the later 1950s the principal elevators of military electronics sales were, first, missiles, with the electronics accounting for 25% to 50% of the overall cost and for $3 billion of missile electronics contracts let in FY '59, and, second, SAGE and BMEWS with a third of all military electronics outlays in FY '60. Ibid., *31:11* (1958), 13; EIA, *Fact book* (ref. 19) [1959], 30.

25. In 1957 three regions of the U.S. differed considerably in their shares of the national military and non-military markets: The mid-West region had 24% of the non-military market, but only 10% of the military; the Pacific had 12% of the non-military, but 17% of the military; and New England had 5% of the non-military, but 10% of the military; A.H. Rubenstein and V.L. Andrews, *The electronics industry in New England to 1970* (Boston, 1959), 19 (Federal Reserve Bank of Boston, Research Report, 1970 Projection, Nr. 4), who also noted, ibid., 5–6, 56, that "at present New England has but little share in the manufacturing of consumer electronics products" and that the industry's "primary advantage....is their proximity to and involvement in the academic and research milieu, which centers around the Boston area." Similar conclusions regarding "the dominance of the federal government as the purchaser of the products and services of Greater Boston electronics firms" were reached by E.J. Burtt, *The electronics industry of Greater Boston* (Boston, 1962), 12 (Greater Boston Economic Study Committee, Economic Base Report Nr. 10).

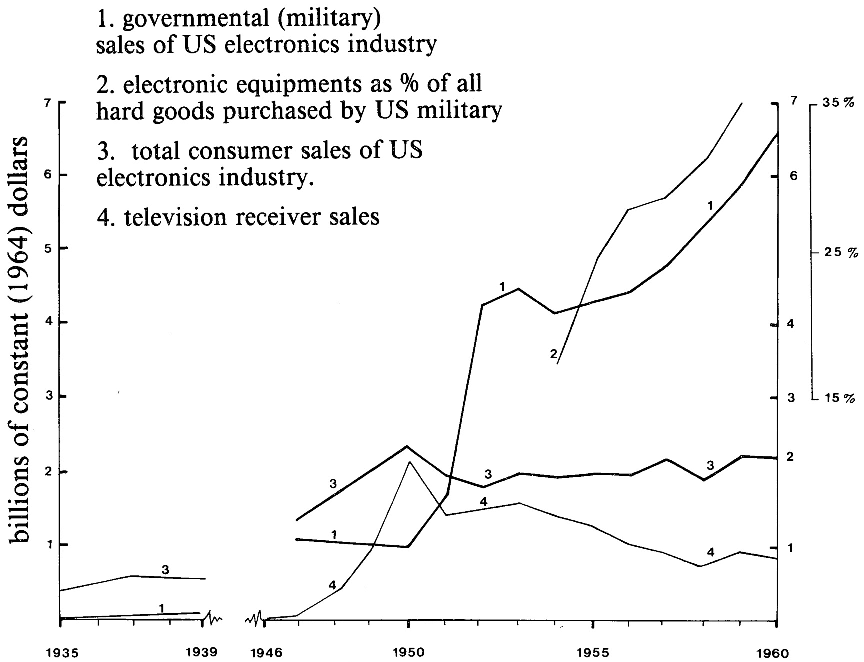

FIG. 2 U.S. ELECTRONICS INDUSTRY SALES AND MILITARY PURCHASES, 1935–1960. Data refer to the manufacturing sector of the electronics industry, and do not include broadcasting and other non-manufacturing revenues. Figures for consumer electronics factory sales are generous, however, including *inter alia* phonograph records (Malito (ref. 20), 25). Omitted from figure 2 are U.S. electronics sales to industry; when these are included the military's share of total manufacturers' sales remains nearly constant between 1952 and 1960, at 50–60%. Throughout this period less than 5% of electronics industry factory sales to government were for non-military purposes. Sources: Electronic Industries Assoc., *Year book 1968* (Washington, D.C., 1968); S.A. Pettingill, *Electronic components: Production and related data, 1952–1959* (Washington, D.C.: U.S. Dept. of Commerce, Business and Defense Administration, 1960), 18 pp.; W. Skinner and D.C.D. Rogers, *Manufacturing policy in the electronics industry* (Homewood, IL, 1968), exhibits 7 and 8; Stanford Research Institute, *A study of small business in the electronics industry* (Washington, D.C.: Small Business Administration, [1962]), 48; H.G. Vatter (ref. 16), 108–109; W.B. Harris (ref. 19), 139, 216.

industry that percentage was certainly higher. Moreover, in the electronics industry the overtaking came earlier, probably at the very start of the 1950s with the initial expansion of R&D occasioned by the Korean war. By 1960, 70% of the R&D performed by the electronics industry nationwide was paid for with federal funds.[26] The fraction of the industry's R&D oriented toward military goals was surely higher yet, due to the leverage exerted upon company-sponsored R&D by the federally funded program, coupling hope for future military contracts with the recognized absence of civilian spin-offs.[27]

Thus even before Sputnik went up, research activities in the electronics industry were largely in the interest of the military. As the Director of Research for National Carbon Company stated in October 1957, apropos of research on dielectrics and ferroelectrics:[28]

26. In 1964, although the civilian space program was then also making large demands for R&D on the U.S. electronics industry, still 75% of its research and engineering employment was supported by DOD funds; Battelle (ref. 20), 24. When the NSF began to compile *Federal funds for science* in the early 1950s, it grouped R&D expenditures by function, as did the Bureau of the Budget, placing the whole of the AEC's outlays with the DOD's under "National Security." In FY '53→55 this category held 85% of all Federal R&D obligations. If, however, we shift NACA's R&D outlays from "Transportation" to "National Security," as did the Bureau of the Budget the category holds nearly 90%. Alternatively, if we recall that the military services, and among them most especially the burgeoning Air Force, relied far more heavily upon private industry for performance of their R&D than did other Federal agencies, the overwhelming predominance of "National Security" in federally funded industrial R&D is clear. Moreover, NSF, *Research and development in industry, 1961* (Washington, D.C., 1964), 5, allowed that "the relative share of R&D performance that was financed by the Federal Government may be somewhat understated in certain industries."

27. That "the bulk of private concerns with divisions or subdivisions devoted to defense research and development programs have not yet found many ways, and, in most cases, no significant ways to transfer the organizational expertise and the technological advances evolved under Government support into the non-defense portions of corporation activities" was admitted by the Deputy Director of Defense Research and Engineering, J.H. Rubel, "Trends and challenges in research and development," in National Security Industrial Association, *Proceedings of R&D Symposium..., 13–14 March 1963* (Washington, D.C., 1963), 9–35, on 12. A survey of electronics firms at this date found that half judged the commercial product fall-out of their defense work "slight or none," and that the larger the company the less likely it was to rate product fallout as "great," reaching zero for the ten largest firms; Battelle (ref. 19), pp. C-14, C-20. Kevles (ref. 1) stresses the leverage exerted today by defense R&D funds over company R&D funds.

28. R.G. Breckenridge's contribution to U.S. Navy, Office of Naval Research, Solid State Advisory Panel, "Solid state physics research: Performance and promise" ("Sproull Report"), Oct 1957, p. 34 (copy in Niels Bohr Library, American Institute of Physics).

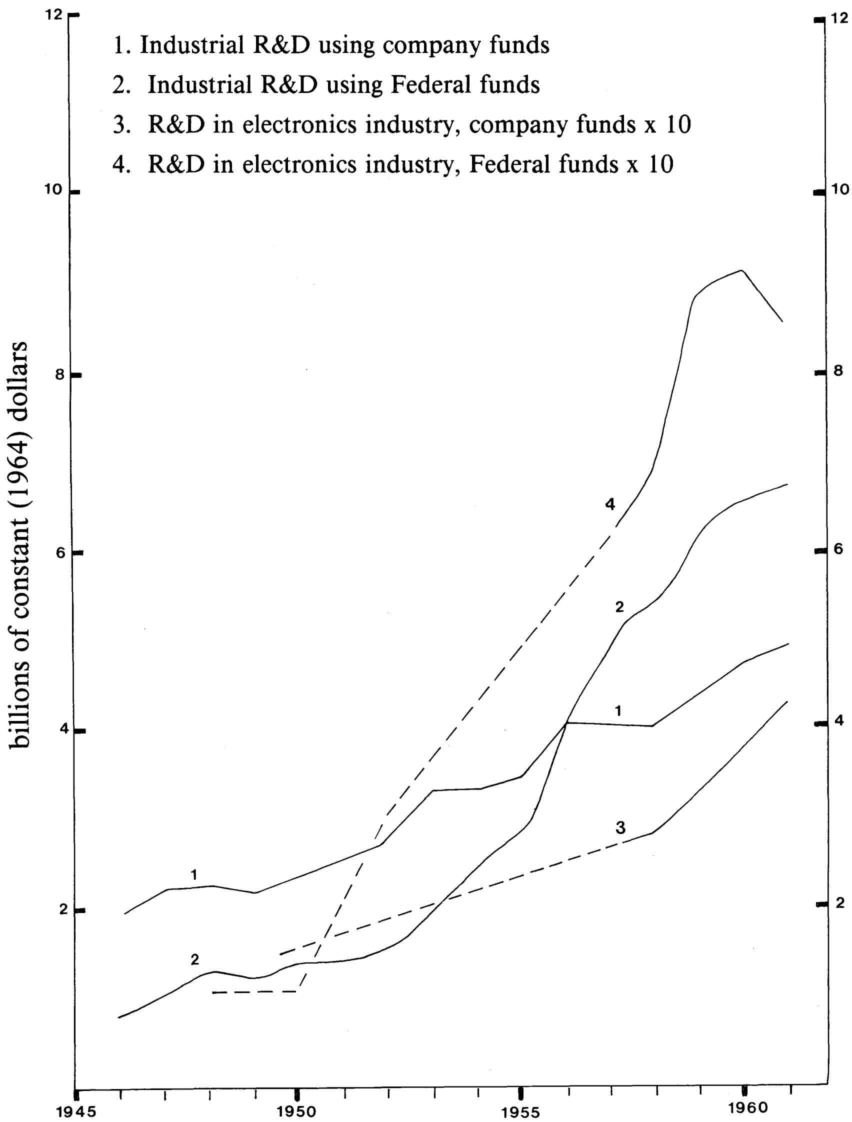

FIG. 3 INDUSTRIAL R&D WITH FEDERAL FUNDS, 1946–1960

NOTES FOR FIGURE 3

Source: NSF, *Research and development in industry, 1961,* "Surveys of science resources series, NSF-64-9" (Washington, D.C., 1964), 7, 9, 12, 43, 64, 65. Data for R&D in the electronics industry are those given for "communication equipment and electronic components" in ibid., 9, 12, and also in NSF, *Reviews of data on science resources,* vol. 1, Nr 3: *Research and development in the electrical equipment and communication industry, 1956–1962* (1965), 4. Electronic industry R&D figures, here multiplied by ten for display on the same scale as total industrial R&D, underestimate electronic R&D in industry since roughly one-quarter of such work was done in industries not primarily electronic, notably aircraft and missiles, computers and office machines, etc. The extrapolation of these figures backwards from 1957 is intended to be merely suggestive. Earlier sources do not separate "electronic" from "electrical" equipment; they do however show that as early as 1953 the Federal government was financing more than half the R&D expenditures in the "electrical equipment" (55%) and the "telecommunications and broadcasting" (52%) industries: NSF, *Science and engineering in American industry: Final report on 1953–1954 survey,* "NSF-56-16," prepared by the U.S. Bureau of Labor Statistics (Washington, D.C., 1956), 66.

Electronic Industries Association, *Fact book* [1958], 14–15, gives the following data for military electronics R&D expenditures in millions of current dollars:

1951	1952	1953	1954	1955	1956	1957
136	209	253	248	244	267	303

When converted to constant FY '64 dollars, the FY '57 figure attains only 60% of our calendar 1957 figure and altogether these latter data are inconsistent with the evidence of substantial growth. However, the data for FY '51 and FY '52 are generally consistent with other data and are confirmed by E. Speakman (ref. 12), 774. The gap here opening between Federal and firm R&D funds continued to widen through the early 1960s, then steadily narrowed. About 1980 the all-industries curves crossed again; in 1987, real Federal funds had barely returned to their Vietnam War peak, whereas company funds were at twice the Federal level.

> The picture that evolves in these solid state problems is clear: Much of the basic research that is the necessary prelude to the practical applications in all cases remains to be done....The responsibility for the support of this work clearly lies with the Department of Defense since the military applications are the primary reason for investigations in these fields; industry does not generally find the subjects of sufficient commercial interest to undertake the work in its own research program.

This picture had been drawn to a purpose, namely to brake the sudden down-turn of military R&D funding in the summer of 1957 just as the U.S. economy began to slide into a serious recession. With the fillip provided that same month by the Russian satellite launches, those small reductions in military funds for R&D in U.S. industry were converted into enormous increases. In the three years following Sputnik, the number (FTE) of R&D scientists and engineers doubled in that half of the electrical equipment industry producing communication equipment and electronic components. By 1962 Simon Ramo could well note not only that "the electronics industry has become greatly dependent now on government support, as have the universities," but also that (as in the universities) "a large fraction of the entire electronics industry seems to have been transformed from production activities...into research and development."[29]

Not only those centrally involved were strongly impressed by the progress of the research industry. "Even if it may not be true that no other industry or economic activity has ever grown as fast as R&D," economist Fritz Machlup reflected in 1962, "this growth has certainly been phenomenal." The editors of *Fortune*, meanwhile, were looking to *The mighty force of research* for collective salvation: "Taken together, the two aspects of research—the pure and the applied—are the mightiest force in the U.S. economy."[30]

This transformation of industry required a special sort of operative: research scientists and engineers. Figure 4 shows the rapid increase in numbers of various sectors of the R&D workforce following the Great Depression, or, in the case of physics Ph.D.'s, the second world war. The curves representing electronic engineers and research personnel in the electrical and electronic industries are perceptibly steeper than those for industrial research personnel generally. Chemists, the first of the academically trained specialists to be employed in industrial research, remained in first place right up to 1946. By 1960, however, there were five times as many engineers as chemists so engaged—overwhelmingly mechanical, aeronautical, electrical and electronic engineers. Rapidly though their numbers grew in the first two postwar decades, The demand repeatedly surged ahead of the supply. From 1949 to 1955 the number of ads for industrial scientists in the pages of the *New York Times* increased by a factor of ten, and by another factor of two by 1959. As throughout the war, so in the decade following, the principal preoccupation of those responsible for managing the expansion of military R&D was "where to find the necessary qualified men."[31] By 1950, in stark contrast with 1940,

29. For "the crisis in research and development" in the summer of 1957: J.S. Dupré and S.A. Lakoff, *Science and the nation: Policy and politics* (Englewood Cliffs, 1962), 39; U.S. Navy (ref. 12), 479; S. Ramo, "The impact of missiles and space on electronics," IRE, *Proceedings, 50* (1962), 1237–1241; NSF, *Reviews of data on science resources, 1:3* (1965), 10.

30. F. Machlup, *The production and distribution of knowledge in the United States* (Princeton, 1962), 155; Editors of *Fortune*, *The mighty force of research* (New York, 1956), preface. Cf. Orlans (ref. 2).

31. K.T. Compton to L. de Florez, 4 Mar 1946 (MIT Archive, AC-4, 204/7); A. Thackray, J.L. Sturchio, T.P. Carroll, and R. Budd, *Chemistry in America, 1876–1976: Historical indicators* (Dordrecht, 1985), 118, 120, 347; graph of *Times* ads for industrial

defense work already occupied so large a fraction of the qualified men as to render scientific mobilization for war superfluous. Most companies simply could not hire enough engineers and scientists to meet their needs. To Lee DuBridge, this short-fall in the early 1950s appeared "not merely a grave problem but something which more nearly approaches a national catastrophe." Then, in the late 1950s, as in the late 1940s, just as demand was slackening and supply catching up, Sputnik shot the demand curve upward once again.[32]

The "industry" of R&D did not wait for needed scientists/engineers to make their way through the lengthy educational pipeline, but substituted less skilled personnel and more expensive equipment for the commodity in shortest supply. Figure 5 gives two quantitative measures of this classical economic process as conflated with the other historical forces making for ever increased "bigness" of research (including notably the uneconomic bureaucratic burden accompanying increased scale). It is clear that the number of auxiliary personnel, as well as the number of (1964) dollars spent per scientist or engineer engaged in R&D, was essentially constant during the fifteen years before the war. Both rose steadily from 1942 onward,

scientists, 1947–70, in National Academy of Sciences, National Research Council, Physics Survey Committee, *Physics in perspective* (Washington, D.C., 1972), 852, reproduced from *Scientific American*; Kevles (ref. 6), 370. In a survey conducted by Syracuse University's Maxwell Research Center referring to 1953 as base year, "The majority of the [363 reporting] commercial laboratories claimed to have experienced in the last few years a shortage of scientists and engineers....Six firms specifically mentioned personnel shortages in the radio and electronic fields. Here the demand is primarily for experienced personnel at the highest educational level." NSF, *Research and development by nonprofit research institutes and commercial laboratories, 1953,* "NSF 56-15" (Washington, D.C., 1956), 37.

32. L.A. DuBridge, "Scientific manpower," IRE, *Proceedings, 40* (1952), 900–901; the argument that only marginal resources remained unemployed in defense research in June 1950 was made by R.D. Bennett, Technical Director, Naval Ordnance Laboratory, *PT, 4:1* (1951), 4–5; see survey cited in ref. 73. Early in 1951 and again early in 1952 officials of the RDB estimated that the military research and development program occupied about two-thirds of the U.S.' scientists and engineers; P.B. Taylor, *Aeronautical engineering review, 10* (Apr 1951), 49; E.A. Speakman, ibid., *11* (Apr 1952), 57. J. Van Allen to B. McDaniel, 9 May 1951, as quoted by A. Needell, "Preparing for the space age: University based research, 1946–1957," *HSPS, 18:1,* 89–109. In autumn 1953, Secretary of Defense Charles E. Wilson gave among other reasons for not reestablishing an OSRD the consideration that "a substantial fraction of the total technical manpower is on defense work now;" *Naval research reviews, 7* (Feb 1954), 26–27. Economists were perplexed by the failure of this shortage to be reflected in salaries paid the scarce commodity. Rather than admit so flagrant a contradiction of the axioms of their science, some (of the best) economists concluded "that there is no evidence of a shortage of engineers." D.M. Blank and G.J. Stigler, *The demand and supply of scientific personnel* (New York, 1957), 32. See also *Scientific manpower*, a series of conference proceedings published annually by the NSF through the 1950s.

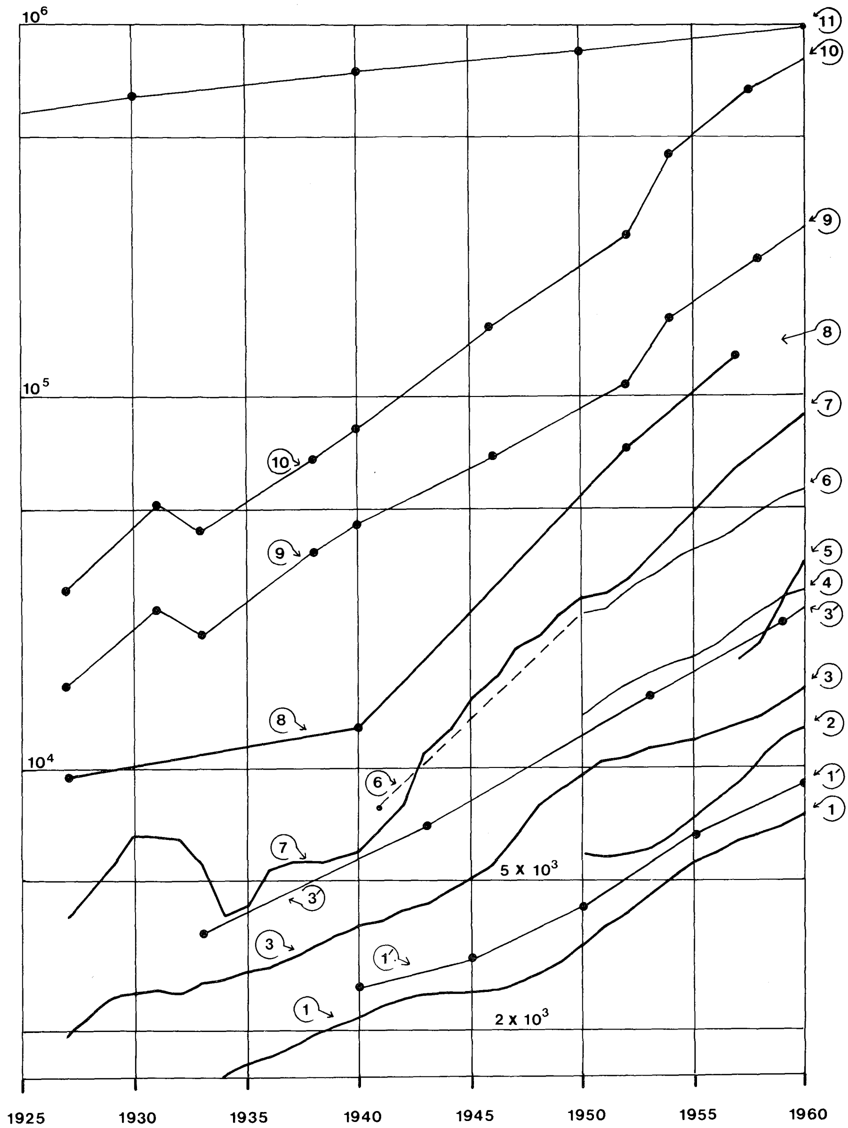

FIG. 4 PHYSICISTS AND OTHERS ENGAGED IN R&D IN THE U.S., 1927–1960. The curves are identified in the notes on the next page.

NOTES FOR FIGURE 4

All curves, apart from 1′, 3′, 8, 9, 10, 11 (on which data points are indicated by bullets) are based on year-by-year data.

1. "Stock of persons" holding doctorate in physics from U.S. universities. Source: D.L. Adkins, *The great American degree machine* (Berkeley, 1975), 47–49. These data agree extremely closely with the estimates (within 10% in the mid-40s, within 1% in the late 30s and early 50s) by A.D. Little, Inc. (ref. 78), vol. 2, 43–44, of U.S. physics Ph.D.'s below retirement age.

1′. Physics "Ph.D. population" of U.S. Source: NAS, NRC, Board on Human Resource Data and Analyses, *A century of doctorates* (Washington, D.C., 1978), 26.

2. Physicists employed at universitites, colleges, and non-profit, non-governmental research organizations. Source: U.S. Bureau of Labor Statistics, *Employment of scientists and engineers, 1950–1970,* "Bulletin 1781" (Washington, D.C., 1973), 54.

3. Members of the American Physical Society. Source: APS, *Bulletin, 30* (1985), 1955–56.

3′. Members of American physical societies (members of member-societies of the American Institute of Physics, corrected for duplicate memberships). Source: AIP (ref. 78), 4.

4. Physicists, however employed. Source: U.S. Bureau of Labor Statistics, *Employment of scientists and engineers, 1950–1970,* "Bulletin 1781" (Washington, D.C., 1973), 54.

5. Full-time equivalent scientists and engineers engaged in R&D in the electronics industry. Source: NSF (ref. 26), 7, 9, 12, 48, 64, 65.

6. Scientists and engineers of all sorts engaged in R&D at universities, colleges, and non-profit, non-governmental research organizations. Source: For 1950–1960, U.S. Bureau of Labor Statistics, *Employment of scientists and engineers, 1950–1970,* "Bulletin 1781" (Washington, D.C., 1973), 54; for 1941, U.S. DOD (ref. 70), 12.

7. Members of the Institute of Radio Engineers. Source: IRE, *Proceedings, 50* (1962), 540. Note the acute sensitivity to political and economic conditions: Cf. curve 3 in this respect.

8. Personnel of every sort engaged in R&D in the electrical equipment and communication industry. Source: N.E. Terlecky, *Research and development: Its growth and composition* (New York, 1963), 109.

9. Scientists and engineers engaged in R&D in industry. Source: Terlecky, ibid.

10. Personnel of every sort engaged in R&D in industry. Source: Terlecky, ibid.

11. One percent of the U.S. population age 25 and over. Source: U.S. Department of Commerce, Bureau of the Census, *Historical statistics of the United States: Colonial times to 1970* (Washington, D.C., 1975), part 1, 15.

and particularly steeply in those periods when qualified research personnel were in the greatest demand. One of the favorable preconditions for the expansion of R&D in electronics in the 1950s was the existence of a large pool of electronic technicians, of whom a full third had attended armed forces technical schools.[33]

These new social relations also gave rise to new literatures indicative of the new organization and function of research. New in form as well as content is the technical report. Specialists in this "grey literature" agree that it was "primarily a result of the growth of defense-related research and development programs sponsored by Government agencies." They considered moreover that "Two basic characteristics of defense-related research have accelerated the prominence of reports: the need for extremely close control over access to the information generated and the need for rapid dissemination of the information."[34] During the 1950s the cumulative number of announced and available technical reports in physics was roughly equal to the cumulative number of papers properly published in U.S. physics journals—about 50,000—but it was probably only some small percentage of the (unknown) number of security classified reports in physics and its technical applications prepared in that decade. The production of such grey literature by academic institutions, completely unknown before the war, proliferated in parallel with, and largely in consequence of, support by government contract.[35]

33. J.J. Treires, *The mobility of electronic technicians, 1940–52* (Washington, D.C., 1954), 6, 46, 59 (U.S. Department of Labor, Bureau of Labor Statistics, *Bulletin*, 1150), considered that "perhaps more significant to the economy than the technical aspects of the wartime electronics program was its impact on the supply of electronics technicians." In 1962 there were 53 R&D technicians per 100 R&D scientists and engineers in the electronics industry; the ratio was then fairly stable. The range in U.S. industry ran from 9 in agricultural chemicals to 103 in coal mining, with 50 the average. NSF, *Scientific and technical manpower resources: Summary information,* "NSF 64–28" (Washington, D.C., 1965), 60.

34. Quotation from R.E. Burton and B.A. Green, Jr., "Technical reports in physics literature," *PT, 14:10* (1961), 35–37, and from M. Herner and S. Herner, "The current status of the government research report in the United States of America," *UNESCO library bulletin, 13* (1959), 186–196. Likewise C.P. Auger, considered that "the development of the report literature as a major means of communication...dates back only to about 1941, with the establishment...of the United States Office of Scientific Research and Development." *Use of reports literature* (London, 1975), 9–10.

35. L.J. Anthony et al., "The growth of the literature of physics," *reports on progress in physics, 32* (1969), 709–767, esp. 713–714, 718, 723–725. In 1951 a specialist on this literature estimated that "150,000 technical reports are issued annually which are of interest to the national defense program;" M.H. Smith, "Defense documents control progress," *Special libraries, 42* (1951), 216–217, 235. The U.S. output of published physics papers in that year was about 4000, and the total U.S. output of published scientific, technical, and medical papers was very roughly 100,000. Ten years later the corresponding figures were 10,000 and 300,000. C.P. Bourne, "The world's technical

Familiar in form but of novel nominal content is the literature on administration of research. The prewar decades are but lightly sprinkled with such publications. During the war this sprinkling quickened to a trickle, and by the late 1940s the literature on research administration had formed a small stream; then, suddenly, from 1950 on, a torrent. In that year the organizers of the First Institute on

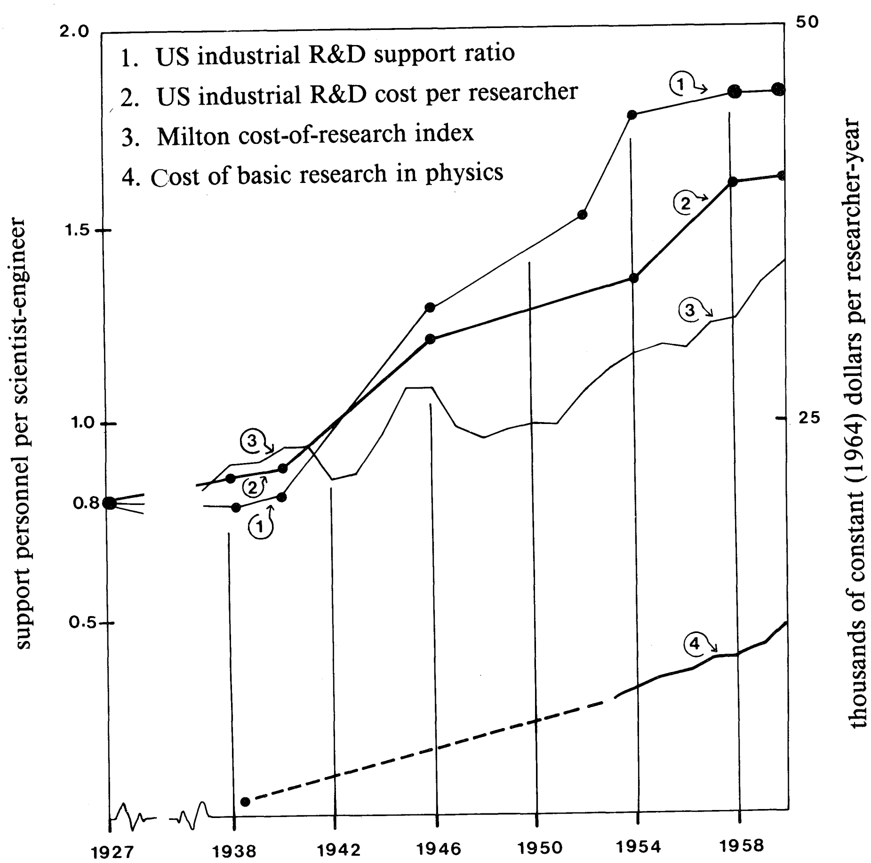

FIG. 5 'BIGNESS' OF RESEARCH.

journal literature," *American documentation, 13* (Apr 1962), 159–168; C.M. Gottschalk and W.F. Desmond, "Worldwide census of scientific and technical serials," ibid. (July 1963), 188–194. Through the 1950s roughly half of the roughly 125,000 items abstracted by *Nuclear science abstracts* were technical reports, half journal articles. The proportion of technical reports in the abstracted literature of U.S. origin was presumably significantly higher. C.S. Gillmor has recently informed me of his as yet unpublished studies of the growth rate of open versus closed literature in several fields of ionospheric research.

NOTES FOR FIGURE 5

This figure presents four indices of the 'cost' of R&D in the U.S. for each scientist or engineer so engaged. The first (1) is the total number of support personnel, both technical and administrative, in industrial R&D in the U.S., divided by the number of scientists/engineers. The second (2) is the constant-dollar gross outlays in industrial R&D divided by that same number of scientists/engineers. The third (3) is an index similar to (2) but derived on the basis of total expenditures and total scientific and engineering staff of seventeen R&D organizations–industrial, governmental, university contract, and private non-profit. The fourth (4) is again an index similar to the two previous, but representing basic research in physics. The funds are those expended for this purpose at universities and other non-profit research institutions (as explained in conjunction with figure 6), while the number of researchers is the total number of physicists employed by such institutions as given by curve 2 in figure 4 for 1953 to 1960. For 1938 I take the number as 2500, and justify it as follows: Weart (ref. 58), Table 2, 333, enables us to infer that 5/8 of the 1938 membership of the American Physical Society, i.e., 2100 individuals, were employed in higher education or non-profit, non-governmental research institutes. This is a lower bound, increased here by 15% to allow for non-joiners.

The general consonance of these indices is striking, both in the absence of any significant increase in the fifteen years before the war and in their rates of rise after its onset. These rates, and *a fortiori* the absolute values, should be accepted cautiously. All four indices are, for any given organization, strongly correlated with its size. For this reason one would expect the Milton index, curve 3, to rise more steeply than the gross index, curve 2, for it is computed for a fixed set of (individually growing) institutions. Such behavior is manifested, but only in the late 1950s and early 1960s (I have no data to compute the cost index for basic physics research between 1938 and 1953, and so have simply drawn curve 4 as a straight dashed line connecting those two points; a more realistic course is taken in figure 6.). The very considerable real difference between curve 4 and curve 2 is exaggerated by differences in accounting for the salary of the basic physics researcher. In research institutes, whether commercial, non-profit, or philanthropic, all of that salary is included, while in universities only a part thereof: in 1938 nearly none, by 1953 summer salary and research assistants, and somewhat more by 1960. Thus at Princeton in the latter 1930s only 20% of the Physics Department's total salary expense, faculty and non-faculty, was charged to separately budgeted research funds. In 1949–50 this fraction was 56%, rising gradually through the decade to 63% in 1959–60 (Princeton University, *Financial report, 1936–1960,* in Princeton University Archives). Sources (1&2): N.E. Terlecky, *Research and development: Its growth and composition* (New York, 1963), 96, 100. Sources (3): H.S. Milton, "Cost of research index, 1920–1965," *Operations research, 15* (1966), 977–991. Milton gives only index values, never dollars per researcher, so that I have felt free, after applying my deflators to her values, to normalize her index in 1927 to that for total industrial R&D. For the (unnamed) organizations from which Milton obtained data, the increase in the (current dollar) cost index ranged between two and five times from 1935 to 1950, and between one third and three times from 1950 to 1960. She found, however, that between 1950 and 1961 the average indices for the four types of R&D institutions–industrial, governmental, university-contract, and private non-profit–showed identical rates of increase.

Administration of Scientific Research and Development—just one of many similarly styled—justified their institution by observing that "in a decade there has sprung full bloom a...big, new industry of scientific research and development," and therewith an urgent need for its scientific management.[36]

This rapid growth of research and development—with military funds and for military purposes, though chiefly in industrial laboratories—was directly or indirectly of crucial importance for all those who pursued physics in post-war America. As Mervin Kelly, President of Bell Labs, told the banqueting members of the American Physical Society early in 1957, "the ivory towered existence is no more and, like it or not, the physicist is in the midst of the fast moving currents of the day.[37]

Risks of security

However physicists felt individually about other fast-moving currents of the day, nearly all regarded the panic over loyalty and internal security as a treacherous undertow. Yet, unquestionably, the national security significance the physicists had gained and claimed for themselves was one of the secondary causes prompting this postwar preoccupation of the American nation with rooting out domestic spies, treasonable policy-makers, and sometime communist sympathizers.[38]

36. L.H. Hattery, "Introduction: New challenge in administration," in G.P. Bush and L.H. Hattery, eds., *Scientific research: Its administration and organization* (Washington, D.C., 1950), 3. Similarly, J.E. Hobson, "Men in research," IRE, *Proceedings, 36* (1948), 650–651: "Not only is research big business; it is also an organized business." In the remarkably extensive bibliography on "The administration of research" appended to Steelman (ref. 10), *3,* only 5 of 70 pages mention publications on "management" proper. Of the items there listed, less than a third appeared in the two decades before World War II, while a third were published in the two years 1946–47.

37. M.J. Kelly, "The work and environment of the physicists: Yesterday, today, tomorrow," *PT, 10:4* (1957), 26–31. Bell Telephone Laboratories were different only to the extent that 40% rather than 80% of their budget came from DOD; Godement (ref. 20), 110; *Fortune, 49,* Apr 1954, 220. In 1954 *Fortune* got some ninety "outstanding young non-industrial scientists" to declare what percentage of scientists in their day were as uninformed abut world affairs as Oppenheimer claimed to have been twenty years earlier. Three-fourths doubted that more than 1% or 2% were so uninformed and an equally large majority affirmed that "scholarly training and status carry with them a responsibility to be particularly well informed on world affairs" (ref. 30), 23–24.

38. P. Boyer, *By the bomb's early light: American thought and culture at the dawn of the atomic age* (New York, 1985), 106, writes that the atomic scientists "served as the unwitting advance agents of the very anti-communist hysteria most of them deplored;" he judges them so not as devisers of new weapons, but as alarmists regarding the destructiveness of those weapons.

As early as March 1947, on the morrow of the announcement of his eponymous doctrine, President Truman issued a sweeping "Loyalty Order," prescribing a thorough loyalty investigation of every person employed by any federal agency. In practice, the investigations were restricted to certain agencies and certain classes of employees, with scientists among those who soonest found their loyalty and livelihood in question. The earliest case of wide notoriety was that of the Director of the National Bureau of Standards, Edward Condon. "No other incident of recent times," *Fortune* estimated in the fall of 1948, "has called forth more explosive statements from all ranks of science."[39]

Following passage of the McCarran Internal Security Act in the autumn of 1950, restrictions were quickly imposed on foreign travel by U.S. citizens and on visits to the U.S. by foreigners. Alfred Kastler, scheduled to address the January 1951 meeting of the American Physical Society, was unable to obtain a visa, being a member of a disapproved association of French scientific workers. His work on optical pumping, fundamental to quantum electronics, remained unknown to Robert Dicke in Princeton, among others, for another two crucial years. In these years, as "security" became equated with "loyalty," and "loyalty" with the right to participate in the institutions of

39. Walter Gellhorn, *Security, loyalty, and science* (Ithaca, 1950), 129, et passim, one of a series of studies funded by a Rockefeller Foundation grant to Cornell on "the impact on our civil liberties of current governmental programs to ensure internal security." David Caute, *The great fear: The anticommunist purge under Truman and Eisenhower* (New York, 1978), chapts. 13, 25. Ellen W. Schrecker, *No ivory tower: McCarthyism and the universities* (Oxford, 1986), is the most recent and most extensively researched study, but in its narrow construction of what pertains to the university misses a good part of what was of concern to academic physicists. Regarding scientists as particular targets of security measures, see Seidel (ref. 14), 145–148, and Rossiter (ref. 16), 316; J.T. Klapper and C.Y. Glock, "Trial by newspaper; being an account of the ordeal of Edward U. Condon," *Scientific American, 181* (Feb 1949), 16–21. Also Condon's own account in his oral history, session 3, 11 Sep 1973, 48–77 (Niels Bohr Library, American Institute of Physics). In an article entitled "The scientists," *Fortune, 38*, 1948, 106–112, 166–176, observed that "nothing has struck deeper at U.S. scientific morale...than *l'affaire* Condon....One reason for the extraordinary heat generated by the Condon affair is that it is representative of other affairs, involving men of lesser prominence, which have not come out into the open." Among those are the revocation of J.W. Mauchley's security clearance in January 1950 because someone claimed to have seen his name on a petition circulated by the Association of Philadelphia Scientists calling for civilian control of atomic energy, and, at the other extreme, the White House's refusal, two months later, to nominate A.T. Waterman as Director of the NSF because his wife was reported to have visited the Soviet embassy on two occasions. S. Augarten, *Bit by bit* (New York, 1984), Appendix: "The FBI dossier of John William Mauchley;" J. Merton England, *A patron for pure science: The National Science Foundation's formative years, 1945–57* (Washington, D.C., 1982), 127.

American society, universities began to discharge individuals whose loyalty had been questioned, and Government agencies to cancel research contracts with universities if they declined to exclude participation of such individuals. Just how seriously this latter circumstance was regarded from the university side may be judged by its having "posed," in C.V. Kidd's apologetic late-fifties view, "the only serious threat of federal control over universities and university faculty members, and has created the only fundamental difference between federal agencies and the universities in the postwar period."[40]

As Senator McCarthy's star rose, presidents wavered. In the spring of 1951 Truman effectively legitimized the Senator's tactics by shifting the burden of proof in federal loyalty reviews to the defendant, replacing "reasonable grounds exist for the belief that the person involved is disloyal" by "reasonable doubt exists as to the loyalty of the person involved." Although candidate Eisenhower declined to repudiate McCarthy, disregarding the pleadings of the Eastern Republican patriciate, many nonetheless hoped that as president he would reform the loyalty-security program. Instead he issued, in April 1953, an Executive Order—soon known as "the famous No. 10450"—requiring that all Federal employees not previously investigated were to be investigated, with yet scantier safeguards and rights of appeal. Far from taking any action to calm the anti-communist hysteria, in January 1954 Eisenhower sought to upstage McCarthy, going before both houses of Congress to boast that under his rules "more than 2200 employees have been separated from the Federal government," and to urge legislation equating participation in "the communist conspiracy" with treason and forfeiture of U.S. citizenship.[41]

40. Charles V. Kidd, *American universities and Federal research* (Cambridge, 1959), 118. Kastler correspondence in Francis Bitter Papers, MIT Archives; R.H. Dicke and P.L. Bender in the Kastler-Festschrift, *Polarisation, matière et rayonnement*, ed., Societé Francaise de Physique (Paris, 1969), 431–465. The McCarran Act figures prominently in Robert E. Marshak, "The Rochester Conferences," in R.S. Lewis and J. Wilson, eds., *Alamogordo plus twenty-five years* (New York, 1971), 211–226. Foreigners wishing to visit the U.S. must still submit to intrusive political tests. See also ref. 43.

41. A. Theoharis, "The escalation of the loyalty program," in B.J. Bernstein, ed., *Politics and policies of the Truman administration* (Chicago, 1970), 242–268; W. Bragg Ewald, Jr., *Who killed Joe McCarthy?* (New York, 1984), chiefly an intimate account of Secretary of the Army Robert Stevens' dealings with McCarthy and his associates. E. Hodgins, "The strange state of American research," *Fortune, 51* (1955), 112–115, 214–224, uses the phrase "the famous No. 10450" and quotes Vannevar Bush as stating in the *Annual report* of the Carnegie Institution of Washington for 1953 that the Executive Order "was poorly conceived and poorly executed, and has made the situation much worse than it was before." Eisenhower's quotation is from Ewald (cited above), 163, and *Public papers of the Presidents of the United States: Dwight D. Eisenhower, 1954* (Washington, D.C., 1960), 12–13.

In the summer of 1953 the Army had begun, within the framework of Eisenhower's order, an investigation of the staff of its Signal Corps Engineering Laboratories at Fort Monmouth, New Jersey. McCarthy, ever vigilant, got wind of this operation, announcing in October the discovery there of "all the earmarks of extremely dangerous espionage." The Army panicked. In short order, and in violation of the Army's own procedures, forty-two scientists and engineers were suspended from this most important military-electronic (and quantum-electronic) R&D facility. Those suspended, roughly one in thirty of the professional research staff, together supervised about 20% of the entire laboratory staff; a third of the suspendees had responsibilities at the level of section chief or above. "The Monmouth case seems to have produced two things," the mainstream illustrated *Life* observed early in March 1954: "serious demoralization of skilled laboratory personnel, and a threat from McCarthy to conduct future hearings."[42]

In December 1953, while McCarthy was using the "hearing" to assail the Army for its alleged breaches of security, Eisenhower, his aides, and his cabinet officers were confronted by an accusation that J. Robert Oppenheimer was an agent of the Kremlin. Once again, it was that disastrous combination of fear of McCarthy and fundamental agreement with his assumptions and objectives that led the President himself to order up a "blank wall" between the savant and America's secrets. Then, because Oppenheimer declined to acquiesce in the implication of his disloyalty, the administration twisted the loyalty hearing into a procedure to confirm and enforce its prejudgement of the case.[43]

42. Dupré and Lakoff (ref. 29), 131; Ewald (ref. 41), Murrey Marder, "The Fort Monmouth story," *BAS, 10:1* (1954), 21–25, 27, 225–26, 366–67; Caute (ref. 39), 479–484; *PT, 7:1* (1954), 22–24; *New York Times,* 11 Jan 1954, 1:4–5, 14:1–8, on the Fort Monmouth affair, and 30 Jan 1954, 6:6, on the session devoted to it at the New York meeting of the American Physical Society; *Life,* 3 Mar 1954, 25–33, on McCarthy and the Army; U.S. Congress, Senate, Committee on Government Operation, Permanent Subcommittee on Investigations, *Army Signal Corps Subversion and espionage: Hearings* (Washington, D.C., 1954), and *Report* (Washington, D.C., 1955); Federation of American Scientists, Scientists Committee on Loyalty and Security, *The Fort Monmouth security investigations, August 1953–April 1954* (Atomic Scientists of Chicago, 1954), 49 pp., and "Fort Monmouth one year later," *BAS, 11:4* (1955), 146–150.

43. B.J. Bernstein, "In the matter of J. Robert Oppenheimer," *HSPS, 12:2,* 195–252 (1982). Richard Pfau, *No sacrifice too great: The life of Lewis L. Strauss* (Charlottesville, 1984), 145–181, makes clear the extent to which the Oppenheimer case was Strauss' own personal vendetta. The opinion of the Security Review Board in the Oppenheimer case was particularly alarming because it substantiated the widespread impression that not only could one lose one's AEC clearance through talking to stigmatized persons, but also that, conversely, loss of or rejection for security clearance should disqualify the individual from participation in scientific as well as academic life. Amer-

Coming on top of one another, the Monmouth and the Oppenheimer affairs excited the American physicists to unprecedented levels of alarm and dismay. J.H. Van Vleck had still been rather exceptional when in January 1953, as retiring President of the American Physical Society, he had chosen to speak about "two barrier phenomena"—firstly, the "political and macroscopic" barrier to visits by foreign scientists, and, secondly, the "physical and microscopic" barrier to the inversion of the ammonia molecule, the phenomenon on which a great part of quantum electronics was being built. On this occasion he communicated a formal resolution of the Society's Council "strongly urg[ing] a more realistic approach by our government." By the end of 1953 spokesmen for science had become far more outspoken, denouncing "the mania for secrecy" and expressing concern for "our whole democratic system."[44] Fortunately, the crisis of the winter and spring 1953/54 proved a climax; thereafter the body politic began gradually to throw off this malignant disease.[45]

Such was the general environment of physics and electronics in the 1950s. But to see its significance for the originators of quantum electronics, we must look more especially at basic physical research, particularly in universities, in the decade or so following the war.

From eleemosynary to military money

World War II had so impressed the armed services with the advantages of contracting for research with industrial and academic laboratories, rather than seeking to do it all themselves in government armories, that the Committee on Postwar Research appointed by the

ican Philosophical Society, J.A. Wheeler Papers, folder "Oppenheimer, J. Robert, Nr. 3."

44. J.H. Van Vleck, "Two barrier phenomena," *PT, 6:6* (1953), 5–11. On these questions Tuve had full concurrence from his ideological antagonist in science policy, L.V. Berkner, "University research and government support," *PT, 7:1* (1954), 10–17. In this address at loyalty-oath ravaged University of California, Berkeley, Berkner, President of Associated Universities, deplored the destruction of academic freedom but strongly supported the military-academic partnership. Likewise restoration of the "wholehearted, intimate partnership among scientists, military men, and civilian policy makers" (*Newsweek*, 12 Jul 1954, 24) was the chief purpose of Vannevar Bush's numerous public statements in the summer of 1954, filled with phrases such as "dismaying situation," "striking blindly in a wave of hysteria," "sink deeper into the morass," "in our haste and in our terror," "a security system which is running wild." Bush, "If we alienate our scientists—," *New York Times magazine*, 13 June 1954, 9, 60–71. See also DuBridge (ref. 61).

45. Although not without significant relapses, as for example, the withdrawal of Condon's security clearance in the fall of 1954. Caute (ref. 39), 399, 445, 471, 484, 519, gives a fragmentary account of the gradual recovery of social sanity.

Secretaries of the Army and Navy in the spring of 1944 agreed that in the future this would continue to be their principal *modus*.[46] It could be so, however, only if the armed services were able to persuade nonmilitary institutions to accept military contracts. In particular, to maintain a hold on the very best scientists and to obtain the desired "mission-directed basic research," it was necessary to deal with the academic institutions and their fetishistic attachment to autonomy. This the services soon set about in their customary competitive way: "As far as could be ascertained, Montana State College has no research contracts with the Army or with other Governmental agencies," of the Office of Naval Research reported to headquarters. "Two Army officers followed Lt. Commander Miller into [Montana State College] President Renne's office, however, presumably to see about participation in one of the Army's research and development programs."[47]

The Navy, by tradition more open to research and technical innovation and now determined to break the Army's unearned monopoly of atomic energy, took the larger steps to put itself in an advantageous position as a research sponsor and to monopolize the military's ties with academia.[48] The Office of Naval Research, having agreed in a meeting with representatives of the Army Signal Corps and Air Corps

46. Kevles (ref. 8) and Sherry (ref. 8), 141.

47. J.E. Laurence, Head, Scientific Section, San Francisco Branch Office, Office of Research and Inventions, 19 June 1946 (ONR Records, Washington National Records Center, Accession 63A2374, 1/Nonr N6-237(00)).

48. H.M. Sapolsky, "Academic science and the military [i.e., ONR]: The years since the Second World War," in N. Reingold, ed., *The sciences in the American context* (Washington, D.C., 1979), 379–399, and Kevles (ref. 6), 353–356, discuss background, formation, and early operation of ONR. In addition to the sources cited by Kevles and Sapolsky the following public statements by ONR staff are useful: A.T. Waterman and R.D. Conrad in "American Scholar Forum: Should the scientists resist military intrusion?" *American scholar, 16* (1947), 213–225, 353–363, on 354–356; E.R. Piore, "The electronic research sponsored by the Office of Naval Research," IRE, *Proceedings, 35* (1947), 1119–1121; Piore, "Investment in basic research," *PT, 1:11* (1948), 6–9; K.E. Spangenberg and W.E. Greene, "Basic research projects under ONR contracts," *Electronics, 22* (June, 1949), 66–69; Waterman, "Government support of research," *Science, 111* (1949), 701–707; D.E. Gray, "Basic research in the Office of Naval Research," *PT, 4:9* (1951), 17–19; L.M. McKenzie, "After six years—A study of the impact of the Physics Branch Program," *Science, 118* (1953), 227–232; Piore, "Some thoughts on Federal science," *PT, 7:7* (1954), 13–15; Piore, "ONR research policy," *Naval research reviews,* Apr 1954, 6–11; G.K. Bell, "The Naval Research Advisory committee," ibid. (Jun 1954), 21–23; A.L. Powell, "The ONR Boston story," ibid. (Mar 1968), 1–20; A.T. Waterman, in U.S. Navy, Office of Naval Research, *Research in the service of national purpose* (Washington, D.C., 1966), 3–9; Mina Rees, "The computing program of the Office of Naval Research, 1946–1953," *Annals of the history of computing, 4* (1982), 102–120. See also Schweber (ref. 3).

to a tripartite sponsorship of MIT's Research Laboratory of Electronics, organized out of the OSRD Radiation Laboratory, turned right around and subjected Julius Stratton, its intended head, "to considerable pressure...to reconsider our plan with a view to having the entire project supported by the Navy alone." To a couple of street-wise New York City toughs like Columbia's I.I. Rabi and J.R. Dunning, the Navy must have appeared a patsy, falling all over itself to cover the full cost of Columbia's as yet undesigned, even undefined, "ultra-high energy machine." The Navy certainly made things easy: "In the absence of definitive information regarding the University's plans for building the accelerator, it is only possible to say that, in view of the scientific reputation of the University's staff, of its own scientific and fiscal integrity, and of the Navy's interest in securing the proposed services and facilities, the proposed total estimate of cost"—which proved to be too low by a factor of three—"is not unreasonable."[49]

As early as the autumn of 1941, with Pearl Harbor still some months away, MIT's President K.T. Compton was sure that the defense contracts he had been signing with the Federal Government "presage a new prosperity for science and engineering after the war." When the war ended, even Compton was surprised by the quantity of contract research pressed upon MIT by the military. Thus he and his like could feel that in a seller's market, as it appeared to be, they controlled the situation. The same Rabi who took ONR's money so freely for an electronuclear machine accepted only $250,000 a year from the Army Signal Corps—half what it wanted to give—to continue the Columbia Radiation Laboratory's applied research directed toward higher frequency radars and to support his own fundamental research with molecular beams as an adjunct thereto. And here again Rabi undoubtedly had the illusion of being in control of the situation, of using the military to pursue the ends of physics. The director of the

49. J.A. Stratton to K.T. Compton, 8 Feb 1946 (MIT Archives, AC-4, 204/7). In the autumn of 1944 Admiral J.A. Furer, the Navy's wartime research chief, following a visit by I.I. Rabi, who was agitated over Bush's announced intention to terminate the OSRD, had expressed to his diary a willingness of the Navy to give MIT a contract to continue the Radiation Laboratory "for an indefinite period;" quoted by N. Reingold, MS prepared for Workshop on the Military and Post-War Academic Science, Johns Hopkins University, 17–18 Apr 1986. The rationale for support of Columbia's request for $600,000 is taken from Lieutenant USNR C.B. Sears' clearance memorandum, 31 May 1946 (ONR records (ref. 47), 8/N6ORI 110(01)). How closely military considerations stood behind government support of high energy physics is clearly indicated by J.L. Heilbron, R.W. Seidel, and B.R. Wheaton, *Lawrence and his laboratory: Nuclear science at Berkeley, 1931–1961* (Berkeley, 1931), and especially by R.W. Seidel, "The postwar political economy of high energy physics," in Laurie Brown et al., eds., *Elementary particle physics in the fifties* (in press).

newly formed Research and Development Division of the Army General Staff viewed the matter from a much different perspective: "The publicly owned laboratories and drafting rooms, as well as the research and engineering staffs of our educational institutions, industries and foundations, are being put to work in as orderly a manner as possible by the research and engineering agencies of the War and Navy Departments." In this, General Aurand was but carrying out the order that Chief of Staff Eisenhower had given his "separate section on the highest War Department level," viz. that "of cultivating to the utmost the integration of civilian and military resources and of securing the most effective unified direction of our research and development activities."[50]

The university administrator, however seriously concerned for his institution's independence, could hardly resist the combined pressures of the would-be sponsors dangling contracts, his own scientists—first and foremost, the physicists—pressing for such facilities as only the military could provide, and, not least important, the leading spokesmen for science—again nearly all physicists, electrical engineers, applied mathematicians—invoking and promoting continuations of the wartime "partnership" with the military. Even so early in the cold

50. K.T. Compton, quoted from *Science*, 17 Oct 1941, by Sherry (ref. 8), 126. "Surprisingly enough," Compton's assistant R.M. Kimball remarked on 17 Aug 1945, in a letter to J.W. Chamberlin (MIT, Archives, AC-4, 36/2), "our research and development contracts have not been terminated to the extent which we felt they would be. In fact it appears that we shall be limited in the number of sponsored research contracts only by the dictates of general Institute policy." Compton claimed that the establishment of guidelines for Government-university research contracts "on a fair 'no profit, no loss' basis and in a readily workable manner constitute one of M.I.T.'s most important contributions during the war;" Compton, "Foreword," in Burchard (ref. 11). For Rabi's refusal I rely, as does J.S. Rigden, *Rabi: Scientist and citizen* (New York, 1987), 190–191, on the recollection of the Signal Corps' chief scientist, H.A. Zahl, *Electrons away; or, Tales of a government scientist* (New York, 1968), 96–98, regarding the difficulty of effecting their "concept of 'buying research'" early in 1946: "Those were strange times, for school officials feared that by accepting dollars there might also come an undesirable type of military control." Cf. Major General H.S. Aurand, "The Army's research program: Role of the Research and Development Division, War Department General Staff—organization for the future," *Mechanical engineering, 68* (1946), 785–786, 833, an address at a dinner in July 1976 sponsored by the engineers' Joint Council to honor E.O. Lawrence and his cyclotron, "symbolic of the powerful tools of modern research and of the new importance justly attached to research and development of the future." Six months later Aurand addressed 1500 physicists in New York on "the absolute necessity for the closest association between the scientist and the military." *Science, 105* (1947), 171–172. Eisenhower issued his memorandum of 27 April, "Scientific and technological resources as military assets," reproduced by S. Melman, *Pentagon capitalism: The political economy of war* (New York, 1970), 231–234, as justification for the establishment of this Research and Development Division, of which Aurand was the first director.

war as the spring of 1947, research on guided missiles in university laboratories was approved by 52% of 200 university scientists surveyed for the Steelman Report. What resistance remained was overwhelmed by remobilization for the Korean War. The year 1951, the president of the Research Corporation observed,
"has been marked by the impact of government sponsored research programs upon the educational and scientific institutions of the country." By 1953 thirteen leading private universities derived, on the average, 25% of their total annual budgets from Government-sponsored research, *exclusive* of separately funded Federal contract research centers they administered under Federal contract. Where "cooperative research" had been the shibboleth of interwar boosterism, "sponsored research" or "contract research" (often equated to "project research") dominated the pursuit and support of postwar science, in both word and deed. Thus although the universities enjoyed a seller's market, the internal pressures to sell were so strong that corporatively the academy retained no effective control. As Rabi allows now, in his and our eighties, "The whole picture changed as a result of government money....It stopped being a collegial affair. I thought it a wonderful thing...but the costs of it, which were great, I didn't realize until later."[51]

To be sure, some few physicists offered early opposition that might properly be called political to predominance of military agencies, and thus of military purposes and priorities, in the support of research—

51. Steelman (ref. 10), 205, 234, 244, 247; J.W. Barker, "Report of the president," in Research Corporation, *Annual report, 1951,* 6–8; American Council on Education, Committee on Institutional Research Policy subsequently, Committee on Sponsored Research, then Committee on Sponsored Projects, *Sponsored research policy of colleges and universities* (Washington, D.C., 1954), 28–30, and *Sponsored research in American universities and colleges* (Washington, D.C., 1968), 3–4; Rabi, conversations with Rigden (ref. 50), 191. L.A. DuBridge, physicist, who went from the directorship of the MIT Radiation Laboratory to the presidency of Caltech, found a self-serving conservative's solution to this problem of institutional autonomy: "Anything which a government supports financially, it must of necessity also control....However, when the government supports a research enterprise in any university by a contract, this is really not a subsidy to the university but only a payment for a service rendered to the government." J.E. Burchard, ed., *Mid-century: The Massachusetts Institute of Technology* (Cambridge, 1950), 367–369. The avidity of the physicists and electrical engineers stands forth clearly from every close examination of particular situations, e.g., P. Forman, "Atomichron®: The atomic clock from concept to commercial product," IEEE, *Proceedings, 73* (1985), 1181–1204; David F. Noble, *Forces of production: A social history of industrial automation* (New York, 1984), 144–146, 325–326, 351–352; Heilbron et al. (ref. 49); Koppes (ref. 8); Leslie and Hevly (ref. 15), Wildes and Lindgren (ref. 15); K.C. Redmond and T.M. Smith, *Project whirlwind* (Bedford, 1980); and Allan A. Needell, "Nuclear reactors and the founding of Brookhaven National Laboratory," *HSPS, 14:1* (1983), 93–122.

much as the physicists were moved to oppose military control of atomic energy from consideration of the ends to which it would in the future be put.[52] Yet, to a considerable extent, the steam that drove both these movements was generated less by the flame of political conviction than by the smoldering resentment of collectivization and compartmentalization in scientific war work, especially in the Army's Manhattan Project laboratories.[53] Arthur Roberts' satirical "Take away your billion dollars," composed in the summer of 1946, sang of an assembly of physicists applauding their "great man"—E.O. Lawrence—who explains "that physics now must change." With the approbation of "all the generals," the physicists, by the thousands, are to concert their efforts upon construction of a huge accelerator "at an ancient Army base." They sing:

> We have chartered transportation,/We'll provide a weekly dance;
> Our motto's integration,/There's nothing left to chance.

But just two years later Roberts could look back on the circumstances of this composition as from another era: "The AEC was not yet in existence, and all financing for new machines was being thought of as from the Armed Forces. This appeared to many people a dangerous situation." Clearly it no longer did.[54]

52. E.g., Philip Morrison predicted (*BAS, 2* (1 Nov 1946), 1–2) that "now-available contracts will tighten up and the fine print will start to contain talk about results and specific weapon problems. And science itself will have been bought by war, on the installment plan." This issue of the *Bulletin* focused on "the position which the Armed Forces are rapidly acquiring in the organization of American science," as Eugene Rabinowitch pointed out in his introductory editorial, "Science, a branch of the military?," and included (p. 11) excerpts from an article that had appeared in *Business Week*, 14 Sep 1946, under the title "Science dons a uniform....Its general direction is being set by military needs; its finances are coming from military funds." On 11 Dec 1946, Vannevar Bush urged Secretary of the Navy Forrestal to make a public statement welcoming the proposed National Science Foundation, for "such a statement would quiet at once all of this talk about the Services trying to dominate science in this country. Moreover, I believe that, among sound scientists, it would go a long ways indeed in...improving the understanding...between science and the military." Likewise Bush to James Webb, Bureau of the Budget, 27 Dec 1946, where this "utterly absurd" criticism and "utterly unnecessary discussion" are linked even more closely with Bush's belief "that the whole controversy of civilian versus military which surrounded the consideration of atomic energy legislation was entirely unnecessary, and also for that matter entirely unfounded" (Bush Papers, LC, 85/1912).

53. This aspect is ignored by Alice K. Smith, *A peril and a hope: The scientists' movement in America, 1945–47* (Chicago, 1965), though it peeks through in sources quoted, ibid., 89, 99. It has not escaped K.M. Jones (ref. 9), 125, 126, 129–131. It is stressed by Philip M. Morse, *In at the beginnings: A physicist's life* (Cambridge, 1977), 217–218.

54. Arthur Roberts, "Take away your billion dollars," *PT, 1:11* (1948), 17–21. The new editor of the then new *PT*, D.A. Katcher, had a lively interest in such matters.

On the other hand, there was considerable opposition that might more properly be called *anti*-political. This derived from a distrust of politicians and distaste for the log-rolling, pork-barrelling, and back-slapping of the democratic political process. Such anti-political inclinations moved Vannevar Bush and his elitist allies to project a post-war National Research Foundation so far above politics as to be politically unrealizable. The resultant five-year delay in the enactment of the legislation produced an irreparably stunted National Science Foundation.[55] But it was not alone the Republican statesmen of American science who held such anti-political attitudes. E.U. Condon, embattled within as well as without the Truman Administration because of his liberal sympathies and associations, stated and restated the pervasiveness of these attitudes in drafting an appeal to the President:[56]

> We have a situation in which a politically-minded cabinet member [Secretary of Commerce, W.A. Harriman] tries to get rid of a distinguished scientist [Condon] of admitted loyalty and high competence for purely political reasons. If such an attempt were to succeed...it would be a great blow to the scientific service of the Federal government since so many scientists already are mistrustful of taking positions where their status is at the mercy of politicians....It would give a basis in fact to the widespread feeling of mistrust of politicians which scientists feel.

It was just such attitudes which had made research support from private foundations particularly congenial to American academic physicists before World War II, when the Federal Government had yet to find any convincing reason to support their efforts.[57] In his valuable statistical reconnaisance of the inter-war period, Weart shows that the funds physicists received from such sources grew steeply through the

"On questioning a number of university people on their attitude to the ONR, I came away with the impression that they were in hearty approval of its program—with one reservation....Their approval hinges on the continuance in ONR's civilian administration of the present policy of sympathy and understanding with the problems and attitudes of the university." In other words, there were no real questions of political principle involved, but only the academic's urgent need for a subjective sense of autonomy and sympathetic understanding. Katcher to A.T. Waterman, 8 Jul 1948 (Waterman Papers, 31/ONR, LC).

55. A.H. Dupree, "A historian's view" (ref. 7), 180–182; N. Reingold, "Vannevar Bush's new deal for research: or The triumph of the old order," *HSPS, 17:2* (1987), 299–344.

56. Condon, draft of letter to Truman, ca. 30 Nov 1947 (Condon Papers, "U.S. Dept. of Commerce, Loyalty Board," American Philosophical Society). Condon (ref. 39), 52–53, recalled with some bitterness the personal and political cold shoulder he got from Bush in Washington.

57. A.H. Dupree, *Science* (ref. 7), chapt. 18; Kevles (ref. 6), chapt. 18; minor exceptions to this generalization are noted ibid., 274–275.

1930s. By 1940 at least one in three of the papers they published in the *Physical review* acknowledged outside support, chiefly from foundations.[58]

Among those foundations a unique role was played by F.G. Cottrell's Research Corporation, which in the 1930s helped support much of the most advanced and expensive experimental work in physics, including E.O. Lawrence's cyclotrons and I.I. Rabi's atomic beam apparatus. As the war was drawing to a close the Research Corporation, mindful of its role, drew heavily upon its resources to create a special $2.5 million fund to underwrite reconversion to peacetime research. In the first year or two after the war this money was indeed much solicited by physicists, and it made a significant contribution to restarting academic research. By 1950, however, five and six figure research contracts from the AEC and the military had made the Research Corporation's four-figure grants no longer worth the asking. Now officers of the foundations were asking—asking whether they ought not "begin to look elsewhere for effective operation." A 1953 survey of 77 major U.S. foundations revealed that the 40 initiating their eleemosynary activities *after* 1939 had largely turned their backs upon research in the natural sciences, adding only $3 million to the $23 million given for that purpose by the 37 foundations already in operation in 1939. The quite secondary role of the grants for scientific research distributed in 1953 by all these foundations collectively is indicated by the fact that their grants went in equal numbers to university faculty and graduate students, with no substantial difference in the average size of the grants—about $4,000—to these two classes of clients.[59]

58. Spencer R. Weart, "The physics business in America, 1919–1940: A statistical reconnaissance," in N. Reingold, ed., *The sciences in the American context* (Washington, D.C., 1979), 295–358, 311–313, 347–348, gives 25%, which I have increased on the assumption that about a third of the papers in the *Physical review* reported work at industrial or governmental laboratories and consequently should be eliminated from the denominator.

59. Charles H. Schauer, "How firm a foundation?" *PT, 3:10* (1950), 6–11; Frank Cameron, *Cottrell: Samaritan of science* (Garden City, 1952). Schauer evidently owed something to W.J. Murphy's editorial under the same title in *Industrial and engineering chemistry,* Jul 1950, summarized by M. Lomask, *Minor miracle: An informal history of the National Science Foundation* (Washington, D.C., 1976), 30–31. NSF, *Scientific research expenditures by the larger private foundations,* "prepared by F. Emerson Andrews, Russell Sage Foundation" (Washington, D.C., 1956), 8–10, 17; F. Emerson Andrews, *Philanthropic foundations* (New York, 1956), 256–276. By the end of 1948 ONR's Physical Science Division was supporting 500 projects with about $20 million per annum; the average for the non-nuclear (i.e., the less expensive) projects was about $20,000/year. Piore, *PT, 1:11* (1948), 7.

Faced with big bucks, neither political persuasion nor anti-political disposition were strong enough to hold the physicists to their traditional non-governmental supporting institutions, or stirring enough to compensate for scientific opportunity and excitement foregone. "The physicist knows the situation is a wrong and dangerous one," physicist Philip Morrison wrote in the autumn of 1946:

> He is impelled to go along, because he really needs the money....to do the work of the future. He needs support beyond the capabilities of the university. If the ONR...comes with a nice contract, he would be more than human to refuse. The result is necessarily bad.

When, however, one recalls how physicists rushed to take advantage of the military's money without knowing just *what* to do with it, or *how* to do what they proposed to do with it, the work question seems rather more that of the moment than of the future.[60]

In the early postwar years, even into the early 1950s, voices were raised in concern over this situation and its anticipated cultural consequences. These were not only unorthodox and untempered voices like Norbert Wiener's, deploring "the degradation of the position of the scientist as an independent worker and thinker to that of a morally irresponsible stooge in a science-factory," which "is ruinous for the morale of the scientist, and quite to the same extent it is ruinous to the quality of the objective scientific output of the country." Even a spokesman of the scientific establishment like Lee DuBridge, so often given to rationalization, might issue warnings: "When science is allowed to exist merely from the crumbs that fall from the table of a weapon development program then science is headed into the stifling atmosphere of "mobilized secrecy" and it is surely doomed—even though the crumbs themselves should provide more than adequate nourishment." Yet as president of the California Institute of Technology, DuBridge was busily sweeping up those crumbs for the nourishment of his demanding brood of scientists.[61]

60. Morrison (ref. 52): "The laboratory demobilizes," i.e., science has not learned how to demobilize. Noble, Heilbron et al., Redmond and Smith, Needell, all in (ref. 51); S.S. Schweber, "Shelter Island, Pocono, and Oldstone: The emergence of American quantum electrodynamics after World War II," *Osiris, 2* (1986), 265–302, esp. 284–285.

61. Wiener writing in *BAS* in 1948, as reprinted in his *Collected works, 4* (Cambridge, 1985), 749–750; L.A. DuBridge, "Science and national security," California Institute of Technology, *Bulletin, 58:3* (1949), 1–19, on 4. While this image was omitted from the abbreviated version of DuBridge's article published in the *Atlantic monthly*, Oct 1949, 26–29, which focused on the "appalling implications" of the "hysteria" over secrecy and security, it did stress to this wider audience that "The distressing thing is that this money [for the support of "pure science"] is coming almost entirely through agencies primarily devoted to military problems—namely, the National Military Establishment and the Atomic Energy Commission." On DuBridge, Caltech, and military research funds, see Koppes (ref. 8), 48–51, 67–68. Even E.A. Speakman, who rose

The Rockefeller Foundation, though it had already rejected the advancement of physical knowledge for its own sake back in 1933, had nonetheless continued to put money into atomic and nuclear physics—including "one spectacularly large" grant in 1940 for Lawrence's 184" cyclotron. But by the end of 1946 Program Director Warren Weaver was ready to "propose to decline, on principle, all requests in this field:"[62]

> It can be argued that, in view of the heavy federal support, it is important that wholly "free" support be also available. The magnitude of the federal support, as compared to anything that could reasonably come from the Rockefeller Foundation, makes this argument (at least in my judgment) a weak one.

Recipients had reached a similar verdict. MIT, having been granted $100,000 in 1946 for "the Rockefeller differential analyzer"—a further development of Vannevar Bush's prewar electro-mechanical device—returned the money the following summer so that "we could then throw all of our effort into the Forrester [electronic digital computer] project, which is much more amply financed"—by the Navy.[63]

Perhaps the most striking instance of the superceding of Rockefeller projects by military-sponsored work is the fate of the Princeton laboratories of the Rockefeller Institute for Medical Research. For reasons chiefly financial, the foundation decided to abandon its Princeton facilities and consolidate the Institute's

through a career in military electronics to the vice-chairmanship of the RDB—but with a Haverford College background—could see, surveying the vast sums going for military R&D, that "obviously, this phenomenon is having a profound effect on our culture." *Aeronautical engineering review, 11:4* (Apr 1952), 57–60.

62. Warren Weaver, "The physics of atomic structure," 8 Dec 1946 (Rockefeller Archives Center, RG3, 4/35–36). Weaver added to the quoted passage, "also it should be noted that universities and other sources are themselves putting in far more than we could." The RF Executive Committee agreed with Weaver's recommendation. A succinct statement of the evolution of RF policy in this regard is given by W. Weaver to Chester I. Bernard, 15 Nov 1948 (copy in the Oppenheimer Papers, LC). President R.B. Fosdick was at pains to point out, in the wake of Hiroshima, that "the only motive behind" the Foundation's prewar support of Lawrence "was to extend the boundaries of knowledge, to stimulate the search for truth, in the belief that there is no darkness but ignorance....But it is this same search for truth that has today brought our civilization to the edge of the abyss." Rockefeller Foundation, *Annual report* (1945), 7.

63. Rockefeller Foundation, *Annual report*, 1946, 168–169; James R. Killian, *The education of a college president: A memoir* (Cambridge, 1985), 42–43, quoting George R. Harrison. For "the Forrester project," see the revealing history by Redmond and Smith (ref. 51); for Bush's project, L. Owens, "Vannevar Bush and the differential analyzer," *Technology and culture, 27* (1986), 63–95, who likewise observes that "the war brought new monies which overwhelmed the older tradition of private philanthropy that had sustained the analyzer."

operations in New York City. With the completion of this removal, in October 1950, "a magnificent tract of 800 acres, and dozens of laboratory buildings were thus left without a tenant," the Office of Naval Research reported.

> Recently the University has acquired the entire property and has named it the James Forrestal Research Center. Thus Princeton was able to find suitable housing for its many Armed Forces research projects and at the same time honor one of its most distinguished alumni [the wartime Secretary of the Navy and the first Secretary of Defense]. Although the initial purchase was arranged through a private endowment fund, the operation of the Center would be impossible without government contracts—many of which were sponsored by ONR and other naval agencies.

The view from the office of *The daily Princetonian* (16 May 1952) was much the same as that from the Office of Naval Research: "The hope of the Center, according to its head, Professor Daniel C. Sayre, will seek [sic] to shift its objectives more and more to specific defense needs."[64] The view from the Office of the Research Corporation, however was not so rosy:[65]

> The finely drawn line between academic research and research at academic institutions seems to be where the cleavage occurs, and the field of physics is peculiarly adapted to its graphic illustration....while the decimal point has marched to the right, vocabularies have undergone a subtle evolution. Gradually but perceptibly, research at academic institutions has achieved a meaning no longer identical in connotation with the meaning of academic research....These changes and evolutions connote an acceptance of research as business, with certain things to be purchased by cash on the barrel head. The fact that the cash frequently fills the barrel makes it big business.

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64. G.W. Corner, *A history of the Rockefeller Institute, 1901–1953* (New York, 1964), 454–459; "Forrestal Research Center Established," *Naval research reviews*, June 1952, 10; Sayre quoted by *"The daily princetonian,"* 16 May 1952, on the occasion of the dedication of the Center. Announcement of the university's acquisition of the Rockefeller spread for $1.5 million had come in January 1951 with an assurance from aeronautical engineer Sayre that "this is still going to be basic research" (*New York Times*, 9 Jan 1951, 1, 22) and from Princeton's president Harold W. Dodds that government contracts would cover the full expenses of operation and one third of the purchase price, but in no way alter "the essence of the Princeton program." Dodd's pacification campaign included a series of congratulatory endorsements by admirals and generals commanding military research (*Princeton alumni weekly*, 19 Jan 1951, 11–15); on 28 May 1951, the *New York Herald Tribune* reported that "in both undergraduate and faculty circles the large land acquisition...is being called 'the second Louisana Purchase'" (Princeton University Archives).

65. Schauer (ref. 59), 7. Cf. Estermann, quoted in ref. 130. Cf. Hattery and Hobson (ref. 36), American Council on Education (ref. 51), 38: "Supported research is not only big business in itself but is especially big business in our major universities."

Monies for basic physical research

Having gained a qualitative sense of the realignment, not to say revolution, in the institutional foundations for the support of basic physical research in the U.S. in the aftermath of the second world war, we must try to grasp in quantitative terms the growth of the resources invested by American society and government in that activity, and something of the social consequences of that growth, from the late 1930s to the early 1960s.

Data assembled by Weart leads to an estimate of the total annual expenditure on research in physics departments at U.S. universities in the late 1930s as approaching three quarters of a million dollars (exclusive of faculty salaries), and the contribution thereto by eleemosynary foundations and other benefactors as between one and two hundred thousand dollars.[66] This is to be compared, on the one

66. Weart (ref. 58) shows that Columbia's Physics department had research funds of about $30,000 annually, 1936–39; that Harvard's and MIT's physics departments spent about $100,000 on equipment annually; and that a quarter of U.S. university research funds derived from non-governmental giving. Taking these few figures, we may approximate the universe of U.S. physics departments and their research expenditures by a triangular distribution, ranging from $30,000 for the first rank university to zero for the 51st rank institution. This gives a total of three quarters of a million dollars. This computation is confirmed by the following: From the data gathered in preparation for, and presented in support of, the Bush report (ref. 8), 121, we find that the total "direct operating expenses of research" of the physics departments at twelve unnamed leading universities in the 1939/40 academic year was $230,000, of which $97,000 derived "from non-university sources"—part governmental, part industrial, and part eleemosynary. Assuming those twelve universities were randomly distributed over the leading twenty-five in the field of physics, I figure $480,000 for the 25, and, assuming again the same triangular distribution as above, estimate $640,000 for the leading 50 universities, i.e., for the whole U.S. academic universe. In justification of equating the entire universe of academic physics with an idealized fifty departments, note that over the three years 1934/5–1936/7 exactly fifty U.S. institutions granted one or more doctorates in physics and that forty-eight institutions had associated with them one or more physicists starred in *American men of science*. U.S. National Resources Committee, Science committee, *Research–A national resource*, vol. 1: *Relation of the Federal Government to research* (Washington, D.C., 1938), 172. Of the leading foundations that reported (ref. 59) having supported scientific research in 1939, five reported grants for basic research in the *physical* sciences, totaling $208,000, and one reported expending $673,000 for such research by its own staff—presumably the Carnegie Institution of Washington, and thus largely astronomical research (ref. 59). The former figure seems consistent with a claim of something more than $100,000 for academic physics alone, inasmuch as in 1953 the $0.9 million of non-Federal monies expended for separately budgeted basic research in physics at U.S. universities (ref. 67) stood in somewhat the same proportion to the $1.7 million provided in 1953 by private foundations for basic research in all physical sciences (ref. 59).

hand, with the roughly forty thousand dollars of laboratory expenditure on research in 1900 by U.S. academic physics departments (estimated by Forman, Heilbron, and Weart) and, on the other hand, with the $4.3 million of non-Federal funds expended in 1953/54 (by the NSF's estimate) on basic research in academic physics departments. Halving the latter figure and adding a third to the former brings both to 1938 dollars, and gives roughly equal average rates of increase in the two periods, 1900–1938 and 1938–1953, viz. $\sim$ 7% per annum, doubling every ten years, plus or minus a year or so.[67]

Meanwhile *Federal* funds for unclassified basic research in academic physics departments had grown from nearly zero throughout the prewar period to about $19 million in 1953, in which year perhaps another $19 million were spent on similarly basic physics research at Federally-funded extra-governmental research institutes, both those closely associated with universities (such as Berkeley's Radiation Laboratory, the University of Chicago's Midway Laboratories, and MIT's Research Laboratory of Electronics), and those less closely associated (such as Argonne, Brookhaven, and Oak Ridge National Laboratories). Yet another $2 million (in part again Federal Government money) was spent at other non-profit, non-governmentally sponsored research institutions (such as Carnegie Institution of Washington, Battelle Memorial Institute, Stanford Research Institute). All these contributions, together with the $4 million of the previous

67. NSF, *Reviews of data on research and development,* Nr. 2: *Funds for research and development in colleges and universities, 1953–54,* "NSF 57–9" (Washington, D.C., 1957), 4 pp., and in greater detail, NSF, *Scientific research and development in colleges and universities: Expenditures and manpower, 1953–1954,* "NSF 59–60" (Washington, D.C., 1959), 173 pp., estimated university expenditures of non-Federal funds for separately budgeted research (with physics attributed $0.9 million, less than 2% of the total) and university expenditures for "departmental research," i.e., research outlays which are neither overhead nor separately budgeted (with physics attributed $3.4 millon, i.e., less than 7% of the total). P. Forman, J.L. Heilbron, and S. Weart, *Physics circa 1900,* "Historical studies in the physical sciences, vol. 5" (Princeton, 1975), 121. This secular rate of growth of funding is about half the 15% annual growth in the literature of vital areas of twentieth century science evidenced by Henry W. Menard, *Science: Growth and change* (Cambridge, 1971), 27–31, 51, et passim., as also in the growth of American physics department budgets, 1900–1910, found by Forman, Heilbron, and Weart (pp. 8, 63). Fifteen percent is indeed the figure for growth of research funds that spokesmen for postwar physics have repeatedly advanced as optimal (see, e.g., Orlans, ref. 2). However, so high a rate of increase of funding is anomalous, and not sustainable. The exceptionality of the earlier period is suggested by T.C. Mendenhall's remark of 1914: "The enormous sums of money now available for scientific investigation...would have been looked upon as a wild dream a quarter of a century ago." T.C. Mendenhall to Arthur L. Day, 28 Feb 1914, in California Institute of Technology Archives, G.E. Hale Papers, microfilm 46, 284–294 kindly drawn to my attention by N. Reingold.

paragraph, add up to forty-four million 1953 dollars. Lumping thus together all non-profit, not strictly governmental research institutions—as the NSF has done (see figure 6)—comparison with the prewar level of support requires that the $700 thousand estimated above for the academic enterprise circa 1938 be supplemented by expenditures on basic physics around that date in non-university, non-profit research institutions. A total of one million 1938 dollars, equivalent to two million 1953 dollars is, I surmise, not far from the mark. Thus in 1953 real funds expended in the U.S. for the increase of fundamental physical knowledge were 20-25 times greater than those expended in 1938, rather than the three or four times to be expected from the secular trend.[68]

68. J.H. McMillen, "Government support of basic research in university physics departments, 1952–53," *PT, 7:5* (1954), 7–9, gives the total of this support as $17.1 million in FY '53 (the 1952–53 academic year), of which $9 million was for the support of large accelerator programs and slightly more than $9 million came from the AEC. For the following fiscal/academic year this inventory was broadened and reported by the NSF as "NSF 57–9" and "NSF 59–10" (ref. 67). Here Federal expenditures for separately budgeted research in physics at colleges and universities, "excluding...Federal research centers operated by contract with educational institutions (e.g., Radiation Laboratory, University of California)," are estimated at $21.9 million, of which $16.6 million are attributed to DOD and only $4.8 million to AEC; of this $21.9 only $16.3 million were for basic research, down from the $17.1 million given by McMillen for the previous academic/fiscal year. While this last figure is consistent with an overall drop of 4% in Federal funding of basic research in the physical sciences from FY '53 to FY '54 to be inferred from NSF, *Federal funds for science, 3* (1954), 35, and *4* (1955), 31, it is inconsistent with the 2% increase in (incomplete) total Federal obligations for basic research in *physics* ($33.6 million in FY '53, $34.4 million in FY '54) given by NSF, *Funds for scientific activities in the Federal Government, fiscal years 1953 and 1954* "NSF 58–14" (Washington, D.C., 1958), 26, 105, and particulary with McMillen's figure of $9 million of AEC monies in FY '53, in as much as AEC obligation for basic research in all physical sciences increased from $31 million in FY '53 to $37 million in FY '54, and the AEC ascribed nearly 60% of its FY '54 obligations for basic research in the physical sciences to physics proper (ibid., 108). Thus my $19 million in university physics departments in calendar 1953 seems justified. I arrive at a second $19 million expended at Federal contract research centers as follows: NSF, *Basic research, a national resource,* "NSF 57–35" (Washington, D.C., 1957), 36–38, estimated that at such centers administered by academic institutions $33 million of the $130 million spent on R&D in 1953–54 was "basic in nature." Of this I ascribe $15 million, or 42%, to physics, for this is the percentage of all basic research at "Federal research centers" in 1957 ascribed to physics in NSF, *Reviews of data on research and development,* Nr. 18: *Research and development expenditures of selected groups of nonprofit institutions, 1957,* "NSF–60–7" (Washington, D.C., 1960), 4 pp. Another $2 million is from such centers administered by other non-profit organizations, and yet another $2 million is similarly derived from such centers administered by industrial firms; altogether, $19 million. It is by no means certain that all relevant Federal outlays are comprehended in these figures. Thus, for example, the most complete listing of Federal contract research centers that I have encountered, NSF, Office of Special Studies, *Methodological aspects of statistics on research and development costs and manpower,*

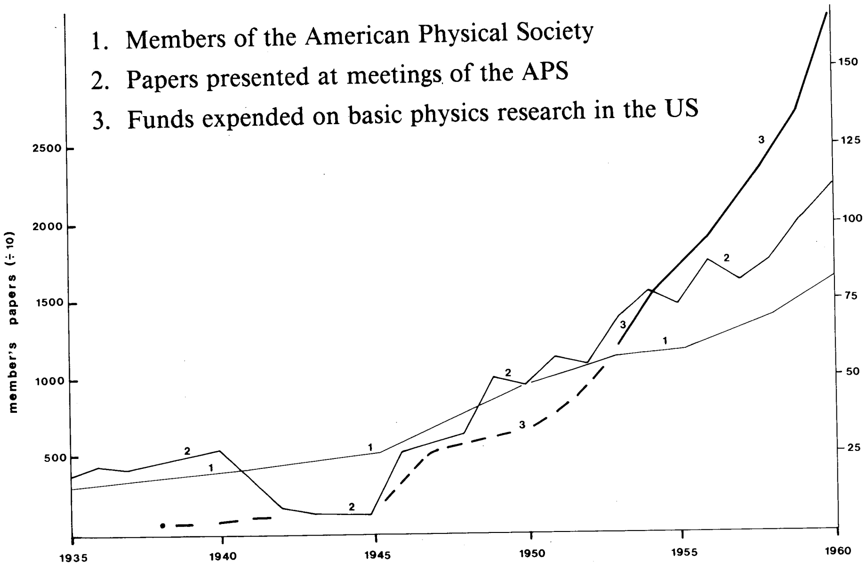

FIG. 6 PHYSICISTS, FUNDING, AND OUTPUT IN BASIC RESEARCH, 1935–1960. American Physical Society membership data from Figure 4, curve 3. To obtain numbers of papers presented at APS meetings year by year counts were made of abstracts published in its *Bulletin,* Funds for basic physics research 1953–1960 are the sum of all basic research funds expended by non-profit institutions (universities, Federally funded research centers, and other non-profits), as given by NSF, *National patterns of R&D resources: Funds and manpower in the United States, 1953–1977,* "NSF–77–310" (Washington, D.C., 1977), 24, reduced to constant dollars, and further reduced by a factor one fourth to give physics' share of such funds, as derived from my estimates (ref. 68) of research funds for basic physics in the year 1953 (cf. ref. 75). Likewise, the figure for 1938 is my reasoned estimate (ref. 66). The dotted line joining those two years is a guess at the course of physics funding.

"NSF 59–36" (Washington, D.C., 1959), 87–89, does not include Princeton University's Matterhorn Project. A large part of this program in plasma physics and controlled nuclear fusion, wholly supported by AEC funds to the tune of $15 million annually by 1959—a half million annually in 1953–54, when still classified secret—would be counted as basic research by any current definitions. But it would appear to have been counted neither as a Federal contract center, nor as a project of the Princeton University Physics Department, which it certainly was not. See notes to figure 5 and Bowen (ref. 109). For completeness an estimate is wanted of expenditures for basic research in physics at profit-making, research laboratories. This figure, $722,000 in 1953, was provided by the Maxwell Research Center of Syracuse University: NSF, *Research and development by nonprofit research institutes and commercial laboratories, 1953,* "NSF–56–15" (Washington, D.C., 1956), 46.

Whereas in the governmental and industrial sectors the number of scientists and engineers engaged in research and development doubled between 1941 and 1952 while current dollar expenditures on R&D in these sectors quadrupled, in universities and non-profit research institutes professional staff increased more than three times and expenditure more than ten times in these same eleven years.[69] Then, through the 1950s, R&D current dollars spent by universities, and also by non-profit research institutes, fell into step with total U.S. outlays for research and development, doubling every five years.[70] Of this growth academic physics appears to have had somewhat more than its share, multiplying its funds more than five times in the ten years 1953/54 to 1963/64, for a *real* growth of 15% per annum (figure 6). Indeed, the Federal funding rocket, now also fueled by NASA, in a final spurt before burning out in the mid-sixties, lifted the total level of government support of basic physics research FY '59 to FY '63 at a rate somewhere between 20% and 40% per annum.[71]

69. U.S. Dept. of Defense, Assistant Secretary of Defense (R&D), *The growth of scientific research and development* (Washington, D.C., 1953), 10–12; NSF, *National patterns* (notes to figure 6), 24. In those eight years, 1953–61, basic research funds spent by U.S. colleges and universities deriving from non-Federal sources (state and local governments, industry, foundations, and internal university funds) also grew very rapidly, increasing four-fold in current dollars, three-fold in constant dollars.

70. A striking demonstration of nearly exact proportionality of national and university R&D expenditures was provided by F.R. Bacon, Jr., and K.A. Remp, *Electronics in Michigan* (Ann Arbor, 1967), 121. They set in parallel columns annual total R&D expenditures in the U.S. aircraft, missiles, electrical equipment, and communications industries, 1956–1963, on the one hand, and, on the other hand, total expenditures in those years on electronic-aerospace research at the four Michigan state universities. The ratios of those two figures remained constant to within a few percent.

71. NSF, *Scientific research and development* (ref. 67), 31, gives $23 million for separately budgeted research, basic and applied, in physics at U.S. universities and colleges in FY '54, to be compared with $117 million in 1964 given by NSF, *Scientific activities at universities and colleges, 1964, "NSF 68-22"* (Washington, D.C., 1968), 42. Over these ten years, physics' share of all R&D funds expended on all sciences at all U.S. universities and colleges, excluding associated Federally funded research centers, fell from 11% to 9.4% (while chemistry, as Thackray et al. (ref. 31), 175, show, fell from 7.1% to 5.5%) as a result of the very rapid rise in the early 1960s of engineering, environmental, and, especially, biomedical research. Unfortunately, breakdowns of research funds, Federal and/or other, *both* by scientific field *and* by type of institution dispensing and/or expending those funds are very rare before the late 1960s. In particular NSF, *Scientific research and development in colleges and universities: Expenditures and manpower, 1958, "NSF 62-44"* (Washington, D.C. [1963]), which stands alone between 1953 and 1964, gives data so aggregated as to be useless for our purpose. The spurt in Federal funding of basic physics is reported and extrapolated by the NAS-NRC Physics Survey Committee, *Physics: Survey and outlook: A report on the present state of U.S. physics and its requirements for future growth* (National Academy of Sciences, 1966), 85–88. For the burnout of the Federal funding rocket in the mid-sixties see Weart (ref. 58), 327–331. It is greatly to Derek Price's credit that he not only anticipat-

By the early 1950s, academic physicists had switched horses completely. More than a hundred educational institutions received AEC funds, and nearly two hundred DOD funds.[72] A survey of 750 U.S. colleges and universities in the spring of 1951 for the DOD's Research and Development Board found that, averaged through all of academia, 70% of all research time of physics faculty and senior research personnel was "devoted to defense research," i.e., spent on studies sponsored by military agencies and/or the AEC. As Table 1 shows, that percentage was higher still in universities doing the greatest amount of research in physics.[73] Of nearly 300 papers contributed to the spring 1951 meeting of the American Physical Society, two-thirds reported work by individuals with U.S. university affiliations. Of these nearly 200 papers, again two-thirds acknowledged support: 6 that of the Research Corporation, 2 that of non-military governmental agencies, and 115 that of the AEC and/or one or more military funding agencies—overwhelmingly the ONR. This distribution of acknowledgements returned corresponds rather closely to that of the funds received, namely five percent from non-Federal extramural sources, and two percent from non-military Federal sources: In 1949, by the Bureau of the Budget's accounting, DOD and AEC were together responsible for 96% of all federal dollars spent by colleges and universities for research in the physical sciences; in FY '54, by the NSF's accounting, 98% of the $22 million bestowed by the Federal Government on academic *physicists* came through the Department of Defense and the AEC.[74]

ed a cessation (though not the abrupt deceleration) of funding growth, but also that his prescience was the product of historical study; *Little science, big science* (New York, 1963), chapt. 4.

72. NSF, *Federal funds for science, 1* (1952), 41. As of the end of FY '49, the Air Force had 350 research contracts with non-profit institutions, according to General D.L. Putt, Director of R&D; *Aeronautical engineering review, 9:3* (March 1950), 41.

73. American Society for Engineering Education, Engineering College Research Council, Committee on Relations with Military Research Agencies, *University research potential: A survey of the resources for scientific and engineering research in American colleges and universities* (ECRC, 1951); A.F. Spilhaus and J.I. Mattill, "Summary of resources for university research," *Journal of English education, 42* (1952), 270–279. Although when averaged through all of academia, certain engineering fields had a larger percentage of faculty research time on military sponsored projects—aeronautical engineering, 82%; electronics, 81%; electrical engineering, 76%—none of these came close to physics in the total amount of research time so sponsored. Only chemistry ran a close second (5/6 of physics' total time on defense research), but only because the academic chemistry establishment was so large; overall 38% of academic chemical research time was sponsored directly or indirectly (through industry) by defense agencies.

74. Abstracts of APS papers in *Physical review, 83* (1951), 197–246; BOB's 1949 figures given by Kevles (ref. 14), 359; NSF's 1954 figures from NSF, *Reviews of data* (ref. 71), and NSF, *Scientific research and development* (ref. 68), 35. C.S. Gillmor,

By 1960 the sum of DOD and AEC contributions to federal obligations for "physics research" ($321 million), as also for "basic research in physics" ($139 million), performed by institutions of all types, academic and nonacademic, had slipped to 92% with the rise of NSF and latterly of NASA (three-fourths of whose efforts went, by the by, toward development of technical capabilities with military utility). This trend is reflected in the distribution of acknowledgements at the spring 1960 American Physical Society meeting, where DOD and AEC garnered only about 90%.[75]

Through the 1950s, the only significant sources of funds for academic physical research in the U.S. were the Department of Defense and an Atomic Energy Commission whose mission was *de facto* predominantly military. The AEC and the DOD each provided roughly half of all funds for unclassified basic research in university physics departments, though considerably more than half came from DOD if funds for construction and operation of particle accelerators are omitted. In the case of solid state science, from FY '52 through FY '57 nearly 80% of Federal support for fundamental research came from DOD, 20% from AEC (Table 2). During these six years NSF's share in this support increased by a factor of ten: from an absolutely insignificant 1/3% to a relatively insignificant 3%. And although NSF's budget grew by another factor of ten in the following eight fiscal years, its share of all Federally funded basic research remained a constant 10%: The mission agencies, with but a small fraction of their

"Federal funding and knowledge growth in ionospheric physics, 1945–1981," *Social studies of science, 16* (1986), 105–133, examining the acknowledgments attached to 259 articles in ionospheric and magnetospheric physics by university researchers, 1945–1981, found that "military support dominated the picture in the twenty years after World War II," reaching 100% in 1953. "Within the military, twenty-six offices spread across the services were involved."

75. Counts for the spring 1960 are taken from American Physical Society, *Bulletin, 5:4* (1960); Federal FY '61 obligations from American Institute of Physics, *Physics: Education, employment, financial support: A statistical handbook, 1964* (New York, [1964]), 66–69, where it is noted that "in recent years, physics has received about one-fifth of the total research funds for the physical sciences," the category for which NSF regularly published figures. The NSF provided this more detailed data to the AIP, as also to the NAS-NRC Physics Survey Committee (ref. 71), 86, which, however, undertook to make its own surveys to estimate the funding, from all sources, of basic research in the several fields of physics in FY '63 ((ref. 71), 91–94, and the companion volume, ...*Reports of the subfields*). Generally NSF and the Physics Survey Committee agreed remarkably, differing substantially only on solid state physics, where the physicists had employed a much more liberal conception of basic research than had the NSF. James Webb's 75% appraisal is quoted by Koppes (ref. 8), 115.

budgets going into basic research, grew as fast or faster.[76]

Table 1

LEADING ACADEMIC PERFORMERS OF PHYSICS RESEARCH, 1951

Institution	Faculty-level researchers	FTE faculty-level researchers	Defense sponsored FTEs	% FTEs defense sponsored	Graduate student researchers	FTE graduate researchers
Penn. State College	32	19	19	100	67	40
Yale Univ.	29	20	20	100	77	46
Chicago, Univ.	25	20	20	100	60	40
Denver Univ.	16	12	12	100	26	15
Tulane Univ.	16	10	10	100	26	20
Minnesota Univ.	14	7	7	100	25	18
Penn. Univ.	13	8	8	100	32	17
Notre Dame, Univ.	11	6	6	100	36	10
Michigan, Univ., Ann Arbor	57	40	37	93	106	54
Johns Hopkins Univ.	13	10	9	90	39	25
Boston Univ.	18	14	12	86	45	-
Northwestern Univ.	15	7	6	86	24	8
Illinois, Univ., Urbana	39	24	20	83	63	43
Rutgers Univ.	12	6	5	83	18	10
Calif., Univ., Berkeley	27	11	9	82	150	40
Texas, Univ., Austin	45	23	18	78	33	15
New York Univ.	32	27	21	78	62	28
Colorado, Univ., Boulder	26	18	14	78	39	20
Rochester, Univ.	17	13	10	77	30	20
Columbia Univ.	27	20	15	75	120	60
Cornell Univ.	24	20	15	75	91	50
Stanford Univ.	19	12	9	75	35	22
Brooklyn Poly Inst.	14	12	9	75	34	20
Mass Inst. of Tech.	36	18	13	72	132	66
Princeton Univ.	39	20	14	70	64	30
Purdue Univ.	23	10	7	70	79	50
Carnegie Inst. of Tech.	20	10	7	70	83	30
Wisconsin, Univ., Madison	20	12	8	67	85	50
Calif. Inst. of Tech.	30	20	13	65	30	20
Iowa State College	32	17	10	59	32	17
Washington, Univ., Seattle	16	13	7	54	28	14
Ohio State Univ.	24	10	5	50	121	30
Case Inst. of Tech.	15	10	5	50	40	15
Lehigh Univ.	14	6	3	50	14	7
Rensselaer Poly Inst.	12	4	1	25	16	7

76. McMillen (ref. 68) reports that $9 million of a total of $17 million of Federal government support of basic research in university physics departments in FY '53 went for construction and operation of large accelerators, chiefly, but not solely, from the AEC. Outlays for accelerator construction and operation by agency, 1946–1958, which do not however distinguish universities and National laboratories, are quoted by R.W. Seidel (ref. 14). The statistics on solid state science, compiled by W.R. Gruner for ONR, Solid State Advisory Panel, "Report" (ref. 28), 69–70, refer to all Federally funded basic research outside government laboratories; see table 2. NSF's funds for academic physics began at the level of prewar private giving—$280,000 in FY '53—and by FY sics at the National Science Foundation," *PT, 9:2* (1956), 10–13; "A decennial look at NSF's research grants program in physics," *PT, 13* (1960), 40–42; U.S. Congress, House, Committee on Science and Astronautics, *The National Science Foundation: Its present and future* (Washington, D.C., 1966), 10.

Under these pressures physics changed. "There is a growing conviction among my friends in academic circles," Merle Tuve recorded in 1959, "that the university is no place for a scholar in science today, because a professor's life nowadays is a rat race of busyness and activity, managing contracts and projects, guiding teams of assistants,

NOTES FOR TABLE 1

Listed are those U.S. institutions of higher education with more than ten faculty (assistant professor and above), or full-time research personnel of equivalent rank, engaged in research in physics in Febrary 1951 (column 1), as reported in the survey cited in ref. 73. Column 2 gives reported full-time equivalent research effort (35–40 hours per week) of the individuals enumerated in column 1. Column 3 gives the number of such full-time equivalents "engaged in research under contract for military agencies or the Atomic Energy Commission, or in research directly related by sub-contracts with government or industry to defense needs," while column 4 expresses this number as a percentage of column 2. Averaging over these thirty-five leading academic performers of physics research, 79% of the research effort of faculty-level physicists was supported by agencies with weapons in mind. That these thirty-five institutions had, however, only two-thirds of the FTE's so supported indicated how widely military monies were spread through academia by this date.

In columns 5 and 6 we give the figures reported for "the total number of instructors, graduate students, and teaching and research assistants...engaged full- or part-time in research in the department or field," and the number of full-time research workers to which the reporting institution judged these to be equivalent. No report was made of the source of support for this work. Some institutions responded for their engineering departments only, notably Harvard University.

I have interchanged the reported entries in columns 2 and 3 for University of California, Berkeley, Cornell University, and Johns Hopkins University, for I suppose that a prior inadvertant interchange was responsible for the military supported FTEs being reported to be greater than the total FTE researchers. An alternative explanation why entries in column 3 occasionally exceed those in column 2—while never exceeding those in column 1—is that the number supplied is not that of the *FTE* s supported by military funds but of the individual faculty researchers so supported. Such a misconstruction of the questionnaire, which the evaluators at any rate seem not to have surmised, would presumably be rare enough to have no great effect upon the results.

and bossing crews of technicians, plus the distractions of numerous trips and committees for government agencies, necessary to keep the whole frenetic business from collapse." This radical alteration of the quality of scientific life, though deriving immediately from the terms of social organization and integration of physics in the U.S., resulted mediately from the quantitative growth in the number of physicists and their increased research orientation, with commerce succeeding community in the Physical Society meetings early in the decade.[77]

Table 2

Federal obligations for "fundamental research in solid state science" performed in non-governmental laboratories, Fiscal Years 1952–1957. Figures, in millions of current (non-constant) dollars, drawn from ref. 28; cf. ref. 76.

Agency	Fiscal Year					
	1952	1953	1954	1955	1956	1957
Navy: Office of Naval Res.	1.1	1.0	.9	1.2	1.0	1.3
Army: Signal Corps	1.1	1.3	1.4	1.1	1.5	1.6
Army: Office of Ordnance Res.	0.2	0.4	0.4	0.3	0.4	0.4
Air Force: Office of Scientific Research	-	1.1	0.9	1.0	2.1	2.4
Atomic Energy Commission	0.7	0.7	0.7	0.6	0.9	1.4
National Security Agency	-	-	-	-	0.1	0.1
Total National Security	3.1	4.4	4.3	4.4	5.9	7.1
National Science Foundation	0.01	0.07	0.10	0.10	0.14	0.25

As may be seen from figure 6, the number of papers delivered at American Physical Society meetings by its members grew even more rapidly than the membership. Other measures of this intensified research orientation are the increase in the fraction of physicists specifying research as their principal work activity—rising from 50% to 60% between 1951 and 1958—and the increased publication rate of

77. From the early 1920s to 1955 the American Physical Society held 6 to 8 meetings annually. The average number of abstracts published from each meeting was about 45 in the late 1920s, 55 in the late 1930s. After dipping to about 20 in 1943 and 1944, the number of papers presented per meeting was up to 100 by 1947, to 150 by 1950, and to 200 by 1953. Tuve (ref. 2), 171, quote; F. Reif, "The competitive world of the pure scientist," *Science, 134* (1961), 1957–1962. S.R. Weart, "The last fifty years—a revolution?" *PT, 34:12* (1981), 37–49, in a thought-provoking essay, describes this "break" in the 1950s, but argues that, whether viewed conceptually or socially, the transformation of American physics since the early 1930s has been far less revolutionary than in the preceding fifty years.

physicists in the five years following their receipt of the doctorate—about twice as high for 1951 Ph.D.'s as for 1936 and 1946 Ph.D.'s.[78] While research output per physicist was rising slightly, output per dollar spent on basic research was plummeting (figures 5 and 6). Research was getting continually more expensive and also bigger in scale of instrumentation and organization. A rare opportunity to look more closely into this process is given by a breakdown of "basic" research support provided by the Air Force Office of Scientific Research. It shows a 40% increase from 1954 to 1960 in the amount spent on supplies and equipment and also in the amount spent on "other" scientists and technicians for each dollar spent on the principal investigator's salary.[79]

Thus in the fifteen years following the war, the central fact of scientific life in physics was unprecedented growth based upon military funding. Yet however "significant" the funds made available for basic research, the total of such funds for all fields of science represented only a relatively insignificant fraction—roughly 5%—of the military's outlays for research and development (figure 7).[80] This rule of the twentieth arose in the earliest efforts to attach basic research to military missions—vide the basic research group established at the

78. Between 1953 and 1960 the 50% increase in the number of words published annually by the *Physical review* (and *Physical review letters*), and also by a set of ten leading U.S. physics and applied physics journals, exactly paralleled the 50% increase in the American Physical Society's membership. American Institute of Physics, *Physics manpower and educational statistics, 1962* (New York, [1962]), 23, 30. Publication rates of newly minted physics Ph.D.'s are from Arthur D. Little, *Basic research in the Navy: Report to the Secretary of the Navy by the Naval Research Advisory Committee*, 2 vols. (Cambridge, 1959), *2*, 50–55. The particularly strong research orientation of the "nuclear" physicists is indicated by the fact that though in surveys made in 1949–55 only 15% identified themselves as "nuclear" physicists, in 1949 63% and in 1959 51% of the papers published in the *Physical review* were in nuclear or high energy physics. NSF, *Scientific personnel resources: A summary of data* (Washington, D.C., 1955), 33; *PT, 3:9* (1950), 20–22; *6:2* (1953), 14–16; *9:1* (1956), 32–36; NAS-NRC (ref. 31), 625.

79. E.D. Brunner, *The cost of basic research effort: Air Force experience, 1954–1964* (RAND Corporation Memorandum, RM-4250-PR, Feb 1965), xii + 52pp. Nearly all of this research support went to universities, and there were no evident major differences between sciences. D.J. de S. Price (ref. 71), 83, citing *Science, 134*, (1961), 2017–2024, reports a doubling of the average current-dollar size of NIH biomedical research grants between 1950 and 1960.

80. J.H. Rubel in NSIA Symposium (ref. 27), 26: "Ninety to ninety-five percent of our research and development expenditures are, of course, outside the 'research' area." Ninety-five would probably be closer to the mark: Trudeau (ref. 3), 23, gives 5% of Army R&D expenditures for basic research circa 1960. The Navy's figures show a higher percentage: A.D. Little, Inc. (ref. 78) and U.S. Navy (ref. 12). However, NSF, *Federal funds for science, 1* (1953), 39, *2* (1953), 29–33, *4* (1955), 28, 31, *5* (1956), 33, 38, *8* (1959), 48, 68, consistently give 5% as basic research's share of Federal expenditures on R&D in the physical sciences.

Johns Hopkins Applied Physics Laboratory in 1946 and promptly referred to intramurally as "the five-percenters."[81] The observance of this rule almost religiously even unto the present day results not from any intrinsic quantitative dependence of technical development upon "basic" research, but because a twentieth is the largest amount still relatively insignificant. It is the highest still inappreciable rate of taxation on social investment in advanced technological enterprise.

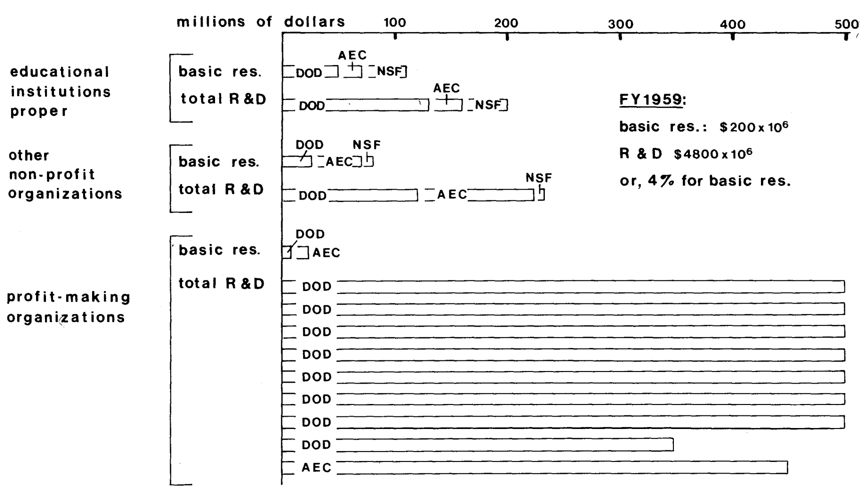

FIG. 7 AEC, DOD, AND NSF OBLIGATIONS FOR R&D AND FOR BASIC RESEARCH, FY '59, BY TYPE OF RECIPIENT ORGANIZATION. Funds are in millions of current dollars. "Other non-profit organizations" include National Laboratories and Federally Funded Research Centers at educational institutions. Omitted here are the Federal Government's intramural laboratories, dominated by the DOD with $1260 million for R&D, of which $35 million were for basic research. Before this date equally complete breakdowns by type and recipient of R&D funds are not available. Already by this date NASA's $190 million, with 60% alleged "basic," severely complicated the accounting of Federal in-house research funds, and after this date that of out-of-house research as well (see the torments of the Physics Survey Committee in ref. 71, 81–88). Source: NSF, *Federal funds for science, 11* , "NSF-60-80" (1960), 52, 53, 77.

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81. M.A. Dennis, "Making space: Sounding the territory of the upper atmosphere research archipelago, 1944–46," MS prepared for Workshop on the Military and Post-War Academic Science, Johns Hopkins University, Apr 1986. Cf. Tuve (ref. 2), 174: "When the opportunity for much larger post-war funds appeared..., we excused ourselves by saying that even five per cent of these new big sums would underwrite larger opportunities than were ever previously available for studying the basic problems which really interested us." And earlier still, Herbert Hoover (1927) as quoted by A.H. Dupree, *Science in the Federal government* (ref. 7), 341.

The point ever to be borne in mind is that these relatively insignificant funds for significant physics would not have been available apart from the military's major R&D outlays—just as the 5% of the Strategic Defense Initiative budget slated to support mission oriented basic research, chiefly in the universities, would certainly not be available apart from the 95% spent otherwise and elsewhere on the "Star Wars" mission. The difference is that today even the 5% is encumbered by an explicit mission orientation.[82]

Patronage and the direction of research

All the foregoing leads unavoidably to the question: What *direction* of the advance of science, and thus what *kind* of science, resulted from military sponsorship? In the case of academic science, we have been too ready to accept the rosy recollection of "those of us in educational institutions"—in this case MIT's past president, James Killian—"who experienced this benign, nonintrusive relationship with government," too ready to believe that this system of patronage aimed chiefly at "the advancement of the best possible physics," too ready to ignore the real rationale for the support of university research manifest in that 95% spent otherwise and elsewhere.[83]

82. Cf. E. Bloch's (ref. 1) mission for the NSF today: "During most of the postwar period, primary reliance was placed on the mission agencies to support research, with a resulting inherent bias toward research that is relevant to these missions and away from basic work....For these reasons, we are proposing a major shift of resources toward the nation's universities..., the Engineering Research Centers. Each center focuses on an important area of engineering." Such double talk expresses not merely the general utilitarian illiberalism of the 1980s, but more particularly U.S. "national policy for controlling the flow of science, technology, and engineering information produced in federally-funded fundamental research at colleges, universities, and laboratories" established by National Security Decision Directive 189, signed by President Reagan on 21 Sep 1985.

83. J.R. Killian's rosy view of the dawn of our era (ref. 63), 49, derives in part from his "wish[ing] desperately that the same spirit will be renewed in the 1980s." Consequently, it is not surprising that he has forgotten much that hardly squares with his picture of "this benign, nonintrusive relationship," in particular MIT's serious and protracted conflict with ONR over the management of the Whirlwind computer project, 1948–50. Redmond and Smith (ref. 51), chapt. 7. In general, conscientious university administrators experienced these early phases of their dependence upon the Federal Government as "complex and distressing;" H.W. Dodds, President, Princeton University, "Sponsored research," *PT, 7:1* (Jan 1954), 4–5, reprinted as "Project research," *American scientist, 42* (1954), 128–130, described by Kidd (ref. 40) as "a well-known essay." ONR's New York office kept very strict financial control over expenditures on the construction of Columbia's (likewise way-over-budget) synchrocyclotron, including control of salary and wages of staff (ONR records, Washington National Records Center, 63–2374, 8 (N6ORI 110(01)). The final quoted phrase is Kevles' (ref. 6), 366.

A bit too crass perhaps is the golden rule—"ye with the gold, rule"—put forward by James Ionson, director of SDI's 5%-for-basic-research program. Yet declarations earlier heard from program managers at the Office of Naval Research or the Air Force Office of Scientific Research "that our primary mission includes the most expeditious expansion of all the frontiers of the science" are also not fully credible.[84] Rather we should listen to vintage 1952 Killian:[85]

> The Department of Defense...has steadily supported the concept of basic research in those areas where the advance of knowledge is important to the military effort. The Department has only recently reaffirmed this policy, which provides for the support of basic research performed as an integral part of programmed research committed to specific military aims and for the support of academic research that promises ultimate military application.

While nuclear physics had already set its course of development and established its priority as a research field before the war[86] —so that the advent of nuclear weapons served principally as a powerful booster—the greater and rapidly growing part of what we identify as

84. James A. Ionson, interview by Daniel S. Greenberg, *Science and government report, 15:7* (15 Apr 1985); Program Manager A.G. Horney, addressing American Chemical Society, Division of Petroleum Chemistry, 23 Mar 1962, as quoted by Nick A. Komons, "A decade of chemical research: History of the Air Force Office of Scientific Research Chemistry Program" (Historical Division, Office of Aerospace Research, 1962), 41, and "A summary view of the AFOSR Solid State Sciences Program" (Office of Aerospace Research, 1961), 12.

85. J.R. Killian, "Military research in the universities," *Journal of engineering education, 43* (1952), 13–17, quoted by S.A. Glant, "How the Department of Defense shaped academic research and graduate education," in M.L. Perl, ed., *Physics careers, employment and education* (New York, 1978), 109–122. In a lecture given in 1963, F.E. Terman similarly emphasized that the sponsoring agencies are not "trying to run a do-gooder program" and sought to correct the "wide spread lack of understanding of the basic factors that determine how this government money is allocated;" Terman in C.W. Pursell, Jr., ed., *Readings in technology and American life* (New York, 1969), 431–438. Cf. I. Estermann, quoted in ref. 130.

86. The rise of nuclear physics to primacy in the United States is persuasively demonstrated statistically by A. Baracca, R. Livi, E. Piancastelli, and S. Ruffo, "La fisica del nucleo negli anni '30 e le premesse della 'big science' negli Stati Uniti," in G. Battimelli et al., eds., *La ristrutturazione delle scienze tra le due guerre mondiali* (2 vols., Rome, 1986), *2*, 367–421. The contributions of K. Hufbauer and B.R. Wheaton to the proceedings of this 1980 conference (*2*, 81–92, and *1*, 189–208) show, respectively, the rise of nuclear theory in the U.S. and the extent to which Germany was left behind in this field in the 1930s. Cf. J. Thomas, "John Stuart Foster, McGill University, and the renascence of nuclear physics in Montreal, 1933–1950," *HSPS, 14* (1984), 357–377. NAS-NRC (ref. 31), 625, found that in 1939 33% of all papers published in the *Physical review* were in "nuclear physics," 10% in "high energy physics;" in 1949 the proportions were 51% and 12%, respectively.

quantum electronics owed its very existence to wartime radar work.[87] The Radiation Laboratory at MIT with its nearly 500 physicists took a third of all the money the OSRD spent through universities. These physicists-turned-engineers, together with at least an equal number in several of the leading industrial research laboratories, created microwave radar[88] —and took it with them when returning to research after the war. The extremely rapid rise of research and publication in microwave spectroscopy would not have been possible otherwise (figure 8).[89]

This constellation of research fields and research techniques, which we may describe as pre-laser quantum electronics, included all the newer means for exciting, charting, and employing the electromagnetic resonances of quantized systems, notably microwave spectroscopy, electron spin resonance spectroscopy, and nuclear magnetic resonance spectroscopy applied to all states of matter, together with the novel devices such as masers, atomic magnetometers, and atomic clocks resulting from them.[90] If, moreover, one chose to be imperialistic—as

87. S.R. Weart, "Solid state physics as a community," in Weart et al., *History of solid state physics* (in press), using co-citation analyses of the physics literature carried out by Henry Small and colleagues at the Institute for Scientific Information, points out that a cleavage of physics into "particles" and "solid state," quite clear by 1970, had not yet begun in the 1920s. The cleavage here suggested is much less clean, for "quantum electronics" as the field of resonant electromagnetic interactions had application both to the study of the properties of the nucleus and those of the solid state. Thus A. Abragam's massive *Principles of nuclear magnetism* (Oxford, 1961) is, above all else, a treatise on quantum electronics.

88. Kevles (ref. 6), 303–308, 342. At Bell Labs some 2000 scientists and engineers were engaged in war work; M.D. Fagan, ed., *A history of engineering and science in the Bell System: National service in war and peace (1925–1975)* (Bell Telephone Laboratories, 1978), 11, 356. If half worked on radar, and something less than half of these were physicists (the Rad Lab ratio was 1:1), there were here some hundreds more—to which must be added those at RCA (Hershberger...), Sperry (W.W. Hansen...), Westinghouse (E.U. Condon...), etc.

89. "The war time development of microwave techniques has opened the way to an entirely new branch of spectroscopy;" D.K. Coles, "Microwave spectroscopy," *Advances in electronics, 2* (1950), 299–369, on 299. Similarly, "the tremendous advances in the fundamental investigation of paramagnetic materials...is in great part due to the existence of microwave equipment and techniques which were evolved during the war;" Benjamin Lax in National Security Industrial Association, Air Research and Development command, *Conference on molecular electronics* (Washington, D.C., 1959), 14. For literature on the history of radar, see G. Shiers, *Bibliography of the history of electronics* (New Jersey, 1972), 184–193.

90. The range of topics is roughly that covered by R.E. Norberg, "Resource letter...on nuclear magnetic resonance and electron paramagnetic resonance," *American journal of physics, 33* (1965), 71–75, together with D.J.E. Ingram, *Spectroscopy at radio and microwave frequencies* (London, 1955, 2nd edn, 1967). M. Bertolotti, *Masers and lasers: An historical approach* (Bristol, 1983), gives some indication of the breadth of the nacent field of quantum electronics; so also does the famous fold-out portraying the

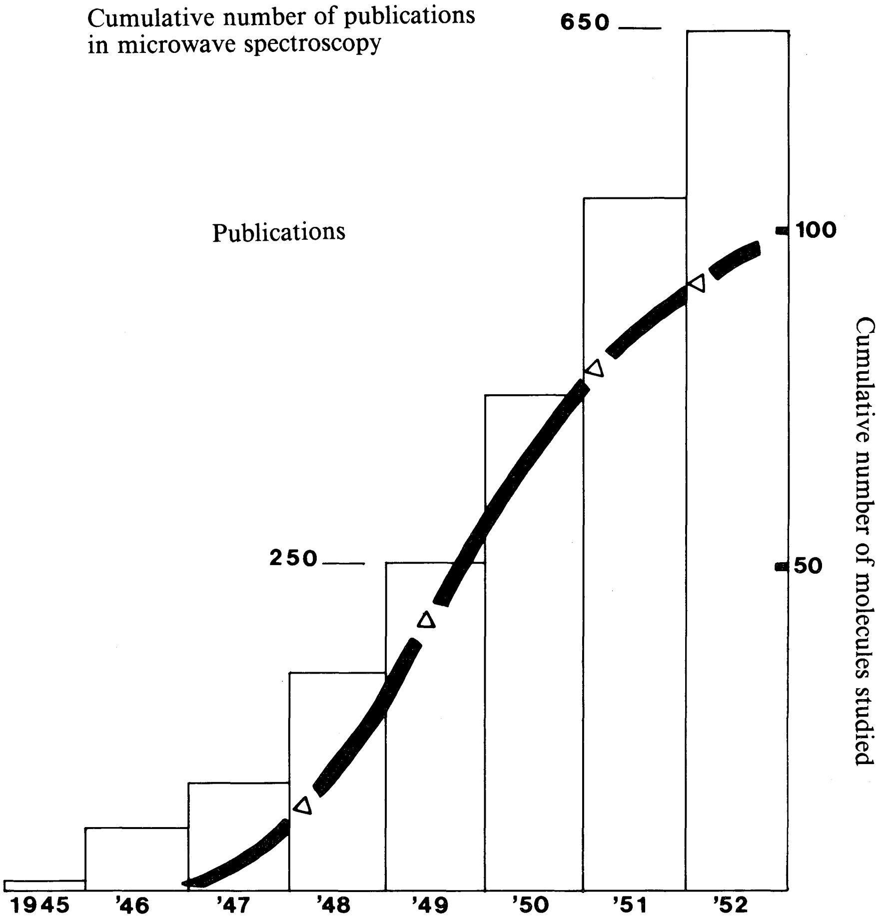

FIG. 8 CUMULATIVE NUMBER OF PUBLICATIONS IN, AND MOLECULES STUDIED BY, MICROWAVE SPECTROSCOPY, 1945–1952. The publication count is based on the chronological bibliography in C.H. Townes and A.L. Schawlow, *Microwave spectroscopy* (New York, 1955), 649–682, and the year-by-year totals reported by W. Gordy (ref. 135). The curve of molecules studied is adapted from Townes, "The present status of microwave spectroscopy," New York Academy of Sciences, *Annals, 55* (1952), 745–750. Although the data is to represent the whole world's work, roughly 85% of the publications listed by Townes and Schawlow appeared in U.S. journals.

sturdy and fruitful tree of research whose trunk is I.I. Rabi's work on molecular beam magnetic resonance; A.D. Little, Inc. (ref. 78), 46–47, and Rigden (ref. 50), 10–16. For the work of Dicke's group, see their contributions to *Polarisation, matière et rayonnement* (ref. 40), 431–465. For the atomic clocks deriving from this work: Roger E. Beehler, "A historical review of atomic frequency standards," IEEE, *Proceedings, 55* (1967), 792–805; Arthur O. McCoubrey, "A survey of atomic frequency standards," ibid., *54* (1966), 116–135.

sub-nuclear physics were off and running within a year after the war, solid state physics required the invention of the transistor to gain the élan and support, within as well as without physics, needed to pull out of its relative doldrums.

In an illuminating chapter on "Solid state physics as a community," Weart points out that between 1951 and 1960, while the total number of Ph.D. physicists in the U.S. doubled, the number in solid state physics increased by a factor of five. The strong orientation of these latter physicists toward applications is clear from their employing institutions: one quarter in higher education, one half in industry–the reverse of the proportions for physics as a whole. Ironically, the NSF, the one agency charged with the support of fundamental science for its own sake, in the first penurious years of its existence also weighted its support of physics towards solid state research as being, piecewise, relatively cheap. With like irony the NSF created an Atomic and Molecular Physics Program, within which quantum electronics fell, in order to help "relatively weak" colleges and universities "to develop some type of research activity." Thus the smaller universities and liberal arts colleges were channelled no less strongly than the major research centers into work closely related to applications.[95]

In the advance of solid-state and quantum electronics, an especially important role was played by the U.S. Army Signal Corps.[96] In addition to its laboratories at Fort Monmouth, the Signal Corps administered contracts sustaining the MIT Research Laboratory of Electronics where Jerrold Zacharias began the development of his atomic beam clocks, and the Columbia Radiation Laboratory, where Charles Townes carried through the development of the maser and the initiation of the laser. These, together with Harvard's Cruft Laboratory, were the original group with which the military services had jointly "negotiated contracts which converted their wartime efforts into long-range unclassified research of military interest." In the early

95. Weart (ref. 87); McMillen (ref. 76), 10–13; U.S. NAS-NRC, Committee on Atomic and Molecular Physics, *Atomic and molecular physics* (Washington, D.C., 1971), 27. A.E. Moyer, "History of physics," *Osiris, 2* (1985), 163–182, on 176–178, cites the growing body of work on the history of solid state physics and points out the role of this field in the increasing historical attention to the integration of American physics with military-industrial goals of American society after World War II.

96. For the Signal Corps and solid-state electronics, see T.J. Misa, "Military needs, commercial realities, and the development of the transistor, 1948–1958," in Smith (ref. 3), 253–287. At Philips research laboratories in the Netherlands, an internal report of Feb 1962 observed that "diodes seem to have been forgotten in the USA" in the late 1950s, for "at that time all large American firms were engaged in the military development of transistors." J. Schopman, "Philips' Antwort auf die neue Halbleiterära," *Technikgeschichte, 50* (1983), 146–161, and personal communication, 8 Apr 1986.

1950s this Joint Services Electronics Program would expand rapidly to include nine universities. The research programs of their laboratories were overseen by a Service technical Advisory Committee that kept them closely coordinated with the industrial (and classified) R&D programs the military services were simultaneously sponsoring.[97]

More strongly and broadly than any other agency, the Signal Corps pushed research on atomic and molecular resonance as a basis for atomic clock technology. On the initiative of program manager Fritz Reder, it organized in 1956 what might well be regarded as the first international conference on quantum electronics—before the letter—under the cumbersome but descriptive title, "Symposium on Generation, Frequency-Stabilization, and Amplification of Electromagnetic Oscillations by Atomic and Molecular Resonances."[98] This special symposium was a complement to the Annual Symposium on Frequency Control—likewise organized and run by the Frequency Control Branch of the Fort Monmouth laboratories—which served as the principal forum for presentation and discussion of work in progress in these areas of quantum electronics—in part because, as Robert Dicke recalls, "all of us who received support from the Signal Corps were expected to go as a matter of duty....Charlie Townes would go—all of us who had some support."[99]

This suggestion of an element of constraint in the scientist's relation to the military patron is confirmed by Harold Zahl, Director of Research at the Signal Corps' Fort Monmouth laboratories, in a

97. J.B. Wiesner, "A successful experiment" (the Joint Services Electronics Program), *Naval research reviews*, Jul 1966, inside front cover, 1–4, 11; ibid., Nov 1974, 1. A. Shostak, ed., *The Joint Services Electronics Program 40th anniversary* (ANSER, Arlington, VA, 1986; available from NTIS as ADA 171610); D. Robb and A. Shostak, eds., *Proceedings of the fortieth anniversary symposium of the Joint Services Electronics Program* (ANSER, Arlington, VA, for U.S. Army Research Office, 1987); L. deFlorez to K.T. Compton, 18 Feb 1946 (MIT, Archives, AC–4, 204/1); National Military Personnel Records Center, 63–321, 69/SC-32037, particularly J.E. Gorham, Chief, Thermionics Branch, Signal Corps Engineering Laboratory, to H.A. Zahl, 18 Nov 1946, and Services Technical Advisory Committee, Minutes, 20 Nov 1946. The Signal Corps maintained a liaison office on the MIT campus to oversee its Research Laboratory of Electronics and other Boston area contracts, while the Office of Naval Research had branch offices in Boston, New York, San Francisco, and elsewhere for these purposes. Powell (ref. 48).

98. H.H. Plotkin and F.H. Reder, "Atomic clocks and microwave amplification," *PT, 9:6* (1956), 44–46. A copy of the program of the symposium is in the files of R.H. Dicke, folder "Signal Corps Conference on Amplification."

99. H.H. Plotkin, "Frequency Control Symposium," *PT, 11:12* (1958), 34–36; G.K. Guttwein and H.H. Plotkin, "Frequency Control Symposium," *PT, 12:11* (1959), 36–42. *Proceedings* of these symposia have been published by the Signal Corps each year, beginning with the tenth in 1956. Interviews by the author with R.H. Dicke, 2 May 1983; with F.H. Reder, 1 Jul 1983; and with H.H. Plotkin, 11 Oct 1985.

forthright description of their program to his fellow physicists early in 1952:[100]

> The pattern of research contracts, which gradually evolved over six years, includes the association of many outstanding investigators with the Signal Corps in projects of broad military interest. Problems assigned in themselves are challenging from most scientists' point of view, but as assurance against restrictiveness which might curb imagination, the investigator is encouraged to follow leads of his own choice.... These contracts are all closely followed by competent Signal Corps personnel and therefore represent an effective increase in the research supporting the applied effort of the Signal Corps. Most of the 132 contracts now in effect in the physical sciences are unclassified, and free interchange of information among other workers in related fields follows without complications. [However,]...once it is seen how information or discovery in a field may significantly influence a military requirement, the researcher finds he is more apt to be, temporarily at least, in the audience of the technical society rather than among those presenting papers.

The view here expressed of the contracted research as "supporting the applied effort of the Signal Corps," the reference to "problems assigned," and the asserted right to classify "information or discovery," with the implication of a right to prior review—these all suggest a proprietary attitude toward the sponsored research.

Military advantage being the purpose behind the support even of basic research, pressures for secrecy, classification, and restriction of access to research results were inevitable. Strictly speaking there was in this period no such thing as unclassified research under military sponsorship. "Unclassified" was simply that research in which some considerable part of the responsibility for deciding whether the results should be held secret fell upon the researcher himself and his laboratory. This was stated quite early and clearly by ONR's spokesmen, A.T. Waterman and Capt R.D. Conrad:

> Since true scientific research is incompatible with any restrictions on the free flow of information..., the arrangements we have found workable are as follows: The contractor is entirely free to publish the results of his work, but...we expect that scientists who are engaged on projects under Naval sponsorship are as alert and as conscientious as we are to recognize the implications of their achievement, and that they are fully competent to guard the national interest.

The totalitarian implications of this "entirely free" research policy were not always overlooked in academia. Still, the assumptions so

100. Harold A. Zahl, "Physics in the Signal Corps," *PT, 5:2* (1952), 16–19.

generally shared in these early years of the cold war rendered them beyond challenge. The drafters of Princeton University's "Report and faculty resolutions with regard to security and classification in Government sponsored research," May 14, 1953, put it this way:

> In a basic sense, there is no contract which can be completely described as a classified contract or as an unclassified contract; it is only information that can be described as classified or unclassified....The University can never be certain that any contract which is undertaken may not, at some time or other, involve classified information and therefore security regulations. One cannot deny the Government the right to classify information whose revelation might do injury to the United States, even if that information resulted from the purest basic research.

It need hardly be said that such a view of the matter could readily be turned, on the one hand, to rationalizing the presence of classified research on campus and, on the other hand, to excluding from the campus any researchers judged unfit for participation in classified work.[101]

In the immediate aftermath of the war even academics who gladly accepted military money generally insisted that the research be "done entirely without restrictions of secrecy"—anyway without restrictions going very far beyond those implied in the foregoing commitment to conscientious attention to national security.[102] In the early 1950s,

101. Waterman and Conrad (ref. 48), 354–356; "Report," 14 May 1953, in Princeton University Archives, cited and partially quoted in American Council on Education 1968 (ref. 51), 198. Cf. ref. 40 and ref. 43. In 1949 Waterman stated in *Science* (ref. 48), 703, ONR's unrestricted right to classify "any result" it saw fit. The AEC went even further, maintaining implicitly, and eventually explicitly, that any information that *should* be restricted was *ipso facto* restricted, regardless of the auspices under which it had originated; R.G. Hewlett, "'Born classified' in the AEC: A historian's view," *BAS, 37:10* (Dec 1981), 20–27. Thus Bell Labs, though they had used their own funds to develop the transistor, withheld public announcement until they "had received word that the discovery would not be classified." Hoddeson (ref. 93), 75.

102. L.N. Ridenour in American Scholar Forum (ref. 48), 217. J.A. Stratton, Director of MIT's Research Laboratory of Electronics (and Chairman of the Research and Development Board's Committee on Electronics), insisted that RLE operated entirely without restrictions on staff and publication (Stratton to Dean Bunker, 10 Feb 1947, MIT, Archives, AC-4, 204/5). In fact, however, the Laboratory was obliged to transmit to the three military services all manuscripts simultaneously with their submission for publication "for two reasons: first, security; and second, as sponsors of the Laboratory they would like to know in advance of any publication by the group" ("Research Laboratory of Electronics—Service Technical Committee Meeting," 20 Nov 1946, Minutes, in U.S. Army Signal Corps records, National Military Personnel Records Center, St. Louis, Accession 63-321, Box 69). In October 1950, after three months of war in Korea, Stratton, as Chairman of the "Research" section of the American Council for Higher Education's Conference on Higher Education in the National Service, submitted the "conclusion...that if universities are to do their part they will to some extent be compelled to accept work of a classified nature" (F.L. Foster, (ref. 11), *1*, FY '51, pp.

however, the situation changed drastically, with massive growth of classified research projects on university campuses and unremitting pressure from the sponsoring agencies for stricter controls over personnel, performance, and research results. Vintage 1952 Killian was quite disturbed "that the greater portion of the total governmental expenditures for research [at universities] is going into classified military projects (including AEC work)."[103]

Early in that year MIT, along with other academic institutions, had been required to incorporate the Department of Defense Industrial Security Manual as part of their military research contracts and to obligate themselves to accept "any amendments to said Manual required by the demands of national security as determined by the Government and made after the date of this contract."[104] The MIT office responsible for administration of these contracts noted in its report for 1953/54 both the increasing volume of classified research and the fact that "the Institute Administration has continued to show increased interest in the general subject of security," with "frequent security memoranda..., a revised security manual, and a new policy for handling personnel clearance cases." After another two years of this regime the director of the office judged so many of MIT's military research contracts involved "greatly detailed specifications for the research, thereby leaving little freedom of action for the researcher," and so constant a tendency for the sponsoring agencies "to increase pressure on publication control," that "a drop or certainly a leveling off" of contract research was to be expected. Apparently, nothing of

41–42.). The Signal Corps' chief scientist recalled Stratton's jumping at the opportunity (Zahl (ref. 50), 117).

103. Killian as Chairman of the American Council on Education's Committee on Institutional Research Policy, 20 Aug 1952 (MIT, Archives, AC-4, 170/7). In June 1952 an Air Force spokesman implied that slightly more than half of the research sponsored by that service at universities and university-affiliated laboratories was classified, but that three-fourths of that classified research was performed in six institutions where classified research was segregated in affiliated laboratories; O.G. Haywood, "The Air Research and Development Program," *Journal of English education, 43* (1953), 373–377. Thus we infer that not less than 13% of the Air Force's research carried on in ordinary campus laboratories was classified. Kidd (ref. 40), 117–122, in a section devoid of references, sets this figure for *all* Federal funds at "about 15 percent," in 1953–54 apparently. On pressure for classification circa 1952: Koppes (ref. 8), 50; H.E. Newell, *Beyond the atmosphere: Early years of space science* (Washington, D.C., 1980), 42, 118–120; D.H. DeVorkin, "Organizing for space research: The V-2 rocket panel," *HSPS, 18:1* (1987), 1–24.

104. Quotation from ONR contract NoD-6964 (Washington National Records Center, Accession 63A2374, Box 9); discussion in G.B. Pegram, Chairman, Columbia University Committee on Government Aided Research, to Air Research and Development Command, 14 Feb 1952 (Columbia University Archives, Physics Department Papers, 24, "Contracts CRL 1950–42").

the sort occurred.[105]

To such a beleaguered administrative officer, the only "obvious alleviating factor in this area is the maintenance of close liaison between the sponsor's technical personnel and the researcher." And indeed, the "program officers" or "contract monitors" at the military funding agencies played an extremely important role in mediating between the requirements of their scientific constituency, often also their *primary* reference group, and the demands of the military patron. Gillmor, interviewing some twenty older "strategic observers" from U.S. academic ionospheric research, found general agreement on the importance of these program officers and even one or two that were referred to by their clientèle "in almost legendary terms." Singled out by Gillmor as one who "did far more than provide funds" is Arnold Shostak, the ONR program officer responsible also for quantum electronics.[106]

Whatever such program officers did beyond providing funds—as indeed much of what they did in order to provide funds—must be reckoned as the direction of research. The funding levels of their programs and the contentment of their stable of researchers depended upon reconciliation of the interests of their military and their scientific constituencies, a reconciliation effected chiefly by envisaging and promoting military applications in and through basic scientific research. For the researcher himself, "the mere need to defend what he is doing to a particular sponsor may be the factor which will trigger an important application." Nor had the mission agency, Harvard's Dean of Engineering and Applied Physics continued, to acquiesce in the criteria of importance the researchers derived from the logic or the fashion within their particular scientific disciplines:[107]

> An important function of an agency is to stimulate interest within the broad scientific community on problems important to [the agency's] future..., to translate applied problems of an agency into generic

105. Foster (ref. 11), *3:1*, FY '54, p. 17, and *3:12*, FY '56, 56–66, quoting L.E. Beckley.

106. L.E. Beckley, quoted by Foster (ref. 11); Gillmor (ref. 74), 123–127; see re Shostak, ref. 97 and ref. 116. Frederick Seitz, "The university: Independent institution or Federal satellite?" in B.R. Kennan, ed., *Science and the university* (New York, 1966), 149–161, on 157, judged that "the success of the project system between 1945 and 1960 depended to no small extent on the presence in the federal agencies of a very remarkable group of scientific administrators."

107. The address by Harvard's Harvey Brooks at ONR's twentieth anniversary, 1966, is here quoted from his collected essays, *The government of science* (Cambridge, 1968), 113, 119. D.J. Kevles (ref. 3) quotes Brooks' well-informed opinion that "military requirements tend to become after-the-fact rationalizations of technical ideas cooked up at a relatively low level in the military-technical contractor bureaucracy."

> scientific problems which can attract the continuing interest and attention of first-rate scientists....ONR...has shown a particular talent for this kind of imaginative stimulation.

One of the most common and effective of those stimulative mechanisms was sponsoring "cross-disciplinary" meetings. In its first five years of operation the Air Force Office of Scientific Research employed "this method of administering research...widely...and to a high degree of success." While admitting "that scientists, at first, were usually reluctant to attend our...meetings," the AFOSR would "not consider that the Air Force has, or will, exert any undue military influence on science by promoting interactions of this type."[108] Altogether they, the program officers of the military funding agencies, were powerful agents directing the advance of knowledge and forwarding the convergence of science and engineering toward military applications and hardware. in moments of need, funding agencies and officers did not refrain from direct intervention.

A striking instance of such a presumption of cognizance appears in a letter written in the autumn of 1954 from the Signal Corps' Fort Monmouth Frequency Control Branch to Princeton Professor R.H. Dicke explaining that "there exists now an urgent need for a very stable oscillator, in package form, having stability of 1 part in 10^9 or better. This Branch has therefore conducted a thorough survey of its external research program whereby emphasis will be given to sponsoring of work which promises technically sound end items rather than purely theoretical results." Program Manager Doxey then went on to tell contractor Dicke which part of his research on atomic frequency standards he was to emphasize and how he was to go about it. Contractor Dicke was agitated—and activated. Appealing over the heads of the Branch staff in letters and personal confrontations, he successfully maintained his own direction.[109]

108. Colonel W.O. Davis, USAF, addressing the National Research Council on "Relief of the fragmentation of the sciences," 7 May 1957 (NA RG167, Astin Papers, folder: NRC). McKenzie (ref. 48), 231–232, stated in 1953 that the ONR program manager "has a power to call conferences of the contractor's representatives and thus create small, intensive, *ad hoc* scientific societies." This "serves to inform scientists of naval problems and weds the program...to the broader program of the ONR, the Navy, and Defense Research. The conferences called by the [Physics] Branch have the enthusiastic support of the contractors."

109. W.L. Doxey to R.H. Dicke, 6 Oct 1954, and R.H. Dicke to Marcel Golay, 11 Oct 1954, in Dicke's files, folder "Signal Corps A-812, 1954–55;" James Wittke, interview by author, 10 Feb 1986. W.G. Bowen, *The Federal Government and Princeton University: A report on the effects of Princeton's involvements with the Federal Government on the operations of the University* (privately printed for Princeton University, 1962), 136–139, emphasized that Princeton's physicists "have been very proud of their tradition of 'independence' and have been determined to continue making their own de-

If only to help us keep the reorientation of academic physics in perspective, it is well to consider briefly the military and quantum-electronic involvements of the nation's premier governmental research establishment. The Bureau of Standards too had been mobilized during World War II. It had taken on a large part of the development of proximity fuses and housed an Interservice Radio Propagation Laboratory to elucidate the regularities and irregularities of radio and microwave transmission through the atmosphere. The Bureau never demobilized; what previously had been described as "a wartime basis" was now called "its normal peacetime functions." The Bureau's activities in electronics were consolidated in the Ordnance Development Division, by far the largest in the Bureau, and supported wholly by funds transferred from the military services. The Bureau's activities in radio and microwaves were centralized in an only nominally demilitarized Central Radio Propagation Laboratory (CRPL), the second largest division of the Bureau. Here in 1948, in the Microwave Standards Section, the first operating atomic clock was constructed, and an ambitious program of research and development in this field was launched.[110]

In contrast to the Ordnance Division, the bulk of CRPL's funds were civil, amounting to more than a third of the Bureau's total Congressional appropriation. The presence of this chunk of money in the Bureau's budget was essentially owing to the clout of CRPL's "Executive Council" made up of representatives of interested Federal agencies, chiefly the armed services. This Executive Council set and ensured both program and budget of the Laboratory, for, as its Secretary observed, "lack of specificity" in its constituting documents "permits the Executive Council to operate at the level of the Division

cisions as to what directions offer the greatest promise for fruitful research." He reported, further, that "some agencies have attempted to use a rather 'heavy hand' in dealing with faculty members engaged in sponsored research. To illustrate, one faculty member stated that an Army sponsor had tended to view his research group as an offshoot of the Army and has complained from time to time that Princeton has not been doing the job the Army needs done. Recently, this particular sponsor rejected parts of a proposal on the ground that there was not enough 'hardware' emphasis." A search of the working papers for this report deposited by Bowen in the Princeton University Archives failed to produce further details. Thus the relation of this episode to that recorded in Dicke's files remains uncertain.

110. R.C. Cochrane, *Measures for progress: A history of the National Bureau of Standards* (Washington, D.C., 1966), 404–406. NBS, CRPL, "Radio research and CRPL," 15 Aug 1950 (National Archives, Record Group 167, Astin Papers, Box 17); NBS, CRPL, "Quarterly report" (Department of Commerce, Boulder Laboratories, Library); NBS, "Budgetary Plan," FY '49 and FY '50 (National Archives, RG 167, Entry 66: "Consolidated Report on Projects, FY '49–63," Box 1). The quoted phrases are from the Steelman Report (ref. 10), *2*, 177; cf. ref. 60.

Chief, the Director of the Bureau, and the Secretary of Commerce. This is a distinct advantage which more than makes up for any difficulties involved through lack of a charter." That unwritten constitution also acknowledged the primacy of the Signal Corps, "inasmuch as the major part of the work of the laboratory...seems to be more closely associated with the Army than any other of the three departments," and consequently the Council's Chairman was regularly chosen from among the officers representing that arm. From this officer Bureau Director Condon received a sharp rebuke for seeking to skim into his contingency fund a mere 1% off CRPL's nearly 40% of the Bureau's FY '52 appropriation.[111]

In the late 1940s nearly all the Bureau's divisions received some "Military Research" funds. Still, through 1950 a certain balance was maintained: Military funds (and I include as such CRPL's civil appropriation) amounted to a mere three-fourths of the total research and testing budget. Then, with the Korean war, the Bureau was inundated with military funds for applied research and development. In the summer of 1953 a blue-ribbon panel, appointed to calm the furor over the new Republican administration's political interference with the Bureau's testing work, found that since 1950 basic research had lost ground "at a tragic rate."[112]

The Bureau, in brief, provides a striking example of Ionson's "golden rule." Its sponsoring agencies not only set the directions of research, but also owned the product. As Condon found it necessary to explain to his superiors, "a large portion" of the Bureau's work on Alaskan radio propagation "has been undertaken at the behest of the National Military Establishment and is subject to military security regulations." Consequently only a small part of that program could be discussed in public.[113]

Patronage and the character of knowledge

Seeing at every turn the orientation of the military toward applications, let us return to the university world and consider more closely the character of the research and the knowledge that resulted under

111. S.W.J. Welch in Radio Propagation Executive Council, Minutes, 9 Dec 1947, and A.G. McNish and Col. W.M. Lautenbach, ibid., 3 Aug 1951 (Department of Commerce, Boulder Laboratories, Library).

112. NBS, "Budgetary Plan," FY '49 and FY '50 (ref. 110); Cochrane (ref. 110), 481–503; *Computer development (SEAC and DYSEAC) at the National Bureau of Standards, Washington, D.C.* (Washington, D.C., 1955).

113. Condon to Asst. Secretary of Commerce Davis, 27 Oct 1950 (National Archives, RG 167, "Director's Routine Correspondence, 1945–1965," 7/IG).

military patronage of academic physicists in the fifteen years following the second world war. Since those patrons believed, with their most highly regarded aerodynamicist, Theodore von Kármán, that "there is a necessity for coordination of what I would call the 'normal scientific life' in the research laboratories of the universities, and one which meets the needs of national defense—to find means which allow a combination of both activities"—and since military patronage of academic scientists was intended to effect just such a "coordination," what should we expect that "combination of both activities" to be? What, further, will be the character of the knowledge emerging from that hybrid?[114]

The resistance shown by historians of science to consideration of such questions is but testimony to our community of ideological commitments with the pursuers of science. Philip Handler, President of the U.S. National Academy of Sciences, replying with some heat to a posse of critical Europeans, insisted in 1968 that "a sample of five hundred people in the life sciences answered 'no' without exception when asked if their work had been affected by the source of funds." But Donald Hornig, then the President's Science Advisor, finding the display of such ideological blinkers unsuited to *his* position, chose to "reply a little differently from Professor Handler, and say that we *do* influence the structure of American science by mission orientation and in fact desire to do it." Nor is it structure only that is shaped by the sources from which, and the ends toward which, research is funded and directed. Again I invoke the authority of Harvey Brooks, forefront figure in all manner of scientific advisory committees in the 1950s and 1960s:[115]

114. Von Kármán's statement, early in 1948, is quoted by Thomas A. Sturm, *The USAF Scientific Advisory Board: Its first twenty years, 1944–1964* (Washington, D.C., 1967), 24; S.S. Schweber, "Some reflections on the history of particle physics in the 1950s," in Brown et al. (ref. 49). Similar calls by Edward L. Bowles, MIT electrical engineer and science advisor to Secretary of War Henry Stimson, for "an effective peacetime integration" of academy and military are quoted by Sherry (ref. 8), 133; Koppes (ref. 8), 25, 258; and A. Michal McMahon, *The making of a profession: A century of electrical engineering in America* (New York, 1984), 207–208. Also ref. 44 and Bowles, "National security and a mechanism for its achievement," IRE, *Proceedings, 34* (1946), 154–155.

115. Handler and Hornig quoted from Organization for Economic Cooperation and Development, *Review of national science policy: United States* (Paris, 1968), 464; Brooks (ref. 107), 112–113. C.V. Kidd (ref. 40), 207–210, clinging to the faith that "the urge to grow is inside the tree [of knowledge], not outside," had yet to admit that Government agencies "have virtually unlimited discretion to determine the terms and conditions under which research funds will be made available."

> Scientific work involves a multiplicity of choices of direction, many of which depend on very small influences in the mind of the investigator. Even in a system of complete scientific freedom the cumulative effect of the small biases placed in the mind of the investigator by his sponsor can have a profound effect on the direction and impact of his research.

Brooks had militarily useful applications chiefly in mind in speaking of "direction and impact," for the intent of his (quite truthful) thesis was to congratulate and encourage the Office of Naval Research in its generous support of scientists who thought of themselves as basic researchers. Among other examples, Brooks had bits of the following history in mind.

In September 1959 Irving Rowe, speaking for the Office of Naval Research, welcomed some hundred physicists and rather fewer electrical engineers to a resort hotel in the mountains northwest of New York City. These select researchers and a sprinkling of research administrators were participants in a symposium on "Quantum Electronics—Resonance Phenomena," soon famed as the First International Conference on Quantum Electronics. "The idea for calling this meeting," Rowe said, "originated with the members of the electronics and physics branches of the Office of Naval Research, who realized the growing significance of the field of quantum electronics, which is actually producing a revolution in microwave techniques." "The Navy," Rowe continued, "is fully aware of the importance of basic research in quantum electronics, and indeed has already begun to utilize its applications." Of such applications Rowe gave two examples. These served him both to define implicitly the content of quantum electronics, and to demonstrate the Navy's alertness to the technical potential of scientific advance.[116]

The first of the devices to which Rowe pointed was the solid state maser that Charles Townes and his students had recently used as an ultrasensitive receiver in conjunction with the Naval Research Laboratory's radio telescope—their work supported by the Army Signal Corps.[117] Second was the atomic clock then in use as a time

116. C.H. Townes, ed., *Quantum electronics: A symposium* (New York, 1960), v–vi, crediting A. Shostak and F. Isakson of ONR for giving the initial impetus; I. Rowe, "An international conference on quantum electronics," *PT, 13:3* (1960), 28–38. Rowe was a research administrator in ONR's New York City office. In 1957 Rowe gained a physics Ph.D. from Brooklyn Polytechnic in x-ray diffraction studies of metals.

117. J.A. Giordmaine, L.E. Alsop, C.H. Mayer, C.H. Townes, "A maser amplifier for radio astronomy at x-band," IRE, *Proceedings, 57* (1959), 1062–1069; J.V. Jelley, "The potentialities and present status of masers and parametric amplifiers in radio astronomy," IEEE, *Proceedings, 51* (1963), 30–45. Townes' experiments at NRL, the first use of a maser amplifier in radio astronomy, began in 1958 and continued into the early 1960s. For the 50-foot radio telescope itself, see J.S. Hey, *The evolution of radio astronomy* (New York, 1973), 83–87; J.E. Sees, "NRL's 50-foot radio telescope," *Naval research reviews*, Mar 1958, 1–7.

standard at the Naval Observatory in Washington. This latter device might conceivably also have been a maser, for the ammonia beam maser had been widely and intensively developed as a potentially practical frequency standard in the five years since Townes and his associates had demonstrated the extremely high coherence and stability of their molecular oscillator late in 1954.[118]

Townes has often recounted that he conceived the ammonia beam maser in the spring of 1951 largely as a result of his service on an advisory committee to the Office of Naval Research concerned with means for achieving shorter wavelength radar. At that time "I already had been active in the use of molecular resonances for frequency control, because the Army Signal Corps was interested in those techniques." His development of the maser from 1951 onward, and of the laser from 1957 onward—with a little gentle prodding by an Air Force program officer—was carried out under the auspices of the Columbia Radiation Laboratory, funded under the Joint Services Electronics Program, again primarily for such fundamental—i.e., unclassified—research as would make possible higher frequency radar.[119]

In fact the Naval Observatory's clock, although an atomic beam device also, was based on a different principle, namely the use of a magnetic resonance to lock the frequency of a quartz oscillator to a hyperfine transition of cesium atoms. Work along these quantum electronic lines had been pursued at the U.S. Bureau of Standards from the late 1940s—with Townes and his Columbia Radiation Laboratory colleague Polykarp Kusch serving as consultants. But the bold idea of transforming such elaborate and temperamental apparatus into a relatively simple, reliable—and thus salable—instrument was conceived and pushed by Jerrold Zacharias from his base in MIT's Research Laboratory of Electronics, likewise funded under the Joint Services Electronics Program. Zacharias too had given, and was then giving, extensive service as advisor to the military. Indeed, ONR would rate

118. J.P. Gordon, H.J. Zeiger, C.H. Townes, "The maser—new type of microwave amplifier, frequency standard, and spectrometer," *Physical review, 99* (1955), 1264–1274; R.C. Mockler, et al., "The ammonia maser as an atomic frequency and time standard," IRE, *Transactions on instrumentation, 7* (1958), 201–202, reporting on the work at NBS; J. DePrins, *Applications des masers à* $N^{15}H_3$ à la mesure et la définition du temps (Neuchâtel, 1961), reporting on work at the Laboratoire Suisse de Recherches Horologères.

119. C.H. Townes, "Masers," in C.F.J. Overhage, ed., *The age of electronics* (New York, 1962), 164–195; C.H. Townes, "Ideas and stumbling blocks in quantum electronics," IEEE, *Journal of quantum electronics, QE-20* (1984), 547–550; C.H. Townes, "Quantum electronics at Columbia University," in Robb and Shostak, eds. (ref. 97), 71–88, on 78, 81; J.L. Bromberg, "Research efforts that led to laser development," *Laser focus*, Oct 1984, 58–60; Zahl (ref. 50), 97; see also ref. 123 and ref. 124.

Project Hartwell, organized and led by Zacharias in the summer of 1950, "exemplary for the studies of the next ten years."[120]

The task of turning Zacharias' laboratory prototype into a marketable device was taken on in the spring of 1954 by a suburban Boston electronics firm. The National Company under new, ambitious management committed itself to Zacharias' atomic clock at the urging of its board member and principal technical advisor, Jerome Wiesner, close colleague and friend of Zacharias and director of the Research Laboratory of Electronics. Thirty months later, with much help from Zacharias, his students, and MIT staff, National delivered its first "Atomichron®" to the first of three military services putting money into their development program.[121]

If these quantum electronic cases may be taken as exemplary—and the Office of Naval Research clearly considered them to be so—then we may say that support by military agencies and consultation on military problems had effectively rotated the orientation of academic physics toward techniques and applications.[122] And still this was called "basic" research, much as the non-demobilization of the Bureau of Standards was described as a return to "its normal peacetime functions." In truth, only a small fraction of that 5% of R&D funds labeled basic research went to support investigation that could reasonably be called fundamental. In this regard American universities had very guilty consciences in the 1950s. In response to a questionnaire from the American Council on Education directed to the thirty-two institutions known to be the predominant carriers of Government-sponsored research, only twenty admitted that any portion of their

120. P. Forman, "The first atomic clock program: NBS, 1947–1954," Annual Precise Time and Time Interval (PTTI) Applications and Planning Meeting, *Proceedings, 17* (1985), 1–17; J.R. Marvin and F.J. Weyl, "The summer study," *Naval research reviews*, Aug 1966, cover–7, 24–28. Zacharias' centrality is underscored by Schweber (ref. 3, 114).

121. P. Forman (ref. 51). The ONR did not hesitate to take credit; I. Rowe, "The ONR laser program," *Naval research reviews*, Dec 1966, 1–13, on 3.

122. An index of this orientation toward technique: Asked in 1951 for their "field of highest competence," 6600 U.S. physicists, 45% holding the Ph.D., gave the plurality (18%) to electronics. T.R. Shapiro and H. Wood, "The new physicists," *PT, 6:2* (1953), 14–16. Again it was A.H. Dupree, "Influence of the past: An interpretation of recent development in the context of 200 years of history," American Academy of Political and Social Science, *Annals, 327: Perspectives on government and science* (1960), 19–26, who pointed out decades ago that "the harnessing of whole national scientific establishments to military applications [in World War II] pulled all scientists, whether they tagged themselves as basic or applied, into the same network....Nothing in the postwar world...has lessened this interwoven pattern." Carol S. Gruber, who is preparing a book on the university-government bond created in and out of World War II, kindly drew this essay to my attention.

sponsored research was *applied,* and only four that that portion was as large as one half; yet these institutions declared with near unaminity the undesirability of universities, accepting "additional responsibility" in applied research.[123]

In the case of the Columbia Radiation Laboratory, the 20% of the budget available for Townes' work on microwave spectroscopy, and for Rabi, Lamb, and Kusch's experiments probing quantum electrodynamics—all carried under the heading "Microwave Physics"—were available only because of the 80% devoted to nominally basic, but factually applied research pointed toward higher frequency radars.[124] And even that 20% appears a bit dubious when we recall, as Townes himself allowed, that "both electronics and spectroscopy are, in fact, more techniques and tools than fields of knowledge in themselves." Although the Stanford Electronics Research Project could describe itself as "devoted to fundamental studies," those studies being initially "of broad band external cavity reflex klystron oscillators...which will enable tube and set designers to realize the maximum potentialities of this type of tube and circuit," it is difficult to view this work as an instance of science using society's resources to its own ends.[125]

123. American Council on Education 1954 (ref. 51), 48–49, 80. U.S. universities estimated that they received from the Federal Government in FY '54 funds for *basic* research amounting to twice what Federal officials estimated they had given; Kidd (ref. 40), 64. Cf. Dodds and Sayre (ref. 64). Recently U.S. National Security Decision Directive 189 (ref. 82) has given "fundamental research" a precise definition more in tune with the spirit of the eighties: "'Fundamental research' means basic and applied research and engineering, the results of which ordinarily are published and shared broadly within the scientific community, as distinguished from proprietary research and from industrial development, design, production, and product utilization, the results of which ordinarily are restricted for proprietary or national security reasons."

124. Zahl (ref. 50), 97; Columbia Radiation Laboratory, *Progress report, 1* (1946), a quarterly. *All* reports from this laboratory carried the heading "Research extending the useful range of the electromagnetic spectrum." Similarly the "task statement" for MIT's Research Laboratory of Electronics called for "a program of research with a view to extending the useful range of the electromagnetic spectrum...to...shorter wavelengths," and specified "research on basic components and techniques necessary for the practical utilization of this region of the spectrum, including circuits and instrumentation." Shostak, ed. (ref. 97), 4–5.

125. Townes, "Introduction," *Quantum Electronics* (1960), x. Here we might add E.L. Ginzton's testimony regarding microwaves: "Research in the microwave field must, of necessity, be applied research." Quoted from *Science, 127* (1958), 841, by S.W. Leslie, paper prepared for Johns Hopkins University Workshop on the Military and Postwar Academic Science, Apr 1986. Stanford Electronics Research Project, [1st] Quarterly Report, Washington National Records Center, Accession 14255, Box 4, 1 Aug 1946 (N6ORI-106 Task 3). Two years later, Karl Spangenberg, author of this report, entered upon a year on ONR's staff, where he wrote "Basic research projects under ONR contracts," *Electronics, 22* (June 1949), 66–69. Acknowledging that "in the field of purely basic research....immediate application...is probably more the exception than

Inseparable from the orientation of research is its organization. In this sense too, as was well recognized at the time, postwar physical research was decisively shaped by the physicists' wartime experiences. In 1953 J.A. Stratton, now advanced from director of MIT's Research Laboratory of Electronics to provost of the Institute, described "this transformation:"[126]

> There was a time when scientific investigation was largely a matter of individual enterprise but the war taught scientists to work together in groups; they learned to think in terms of a common project, they were impressed by the progress to be made through unified action. A notable degree of this spirit has been transfused into the life of our larger universities....The scale of research and the complexity of its techniques have grown beyond anything imagined a few decades ago....It was the war that contributed principally to a major revolution in the method and spirit and scale of laboratory investigations.

To a physicist like Merle Tuve, who carried substantial responsibility for the creation of the patterns and institutions by which this technologizing of academic research was effected, but who happened also to be a cultural traditionalist, the new situation of "basic" research was both clear and deplorable. "Justly renowned for a habit of saying whatever is on his mind," Tuve offered "a little mustard" to a spring 1959 "Party Congress" called to celebrate that 5% of so-called basic research, and to agitate for the increase thereof by another percentage point or two:[127]

the rule," Spangenberg devoted his article to such, evidently amply numerous, exceptions.

126. J.A. Stratton, "Research and the university," *Chemical and engineering news, 31* (1953), 2581–2583, quoted by Kidd (ref. 40), 172–173. A measure of the degree to which the academy had, become transfused by this new spirit by the end of the decade: "In a survey of 10 universities with new physics research facilities, only one indicated that more than 10% of their research is carried on in single-person laboratories." R.R. Palmer and W.M. Rice, *Modern physics buildings* (New York, 1961), 224, as quoted by P.J. Lorenz, *The research function of American colleges* (Fayette, Iowa, 1961), 30. Tuve (ref. 5), 291, replying directly though not explicitly to Stratton, urged that "the most important objection to the present large-scale contract support of research in educational institutions by the AEC and the Armed Services is the subtle change in the motivation and the aims of research which it entails."

127. Tuve (ref. 2), 169, 174, 182. W.D. Carey attended this historic "Symposium on Basic Research" as Bureau of the Budget observer, and devoted nearly the whole of his report (National Archives, RG 51, 52.1/E4–1) to Tuve's address:

> As it turned out, the "symposium" was not a symposium at all, but rather resembled a rally at which practically all of the noise was furnished by speeches from the rostrum. The chief purpose of the assembly appeared to be that of extolling the values of basic research, in various chords and melodies. Each speaker manfully attempted to give a definition of the animal being celebrated, but at the windup there was no clear agreement as to what basic research is. Nevertheless, all agreed that whatever it is it merits more financial support, even though the absence of a definition makes

> Regardless of the doubling and redoubling year by year of the announced annual expenditures by government and industry for basic research in science, we all feel a bit helpless and disappointed because these large sums seem to contribute so little to the really basic core of scholarly accomplishment....I feel that we have directed most of our efforts toward the creation and support of large-scale activities essentially technological in character and conspicuous in possible effects, ...that only an extremely small fraction of the various budgets we help to defend are for truly basic research in the sense of intensive studies devoted to the perception and formulation of new knowledge toward deeper understanding.

However sympathetic we may be to Tuve and to his antique distinction between "effects" and "understanding," we must recognize his mistake—the common mistake, in which I too have acquiesced thus far—namely reifying "basic" and "applied," treating them as time-invariant types of research activity, of which only the mix is a function of the place, period, or institutional setting. Instead, as historians we should try to see physics as a socially integrated institution, and should seek the "integrity" of the knowledge resulting from research activity largely determined by the forms, bases, and extent of the integration. But how? Grappling as we are with the connection between knowing and doing, we might recall that Marx originated our

> it impossible to say how much we are already spending on it.
>
> In such an environment of simple faith and aspiration, the proceedings rolled smoothly along until a large monkey wrench sailed through the skylight in the shape and person of Dr. Merle Tuve, head of the Carnegie Institution of Washington. Dr. Tuve is justly renowned for a habit of saying whatever is on his mind. He had a lot on his mind last Friday morning. Here, despite their length, are a few excerpts, aptly described by the author as "a little mustard"....
>
> When Tuve stepped down, the hall rocked with applause. Indeed, such was the intensity of the enthusiasm that it prompted the high moguls to take to the microphone and sharply criticize Tuve for getting out of line. It was also somewhat remarkable that the invited press failed to accord the Tuve statement any press coverage, although the bland speeches of the leadership were duly reported. In short, the "symposium" seems to have been very capably disciplined, in a way reminiscent of a Party Congress.

Carey has published a few, slightly revised extracts from this memo in *Science, technology, and human values, 10* (1985), 7–16. His account is confirmed by John Lear, science editor of the *Saturday review*, 6 June 1959, 48–52, who chose as simile "like a thunderclap on a sunny day," and published Tuve's remarks *in extenso.* Lear also reported on the symposium and Tuve's contribution in his weekly "American newsletter" in *The new scientist, 5* (21 May 1959), 1126, and (28 May 1959), 1188, where he states that he has "been importuned since from three different directions to lay the story aside." To these latter reports my attention was drawn by A. Baracca, "'Big science' vs. 'little science': Laboratories and leading ideas in conflict; nuclear physics in the thirties and forties," paper delivered at the symposium "Laboratories: The place of experiment," the Royal Institution, London, 17–19 Sep 1986.

endeavor, and accept the injunction "to grasp the implicit plan of practical activity which, whether manifest or mystified, is always present in knowledge."[128]

For aid in apprehending that plan, and the corresponding integrity of knowledge attained, we can hardly do better than consult an academic administrator of contract research such as MIT's R.H. Robnett. Verging toward a categorical denial of validity to the conventional (and invidious) distinction between "fundamental" and "applied" research, as early as February 1950 Robnett insisted to President Killian that "there is no priority of purity between the most abstract problem of chemical or biological analysis and the most specific problem of component design for a reactor....There is in fact no measurable difference between fundamental and applied research with respect either to Governmental objective, the national interest, or the relation to the normal business of education."[129]

Though we may here withhold our sympathy, we cannot withhold our assent; Robnett had clearly grasped the plan of practical activity which, with only minimal mystification, was implicit in the program for knowledge of the patrons of postwar physics. Even the most liberal of the military funding agencies had at bottom little interest in the advancement of knowledge as conceptual understanding, nor even the replenishment of a depleted reservoir, as the modish metaphor presented it. Their interest in "basic" research was primarily as a means for creating new techniques, and secondarily as a means for maintaining contact with persons possessing intellectual abilities that could be turned to the advantage of the military services. "The ONR," that avatar of funding agencies, "interprets...its function as justification for backing a limited program of basic research in scientific areas which offer the chance of ultimate effect upon

128. G. Ciccotti, M. Cini, M. DeMaria, "La produzione di scienza nella società capitalistica avanzata," in the authors' *L'ape e l'architetto* (Milan, 1976); French translation, *L'araignée et le tisserand: Paradigmes scientifiques et matérialisme historique* (Paris, 1979). The cited chapter appeared revised and in English translation in H. Rose and S. Rose, *The political economy of science* (London, 1976), 32–58.

129. R.H. Robnett, memo to J.R. Killian, 27 Feb 1950, in Foster (ref. 11), *3*, FY '50, 19–25. In the following two decades less interested students of the U.S. R&D enterprise also found themselves forced to question the distinction between "basic" and "applied" research. Yet, deny it though they would, such students have had no other terms or concepts to grasp the new character of scientific knowledge. So, for example, R.R. Nelson concluded that "one of the most important things which can be learned from the history of the transistor is that the distinction between basic research and applied research is fuzzy....The project was marked by duality of results, and of motives. Yet by the standards of the National Science Foundation the Bell semi-conductor research work most certainly would be considered basic research." National Bureau of Economic Research, *Rate and direction of inventive activity* (Princeton, 1962), 581.

developments in weapons, devices, and techniques of warfare." So its Chief Scientist explained in *Science* in 1949, stressing that "the government expects practical results from the support of research." That ONR valued even basic research only as productive of technique is clear enough in the example Waterman chose to illustrate the smooth meshing of academic research and military practice: "A research investigator may show a high degree of ingenuity in devising a piece of his apparatus to perform a special, practical function," but remain oblivious to its wider applicability. The supporting agency, however, will "choose its staff to include scientists of a more practical turn of mind who by association with the needs of the agency are in the best possible position to spot possible applications."[130]

As for access to intellect, Air Force Chief of Staff Nathan Twinning, by way of congratulating his Science Advisory Board in the autumn of 1953 on their activities, expressed himself as "satisfied that our best minds are working hard on air defense." Twining was not mistaken. The best minds were working hard on military problems. In 1950/51, when the National Science Foundation was deciding its course, a majority of the guiding Board opposed renunciation of military research, not only because the NSF needed "a National Defense label to get appropriations" but equally because exclusion from military work would prevent the Foundation from "recruiting good people and arousing real enthusiasm."[131] That the ivory-towered physicists

130. Waterman, 1949 (ref. 48), 703, 706, who saw an additional advantage of this division of labor in that "the tendency toward improper pressure on the research worker or his institution is eased thereby." See also Kevles (ref. 6), 358–359; Jones (ref. 9), 360; cf. Killian (ref. 85). For forthrightness one could scarcely do better than Immanuel Estermann, prewar devotee of the purest physics, postwar advocate of applications, and now, in the spring of 1955, standing before his former Pittsburgh colleagues as the Director of ONR's Material Sciences Division: "In summary, research in many universities, where it was formerly only a minor part of campus life, is now 'big business.' An examination of the financial side of the picture discloses that most of this 'business' has a single customer, the Federal Government, with the Defense Department and the AEC paying the largest share of the bill. And it is not exactly a secret that the interest of these Government agencies in research does not stem from philanthropic emotions, but from inseparable connection between modern warfare and scientific-technological development." *Naval research reviews*, Jul 1955, 1–5.

131. Sturm (ref. 114), 40; England (ref. 39), 125–126, 142–144; similarly I.I. Rabi, as quoted by Heilbron et al. (ref. 49), 63. What was true of the scientific elite was equally true of the rank and file: "The feeling was that if the memos and reports you wrote weren't stamped 'secret,' they just weren't important; they didn't involve 'real' science or engineering." Recollections of an engineer of the late 1940s quoted by C.A. Ziegler, *Looking-glass houses: A study of the process of fissioning in an innovative science-based firm* (Ph.D. thesis, Brandeis University, 1983), 192–196; the firm was Tracerlab, 1946–1956. Similarly, the memoirs of Jeremy Bernstein, "The life it brings," *The New Yorker*, 26 Jan 1987, 35–68, and 2 Feb 1987, 36–69, on 40–42: "Although all of us at Los Alamos had our Q clearances, we were in a sense divided into two classes....The

were well aware of "the fast moving currents of the day" is amply evident from their grant and contract proposals, blatantly dangling the prospect of more powerful bombs, fuels, and other factors of military-technical superiority.[132] Had these lures, this bait for a whale of a contract, any operative function in shaping the pure knowledge sought and found? Had the consultancies and advisory services to industry and military any significant effect upon the plan of practical activity implicit in that fundamental physics?

Addressing just this question, Cini has drawn attention to the remarkably rapid establishment of "dispersion philosophy" in theoretical particle physics in the latter 1950s—first and foremost in the U.S.—asking why those tilling the most fundamental of all fields of physics accepted such a "utilitarian and pragmatic, but fragmentary, concept of science with the consequent abandoning of its traditional aim of the unification of knowledge." In answer Cini, pointing particularly to the Institute of Defense Analysis' "Jason" group, and stressing that inclusion in this consultative elite functioned as a mark of *scientific* eminence among U.S. theorists, suggested that this drastic shift in epistemic goals "was not a mechanical adaptation [to] an environment...but an active identification of its own interests—in the widest sense—with the...objectives...and the scale of values of the American society of those years by a leadership that came from within that environment."[133]

adults...knew the 'secret'...the rest of us...were not going to know it until we became adults, i.e., actually began working on weapons."

132. Seidel (ref. 14). While representing it as innocuous, L. Ridenauer, *American scholar, 16* (1947), 216, suggests the prevalence of this practice. An example is Columbia's proposal to ONR for support of their unspecified "Ultra-high energy machine" (ref. 49): "The whole field of nuclear physics and nuclear energy is based on a rather scanty theoretical foundation....One must be prepared for the most extraordinary surprises such as came about through the discovery of nuclear fission. No real security can exist until this whole field of nuclear and high energy phenomena are understood as well as electromagnetic phenomena."

133. M. Cini, "The history and ideology of dispersion relations: The pattern of internal and external factors in a paradigm shift," *Fundamenta scientiae, 1* (1980), 157–172. On IDA/Jason: G.M. Lyons and L. Marton, *Schools for strategy: Education and research in national security affairs* (New York, 1965), 252–257; also the chapters devoted to Townes at IDA and to Gell-Mann at Caltech and RAND ("there they do physics in secret for special defense projects") in T. Berland, *The scientific life* (New York, 1962). Such involvements on the part of scientists might be expected to be reflected in their rhetoric. D.H. DeVorkin, "Electronics in astronomy: Early applications of the photoelectric cell and photo-multiplier," IEEE, *Proceedings, 73* (1985), 1205–1220, on 1207, points out that "just after the war, it was common to see titles such as 'Photometry as a Weapon of Astronomical Research'...and 'The astronomer's new weapons: Electronic aids to astronomy.'" Cf. Killian (ref. 11).

Recently the more detailed historical studies by Schweber and Pickering have confirmed the insights of Cini and his associates, presenting much evidence of the phenomenologic turn taken by elementary particle theory in the late 1950s, with ontologic commitments rejected in favor of instrumentalist rules of dispersion and S-matrix theory. They find this reorientation was far more characteristic of America than Europe, and they give further arguments for regarding this turn as reflecting both a general militarization of the social purposes of physics in the U.S., and a particular mental posture fostered by the application of brain-grease to military matters.[134]

Thus supported from the least expected quarter of physical research, we turn our gaze down again from the heights of elementary particle physics to the more mundane quarters of fundamental physics: Nuclear physics, solid state physics, atomic and molecular physics. Through all these fields there runs, as a pervasive peculiarity of postwar physics, a new and widespread preoccupation with population redistribution, with modification of the normal or equilibrium populations of the various energy states of physical systems—whether those systems be molecules in a gas, electrons in a semiconductor, or nuclei in atoms. Presented in the earliest postwar experiments in microwave spectroscopy and nuclear magnetic resonance as the nuisance of "saturation"—the reduction and broadening of resonant absorption as the power density of the absorbed radiation rises—the phenomenon of population redistribution had a central place in quantum electronics well before the advent of the maser.[135] In nuclear physics too the goal

134. S.S. Schweber, "Some reflections on the history of particle physics in the 1950s," and A. Pickering, "From field theory to phenomenology: The history of dispersion relations," both to appear in Brown et al. (ref. 49); Pickering, "Pragmatism in particle physics: Scientific and military interests in postwar United States," a sharp, heuristic restatement of the Cini-Schweber thesis, presented at the History of Science Society Annual Meeting, 1985. S.S. Schweber, "The empiricist temper regnant: Theoretical physics in the United States, 1920–1950," *HSPS, 17:1* (1986), 55–98, quotes *Five years at the Radiation laboratory* (Cambridge, 1946), 6: "From first to last, the laboratory had lived by the law of cut-and-try....Pragmatism had beaten the [Germanic] *a priori*." In a paper of 1964 that was widely circulated and reprinted, Harvey Brooks (ref. 107), 209, maintained that "One of the paradoxes of modern science has been that the greater its success in a pragmatic sense, the more modest its aims have tended to become in an intellectual sense."

135. E.M. Purcell, H.C. Torrey, R.V. Pound, "Resonance absorption by nuclear magnetic moments in a solid," *Physical review, 69* (1946), 37–38. The authors considered power saturation in this, their first publication on nuclear magnetic resonance. The continuing attention of this Harvard group to power saturation in solids was a significant factor in most of their work on population inversion in the following years, achieving quite practical significance in N. Bloembergen's "Proposal for a new type solid state maser," *Physical review, 104* (1956), 324-327. The coherence between the absorbed and the stimulated radiation is implicitly assumed in all discussions of saturation; it is stated explicitly by W. Gordy, "Microwave spectroscopy," *PT, 5:12* (1952),

of altering the natural populations of energy states, especially to achieve orientation of nuclear spins, was a lodestar for much of the nominally purest research of the 1950s.[136] Yet examined more closely it is clear that this is a program for knowledge only in a typically postwar sense of the term. What is involved here is certainly not new understanding, since phenomena thus produced are "new" only in the very restricted sense of an extreme intensification of known physical processes. It is, rather, a kind of instrumentalist physics of virtuoso manipulations and *tours de force*, in which refined or gargantuan technique bears away the palm. It is just such a physics as the military funding agencies would have wished.

The military-scientific complex: Who's using whom?

President Eisenhower's parting warning in January 1961 against "unwarranted influence...by the military-industrial complex" is often quoted. It is not so often recalled that this phrase opened a lengthy and comprehensive indictment of the national security state, particularly the role of scientific research within it. "The free university," Eisenhower's warning continued, "historically the fountainhead of free ideas and scientific discovery, has experienced a revolution in the conduct of research....The prospect of domination of the nation's scholars by Federal employment, project allocations, and the power of money is ever present—and gravely to be regarded."[137]

5-9. About 1953 U.S. physicists became acquainted with A. Kastler's "Quelques suggestions concernant la production optique....d'une inégalité de population," *Journal de physique et le radium, 11* (1950), 255–265. I am indebted to Felix E. Geiger for drawing my attention to this widespread concern with population redistribution and to the history of quantum electronics generally. In this connection see also the recent work of J.S. Rigden, "Quantum states and precession: The two discoveries of NMR," *Reviews of modern physics, 58* (1986), 433–448; "The birth of the magnetic-resonance method," in P. Achinstein and O. Hannaway, eds., *Observation, experiment and hypothesis in modern physical science* (Cambridge, 1985), 205–237.

136. E.g., N. Kurti, "Nuclear orientation and nuclear cooling," *PT*, (1958 March), 18–25; R.W. Webb, "Steady-state nuclear polarizations via electronic transitions," *American journal of physics, 29* (1961), 428–444; "Discovery of parity violation in weak interactions," in B. Maglich, ed., *Adventures in experimental physics, 3* (1973), 93–162; P. Forman, "The fall of parity," *The physics teacher, 20* (May 1982), 281–288. Although there are diverse examples before World War II of experimental techniqueS aiming at alteration of population distributions—such as cooling by adiabatic demagnetization and state selection in the atomic beam technique—the object was not usually conceived in such terms. After the war, however, this point of view seems quickly to have become universal.

137. *Public papers of the presidents of the United States: Dwight D. Eisenhower, 1960–1961*, pp. 1035–1040; reprinted in Pursell, ed. (ref. 17), 204–208; Dupree, "The great instauration of 1940" (ref. 7), 464. The themes of Eisenhower's speech, as they relate to academic research, had been raised by Warren Weaver in convoking the May

It was this military-scientific complex, if I may so denominate the "enormous momentum" built up in an academic research enterprise funded by military appropriations, that in the late 1960s Senator Mansfield sought to disentangle. His efforts, allied with the spirit of the times, were not without some real effect (figure 1, curves 1bis and 2bis). Since about 1980, however, that enormous momentum of the military-scientific complex has again built rapidly to unprecedented levels: The Department of Defense is again offering U.S. universities large sums, and—as an ironic legacy of Mansfield's efforts—doing so with even more specifically military goals in mind. "There is," as Hacking says so poignantly,[138]

> no monolithic military conspiracy in any part of the globe to determine the kinds of possibilities in terms of which we shall describe and interact with the cosmos. But our ways of worldmaking...are increasingly funded by one overall motivation....It is not just the weapons...but the world of mind and technique in which those weapons are devised. The forms of that world can come back to haunt us even when the weapons themselves are gone.

How did all this come about? As is so common in historiography, those who applaud this development are inclined to regard it as the natural, indeed inevitable, expression of historical progress, while those who approve it less than unreservedly—in which broad category nearly all historians fall—explain it away as an historical accident.[139] Over and again we have lamented the delay of National Science Foundation legislation, implying that its prompt enactment would have forestalled all that I have here described. Recently Roland has rightly

1959 Symposium on Basic Research (ref. 127), which Eisenhower addressed. Cf. Eisenhower (ref. 50).

138. The quotation is from Mansfield's testimony before the Daddario Subcommittee in 1970, as reprinted in Penick et al. (ref. 7), 338–349. What Mansfield found especially disturbing was that, as NAS President Handler informed him, three quarters of Federal funds paid out to universities for research went into general institutional funds, thus creating a heavy dependence on the source. For current trends, see ref. 1, ref. 80, and ref. 84, and "DOD program proves attractive," *Science, 228* (1985), 1291. I. Hacking, "Weapons research and the form of scientific knowledge," *Canadian journal of philosophy,* suppl., *12* (1986), 237–260, on 259–260; Hacking, review of M. Minsky, *The society of mind* in *The new republic, 196:16* (20 Apr 1987), 42–45.

139. "History of the support of science in the United States proves the [ONR Physics Branch] program to be a striking development in an evolutionary process....Direct government aid through the Office of Naval Research simply multiplies manyfold the available funds to carry American science to new heights in the traditional manner." L.M. McKenzie (ref. 48), 227–228. F. Joachim Weyl, in his foreword to the ONR vigesimal volume, *Research in the service of national purpose* (Washington, D.C., 1966), vi–vii, voices a celebrant's disagreement with the tendency "to think of this as an historical accident."

criticized this delusory tendency, pointing out that national priorities, not administrative agencies, are the real issue. The nature of the research supported, and the levels at which it is supported, follow the rationale and desiderata of American policy. Consequently, "the military was bound to have a major voice in government patronage of science, no matter what institutional arrangements were adopted."[140]

Indeed, the postwar fate of NACA—of which Roland has been the closest student—forcefully argues this inevitable predominance of the military. Institutional structures and agency missions not withstanding, NACA got no part of the new fields of rocket and nuclear propulsion, while yet it was tied so closely to the military that in 1949 the Bureau of the Budget shifted NACA from the functional classification "Transportation and Communications" to that of "National Defense." The Bureau explained that the agency's growth in the previous decade had "been based entirely on military considerations" and that "all NACA officials agree that the primary mission of the agency for the foreseeable future is military in nature."[141]

We find this same "inevitability," moreover, in the history of Air Force sponsorship of basic research. Although the creation of an Air Force Office of Scientific Research was resisted internally as well as by the Bureau of the Budget and the DOD's Research and Development Board, and although the Office did not get started until after the NSF was already in existence, nonetheless it was almost immediately funding academic physical research at about the same level as ONR while NSF lagged far behind.[142]

I cannot, however, leave the question of historical responsibility for this military scientific complex without at least briefly considering the role of the scientists as accessories. Sapolsky has gone so far as to argue that academic scientists, by "their enthusiasm for research subsidies and their willingness to accept whatever rationales appeared to be effective in gaining such subsidies have produced precisely the impact upon science they sought to avoid—its permanent mobilization." Yet even if we take account also of the open and concerted advocacy by elements of the scientific-political elite of a program of

140. A. Roland, "Institutionalization of science in the military establishment," paper prepared for the annual meeting of the American Historical Association, 1984.

141. A. Roland, *Model research: The National Advisory Committee for Aeronautics, 1915–1958* (2 vols., Washington, D.C., 1985), *1*: 261, and chapts. 10–11.

142. Komons (ref. 48) is the story of such opposition. AFOSR's instant leadership in solid state science, a field NSF had itself targeted, is well shown by Table 2. In 1961, AFOSR claimed that from 1955 through 1960 25% of the papers on solid state topics published in the *Physical review* reported work supported in whole or in part by them. U.S. Air Force, Office of Aerospace Research, "A summary view of the solid state" (Washington, D.C., Nov 1961), 16.

national security based upon a permanent technical revolution in armaments, it remains unrealistic to ascribe to the scientists so much control over, and thus so much responsibility for, this enormous expansion and fundamental transformation of their social function.[143]

On the other hand, it would be hard to gainsay Sherry's observation that "their fastidiousness about their autonomy from controls obscured from them the changing role played by their profession. In agitating themselves over graft, patronage, influence-peddling, and professional standards, most scientists ignored larger questions about the uses to which they were putting their skills."[144] Yet what could the scientist do; what freedom of action and direction remained to him were he to recognize a conflict between his personal values and the goals toward which society had set its sails?

A glance at Norbert Wiener shows us that no enthusiast for his science, however acute his social consciousness and strong his social conscience, could avoid helping to fill those socially set sails with his own enthusiasm. Employed by MIT, where defense agencies funded more than 80% of the institution's research, and that in great part for military applications in fields of his own exceptional expertise, Wiener was perfectly clear that here "as everywhere, the man who pays the piper calls the tune." Having made significant contributions to guidance and tracking during the war, Wiener rebelled shortly afterwards, publically proclaiming his refusal of cooperation with the purposes and patrons that so largely shaped his institutional environment. And yet, overflowing with mental exuberance, Wiener could not refrain from promiscuous intellectual intercourse, from making himself the intellectual spark-plug of laboratories that existed only because they served ends that he deplored.[145]

143. Sapolsky (ref. 48), 389. A more balanced statement is offered by Sapolsky in ref. 3, 446. S.F. Wells (ref. 13), 158, pointing to the tocsin-sounding in 1950–52 by Bush, Conant, Oppenheimer, and other members of the original Committee on the Present Danger, seems inclined to attribute to this group a significant role in launching the spiraling technological arms race with the Soviet Union. Certainly Bush's *Modern arms and free men*, published late in 1949, was the broadest and most popular statement of this program. See *Life*, 14 Nov 1949, 112–130, and Reingold (ref. 55).

144. Sherry (ref. 8), 158.

145. N. Wiener, *Cybernetics* (New York, 1948), 188, in Wiener (ref. 61), 748–751; D.F. Noble, *Forces of production: A social history of industrial automation* (New York, 1984), 71–76. Testimony to Wiener's intellectual role comes from S.J. Heims, *John von Neumann and Norbert Wiener* (Cambridge, 1980), 379–380; Wildes and Lindgren (ref. 15), 266, et passim; J.R. Killian, interview by the Smithsonian Computer History Project, June 1972, deposited in the Archives Center of the Museum of American History. As President Killian saw it, Wiener "was all over the place, fertilizing this program and that program by simply having discussions with the people involved. He had tremendous influence on the whole engineering effort of the Institution."

Leaping four decades forward to the late 1980s, we have today no difficulty recognizing that "scientific talent will inevitably flow to those fields where national priorities put incentives of money, prestige, or excitement." Why then have we had such difficulty recognizing this circumstance in respect of the 1940s and 1950s? Why did it then appear to the Bureau of the Budget's astute student of science policy as a "fact that the scientists call the tune?" Why has it taken us so long to see, for example, that the often noted failure of radio astronomy to flourish in the United States in the late 1940s, where repeatedly interest and initiative in that direction came to nought–like the efforts of R.H. Dicke and of C.H. Townes, who were then soon instead to distinguish themselves through contributions to quantum electronics–might reasonably be layed to the lack of an evident military mission. Certainly the belated vitality of this field in the United States coincided with the recognition, in the early 1950s, of its national security significance.[146]

Certainly an important factor here was the scientists' own false consciousness, which succeeded so well in what it was intended to do, to mislead others even as it blinded themselves. On the one hand they focused so narrowly on immediate cognitive goals of their work as to miss its instrumental significance (and financial insignificance) to their military patrons. On the other hand they pretended a fundamental character to their work that it scarcely had, and/or they compartmentalized, as scientifically irrelevant, the very large technical component that bought them their quota of scientific freedom.[147]

146. W.H. Press, Harvard professor and member of the Defense Science Board, reviewing W.J. Broad, *Star warriors*, in *BAS*, *42:6* (1986), 60; W.D. Carey, memo to files on discussion of "Research and Development" with Deputy Director, BOB, 2 Oct 1956 (National Archives, RG 51, 52.1/E4-1). Confronting the question, "What is the validity of the Government's present effort?", Carey felt that they had not "a clue as to how to get hold of this runaway horse." W.T. Sullivan, III, ed., *The early years of radio astronomy* (Cambridge, 1984), 88, 391–392; J.S. Hey, *The evolution of radio astronomy* (New York, 1973), 83–89; A.A. Needell, "Berkner, Tuve, and the Federal role in radio astronomy," *Osiris, 3* (in press). Noble (ref. 145), 144–146, 325–326, 351–352 is eloquent on the general issue of roads taken and not taken, and the larger forces behind such choices.

147. The shortsightedness resulting from focusing narrowly upon the immediate cognitive goals of the research is particularly well exemplified by DeVorkin's V-2 panelists (*HSPS*, this issue) who, as he shows, considered their work as as little useful to their military sponsors as would the purest of particle physicists–and experienced the same distress at straining for what they regarded as far-fetched rationalizations. In fact, however, those rationalizations were not at all far from the truth, a truth of which the military was sufficiently sure. The panelists, in short, had a bad conscience because they mistakenly imagined that they were using, rather than being used by, the military. The alternative reaction is politically vicious aggressiveness such as manifested by N. Metropolis and G.C. Rota in their preface to *A history of computing in the twentieth century* (New York, 1980), xvi–xvii: "On a more practical plane, another unmistakable

If we can now see, as we could not then, just how little of the tune was being called by the scientists, that is largely owing to some decades of experience of "the new world" formed in those two central decades of this century.[148] It seems so patent now that the academic physicists, who, as Seidel observes, "could not be commanded," had been, as Morrison warned, "purchased on the installment plan." At the same time the AEC, as Henry Smyth soon observed, was gathering into its National Laboratories "big groups of scientists who will take orders."[149] By the mid-1950s, as the Oppenheimer case showed so clearly, scientific talent was available in sufficient depth at all levels that an uncongenial individual was a dispensable individual.

Though they have maintained the illusion of autonomy with pertinacity, the physicists had lost control of their discipline. They were now far more used *by* than using American society, far more exploited by than exploiting the new forms and terms of their social integration.

message emerges from these essays. Over the years, the constant and most reliable support of computer science—and of science generally—has been the defense establishment. While old men in congresses and parliaments would debate the allocation of a few thousand dollars, farsighted generals and admirals would not hesitate to divert substantial sums to help the oddballs in Princeton, Cambridge, and Los Alamos. Ever since Einstein wrote a letter to President Roosevelt, our best friends have been in the branch of government concerned with defense."

148. A.H. Dupree, "Preface" (ref. 7), ix, to the 1986 reissue of *Science in the Federal Government* (Cambridge, 1957), noting that "the relevance persists, but the perception of the book's meaning has changed with the years since 1957," implies that the huge antenna dish depicted on the dust jacket of the original edition was then construed as instrumentation for radio astronomy, but can now be recognized as equally or more probably for missile guidance.

149. Seidel (ref. 14), 151; Morrison (ref. 52); H.D. Smyth, quoted by Seidel (ref. 14), 148, from *BAS*, Jan 1950.