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CHAPTER 2

The Evolution of U.S. Science Policy

Harvey Brooks

THE CURRENT national debate over science policy can trace its lineage to an argument that took place in Great Britain in the 1930s between Michael Polanyi and J. D. Bernal. Then, as now, the debate concerned the degree to which planning the agenda of the national science and technology enterprise to achieve explicit social or economic goals is feasible and desirable. Polanyi stressed the need for autonomy and self-governance of the scientific community if it were to contribute most efficiently to societal goals in the long run. His view was succinctly summarized by the sociologist Bernard Barber: "However much pure science may eventually be applied to some other social purpose than the construction of conceptual schemes for their own sake, its autonomy in whatever run of time is required for this latter purpose is the essential condition of any long run applied effects it may have."[1] In contrast, Bernal, who was strongly influenced by Marxist thought, saw tremendous inefficiencies with autonomous science and believed that its enormous potential benefits for humanity could be realized only through a publicly debated plan involving government and many representative elements of society.[2] In 1934 Bernal estimated that the United Kingdom was spending only 0.1 percent of its gross national product (GNP) on research and development (R&D); the United States, 0.6 percent; and the USSR, 0.8 percent. In 1939 he called for a tenfold increase in Britain,

1. Barber (1962, p. 139).

2. An excellent summary of the Polanyi-Bernal debate of the 1930s is given in chapter 1 of Freeman (1993). The quote is from p. 21. According to Freeman, Bernal felt a gentlemen's agreement existed to gloss over the internal inefficiencies of science for fear of losing even the present inadequate resources that science gets.

a level that was achieved twenty-five years later.[3] Bernal saw the USSR as a hopeful model of the potential contribution of science to society, based partly on his implicit acceptance of Marxism as a social science rather than as a political ideology. Later he came to share the general disappointment with the social and economic returns from R&D in the socialist countries that made the changes that he had suggested.[4] But he may have also recognized that this relatively low payoff from increased R&D was partly the result of the concentration on military-oriented R&D and that the spinoff of a few high-tech industries from military R&D could not offset the lack of R&D investment in a much wider range of industries.[5]

The Bush versus Kilgore Debate

At the close of World War II, many features of the Polanyi-Bernal debate of the 1930s were renewed in the United States over competing visions of the future of American science policy. The wartime experience helped to shape the debate. Vannevar Bush became the exponent of the Polanyi view, articulated in the famous 1945 report *Science: The Endless Frontier,* while Senator Harley Martin Kilgore, D-W.Va., became the exponent of the Bernal view, with more active government direction of the research agenda.

In his recommendations to President Harry S. Truman in 1945, Bush selected the university as the centerpiece of postwar science policy specifically because of its independence and autonomy:

> It is chiefly in these institutions [universities] that scientists may work in an atmosphere which is relatively free from the adverse pressure of convention, prejudice or commercial necessity. At their best they provide a substantial degree of personal intellectual freedom. . . .
>
> Satisfactory progress in basic science seldom occurs under conditions prevailing in the normal industrial laboratory. There are some notable exceptions, it is true, but even in such cases it is rarely

3. Freeman (1993, pp. 7, 10); Bernal (1978, pp. 62, 242).
4. Freeman (1993, p. 14); Bernal (1958).
5. Freeman (1993, pp. 15–16); see also Alic and others (1992), especially chapter 12.

possible to match the universities in respect to the freedom which is so important to scientific discovery.[6]

Partly because of the unforeseen advent of the cold war before the establishment of the National Science Foundation (NSF), the universities came to represent only a fraction of public spending on R&D (about 9 percent, rising to 12 percent for universities proper and eventually to about 16 percent, including separately organized, government-owned R&D centers operated under contract by universities). Nevertheless, universities remained central to the debate about American science policy. They employed the majority of Ph.D.s in science and in engineering, performed the majority of what was defined as basic research sponsored by the federal government, supplied a majority of the science advisers to government agencies, and were looked to by government as a first option when the need for new initiatives and institutional arrangements relating to science and technology were identified by policymakers.[7] The average person who follows the press and other media is frequently incredulous when told what a small proportion of federally funded R&D is performed in universities (in the mid-1990s, for example, only about half that performed in government-owned laboratories). According to some scholars, Bush's statement quoted above served its author's purposes in urging expanded federal support for the conduct of basic research by universities, and it reflects the basis of the current justifications for public patronage of university-based research in the domain of science and technology.[8]

Senator Kilgore was equally bullish regarding the support of science and its promise for the achievement of social and economic goals, but he called for a much closer linkage to political institutions and less autonomy for the scientific community in setting the research agenda. The model for science envisioned by Kilgore was akin to that established in the nineteenth century for the agricultural research system, the extension service, and the land grant universities based in the states, with more emphasis on the wide geographical distribution of research capacity,

6. (Bush and others, 1960, p. 19). Bush's observation seems to be supported by the gradual migration to academia of some of the most creative and productive scientists even from such exceptional industrial laboratories as the Bell Laboratories, the General Electric Research Laboratory, the various IBM Corporate R&D centers, and several other notable industrial laboratories with a long history and strong traditions.

7. Brooks (1986a).

8. David, Mowery, and Steinmueller (1994).

stricter political accountability, the diffusion of new knowledge, and its adaptation to the needs of potential users.

Nor was the scientific community unanimously on the side of Bush. Important and prestigious scientists such as E. U. Condon and Harlow Shapley favored the Kilgore view. For the most part, however, the outcome of the debate was a fairly conclusive victory for the Bush approach, with the important exception that the director of the new National Science Foundation was to be appointed by the president and confirmed by the Senate, not by the National Science Board without Senate confirmation as originally proposed. The controversy over political accountability and responsiveness of the NSF delayed its establishment for nearly five years, allowing time for the habits and the culture of the support system to evolve as an adaptation of the military R&D system inherited from the war effort.[9]

The Watershed of World War II

The United States is unique among the industrialized countries in how its R&D system and science policy was transformed permanently by World War II. In both Britain and America, science and technology were mobilized on an unprecedented scale—more or less the scale that Bernal had in mind, only for war and not social and economic development as he had advocated. In the United States, R&D as a proportion of a much larger gross domestic product (GDP) reached about 0.8 percent in 1945.[10] Even with respect to the military, the role of science and scientists was far different in World War II from what it had been in World War I or in the interwar years. Then, military research was performed largely in civil service laboratories under direct military supervision, and weapons were purchased from government arsenals or from industry to detailed specifications set by the military with relatively little support from R&D. In the case of industry, R&D consisted mostly of experimental design and testing, which were integral parts of the procurement process. The separate R&D contract for development of a weapons system was largely unknown, as was the simultaneous parallel pursuit

9. See, for example, Sapolsky (1990).

10. According to Galbraith, gross domestic product (GDP) increased by 80 percent between 1939 and 1944. See Galbraith (1994, p. 115). *McGraw-Hill Yearbook of Science and Technology* (1963, pp. 11–21; table 1, p. 13).

through R&D of alternative approaches to the design of a given weapons system.

In 1935 the U.S. federal government accounted for only 13 percent of total national expenditures for R&D, which amounted to only 0.35 percent of national income; the federal contribution to R&D was thus only 0.05 percent of national income. As late as 1938, agricultural research was about 40 percent of federal R&D (more than military R&D, which accounted for only about 25 percent).[11] Total public R&D represented only 0.25 percent of the federal budget in the 1930s. Both agricultural and military R&D were performed principally in civil service laboratories and supported primarily on a level-of-effort basis institutionally rather than a project-by-project basis, so that expanding effort rapidly was difficult as unforeseen possibilities were revealed by research.

By 1962 the federal share of total national R&D support had risen to nearly 70 percent and represented 11 percent of the discretionary spending in the federal budget. Of federal R&D expenditures in 1962, nearly 93 percent were in defense, space, and atomic energy—all essentially deriving from the cold war. Agricultural research, though larger in real dollars than in 1938, was now only 1.6 percent of federal R&D. As to the actual performance of research (as opposed to federal support flowing to external research-performing entities), only 14 percent was performed in laboratories staffed by civil servants. An additional 6 percent was in wholly owned government facilities run by contractor personnel (known as Federally Funded Research and Development Centers, or FFRDCs). The balance of 80 percent was contracted out to private industry, independent private not-for-profit laboratories, and colleges or universities.[12]

As an indication of the climate of opinion at the time, many in the military on the eve of World War II saw increased investment in R&D as delaying the procurement of badly needed weapons of existing design.[13] Largely on the initiative of the university scientific community, led by Vannevar Bush and James B. Conant, American scientists and engineers began to mobilize for the impending war effort. Particularly involved were the nuclear physicists, who were at the forefront of the

11. Brooks (1968, p. 24).

12. These statistics are taken from a variety of sources. They are summarized in Brooks (1990, pp. 11–12).

13. According to an Army General Staff report issued at the time, "The Army needs large quantities of excellent equipment that has already been developed" but "the amount of funds allocated to research and development in former years is in excess of the proportion for that item in consideration of the rearmament program." See Dupree (1957, p. 367).

most exciting science of the times. They began to mobilize in a few university-based laboratories, where they could work with their own colleagues in a familiar environment. Methods of contracting for R&D at these university-managed laboratories were rapidly devised, based on the principle of full-cost reimbursement (including indirect costs, an accounting innovation that has continued to be unique to the United States for government support of research) and "no gain, no loss" financially to the institutions or individuals who joined the effort. The laboratories, in turn, undertook to provide their best efforts with no guarantees as to outcome. As the United States entered the war and urgency increased, the simultaneous pursuit of parallel technical paths to solve military problems became standard practice in the belief that the extra costs of possibly duplicative R&D could be ignored, given the losses that might be incurred by military failure.[14] This "cost is no constraint" approach was facilitated by the technical resources of the nation, like its other productive resources, which were grossly underused in 1939.[15] The parallel project approach assumed its most extreme form in the Manhattan Project for the production of the atomic bomb.

The most dramatic technical developments of World War II emerged from the university-centered wartime laboratories organized under the auspices of the Office of Scientific Research and Development (OSRD), a civilian agency reporting directly to President Franklin D. Roosevelt and headed by Vannevar Bush. Bush had the authority to undertake strategic research initiatives independently of the War Department and the military services, although in practice he usually cooperated closely with them. To Bush, an engineer and inventor and not a scientist, the arrangement was a lesson in the necessity for autonomy and independence in technical decisionmaking. The most dramatic and visible parts of the technical agenda were in the hands of people whose backgrounds and original acculturation had been in the world of academic science, particularly the international fraternity of nuclear physics, where engi-

14. This came to be known as the "whole problem approach." In the words of one observer, scientists "were eventually able to persuade the soldiers to inform them of the general military problems involved in order that the scientists might reach their own conclusions about the kinds of weapons and devices the military would need to meet these problems." See Schilling (1964).

15. Unemployment in the United States was at 17.1 percent in 1939 compared with 3.9 percent in 1947. See Galbraith (1994, p. 136). By contrast, the German economy was already at close to full employment at the beginning of the war.

neering skills in the design of complicated apparatus were at a premium. The physicists had already developed a tradition of teamwork, which was still absent from most of the rest of academic science. The social organization of the nuclear physics community at that time was uniquely adapted to meeting the particular technical challenges of World War II and helped set the pattern of thinking that heavily influenced the evolution of science policy during the cold war and beyond.

Among other things, this pattern of thinking tended to reinforce a linear model of innovation in which research largely preceded engineering and development. Research thus dominated the innovation agenda even though it absorbed only a small fraction of the total resources and human effort going into the innovation process. Descriptions of the innovation process emphasized the flow of information from science to engineering, and then successively to production and to the market. Overlooked were the reverse flows back along the chain from the market to production and to research as well as the leaps that often bypassed the intermediate steps in the linear description. Thus, in the idealized linear model, innovation begins with basic research that turns up discoveries while being pursued almost without thought of application. These discoveries in turn suggest opportunities for applications that are pursued through applied research, development, design, production, and marketing. In this model, the rest of the innovation chain cannot exist without basic research, which is the foundation on which the productivity of all subsequent investments depends. This simplistic model, though increasingly challenged by scholarly research, has had an important and persistent influence on the organization and management of innovation in the United States until recently. It still provides a plausible description for many of the radical paradigm-shifting technological innovations during the cold war period, not only in defense but also in a number of commercial areas.[16] The most classic case is the development of nuclear weapons and nuclear power following the discovery and theoretical explanation of nuclear fission, but other examples include the transistor, the laser, genetic engineering, and many biomedical technologies.[17] None of these

16. For an analysis of the types of innovations for which the linear sequential or staged model is reasonably appropriate, see Lee (1992).

17. For some recent examples in the mathematical and physical sciences, see National Science Foundation (1994), especially "Origins of the Information Superhighway: A Retrospective Look at the Discoveries That Made Possible the Emerging Technologies of Today and the Services of Tomorrow," pp. 12–16.

cases is as pure as the linear model predicted, however, and their success depended heavily on follow-on investments for incremental improvement. Nevertheless, enough truth exists to suggest that abandoning the linear model would be a serious mistake.

The linear model works best for a firm when: (1) the company is seeking radical product innovations that do not fit into its existing business and marketing organization; (2) the new product is expected to have a long shelf life without the necessity for many new models or fine product differentiation; (3) the technical resources of the company and its R&D investment are large compared with those of its potential competitors; (4) the market is protected by oligopoly, a legal monopoly, or public procurement policies biased in favor of domestic suppliers; (5) the nature of the demand is such that the trade-off between cost and performance tends to be weighted heavily in favor of performance; and (6) the total number of units sold is expected to be relatively small. The bias toward performance over cost tends to be particularly marked in the case of defense applications and curative biomedical technologies but is also true for major categories of capital equipment. All six of the criteria were present in much of the weapons industry, the nuclear power industry, the telecommunications industry, and, in some degree at least, the U.S. computer industry in the early period of the cold war, when the biggest market was still the military or the space program. In some measure the isolation of the R&D function was an advantage, because new ideas were less constrained by existing organizational routines and concepts of market demand. For novel products, customers usually do not know what they want until they have some notion of what they can get—in other words, of what is technically possible. The history of radical innovations is studded with ludicrous underestimates of potential markets, such as the prediction attributed to Thomas Watson that the world market for computers might eventually be five large computers. The lack of interest of many large companies such as Kodak in the early concepts for xerography is another illustration of the same phenomenon.

However, the persistence of the linear mental model of innovation probably led to an organizational trend in U.S. industry that was ill adapted to the emerging international competitive environment in the 1970s and 1980s. Many large companies had established corporate R&D centers on idyllic rural campuses far removed from their business divisions in the expectation that this was the best way to produce breakthrough innovations that would replace the mature businesses that some corporations seemed prepared to abandon to low-wage countries, ac-

cording to the product cycle theory fashionable in the 1960s.[18] This mindset proved especially dysfunctional for the U.S. automobile industry.[19]

The Challenge of the 1970s

The years 1967 and 1968 marked the high-water mark of the post–World War II federal investment in R&D in the United States and a turning point in U.S. science policy. Up until then, science policy had been dominated by the cold war. By 1963 the national investment in R&D was approaching 3 percent of GDP, 2.5 times the peak reached just before the end of World War II, and more than 70 percent of this effort was supported by the federal government. Of this, 93 percent came from only three federal agencies: the Department of Defense (DOD), the Atomic Energy Commission (AEC), and the National Aeronautics and Space Administration (NASA)—all largely driven by the rivalry with the Soviet Union. The technology management styles of these three agencies were similar, although the organic statutes of both AEC and NASA gave them a more explicit mission of ensuring the commercial spinoff of the knowledge and technologies they had developed. Much of the rest of federal R&D was in the biomedical field; the budget of the National Institutes of Health (NIH) had soared since about 1956 and was growing at close to 30 percent a year. During this period, 55 percent of the federally financed research performed in universities was in the life sciences, supported mostly by NIH and secondarily by the Department of Agriculture.

Federal support for R&D as a whole and for (mainly) basic research in universities reached a peak in 1967, after which it declined in real terms until about 1976. A partial exception was the life sciences, which continued to grow but at a much diminished rate. In the mathematical and physical sciences and engineering, a drop occurred in actual or nominal dollars; the drop was about 14 percent below the 1967 peak in real terms.[20] It was a period of sharp decline in job opportunities for scientists and engineers, especially junior-level faculty appointments in universities. Unemployed scientists and engineers received considerable attention in

18. Vernon (1966).
19. Florida and Kenney (1990); see also Dertouzos, Lester, and Solow (1992).
20. Brooks (1986b, pp. 119–67).

the media and in Congress, even though their unemployment rate remained well below that of skilled production workers and technicians.[21]

More germane here were the challenges to the linear model of innovation. An amendment sponsored by Senator Mike Mansfield, D-Mont., to the fiscal 1970 defense authorization bill required that "none of the funds authorized to be appropriated by this Act may be used to carry out any research project or study unless such project or study has a direct or apparent relationship to a specific military function or operation."[22] The Mansfield amendment was directed against general purpose basic research funded by DOD. Although the legal force of the amendment applied for only one budget year, it sent a strong message to all research funding agencies and resulted in the transfer of several long-term DOD university research programs to civilian agencies, usually with diminished funding. For example, the innovative Materials Research Laboratories started by the Advanced Research Projects Agency (ARPA) of DOD in 1961 were moved to the National Science Foundation. The National Magnet Laboratory at the Massachusetts Institute of Technology (MIT) started by the Air Force, was also transferred to NSF sponsorship. Several DOD-sponsored programs involving the social sciences were canceled.

Another challenge came from a widely publicized DOD-sponsored study known as Project Hindsight, which was widely interpreted as showing that few ideas originating from basic research had contributed to specific DOD weapons.[23] This argument was seized upon by critics to denigrate the value of federal sponsorship of academic research in general. A counterstudy commissioned by NSF called Technology in Retrospect and Critical Events in Science (TRACES) sought to show that the Hindsight study had utilized too short a time horizon in trying to identify the basic research events that had contributed to technological advances. However, the TRACES study dealt with wholly different examples, mostly from the commercial area, so that directly comparing the two sets of results was difficult.[24]

News commentators, citizens, college students, and members of Con

21. Brooks (1977).

22. P.L. 91-121, Section 203. For Senator Mike Mansfield's explanation of the rationale behind this amendment, see U.S. Senate (1970, pp. 604–09). See also Smith (1990, pp. 78–85).

23. Sherwin and others (1966); Isenson (1969).

24. Illinois Institute of Technology (1968). Mowery and Rosenberg show that the concept of research events is much too simplistic and that neither Hindsight nor Technology in Retrospect and Critical Events in Science (TRACES) employs a methodology that is capable of showing what they purport to show. See Mowery and Rosenberg (1982).

gress were also pressuring the scientific community in many forums to turn away from curiosity-driven basic research to pressing social problems, especially those that had been raised to high public visibility by the Great Society programs of President Lyndon B. Johnson. A considerable turning away from science by college undergraduates became evident, while at the graduate and postdoctoral level interest in the social sciences expanded.[25] Some claims advanced on behalf of the social sciences at this time seem overblown and simplistic in retrospect, often amounting to the advocacy of social engineering with strong manipulative overtones. This eventually helped to trigger a backlash against federal support of the social sciences when the Reagan administration assumed power in the 1980s. Nevertheless, this period did produce some ultimately useful soul-searching on the part of the entire scientific community.

One result of the pressures on NSF was the invention of a new program known as Research Applied to National Needs (RANN). A separate part of NSF, RANN consisted of a series of interdisciplinary projects carried out by teams of scholars in universities drawn from several different disciplines and focused on a particular societal problem. It received mixed reviews, especially for those projects involving a large social science component.[26] The most successful programs were those dealing with energy conservation, renewable resources, certain environmental problems, and solar energy. These served to some extent as pilot programs for the much larger programs that the newly created Department of Energy (DOE) developed in the 1980s.

A parallel program managed by the National Bureau of Standards (NBS)—Experimental Technology Incentives Program (ETIP)—worked mainly with regulatory agencies in designing strategies of regulation that would encourage technological innovation (for example, general performance standards for household appliances in place of restrictive detailed design standards). The program was successful in achieving its limited objectives, but the scale was too small to have any permanent or widespread influence on regulatory strategies as a whole. In the end, both RANN and ETIP failed to take root as continuing programs in their respective sponsoring agencies.

From 1968 to 1975, scholars and a few government researchers began

25. For a contemporary evaluation of the status and future prospects of the social science disciplines, see the Social Science Research Council (1969), the so-called BASS report.

26. National Science Foundation (1978), especially "RANN Programs in the Social and Behavioral Sciences," pp. 63–72.

sounding the alarm about an impending loss of U.S. international competitiveness as a result of an apparent decline in the rate of growth of manufacturing productivity, usually attributed to underinvestment in R&D.[27] The principal response of the Nixon administration was the planning of a large-scale program of federal technological investment, following the model of spinoff from space and defense expenditures that had held sway at DOD, NASA, and AEC.[28] This program never got far because of budgetary stringency.

Another casualty of the political turbulence of this period was the dismantling of the White House science advisory apparatus. The functions of the science advisor and the President's Science Advisory Committee reverted to the director of NSF and the National Science Board (to whom they had been assigned by the law establishing NSF, but who had done little in practice). The functions of the Office of Science and Technology and some of those of the interagency Federal Council on Science and Technology were absorbed by a somewhat augmented NSF staff in the office of the NSF director. This development was viewed with some dismay by the scientific community, but the influence of the original apparatus had already been greatly diminished before the Nixon reorganization.

Shortly after the inauguration of President Gerald R. Ford, interest in the reestablishment of the science advisory apparatus in the White House arose in a number of quarters. Vice President Nelson Rockefeller was asked by President Ford to make recommendations in this regard. In the meanwhile, the National Academy of Sciences also initiated with private funding a comprehensive study of the top-level science organization of the federal government. This study was chaired by James Killian, former president of MIT, who had been President Dwight D. Eisenhower's first science advisor.[29] This combination of initiatives ultimately led to the creation by Congress of a new Office of Science and Technology Policy in the Executive Office of the President and to the reestablishment of the science advisor position, along with some additional associate director

27. Boretsky (1973). For a very early comprehensive academic analysis of this problem, see Hollomon and Harger (1971). See also discussion of Boretsky's gadfly role in Branscomb (1993, p. 65).

28. For a well-documented skeptical study of the technology transfer program in the National Aeronautics and Space Administration (NASA) and the spinoff model in general, see Doctors (1971).

29. National Academy of Sciences (1974); see also Brooks and Skolnikoff (1975) and Smith (1992, chapter 8).

positions. The new apparatus was nominally more influential than the old, though in practice the influence of the science apparatus depends much more on the interests and receptivity of the president than on its formal status. The main strength of the new legislation was that for the first time the science apparatus in the Executive Office had full statutory sanction, so that it became difficult to abolish completely and easier to revive when a more interested president came into power.[30] Previously, the office had been established and disestablished through the president's reorganization authority without explicit legislative sanction.[31]

Resurgence of the Cold War

After the era of social priorities and the collapse of the Nixon administration, there began in 1976, unforeseen by much of the scientific community, a resurgence in federal support of R&D as well as private R&D investment.[32] Much of this was stimulated by the reemergence of foreign affairs at the center of national attention. The Iran hostage crisis, the Soviet invasion of Afghanistan, and the general reintensification of the cold war produced a new military buildup beginning in the second half of the Carter administration and greatly accelerating in the Reagan administration. Two factors were especially important in the increased R&D spending. First, energy policy and, to a lesser extent, health and environmental policy emerged on the national political agenda. The AEC eventually became the Department of Energy, with a much broader charter covering all energy sources, not just nuclear power, and with renewable energy briefly achieving a larger budget than nuclear energy during the Carter administration. Federally supported energy R&D increased by a factor of 4.6 in real terms between 1969 and 1981 but sank back to only 1.9 times its 1969 level by 1994. A parallel increase was seen in private energy R&D investment during the same period. By contrast, defense and space together had fallen to only 58 percent of its 1969 level by 1976 (65 percent of total federal R&D).[33] The falloff in total

30. National Science and Technology Policy, Organization, and Priorities Act of 1976, U.S. Code 6683; see also Branscomb (1993, chapter 1, especially pp. 7–9).

31. Branscomb (1993, p. 7) and Brooks (1986a).

32. I speculated about such a development in the Donald Hamilton Memorial Lecture given at Princeton in 1973. See Brooks (1974).

33. Brooks (1986b, p. 131).

federal R&D after 1967–68 lasted to 1976, when it began to recover slowly.[34]

Second, public concern grew steadily about the loss of U.S. competitiveness in international markets, which was still at that time largely attributed to lack of investment in R&D, particularly civilian R&D. Although the total U.S. R&D investment as a fraction of GDP was comparable to that of Europe and Japan, a much larger fraction was in the defense and space sectors. Economists were increasingly arguing that military R&D produced much less benefit to the economy as a whole per dollar than commercially oriented R&D. Thus a complete reversal occurred of the previous terms of debate in the late 1960s, when J. J. Servan-Schreiber, a French journalist, had written a best-selling book arguing that the U.S. investment in space and defense R&D was inevitably generating an insuperable competitive advantage for the United States in world markets—the so-called technology gap in favor of the Americans.[35]

Already in the early 1970s the argument was turning around to say that the U.S. defense burden was creating a technology gap in the opposite direction, favoring competitors whose investments in space and defense were smaller. The proponents of this view tended to dismiss as irrelevant that civilian R&D and privately financed commercial R&D in the United States far exceeded that of its principal competitors—about the same as that of West Germany, France, the United Kingdom, and Japan combined (as of 1979).[36] This dismissal assumed that virtually no economies of scale exist in R&D and that any spinoff of R&D between different institutions in a country stops abruptly at national boundaries. Nevertheless, the R&D-to-GNP ratio has become a shibboleth of technology and competitiveness arguments that persists to this day.[37] Such arguments apparently had a considerable influence on both private and public decisionmaking, as evidenced by the accelerated growth of private R&D spending after 1976, even through the recession of 1980–82, when many companies were laying off production workers while continuing to expand their technical work forces.[38]

When the Carter administration took office in 1977, one of its earliest

34. Brooks (1986b, p. 127).

35. For a later review and critique of Servan-Schreiber's argument, see Brooks (1973, pp. 1, 8–11).

36. Brooks (1986b, p. 136).

37. For an extended discussion of this issue, see Brooks (1985).

38. Brooks (1986b, p. 127).

initiatives was to launch a major study of the impact of federal policies, including R&D, on U.S. competitiveness. The study was to make recommendations for changes in federal policy that would enhance private incentives for innovation and investment in R&D.[39] As the first major official study to emphasize the demand side of R&D policy, it represented a major departure from basing policy on a linear model of technological innovation. Its impact was blunted, however, by a number of foreign policy events that temporarily diverted both public attention and that of decisionmakers from the economic competitiveness problem. The Iran hostage crisis and the second oil embargo were notable, the latter leading to the rapid buildup of energy research and development and demonstration programs. Because of the unprecedented inflation associated with the energy crisis, the underlying (mainly microeconomic) sources of declining U.S. competitiveness were put on the back burner until well into the 1980s, after inflation had been brought under control.

The rapid defense buildup in the Reagan administration peaked in 1985 but probably helped to provide a Keynesian boost to the economy. The defense R&D program of sophisticated weapons such as the strategic defense initiative (SDI) also had the effect of prolonging implicit acceptance of the linear model of innovation until the leveling off of defense spending once again began to expose the deeper structural problems of the civilian economy. Although the Reagan defense buildup was more development- and less research-intensive than the 1960s buildup, basic and academic research still fared well financially in the 1980s, assisted by the continuing boom in biomedical research and the emergence of new commercial opportunities in biotechnology, microelectronics, computers, and optoelectronics. Many of these commercial opportunities were closely linked to basic and academic research, even though the technological developments required larger downstream investments by industry, many of which were venture capital startups.

At the same time, subtle but important changes were taking place in the role of universities in the U.S. research system. One of the most important had to do with intellectual property. Until the mid-1970s it was almost a matter of pride with most university researchers that the academic research system was completely open. Patents were seen as obstructing the free flow of information. The discoveries in molecular biology in the early 1970s for the first time revealed the possibility of

39. U.S. Department of Commerce (1979); see also U.S. House of Representatives (1980, especially pp. 155–73).

commercial application of fundamental phenomena hitherto thought to be of only academic interest. Before the late 1970s, if patents were taken out on university research supported by the federal agencies, they were most frequently assigned to the government, which had no particular mechanisms or policies for fostering pursuit of their further development other than for government end-use. No governmentwide patent policy was in place, and each sponsoring agency had its own guidelines ranging from compulsory ownership by government (AEC, NIH, and NASA) to some system of rights for the individual inventor (DoD and Agriculture). Permission to license for commercial exploitation did exist (though exclusive licenses were rare). By the end of the 1980s the situation had dramatically changed. Most universities with significant research programs had established policy guidelines and a special office to deal with intellectual property issues. These offices were becoming more aggressive in patenting inventions made by university personnel and in seeking to license such patents for commercial development to interested companies or to assist the inventor(s) to establish a new company. Legislation enacted in 1980 and 1984 encouraged this process and specifically permitted university ownership of patents on inventions made with the support of government grants or contracts. The legislation also allowed the (possibly exclusive) licensing of these patents to small firms, and subsequently to larger firms, always with a free license for government end-use.[40]

During the 1980s a very dramatic growth also was seen in the number of University-Industry Research Centers (UIRCs) supported jointly by one or more federal agencies, industries, and the states. By 1990 more than one thousand such centers had been identified having an aggregate budget from all sources of more than $4.12 billion, with about 31 percent of the funds on average coming from industry—about three to four times the share of other industry support for academic research. About 60 percent of these centers had been founded after 1980. Although the majority of them saw their role as "principally providing windows on new technological developments" to industry instead of "more direct contributions," 26 percent of the UIRCs surveyed reported "transfer of technology to industry" and "improving industry's products and processes" as significant goals. These observations are consistent with the fact that only 16 percent of their R&D activity was reported as "devel-

40. Sandelin (1994); cf. also Public Law 96-515, December 12, 1980, "Patents and Trademark Laws, Amendment," and Public Law 98-622, "The Patent Law Improvement Act," November 8, 1984; and Smith (1990, pp. 58–62).

opment." Relations with industry were spread over a wide range of fields. Furthermore, the UIRCs differed greatly among themselves in the relative amount of support from various sources and in the closeness with which they worked with industry.[41] In the 1960s more than 70 percent of academic research support came from federal agencies; the proportion had dropped to only 57 percent by the 1990s and was still declining. Large differences existed among individual universities, with many private universities being more dependent on federal funds than the state universities.

Economic Performance and Government-Sponsored Research

During the resurgence of the cold war, the pressure for societal direction of research, especially university research, had lost some of its political impetus. The steadily growing resources enjoyed in aggregate by R&D after 1976 allowed a relatively autonomous science culture to live more easily side-by-side with a more societally driven agenda within the universities. But the end of the cold war brought a new surge of interest in tight social direction of science. This time, instead of reflecting goals of the Great Society, interest focused on international economic competition and private sector job creation. For the first time those on the lower rungs of the economic ladder were not the only ones being hurt by the weak performance of the U.S. economy. The middle class was also affected by the restructuring of the economy. Job losses and downsizing of firms resulted as the deregulation of protected monopolies or semiprotected oligopolies took effect. The universities came under increasing pressure to do something about the economy, perhaps stimulated by some of the rhetoric that had often been used in justifying federal support of academic research in the past.

A subtheme of the debate was the tension between open science, represented institutionally by the elite research universities, and "appropriable science," represented by influential segments of industry and politicians in support of "technomercantilism."[42] Many paradoxes and crosscurrents arose in the debate not congruent with political ideologies

41. Cohen, Florida, and Goe (1994). On average about 34 percent of the support for these centers was provided by federal agencies, a far smaller fraction than for university research overall. About 12 percent came from state funds and 18 percent from internal university funds.

42. Dasgupta and David (1992).

in other areas. A second subtheme related to the degree to which social need rather than scientific opportunity should govern research priorities, especially for academic research. The sharp dichotomy between the Polanyi view of autonomous science and the Bernal view of science entirely driven by social need seems increasingly artificial. Most science and technology are, and must be, driven by social need, so the real issue is the proportion of the total public resources for research that should be driven primarily by the internal logic of the subject (scientific opportunity), not an either/or choice between extremes that are seldom seen in their pure form. Further complicating the issue is that in practice much significant fundamental science has been derived from practical problems first encountered in applications but subsequently pursued much more deeply in a conceptually oriented mode. Thus the real choice lies in how far to encourage investigators to go beyond the immediate need for a solution to a practical problem close at hand to the more general conceptual problems that arise in the course of the research. Often entirely new domains of fundamental research open up that would not otherwise have been identified in the absence of the original applied problem.[43]

Two separate issues arise in the debate about the allocation of resources for research, which have frequently been confused in public and congressional discussion. One is the proper balance between top-down management by the government sponsors of research—usually a program officer in a federal agency—and reliance on bottom-up generation from the grass roots of the scientific and technical community. The other is the appropriate level and mode of participation of nonscientists and politically accountable generalist policymakers in the design and evaluation of research programs.

A program could be tightly managed from Washington by purely technical experts determining the research agenda of projects in considerable detail, leaving only narrow discretion over strategy to the performer of the research. This tended to be the style in which the NSF RANN program of the early 1970s was managed. Or a general field of research could be determined at a high level in Washington, but scientific peer evaluation and selection of competitive proposals from the grass roots of the scientific community could be depended upon to respond to a broad RFP (Request for Proposal) for the research strategy within the broad program guidelines.

Conversely, an open competition could be held among proposals,

43. Brooks (1980)

originating entirely bottom-up from the scientific community, unconstrained by need-driven guidelines but utilizing a competitive evaluation process that incorporates the views of laymen and generalists as well as technical experts. Various balances of representation between laymen and experts could be decided at the start, but the societal need component could be allowed to develop directly out of the evaluation process instead of being determined in advance.

Many different combinations of the two dimensions—top-down versus bottom-up and generalist/laymen versus experts—are possible. In the Advanced Technology Program (ATP) of the National Institute of Standards and Technology (NIST), for example, some projects are responsive to broadly defined RFPs and are evaluated primarily by experts, with a certain proportion of the available funds reserved for an unrestricted competition in which the evaluations are made by a mix of experts and generalists. A further complication is the possibility of a staged project evaluation process in which proposals are first screened by experts for technical quality, and then the technically high-quality projects are further winnowed by more broadly constituted panels of experts and laymen.

If the process of using science for social purposes is thought of as one of optimally matching scientific opportunity with social need, then the total evaluation process must embody both aspects in an appropriate mix. Experts are generally best qualified to assess the opportunity for scientific progress, while broadly representative laymen in close consultation with experts may be best qualified to assess societal need. The optimal balance between opportunity and need can only be arrived at through a highly interactive, mutual education process involving both dimensions.

The original rationale of Vannevar Bush was to stress opportunity as the prime criterion of government support, assuming that the need-fulfillment aspect would take care of itself through the workings of the market or the political process after the results of the initial research were widely available. In other words, the function of government support would be primarily to push out the frontiers of opportunity and thereby generate a new technical capacity or resource on which society would be able to draw to fulfill present or future needs. Even though the opportunities might overlap, the spectrum of needs would be so broad that most opportunities would prove relevant to some need—if not immediately, then at some time in the future. The superior efficiency of the opportunity-oriented mode of approach would more than compensate for its less certain immediate relevance to an identified need. This is

especially true because knowledge in the context of a well-developed conceptual framework can usually be communicated much more efficiently, and with lower transaction cost than knowledge in bits and pieces generated in the course of solving a technological problem.

The new paradigm, as articulated in a 1994 report by President Bill Clinton and Vice President Al Gore, is "Science, the Endless Resource" instead of "Science, the Endless Frontier."[44] At first sight, little difference is apparent, because in both cases a resource for solving societal problems is being generated. However, the new metaphor demands not only that the ability to create new knowledge be continually improved, but also that the ability to integrate new knowledge with old knowledge and enlist it in the betterment of the human condition be continually enhanced. This modification of the old Bush idea has been recently formulated by Paul A. David, David C. Mowery, and W. Edward Steinmueller in the following terms:

> The emphasis in science and technology policy has been placed on fostering the generation of new knowledge, rather than the distribution of knowledge and the possibilities of improving the performance of the system by improving access to the existing knowledge stock. We too are persuaded that this thrust has been maintained for too long, and there is a case to be made now for restoring some balance; in other words, to raise not only the marginal social rate of return on future R&D expenditures, but to increase the social payoff from such outlays made in the past by increasing the commercial exploitation of the knowledge.[45]

This is a genuinely new challenge to science policy and one toward which the new national policy is groping. The problem is that no understanding has emerged about what it means in practical terms for the conduct of the technical enterprise. David, Mowery, and Steinmueller suggest that

> the educational programs of those individuals that pursue a professional course of study in management need to incorporate an understanding of the nature of technology and the relation between technology and business. Similarly, those individuals pursuing

44. Clinton and Gore (1994, pp. 1–2).
45. David, Mowery, and Steinmueller (1994).

courses of study in the scientific and technical disciplines need an understanding of the legal and economic structure which will impinge directly on their careers. Lastly . . . American universities might be asked to . . . enhance the capacity of domestic business to monitor and benefit from timely information with regard to market developments, as well as technological changes, taking place in other countries.[46]

Unresolved Issues

A number of issues of future research policy and strategy have come to the fore with the damping down of the cold war, the emergence of a new set of public concerns, and the new political constraints engendered by the 1994 election.

Will the national laboratories or other free-standing nonprofit research institutions separate from universities be the principal locus of government support for addressing the competitiveness and economic performance missions, or will the main locus remain within or in close association with the research universities?

The national laboratories have recently been moving aggressively to embrace the new mission of technological innovation for economic performance. They have certain natural advantages in competing for these opportunities and powerful incentives to do so. They have large numbers of permanent professional employees covering a narrower range of disciplines than universities, because the teaching mission of universities requires them to have relatively smaller numbers of faculty in any one area of specialized expertise. Thus the large laboratories are in a better position to focus high-level, experienced talent on a single well-identified problem, and, especially in the large DOD and DOE laboratories, they are accustomed to such mobilization of talent by virtue of their past public missions. They are more comfortable with tight time deadlines and progress milestones, even though some individual scientists may have enjoyed considerable freedom within their general domain of expertise.

However, university people are more accustomed to pursuing even applied problems in much greater depth than might be strictly justified as cost-effective in the context of a tightly defined mission objective. Through this pursuit they may make a larger net societal contribution in

46. David, Mowery, and Steinmueller (1994).

the long run, and they tend to remain in contact with a much wider range of scientific developments and be more flexible in changing their agendas in response to newly appearing technical opportunities.

A fine balance thus is involved from society's standpoint in getting the most out of the advantages of each type of institutional culture. Careful monitoring is required to prevent immediate urgencies and competitive pressures from weighting project and program selection in favor of supporting the labs because they fall under the auspices of federal agencies in a sense that university groups do not and should not be expected to, given their wider social mission and their multiple constituencies and sources of funding.[47]

The new political climate in Washington, however, further complicates this choice. The new Republican majority is much more hostile to using government to help industry, especially large industry, and thereby intervening in the domestic competitive balance. In general the Republicans appear more friendly to basic research, while lacking a clear understanding of what it is. A good deal of hostility is evident toward universities as institutions and what is seen as their loose governance. Pressures to balance the federal budget are likely to result in measures, such as caps on indirect cost reimbursement, federal support for graduate students, the downsizing of government health care systems, and requirements for cost sharing, that will erode the institutional capacity of universities to respond to national needs. In the future, the maintenance and enhancement of this capacity is likely to depend on the development of new types of industry associations to support academic research of a largely public good nature that will replace the type of research supported in both universities and national laboratories by government during the cold war. This is a type of infrastructural research on which much industrial innovation has implicitly depended (and to a growing degree) in the past, but which most industry does not know it needs or will need until the research disappears.[48]

What are the implications for university priorities of the increased importance of the knowledge synthesis and diffusion functions as compared with the previous emphasis on the creation of new knowledge?

A consensus is developing among science policy analysts that the mission of research universities should give higher weight to knowledge

47. For a more detailed discussion of the future role of universities in the U.S. research system, see Brooks (1993, pp. 202–34).

48. As an example of this type of thinking, see Romer (1993).

synthesis, repackaging for use, and dissemination of new knowledge. This implies everything from radical changes in instruction methods based on findings in cognitive science to the development of hardware and software as aids to knowledge access and retrieval and the training of future scientists and engineers in its use. However, the practical implications of this new priority for the organization and procedures of universities and for the development of intermediary institutions between universities and knowledge-intensive practitioners delivering services to the public outside universities have not been worked out, or even much discussed. This is a serious gap in current science and technology policy debates.

What will be or should be the impact of new governmental priorities of national economic performance and competitiveness on the participation of foreign nationals in government-sponsored (unclassified) research at universities and other research institutions, both governmental and non-governmental?

The productivity of the U.S. technical work force has become steadily more dependent on a continuously growing influx of scientists, engineers, and skilled practitioners originating from all parts of the world. Many come from abroad to complete their advanced education in the United States. Others are trained abroad and come to the United States because of the wide opportunities available for using their skills more creatively than is possible in their home countries, as well as better salaries and living conditions. This has been regarded as an almost unalloyed advantage for the United States in enhancing its capacity for technological innovation for economic growth and job creation. Nevertheless, many inconsistencies exist among the legal mandates of new government programs with respect to encouraging the attraction and participation of foreign nationals or foreign-owned corporations.

Furthermore, as the prosperity of the rest of the world has increased, a reverse brain drain has occurred of people who have received either advanced education or substantial hands-on technological experience—or both—in U.S. industry, making them an attractive human asset for acquisition by foreign competitors. The rise of technonationalism as a consequence of U.S. competitiveness problems threatens the open science culture of American universities. Debate and disagreement are heard as to net costs and benefits in the long run associated with the unique openness in human terms of the U.S. science and technology system, in the research universities, and, to some extent, throughout all the technical activities of American society.

The U.S. scientific community has a strong bias toward open science,

but, because of rising criticism from some quarters—whether justified or not—the technical community as a whole may no longer be able to treat the subject with benign neglect. It may have to be dealt with as an important policy issue, worthy of the development of a well-thought-out rationale for an explicit national policy even if it turns out to be the present one. My own bias is that U.S. openness in science is a long-term asset that should be preserved and enhanced but is already being eroded from within. Openness is no longer an automatic assumption even within parts of academia and will need to be debated frankly. The effect of the new postelection political climate is unclear.

What kinds of ground rules, criteria, and procedures should be developed for the extent and form of participation of government agencies at both the state and federal levels in international cooperative research and educational projects?

The United States is acquiring a reputation as an unreliable and unpredictable partner in international collaborative projects in science and technology. So far the focus of policy attention has been on so-called megaprojects, projects requiring costly one-of-a-kind facilities or complex and expensive logistic support (as in the Antarctic program, or certain oceanographic or atmospheric joint ventures). However, megaprojects are also becoming an important issue in the case of collaborative international research institutions, not restricted to fundamental science. They can be an important issue for institutions to which the United States contributes only a small proportion of the funds required to operate a jointly agreed program but insists on its own exclusive unilateral peer review of the program as a condition of its financial participation. A consistent policy and ground rules need to be formulated for multinational peer review, with the agreement of all the participating governments to abide by the results under all but the most exceptional circumstances, to avoid loss of one of the main advantages of collaborative effort. In the new political climate, international cooperative activities in science that depend on government funds are likely to fare badly, with international science being the first to feel the budgetary ax. Such activities are likely to have to depend on private funding sources from both the nonprofit and corporate sectors, and here the outlook is somewhat more favorable. However, whether this will come close to replacing the role the U.S. government has played in the past seems doubtful.

Given that the primary goal of public sector investment in technology creation or diffusion is net job creation in the long run (that is, more new and better jobs than those displaced through technology), what are the

implications for the selection of the most appropriate institutions and technologies for government support? By what processes will these goals be translated into selection criteria that command widespread support in the communities outside as well as inside the science and engineering communities?

In the mid-1990s, U.S. agencies lack a consistent, well-thought-out rationale for government support of projects that involve collaboration and cofunding with industry for the benefit of economic development and job creation. Sharing of costs between government and particular companies should imply a demonstrable public benefit that cannot be fully captured in the form of profits to the contributing companies. More rapid environmental improvement is one example of such a public benefit. But profit and new jobs are often interpreted as public benefits as well. Criteria need to be developed that can lead to reasonably objective discrimination between alternative public investments in different projects that include private companies as partners. The development of such a rationale seems far away, with the consequent hazard of steadily increasing politicization of the choices as programs grow. In the post-1994 political climate, the case for public benefit will have to be made much more cogently and concretely for private-public cooperative projects, if any of the present ones are to have any hope of survival.

How can criteria for public investment in new knowledge creation or diffusion be developed to provide assurance that public funds are not displacing potential private investment that is likely to be more cost-effective because of its greater responsiveness to markets? Put in another way, how can the net increase in effective national investment for a given public investment be estimated reliably?

Ideally, the benefit of public investment in technology has to be judged by the extent to which it can be demonstrated to increase the total net investment, making realistic allowances for displacement of private spending, including consideration of the relative efficiency in terms of knowledge diffusion and exploitation of public and private R&D.

What are the long-term implications of the priority now being given to the economic performance mission as a justification for public investment in science and technology and for the allocation of public resources among four classes of institutions: universities, government laboratories and FFRDCs, nongovernmental nonprofit organizations of various types, and private industry? To what extent, if any, does this new mission require that public funds be partially channeled through political subunits such as states, regional compacts among states, or even lower political levels?

During the entire period since the end of World War II the percentage allocation of R&D support among research universities, national laboratories (including both civil service laboratories and FFRDCs), and private industry has changed relatively little except for one sharp increase in university funding between the mid-1950s and mid-1960s. The question is whether this relative allocation is still appropriate to the new priority being accorded to the economic performance of the private sector. Recent federal initiatives and their projected expansion in the next five or six years suggest the possibility of some shift in favor of industry. However, this likely will chiefly compensate for the reduction of military development programs presently contracted out to industry. The new emphasis on Cooperative Research and Development Agreements (CRADAs) in major national laboratories also suggests the possibility of some shift of resources from universities to national labs, although this is by no means certain. Recent developments also indicate that states or regional state compacts may become more important players in determining the allocation of R&D resources associated with economic performance, analogous to the land grant system for agriculture, though not with the same degree of decentralization.

Does the scale and dispersion of the economic performance mission imply some degree of splitting up and possible dispersion of the large aggregations of technical capacity now represented by the major national laboratories?

This issue raises the question as to whether such large centralized organizations as national labs are still appropriate to the new economic performance mission, or whether the labs themselves may be split up into more or less independent self-governing units, while perhaps retaining their geographical proximity to take advantage of scale economies in support services. This seems more likely than actual geographical dispersion of national lab-type activity because modern communications facilitate close relations with clients and collaborators, while geographical shifts entail serious difficulties in the context of the regionally based U.S. political system. Parts of the national laboratory system likely may be candidates for privatization, for which the present CRADA arrangements may be a sort of halfway house.[49]

The breaking up and closure of government laboratories is much more imminent in the new political climate than before the 1994 election. The

49. For an explanation of Cooperative Research and Development Agreements (CRADAs) and other matters relating to national labs, see Branscomb (1993, pp. 103–34).

luxury of waiting to see how things work out and to optimize gradually may no longer be an option. The great technical capabilities that these laboratories represent are likely to be lost in the absence of a viable and well-rationalized plan for their restructuring in some new configuration.

What problems does the emerging prospect of a rapid downsizing of the U.S. government-funded R&D system raise for the institutions most directly involved with the training of scientists and engineers, and what near-term changes may be required in the nature of their training and career expectations?

Even if the funding for university research were not to shrink rapidly—an unlikely contingency—the effect on the educational mission of the universities would be far reaching because of the sudden lack of scientific and technical jobs outside of academia and the nature of the external job market into which the majority of advanced students go. However, the present situation is so uncertain that speculation is difficult. The United States could conceivably be facing a situation with respect to its highly trained personnel that would look for a few years not totally dissimilar to that now faced by scientists and engineers in the former Soviet Union. The private sector is unlikely to absorb these people except very slowly because most of what industry does requires a large downstream investment relative to R&D in contrast to activities in defense, space, health, and environmental management—the government functions toward which most present government-funded R&D is directed. The private sector probably would not soon be in a position to support this downstream investment on a self-sustaining basis even if it greatly expanded its R&D investments. A tremendous and rapid step-up would be required in the R&D-intensity of the U.S. economy analogous to what happened in the World War II economy, which, however, was financed by government. A look at the world's problems that need to be attacked could theoretically justify such a step-up, but it seems highly unlikely politically except over a long period of structural adjustment. Such a step-up did occur in the 1970s, in the period of the energy crisis, but in retrospect the R&D investment of that period appears to have been inefficient and wasteful.

Comment by E. J. Moniz

I am pleased to bring the perspective of a practicing scientist to this discussion of the social contract. My perspective is shaped by my expe-

rience as a physicist at the Massachusetts Institute of Technology, a university with a strong focus on science and technology and an especially strong tradition of industrial collaboration. In general I believe it is important for science policy analysts and science policymakers to maintain strong links to and a firm foundation in the research community.

What is the working university scientist's view of core responsibilities in the social contract? First and foremost, frontier research must be done, research that adds substantively to the knowledge resource base and occasionally breaks through to a significantly different understanding of the world. Teaching is another responsibility, principally using the apprenticeship mode for advanced training. University scientists should be engaged in service—to the government, to industry, to the public. Those core responsibilities carry on through periodic reexaminations of the contract.

The contract also is interpreted as requiring societal support for certain conditions that are critical to the pursuit of frontier basic research. One is a commitment to excellence, as judged through peer review. This commitment is not sustained automatically in a democratic society. Another condition, particularly important in a university setting, is freedom of inquiry, which entails pursuit of issues driven by the intellectual imperatives of a scientific discipline. Such research is clearly only part, but a very important part, of the overall research and development (R&D) portfolio. Both conditions are under considerable pressure. In addition, the basic disciplinary structure of academic science may be under pressure. This organization has traditionally offered an important framework for excellence while, contrary to some opinions, still providing opportunities for multidisciplinary work. I am unaware of alternate schemes of university organization that offer the same degree of quality control.

The growing tension between agenda-driven (strategic) and discipline-driven (curiosity-driven) research has a long history and is healthy if not elevated to a focal point of the national science policy debate. All the federally supported R&D is presumably viewed as supporting national goals, broadly defined. As set forth in the Clinton-Gore statement *Science in the National Interest*, these goals include leadership in basic science and education through forefront research, as well as the knowledge base for future technologies. However, even with a narrower definition of national goals, only about 10 percent of the federal basic research investment (that is, federal R&D, excluding applied research and development) is aimed at scientific knowledge with little bearing on federal missions (even there, the direct technology developments, such as accel-

erator technology in subatomic physics research, can be of very immediate consequence). And that research is generally an important part of the tightly interwoven scientific fabric.

The focus on the National Science Foundation (NSF) in the strategic research debate distorts this picture. The NSF-supported research programs are a critical part of the overall portfolio, particularly for university-based research. However, about three-quarters of the federal support for the physical sciences resides with mission agencies, particularly the Department of Defense, the Department of Energy, and the National Aeronautics and Space Administration (NASA). And the vast majority of support in the health-related and life science areas is explicitly mission-driven. Thus the entire cross-agency portfolio must be considered in reexamination of the social contract. That portfolio is already overwhelmingly strategic, in the sense that basic research support is seen from the agency perspective as relevant to a long-term mission just as the investigator may be motivated primarily by disciplinary considerations.

The mission agency support of basic research is important for reasons other than the obvious one of increasing opportunity. For the agency, the basic research support and strong university connection, with its foundation in demanding peer review, can help sustain quality control across many activities. For the university researchers, the advantage often exists of institution-building, which is less readily available in the individual investigator, who usually receives NSF support. That is, when supported by a mission agency, researchers are basically doing the agency's work; at the NSF researchers compete for funds to do their own work. I do not wish to push this point too far in terms of practice. Nevertheless, this distinction is reflected in the level of ongoing commitment to an institution.

This diversity of approaches, to be distinguished from simply a diversity of sources, is a significant strength of the system. Even within the NSF, a variety of funding mechanisms are available. For example, at MIT, the NSF-supported Center for Material Science and Engineering is one of the most successful multidisciplinary activities. The NSF supplies an umbrella grant through peer-reviewed competition under the ground rules that faculty from different departments join together for specific purposes. Many, such as one in funny fluids (liquid crystals, gels, and so on.), have produced first-class basic research (phase transition phenomena), several startup companies, and interesting applications (for example, work on eye cataracts as a phase transition). This diversity of research styles and the broad support of agency missions are the attraction

of the multiple-agency portfolio of basic research support. It enhances the connection of basic research with the national interest.

Although the continuing debate about strategic research would benefit from a broader perspective, other policy areas need more attention—for example, capital budgeting. The lack of originality associated with this suggestion does not necessarily diminish its importance or timeliness. The nonexpert is struck by the idea of capping discretionary spending without imposing discipline on the much larger entitlement budget. The labels "discretionary" and "entitlement" are themselves not helpful; although perhaps equally imprecise, labels such as "investment" and "operating" come closer to capturing the truth. Arbitrarily limiting one's investments for the future but not controlling operating costs seems backwards. I recognize the difficulty of defining investments and of sustaining discipline, but an intellectual response to the issue must be distinguished from a political one.

Specific development would surely be affected by more extensive use of capital budgeting. Considerable debate presumably would take place over what part of the R&D budget fits in the investment category. One area that is likely to become problematic and is clearly an investment is the development of large scientific facilities essential for frontier research in many disciplines. These long-term ventures could easily span a quarter century and would encompass R&D, design, construction, and operation. Front-loading much of the cost as an operating expense is prejudicial. The budget environment will make providing these essential capabilities increasingly difficult. The U.S. science leadership position will not be sustained without a national long-term planning and budgetary process that advances the development of major facilities along with other research and education support and taming of the budget deficit.

Another part of the social contract is concerned with the responsibilities of scientists in graduate training. Plenty of room exists for improvement outside of the primary task of guiding doctoral research. A certain priest-class mentality, while far from uniformly prevalent (and not uniformly deserved) and indeed diminishing, undoubtedly still serves many students poorly. The career outlook of many students is shaped in a way that narrows their future contributions to science and to society. This mentality can also lead to magnified attention to Ph.D. employment, when the issue is more one of expectations. For example, in the research area of nuclear physics, the employment pattern remains stable: basically full and useful employment, with comparable representation in the academic, national laboratory, and industrial (including medical applica-

tions) sectors. I am constantly surprised to find former students enthusiastically and very effectively pursuing problems far from their doctoral research. On just one day during a recent visit to Los Alamos, I came across a former nuclear experimental physics student who had just completed measurements over the Pacific Ocean on ocean-atmosphere couplings and a former quantum field theorist advancing quantum cryptography. These activities need to be celebrated as important and satisfying directions for students, and some concrete reforms need to be implemented, while preserving the depth of a frontier research experience as the defining element of doctoral work. For example, at the MIT physics department, the options for satisfying the Ph.D. breadth requirement are being enlarged, from a narrow set of courses in neighboring physics subdisciplines to a varied menu of other science and engineering courses, industrial internships, and multidisciplinary courses specifically developed in partnership with other departments. Progress is apparent, and its pace is affected by the broad policy discussions going on nationally. Much remains to be done; for example, teachers must be enfranchised as part of the research community.

The mode of support of graduate students also needs reexamination in the context of a broader human resources policy for science. A heavy reliance on research assistantships ties student support to the best research. In contrast, fellowships are tied to the best students, and traineeships, roughly speaking, are tied to the best programs. Traineeships can be tied much more closely to larger objectives in the research agenda and can be used more easily to evaluate institutional performance (in both research and education). The physical sciences could do with somewhat increased emphasis on traineeships.

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