CHAPTER 6

Estrangement and Reconciliation: 1975–1985

Questions of efficiency and management arise.

In retrospect, it is easy to see that the close rapport between the federal government and the science community would ebb away naturally over the course of time. That rapport emerged from WWII and was sustained by the cold war and the peacetime contributions of science and technology to the quality of American life. But other national cares and worries and a natural tendency to take the science establishment for granted brought about the separation. The reinstatement of a science advisory structure in the executive office of the Ford administration was reassuring, as was President Carter's appointment of a well-respected scientist as his science adviser. Moreover, there was no movement by either executive to make overly large cuts in the science budget despite the need to pay for the Vietnam War and the ongoing cold war. Federally funded science and technology continued to be recognized as a proper responsibility of the government, and the science establishment was regarded as a valuable national asset. Emphasis in Washington in the decade 1975–1985 turned instead to the more pragmatic issue of how to improve the efficiency and effectiveness of the science establishment through better management.

"Bigger bang for the buck" was by no means a new idea. It had been raised frequently since the end of WWII as science budgets increased with such rapidity, but it became more difficult to address as time went on. The issues of management were twofold. Management that was too loose would tolerate funds spent poorly on low quality or ill-directed research and would also limit the support available for superior work. Management that was too tight would frighten scientists away from more daring and often more rewarding programs; it could encourage mediocrity even in those areas of study considered to be most valuable.

Both the executive and legislative branches of the federal government had searched diligently for a plan that would help them stay informed about the science agencies and oversee the science research those agencies funded. For example, one responsibility of President Eisenhower's science adviser and the PSAC was to report on the status of science both in and out of the federal government. To strengthen the institutional base for information, Eisenhower also established the Federal Council for Science and Technology (FCST), whose members were the top-ranking scientific and technical officers of the federal departments and agencies that housed the largest research and development programs. The FCST was chaired by the science adviser and was intended to be the internal equivalent of the PSAC. Neither the FCST nor the PSAC developed as the desired encyclopedic source of information or as a critical force for management. Later, the science adviser to President Kennedy, Jerome Wiesner, became the White House contact point for almost the entire governmental science apparatus, although health research remained largely outside his orbit. In the summer of 1962 the natural expansion of the duties and activities of the science adviser—particularly in science matters related to national security—led to the creation of the Office of Science and Technology (OST), the predecessor of the OSTP, within the executive office of the president. It had its own budget and its own staff and concentrated on the nation's security. It did not serve as an important presence within the federal science enterprise.

Even if these attempts by the White House had worked better than they did, Congress would not have been satisfied, because it had only limited access to the president's science adviser and the OST. At the end of 1963 the House Subcommittee on Science, Research, and Development began hearings on "Government and Science, to identify problems in the relationship of the government and the science establishment, and to assign priorities for dealing with them." This was one of several attempts by Congress to address the subject, all fruitless. Over the years, Congress made more attempts at information gathering and regulation of the science establishment. Like those before, none succeeded.

Why didn't successive presidents and Congresses bite the bullet and create one single agency that would supervise all science in and out of the government, an agency through which all funding would pass? Actually, it was an old idea, one that arose as early as 1884. The growth of the science establishment after WWII revived the idea and led to a proposal for the creation of a Department of Science and Technology that surfaced in the Senate Committee on Government Operations in 1958. At hearings in 1959 a revised bill to that effect was reported favorably to the Senate, but one finds the same Senate committee considering a substitute bill in mid-1962 for the establishment of a Commission (not a Department) on Science and Technology "to bring about better coordination of the science activities of the federal government." The idea of a commission or a department has been revisited since then, but it never gained wide appeal.

It would be quite wrong to view these bits of history as typical examples of Washington's inability to resolve difficult problems. It was then and is still unclear that a single agency in charge, so to speak, of science and technology would benefit either the nation or the science establishment. It was argued then and is still that such centralization would lead inevitably to a huge bureaucracy, a heavy weight on the free-enterprise spirit so necessary to new ideas and new directions in scientific research and technology. This is the same thought so well articulated in Vannevar Bush's original treatise *Science: The Endless Frontier*. Furthermore, even in the early 1960s it was by no means obvious that the AEC, NIH, NSF, and NASA, apart from other federal agencies, could be fitted into a single department of science in the federal government. True, their scientific efforts were interrelated and reinforced one another, but their diversity—one of the reasons they were separate to begin with—tended to make any merger into a single entity extraordinarily difficult and at best unrealistic. It is not surprising that responsible government has been unable since to find a unifying mechanism to manage the science establishment. Improvements in efficiency and effectiveness have to come piecemeal, in consonance with the piecemeal nature of the science establishment itself.

The Department of Energy was created and given the energy responsibilities of all other federal agencies.

Barely two years after the termination of the Atomic Energy Commission, President Carter signed into law a bill creating the Department of Energy (DOE) to replace ERDA. The major provision of that law called for the functions of ERDA to be transferred to the new department, along with those of the Federal Energy Administration, the Federal Power Commission, and a number of generally similar functions in the Interior and Commerce Departments, as well as in Housing and Urban Development and the Interstate Commerce Commission. Responsibility for the naval petroleum reserves of the Department of Defense (DOD) was also placed in the DOE.

The first secretary of energy, James Schlesinger, attempted to organize and structure the DOE to fit the national energy policy of the Carter administration. The department would be led by the secretary, a deputy secretary, and an undersecretary. Energy technologies would be grouped under several assistant secretaries "according to their evolution from research and development through application and commercialization."[1] Basic research was placed in the Office of Energy Research. Individual research and development projects in solar, geothermal, fossil, and nuclear energy were placed under the assistant secretary for energy technology. After scientific and technical feasibility were determined, projects would be transferred to the appropriate assistant secretary for resource applications or for conservation and solar applications, both of whom had specialized expertise in commercialization and energy markets. The assistant secretary for environment would assure that all departmental programs were consistent with environmental and safety laws, regulations, and policies. The assistant secretary for defense programs would inherit responsibility for the nuclear weapons programs.

To allow for the continuity of programs and functions from its predecessors, all activities of the Federal Energy Administration and ERDA without exception were distributed throughout the DOE. In addition, the Federal Energy Regulatory Commission (FERC) was established as an independent agency within the DOE. This five-member commission was made responsible for the licensing and regulation of hydroelectric power projects, regulation of electric utilities, transmission and sale of electric power, transportation and sale of natural gas, and the operation of natural gas and oil pipelines. Regulatory programs not included in FERC were placed under the Economic Regulatory Administration (ERA), one of two administrations created in the department. The ERA took on oil pricing, allocation, and import programs, most of which had been established during the energy crisis of 1973–1974. A second administration within the DOE was the Energy Information Administration, which consolidated the government's many diverse energy data systems to provide comprehensive data and analysis for the president, the Congress, and the DOE.

At the same time that the DOE developed into an enormous bureaucracy with about twenty thousand employees and an annual budget of $10.4 billion, it was left without the committee structure that so ably supported the AEC. The Joint House-Senate Committee on Atomic Energy was abolished, and its responsibilities assigned to several committees in each chamber. The representation in Congress that a joint committee would have provided was not deemed necessary, although the multiplicity of functions and problems that the DOE had inherited were certain to make that representation imperative. Perhaps the idea of direct congressional oversight was seen as inappropriate for a federal department with a secretary sitting in the president's cabinet at its head. Whatever the reason, the DOE, a vastly expanded version of the AEC, was left to shift for itself as far as Congress was concerned. Other committees were also dismantled: the Military Liaison Committee and the General Advisory Committee, both of which served as in-house, constructive critics of the policies and operations of the AEC. Those same policies and operations became the province of the DOE and required the same constructive criticism.

The Joint House and Senate Committee on Atomic Energy had been a stern overseer of AEC decisions. Yet it also provided the necessary liaison between Congress and the public, given that the AEC would otherwise have functioned behind a veil of secrecy. Possibly most valuable of all, the joint committee stood between the AEC and the many congressional interests eager to grasp control of the agency.

The Military Liaison Committee brought to the attention of the all-civilian AEC still another point of view. Military personnel would have to devise the U.S. strategy in which atomic weapons would play a substantial part. They would deliver them to the enemy should the time ever come. Their concern was for adequate production and product efficiency. They represented the pragmatic outlook of the century-old military tradition, still important to national security in 1977.

Finally, the General Advisory Committee (GAC) represented the view of those at the heart of the enterprise, the scientists and engineers who had devised the reactors and the bombs. The GAC served to monitor the technical progress of the enterprise the AEC inherited and to advise it on further U.S. and international nuclear developments. The GAC was the vital technical body in the crucial international negotiations on nuclear weapons test bans and the verification procedures required of any test ban agreement. It insisted that the AEC sponsor applied research in its own laboratories to foster the continuing improvement of atomic weapons and the safe development of power reactors. Moreover, it insisted, with equal emphasis, on basic research in the new sciences that emerged after WWII to ensure that the United States did not fall behind in the science-dominated new world.

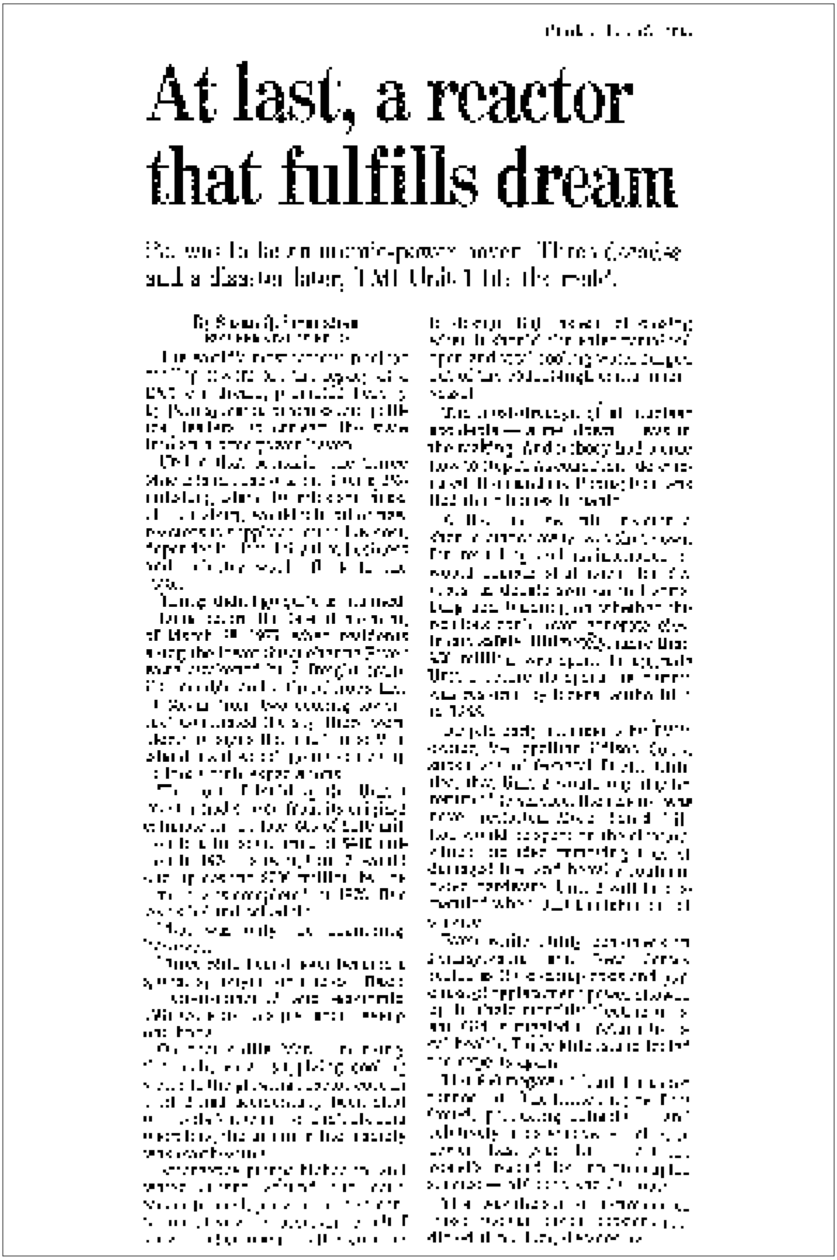
After the first rush of enthusiasm and approval of the creation of the DOE, it became clear that no energy policy would satisfy the contending forces in Washington. On one side were the proponents of a network of privately owned and operated nuclear power reactors as the solution to the nation's energy needs. These reactors, they argued, would in time replace the existing oil-and-coal-powered plants, eliminating much U.S. dependence on imported oil, and would help the country to enjoy a healthier environment. Opponents argued that the cooling required by nuclear power reactors was equally abusive to the environment and that the threat of accidents held hostage all who lived in proximity to a nuclear power plant. They offered natural gas, wood, and solar power as alternatives, at least in part, to oil, coal, and nuclear power.

In March 1979, two years after its birth, the DOE faced an accident at the privately owned and operated Three Mile Island nuclear power plant near Harrisburg, Pennsylvania. This event both fascinated and frightened the U.S. public during the several weeks required to secure the plant. It did not matter that no one was physically injured or exposed to anything more than a very small amount of radiation in the accident. Some months later, the presidential commission on Three Mile Island concluded that the accident was the result of "people-related problems and not equipment problems" and that "except for human failures, the major accident at Three Mile Island would have been a minor incident."[2] Nevertheless, Three Mile Island represented the end of growth for the U.S. nuclear power industry. The unease and outright panic generated in the public by Three Mile Island focused itself on the DOE, since it was the government agency purportedly responsible.

FIGURE 6.1. The Three Mile Island (tmi) power plant ten miles south of Harrisburg, Pennsylvania, showing the two cylindrical containment buildings (*center*) and two of the cooling towers (*background*) of the plant. The history of TMI is described in the newspaper article reproduced in figure 6.2.

Source: T. R. Fehner and Jack M. Holl, *Department of Energy, 1977–1994: A Summary History* (Oak Ridge, Tenn.: Office of Scientific and Technical Information, 1995), p. 27.

President Carter and his secretary of energy issued conflicting statements about the future of nuclear power in the United States. Following Three Mile Island, Secretary Schlesinger restated that the United States had "no real alternative . . . than to make effective use of nuclear power."[3] But the administration's second national energy plan, sent to Congress little more than one month after Three Mile Island, declared that during the past quarter-century the federal government placed a "disproportionate emphasis" on the nuclear production of electricity. President Carter also said that "we cannot shut the door on nuclear power for the United States" but added that once other energy sources were developed, "we can minimize our reliance on nuclear power which is the energy source of last resort."[4] Given this ambivalence, what then would be the DOE policy for nuclear power?



At last, a reactor that fulfills dream

FIGURE 6.2. TMI twenty years later. This July 18, 1998, article in the *Philadelphia Inquirer* recounts the history of the accident at TMI.

Source: Courtesy of the *Philadelphia Inquirer*.

Not long afterward, dissatisfaction with the DOE began to take form. It was directed at DOE involvement in the disappointing record of the nuclear power industry in general but especially Three Mile Island. And there were other reasons. The Carter energy policy, which the DOE was to implement and present to the public, was a curious mixture of inconsistent ideas. On the one hand, it spoke in favor of reliance on undeveloped energy sources such as solar energy and an enormously expensive investment ($88 billion) in a decade-long effort to improve the production of synthetic fuels from coal and shale oil reserves. On the other, the public was exhorted to conserve power in every aspect of its daily life. The ambivalence of the administration's attitude toward the nuclear issue, despite years of investment by the federal government in nuclear power, closed off that option and presumably led to the resignation of Secretary Schlesinger in July 1979, after two years in office.

President Carter quickly selected Charles W. Duncan Jr. to be the second secretary of energy. Duncan had a background in chemical engineering and management and previously had been deputy secretary of defense. The function of the DOE, as he saw it, was to carry out an energy program that was strictly defined by the national objectives set forth by the president. The DOE, he commented, should not be in the energy business. And he emphasized that "market forces must be allowed to regulate the price and allocation of energy resources such as petroleum."[5] Duncan began a tradition more or less faithfully followed by successive secretaries of energy. To streamline management and better delineate responsibilities for accomplishing DOE objectives, he moved the department toward a more traditional organization that managed programs by technologies or fuels. He discarded most of Schlesinger's philosophy and organizational programs.

In the presidential campaign of 1980, Ronald Reagan, the Republican candidate, advocated abolishing the DOE completely. He declared, "The DOE with its multibillion dollar budget had not produced a quart of oil or a lump of coal or anything else in the line of energy."[6] Nevertheless, in the midst of that management turmoil, the DOE continued to be one of the major research agencies in the nation. It owned and contracted out operation of the weapons research laboratories. It was the funding agency for several of the highest energy particle accelerator laboratories in the world, as well as of a host of smaller multidisciplinary laboratories, some with high-intensity research nuclear reactors. The research in those laboratories was unclassified and proposed and carried out independently by university and government scientists with no connection to weapons research. The DOE, through its Office of Energy Research, was thus one of the leading federal funding agencies for university scientists in the United States, both in number and dollar sums. The directors of the Office of Energy Research—equivalent to assistant secretaries in most government departments—were distinguished scientists, and as in the AEC and ERDA they required Senate confirmation for their appointments. They labored long and hard to promote science and technology from which universities, industry, and the entire nation benefited.

The DOE was submerged, however, in a multitude of chores and responsibilities, besides its function as a science agency, chores and responsibilities foisted on it in the interest of more efficient government organization. The secretary of energy, sitting in the president's cabinet, saw those other chores and responsibilities as the main business of the DOE. Each secretary of energy lived in fear of another energy crisis. And each secretary was the arbiter of deep differences of opinion between the administration and Congress concerning the government's proper roles in subsidizing the search for and development of new energy sources. Still, although preoccupied with those issues, the DOE did not desert its obligation to sponsor basic research. It simply gave less significance to that obligation while it concentrated on current business. Despite that difficulty, the science function of the DOE prospered. But as time went on, it acquired a foxhole mentality; it became more bureaucratic and more cautious and tended to micromanage its facilities and the research it supported. This mentality was in part a product of the attitude toward the DOE as a whole shared by the public and Washington. When Reagan advocated the abolition of the department, it was hard not to develop such a mentality.

The National Science Foundation budget passed a billion dollars while its directors came and went after brief service.

During President Carter's single term (1976–1980), he maintained that basic research was both a responsibility of and a wise investment by the federal government. In that period, the NSF's annual budget increased by sizable amounts, but high inflation and stagnant economic growth caused large federal deficits and severely limited real budget gains. Following Carter, the Reagan administration determined to continue and intensify the buildup of

U.S. armed services. This was intended to strain the USSR economically and militarily, since the two superpowers were still in cold war competition. As a result, federal deficits and military expenditures led to further retrenchments in nondefense spending.

In the mid-1970s, growing mistrust of the White House stemming from the Vietnam War and the Watergate affair led to increased congressional examination of the science establishment. Congress was suspicious of the integrity of the science agencies in the executive branch of the government and questioned whether what they were doing was worth the money they were spending. In the case of the NSF, questions started with the titles of grants and descriptions of funded research. Senator William Proxmire, chairman of the subcommittee with jurisdiction over the NSF budget, was especially critical of several grants in anthropology, sociology, and social psychology. He questioned the value to either the public or the government of such projects as "Hitchhiking—A Viable Addition to the Multimodal Transportation System" and "Social Behavior of Alaskan Brown Bears." Once again, the efforts of the NSF in those areas of the social sciences were deemed to be misguided and even harmful. This examination began near the end of the term of NSF director Stever, who vigorously defended the foundation against these congressional misgivings and attempted to counteract the false picture given to the public by emphasis on a few frivolous grant titles. Stever felt, however, that Congress was asking a legitimate question: what was the public getting for its money? He believed that both Congress and the public were entitled to a satisfactory answer and set about to provide it with NSF's grant application data.

Stever's testimony occurred at the time that the House Subcommittee on Science, Research, and Technology opened six days of hearings on the NSF peer review system. Peer review had been criticized ever since the earliest days of the NSF. Congressman John B. Conlan argued that the system was "closed and unaccountable to the scientific community and the Congress" and that "the NSF program managers could get whatever answer they want out of the peer review system to justify their [private] decision to reject or fund a particular proposal."[7]

The subcommittee, chaired by Representative James W. Symington, heard testimony from Congressmen Conlan and Robert Bauman, as well as Stever and his new deputy, Richard C. Atkinson. Conlan accused the NSF program directors of arbitrarily discarding negative reviews and purposely misrepresenting reviewers' comments. He advised the subcommittee "to make the peer review system open and accountable."[8] Bauman berated both Congress and the NSF. He recommended stricter congressional supervision of grant procedures by the authorizing and appropriations committees, in line with an amendment to the NSF bill he had previously put before the House. His amendment required the NSF to submit to Congress every thirty days a list of proposed grant awards, along with their justifications. Either chamber could line-item veto any grant award. Fortunately, the amendment did not pass.

These criticisms, initially prompted by the apparently frivolous titles and descriptions of a few of the NSF's awards, were not themselves frivolous. John Conlan, a graduate of Northwestern University and Harvard Law School, had been a Fulbright scholar in Germany and had taught at the University of Maryland and Arizona State University. His constituency had voiced concern about an educational project in the social sciences called "Man: A Course of Study" (MACOS), initiated under RANN, that the NSF had funded. As a course for fifth graders, it had reached seventeen hundred elementary schools in forty-seven states by 1975, when it was subjected to the charge that it severely distorted basic family values. The MACOS project had initially received favorable review by outside experts. It centered on the social habits of Netsilik Eskimos, but some of these were considered distasteful and ill suited for dissemination to schoolchildren in the lower grades. This concern caused Conlan to look more deeply into NSF grant procedures. Robert Baumann was also stimulated by local concern about MACOS and the expenditure of funds on what appeared to be foolish research.

Director Stever rebutted the charge that program directors manipulated the peer review system to benefit their friends. He insisted that all reviews of grant applications were required, as an agency rule, to be included verbatim in the application records. And he argued that the behavior of the system could be checked directly by assembling data that, when analyzed statistically, would show evidence of bias if it were present or, conversely, if it was not. He agreed, however, that the NSF should spot-check individual cases in the future, which it had not done in the past, and turned to Atkinson to present a statistical analysis of recent NSF grant performance.

Richard Atkinson had been chairman of the psychology department at Stanford University. He published extensively on mathematical models of learning and memory and was well equipped to make a quantitative statistical analysis of NSF data. Atkinson argued from the data that applications submitted by scientists from the top twenty departments in a given field had the same distribution of reviewers, geographically and otherwise, as applications from other schools. Nor did the eminence of the reviewers' universities correlate with the eminence of the universities from which the applications came. He concluded that the data he presented had "confirmed his faith in the fairness of the NSF review process" but that it was necessary for the NSF to collect data over a longer period of time and to explain more fully the working of the peer review system to the public and Congress.[9] Other researchers and administrators from outside the NSF also testified. In the main their views were completely consistent with Atkinson's presentation.

The report of the House Subcommittee stated that the NSF's "peer review evaluation systems appear basically sound" and that the NSF should continue to use them.[10] The report also recommended that the NSF attempt to achieve as much openness in the system as possible, but it firmly declined congressional review of individual research awards.

The Symington subcommittee report did not propose methods to open the peer review system. And, at a time when the nation was moving toward greater public access to the operations of the government as a reaction to Watergate, the NSF's position on the confidentiality of reviews and anonymity of reviewers continued to be questioned by critics who remained unconvinced of the fairness of its procedures. This issue was addressed by the National Academy of Sciences in a study of NSF practices. The NSF provided complete access to its records. Two professors of sociology not affiliated with the NSF did the study. The results were published in two parts, the first in 1978 and the second in 1981. No evidence was found for the existence of an old boys' network, but evidence of the high correlation between review ratings and awards was clearly demonstrated. Moreover, neither the age, race, or gender of the applicant nor his or her previous research accomplishments were found to have a negative influence on either the rating of an application or the probability of it receiving a grant.

To attack the question of personal bias more directly, one study was directed at evaluating the feasibility and promise of anonymous or blind applications, that is, applications in which the name of the author is suppressed. (Some institutions such as symphony orchestras had recently adopted such procedures and begun to hold auditions where the candidate was screened from the reviewers. In this way, the quality of playing and musicianship was the sole basis for judgment.) In its second phase, the study requested that program directors send 150 previously reviewed NSF applications to new reviewers. Half of them were edited to conceal the author's identity; authors were identified in the other half. The results of both surveys indicated that anonymous applications offered no clear advantage to the applicant or the NSF. No bias for or against any group was detected within either the anonymous or the author-identified applications.

Perhaps surprising, though not to experienced reviewers, was the result that about 25 percent of the funding decisions would be reversed if the applications were evaluated by another qualified group of reviewers. That finding attested to the substantive differences of opinion possible concerning the intrinsic value of an application and the importance of the area of science to which the application was directed. Reviewers also differed in their views as to whether the proposed work would be carried out successfully; they looked for originality of both purpose and method in an application. These attributes could be found to some degree by one reviewer and to a lesser degree by another, the former recommending approval and the latter, rejection. One pragmatic way to minimize the effects of these differences was to solicit evaluation from more than just a few reviewers—say, five to ten—which was a major final recommendation of the study. This way, perhaps in the final assessment the most negative review would be canceled out by the most positive one, as is done in judging some athletic competitions, such as figure skating or diving.

Examination of the NSF award system substantiated none of the accusations of bias or subjectivity. The system modified itself, however, in accord with the suggestions for improvement stemming from its self-examination. By 1977 the foundation routinely began sending copies of the reviews of their applications, on a trial basis, to investigators in the biological, behavioral, and social sciences. By 1983 that practice was adopted agencywide. It allowed all applicants to understand the basis for the funding decisions and provided information that made possible modification and resubmission of the original applications. The NSF also established a three-stage procedure for reevaluation of rejected applications but pointed out that funding did not necessarily follow review approval even of the first submission. The decision also took into account other factors, such as availability of funds, the relevance and significance to the NSF program from which the funds would flow, and the need to strengthen research throughout the nation. To reduce the complexity and number of applications, the NSF required that none exceed fifteen pages, and in 1980 it would reward effective, creative researchers with two-year extensions of their three-year grants without additional paperwork. To understand the process better, the NSF introduced an external oversight and review procedure of individual grants every three years. All these improvements became part of the NSF's methods, and a new Office of Audit and Oversight maintained records of activity. This allowed the NSF to justify its practices to Congress and to the public in periodic accountability hearings.

The peer review system was the core of the NSF's grant award procedures. Examinations of the NSF in the decade 1975–1985 modified the process toward greater openness and reception to applicant responses, but the process itself remained fundamentally intact. Despite the criticism of peer review elitism in the NSF, it remained thirty-five years later the fairest, workable method for the selection of good scientific research. No other system has ever been seriously proposed.

In 1981 and 1982 the Reagan administration cut the NSF's budget, especially in the areas of the social sciences and science education, which the Reagan White House believed were more properly supported by the states and the private sector. Nevertheless, in 1983, the third year of Reagan's first term, the NSF's budget passed the one-billion-dollar mark.

With its billion-dollar budget, the NSF was expected to increase innovative technology and engineering in its programs. It was argued that doing so would advance U.S. competitiveness worldwide. The NSF had raised engineering to a separate directorate in 1979, in which applied science programs were included. Two years later, the applied science programs were distributed to other directorates, but engineering was given a place alongside science in the science and engineering education directorate, just when the budget cuts for the education directorate occurred in the Reagan administration. At the same time, the engineering directorate established an office of interdisciplinary research to take advantage of collaborations among several science and engineering disciplines. Soon after that, an advisory committee recommended the creation of engineering research centers, each composed of voluntary groupings of scientists and engineers active in different but related science areas. Those centers were intended to facilitate cross-fertilization and possibly extend to technology transfers between universities and industry. Awards to six centers were made in 1985, ranging from a center for microelectronic robotics systems at the University of California at Santa Barbara to a center for biotechnology process engineering at the Massachusetts Institute of Technology. Apart from the recognized success of the centers, they served also to deflect Congress from submitting bills to create a National Engineering Foundation.

Despite favorable financial and scientific developments, however, a puzzling and questionable trend emerged within the NSF. The length of the terms served by successive NSF directors was alarmingly short. The leadership of the foundation changed frequently during the Nixon and Ford administrations; no director served more than half of a full six-year term: Richard Atkinson, previously deputy director under Stever, served three years, while John B. Slaughter and Edward A. Knapp each served only two years. Stability was restored in 1984 when Reagan appointed Erich Bloch, an engineer and former corporate executive, the first director to come from industry, who served a full six-year term. His immediate predecessors resigned to accept academic positions or to return to their professions. But a more likely explanation of the trend was that the challenge and sense of accomplishment offered by the directorship of the NSF was overshadowed by the stress and aggravation coming from the White House and Congress during that fifteen-year period.

The first linkup in orbit of Soviet and U.S. spacecraft occurred in 1975, and spacecraft shuttling between orbit and Earth became a regular feature of NASA's program; then came the Challenger *disaster.*

The decade 1975–1985 began with an important success for space flight, namely the linkup of a USSR Soyuz spacecraft, already in orbit, with a U.S. *Apollo* spacecraft. In the two days spent together, crewmembers moved between the spacecraft, and the first concrete example of USA-USSR cooperation in space went smoothly. This was a remarkable feat given the cold war. Several years of joint planning, cooperation, and concern for the safety of the astro- and cosmonauts had been required. The people and governments of the world's two superpowers were aware of the broader implications.

The USA-USSR collaboration heralded a new era for NASA. The eleven-and-a-half-year preoccupation with the *Apollo* mission was over, and a self-confident NASA launched a variety of new tasks emphasizing Earth-oriented applications and basic science missions, all with international collaboration.

The space science program, *Viking*, would send missions to the planets of

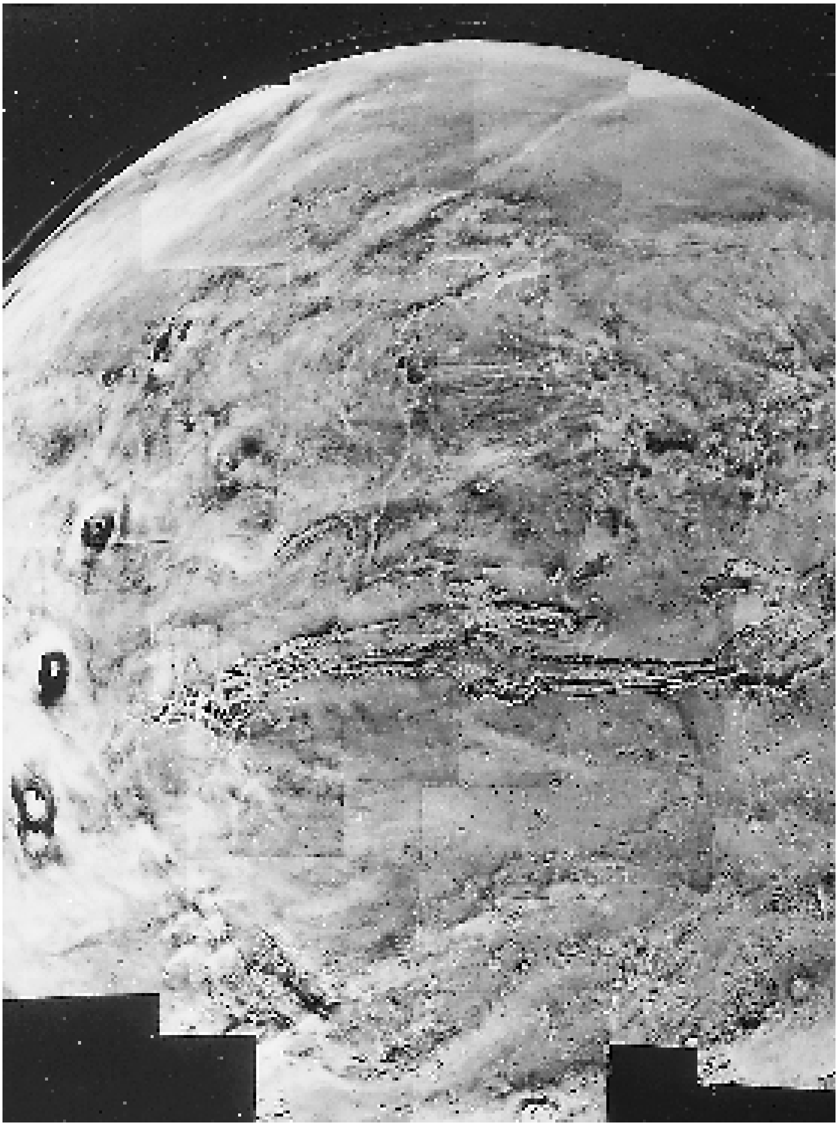


FIGURE 6.3. Viking orbiter montage of 102 photos of Mars in February 1980 shows the Valles Marineris bisecting the planet, a gorge that would stretch from coast to coast of North America; to its left, three large volcanoes poke up through the unusual cloud cover.

Source: R. E. Bilstein, *Orders of Magnitude: A History of the NACA and NASA, 1915–1990*, NASA SP-4406 (Washington, D.C.: NASA, 1990), p. 112.

the solar system, first to the inner planets and later to the more enigmatic outer planets. Mars was the first target. *Viking* deployed four spacecraft in the vicinity of Mars, two orbiters to photograph the surface and serve as communication relay stations and two landers to descend to the Martian surface to measure the atmosphere and climate and search for evidence of rudimentary life forms. The spacecraft went into orbit around the planet in 1976, and subsequently the two landers descended safely to the rock-strewn surface. At that time, the planet was quiescent, but volcanoes half again as high as any on Earth and canyons deeper and longer than Earth's indicated a period several billion years earlier when Mars was active volcanically. Water was located in the frozen polar ice caps, but there was no evidence of life.

Venus was probed in late 1978. Its heavy, thick, hot atmosphere exhibited a high sulfur content with lesser amounts of oxygen and water vapor. The surface appeared to have two major continents and a massive island without an ocean, and there were mountains taller than Earth's Mount Everest.

In 1979 a new spacecraft system, *Voyager*, was sent to Jupiter with two spacecraft. Using Jupiter's gravitational field as a kind of slingshot, the two *Voyager* craft then set off for Saturn, where they arrived about a year and a half later. The mission was extended to a fly-by of Uranus in 1986 and to a planned fly-by of Neptune in 1989, if sufficient control fuel remained.

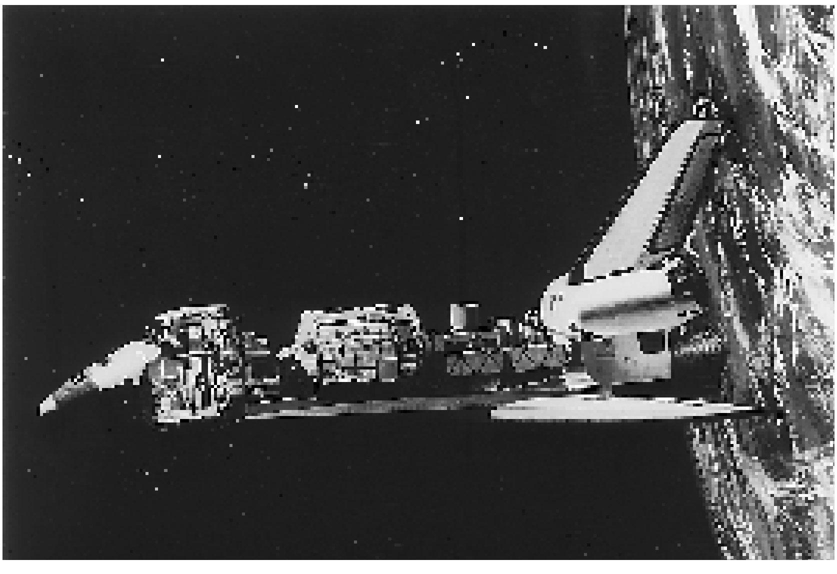
Studies of the Sun continued steadily also. Solar spacecraft received data about the effect of solar radiation on the earth's magnetosphere and the Sun's extraordinary eleven-year cycles. Part of this research was done jointly with the Federal Republic of Germany. Congress mandated a program to study the Earth's upper atmosphere to learn about the effects of gases such as freon on the ozone layer; this occupied NASA during the latter

FIGURE 6.4. *Top*: *Landsat 4* spacecraft photograph of New York City area in 1983. Images from the satellite were combined at the Goddard Space Flight Center. The island of Manhattan is near the center at the confluence of the Hudson and East rivers.

Source: R. E. Bilstein, *Orders of Magnitude: A History of the NACA and NASA, 1915–1990*, NASA SP-4406 (Washington, D.C.: NASA, 1990), p. 97.

Bottom: In the cutaway illustration, the shuttle orbiter is shown with the European Space Agency (ESA) Spacelab as the prime payload. Scientific instruments were mounted on ESA-built pallets arranged in the rear of the shuttle's cargo bay.

Source: R. E. Bilstein, *Orders of Magnitude: A History of the NACA and NASA, 1915–1990*, NASA SP-4406 (Washington, D.C.: NASA, 1990), p. 109.



half of the 1970s. A profile and model of the ozone layer was the result. And *Landsat 3*, launched in 1978, continued the flow of worldwide data on Earth resources, collected mostly for the U.S. Department of Agriculture.

The space agency also remained true to its legacy of aircraft research. More efficient wing construction and improved fuel efficiency in jet engines influenced the construction of jetliners in the early 1980s. Other issues evaluated were aircraft noise during landing and takeoff, bad weather procedures, and control of high-density traffic patterns. The Ames Aeronautical Laboratory at Iowa State University began research on short-haul aircraft, especially vertical takeoff and landing (V/STOL) aircraft. The laboratory also included flight testing and wind tunnel testing. Ames grew into NASA's leading center for helicopter research and contributed to research on tilt rotor aircraft.

In short, soon after the *Apollo* mission was completed, NASA had many irons in the fire. But the largest consumer of the NASA budget and management attention during the late 1970s was the space shuttle program. This $5.2 billion program included new designs of satellites and space flights that would carry academic scientists in addition to astronauts. The shuttles would carry payloads that could be placed in chosen orbits and retrieved. Shuttles could be reused many times. To make the cost manageable and the project salable, the shuttle would be launched vertically, jettison the solid-fuel booster rockets and the liquid hydrogen–liquid oxygen fuel tank, and return to Earth, landing like an airplane. The empty booster rocket casings that parachuted to Earth would be reused, but the fuel tank would burn up on reentry to the atmosphere. The shuttle was designed to carry a payload of sixty-five thousand pounds in orbit at 230 miles above the Earth and to accommodate up to seven crewmembers living and working in the flight deck area for long periods. Smaller payloads would allow orbits up to 690 miles. Nine years after the project had been approved by President Nixon, the shuttle *Columbia* went into orbit 130 miles above Earth for a two-day mission, the first of twenty-four missions by four different shuttles—*Discovery*, *Atlantis*, *Challenger*, and *Columbia*—in the following five years.

At liftoff, a shuttle looked and sounded like an oversized rocket booster with wings. It perched atop a cylindrical liquid propellant tank that fed the trio of main engines mounted in the shuttle's tail. A pair of maneuvering engines plus several small rocket thrusters refined the orbital path as needed during the mission. A shuttle in orbit was much larger than an *Apollo* spacecraft: it had a length of 120 feet and a wingspan of 80 feet. The cargo bay measured 60 feet in length by 15 feet in diameter. Shuttles were equipped with ceramic tiles over their outer surfaces to enable them to withstand the intense heat generated by air friction on reentry into the Earth's atmosphere. Some of these tiles worked loose during the flight of the first shuttle, *Columbia*, and occasioned some anxious moments among the mission controllers. At a speed of Mach 24, the shuttle entered the atmosphere and became enveloped by a blanket of ionized gases emitted by the white-hot tiles that disrupted radio communications. When *Columbia* slowed to Mach 10, it was cool enough to retransmit and reassure mission control that all was well. It was greeted at touchdown by an estimated half-million people who came to observe the "airplane" that had been in Earth orbit.

More than a thousand different payloads were proposed for shuttle space flights. Among them were several that were exclusively scientific, aimed at bringing into reality the observational and measurement capabilities that before had only been dreams.

In the early 1970s, NASA refurbished an airliner christened *Galileo* to carry out a variety of tasks such as observations in infrared astronomy, which at the time was a powerful new technique, photography of the Earth, and meteorological studies. The oceanic companion to *Landsat*, *Seasat*, despite a short life, provided information on the seas that had never before been available. But the shuttle space flights opened the way for complex scientific and communication equipment to be put in space, revisited for maintenance, and, if necessary, returned to Earth for modernization. The agency was on the threshold of new careers in space: a career in information gathering for civilian and military purposes; a career facilitating communication between far-removed points on Earth; and a career in science working in close partnership with academic research scientists. It planned a new family of observatories using shuttle-placed satellites that would carry equipment to study the broad spectrum of radiation from the most distant objects in the sky. A revolution in astronomy and astrophysics was in the making.

But before those space flights could be realized, a seemingly routine flight of the shuttle *Challenger* at the very beginning of 1986 became NASA's second tragedy. Seven Americans, among whom was a New Hampshire high school social studies teacher, Christa McAuliffe, were aboard *Challenger* when, about seventy-three seconds after liftoff, the shuttle exploded, destroying itself and the lives of its crew. It was a horrifying event that also destroyed the perception of take-for-granted success for every mission NASA conducted. Awareness was rekindled that travel into space was a dangerous business, requiring unceasing vigilance and attention to detail.

President Reagan appointed a special commission to conduct a formal enquiry into the tragedy. That report, released a few months later, was highly critical of NASA management and recommended that it be overhauled significantly. Technical evaluation led to the discovery of leaky booster joints that the commission held were the cause of the explosion. Richard Feynman, a Nobel Laureate in physics and member of the commission, showed by a simple experiment during one of the commission meetings that synthetic rubber o-ring seals forced to operate at very low temperatures were the cause of the leaky booster joints. The agency changed the booster design and introduced improvements in the shuttle's main engines, a crew escape system, and changes in other aspects of the shuttle's operations. It would be almost three years before the flight of shuttle *Discovery*, NASA's first manned mission after the loss of *Challenger*, would take place.

The *Challenger* disaster came twenty years after the loss of the lives of three astronauts, Virgil I. Grissom, Edward H. White II, and Roger B. Chaffee, in the flash fire that enveloped a spacecraft on the ground. Once again the issue of the price in human life and dollars that NASA was willing to pay to put astronauts in space was raised. It was argued that humans were not necessary for the scientific, economic, or military programs that were NASA goals because these could be accomplished with much less risk and much less cost by robotic instrumentation. It was claimed that NASA would have done better to invest in the development of robotics and eliminated the fragile human component of space flights. The money saved, said the critics, could have funded a number of programs with high scientific promise that would be either delayed or left undone because of the tragedy.

This issue is still hotly debated today with little prospect of resolution. The space agency functions differently from the other science agencies. It employs a large number of space scientists, astrophysicists, and astronomers. At the same time, it cultivates and supports university scientists, who have benefited enormously from the shuttle program but whose influence on NASA's long-range scientific policy is less than they would like. They find NASA's emphasis on costly nonscientific space flights—particularly on humans in space—to be superfluous and wasteful.

But NASA is in the public eye in everything it does. Ever since the *Apollo* program put men on the moon, NASA has been associated in the public mind with the romantic idea of humans risking themselves in the challenge to explore space, a new but marvelously mysterious and attractive frontier. Even scientists, when asked what the twentieth century will be remembered for, recognize that high on the list—possibly first—will be the human escape from the Earth to visit the moon, the first of many escapes to space that are likely to occur in the next millennium. But NASA has not yet found an effective way to resolve the principal issue raised by scientists: that it does not listen to them as well as it might.

The National Institutes of Health no longer had sufficient funds for the rapidly increasing number of research grant applications; directors were dismissed, and morale plummeted.

The science agencies of the federal government breathed a collective sigh of relief when Nixon resigned his administration, and none was more heartfelt than that of the NIH. The institutes were in the midst of another revolution in biomedical research, particularly in cell biology and recombinant DNA, and in their many startling clinical applications. These stimulated a corresponding increase of competing grant applications by almost a factor of two between 1973 and 1978. This deluge of applications came at a time when budget cutbacks in the overall medical sector were motivated by the perceived danger in the runaway Medicare/Medicaid budgets. The combination of increased demand for grants and decreased resources as a result of high inflation perpetuated the problems that the NIH had experienced in the previous decade. Both Congress and the grant applicant community believed, each for its own reasons, that the fault lay with the NIH management. Once again, Congress set out to provide better management procedures by legislative directives. The applicants, feeling desperate about what they thought to be an outrageously low approval rate, sallied forth, often with vehemently expressed suggestions for changing the system, even including the elimination of peer review.

The one issue on which Congress and applicants were agreed was the distressing complexity of the application procedure. Congress reacted because of complaints from constituents; applicants complained because of personal experience. There was some justification for their criticism. Soon after it was created, the Division of Research Grants established a peer review system intended to provide study section reviewers with all the information they might require to make a quick, informed, and balanced recommendation for each and every grant request. Over time, the applications became significantly more complicated; for example, some required additional information to satisfy federal and state laws governing the use of radioactive materials, the handling of laboratory animals, and myriad other details about the anticipated support to be furnished by the host institution. The number of pages and number of copies of an application needed for the many member review sections had become an expensive, time-consuming burden for both the applicants and the DRG. The DRG tried to lighten the load on applicants by organizing tutorial sessions on application procedures. But it was caught on the horns of a major dilemma. On the one hand, for the purposes of accountability within the NIH and to Congress, elaborate records of the reviews of each application had to be maintained for long periods. Statistical analysis of the numbers of applicants, their institutions, their geographical locations, and so forth, were likewise necessary for the DRG's yearly presentation to Congress. Moreover, the analysis had to be up-to-date to satisfy inquiry at any time by a member of Congress. On the other hand, the DRG was unwilling to prejudice the peer review system by shortcutting any but the most innocuous of its requirements. The result in the restrictive economic climate of the Carter and early Reagan administrations was a stalemate.

The political autonomy of the NIH was compromised when outsiders took sides. Attempts to defend the integrity of the biomedical research establishment led to the Nixon administration's summary firing of the NIH's director, Robert Q. Marston, in 1973 and forced the resignation of another NIH director, Robert S. Stone, in 1975. A decade later, the situation of the NIH was not much better; it was still unstable and unpromising.

In 1982 William F. Raub, the NIH associate director for research and training, had written a strategy paper containing this passage:

> During the last few years, there has been a slowly spreading realization within the biomedical research community that the enterprise not only has stopped growing but actually has begun a contraction of unpredictable duration. Competition for funds from NIH and other sponsors, intensifying year by year, now stands at an unprecedented level, and shows no signs of abating. Never before have so many established investigators faced so much uncertainty about their longevity as active scientists. Never before have so many novices faced so many disincentives to entering or continuing a research career.[11]

FIGURE 6.5. *Top*: Dr. James B. Wyngaarden, director of the NIH, 1982–1989.

Source: Richard Mandel, *A Half Century of Peer Review (1946–1996)* (Alexandria, Va.: Division of Research Grants, National Institutes of Health, Logistic Applications, 1996), p. 180.

Bottom: Dr. Antonia C. Novello, executive secretary, General Medicine B Study Section, 1981–1986; surgeon general, U.S. Public Health Service, 1990–1993.

Source: Richard Mandel, *A Half Century of Peer Review (1946–1996)* (Alexandria, Va.: Division of Research Grants, National Institutes of Health, Logistic Applications, 1996), p. 188.

Five years later, in a briefing on peer review, the NIH's director, James B. Wyngaarden, described the process as follows: "It is no myth that the pressure for greater accountability for the use of federal funds has (1) made the grants application process more burdensome for investigators, university administrators, and members of peer review groups; (2) contributed to additional uncertainty and insecurity in the careers of extramural scientists; (3) created impediments to the creativity and productivity of investigators."[12]

These assessments described the dreary and foreboding attitude of the NIH as it made ready for the last decade of the twentieth century. Nevertheless, on the bright side, there was a commitment to at least five thousand new and renewed investigator-initiated awards at the then-current level of federal funding. Study sections, despite being overworked and understaffed, were reviewing successfully large numbers of applications per year. And from 1980 to 1989 funding for extramural awards increased from $2.8 billion to $3.5 billion, adjusted for inflation. Once again, the NIH overcame its continuing troubles and survived, the peer review system along with it. Moreover, the NIH emerged as the preeminent world institute for the health sciences.

The close of the decade sees tighter management and overall expansion.

The nation that the Carter and Reagan administrations inherited was politically turbulent and economically distraught. A natural reaction of each administration and Congress in those periods was to look critically at various parts of the federal government and, where possible, to modify the principles and practices under which each was operating, much as an individual who has lived through a traumatic experience turns with relief to the job of restoring order and efficiency in his or her own life.

The urge to put its house in order took a different form in each of the federal science agencies. For the AEC, it meant absorption into the Department of Energy. For the NSF, there were increased demands to direct its programs toward greater relevance to national needs. The NSF also undertook self-scrutiny of its award practices to appease congressional demands. Following the history-making achievements of the *Apollo* missions, NASA consolidated its many interests and focused on the space shuttle program, which brought it closer to collaboration with academic scientists. The NIH was caught up in government concern over the rapidly growing cost of medical care and plagued by increasing fiscal and procedural constraints. Within the ranks of the U.S. medical research community, there were strident cries to an overburdened, overexpanded system for more support for more investigators.

Despite this woeful litany, the science establishment managed to expand in the decade 1975–1985. The establishment proved to have the stamina and flexibility to survive the stresses in its evolving relationship with the government.