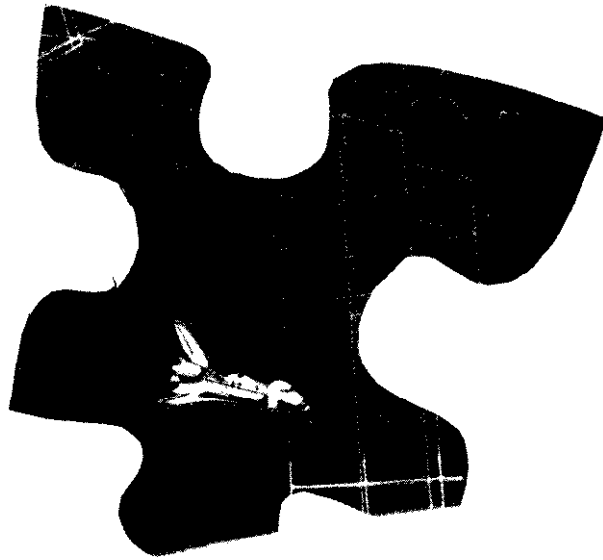


CHAPTER 5

Priority Setting in Science



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Priority Setting in Science

Even if we could double the science budget tomorrow, we would not escape the need to establish priorities. . . . At present we have no well-de fined process. . . for systematically evaluating the balance of the overall Federal investment in research and development and in the variety of fields that we try to serve”

Doug Walgren¹

Introduction

At every level of decisionmaking in the Federal research system, goals are outlined and translated into plans for their achievement. For the system to provide both continuity and flexibility in research funding, priorities are set, chiefly through the budget process. Both the executive and legislative branches have mechanisms to set priorities, many of which were detailed in the two previous chapters. However, broad priority setting is generally resisted by the recipients of Federal funding because it orders the importance of research investments, often in ways that groups within the scientific community do not support. This problem is especially perplexing, because there are few mechanisms and no tradition of ranking research topics across fields and subfields of inquiry.

Priority setting can help to allocate Federal resources both when they are plentiful, as they were in the 1960s, and when they are scarce, as is expected in the early 1990s. Governance requires that choices be made ultimately to increase the benefits and decrease the risks to the Nation. For example, decisionmakers in the Office of Management and Budget (OMB) routinely compare the projected costs, benefits, and risks of certain programs. The benefits of research increase technological capability, national security, health, economic activity, and educational resources. Setting priorities is a way the government achieves national goals.

In the grand scheme of things, research is one Federal concern among many, routinely costing less than 2 percent of the domestic and defense budgets. Research has traditionally been a favored part of the budget—only four budget areas have consistently received increases over the 1970s and 1980s: entitlements, defense, payments on the debt, and research.² Consider the President’s proposed fiscal year 1991 budget. The items in this \$1.4 trillion budget are organized under five themes. The first theme, “Investing in the Future,” features science and technology items most prominently among the 10 categories listed (see table 5-1). Five of these categories explicitly mention science or research goals.

What the Federal Government values more or less in research can be inferred in part from the Federal budget. The budget process compares the goals of the President, the Office of Science and Technology Policy (OSTP), the agencies, and Congress—not only what each seeks to achieve, but also how they plan to do so. However, no organization looks across the Federal research system to determine the framework for making choices.

From the discussion in chapter 3, one could conclude that OMB has been the surrogate for such an agent, with Congress then adding its own priorities through budget negotiations.³ The agencies spend these appropriated sums based on strategic plans that reflect their research missions, sorting long-range from short-range investments, weighing new initiatives against ‘out-year’ commitments (in multiyear

¹Doug Walgren, Chairman of the Subcommittee on Science, Research, and Technology, in U.S. Congress, House Committee on Science, Space, and Technology, *The Hearings Inadequacy, Direction, and Priorities for the American Science and Technology Effort*, 101st Cong., Feb. 28-March 1, 1989 (Washington DC: U.S. Government Printing Office, 1989), p. 1.

²‘Outlays by Category,’ *Government Executive*, vol. 22, September 1990, p. 44. Furthermore, within the category of “R&D,” research has seen much greater increases than development (which has decreased in constant dollars) since the late 1960s. See Lois Ember, “Bush’s Science Advisor Discusses Declining Value of R&D Dollars,” *Chemical & Engineering News*, vol. 68, No. 17, Apr. 23, 1990, pp. 16-17.

³For an overview, see Elizabeth Baldwin and Christopher T. Hill, “The Budget Process and Large-Scale Science Funding,” *CRS Review*, February 1988, pp. 13-16.

**Table 5-I—Summary of President Bush's \$1.4 Trillion Fiscal Year 1991 Budget,
Items Listed Under Theme 1: Investing in the Future**

Increasing saving, investment, and productivity
Expanding the human frontier
Space:
1. Infrastructure
2. Manned exploration (Space Station Freedom, Moon-Mars Mission)
3. Increasing scientific understanding (global change, developing commercial potential, other)
Biotechnology
Superconducting Super Collider
Enhancing research and development
1. Doubling the National Science Foundation budget
2. Global change
3. Agricultural research initiative
4. HIV/AIDS
5. R&D for advanced technology
6. Magnetic levitation transportation
7. Science and engineering education
8. Research and experimentation tax credit
9. R&D by transnational companies
Investing in human capital
Education:
1. Preparing children to learn (including Head Start)
2. Targeting resources for those most in need (including K-12, Educational Excellence Act, mathematics and science, historically Black colleges and universities)
3. Education research and statistics
Job training
Enhancing parental choice in child care
Ending the scourge of drugs
Protecting the environment (including global climate change research)
Improving the Nation's transportation infrastructure
Bringing hope to distressed communities
Preserving national security and advancing America's interests abroad (including the Department of Defense research and technology)
Preserving America's heritage

SOURCE: "President Bush's 1991 Budget Fact Sheet," Jan. 29, 1990.

awards), and **allocating** resources by program, project, and performer. Even this picture is too simple, however, since many decisions involve extensive debate within the government and the public, and developments within programs and the scientific community also influence the decisionmaking process.

Congress wishes—perhaps now more than ever—that the scientific community could offer priorities at a macro level for Federal funding. However, this community has long declined to engage in priority setting, claiming a lack of methods to compare and evaluate different fields of science and desiring to maintain high levels of funding for all fields, instead of risking cuts in any particular one. It has fallen

primarily to the Federal Government to set priorities, both among and within fields of science, and this situation will most likely continue through the 1990s.

In the scientific community, calls for priority setting are also often confused with calls to direct *all* research along specified lines. Even with greatly enhanced priority setting, one goal would certainly be the maintenance of funding for a diverse science research base. This priority has been preeminent since the Federal support of research began.⁴ Other priorities would include training for scientists and engineers, and supplying state-of-the-art equipment. At present, the means to meet these goals are a matter of continuous debate and policy revision.

⁴See U.S. Congress, House Committee on Science and Technology, Task Force on Science Policy, *A History of Science Policy in the United States, 1940-1985, 99th Cong.* (Washington, DC: U.S. Government Printing office, September 1986).

In an era of greater priority setting, the Federal Government would seek to target specific goals. For instance, the allocation of additional monies to the National Institutes of Health (NIH) for AIDS research, beginning in the late 1980s and continuing to the present day, has been a clear designation of a priority research area. Future decisions may center on ranking projects designated “big science,” since not all of them can be supported in the current fiscal climate. Similarly, fields that have received large increases in funding during the 1980s, such as the life sciences, may grow more slowly, as others are given precedence.

Although priority setting occurs throughout the Federal Government, it falls short in three ways. First, criteria used in selecting areas of research and megaprojects (e.g., the Superconducting Super Collider (SSC) and the Space Station) are not made explicit, and appear to vary widely. This is particularly a problem at the highest levels of priority setting, e.g., in the President’s budget and the congressional decision process. Second, there is currently no formal or explicit mechanism for evaluating the total research portfolio of the Federal Government in terms of progress toward national objectives. Third, the principal criteria for selection, “scientific merit” and “mission relevance,” are in practice coarse filters.

This chapter examines priority setting in the Federal research system. First, it describes the historical justification for priority setting and recent pressures stemming from budgetary constraints. Second, it reviews specific frameworks for setting priorities generated by various parts of the research system. (For a discussion of priority setting in other countries, see appendix D.) Most proposed frameworks include a distinction between “big” and “little” science, both as research strategies and as accounts with certain expectations. But definitions are murky. OTA thus discusses the criteria applied to justify investments in various categories and the decisions that generate agency research “portfolios.” Finally, the use of priority setting to clarify

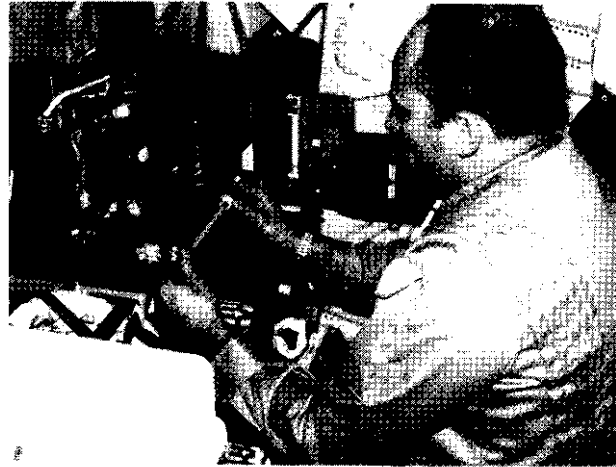


Photo credit: National Aeronautics and Space Administration

Astronaut prepares experiments, which separate cells according to their electric charge, on board the Earth-orbiting Columbia Space Shuttle. The difference between big and little science is murky, in part because the advent of new large equipment (such as the Shuttle) often allows new forms of what would be called “little science” when performed in other environments.

goals, strategies, and outcomes is analyzed as part of democratic decisionmaking.⁵

Historical Justification for Priority Setting

Investment in research is open-ended and uncertain in outcome. Thus, Federal decisionmakers bring different expectations and justifications to making choices in research. Recognizing this, Alvin Weinberg, former Director of Oak Ridge National Laboratory, proposed over a quarter-century ago a set of “criteria of scientific choice.”⁶ He wrote:

Society does not *a priori* owe the scientist, even the good scientist, support any more than it owes support to the artist or to the writer or to the musician. Science must seek its support from society on grounds other than the science is carried out competently and that it is ready for exploitation. . . . Thus, in seeking justification for the support of science, we are led inevitably to consider external criteria for the validity of science, those criteria external to science or to a given field of science.⁷

⁵The Federal budget process plus the annual cycle of authorization and appropriations hearings allow ample opportunity for iteration—to revisit projects, check their progress, revise cost and time estimates, and so on. But this is done piecemeal. Some mechanism viewing the entire research portfolio is needed, perhaps on a different cycle than the budget. A more “ideal” Federal research portfolio could be constructed iteratively—a process which could fortify the science base while allowing for the pursuit of some, but not all, new big science initiatives.

⁶Two papers on the topic, originally published in *Minerva*, are reprinted with additional discussion in A.M. Weinberg, *Rejections on Big Science* (Cambridge, MA: MIT Press, 1966).

⁷*Ibid.*, p. 72.

Weinberg's "external" criteria consist of *social merit* and *technological merit*. They declare the support of science as a priority to be judged against conscience investments and favor the "applied" end of the research continuum. These criteria conjure up the potential applications and social value of scientific research. Science *for* society is epitomized by such investment criteria.

Weinberg's "internal" criteria, on the other hand, are those embraced by research performers and, to a lesser extent, agency sponsors. For them *scientific merit* is the prime justification for Federal support, one that "... puts value on the progress of the scientific enterprise as a whole. Knowledge production is thus held to be a meritorious activity in its own right. ..."⁸ With no promised immediate benefit to society, the support of research has a more esoteric justification, such as the "ripeness" of a field for exploitation that will advance the state of theory or technique. The significance of this outcome may remain within a research community or be shared only by specialists in neighboring fields. For them, such developments become a priority. Making this intelligible and persuasive to those who control resources, e.g., within agencies or to one's congressional representative, however, is what may influence the policy process. A 1988 statement of the priorities issue suggests that the criteria have not changed much from Weinberg's original formulation (see box 5-A).

Historically, the notion of criteria, with scientific merit at its core, rearticulates the social contract that ties Federal research funding policies to investigators and research programs that bubble up to excite other specialists and agency sponsors. For Weinberg, "... the *purest* basic science [can] be viewed as an overhead charge on the society's entire scientific and technical enterprise." This conception of research as overhead on society's near-term

goals has been reasserted of late with changes in the Federal funding climate. Under the strain of demands on the Federal budget, the call for priority setting has grown louder.

The Funding Climate and Research Priorities

The 101st Congress engaged in what has been characterized as "... six of the most consequential and rancorous science and technology debates."¹⁰ Four of these six are unambiguously research related; they are presented by Senate and House votes in table 5-2: mathematics and science education, the SSC, environmental protection, and space/National Aeronautics and Space Administration (NASA). (Note the overlap between the items listed here and in the President's priorities.) The need for trained people, sophisticated instrumentation, the reduction of risk, and continued exploration of space reflect the relation of science and technology to the Nation's total market basket of investments.

Even though R&D still sit in the vulnerable corner of the budget that carries the label of "discretionary" spending, it's clear that science and technology no longer are viewed as flip-of-the-coin judgment calls. Rather, they are now seen as necessary and strategic obligations tied to national needs, and no matter how awful the budget deficit looks, R&D will get better relative consideration than anything else in the discretionary sector. ...¹¹

However, under tight fiscal conditions, no part of the budget may fare well. As Association of American Universities President Robert Rosenzweig states:

Another thing that concerns me. . . is the dynamic that seems to be set up by the next three to five years of budget problems. We're going to be fighting among ourselves a lot--universities and elements within universities. . . . The domestic discretionary [budget] pool . . . is not supposed to grow for the next five years, save for inflationary increases. But

⁸John Ziman, *An Introduction to Science Studies* (New York, NY: Cambridge University Press, 1984), p. 163.

Weinberg, op. cit., footnote 6, pp. 97-99. Also see Harvey Brooks, "Models for Science Planning," *Public Administration Review*, vol. 31, May/June 1971.

¹⁰Wade Roush, "Science and Technology in the 101st Congress," *Technology Review*, vol. 93, No. 8, November-December 1990, p. 59. These six differed slightly in the House and Senate, and two--having to do with the Clean Air Act and the B-2 Stealth Bomber--have arguably little science content.

¹¹William D. Carey, "R&D in the Federal Budget: 1976- 1990," *Science and Technology and the Changing World Order*, colloquium proceedings, Apr. 12-13, 1990, S.D. Sauer (ed.) (Washington DC: American Association for the Advancement of Science, 1990), p. 48.

Box 5-A—A Statement From the Scientific Community on the Evaluation of Competing Scientific Initiatives

The following criteria were proposed in 1988 for evaluating competing scientific initiatives. They are presented here (in abridged form) in the three categories developed by the authors.¹

Scientific Merit

1. Scientific objective and significance
Example: What are the key scientific issues addressed by the initiative?
2. Breadth of interest
Examples: Why is the initiative important or critical to the discipline proposing it? What impact will the science involved have on other disciplines?
3. Potential for new discoveries and understanding
Examples: Will the initiative provide powerful new techniques for probing nature? What advances beyond previous measurements can be expected with respect to accuracy, sensitivity, comprehensiveness, and spectral or dynamic range? In what ways will the initiative advance the understanding of widely occurring natural processes and stimulate modeling and theoretical description of these processes?
4. Uniqueness
Example: What are the special reasons for proposing this initiative? Could the desired knowledge be obtained in other ways? Is a special time schedule necessary for performing the initiative?

Social Benefits

1. Contribution to scientific awareness or improvement of the human condition
Examples: Are the goals of the initiative related to broader public objectives such as human welfare, economic growth, or national security? Will the results assist in planning for the future? What is the potential for stimulating technological developments that have application beyond this particular initiative? Will the initiative contribute to public understanding of the goals and accomplishments of science?
2. Contribution to international understanding
Example: Will the initiative contribute to international collaboration and understanding?
3. Contribution to national pride and prestige
Example: Will the initiative create public pride because of the magnitude of the challenge, the excitement of the endeavor, or the nature of the results?

Programmatic Concerns

1. Feasibility and readiness
Examples: Is the initiative technologically feasible? Are there adequate plans and facilities to receive, process, analyze, store, distribute, and use data at the expected rate of acquisition?
2. Scientific logistics and infrastructure
Examples: What are the long-term requirements for special facilities or field operations? What current and long-term infrastructure is required to support the initiative and the processing and analysis of data?
3. **Community commitment** and readiness
Example: In what ways will the scientific community participate in the operation of the initiative and the analysis of the results?
4. Institutional implications
Examples: In what ways will the initiative stimulate research and education? What opportunities and challenges will the initiative present for universities, Federal laboratories, and industrial contractors? What will be the impact of the initiative on federally sponsored science? Can some current activities be curtailed if the initiative is successful?
5. International involvement
Example: Are there commitments for programmatic support from other nations or international organizations?
6. Cost of the proposed initiative
Examples: What are the total costs, by year, to the Federal budget? What portion of the total costs will be borne by other nations?

¹Adapted from John A. Dutton and Lawson Crewe, "Setting Priorities Among Scientific Initiatives," *American Scientist*, vol. 76, November-December 1988, pp. 600-601.

Table 5-2—Favorable Senate and House Votes on Science Issues in the 101st Congress

Senate votes

Mathematics and science education programs: S.695, President Bush's "Excellence in Education Act," includes \$5 million for a national Science Scholars program. Passed 92-8 on Feb. 7, 1990; R 37-8, D 55-0.^a

Superconducting Super Collider authorization: H.R. 5019 appropriates \$20.8 billion for energy and water programs, including \$318 million for the accelerator. Passed by voice vote on Aug. 2, 1990.

Technology programs authorization: S. 1191 authorizes \$320 million in fiscal year 1990 funds for research on high-definition television and other new technologies through the Advanced Technology Program of the National Institute of Standards and Technology. Passed by voice vote on Oct. 26, 1989.

House votes

Superconducting Super Collider authorization: H.R. 4380 limits Federal spending on the advanced atom smasher to \$5 billion, with \$2.4 billion more to come from Texas and foreign sources. Passed 309-109 on May 2, 1990; R 115-57, D 194-52.

Mathematics and Science Education amendments: H.R.5115 authorizes \$1.1 billion in fiscal years 1991 to 1995 for congressional science scholarships and other education reforms. Passed 350-25 on July 20, 1990; R 123-25, D 227-0.

Technology programs authorization: H.R. 4329 funds the National Institute of Standards and Technology through 1992, including \$100 million in fiscal year 1991 and \$250 million in fiscal year 1992 for research on high-definition television and other new technologies under the Advanced Technology Program. Passed 327-93 on July 11, 1990; R 83-90, D 244-3.

National Aeronautics and Space Administration funding: H.R.5158 appropriates \$14.3 billion for NASA. Passed 355-48 on June 28, 1990; R 128-39, D 227-9.

KEY: R= Republicans; D= Democrats; NASA=National Aeronautics and Space Administration.

^aBoth the House and the Senate passed the Excellence in Mathematics, Science, and Engineering Act of 1990 (Public Law 101-589) in October 1990, and \$149 million was appropriated.

SOURCE: Based on Wade Roush, "Science and Technology in the 101st Congress," *Technology Review*, vol. 93, No. 8, November-December 1990, p. 65.

everybody is going to be out to get more money. They all feel that they deserve and need more money, and they're probably right.¹²

These commentators, speaking 2 years after National Academy of Sciences (NAS) President Frank Press warned of constrained research budgets as 'the dilemma of the golden age,'¹³ suggest some accommodation to this reality: while the Federal Government could invest more in science and technology, the scientific community could do a better job of sorting research opportunities by whatever criteria chosen to assist decisionmakers at all levels of the system.

Science Advisor Bromley and Former Science Advisor Press have stated criteria and categories of priority that they consider essential for science,

listed in table 5-3. (Projects are compared under each category to compete for monies allocated within that category.) Note the convergence between the Science Advisor's (OSTP/OMB's) and the NAS President's (and former Science Advisor's) formulations. Each emphasizes the separation of large projects requiring new infrastructure from "small science." Press distinguishes human resources from national crises and extraordinary scientific breakthroughs in his primary category. Bromley places national political exigencies above all else,¹⁴ whereas Press prefers to put these items into a "political category" of third priority. One effect of these rank orders is the seeming creation of separate accounts, i.e., that choices could be made within each category and then across categories.¹⁵ Of course, such choices are being made by various participants in the research

¹²Quoted in "A Good Budget for Science, But Troubles Lie Ahead," *science & Government Report*, vol. 20, No. 18, Nov. 15, 1990, pp. 1,4. In the President's proposed fiscal year 1992 budget, civilian R&D spending would rise 13 percent to \$76 billion, with basic science increasing 8 percent to \$13 billion. See William Booth, "President Puts Fiscal Faith in Science," *The Washington Post*, Feb. 13, 1991, p. A17. Also see Jeffrey Mervis, "Bush's Science Budget: Will It Hold?" *The Scientist*, vol. 5, No. 5, Mar. 4, 1991, pp. 1,6-7.

¹³Frank Press, "The Dilemma of the Golden Age," *Congressional Record*, May 26, 1988, pp. E1738-E1740. Press's categories and priorities are presented below.

¹⁴Bromley's statement was augmented in September 1990 by a brief Office of Science and Technology Policy document, "U.S. Technology Policy." The document seeks to bridge the roles of the private sector and the Federal Government in research and development. Justifications for the President's fiscal 1991 budget requests for "education and training" and "Federal R&D responsibilities" are presented by agency in addition to discussion of federally funded technology transfer and Federal-State activities. See Executive Office of the President, Office of Science and Technology Policy, "U.S. Technology Policy," unpublished document, Sept. 26, 1990.

¹⁵Note that scientific merit is assumed in both formulations and not explicitly stated as a funding criterion. The issue becomes one of first ranking science projects according to scientific merit and then assigning them to national goal categories, or alternatively starting from a national goal and organizing a research strategy to meet it.

Table 5-3—Two Statements on Research Priorities

Source	Criteria	Categories in rank order
Bromley	"... guiding principles on prioritizing the agency requests..."	1. National needs and international security concerns (global change, preeminence in space, defense technology base). 2. Support for basic research (particularly university-based, individual-investigator and small-group research-'small science'). 3. Funding for scientific infrastructure and facilities (SSC, Space Station, and"... in a more distilled sense..." Human Genome).
Press	"... appropriate for the unprecedented Federal deficit..." and "... to maintain American leadership in science and technology..."	1. Human resources, national crises (AIDS, space launch capacity), extraordinary scientific breakthroughs (high-temperature superconductivity). 2. Large projects (SSC, Human Genome). 3. Political category (DOD and national security; Space Station; regional economic development and employment; U.S. image enhancers like manned space flight; U.S. "competitiveness" enhancers like education, training, and civil sector R&D).

KEY: SSC=Superconducting Super Collider; DOD=U.S. Department of Defense.

SOURCES: D. Allan Bromley, "Keynote Address" *Science and Technology and the Changing World Order*, colloquium proceedings, Apr. 12-13, 1990, S. L. Sauer (ed.) (Washington, DC: American Association for the Advancement of Science, 1990), p. 11. Also see "Q&A With D. Allan Bromley, Bush's Science Advisor," *Science & Government Report*, vol. 20, June 1, 1990, p. 5; and Frank Press, "The Dilemma of the Golden Age," *Congressional Record*, May 26, 1988, pp. E1738-E1740.

system simultaneously. The congressional budget process may be the final arbiter, but even after Federal monies are obligated, choices at the agency and program levels occur.

In addition to supporting meritorious research, most Federal research agencies would embrace the following as relevant to their mission:¹⁶

- to provide fiscal support to the research system (both the infrastructure needed to conduct research and the research itself);
- to invest in human capital today (i.e., the research work force) and tomorrow (i.e., student apprentices);
- to sustain the performance sector of research (especially the research universities) and to build institutional capacity (especially as viewed by region or State); and
- as a factor in economic development and the application of research to solving local problems.

Clearly, not every program in every research agency can apply these as finding criteria without compromising any single one.

In response to a congressional request in 1988, NAS also devised a framework for thinking about Federal science and technology budget priorities. The result is presented in table 5-4. In this four-

category scheme, "agency budgets and missions" are viewed as separate from needs of the "science and technology (S&T) base," "national [political] objectives, and "major S&T initiatives." All are illustrated by NAS at the agency level, listing the following needs: educating science and engineering personnel; modernizing equipment and facilities; supporting a mix of basic and applied research; capitalizing on promising new research opportunities; promoting interactions between related fields of science and engineering research; distributing research support by geographic region and type of institution; maintaining a mix of research modes, e.g., individual investigators, large groups, centers, and university-industry partnerships; and balancing competitiveness and cooperation with research programs in other countries.¹⁷

If these items were interpreted as listed in *order of importance*, top to bottom, the projects funded by the research agencies (indeed, the proposals received) might look quite different from the research projects currently supported. Priorities can perturb the funding system; they can redefine the "haves" and 'have nets' (e.g., institutions, fields, investigators) by changing the value of certain criteria. For instance, some agency funding decisions signal that a premium has been placed on other needs (see box 5-B).

¹⁶OTA interviews at the Federal research agencies, spring-summer 1990.

¹⁷National Academy of Sciences, *Federal Science and Technology Budget Priorities: New Perspectives and Procedures*, a report in response to the Conference Report on the Concurrent Resolution on the Budget for Fiscal Year 1989 (H. ConRes. 268) (Washington, DC: National Academy Press, 1988), p. 10.

Table 5-4-Framework for Assessing Science and Technology Budgets (categories are not mutually exclusive)

Category	Definitions	Examples
Agency budgets and missions	Agency S&T activities viewed in terms of their contributions to individual agency goals and objectives	Nuclear alternative energy R&D in DOE Submarine acoustics in DOD Cell biology in HHS Influence on learning in ED Plant disease resistance in USDA Fundamental research in chemistry in NSF Standards development in NIST Aeronautical research in NASA
S&T base	Activities that provide the people knowledge, and infrastructure to carry out S&T Activities supported across many agencies and under the jurisdiction of several congressional committees	Basic and applied research programs in NSF, HHS, DOD, DOE, NASA, USDA, EPA, etc. Student fellowships in ED, NSF, HHS, DOD, DOE, NASA, etc. Equipment and instrumentation programs in HHS, DOE, NSF, USDA, NASA, DOD, etc. Facilities for research, animal care, and growing and using special materials supported by NSF, DOD, HHS, DOE, NASA, etc. K-12 materials development in NSF, ED, NASA, etc. Student internships in Federal laboratories in DOE, NIH, etc.
S&T applied to national objectives (Presidential and congressional priorities)	Stated priorities of the President and Congress with major S&T components Frequently supported by several agencies and within the purview of several congressional committees	Understanding and ameliorating global change in EPA, DOE, NSF, NASA, USDA, NOAA, etc. Industrial development in biotechnology, superconductivity, manufacturing technologies in HHS, DOD, Commerce, NASA, NSF, DOE, USDA, etc. Alternative sources of energy in DOE, NSF, DOD, USDA, etc. AIDS in HHS, ED, DOD, State Department, etc. Creation of nuclear defense (Strategic Defense Initiative in DOD) Increase capacity for exploration of space (Space Station in NASA)
Major S&T initiatives	Significant increase (and sometimes decreases) in budgets over several years Budgetary consequences across agencies Fail in one or more of above three categories	Superconducting Super Collider Mapping and sequencing the human genome Space Station

KEY: DOD=U.S. Department of Defense; DOE=U.S. Department of Energy; ED=U.S. Department of Education; EPA=U.S. Environmental Protection Agency; HHS=U.S. Department of Health and Human Services; NASA=National Aeronautics and Space Administration; NIH=National Institutes of Health; NIST=National Institute of Standards and Technology; NOAA=National Oceanic and Atmospheric Administration; NSF= National Science Foundation; R&D=research and development; S&T=science and technology; USDA=U.S. Department of Agriculture.

SOURCE: National Academy of Sciences, *Federal Science and Technology Budget Priorities: New Perspectives and Procedures* (Washington, DC: National Academy Press, 1988), table 1, p. 7.

Concern for the S&T base closely approximates the needs of research. In the words of the NAS report:

The S&T base is the bedrock of the Nation's ability to use science and technology in the national interest and. . . it requires continual replenishment. Continuity does not imply steady funding of the same activities and institutions through the same programs and agencies year after year. On the contrary, the enterprise ought to be highly dynamic. Policymakers must be able to respond flexibly to scientific breakthroughs that suddenly transform an area of research (e.g., high-temperature superconductivity), the invention of a powerful new instrument (e.g., gene-sequencing machine) or conceptions of new facilities that would aid research and training (e.g., supercomputer centers and networks),

unexpected shortages of science and engineering personnel, or changing institutional relationships (e.g., the emergence of university-industry research partnerships). And as if that were not a sufficient challenge, budget makers and analysts must be attuned to differences among a wide range of fields. Some changes affect many disciplines, others only a part of a single discipline.¹⁸

Frameworks such as OSTP's and NAS's help to demarcate the tradeoffs that could be made and assist decisionmakers to understand that priority setting is a dynamic process. Priorities change with goals. As Weinberg put it:

. . . we cannot evaluate a universe of scientific discourse by criteria that arise solely from within that universe. Rather, we find that to make a value

¹⁸Ibid., p. 5.

Box 5-B—Criteria for Awarding a Magnet Research Laboratory: NSF, Florida State, and MIT

In August 1990, the National Science Board (NSB) of the National Science Foundation (NSF), decided to award a \$60-million grant to Florida State University to establish a national laboratory for magnet research. Then-NSF Director Erich Bloch admitted that peer reviewers had found the proposal from the Massachusetts Institute of Technology (MIT), home of the Francis Bitter National Magnet Laboratory, “technically superlative,” but cited the greater “enthusiasm” of the Florida investigators, the State of Florida’s pledge to contribute \$58 million, and other factors in funding the Florida proposal.¹

The issues involved in the NSF decision are many. At one level, the award is evidence that scientific merit is not enough to guarantee success in competition for a facility where there can be only one winner.² NSF cited as decisive the superior “management plan” in the Florida proposal. Clearly, the message being sent—part of Bloch’s larger emphasis on centers and government-industry partnerships to enhance U.S. economic competitiveness—was the rules of the game are changing: criteria other than technical merit are weighed in determining qualification to manage and execute a multiyear research program requiring the expertise of investigators from various institutions.³

In the magnet laboratory competition, the commitment of MIT was found wanting. According to NSF Assistant Director David A. Sanchez: “. . . you need support from the institution, you need support from the State, and we did not see that . . .” from MIT.⁴ NSF concurred.

The MIT protest of Florida State’s selection was not limited to NSF’s decision to overrule its reviewers’ recommendations. MIT President Paul Gray appealed on several grounds. First, the delay caused by construction of the Florida State facility “. . . is hardly compatible with NSF’s interest in the competitive posture of the United States. Second, some fear that projects with significant State support, so-called leveraging of Federal funding, will put private universities at a disadvantage. Third, expertise in the Florida State physics department may be lacking.”⁵

Consider, too, the symbolism of the decision. As one columnist put it: “So maybe the mandarins from MIT got caught napping. Maybe. Or maybe not.”⁶ MIT epitomizes the *Northeast* science establishment.⁷ The Southeast is, in a sense, an underutilized region for research. Awards such as the magnet laboratory signify that, in specific cases, institutional collaborations can make a State or region competitive for Federal research funding.

Such awards build research capability almost from the ground up; they are a capital investment that diversifies research performers—with short- and long-term consequences for the research community and the Nation. Decisions such as this one also call for evaluation: what happens to magnet research while the Florida State facility is being constructed? Will the State of Florida deliver on its pledges? And is there any impact on the competitiveness of U.S. researchers in fields that use powerful magnets, such as superconductivity and magnetic-resonance imaging?

¹See @ Blumenstyk, ⁶stia= Agency Picks Florida State Over MIT as Site for \$60-Million Magnet-Study Lab,” *The Chronicle of Higher Education*, vol. 37, No. 1, Sept. 5, 1990, p. A21.

²The award of a 5-year, \$25 million earthquake project to a consortium centered at the State University of New York at Buffalo sent a similar signal to Caltech and a California consortium in 1987. It also led to a General Accounting Office (GAO) investigation of the National Science Foundation (NSF) review process that sanctioned the award. While it sustained the fairness of the NSF process, it did question its documentation procedures. See U.S. General Accounting Office, National Science Foundation: Problems Found in Decision Process for Awarding Earthquake Center, GAO/RCED-87-146 (Washington DC: June 1987).

³Florida State is to be joined by the University of Florida and Los Alamos National Laboratory in New Mexico in making the magnet laboratory a reality.

⁴In Blumenstyk, *op. cit.*, footnote 1, p. A22. National Science Foundation reviewers said the Massachusetts Institute of Technology’s “decaying plant” “would require substantial modernization. The institution will submit a proposal for further support until the Florida State laboratory begins operations in 1993.

⁵All of these plus criticism of the National Science Bored (ep~y~C&kof “work@ scientists” among its members) are cited by a trio of Princeton physicists in Philip W. Anderson et al., “NSF Magnet Lab,” letter, *The Scientist*, vol. 4, No. 23, Nov. 26, 1990, p. 14. The Florida State proposal included a pledge from the State “. . . to add 24 new faculty members and 10 laboratory experts and to provide 20 annual fellowships for visiting scientists from around the world.”

⁶David Warsh, “Will Florida Become a New Bastion of Industrial Science?” *The Washington Post*, Sept. 12, 1990, p. C3.

⁷The Massachusetts Institute of Technology consortium was to include Boston, Brandeis, Harvard, Northeastern, and Tufts universities,



Photo credit: National Aeronautics and Space Administration

Researcher studies magnetic liquids. In this example of little science, the research is supported by the National Aeronautics and Space Administration.

judgment, we must view the enterprise from a broader point of view than is afforded by the universe itself. . . . And so it is with the rest of science. The scientific merit of a field must be judged in large part by the contribution it makes, by the illumination it affords, and by the cohesion it produces in the neighboring fields.¹⁹

Leaders of the scientific community have subscribed to the need for something other than ad hoc policymaking for research funding. OTA next examines the problems inherent in two categories of this funding—the science base and science megaprojects.

The Science Base

Little science is the backbone of the scientific enterprise, and a diversity of research programs

abounds. For those who believe that scientific discoveries are unpredictable, supporting many creative researchers who contribute to S&T, or the science base, is prudent science policy. In the words of one geographer: “The continued survival of our intellectual free market is important to scientific progress.”²⁰ Not surprisingly, many investigators and their teams shudder at the thought of organizing Federal research funding around a principle other than scientific merit. They fear that setting priorities would change the criteria by which research funds are awarded.²¹ They would run the risk of losing what they consider their fair market share. Does priority setting necessarily curb the search for new knowledge, or just redirect it?

Consider the research portfolios of the Federal Government. As shown in figure 5-1, broad field funding, 1969 to 1990, has favored the life sciences, almost doubling in constant dollars during that period. Mathematics/computer, physical, and environmental sciences have also increased; engineering has remained stable in funding; and social sciences have decreased. In retrospect, should these be decried as less than rational choices? With a change in the Federal funding environment, should the ground rules for allocating resources among broad fields and performers also change? And what role can peer review play?

Peer Review and Priority Setting Across Broad Fields

Peer review is used in a variety of ways within the Federal agencies. As seen in chapter 4, only a few agencies, primarily the National Science Foundation (NSF) and NIH, employ peer review throughout their priority-setting and funding allocation processes. At NSF and NIH, peer review is considered to be:

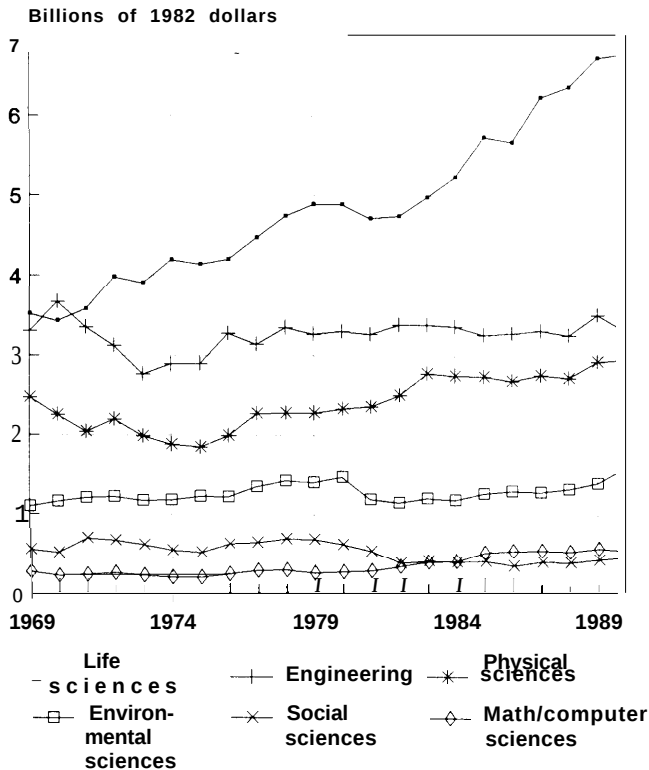
- effective for communicating expert opinion about what proposals definitely should and should not be funded (and the large gray area in between) within a narrow band of specialization corresponding to the scope of an agency program;

¹⁹Weinberg, *op. cit.*, footnote 6, p. 116.

²⁰G. Robert Brakenridge, “Evaluating Scientific Initiatives,” letter, *American Scientist*, vol. 77, No. 3, May-June 1990, p. 213.

²¹They also seem to confuse strategy (what to do) with tactics (how to do it). Criteria correspond to strategies, while project selection methods (e.g., peer review) represent tactics or ways to identify research that helps achieve state priorities.

Figure 5-1—Federally Funded Research by Broad Field: Fiscal Years 1969-90
(in billions of constant 1982 dollars)



NOTE: Research includes both basic and applied. Fields not included in this figure collectively accounted for \$1.1 billion (4.9 percent) of all federally funded research in 1990. Figures were converted to constant 1982 dollars using the GNP Implicit Price Deflator. 1990 figures are estimates.

SOURCES: National Science Foundation, *Federal Funds for Research and Development, Detailed Historical Tables: fiscal Years 1955-1990* (Washington, DC: 1990), table 25; and National Science Foundation, *Selected Data on Federal Funds for Research and Development: Fiscal Years 1989, 1990 and 1991* (Washington, DC: December 1990), table 1.

- efficient, in terms of the time, money, and energy involved in the process of deciding how resources should be allocated; and
- accountable, ensuring that the highest standards of rigor (valid and reliable measurement), safety (for animals, human subjects, and laboratory personnel), and freedom (e.g., to follow hunches, train students, and exchange data) in research are observed.

In sum, peer review is expected to be robust and responsive to changing agency and program needs.

Satisfying all of these criteria simultaneously, however, is difficult at best (see box 5-C) and, in practice, a compromise is struck between them.

Federal monies awarded to researchers for some expressed purpose other than or in addition to “scientific merit” are seen by many as inferior to monies for projects selected by peer review processes using scientific merit alone. Some are inclined to the view that there is something inherently wrong with such “political allocations.” The policy issue is whether peer review can simultaneously serve to discern scientific merit *and* help in project-based priority setting.

Reviewing for “truth,” as science policy statesman Harvey Brooks writes, differs from reviewing for “utility.” Peer scientists are not very helpful with the latter.²² In Weinberg’s terms, criteria of scientific merit clash with criteria of social or technological merit. Peer review as a tactic tends to break down when confronted with incommensurate information from competing disciplines, fields, or projects. As two commentators ask:

Should peer review operate only to evaluate merit or should it also help establish priorities? Can it or should it be effective in changing the direction of a program, in allocating resources among programs within agencies themselves? These questions are significant because they challenge the assumption that peer review is the best possible way to allocate resources in the best overall interests of both science and society.²³

Recognizing the limits of specialization, agencies maximize expertise in subject-focused programs. Specialists are quite well-suited to the task of making quality distinctions within disciplinary or problem-centered boundaries. But discriminations that must cross boundaries, no longer comparing like with like, are rarely ever accomplished by peer review, since reviewers in one field are very reluctant to judge the scientific or technical merits of information from other fields. There are no rules inside the scientific enterprise that suggest that one kind of information is superior to another. The

²²Harvey Brooks, “The Problem of Research Priorities,” *Daedalus*, vol. 107, No. 2, spring 1978, pp. 171-190.

²³Richard C. Atkinson and William A. Blanpied, “Peer Review and the Public Interest,” *Issues in Science & Technology*, vol. 2, summer 1985, p. 110.

Box 5-C—Peer Review in Changing Environments: Remarks at a Roundtable Discussion

In June 1990, the Forum on Research Management (FORM), consisting in equal parts of program officers from the behavioral science divisions of various Federal agencies and of senior researched and research managers from academia and the private sector, met to discuss peer review.¹ Two dozen FORM members discussed some of the pros and cons of peer review in an era of fiscal austerity. Their positions as agency administrators faced with allocation decisions, as lobbyists surveying the funding scene, and as researchers competing for scarce program dollars give them an acute sensitivity to proposal review and the environments in which it is carried out. The remarks are as verbatim as the edited transcription allowed. Each bulleted item represents a different speaker.

- There is a connection. . . between tight funding and peer review. As money gets tighter, peer reviewers become more conservative, less prone to take risks.
- What they [peer reviewers] are doing is giving higher and higher ratings, which in effect increases the noise in the system. So the peer **review system** is calling more proposals “excellent” and “outstanding,” and the consequence is that it is very difficult for program managers to make evaluations. What results is a beauty contest or just chance.
- Has the science changed? Has the quality of the proposals changed? I think the answer to both questions is yes. . . . Peer reviewers used to be tightly knit groups examining proposals from people they knew extremely well—it was a very closed society. Now it is a much more complicated task.
- Would it really be valuable to have a peer review system and an amount of money where everything was funded? I suspect it may lead to very bad science.
- There are two things going on in peer review---one is selection, which is important, but the other is education (of the proposer and reviewer). I think the latter function sometimes gets lost. Unfortunately, crushing workloads are reducing the educational function of peer review.
- When I serve on a [National Institutes of Health] study section, I find it extremely disconcerting and distracting to be told by program people about what percentage of the applications are likely to be funded. It distorts my entire approach, as well as that of my colleagues. For instance, if we’re told only 10 percent are likely to be funded, we start playing with the ratings to ensure certain results.
- Study sections are not supposed to be making funding decisions. They are to make scientific recommendations. There should be recognition that there are two discrete sets of staff used in NIH peer review. . . . Priority scores do not determine funding. That’s what advisory councils and institute directors are for.
- People are increasingly reluctant to get involved [in peer review]. . . . I wonder if we are losing certain types of reviewers from the process--not just to get women and minorities on the panels---with increasing demands on time.

These observations illustrate the challenges posed by competition and resource scarcity. Other challenges include the consequences of age and prestige on the allocation of Federal funds, the fate of proposals that cross disciplines and fall between agency programs, and the psychology of collective decisionmaking.² Debate on the burdens absorbed by Federal peer review systems is healthy if it informs the practices of agencies, investigators, and reviewers.

¹The Forum on Research Management was created in 1982 as a working group of the nonprofit Federation of Behavioral, Psychological, and Cognitive Sciences. Most of the attendees at the meeting were from the National Science Foundation, the National Institutes of Health, and a few professional associations headquartered in Washington, DC. The excerpts below are based on a transcript of the meeting supplied by David Johnson, executive director of the Federation.

²Some of three have been addressed empirically. See the special issue, “Peer Review and Public Policy,” *Science, Technology, & Human Values*, vol. 10, No. 3, summer 1985, pp. 3-86.



Photo credit: U.S. Department of Agriculture

This type of flea beetle is one of several studied by the Department of Agriculture to combat weeds. Research on animals and weeds is very different from research on space exploration, and expert judgments require specialized knowledge.

existence of such rules would imply that information from different fields could be made commensurable.²⁴

Peer review thus cannot help to set priorities beyond the limits imposed by agency organization. Whereas priorities and resource allocations for megaprojects are usually set by a tacit bargaining and lobbying process, the science base is governed by another dynamic altogether. As agencies evaluate their research needs and modify the emphases of their programs, research performers are intimately involved. But seldom does a research community

coalesce around a single agenda (for an exception, see box 5-D).

The Dilemma of Agency Priority Setting

Universities or States can be analyzed as aggregate categories that receive Federal research monies, and agencies as the source of those sponsored funds. But the actual funding decisions are made in different agency programs and the research performance occurs in laboratories and departments.²⁵ Decisions are thus made at several levels. Priorities that originate outside the agencies as “national goals” do not simply trickle down; they are adapted to what may be called an agency research portfolio, which in turn is comprised of various program portfolios (“funding strategies”). Within these organizational niches, priorities are set all the time. Thus, agencies may have the discretion to pursue certain national needs by applying a different or reordered set of criteria to the selection of research performers.

Because disciplines tend to overlap agencies, priorities in physics, for example, can be set within an agency, but not readily across agencies. There is simply no routine mechanism for doing so. Physics research is distributed across three mission agencies plus NSF. While high-energy physics is supported primarily by the Department of Energy (DOE) and astrophysics by NASA, theoretical physics “belongs” to no single agency.²⁶ This is even more dramatically apparent in the case of neuroscience. Congress and the President declared the 1990s the “Decade of the Brain.”²⁷ As seen in figure 5-2, the Federal Government supports neuroscience research in 6 institutes of NIH; in 3 within the Alcohol, Drug Abuse, and Mental Health Administration; and in 10 other agencies, with the National Institute of Neurological Disorders and Stroke and the National Institute of Mental Health leading the way. Unless a “lead” agency is recognized by all participants (as in computer science, see box 5-E) or an OSTP Federal Coordinating Council for Science, Engi-

²⁴That is, although some agencies use peer panels that rate multiple proposals and make direct comparisons of proposed work in their field, they do not compare their findings with those of panels in other fields, since between-field information is held to be incommensurable. Instead, they judge the technical merit of a research design, the competence of the investigators, and the institutional infrastructure available for executing the proposed design. As Harvey Brooks points out, who is the best judge of social merit? There are no experts on social merit, which has to be a collective decision involving several different kinds of expertise as well as generalists' political judgments. Personal communication, February 1991.

²⁵For example, see National Science Foundation, “Planning and Priority-Setting in the National Science Foundation,” a report to the Committee on Science, Space, and Technology of the U.S. House of Representatives, Feb. 28, 1990.

²⁶See, for example, Sebastian Doniach, “Condensed Matter Theory’s Fragile Funding,” letter, *Physics Today*, November 1990, pp. 13, 117.

²⁷Elizabeth Pennisi and Diana Morgan, “‘Brain Decade’ Neuroscientist Court Support,” *The Scientist*, vol. 4, No. 21, Oct. 29, 1990, pp. 1, 8.

Box 5-D—Priority Setting by the Ecological Research Community

In fall 1990, the Ecological Society of America proposed the Sustainable Biosphere Initiative (SBI), a research initiative that focuses on the necessary role of ecological science in the wise management of Earth's resources and the maintenance of Earth's life support systems.¹ The process of developing the research agenda affirms that a community can set priorities.² The document was intended as a call-to-arms for all ecologists. It was also to serve as a means of communication with individuals in other disciplines with whom ecologists must join forces. Many of the environmental problems that challenge human society are fundamentally ecological in nature.

In response to national and international needs, the SBI represents a framework for the acquisition, dissemination, and utilization of ecological knowledge in support of efforts to ensure the sustainability of the biosphere. The SBI calls for: 1) basic research for the acquisition of ecological knowledge, 2) communication of that knowledge to citizens, and 3) incorporation of that knowledge into policy and management decisions.

Research Priorities

The criteria used to evaluate research priorities were: 1) the potential to contribute to fundamental ecological knowledge, and 2) the potential to respond to major human concerns about the sustainability of the biosphere. Based on these criteria, the SBI proposes three research priorities:

1. *global change*, including the ecological causes and consequences of changes in climate; in atmospheric, soil, and water chemistry (including pollutants); and in land- and water-use patterns;
2. *biological diversity*, including natural and anthropogenic changes in patterns of genetic, species, and habitat diversity; ecological determinants and consequences of diversity; the conservation of rare and declining species; and the effects of global and regional change on biological diversity; and
3. *sustainable ecological systems*, including the definition and detection of stress in natural and managed ecological systems; the restoration of damaged systems; the management of sustainable ecological systems; the role of pests and pathogens; the transmission of disease among humans; and the interface between ecological processes and human social systems.

Existing national and international initiatives address parts of the first two priorities. Success of these programs will require increased emphasis on key ecological topics. The SBI proposes three research recommendations:

1. Greater attention should be devoted to examining the ways that ecological complexity controls global processes.
2. New research efforts should address both the importance of biological diversity in controlling ecological processes and the role that ecological processes play in shaping patterns of diversity at different scales of time and space.
3. A major new integrated program of research on the sustainability of ecological systems should be established. This program would focus on understanding the underlying ecological processes in natural and human-dominated ecosystems in order to prescribe restoration and management strategies that would enhance the sustainability of the Earth's ecological systems.

Implementation

Successful implementation of the SBI will require new interdisciplinary relationships that link ecologists with the broad scientific community, with mass media and educational organizations, and with policy makers and resource managers in all sectors of society.

In sum, while the goals and action items of the Sustainable Biosphere Initiative may not seem revolutionary, few ecologists would have accepted them even a decade ago. But times have changed and so has the science. The public is more aware of environmental issues than ever before, and opportunities for ecologists have never been greater.³

Such statements are rare. When they do appear, they can supply to policymakers an unusual tool for judging a hierarchy of research emphases and perhaps channeling resources to agencies and programs accordingly.

¹The following is based on Jane Lubchenco et al., Ecological Society of America, "The Sustainable Biosphere Initiative: An Biological Research Agenda," draft document, Oct. 30, 1990.

²For details on the fragile process by which the Society's 2,000 members and its leaders reached consensus, see Elizabeth Pennisi, "Ecology Society Reaches Rare Consensus on Research Agenda," *The Scientist*, vol. 4, No. 17, Sept. 3, 1990, pp. 3, 9, 20.

³Ibid., p. 3.

⁴To take another example, the astronomy community, working through the National Academy of Sciences, has issued four decadal surveys of the field. For the latest, see National Academy of Sciences, *A Decade of Discovery in Astronomy and Astrophysics* (Washington, DC: National Academy Press, 1991); and the statement of the study committee chairman, John N. Bahcall, "Prioritizing Scientific Initiatives," *Science*, vol. 251, Mar. 22, 1991, pp. 1412-1413.

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Box 5-E—Federal Investment in Computer Science

Since 1976, the Federal Government has had a stellar record of support for academic computer science. Funding has grown faster than for any other scientific discipline in the United States. However, support for computer science *basic* research has declined. As a consequence, questions are being raised about the amount of Federal support for computer science and the manner in which it is being distributed. A forthcoming Association for Computing Machinery (ACM) report on a 3-year study, *A Field in Transition: Current Trends and Issues in the Funding of Academic Computer Science Research*, can be viewed as a fine example of how the Federal Government invests in academic science.¹

The Federal Government has been instrumental in the computer's rise to strategic importance, starting with the first electronic digital computer, ENIAC, built under Army contract during World War II. The computer might be called an "enabling technology," a tool for advancing research across the spectrum of disciplines. But the Federal agencies' long-term funding of computer science and engineering research, particularly in the universities, has been a primary factor in the emergence and maturation of computer science as a distinctive discipline as well. Today, there is a call for new initiatives in high-performance computing to enhance the Nation's economic and scientific capabilities.²

Federal Funding of Computer Research

Between fiscal years 1976 and 1989, Federal obligations for computer science research rose from about \$89 million to \$487 million. This is equivalent, in 1990 constant dollar terms, to an annual (compound) rate of growth of 8 percent, or a total gain of 170 percent. About 85 percent of this increase occurred after 1980,

Historically, the Department of Defense (DOD) has provided about two-thirds of the Federal funds for computer science research, and accounted for over 60 percent of the increase in total funds since fiscal year 1976.³ While the National Science Foundation (NSF) is considered the second most important agency in computer science, in funding it has jockeyed for second place with the National Aeronautics and Space Administration (NASA) since the 1980s. Except for its work on the ILLIAC IV supercomputer, NASA's computer science funding was minimal

¹See Joel S. Yudken and Barbara Simons, "A report Summary--Final Report of the Project on Funding Policy in Computer Science," unpublished document Oct. 15, 1989. Unless otherwise indicated, the data presented below are drawn from the draft report summary, as updated by Joel Yudken Rutgers University, personal communication, February 1991.

²National Research Council, Computer Science and Technology Board, *The National Challenge in Computer Science and Technology* (Washington, DC: 1988), p. 30.

³John R. II. Clement, "Computer Science and Engineering Support in the FY 1988 Budget," AAAS Report XII, Research & Development FY 1988, AAAS Committee on Science, Engineering and Public Policy, Intersociety working Group (ed.) (Washington, DC: American Association for the Advancement of Science, 1987), pp. 251-261; and John R. II. Clement and Dianne Edgar, "Computer Science and Engineering Support in the FY 1989 Budget," AAAS Report XIII, Research & Development FY 1989, AAAS Committee on Science, Engineering and Public Policy, Intersociety Working Group (d.) (Washington, DC: American Association for the Advancement of Science, 1988), pp. 260-271.

success. But the metaphor breaks down here because, while success in stock market investments can be gauged by money earned, nothing as tangible results from research—at least not in the short run.²⁹

A program "purchases" a portfolio of research projects in a field. The selection of projects for inclusion in this portfolio has been determined by their predicted or estimated quality as seen by contemporary research performers (reviewers) or by knowledgeable research managers (with or without the aid of reviewers). Reviewers usually make judgments about the quality of a project without any

direct comparisons of the alternatives facing the investor. Priority setting forces such comparisons. Rather than choosing projects on a one-by-one basis up to the point of resource exhaustion, they could be recommended with reference to their incremental value, i.e., as projects that concentrate or diversify strength in the portfolio. Managers, on the other hand, compare projects with reference to the objectives of the entire program portfolio.

At least for basic research, researchers, reviewers, and program managers are supposed to adjust their activities so quickly that judgments about the quality

²⁹See Harvey Averch, "New Foundations for Science and Technology Policy Analysis," paper presented at the Conference on The Mutual Relevance of Science Studies and Science Policy, May 12, 1989, p. 7.

until 1982, when its funding jumped up substantially. NSF supports mostly basic research not tied to missions or applications in a full range of computer science and engineering subdiscipline, including theory, software systems and engineering, artificial intelligence and robotics, and advanced computer architecture. The Department of Energy (DOE) involvement in computers dates back to ENIAC in 1945, which was used for calculations for nuclear bomb research at the Los Alamos National Laboratory. DOE (and its predecessor agencies, the Atomic Energy Commission and the Energy Research and Development Administration) has been a major force in the development of high-performance scientific supercomputers ever since.⁴

Federal funding for *academic* computer science research rose dramatically between 1976 and 1989, from over \$27 million to **\$235 million** (current dollars), or **320** percent in real terms. DOD, NSF, NASA, and DOE account for virtually all Federal funding of academic research in computer science. (The National Institutes of Health and the National Institute of Standards and Technology both allocate a small number of extramural contracts and grants to universities and colleges.)

DOD has historically been the largest funder of academic computer science and its role increased substantially since 1976. DOD's share of Federal funding for academic computer science rose from 45 percent to 62 percent in fiscal years 1976 to 1989, accounting for over two-thirds of the total increase in this funding during this period. Although NSF funding for academic computer science increased from roughly \$14 million to \$64 million (current) between fiscal years 1976 and 1989—a real growth of 126 percent—its share of total Federal support for academic computer science declined from 51 percent to 27 percent.

Policy Initiatives in Computer Science Research

Policy initiatives from the Federal Coordinating Council on Science, Engineering, and Technology (FCCSET) and the Computer Science and Technology Board of the National Research Council call for substantial funding increases in high-performance computing. The FCCSET proposal has already led to a multiagency request for a \$149 million funding augmentation (in what is now called the High Performance Computing and Communications Program), and to new joint Defense Advanced Research Projects Agency-NSF projects.⁵ The question of balance and priorities—the shape of the Federal research portfolio for computer science—is likely to persist well into the 1990s.⁶

⁴Kenneth Flamm, *Targeting the Computer* (Washington DC: The Brookings Institution, 1987), pp. 78-85.

⁵National Science Foundation Committee on Physical, Mathematical, and Engineering Sciences, *Grand Challenges: High Performance Computing and Communications* (Washington, DC: February 1991); Executive Office of the President, Office of Science and Technology Policy, "A Research and Development Strategy for High Performance Computing," unpublished document, Nov. 20, 1987; and National Science Foundation, "Crosswalk of NSF Research Related to the Department of Commerce Emerging Technologies List and the Department of Defense Critical Technologies List" in "Background Material for Long-Range Planning: 1992- 1996," prepared for a meeting of the National Science Board, June 14-15, 1990, pp. E-1 to E-6.

⁶For a "call to action" to computing researchers, see Terry M. Walker, "Influencing Federal Support for Computing Research," *Computing Research News*, vol. 2, July 1990, pp. 1, 10-11.

of any isolated single project remain congruent with developments at the frontiers of knowledge. In practice, the agency investor has no way of knowing whether this "invisible hand" is efficient, rapid, and has good discriminating power. So portfolio evaluations could be used to set relative investment priorities since they provide a check on performance at a useful level of budgetary aggregation. But this would require some modification of the criteria for project selection. Reviewers would no longer be ranking proposals by scientific merit alone, but with respect to standards about which they as experts have no special competence, i.e., issues of social merit.

The burden for priorities, then, rests not with those who give advice, but with those who receive and sort it along with other program and agency objectives. To take an example, for the period 1987 to 1991 at NSF, the increase in appropriations for "research and related activities" directorates (R&RA) was 39 percent to \$1.95 billion (in current dollars). This compares to a 153-percent increase in "science and engineering education" to \$251 million and a 49-percent increase for the U.S. Antarctic Program to \$175 million. Looked at thematically, 80 percent of the requested fiscal year 1991 NSF budget was for research and facilities, and 20 percent for education and human resources (a virtual doubling

Table 5-5—Inhouse Evaluation for the NSF Strategic Plan: Research Advances and Opportunities Lost or Postponed, Research and Related Directorates, Fiscal Years 1987-90

Directorate	Percent change in funding (current dollars)	Research advances	Opportunities lost/postponed
All Research and Related Directorates	39.0%	- Research initiatives to enhance economic competitiveness in biotechnology, global change, manufacturing, materials, supercomputing/networking, superconductivity. 10 new ERCs, 11 new STCs. - Programs on women/minorities/disabled and undergraduate research and teaching expanded. - Number of proposals up 11.2 percent; number of awards up 4.6 percent.	- Decline of 6.1 percent in proposal success rates and in average annualized award amounts in 5 of 6 directorates (1987-89). 3 ERCs and 5 materials research labs terminated. - Other STCs deferred.
Biological, Behavioral, and Social Sciences	26.3	5 new centers (3 in biotechnology, 1 in plant science-cooperatively with DOE and USDA, and 1 in geography). - Other initiatives in neurobiology, human dimensions in global environment change. - Equipment and instrumentation increases.	- Pursued all proposed, but at reduced levels. 3-percent decline in proposal success rates.
Computer and Information Science and Engineering	65.5	- NSFNET expansion. - New joint initiative with DARPA in parallel processing. 4 supercomputer centers renewed. - Infrastructure activities in minority institutions.	27-percent decline in success rates. - Fewer grants to groups than planned. - Software engineering initiative delayed. 1 supercomputer center phased out.
Engineering	39.5	7 group research grants for Strategic Manufacturing Initiative. - New initiatives in optical communications, nondestructive evaluation, and management of technology.	- Number of proposals and awards down slightly. - Materials synthesis and processing initiative (with MPS) delayed.
Geosciences	34.8	- Research on Loma Prieta Earthquake. - Initiated active Systems Service.	- Success rates down 3.2 percent. - Canceled some atmospheric science filed programs. - New initiative in mesoscale meteorology deferred.
Mathematical and Physical Sciences	34.1	- Major research equipment subactivity for large research equipment construction projects. - Augmented support for new investigators.	- Success rates down 13.9 percent. - Illinois Macrotron construction canceled. - Material synthesis and processing initiative postponed.
Scientific, Technological, and International Affairs	69.0	- Growth of EPSCoR. - Implementation of Scientific and Technical Personnel Data System. 6 Minority Research Centers of Excellence initiated.	- Success rate down 9.2 percent. - Undergraduate Education Data System in SRS delayed.

KEY: DARPA=Defense Advanced Research Projects Agency; DOE=U.S. Department of Energy; EPSCoR=Experimental Program to Stimulate Competitive Research; ERC=Engineering Research Centers; MPS=Mathematical and Physical Sciences; NSFNET=National Science Foundation electronic network; SRS=Science Resources Studies; STC=Science and Technology Centers; USDA=U.S. Department of Agriculture.

SOURCE: National Science Foundation, "Background Material for Long-Range Planning: 1992-1996," NSB 90-81, prepared for a meeting of the National Science Board, June 14-15, 1990, pp. C-3 to C-17.

from its share in fiscal year 1987). This reflects the congressionally mandated priority of science education at NSF.³⁰

Table 5-5 highlights research advances in its R&RA directorates since 1987, as well as research

opportunities seen as lost or postponed. This inhouse evaluation was provided to the National Science Board to assist in its long-term planning. It could also serve as a tool for organization and reorganization (see box 5-F), and as a priority scorecard for the mostly little science that NSF supports.³¹

³⁰These percentages and amounts are based on requests in the fiscal year 1991 budget. Still, they approximate how the research directorates have fared relative to other activities at the National Science Foundation. See National Science Foundation "Background Material for Long-Range Planning: 1992-1996," NSB 90-81, prepared for a meeting of the National Science Board, June 14-15, 1990, p. C-3.

³¹The Antarctic Program is the chief exception, though the National Science Foundation also funds research and development centers such as the National Center for Atmospheric Research, the Kitt Peak and Green Bank telescopes, and five National Supercomputer Centers.

Box 5-F—Behavioral and Social Sciences: Organization and Federal Funding

In the concluding chapter of a National Research Council (NRC) committee report on achievements and opportunities in the behavioral and social sciences, titled "Raising the Scientific Yield," a prescription is offered for ". . . new investments and modifications in research infrastructures that are needed for further progress." "The program of prescribed investments total \$240 million annually in 1987, a year in which Federal expenditures on behavioral and social sciences research reached the \$780 million mark." The research frontiers singled out by the NRC committee for investment include ". . . new inquiries into the connections among behavior, mind, and brain, . . ." ". . . research on the mechanisms of choice and allocation, . . ." ". . . comparative and historical (including prehistorical) study of the institutional and cultural origins of entire societies, . . ." and methodological advances in ". . . data collection, representation, and analysis." But is the level of Federal investment in a broad field of science indicative of its potential contributions?

The behavioral and social sciences tend to get less visibility than other sciences at the Federal agencies, especially the National Science Foundation (NSF) and the Alcohol, Drug Abuse, and Mental Health Administration (ADAMHA) of the Department of Health and Human Services, which are the primary providers of basic research funding. This dilemma was addressed at a Senate hearing in 1989 by the economist-psychologist and Nobel laureate Herbert Simon.

It is misleading to talk about "hard" and "soft" sciences. In the physical sciences, classical mechanics is hard, but meteorology (e.g., the greenhouse effect) and the theory of high-temperature superconductivity or low-temperature fusion can be (as recent news stories tell us) exceedingly soft. Similarly, in the social sciences, knowledge about the operation of competitive markets or the capacity of human short-term memory is quite hard; but knowledge about how businessmen and consumers form expectations about the future, or about motivations surrounding drug usage can be quite soft.⁴

To study scientifically what makes us human is as *daunting* a task as to discover the fundamental forces of the universe or to understand how normal cells become factories of disease.⁵ The problem is the priority of funding social research, and opinions may differ on how to institutionalize a Federal commitment to behavioral and social science research.

An Organizational Solution?

In August 1990, Reps. Walgren and Brown introduced H.R. 5543, The Behavioral and Social Science Directorate Act of 1990. This was proposed because, according to Walgren: "NSF's enthusiasm for the behavioral and social sciences is at best lukewarm . . . and the cause is largely structural. Since its creation, this Biological, Behavioral, and Social Science [BBS] Directorate has been headed by a biologist." Brown added that: "NSF as a whole has enjoyed a relatively large increase in funding over the past decade. . . . However, rather than sharing in the Foundation's good fortune, these areas of science have been languishing." "The current BBS budget totals \$293 million, including \$48 million for 'behavioral and neural sciences' and \$33 million for 'social and economic sciences'.

While the concept of a separate directorate has been around for at least a decade--the time of Reagan-era cuts--at the 1990 National Behavioral Science Summit, held under the auspices of the American Psychological Society, 65 psychological and behavioral science organizations endorsed the idea as a solution to needed visibility

¹Dean R. Gerstein et al. (eds.), *The Behavioral and Social Sciences: Achievements and Opportunities* (Washington, DC: National Academy Press, 1988), p. 239.

²*Ibid.*, p. 249.

³*Ibid.*, pp. 239-244.

⁴Herbert Simon, testimony, in U.S. Congress, Senate Committee on Commerce, Science, and Transportation, Subcommittee on Science, Technology, and Space, *National Science and Technology Policy*, Sept. 28-29, 1989, 101st Cong. (Washington, DC: U.S. Government Printing Office, 1989), pp. 264-269.

⁵This was formally recognized when the organic act of the National Science Foundation was amended in 1968, placing within its legal mandate. . . . a formal responsibility to look after the health of basic research in the social and behavioral sciences." See Roberta Balstad Miller, National Science Foundation, "The Contribution of Social Research," John Madge Memorial Lecture, London School of Economics, November 1986, quote from p. 6.

⁶"Behavioral Directorate for NSF Proposed in Congress," *APS Observer*, vol. 3, September 1990, p. 7.

⁷"Social, Behavioral Sciences Seek Upgrade at NSF," *Science & Government Report*, vol. 20, No. 15, Oct. 1, 1990, p. 6.

Continued on next page

Box 5-F—Behavioral and Social Sciences: Organization and Federal Funding-Continued

and bigger budgets.⁸ NSF appointed a task force on “Imdcing to the Twenty-First Century” to study the idea and “... keep several thoughts in mind: 1) BBS must have the flexibility to meet new mandates; 2) BBS must meet the infrastructure needs of its disciplines; and 3) the zero-sum budget situation makes funding reallocations difficult. ’

In December 1990, the task force voted its intention to recommend establishing a separate NSF directorate for social and behavioral sciences. It would be called Social, Economic, and Psychological Sciences (SEPS).¹⁰ Foremost among the issues the task force must consider are how the boundaries for the behavioral sciences would be drawn, and how interdisciplinary research would be affected. Recommended for inclusion in SEPS are economics, geography, law, linguistics, political science, psychology, and sociology. The interdisciplinary fields of cognitive science and of decision, risk, and management sciences would also be included. Most of neuroscience would stay in the biological directorate. Unresolved are the place of anthropology and some of the programs supporting research on information, robotics, and intelligent systems (now housed in the Computer and Information Science and Engineering Directorate).¹¹ In lieu of immediately developing a divisional or programmatic structure for SEPS, a new group (including NSF program officers) may be asked to take up the issue.

Whether a separate directorate could aid the management and funding of social and behavioral science research at NSF, and how the agency could assess the effectiveness or productivity of such a new directorate, remains to be seen. Implementation of whatever is finally approved would not occur until fiscal year 1993. It is clear that advocates in the vast majority of behavioral and social science fields (led by psychology) are convinced that “. . . only by elevating representation of our scientific disciplines will we successfully compete and increase our funding capabilities and our potential contributions to science. 12

⁸The question of political vulnerability that accompanies a consolidation of interests in a single structure is the flip side of political gain. For example, concern for the environmental sciences has led to a call for Congress to create a new agency, the National Institutes for the Environment (N’KE), modeled on the National Institutes of Health, to stop the erosion of research and training programs related to environmental biology, economics, and policy. Congress has asked the National Academy of sciences to study the concept of an NIE. See Henry F. Howe and Stephen P. Hubbell, “Progress Report on Proposed National Institutes for the Environment,” *Bioscience*, vol. 40, No. 8, September 1990, p. 567; and William Booth, “Does Earth Need a Government Institute?” *The Washington Post*, Dec. 10, 1990, p. A13.

⁹“BBS Task Force Meets: Separate Directorate Issue on the Table,” *COSSA Washington Update*, Sept. 21, 1990, p. 1.

¹⁰The final report of the task force is forthcoming. See “NSF Task Force to Recommend New Directorate for Social and Behavioral Sciences,” *COSSA Washington Update*, Dec. 7, 1990, pp. 1-2; and “NSF Task Force Discusses Upcoming Report, Agrees to Support SEPS Directorate,” *COSSA Washington Update*, Jan. 28, 1991, pp. 1-2.

¹¹Anthropology opposes the creation of a new directorate, but the American Anthropological Association has announced that: “were the proposed reorganization to occur, [anthropology] would elect to be housed . . . “ in it. See “Anthropologists Opt for SEPS Directorate,” *COSSA Washington Update*, Feb. 10, 1991, p. 2. Applied statistics, “. . . measurement and methodological research, as well as infrastructure issues . . .” are favored by the task force to join the Social, Economic and Psychological Sciences as well. See “NSF Taak Force Discusses Upcoming Report,” op. cit., footnote 10, pp. 1-2.

¹²Quoted from the American Psychological Association in “NSF Task Force to Recommend New Directorate for Social and Behavioral Sciences,” op. cit., footnote 10, p. 1. Also see Alan G. Kraut, “Statement of the American Psychological Society to the National Science Foundation’s BBS Task Force on Looking to the 21st Century,” unpublished document, Nov. 29, 1990.

The science base, especially at NSF and NIH, carries not only the traditional responsibility for funding scientifically meritorious research, but also for satisfying the expectations that the political system associates with the support of research. These expectations, together with budget constraints, create tougher and tougher choices. The agencies cope admirably with this complex task. In the next section, OTA examines another category of research funding: science megaprojects.

Science Megaprojects

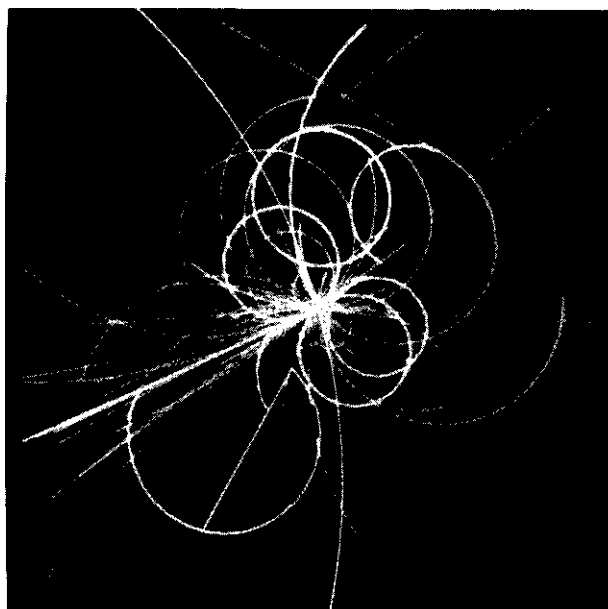
The Federal Government has a long history of supporting projects such as the building and operation of dams, bridges, and transportation systems. These projects are large-scale, complex, costly, and long-term undertakings. In addition to performing their primary function, these programs also provide jobs and local public works, and have long-term economic value. Thus, they are often called “mega-projects. ’

As the body of scientific knowledge grows more sophisticated and costly, research instrumentation and infrastructure are required in some fields.³² As projects expand, they become valuable economically and politically. For example, the Hubble Space Telescope, launched in April 1990, cost over \$2 billion, although the original estimate was as low as \$300 million.

By the time the real costs were known, it was too far along to stop. As a practical matter, Congress refused to write off as wasted the hundreds of millions it already had sunk into the project. Politically, the device had taken on a pork-barrel life of its own, sending government money to nearly half the 50 States and employing thousands.³³

Although economic activity may be a second- or third-order consideration among the initial criteria of project selection, the distribution of Federal monies, as an interim payoff on a long-term investment, can be substantial. For instance, the three megaprojects shown in figure 5-3, the Hubble Space Telescope, the Space Station, and the SSC, enjoy widespread economic and social merits, regardless of their scientific merit.

Among the megaprojects recently listed by *The Chronicle of Higher Education* are the SSC, the Space Station, the Moon-Mars mission, the hypersonic aircraft, the Earth Observing System, the Strategic Defense Initiative, and the Human Genome Project (HGP). The original estimates for these seven projects alone totaled \$528 billion. A September 1990 cost estimate for the same projects was about \$580 billion,³⁴ or \$65 billion annually.³⁵



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What Constitutes a Science Megaproject?

Megaprojects are large, lumpy, and uncertain in outcomes and cost. “Lumpy” refers to the discrete nature of a project. Unlike little science projects, there can be no information output from a megaproject until some large-scale investment has occurred.³⁶ OTA also would define a science megaproject as requiring very large expenditures (especially when

³²One philosopher argues that the further and further we get from direct sense experience, the more costly and complex research technologies we need for progress. See Nicholas Rescher, *Scientific Progress: A Philosophical Essay on the Economics of Research in Natural Science* (Pittsburgh, PA: University of Pittsburgh Press, 1978). The section below is based on Averch, op. cit., footnote 28.

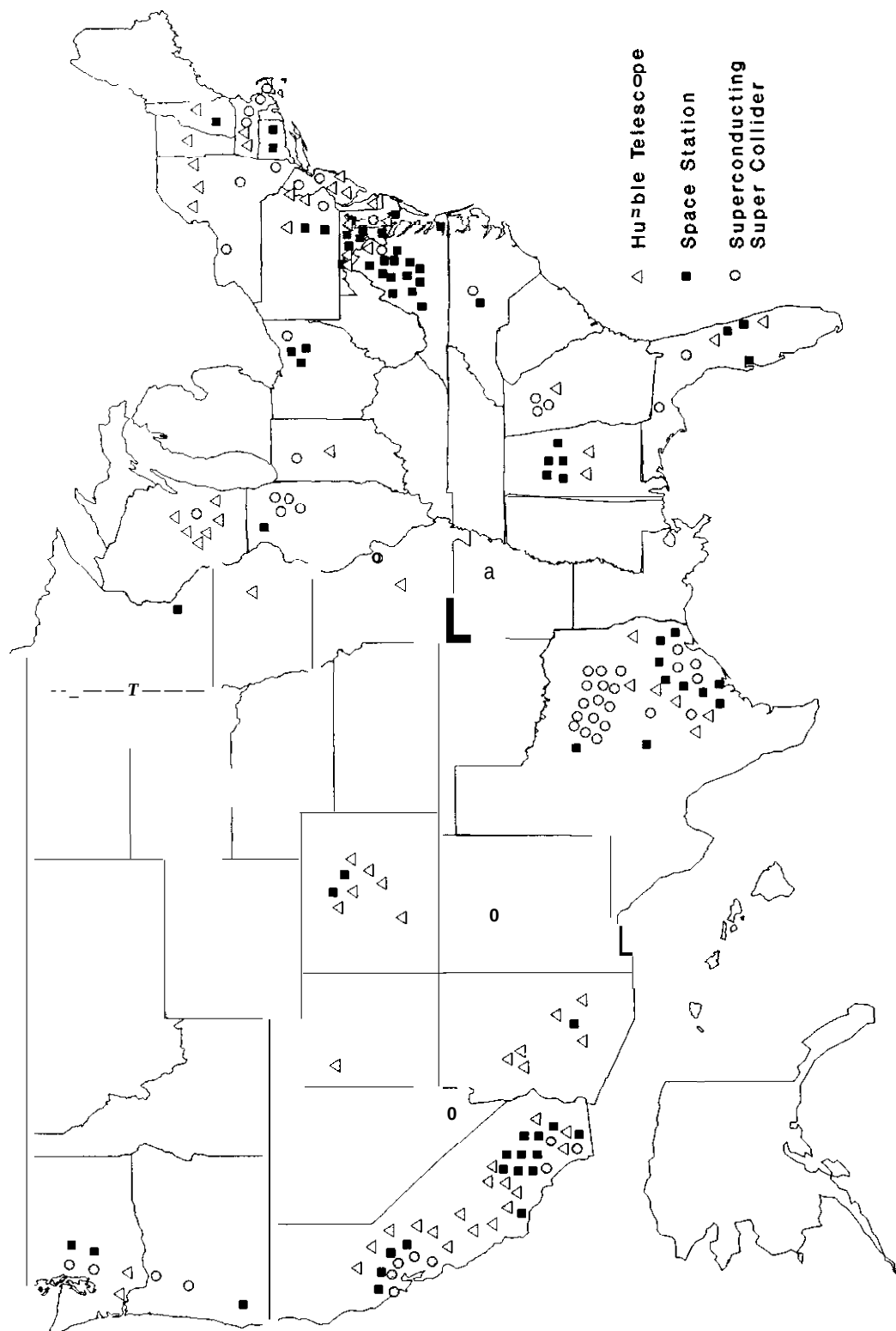
³³Phil Kuntz, “Pie in the Sky: Big Science Is Ready for Blastoff,” *Congressional Quarterly*, vol. 48, Apr. 28, 1990, p. 1254. Also see Kim A. McDonald, “Researchers Increasingly Worried About the Unreliability of Big Science Projects,” *The Chronicle of Higher Education*, vol. 36, No. 48, Aug. 15, 1990, pp. A1, A8-A9.

³⁴Colleen Cordes, “Big Science and Technology Projects Near Important Milestones in Face of Federal Budget Crunch and Mounting Criticism,” *The Chronicle of Higher Education*, vol. 37, No. 2, Sept. 12, 1990, p. A28, reports original and current cost estimates submitted to Congress just for the R&D investments required for the seven projects (most not adjusted for inflation). High-technology R&D project cost estimates are notoriously (but unsurprisingly) low. For example, a number of RAND Corp. studies show the underestimation in the actual costs to develop high-tech military aircraft. See P.W.G. Morns and G.H. Hough, *The Anatomy of Major Projects: A Study of the Reality of Project Management* (New York, NY: John Wiley & Sons, 1987).

³⁵See “Big Science: Is It Worth the Price?” *The New York Times*, May 27, 1990, p. 11, and June 5, 1990, p. C1.

³⁶It is not possible to build one-half of a dam, one-half of a ship, or one-half of an airplane and get the desired performance. These technologies are, of course, well enough in hand that one can estimate or predict the results from investing in them. There is a very extensive literature on appraisal and management of capital projects such as dams, airports, ports, and also a sizable literature on estimating the worth of private sector capital investments in factories and equipment. Among the literature on managing large complex technological systems, literally nothing is written on selecting them. But see Harvey Sapolsky, *The Polaris System Development: Bureaucratic Success in Government* (Cambridge, MA: Harvard University Press, 1972).

Figure 5-3—Where the Money Goes for Three Megaprojects



SOURCE: Map by Sharon Parkinson in Phil Kuntz, "Pie in the Sky: Big Science Is Ready for Blastoff," *Congressional Quarterly Weekly Report*, vol. 48, Apr. 28, 1990, p. 1255.

compared with other investments in the same or similar fields) to create knowledge that is unattainable by any other means.

Perhaps equally important in the definition of science megaprojects, however, are the political components. Each project is unique in its development, especially in its progress through the budget processes at the research agencies. Also, science megaprojects are supported by large political constituencies extending beyond the scientific community. In short, there are few rules for selecting and funding science megaprojects; the process is largely ad hoc. To illustrate, OTA presents two widely acknowledged examples of big science projects—the SSC and the HGP.

The Superconducting Super Collider

The SSC, when built, will accelerate two counter-circulating beams of protons at energy level 20 TeV to “create” rarely seen elementary particles when these beams collide. Expected to cost at least \$8 billion to construct, the SSC represents one of the world’s largest scientific instruments.³⁷ Amidst contentious debate in Congress, the SSC won funding approval to begin construction in June 1989. DOE decided to build the 54-mile-in-circumference accelerator south of Dallas, Texas; it is expected to begin operation in 1999.³⁸ Director Emeritus of Fermi National Accelerator Laboratory, Leon Lederman, has said in House testimony that “. . . instead of trying to kill off a big target like the SSC . . . the collider should be seen as the ‘flagship’ for big increases for science [funding].”³⁹

The SSC clearly meets the specific criteria outlined by OTA for a big science project (high cost and unique outcomes). It also satisfies the political criteria. First, the SSC has important scientific goals that can be obtained in no other way. Second, the high-energy physics community has marshaled support of the SSC from DOE, which administers the project, and the State of Texas, where it will be built. Finally, the SSC originated in DOE discussions with the high-energy physics community, and was preferred by the Department’s High Energy Physics Advisory Panel to the equivalent amount invested in smaller, less costly high-energy physics.⁴⁰ As with many big science projects, however, it is also true that, without the prospect for such an accelerator, the equivalent amount would most likely *not be* available to physicists—or indeed to scientists in other fields—at all.

The Human Genome Project

The HGP, estimated to cost \$3 billion to complete, is expected to yield a high-quality genetic map of the human genome. The HGP is “big” biology in lifetime costs and mode of organization (e.g., scientists clustered in “mapping” centers) relative to the rest of biomedicine.⁴¹ An annual \$200 million appropriation would represent 5 percent of NIH’s funding for untargeted research.⁴² Its fiscal year 1991 appropriation is \$135 million. HGP organizers stress that funding for the project since its inception has been “new” money. Funds from other budget categories at NIH have not shifted to the HGP, i.e., none have decreased since the inception of the project.⁴³

³⁷See David P. Hamilton, “The SSC Takes On a Life of Its Own,” *Science*, vol. 249, Aug. 17, 1990, pp. 731-732; and Marcia Barinaga, “The SSC Gets Its (Official) Price Tag: \$8.3 Billion,” *Science*, vol. 251, Feb. 15, 1991, pp. 741-742.

³⁸See U.S. Department of Energy, Office of the Inspector General, *Special Report on the Department of Energy’s Superconducting Super Collider Program*, DOE/IG-0291 (Germantown, MD: Nov. 16, 1990).

³⁹Quoted in Colleen Cordes, “Calls for Setting Science-Spending Priorities Are Renewed as Supercollider Gets Go-Ahead, NSF Faces Pinch,” *The Chronicle of Higher Education*, vol. 35, No. 46, July 26, 1989, p. A23. Notice the tradeoff language in the headline.

⁴⁰In spring 1990, the Department of Energy’s High Energy Physics Advisory Panel ranked research goals for the 1990s: building the Superconducting Super Collider (SSC) was first, upgrading Fermilab’s proton-proton Tevatron collider was second, and supporting university-based investigators received honorable mention. See “Physics Panel Sets Priorities for 1990s,” *Science*, vol. 248, May 11, 1990, p. 681. This also emerged from OTA staff interviews at the Department of Energy, spring 1990. In a January 1991 news release, the Council of the American Physical Society, for the first time in its 93-year history, “. . . overwhelmingly adopted a public position of funding priorities. Top priority is given to support of individual investigators and ‘broadly based physics research.’” “The statement also endorses construction of the SSC in a”. . . timely fashion, but the funding required to achieve this goal must not beat the expense of the broadly based scientific research program of the United States.” See American Physical Society, “First APS Council Statement on Funding Priorities,” news release, Jan. 28, 1991.

⁴¹Tom Shoop, “Biology’s Moon Shot,” *Government Executive*, February 1991, pp. 10-11, 13, 16-17.

⁴²In fiscal year 1990, the Human Genome Project is budgeted for \$90 million at the National Institutes of Health (NIH) and \$46 million at the Department of Energy. The NIH amount represents 1 percent of its total budget. See Leslie Roberts, “A Meeting of the Minds on the Genome project?” *Science*, vol. 250, Nov. 9, 1990, pp. 756-757.

⁴³Stu Borman, “Human Genome Project Moving on Many Fronts,” *Chemical & Engineering News*, vol. 68, No. 50, Dec. 10, 1990, pp. 6-7.

Critics of the HGP contend that it flaunts tradition in the administration and performance of biomedical research. They are also “. . . not convinced that a crash program for analyzing the structure of genomes will advance either health or the life sciences for many years to come.”⁴⁴ Proponents of the project stress its development of automated technologies for molecular biology, including mapping and sequencing, and of new computational approaches that apply computer science to biology. Thus, “. . . a new type of interdisciplinary biologist who understands technology as well as biology . . .” is being trained. Besides, in the words of molecular biologist Leroy Hood:

The HGP primarily funds single investigator-sponsored research. It is not big science. Rather, by making the human chromosomal map and sequence available to small laboratories, it allows them to compete with large laboratories. Hence, the HGP is the guarantor of small science.⁴⁵

The HGP was originally billed as a project that would contribute to the cures for all disease.⁴⁶ As legislators skeptically claimed that they had heard this “promise” from life science projects before, proponents of the project began to promote the HGP on its other potential strengths, including contributions to economic competitiveness. For instance, Hood stated that HGP “. . . will in turn spawn new industrial opportunities. . . . The HGP will prime the American economic pump.”⁴⁷

At issue in the designation of this science project as “big” is more than cost and organization, but the timing of the research investment and its impact on both the culture and justification for biomedical

research. The 1 percent of NIH’s budget is a small investment, but it represents the reordering of criteria and the disruption of research-m-usual.

The Process of Megaproject Selection

From a national perspective, megaprojects are very large projects that stand alone in the Federal budget and cannot be subject to priority setting within a single agency. Nor can megaprojects be readily compared. The SSC and HGP are not big science in the same sense. One involves construction of a large instrument, while the other is a collection of small projects. There also exists virtually no scholarly literature to guide the selection of megaprojects designed to promote the state of the art of scientific fields.⁴⁸ At issue with many megaprojects is their contribution to science. For instance, the Space Station has little justification on scientific grounds,⁴⁹ especially when compared with the SSC or the Earth Observing System, which have explicit scientific rationales. At present, the Space Station does have considerable momentum as an economic and social project.⁵⁰ However, many question the uniqueness of these benefits because other projects, such as the Earth Observing System, could certainly provide many jobs as well. Because the problem of selecting among science megaprojects has most in common with the selection of complex capital projects, timeliness (why do it now rather than later?) and scientific and social merit must all be considered.

The tradeoffs among these criteria are complex, even when restricted to considerations solely within

⁴⁴Bernard D. Davis, “Human Genome Project: Is ‘Big Science’ Bad for Biology?—Yes: It Bureaucratizes, Politicizes Research” *The Scientist*, vol. 4, No. 22, Nov. 12, 1990, p. 15.

⁴⁵Leroy E. Hood, “Human Genome Project: Is ‘Big Science’ Bad for Biology?—No: And Anyway, the HGP Isn’t ‘Big Science,’” *The Scientist*, vol. 4, No. 22, Nov. 12, 1990, p. 15. For an elaboration, see Walter Gilbert, “Towards A Paradigm Shift in Biology,” *Nature*, vol. 349, Jan. 10, 1991, p. 99.

⁴⁶See U.S. Congress, Office of Technology Assessment, Mapping, *Our Genes: Genome Projects—How Big, How Fast?* OTA-BA-373 (Washington, DC: U.S. Government printing Office, April 1988).

⁴⁷Hood, *op. cit.*, footnote 45, p. 13.

⁴⁸The journal *Technology in Society* devoted one full issue in 1988 and another in 1990 to the political and social consequences of large technological projects, but no author discussed algorithms for selecting them. Also see William C. Boesman, “Historical Perspective on Large U.S. Science Facilities,” *CRS Review*, February 1988, pp. 8-10; and Peter Monaghan, “Historians Seek More Detailed Study of Big Science Projects to Inform Debate Among Researchers and Policy Makers,” *The Chronicle of Higher Education*, vol. 37, No. 14, Dec. 5, 1990, pp. A5, A8.

⁴⁹For an early statement of this view, see U.S. Congress, Office of Technology Assessment, *Civilian Space Stations and the U.S. Future in Space* (Springfield, VA: National Technical Information Service, November 1984).

⁵⁰This momentum may be slowed by the latest statement of “no confidence” in the scientific content of Space Station Freedom in a forthcoming National Academy of Sciences report. See David P. Hamilton, “Space Station Shrinkage To Affect Scientific Mission,” *Science*, vol. 251, Mar. 8, 1991, p. 1167; and Eliot Marshall, “Two Thumbs Down for Space Station,” *Science*, vol. 251, Mar. 22, 1991, p. 1421.

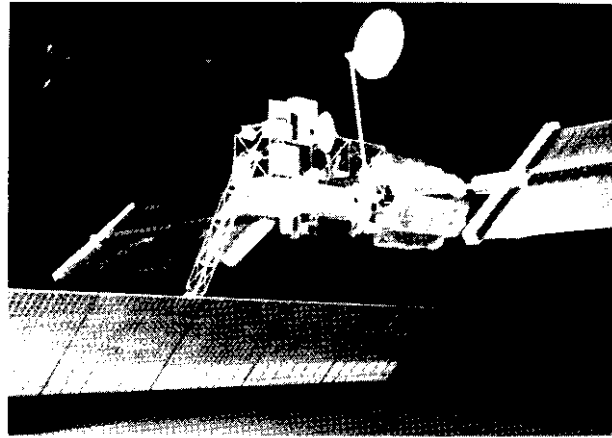


Photo credit: National Aeronautics and Space Administration

In the early stages of megaproject development, it is often difficult to obtain firm estimates of cost because plans can change radically. In 1982, the Earth Observing System (EOS) was conceived as a large space antenna system, as in the artist's rendering on the left. By 1990, the conception of EOS had changed to include "platforms" in space and other features, as shown on the right.

the scientific community. One observer puts the dilemma of weighing social and scientific merit this way:

Scientific progress depends heavily on scientific capital; scientific capital is built up by investments in training, equipment, pilot research, and the accumulation of expertise over extended periods. A single very large project may have great scientific and social benefits, but if it can be done only by shutting down existing lines of research in other areas, the opportunity losses—the loss of the benefits these lines of research would have produced, plus the cost of duplicating in the future the capital investments in them—can outweigh the gains from the larger project. It is very difficult to estimate the losses from opportunities foregone; however, we do know that a small proportion of studies trigger the kind of dramatic breakthroughs that transform life in ways the original researchers themselves rarely envision.⁵¹

In addition, funds are still obligated to agencies. So assurances notwithstanding, the research community perceives megaprojects as new money for an

initiative that could supplant older, S&T base programs, and would be added to agency budgets if the megaproject did not exist.⁵²

On a national scale, criteria and tradeoffs are even more difficult to quantify, since completely separate fields are represented. The social and scientific benefits that will derive from investing in one are incommensurable with those that would be derived from investing in some other.⁵³ So weighing the scientific, technological, and development benefits that will result from the projects will not suffice; economic and labor benefits must also be considered. Other criteria might also include education and training benefits, and the impact of the project on the research community measured in per-investigator costs. For instance, if one project will benefit only a few researchers, while a second of similar cost will benefit a larger number of researchers, then perhaps the second should be favored.

One might also expect preference for megaprojects that can be cost-shared internationally over those that cannot be. This conceives of megaproject output as a contribution to world science, i.e., as

⁵¹Robert L. Stout, "Evaluating Scientific Initiatives," letter, *American Scientist*, vol. 77, No. 3, May-June 1990, pp. 211-212. The opportunities presented by megaprojects should be compared with the foregone benefit of little science at the margin, not on average.

⁵²This has been charged repeatedly about AIDS funding relative to the rest of the National Institutes of Health budget. For example, see David T. Denhardt, "Too Much for AIDS Research," *The Washington Post*, Oct. 2, 1990, p. A19.

⁵³See, for example, J. E. Sigel et al., "Allocating Resources Among AIDS Research Strategies," *Policy Sciences*, vol. 23, No. 1, February 1990, pp. 1-23. The authors asked 17 nationally known AIDS experts to estimate the marginal or incremental value of additional funds for different AIDS research investments in terms of some prespecified social outcomes. The information that would be gained from these different investments is incommensurable, but their expected contribution to the pre-specified social outcome allows them to be ordered. The megaproject problem, however, is more like judging research investments in AIDS v. heart disease v. cancer.

Table 5-6-A Comparison of Science Megaprojects (in billions of dollars)

Project	Original cost estimate	Most recent estimate	Spent so far (since)	Timeframe (years)
Hubble Telescope	\$0.29-0.34 (1973)	— ^a	\$2 (1978)	— ^a
Space Station	(1983)	\$37	(1985)	16
Superconducting Super Collider	(1987)	8.6	0.35-0.43 (1988)	11
Human Genome	(1988)	3	0.16 (1988)	15

^aProject completed

NOTE: Original cost estimates do not include inflation, while the recent estimates and the amount spent so far include inflation. Hubble expenditures include development (\$1.5 billion) and operating costs (\$0.5 billion), from fiscal years 1978-1991. All cost estimates are rounded.

SOURCES: Based on "The Outlook in Congress for 7 Major Big Science Projects," *The Chronicle of Higher Education*, vol. 37, No. 2, Sept. 12, 1990, p. A28; Genevieve J. Knezo, "Science Megaprojects: Status and Funding, February 1991," *CRS Report for Congress*, 91-258 SPR (Washington, DC: Congressional Research Service, Mar. 12, 1991); Phil Kuntz, "Pie in the Sky: Big science Is Ready for Blastoff," *Congressional Quarterly Weekly Report*, vol. 48, Apr. 28, 1990, pp. 1254-1260; and National Aeronautics and Space Administration, Office of Resources Analysis, Office of the Comptroller, personal communication, March 1991.

information appropriable by all who want it and can benefit. The SSC would not be defined as a competitor of CERN (the European Organization for Nuclear Research in Geneva, Switzerland) in some private particle race pursued by U.S. high-energy physicists; there would also be no national objective to keep the American rate of discovery above that of European or Japanese physicists.⁵⁴ While the prevailing claim is that "priority races" are necessary to make progress in science, cost- and information-sharing are consistent with a view of research as an appropriable, world public good.⁵⁵

While scientific and social merit are abstract, they provide a framework to evaluate the merits of proposed big science projects. More concrete concerns include the range of megaproject costs and their management.

Megaproject Costs and Management

The Federal Government buys big science initiatives, and the initial investment may represent a point of no return. Once the "go, no-go" decision

has been made at the national level, the commitment is expected to be honored, no matter how much the cost estimates or timetables for completion change. However, criteria for consideration in the funding of a science megaproject could conceivably include: startup and operating costs, and likely changes in the overall cost of the project from initial estimate to completion. Table 5-6 presents a comparison of four projects, which shows that the cost estimates for some big science projects double before they are even begun.

Table 5-7 presents the budget authority for four projects in fiscal years 1990 and 1991. The percentage increases requested are considerably larger than the average annual increase in total budgets proposed for the cognizant research agencies. The costs incurred in future years by most megaprojects are enormous, and it is unclear that all of the projects currently receiving funds can be supported in coming years.

In addition, costs for maintenance of a big science facility once it is operational are rarely considered.

⁵⁴See S.S. Yamamoto, "A Genuine Global Partnership?" *Nature*, vol. 346, Aug. 23, 1990, p. 692. This has likewise been an issue in the Human Genome Project, since James Watson, Director of the National Institutes of Health's National Center for Human Genome Research, has been outspoken about the disappointing level of funding and commitment to the project by the government of Japan and France. See Borman, op. cit., footnote 43, p. 7. Also, the benefits of megaprojects include not only the scientific knowledge generated, but the technological know-how gained in designing and building the instruments.

⁵⁵Two literatures are relevant here. One is on analysis of 'simultaneous multiple discoveries' and creativity in science; the other is on data sharing and the diffusion of knowledge. On the former, see Dean Keit Simonon, *Scientific Genius: A Psychology of Science* (Cambridge, England: Cambridge University Press, 1988); on the latter, see David S. Cordray et al., "Sharing Research Data: With Whom, When, and How Much?" paper presented at the Public Health Service Workshop on Data Management in Biomedical Research, Chevy Chase, MD, Apr. 25-26, 1990; and Eliot Marshall, "Data Sharing: A Declining Ethic?" *Science*, vol. 248, May 25, 1990, pp. 952, 954-955, 957.

Table 5-7-Four “Big Science” Initiatives in the Fiscal Year 1991 Budget
(estimates in millions of current dollars)

Initiative	Fiscal year 1990 enacted	Fiscal year 1991 proposed	Proposed percent increase	Fiscal year 1991 enacted	Enacted percent change
Strategic Defense Initiative	\$3,600	\$4,500	25%	\$2,900	-19%
Space Station	1,750	2,451	40%	1,900	90%
Superconducting Super Collider	225	331	47%	243	8%
Human Genome	60	108	41%	88	47%
Total	5,635	7,390	31%	5,131	-9%

SOURCE: Michael E. Davey, Congressional Research Service, “Research and Development Funding: FY1991,” issue brief IB90048, Nov. 13, 1990.

The Space Station promises to require at least \$1.5 billion per year in maintenance—an amount not figured into original cost estimates.⁵⁶ Much of the maintenance support will be transported by the Shuttle, which has proven less than reliable in recent years. These concerns raise questions about how realistically operations are weighed in securing approval of megaprojects.

Another concern is the “‘top-down’ organization of big science projects. For example, one critic of the HGP endorses both the goals and the quality of the science so far, but calls it “. . . overtargeted, over-budgeted, overprioritized, overadministered, and . . . micromanaged.”⁵⁷ In contrast, some projects are criticized for a lack of management: “Though over \$4 billion has been spent so far on the Space Station, it exists only as a paper design, and with virtually no purpose beyond serving as a platform for the glamour of man in space.”⁵⁸ Clearly, management is an important consideration in megaproject development.

Any big science project on the forefront of expertise will involve considerable learning by

doing. Once a megaproject has been selected, real-time evaluations of its progress can also be carried out that give rapid feedback to those involved.⁵⁹ While there is no guarantee that agency sponsors of megaprojects will listen to evaluators, the latter can become another constituency defined into the decisionmaking process.

In sum, megaprojects will always be selected through a political, public process because of their scale, lumpiness, and incommensurability. Yet, for each initiative, as the NAS priority report reminds: . . . it is necessary to specify the institutions, individuals, and organizations that will be served; the costs; the opportunities for international cooperation and cost sharing; the management structure; and the timeliness of the program.”⁶⁰ The cost of investment for the Federal Government and the cost per investigator are criteria that apply to all science initiatives. The designations “big” and “little” are quite variable when projected over time and relative to the total value of an agency’s portfolio. Clearly, the process of making Federal research investments could become more iterative, less sequential, and better oriented to national goals. OTA next examines an alternative to current practice.

⁵⁶Cordes, *op. cit.*, footnote 34, p. A28.

⁵⁷Don Brown quoted in Roberts, *op. cit.*, footnote 42, p. 756. For example, there are now a total of six National Institutes of Health-supported genome research centers and growing questions about acceptable costs and error rates in sequencing a genome before deposit in a database. See “New Human Genome Centers Established,” *Chemical & Engineering News*, vol. 69, No. 6, Feb. 11, 1991, p. 16; and Leslie Roberts, “Large-Scale Sequencing Trials Begin,” *Science*, vol. 250, Dec. 7, 1990, pp. 1336-1338.

⁵⁸“Man-in-Space: The Self-Inflicted Curse of NASA,” *Science & Government Report*, vol. 20, No. 13, Aug. 1, 1990, p. 4.

⁵⁹ See K. Guy and L. Georgiou, “Real-Time Evaluation and the Management of Mission-oriented Research: The Evaluation of the Alvey Program—Aims, Achievements and Lessons,” unpublished paper, presented at the ECE Seminar on Evaluation in the Management of R and D, Apr. 3-7, 1989. In addition, if real-time evaluation had been heeded in the construction of the Hubble Telescope, for instance, a full-scale test of the mirror could have been performed. See Bob Davis, “NASA Management Flaws Led Agency to Overlook Hubble Defect, Panel Finds,” *Wall Street Journal*, Nov. 27, 1990; and William Booth, “Hubble Report Faults Builder, NASA,” *The Washington Post*, Nov. 28, 1990, p. A8.

⁶⁰National Academy of Sciences, *op. cit.*, footnote 17, p. 11.

Research Priorities and the “Big Picture”

Figure 5-4 depicts the projected outlays for the science base and science megaprojects discussed above. The projected expenditures for big science projects rise in the 1990s as an increasingly significant portion of those for science projects as a whole. (Since cost estimates for megaprojects tend to grow precipitously, a similar figure that doubles those expenditures are included for sake of comparison in figure 5-4.⁶¹) Within the current funding climate and that predicted for the 1990s, perhaps not all components of the current Federal research portfolio can be supported. Choices among science projects may need to be made. Because of the large projected lifetime costs associated with each megaproject, sorting and recalibrating the costs of each earlier rather than later would be useful.

How could such choices be made? Ideally, one might ask that Federal funds be allocated to the science base and then add megaprojects in order of importance until funds are depleted. However, such a sequential approach is not realistic. First, there is nothing that corresponds to a single research budget. Many countries, for example, Canada, Germany, and Sweden, have capital budgets for all functions, including research. If the United States had a capital budget distinct from its operating budget, then it could rate megaprojects against one another and compare them with other capital investments. Second, megaprojects are funded on an equal footing in many agencies with other research programs. Finally, in the words of Albert Teich, of the American Association for the Advancement of Science:

Advocates of systematic priority setting and those who may be called on to advise in the process need to recognize that any such rational analysis is just one element of the picture. Such analysis may influence the process, but it does not determine priorities. Other factors and other voices will and should be heard. Political criteria are not a contaminant in the allocation of public resources for

research; they are absolutely essential to the democratic process and to the long-run effective functioning of the system.⁶²

An annual review of commitments across categories of investment would help to gauge balance by field, research problem, and agency contributions to the achievement of national goals. By revisiting these categories year after year, Federal investments could be appraised to add and subtract from the Nation's research portfolio.⁶³

Once the context for priority setting is examined, tradeoffs and choices take on another dimension. What do U.S. society and the Federal Government expect for their research investment? What does the scientific community promise to deliver? The answers differ among participants and over time. The answers differ because criteria and expectations differ, because there are plural research systems, and because participants can influence the process of budgeting and research decisionmaking at many levels.

Although scientific merit and program relevance must always be the first criteria used to judge a research program or project's potential worth, they cannot be the *sole* criteria. First, in today's research system, there are many more scientifically meritorious projects than can be funded. In its initial effort to document stress on the Federal research system created by an abundance of research applications, OTA found that an increasing proportion could not be funded by various research agencies due to budget limitations, rather than to deficiencies of quality.⁶⁴ Second, rewarding scientific merit and relevance alone can inhibit the system from preparing for the future. This problem is seen clearly in the finding of young investigators. Since the prospective yield of new knowledge is judged by the technical merit (e.g., soundness of design or experimental protocol) of a project proposal, its scientific creativity, *and the* track record of the scientist, young investigators are at a disadvantage, and other criteria must be weighed when evaluating their proposals.

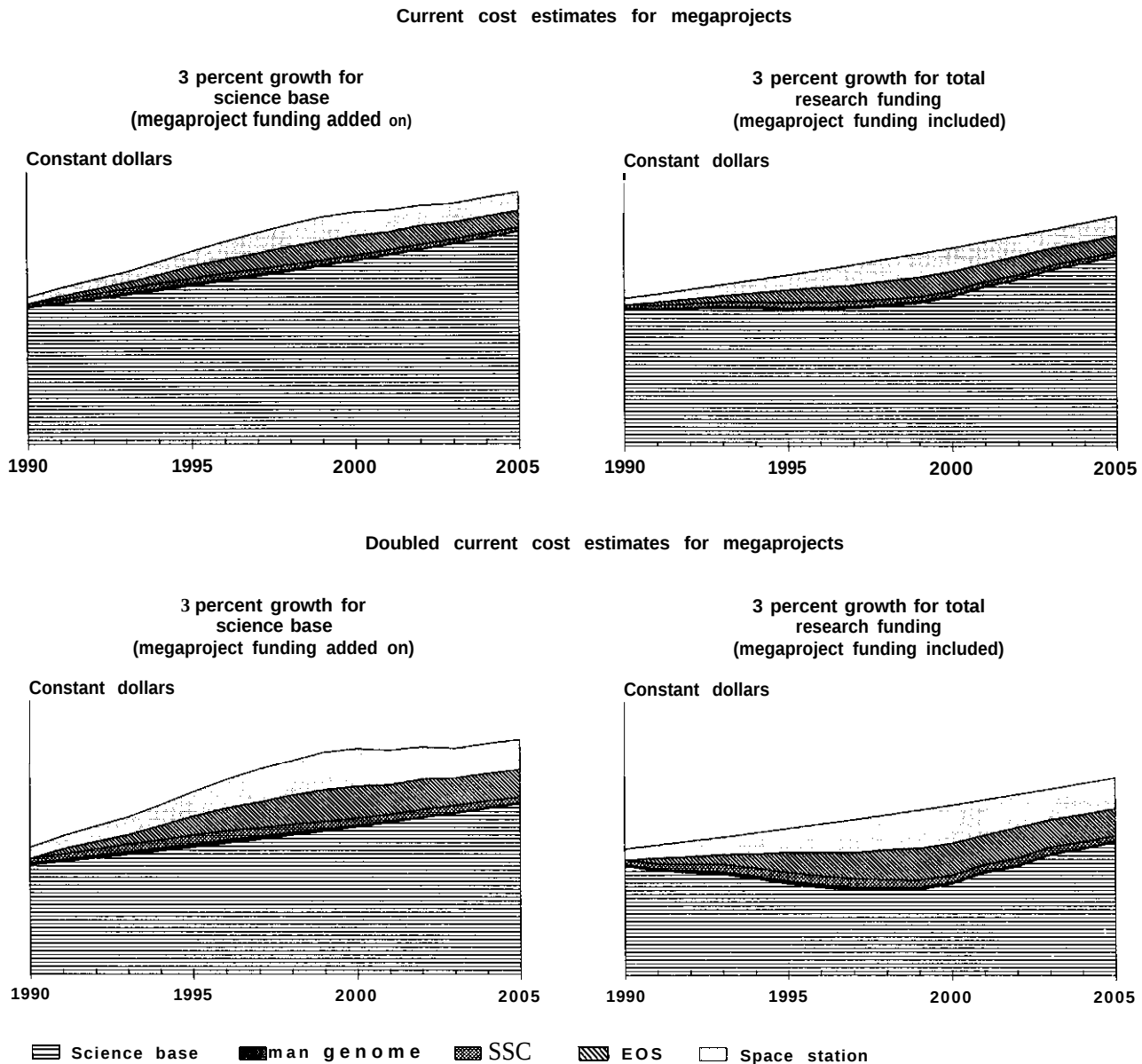
⁶¹Note that some estimates of megaprojects include only capital costs, while others include capital and operating costs.

⁶²Albert H. Teich, “Scientists and Public Officials Must Pursue Collaboration To Set Research Priorities,” *The Scientist*, vol. 4, No. 3, Feb. 5, 1990, p. 17.

⁶³To iterate is to plan and exercise flexibility within a budget envelope—much like a National Basketball Association team shuffling its roster to stay under the league's imposed salary cap while enjoying a full complement of players at every position.

⁶⁴U.S. Congress, Office of Technology Assessment, “Proposal Pressure in the 1980s: An Indicator of Stress on the Federal Research System,” staff paper of the Science, Education, and Transportation Program, April 1990.

Figure 5-4—Cost Scenarios for the Science Base and Select Megaprojects: Fiscal Years 1990-2005



KEY: SSC=Superconducting Super Collider; EOS=Earth Observing System.

NOTE: These figures are schematic representations of projected costs for science projects. In the figures on the left, the *science base* is projected to grow at an annual rate of 3 percent above inflation. In the figures on the right, *total research funding* is projected to grow 3 percent above inflation. The cost estimates for the *megaprojects* are based on data from "The Outlook in Congress for 7 Major Big Science Projects," *The Chronicle of Higher Education*, vol. 37, No. 2, Sept. 12, 1990, p. A28; and Genevieve J. Knezo, "Science Megaprojects: Status and Funding, February 1991," *CRS Report for Congress*, 91-258 SPR (Washington, DC: Congressional Research Service, Mar. 12, 1991).

SOURCE: Office of Technology Assessment, 1991.

There is a role for Congress to set priorities across and within categories of science and engineering research. The application of criteria that augment “scientific merit” and “program relevance”—which are today’s judgments of quality—would clarify *tomorrow’s* objectives of research investment. As discussed in chapter 1, broadly stated, there are two such criteria: strengthening education and human resources (i.e., increasing the number and diversity of participants); and building regional and institutional capacity (including economic development by leveraging Federal research support).⁶⁵ Both sets address the future capability of the research system in response to national needs, and both can be employed in mainstream and set-aside programs.

Conclusions

Since progress begets more opportunities for research than can be supported, setting research priorities may be imperative for the success of science in the 1990s.⁶⁶ And while the questions raised in this chapter have a familiar ring—how should Federal monies for research be spent? which opportunities for scientific advance merit funding now? who should decide?—the search for a framework to judge criteria of choice has grown urgent. In the pluralistic and decentralized system of research decisionmaking, sponsorship, and performance, there are ample voices to justify most any hierarchy of programs and projects on the grounds of ‘social’ or “scientific” merit. The question of what do U.S. society and the Federal Government want for their research investments has many answers.

Long before the onset of stringency in Federal discretionary funding, priority setting was an integral part of the regular budget process:

By the time any budget for science has been pulled apart by function in the budget committees, by agency in the legislative committees, and by appropriations bills in the appropriations committees

(in both House and Senate at each of these levels) and reassembled among the various other programs of veterans’ benefits, sewage treatment grants, and agricultural price supports, its internal priorities will be unrecognizable.⁶⁷

The problem is not a lack of priority setting. The problem is implementing priorities in the name of national goals and scientific needs. How can that be achieved?

Some observers of the current priority-setting process have suggested improvements to the process that are structural, in particular centralizing the budget process and intensifying research planning within and across the agencies. This would make the tradeoffs more explicit and less ad hoc, and the process more transparent. At a minimum, multiyear budgeting and an agency crosscutting budgetary analysis (proponents like NAS say) could reduce uncertainty in budgeting.⁶⁸

To ensure that priorities are set, some persons, committees, or bodies of the Federal Government, in addition to the President, must be invested with the power to set priorities. Agency managers are already performing this function at a program level, with oversight from the legislative branch. At the highest level of decisionmaking, however, a crosscutting function is required. In the executive branch, OSTP and OMB are the only actors with the ability to play such a sweeping role. Without additional legislative initiatives, however, OSTP is hampered by the powerlessness of its advisory position. And OMB, which has been serving a crosscutting function in the executive branch, is not receptive to incorporating debate and public decisionmaking on these issues. Congress already serves, in part, a crosscutting priority-setting function. However, Congress has traditionally been reticent to set priorities. Suggestions have been made to strengthen Congress’ hand in research decisionmaking through structural

⁶⁵Some agency programs already incorporate these criteria. They are explicitly in use, for example, at the National Science Foundation (NSF) (though not in every program or directorate) and there have been no claims that scientific merit has been compromised. At other agencies, however, these criteria are seen as not as important to the research mission (OTA interviews, spring 1990). At the same time, set-aside programs at NSF and elsewhere underscore the continuing need for “sheltered competitions” for researchers who do not fare well in mainstream disciplinary programs.

⁶⁶Brooks writes: “Today many of the same negative signals that existed in 1971 are again evident. Will science recover to experience a new era of prosperity as it did beginning in the late seventies, or has the day of reckoning that so many predicted finally arrived?” Harvey Brooks, “Can Science Survive in the Modern Age? A Revisit After Twenty Years,” *National Forum*, vol. 71, No. 4, fall 1990, p. 33.

⁶⁷Teich, *op. cit.*, footnote 62, p. 18.

⁶⁸This was, of course, prior to the 1990 budget summit and passage of the Deficit Control Act discussed in ch. 3. The National Academy of Sciences discussion is nevertheless instructive. See National Academy of Sciences, *op. cit.*, footnote 17, pp. 11-16, especially table 2. Also see U.S. General Accounting Office, *U.S. Science and Engineering Base: A Synthesis of Concerns About Budget and Policy Development*, GAO/RCED-87-65 (Washington DC: March 1987), especially pp. 22-56.

change in the budget process, and there has been an evolution toward greater congressional activism. However, Congress may wish to strengthen its current role as the final arbiter of priorities and invest others with the discretion to propose priorities.

Whatever Federal body is designated as having the authority of initial choice, its task should extend at a minimum to the iterative planning and appraisal of accounts that results in: a) limiting the number of (or budget commitment entailed by) megaproject initiatives, and b) making tradeoffs among research fields in the S&T base. For instance, the broad field of the life sciences has received substantial increases in funding over the last 15 years, while other fields have climbed more slowly. Seen as part of the Federal research portfolio, the life sciences could be stabilized in funding, while certain other fields, ranked according to other criteria (e.g., training of students), could be slated for augmented funding. Already included in most research decisionmaking are criteria based first and foremost on scientific merit. OTA suggests that two other criteria could be added to scientific merit. These criteria emphasize planning for the future-strengthening education and human resources, and building regional and institutional capacity. Education, human resources, and regional and institutional capacity are valid outcomes of Federal research investments. Progress

toward achieving national objectives that incorporate these criteria should be monitored with congressional oversight.

Reordering the criteria of choice changes the process and the expectations of returns from the investment in research. Such reconfiguration, perhaps seen most clearly in big science projects, demonstrates how embedded science and technology have become in the myriad needs of the Nation. These initiatives are appropriated by political actors because they are much more than cutting-edge research. They represent ‘‘real money’’—in jobs, industrial development, innovation, trade, and prestige regionally, nationally, and internationally. This is why the constituencies for them are broad and why they remain controversial within their respective research communities years after having been proposed and the down payment made by the Federal Government.

Enhanced priority setting could be the 1990s’ expression of the post-World War II social contract that bound science to government. However, greater priority setting in science is no panacea for the problem of research tiding. It is a partial response to the problem of how the Federal research system can make choices in the coming decade. Another response of comparable urgency—understanding and coping with research expenditures—is discussed in chapter 6.