CHAPTER 3

Courtship: 1945–1955

The end of the war brought joy, relief, and new challenges to the United States. The nation's wartime accomplishments were harbingers of a bright future, and its wealth was at last free to be invested in that future. But the challenges were many, and the ways to meet them unclear. The United States was in a position similar to that of the winner of a very big lottery prize. A new way of life lay within its grasp if—and it was a big if—the unfamiliar new wealth could be safeguarded and invested wisely.

The challenges in science and technology were especially pointed because the close connection between atomic bombs and national security was obvious, and the impact of wartime advances in medicine was already part of U.S. daily life. It was natural to ask what the government could do to preserve and expand the institutions that produced these and other benefits. This question, so much easier to ask than to answer, would occupy Washington for many years after the war. The answer would come as a series of seemingly disconnected actions. These began in 1945 with the report from Bush to President Roosevelt proposing a federally funded foundation dedicated to the support of science in U.S. universities. Washington, however, was occupied with science problems that had carried over from the war because they were directly related to national security. Solutions to those problems involved creation of several science agencies ostensibly different from Bush's proposed foundation that nevertheless functioned in the manner he had foreseen. In this way, almost inadvertently, scientists and the government began to establish a close relationship during the five-year period before the passage of the actual act that created the National Science Foundation.

In the beginning, there was the Bush report: Science: The Endless Frontier

One way or another, a majority of scientists in the United States had been involved in the OSRD or the Manhattan Project during WWII. They experienced the power that science and technology could wield when self-organized and directed toward definite goals, in each instance to produce an object or device to do a specific job. They were well aware, however, that underlying the accomplishments of applied research during wartime was the scientific knowledge acquired from basic research, done mostly for its own sake in the years before WWII. Many who worked to develop radar, for example, knew of experiments that studied the ionosphere—a layer of electrically charged atoms surrounding the earth—by reflecting radio waves from it. These experiments, performed as early as 1925, were motivated by interest in the properties of the earth's atmosphere, not by the idea that they might lead to a method for tracking aircraft more than a decade later. In like fashion, the chemists who did the basic research on the properties of proteins during the twenty-year period before WWII pursued their work because proteins were recognized to be fundamental biochemical compounds, not because they would provide blood and blood derivatives for battlefield transfusions decades later.

Similarly, the achievement of the Manhattan Project was rooted in more than ten years of study of the atomic nucleus, a subscience of physics, pursued for its own sake and thought then to be remote from any practical application. There is a story of the effect of a lecture given at a meeting of the American Physical Society in 1939 by Niels Bohr, the world-famous Danish physicist. Bohr had become aware of the experiments in Germany that demonstrated the fission of uranium, and he readily understood their significance. He described the experiments to the U.S. audience. Before he finished his lecture, so the story goes, physicists in the audience began to rush out of the hall either to telephone their laboratories or to return directly to them to initiate experiments that would test the validity of the information Bohr had just given them. These experiments, which effectively launched the United States into the Manhattan Project, could not have been done so quickly and conclusively without the years of basic research that preceded them. Nor could the work of the Manhattan Project have prospered without that fund of knowledge.

Of course, the work of both the OSRD and the Manhattan Project also involved the acquisition of substantial new knowledge, which illustrates the difficulty of making a clear-cut distinction between applied and basic research, particularly in cases where they merge almost seamlessly into one another. Nevertheless, most scientists agreed that the research done during WWII was for the immediate purpose of reaching practical goals, and in that respect it was applied research. They also agreed that its success rested on the strong foundation of basic research done without practical goals in mind. No one was more aware of this than Vannevar Bush and his colleagues in the OSRD and the Manhattan Project. They recognized that the successful technology of wartime—indeed of any time—depended on access to a flourishing national resource of basic scientific research. This in turn led them to the belief that the federal government had to provide support for basic research in peacetime as it had for applied research in wartime. Although basic research is not easily justified by short-term practical accomplishments, they had become convinced that it was an essential component of modern technological progress and therefore vital to the health and security of the nation in peacetime. The first step taken to bring the government into peacetime science was the Bush report, *Science: The Endless Frontier*, written in response to the following letter from President Roosevelt.

The White House

November 17, 1944

Dear Dr. Bush:

The Office of Scientific Research and Development, of which you are the Director, represents a unique experiment of team-work and cooperation in coordinating scientific research and in applying existing scientific knowledge to the solution of the technical problems paramount in war. Its work has been conducted in the utmost secrecy and carried on without public recognition of any kind: but its tangible results can be found in the communiques coming in from the battlefronts all over the world. Some day the full story of its achievements can be told.

There is, however, no reason why the lessons to be found in this experiment cannot be profitably employed in times of peace. The information, the techniques, and the research experience developed by the Office of Scientific Research and Development and by the thousands of scientists in the universities and in private industry, should be used in the days of peace ahead for the improvement of the national health, the creation of new enterprises bringing new jobs, and the betterment of the national standard of living.

It is with that objective in mind that I would like to have your recommendations on the following four major points:

> First: What can be done, consistent with military security, and with the prior approval of the military authorities, to make known to the world as soon as possible the contributions which have been made during our war effort to scientific knowledge?

The diffusion of such knowledge should help us stimulate new enterprises, provide jobs for our returning service men and other workers, and make possible great strides for the improvement of the national well-being.

> Second: With particular reference to the war of science against disease, what can be done now to organize a program for continuing in the future the work which has been done in medicine and related sciences?

The fact that the annual deaths in this country from one or two diseases alone are in excess of the total number of lives lost by us in battle during this war should make us conscious of the duty we owe future generations.

> Third: What can the Government do now and in the future to aid research activities by public and private organizations: The proper roles of public and of private research, and their interrelation, should be carefully considered.

> Fourth: Can an effective program be proposed for discovering and developing scientific talent in American youth so that the continuing future of scientific research in this country may be assured on a level comparable to what has been done during the war?

New frontiers of the mind are before us, and if they are pioneered with the same vision, boldness, and drive with which we have waged this war we can create a fuller and more fruitful employment and a fuller and more fruitful life. I hope that, after such consultation as you may deem advisable with your associates and others, you can let me have your considered judgment on these matters as soon as convenient—reporting on each when you are ready, rather than waiting for completion of your studies in all.

Very sincerely yours,
Franklin D. Roosevelt

Bush's report and the letter of transmittal that went with it were not delivered to President Roosevelt. Between the president's letter of November 1944 and Bush's report of July 1945, Roosevelt had died of exhaustion and a massive cerebral hemorrhage, on April 12, 1945. Harry S. Truman, essentially a stranger to Bush, assumed the presidency, and his relationship with Bush would prove to be very different from the one Bush had with Roosevelt. Bush's letter, which follows here, outlined his personal frame of reference and the method he selected to prepare the report. He took full responsibility for its recommendations and the mechanics of implementing them.

July 5, 1945

Dear Mr. President:

In a letter dated November 17, 1944, President Roosevelt requested my recommendation on the following points:

(1) What can be done, consistent with military security, and with the prior approval of the military authorities, to make known to the world as soon as possible the contributions which have been made during our war effort to scientific knowledge?

(2) With particular reference to the war of science against disease, what can be done now to organize a program for continuing in the future the work which has been done in medicine and related sciences?

(3) What can the Government do now and in the future to aid research activities by public and private organizations?

(4) Can an effective program be proposed for discovering and developing scientific talent in American youth so that the continuing future of scientific research in this country may be

> assured on a level comparable to what has been done during the war?

It is clear from President Roosevelt's letter that in speaking of science he had in mind the natural sciences, including biology and medicine, and I have so interpreted his questions. Progress in other fields, such as the social sciences and the humanities, is likewise important; but the program for science presented in my report warrants immediate attention.

In seeking answers to President Roosevelt's questions I have had the assistance of distinguished committees specially qualified to advise in respect to these subjects. The committees have given these matters the serious attention they deserve; indeed, they have regarded this as an opportunity to participate in shaping the policy of the country with respect to scientific research. They have had many meetings and submitted formal reports. I have been in close touch with the work of the committees and with their members throughout. I have examined all of the data they assembled and the suggestions they submitted on the points raised in President Roosevelt's letter.

Although the report which I submit herewith is my own, the facts, conclusions, and recommendations are based on the findings of the committees which have studied these questions. Since my report is necessarily brief, I am including as appendices the full reports of the committees.

A single mechanism for implementing the recommendations of the several committees is essential. In proposing such a mechanism I have departed somewhat from the specific recommendations of the committees, but I have since been assured that the plan I am proposing is fully acceptable to the committee members.

The pioneer spirit is still vigorous within this nation. Science offers a largely unexplored hinterland for the pioneer who has the tools for his task. The rewards of such exploration both for the Nation and the individual are great. Scientific progress is one essential key to our security as a nation, to our better health, to more jobs, to a higher standard of living, and to our cultural progress.

Respectfully yours,
V. Bush, Director

Science: The Endless Frontier ***discussed the purpose of a federal science agency and presented specific recommendations for its organization and functions.***

It consisted of six parts whose titles reveal the pattern of Bush's thinking and whose contents were specific enough to provide plans of action: "Introduction," "The War Against Disease," "Science and the Public Welfare," "Renewal of Our Scientific Talent," "A Problem of Scientific Reconversion," and "The Means to the End."

The major recommendation of the report was made in part 6, "The Means to the End": "The federal Government should accept new responsibilities for promoting the creation of new scientific knowledge and the development of scientific talent in our youth." Bush went on to say, "The effective discharge of these responsibilities will require the full attention of some over-all agency devoted to that purpose, and there should be a central point within the Government for a concerted program of assisting scientific research conducted outside of Government." Bush emphasized that "the agency should furnish funds needed to support basic research in the colleges and universities; should coordinate where possible research programs on matters of utmost importance to the national welfare; and should formulate a national policy for the Government toward science." This was his proposal for a national research foundation to be established by Congress.

The membership, functions, and initial level of funding of the foundation were outlined in detail. Responsibility for the foundation would be in the hands of nine "Members of the Foundation" who would not otherwise be connected with the government or represent any special interest. They were to be selected by the president to promote the purposes of the foundation. The members were to serve four-year terms, choose their chairperson annually, and be reimbursed for their expenses only. The responsibilities of the members and of the chief executive officer, the director of the foundation, who was to be selected and appointed by the members, were also specified. The foundation would consist of several professional divisions responsible to the members of the foundation: medical research, natural sciences, national defense, scientific personnel and education, and publications and scientific collaboration. The functions and authority of the divisions were spelled out at length, so that the proposed foundation was more than a skeleton organization. The proposal could be considered as a draft of legislation for Congress.

Bush was careful to note the foundation's need for special authority. He argued that the foundation should be free from the obligation to put its contracts for research out for bids, since the measure of a successful research contract should not be its dollar cost but its contribution to knowledge. He also asserted that the foundation should be free to place its research contracts and grants with institutions whose latent talent or creative atmosphere would afford the promise of research success, as well as with institutions that had demonstrated research capability. As in wartime research sponsored by the OSRD, he proposed that the research sponsored by the foundation be conducted on an actual cost basis, including appropriate overhead, but not for profit.

Bush provided a table of rough estimates of the budgets for the foundation's first and fifth years, after which he expected operation would reach a stable level. In 1945 his dollar amounts were realistic, although a $50 million budget for the Division of Natural Sciences in its fifth year shocked many in Congress. Later, these costs would reflect the effects of progress and inflation.

Bush argued that the creation of the foundation was so important that prompt action by Congress was necessary. Legislation drafted with great care and speed was imperative if the nation was to meet the challenge of science and fully utilize its potential without losing momentum in the

Table 3.1 Budget Estimates for the NRF for the First and Fifth Years

ACTIVITY	MILLIONS OF DOLLARS	
	FIRST YEAR	FIFTH YEAR
Division of Medical Research	5.0	20.0
Division of Natural Sciences	10.0	50.0
Division of National Defense	10.0	20.0
Division of Scientific Personnel and Education	7.0	29.0
Division of Publications and Scientific Collaboration	0.5	1.0
Administration	1.0	2.5
TOTALS	33.5	122.5

Source: Vannevar Bush, Science: The Endless Frontier (Washington, D.C.: U.S. Government Printing Office, 1945), p. 33.

transition from war to peace. Since its organization was not without precedent—it was patterned after the successful National Advisory Committee for Aeronautics (NACA), which had promoted basic research in the problems of flight during the previous thirty years and with which Congress was familiar—such legislation, he averred, should be possible.

If the recommendation to create a national research foundation was the heart of the report, then its soul was contained in the section titled "Five Fundamentals." Indeed, in his two-paragraph request for action by the Congress, Bush made it dramatically clear that "whatever program [for the foundation] is established it is vitally important that it satisfy the Five Fundamentals":

> There are certain basic principles which must underlie the program of Government support for scientific research and education if such support is to be effective and if it is to avoid impairing the very things we seek to foster. These principles are as follows:
>
> (1) Whatever the extent of support may be, there must be stability of funds over a period of years so that long-range programs may be undertaken.
>
> (2) The agency to administer such funds should be composed of citizens selected only on the basis of their interest in and capacity to promote the work of the agency. They should be persons of broad interest in and understanding of the peculiarities of scientific research and education.
>
> (3) The agency should promote research through contracts or grants to organizations outside the federal Government. It should not operate any laboratories of its own.
>
> (4) Support of basic research in the public and private colleges, universities, and research institutes must leave the internal control of policy, personnel, and the method and scope of the research to the institutions themselves. This is of the utmost importance.
>
> (5) While assuring complete independence and freedom for the nature, scope, and methodology of research carried on in the institutions receiving public funds, and while retaining discretion in the allocation of funds among such institutions, the Foundation proposed herein must be responsible to the President and the Congress. Only through such responsibility can we maintain the proper relationship between science and other aspects of a democratic system. The usual controls of audits, reports, budgeting, and the like, should, of course,

> apply to the administrative and fiscal operations of the Foundation, subject, however, to such adjustments in procedure as necessary to meet the special requirements of research.
>
> Basic research is a long-term process—it ceases to be basic if immediate results are expected on short-term support. Methods should therefore be found which will permit the agency to make commitments of funds from current appropriations for programs of five years duration or longer. Continuity and stability of the program and its support may be expected (a) from the growing realization by the Congress of the benefits to the public from scientific research, and (b) from the conviction which will grow among those who conduct research under the auspices of the agency that good quality work will be followed by continuing support.

Two of the recommendations in particular were strikingly new. The first was the notion that the principal function of the new agency should be to furnish funds to support basic research outside of the government. That idea was emphasized throughout the report. For example, in part three, "Science and the Public Welfare," Bush argued in the subsection "Science and Jobs" that the nation would not reach full employment or increase the production of goods and services by standing still, by making the same things made before and selling them at the same or higher prices. The solution, argued Bush, lay in basic research performed without thought of practical ends, which would lead to general knowledge and an understanding of nature and its laws. This would provide the means of answering important practical problems without which the progress of industrial development would eventually stagnate. And Bush was careful to hammer home the points that a peculiarity of basic science has always been the variety of paths that lead to new knowledge and that many of the most important discoveries had come as a result of experiments undertaken with very different purposes in mind.

Second, the report insisted that colleges, universities, and endowed research institutes would be the principal institutions to furnish new scientific knowledge and trained research workers. The reasons for this choice were simple and straightforward: "They are uniquely qualified by tradition and by their special characteristics to carry on basic research; . . . scientists may work in an atmosphere which is relatively free from the adverse pressure of convention, prejudice, or commercial necessity." Bush knew that scientists in those institutions were led to study phenomena by curiosity and instinct that guided them to experiments and observations that occasionally opened vast areas of entirely new information. Within three years of his report, as if designed to validate his claim, university scientists conducted several deeply revealing experiments. One of these involved renewed, more accurate observation of the invisible, high-energy cosmic ray particles that constantly bombard Earth, among which were found elementary particles that had never before been observed. These results, products of basic research with no foreseeable practical application, brought recognition of an entirely new particle universe and began the new subscience of elementary particle physics.

In contrast, the report went on to say, scientists employed in research laboratories that were supported by individual industries—with the aim of improving their products and developing new ones—were usually inhibited from pursuing wide-ranging basic research. Industrial laboratories generally have clearly defined standards and goals and are subject to the constant pressure of commercial necessity, all of which limit the opportunity for scientists to engage in basic science. Furthermore, the report found that the government laboratories of 1945 were not qualified for basic research because most of their research was applied, that is, directed toward results that were important to the operations of the government agencies that supported them and not to the increase of basic, general knowledge.

Although Bush was firmly convinced that basic science research and education would be done best in universities, he knew firsthand the value of applied research in peacetime. He wanted to make sure that industries with laboratories focused on applied research were encouraged to continue and even extend their support of those laboratories. From his own experience, Bush knew the increase in the cost of doing business when a research laboratory was supported by an industry. It was an expensive proposition and, without some compensatory help by the government, likely to be cut back or possibly terminated in difficult financial times. He was also aware that industry, when allocating resources for research, was often dependent on the extent to which the government provided financial help. No one proposed that direct government support be given, but Bush suggested that the Internal Revenue Tax Code should be amended to provide for the deduction of expenditures for research and development as current charges against net income. This would serve as a significant incentive.

Bush directly addressed the question of government research laboratories as he had the question of industrial laboratories. They would not be sources of basic research; nevertheless, government agencies and departments faced practical, technical problems in peacetime that required solutions only systematic research could provide. Many in Washington recognized this after the war. Bush understood that government research directed toward well-stated, well-justified goals would prosper under the watchful eye of Congress without any special pleading.

The government traditionally regarded research and education as two avenues of undesirable entry into the private lives and values of its citizens. Time-honored American insistence on the separation of church and state carried over into the minds of many to a separation of school and state. There was long-standing fear that subsidies by the government would lead to domination by the government and ultimately to a single state religion or a single state educational system. Bush felt that the accomplishments of science and technology in WWII and their promise for peacetime presented a unique opportunity to deal directly with that fear. He realized that his most persuasive argument consisted of the proposed agency that would fund scientific research in universities under the auspices of both the executive and legislative branches of the government, either of which could protect against excessive zeal or misbehavior. *Science: The Endless Frontier* outlined his proposal to do so. It was a distillation of the important lessons of WWII that needed to be taken to heart in peacetime. Those lessons had also been learned by others in the government.

Before any federal science foundation could be created, however, Congress, the Truman administration, and scientists had to find common ground on which to build it. A strength of Bush's report was the detailed nature of the plan it presented, but that was also a weakness because the devil lay in the details. It seemed that everyone in Washington had suggestions for change, some major, some minor, that needed to be aired and debated. But the real reason for a delay of five years before Congress passed and Truman signed the National Science Foundation Act was not quibbles over details. Creation of any science foundation was intimately tied to the fates of the Manhattan Project and the OSRD. Congress knew that it had to attend to the future of the Manhattan Project before it did anything else concerned with the future of science and technology in the United States, and Bush knew it too.

The Atomic Energy Commission and the Joint Committee on Atomic Energy of the House and Senate were created to take control of atomic energy and its future.

Informal planning for the postwar future of the Manhattan Project began well before the end of the war, but the call for legislative action after the surrender of Japan found Congress not yet ready to act. The issues were too numerous and broad, and Congress needed an extended period of preparation to come to grips with them. One difficulty was how to define the limits within which scientific information only—not weapons information—might be exchanged with other nations. A clear statement would make possible a proposal from the president to the United Nations "under which cooperation might replace rivalry in the field of atomic power." That position had been advocated by Stimson before he left office and was taken up by the president and Secretary of State James Byrnes. Some language to that effect was likely to be in any bill that would receive serious consideration, but the scope of the offer to share U.S. knowledge was a matter of dispute.

The first bill off the mark was sponsored by Edwin C. Johnson, the ranking member of the Senate Military Affairs Committee and Andrew J. May, chairman of the corresponding committee in the House. Their bill had been prepared within the War Department at the direction of the secretary of war, Robert P. Patterson, who had succeeded Stimson, but the bill had supporters from a variety of backgrounds including some of the leading wartime science administrators. The proponents of the May-Johnson bill hoped for quick action in the Senate, but Senator Arthur Vandenberg objected to assigning the bill to the Military Affairs Committee on the ground that properly it should be considered by a special joint committee of Senate and House competent on the issues. This stalled the bill in the Senate. The House moved rapidly, however, since May was chairman of his committee, and hearings began as soon as the bill was submitted. Testifying on the first day were Patterson, General Leslie Groves, Bush, and James Conant, after which the committee, to everyone's surprise, went into executive session to consider the bill on the same day.

The testimony and questions from the committee had concentrated on the maintenance of security and on the broad powers of control of every aspect of atomic energy. This emphasis and the brevity of the hearing alienated many scientists in Manhattan Project plants and laboratories. They were leery of the strong influence of the army in the May-Johnson bill and believed that Bush and Conant were misguided in supporting it. These objections brought about a resumption of the hearings, and physicists Leo Szilard, A. H. Compton, and J. R. Oppenheimer gave testimony. Szilard was the only scientist to testify before the May committee during the two days of hearings who had not held an administrative position in the Manhattan Project. He was an individual of considerable originality and insight, a seer into the future, and a clever scientist. One of the first to recognize the possibility of a fission bomb, he had been instrumental in persuading Einstein to write his famous letter to Roosevelt. His advice was sought, but he was neither strongly for nor against the House bill; it was not an arena in which his brilliance would shine. For his part, Compton agreed that control was important but preferred to stress development of atomic energy more than the bill did. Still, he felt that the bill was satisfactory. Oppenheimer, on the other hand, suggested that the bill should define the powers of the future Atomic Energy Commission (AEC) more sharply but observed that he could support it because, as he put it, it would get the army out of atomic energy.

Nevertheless, the May-Johnson bill gathered opponents as time went on. Scientists, public figures, and citizen groups urged its withdrawal to provide an opportunity for more public debate on this critical issue. Mistrust of the main features of the bill—emphasis on secrecy and control and loosely specified limits on the powers of the proposed commission—tended to put people off, as did the effort to rush the bill to passage. Meanwhile, the Senate created an eleven-man select committee of its own with a freshman senator, J. Brien McMahon, as chairman a few days after the House committee hearings began. The White House staff took a second look at the May-Johnson bill, specifically at the limits it placed on the president's authority to control the proposed commission. At earlier hearings, the claim had been made that the bill represented the views of the administration, but the Bureau of the Budget and the Office of War Mobilization and Reconversion (OWMR), speaking for the White House, questioned that claim. They argued that the bill would make the commission essentially independent of executive control, because commission members would serve nine-year terms and only three positions would come up for appointment in any administration. Possibly more important, the president's power to remove a commissioner, even for good cause, was limited. Truman reacted to this information by qualifying the White House commitment to the bill.

By this time, support for the bill even within the May committee had become frayed. Amendments were proposed to have the commission head and other commissioners appointed by the president and for each to serve an indeterminate term at the president's pleasure. To encourage research and development in atomic energy, the bill's restrictions on nonweapon research outside the commission were loosened. These amendments gave rise to a majority report and two minority reports from the committee.

Here was a good opportunity for the select committee of the Senate to enter the debate. However, the chairman, McMahon, first had to overcome the committee's lack of knowledge of the elements of atomic energy and the broader issues involved. He did this by soliciting advice from scientists, choosing as consultant to the committee a veteran of the Manhattan Project, Edward U. Condon, recently appointed director of the National Bureau of Standards, who with others conducted a series of tutorial sessions on atomic energy for the select committee. McMahon found an important ally in James R. Newman, a young lawyer in the OWMR, who began work on the draft of a bill to replace the May-Johnson bill. By the end of 1945, that draft, McMahon's bill, was ready to be released to the public.

Learning from objections to the May-Johnson bill, McMahon and Newman proposed an exclusively civilian commission of five full-time members appointed by the president with consent of the Senate, serving indefinite terms at the pleasure of the president. Four mandated divisions of the commission—research, production, materials, and military applications—would each have a director appointed by the president to ensure adequate attention by the White House.

The bill insisted that production and stockpiling of fissionable material remain strictly within control of the commission, which would, however, be allowed to finance basic research by nongovernment institutions in the physical, biological, and social sciences. There would be a minimum of restriction on the flow of information. Basic scientific information would be completely in the public domain, while related technical information would be published to the extent consistent with national security. The commission would hold all patents relating to the production of fissionable material and weapons, but patents covering devices or processes utilizing atomic energy would be subject to compulsory, nonexclusive licensing to prevent private or government monopoly.

For the most part, these provisions were received favorably in the country. Scientists were pleased by the emphasis on civilian control and the freedom to finance and conduct research and disseminate scientific information. Bush and Conant had spoken in favor of the May-Johnson bill because it limited the commission's freedom to carry out or finance research; they believed that research and the business of operating plants and building weapons were largely incompatible functions. Research, they argued, should be sponsored exclusively by an agency dedicated to that purpose, like the National Science Foundation, also under consideration in the Senate at that time. On the other hand, the production of fissionable materials and military applications should be the full-time province of the commission. Many scientists and other professionals, however, were frightened by a commission closed off from the scientific community but open to the military. Public interest in atomic energy and a bill to regulate it was heightened by the activities of newly formed organizations of scientists, particularly the Federation of Atomic Scientists, which rallied women's groups, labor unions, and religious and civic organizations to speak out in favor of the mixture of civilian administration and scientific freedom that scientists and educators found in the McMahon bill.

By February 1946 both the May-Johnson and McMahon bills had gone through a series of modifications, and each appeared in Congress in near-final form. They were compared in public debate by Henry Wallace, Truman's secretary of commerce and Roosevelt's former vice president. Wallace had been a member of Roosevelt's Policy Committee for the Manhattan Project and was in a position to speak with authority on most aspects of atomic energy, particularly the economic implications for peacetime. He testified before the select committee in support of the McMahon bill, insisting that the ultimate international control of atomic energy provided for in the bill was the only alternative to an atomic arms race. For this reason he supported civilian control of the commission, free exchange of basic scientific information, and early development of methods for international inspection of atomic energy activities. Wallace approved of the bill's recommendation to foster the peaceful uses of atomic energy and its patent provisions.

In contrast, Wallace found the May-Johnson bill to be inconsistent with the administration's avowed policy of eventual international control of atomic energy. The bill was intended to promote military development of atomic energy. It would place sweeping powers in the only full-time administrator of the commission (all others being part-time), who might be a military officer, subject to removal by the president only with difficulty. Finally, Wallace said, the May-Johnson bill placed far too little emphasis on the development of peaceful uses of atomic energy.

The other voice from the president's cabinet was that of Secretary of War Patterson. He stressed the Pentagon's objections to the McMahon bill's investment of exclusive control of production and ownership of all fissionable materials in the commission. This, Patterson stated, would inevitably lead to serious complication and confusion in the use of atomic weapons in an emergency. The Pentagon was also concerned by the failure of the bill to provide for a general manager of the commission to ensure efficiency of its operations and by the inadequate penalties for careless or intentional mishandling of weapons information.

Patterson was strongly opposed to the exclusion of the armed forces from the commission since it would leave the military with the responsibility for delivering atomic weapons but without authority to produce or control them. This issue of military exclusion was not easily resolved. On the one hand, civilian control was seen in Congress as likely to be less efficient and less capable of managing atomic weapons and maintaining secrecy than was military control. On the other hand, six months after the end of the war, the country at large was no longer convinced of the virtues of the military. A strong military voice in atomic energy was less appealing than it might have been earlier. Then, overnight, the situation changed drastically when news came from Canada that bomb secrets had reached the Russian embassy through British physicist Alan Nunn May. When he visited the Chicago laboratory in 1944 as a member of a Canadian atomic energy delegation, he had learned about the research there and about the production of fissionable material at Hanford. Perhaps for the first time, the U.S. public was alerted to the intensity of Soviet expansionist aims. These would separate Eastern and Western Europe by what Winston Churchill called the Iron Curtain and create a long-lasting period of deep contention for physical and ideological global dominance between the United States and the USSR that Churchill described as the cold war. Patterson's cautionary comments grew more meaningful, and General Groves's testimony on security before the McMahon committee made a deep impression. There was nothing that could be done about Russian espionage, but public opinion that had been moving away from inclusion of the military in the affairs of the commission turned back toward that direction.

Opinion on the civilian-military question was not divided along narrow partisan lines. Dwight Eisenhower, the army chief of staff and Chester Nimitz, the chief of naval operations, emphasized their "desire to establish civilian control to the last possible degree of national safety" but also believed that the military services should have a strong voice in matters of national security.[1] Senator Vandenberg, after hearing General Groves, denounced military exclusion in favor of military review of the decisions of a civilian commission. He proposed an amendment to the McMahon bill by which the commission would have absolute freedom to make any decision it wished but the army chief of staff would review any action on military questions. A majority of scientists remained committed to military exclusion as did a majority of citizens' groups, but not all spoke against the Vandenberg amendment.

The differences persisted into April 1946. By then, much of the emotional heat had been dissipated in the stalemate that had developed. May-Johnson had fallen behind because it overemphasized the military and weapons aspects of atomic energy, and McMahon's bill was perceived as too liberal in granting power to a civilian commission without assurance that it could conduct business efficiently and provide for national security. This led to further modification of the McMahon bill: the five civilian members of the commission were retained but in staggered five-year terms, again appointed by the president; they were to be supplemented by a general manager also appointed by the president, which would allow the directors of the divisions to be chosen by the commission.

Those revisions were important, but additional substantive changes that took into account concerns of the scientific community and the military were also introduced. Three mandated committees were incorporated into the bill: a general advisory committee of scientists and engineers for technical matters, a military liaison committee for military matters, and a joint House-Senate committee on atomic energy.

The General Advisory Committee (GAC) not only gave the scientists a consultative voice in atomic energy affairs, but gave the commission a powerful arm to inspect, criticize, and evaluate technical progress under the management of the commission. The GAC would neither vote nor attend commission meetings, but its advice would be sought and, in the event of a severe difference with the commission, the GAC could always appeal to the Joint Committee or even to the president.

The Military Liaison Committee (MLC) was intended to give the armed services the highest-level information on atomic energy. It would give them

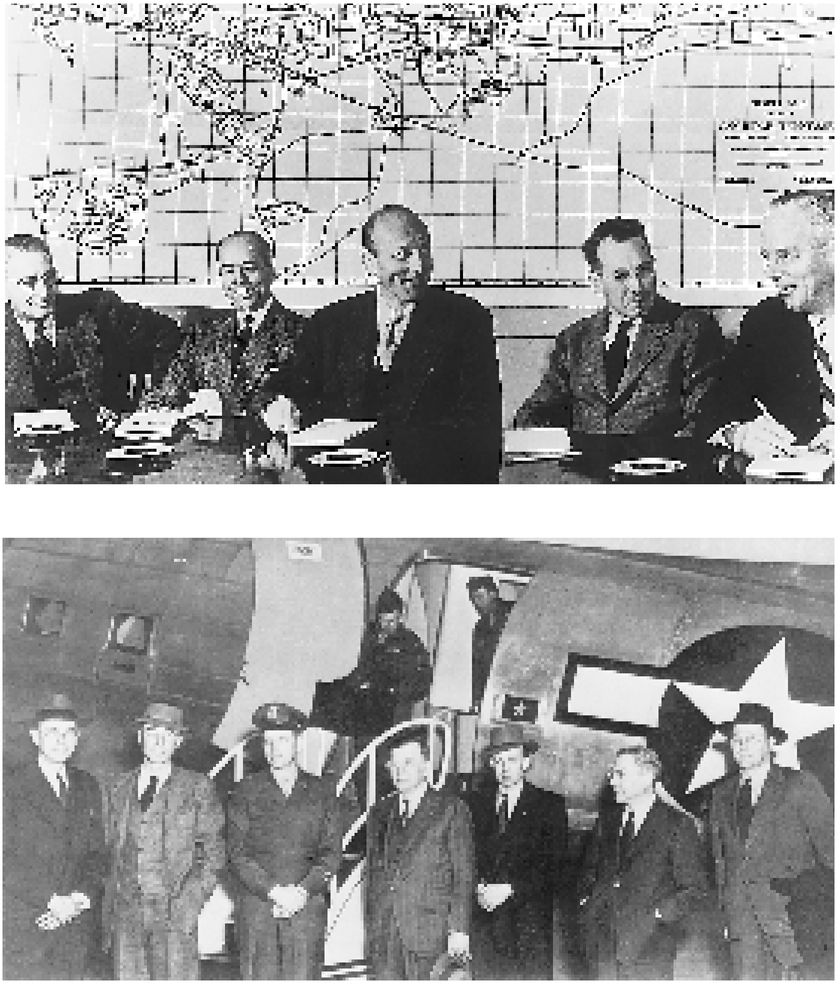

FIGURE 3.1. *Top*: The first AEC Commission. *Left to right*: William Waymack, Lewis Strauss, David Lilienthal (chairman), Robert Bacher, and Sumner Pike.

Source: Department of Energy.

Bottom: Members of the General Advisory Committee visited Los Alamos. The picture was taken shortly after the landing at the Santa Fe, New Mexico, airport, April 3, 1947. *Left to right*: James B. Conant, Robert Oppenheimer (chairman), General James McCormack, Hartley Rowe, John H. Manley, Isadore I. Rabi, and Roger S. Warner. Manley was the committee's executive secretary. McCormack and Warner were members of the commission's staff.

Source: R. G. Hewlett and Francis Duncan, *Atomic Shield: A History of the U.S. Atomic Energy Commission*, vol. 2, *1947/1952* (Washington, D.C.: U.S. Atomic Energy Commission, 1972), p. 46.

the opportunity to be heard by the commission and to guide the commission on military matters. Creation of the MLC did not specifically enlarge the role of the armed services in military applications of atomic energy—the MLC would not vote or attend commission meetings—but it reduced the exclusive control of the commission.

The provision in the bill that stated the commission alone would own and operate its plants was changed to specify ownership only and allow the commission to continue the system of contractor operation of the plants that had originated with the Manhattan Project. Similarly, wording that gave exclusive control of the weapons stockpile to the commission was changed to state that the president might direct the commission to deliver such quantities of weapons to the armed services as were deemed necessary. Presumably, the president might do so without the pressure of an immediate emergency. Changes in the section on information control were introduced because the Canadian spy case had heightened the fear of espionage. The distinction between basic scientific and related technical information was eliminated, and the emphasis was shifted to restrict transfer of information concerning military applications. Patent provisions were also extended, and penalties for violations to benefit any foreign nation were stiffened.

Finally, a compromise that went to the heart of the civilian-military issue was inserted to change Senator Vandenberg's amendment. Rather than have the president appoint members of the MLC, the senator proposed that the armed service secretaries do so, which would create a direct line of appeal of commission decisions by the armed services.

With these many changes, the bill went to the full Senate as a triumph of compromise. Secretary Patterson, General Groves, and the armed service heads, General Eisenhower and Admiral Nimitz, regarded the responsibility and authority of the military to be reasonable and satisfactory. Citizen groups that were determined to have civilian control of atomic energy were satisfied and even relieved to have a clearly contained and defined role for the military. Scientists saw two important safeguards firmly in place: The presence of the GAC gave reassurance that understanding and appreciation of the scientific aspects of atomic energy would be available to the commission, the MLC, and the Joint Committee, thus new ideas and new actions would be less likely to get lost as the commission became immersed in managing the atomic energy enterprise. Equally important to the scientists was their freedom and the freedom of the commission to perform research that would take them beyond the scientific threshold they had already crossed. By maintaining leadership in basic research, the United States would retain its leadership and security in the atomic age.

The Senate passed the McMahon bill unanimously on June 1, 1946. No such action could be expected in the House. A protectionist flavor was stronger there, and many members preferred the May-Johnson bill, although they realized that it could not be resurrected. Despite this, the House adopted the McMahon bill with few changes on July 20, by a vote of 265 to 79. It then went to the Senate-House conference committee, from which it emerged essentially in the form it had when it left the Senate. On July 26 both houses accepted the McMahon bill with minor modifications, and six days later the Senate Special Committee witnessed the signing of the Atomic Energy Act of 1946 by the president.

There were many who helped bring to pass the Atomic Energy Act, but the contributions of a few were especially important. Foremost were the bill's authors, Senator McMahon and James R. Newman, who kept their focus on the vital issues and refused to be discouraged. Byron S. Miller, a lawyer who had worked during the war in the Office of Price Administration, might also be credited as an author of the bill for the interviews he conducted and the drafts of the bill he helped to write. Senator Vandenberg was a valuable resource in the Congress, and President Truman entered the argument when it was most appropriate.

CREATION OF BASIC RESEARCH LABORATORIES

The next order of business was for the president to appoint the five AEC commissioners. In the autumn of 1946, however, General Groves continued as the head of the Manhattan Project, the fabric of which, to his dismay, was beginning to unravel. The isotope separation plants were operating more efficiently than before, but demand for their products was on hold. The technical challenge had faded, and everyone, managers and workers, was eager to return to peacetime work. The du Pont Company, for instance, notified the Manhattan Project that it intended soon to relinquish its management responsibilities at Hanford, and Groves was unable to dissuade them.

The situation at Los Alamos was even more serious. The state of high excitement and urgency had passed, and scientists and their families were in a hurry to return to the universities, as were students seeking advanced degrees and jobs. Engineers and technicians, finding themselves in demand by the resurgent peacetime industry, also left the laboratory. The uncertainty of the future of atomic energy during the many months of debate over the Atomic Energy Act had even frightened away some who saw new scientific challenges at Los Alamos. The result was a laboratory with a sadly reduced capability.

Nevertheless, Groves's responsibility to the Manhattan Project appeared to be intact for the near term, and he set about repairing the setbacks with energy and decisiveness. He approached the General Electric Company, which had expressed interest in constructing electric power plants driven by reactors, striking a deal by which GE would take over management of Hanford, with its two plutonium-generating, high-power reactors. In return, the Manhattan Project agreed to finance a laboratory in Schenectady, New York, which would be government owned but used by GE for fundamental research and development of reactors. In addition, Groves persuaded A. H. Compton, who had recently become chancellor of Washington University, in St. Louis, Missouri, to remain as head of atomic energy research in the Chicago area and to oversee a program of reactor development at the Argonne Laboratory in that area, under its director, Walter Zinn.

Although criticized for his strict military outlook, Groves had learned the lesson that research pays. He used the opportunity given him by the

FIGURE 3.2. *Opposite page top*: Laboratory directors of the AEC with the general manager, January 18, 1947. *Front row, left to right*: Frank H. Spedding, Ames, Iowa; Carroll Wilson (general manager); and C. Guy Suits, Knolls. *Standing, left to right*: Ernest O. Lawrence, Berkeley; Philip M. Morse, Brookhaven; Eugene P. Wigner, Clinton; and Walter H. Zinn, Argonnne.

Source: R. G. Hewlett and Francis Duncan, *Atomic Shield: A History of the U.S. Atomic Energy Commission*, vol. 2, *1947/1952* (Washington, D.C.: U.S. Atomic Energy Commission, 1972), p. 110.

Opposite page bottom: David Lilienthal with members of the AEC testifying at hearings before the Joint Committsee on Atomic Energy of the House and Senate in the spring of 1949. At these hearings, Senator Bourke B. Hickenlooper (Iowa) charged the AEC with "incredible mismanagement," but the Joint Committee's eighty-seven-page majority report vindicated the Lilienthal commission.

Source: R. G. Hewlett and Francis Duncan, *Atomic Shield: A History of the U.S. Atomic Energy Commission*, vol. 2, *1947/1952* (Washington, D.C.: U.S. Atomic Energy Commission, 1972), p. 270.

time interval between the Manhattan Project and the AEC to encourage the establishment of research laboratories. In addition to funding an R&D laboratory for the GE Company and a research program for the Argonne Laboratory, he accepted Ernest Lawrence's request for a subsidy of $170,000 to support completion of the 184-inch-diameter particle accelerator on the Berkeley campus of the University of California. It proved to be one of the first uses of government funds for basic physics research in a university.

Groves also recognized that renewed investment in the Los Alamos Laboratory was necessary if it was to survive as the vital force it had been during the war. The laboratory was ideally located for people fond of living near the high desert, but it needed to make the transition to a peacetime community to attract scientists and their families. Groves arranged for the construction of wells, pipelines, and pumping stations to bring water to a central station and eliminate the queues that resulted when existing water lines had frozen. He persuaded Patterson—Los Alamos was still an army base—to authorize three hundred units of permanent housing. And he directed the new laboratory director, Norris Bradbury, to develop a master plan for the laboratory itself, to replace its previous hodgepodge construction. These actions represented a vote of confidence in the future of the laboratory and continuing research in atomic energy.

Although the laboratory's staff was spread thin, given the responsibility for bomb tests in the South Pacific and work on improved detonation of early-type atomic weapons, the depleted Theory Division continued to study the prospect of atomic bombs made of hydrogen instead of uranium and plutonium. In September 1946 the reported promise of this so-called thermonuclear weapon began to influence planning for the future of the laboratory. At the same time, Groves took steps to move many of the routine operations still conducted at Los Alamos elsewhere, recognizing that the military had to take responsibility for straightforward operations still conducted at the laboratory in order to free the laboratory to do research that only its specialized personnel were trained to do. The scientists welcomed relief from these tasks as they channeled greater effort into improved fission bombs and the possibility of hydrogen bombs.

In early 1946 Groves had appointed an advisory committee on research and development to help him prepare a 1947 budget for the Manhattan Project. The committee consisted of seven members who had figured prominently in science during the war, most of whom had since returned to the university. They recommended that the Manhattan Project expand its activities to include a larger number of institutions, with the aim of developing fissionable materials and power. The committee proposed establishing two laboratories, one at Argonne and one somewhere in the northeastern states. These laboratories, each managed by a board of directors chosen primarily from universities, would be channels through which federal funds would flow to support nuclear research. Creation of a national laboratory in the West was also recommended for a later time. The committee recommended setting the fiscal year 1947 budget for research and development at $20 to $40 million. It also endorsed the distribution of radioisotopes for medical research, particularly for diagnostic purposes and cancer treatment, and recommended that nuclear physics research at Berkeley continue to be subsidized with the understanding that Berkeley—possibly as a special type of national laboratory—would assist other U.S. institutions in the design and construction of accelerators.

The budget that was finally submitted went far beyond the recommendations of the advisory committee. It allocated $72.4 million for research, with 68 percent for construction: $20 million for Clinton at Oak Ridge, Tennessee; $10 million for the laboratory GE was to operate at Schenectady; $9.4 million for the proposed northeastern national laboratory; $5 million for Argonne; and $2.5 million for miscellaneous laboratory construction. The remaining $23 million were reserved for operating expenses for those laboratories and for nine other institutions, all universities except one. The Military Appropriation Act of July 1946 contained this budget and was the first substantial appropriation of federal funds for atomic research in peacetime outside the Manhattan Project.

Groves moved rapidly to create the national laboratories proposed by his advisory committee. The University of Chicago accepted a contract to operate Argonne National Laboratory, and the university and the Manhattan Project approved a statement defining its organization and operating policy that had been drafted by twenty-four participating midwestern institutions. The New York State Board of Regents chartered nine private universities in the Northeast as the Associated Universities, Inc., to manage the Brookhaven National Laboratory, a new fundamental science research laboratory in Long Island, New York. Elsewhere, fourteen universities spread in an arc from the District of Columbia to Texas formed the Oak Ridge Institute of Nuclear Studies to use the facilities of the Clinton Laboratories for basic research, the equivalent of a national laboratory in the Southeast.

The Manhattan Project approved the institute, and it received a Tennessee charter of incorporation.

While this plan for a network of federally supported research institutions was undertaken, on October 28, 1946, the president appointed the five AEC commissioners proposed in the McMahon bill, and on December 31 the commission formally took control of the Manhattan Project, subject to the conditions of the Atomic Energy Act and the oversight of the Joint House and Senate Committee on Atomic Energy. The chairman of the commission was David E. Lilienthal, who had served fifteen years on the Wisconsin Public Service Utility Commission and since 1933 had been chairman of the Tennessee Valley Authority, the federal electric power utility that had been an integral part of Roosevelt's New Deal program of rural electrification. His knowledge of the ways of the federal government and of power technology on a national scale, combined with his leadership ability, made him a natural choice to head the commission. Appointed with him were Sumner T. Pike, a former member of the Securities and Exchange Commission; Lewis Strauss, an independently wealthy financier, who as a navy reservist rose to the rank of admiral and adviser to the secretary of the navy; William W. Waymack, editor of the *Des Moines Register and Tribune*, who was serving as a public director of the Federal Reserve Bank of Chicago and had received a Pulitzer Prize for editorial writing; and Robert F. Bacher, one of the key scientists at Los Alamos, a professor of physics at Cornell University and aide to U.S. representatives to the United Nations. President Truman proudly claimed that he had chosen the five members without knowledge of their political affiliations, and in fact all but the chairman—who was an independent—were Republicans. The commission realized that they could sustain public approval for the difficult decisions they would face only if they remained and were perceived as nonpolitical.

The commission began doing business as proprietors of the atomic energy enterprise without offices or staff of their own, although they did receive a million dollars from the Treasury to pay expenses. Consequently, they were initially dependent on the plans for the future inherited from General Groves and the Manhattan Project staff. The commission had the authority to replace contractors and employees in any commission plant or laboratory with its own contractors and people and thereby to take active control in addition to ownership. Instead, it chose to retain the army's system for dealing with the plants and laboratories it owned and to work with the thousands of scientists, engineers, and technicians already in place in

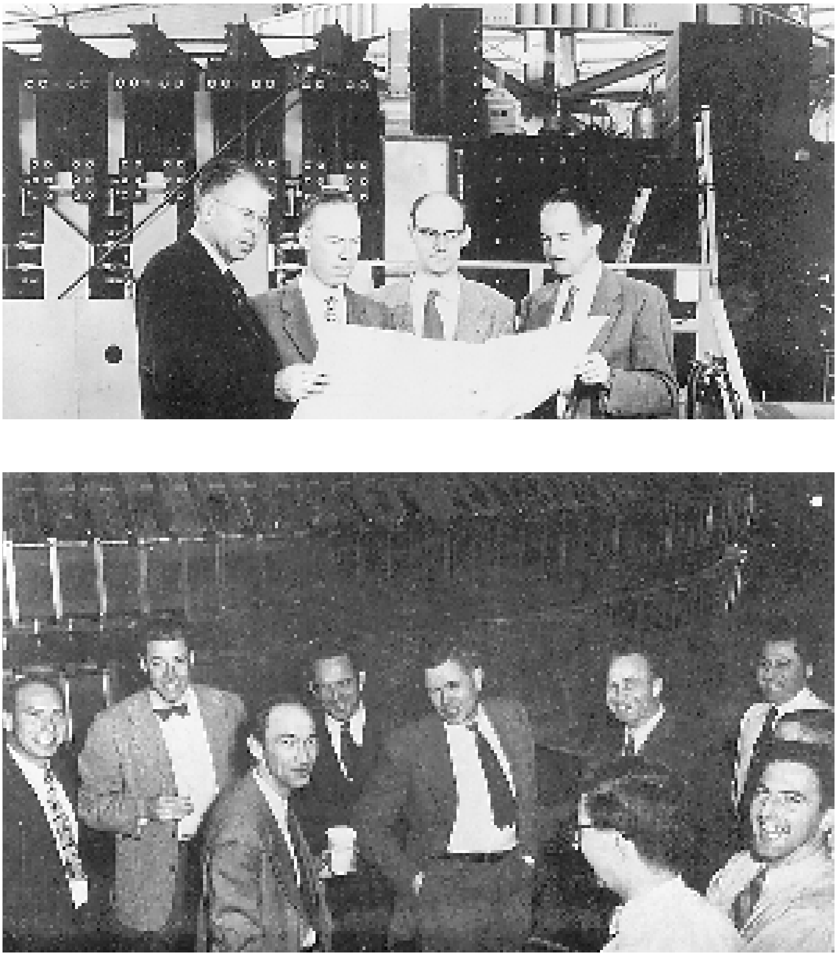

FIGURE 3.3. *Top*: Builders of the Bevatron. Standing in front of the giant particle accelerator at Berkeley are the scientists principally responsible for its design and construction. *Left to right*: Ernest O. Lawrence, William M. Brobeck, Edward J. Lofgren, and Edward M. McMillan.

Source: R. G. Hewlett and Francis Duncan, *Atomic Shield: A History of the U.S. Atomic Energy Commission*, vol. 2, *1947/1952* (Washington, D.C.: U.S. Atomic Energy Commission, 1972), p. 302.

Bottom: Celebrating a milestone in the construction of the Cosmotron, the particle accelerator at the Brookhaven National Laboratory at Long Island, New York, in December 1950. G. Kenneth Green stands in the center of the group. *Left to right around the circle*: Abraham Wise, George B. Collins, Charles H. Keenan, Gerald F. Tape, M. Stanley Livingston, Martin Plotkin, Lyle Smith (mostly hidden), Joseph Logue, and Irving L. Polk.

Source: R. G. Hewlett and Francis Duncan, *Atomic Shield: A History of the U.S. Atomic Energy Commission*, vol. 2, *1947/1952* (Washington, D.C.: U.S. Atomic Energy Commission, 1972), facing p. 302.

private industry and universities. As a result, the administration of the AEC remained decentralized and flexible.

The commission did not immediately agree with General Groves on decisions involving long-range commitments, particularly the new basic research laboratories, for which there was no precedent. It inspected the organization and location of the proposed General Electric laboratory and the national laboratories at Argonne and Brookhaven, for which funds had already been appropriated, before finally confirming the earlier decisions. This illustrated the boldness with which General Groves could act within the relatively loose constraints of the War Department compared with the initial caution of the commission. The commission found itself on firmer ground after the president appointed a general advisory committee (GAC) consisting of nine experienced leaders in the wartime scientific effort, three of whom had won or would win Nobel Prizes. The initial recommendation of the GAC, with Oppenheimer as chairman, underscored the need to compete in a dangerous international environment. Furthermore, the GAC recommended extending the policy of building new basic research laboratories that General Groves and his advisory committee had advocated, arguing convincingly that investments should be made not only in the bricks and mortar of new laboratories but also in new equipment—especially higher-energy particle accelerators—that would permit physicists to expand the boundaries of their science.

Thus, in the history of U.S. atomic energy, the AEC became the principal funding agency of university and industrial science and technology. It did so not only in nuclear physics and chemistry but also in areas of science peripheral to and in many instances far from those subjects. Because ideas are not enclosed by administrative boundaries, the AEC found itself supporting research in biology, nuclear medicine, and materials science, in addition to the core research in high energy physics.

THE HYDROGEN BOMB

Three years after passage of the Atomic Energy Act of 1946, the United States procured evidence that the USSR had detonated an atomic bomb. This was not completely unexpected because U.S. scientists expected that Soviet scientists would need about the same length of time to achieve that goal as they had. Nevertheless, it came as a shock to realize that the U.S. monopoly of atomic weapons was at an end. Russian possession of the bomb was viewed as an immediate threat to U.S. security. Few in the government believed that the USSR, dominated as it was by Stalin, would refrain from an atomic attack on the United States.

The AEC responded by compiling a detailed inventory of the U.S. atomic arsenal. Was it sufficient in number and power to ensure devastation of the USSR in the event of a first strike against the United States? A partial answer to this question could be made by listing the quantity of fissionable material produced each week and the number of atomic weapons ready for delivery. Answers, however, were also needed to questions concerning more powerful weapons than the WWII type and also small atomic weapons for tactical use. The joint committee, chaired by Senator McMahon, and the MLC pressed for answers to those questions. In particular, they urged progress on the much more powerful hydrogen bomb then under study by the rechristened Theoretical Division at Los Alamos.

That study indicated that a bomb made of hydrogen would have as much as one thousand times the explosive power of the atomic bombs of WWII. The principle on which the idea of the new bomb was based—the fusion or joining of hydrogen nuclei, the same process that generates the energy in stars—was recognized as a thermonuclear reaction, that is, a nuclear reaction brought about by extreme heating of nuclei (as in the core of a star) that would give rise to the emission of a large quantity of energy.

Before the Soviets exploded a uranium weapon, there was no compelling motivation to explore the possibility of a thermonuclear weapon and there were practical reasons to refrain from doing so. The issues involved in proceeding toward a hydrogen bomb were essentially the same as those that had been faced in deciding to make a uranium bomb in 1940. Research on hydrogen bombs would be enormously expensive and impede work on improving fission bombs, just as it was anticipated in 1940 that research on a uranium bomb would use scarce resources and slow the improvement of conventional weapons.

The Soviet explosion of a fission bomb changed everything. Once again, Americans were concerned about being left behind. Scientists at Los Alamos began to explore more intensively the technical questions involved in constructing a fusion weapon. As they reported on their progress, the idea of a bomb of that extraordinary power captured the imagination of the members of the joint committee and the MLC. They saw it as a way to reassert U.S. ascendancy in atomic weapons. Within the AEC and the GAC,

Table 3.2 Members of the U.S. Atomic Energy Commission and the General Advisory Committee

U.S. ATOMIC ENERGY COMMISSION	
David E. Lilienthal, chairman	November 1, 1946–February 15, 1950
Robert F. Bacher	November 1, 1946–May 10, 1949
Sumner T. Pike	October 31, 1946–December 15, 1951
William W. Waymack	November 5, 1946–December 21, 1948
Lewis L. Strauss	November 12, 1946–April 15, 1950
Henry D. Smyth	May 30, 1949–September 30, 1954
Gordon E. Dean	May 24, 1949–June 30, 1953
chairman	July 11, 1950–June 30, 1953
Thomas E. Murray	May 9, 1950–June 30, 1957
T. Keith Glennan	October 2, 1950–November 1, 1952
Eugene M. Zuckert	February 25, 1952–June 30, 1954
GENERAL ADVISORY COMMITTEE	
James B. Conant	December 12, 1946–August 1, 1952
Lee A. Dubridge	December 12, 1946–August 1, 1952
Enrico Fermi	December 12, 1946–August 1, 1950
J. Robert Oppenheimer, chairman	December 12, 1946–August 8, 1952
Isidor I. Rabi	December 12, 1946–August 1, 1956
chairman	October 1952–July 1956
Hartley Rowe	December 12, 1946–August 1, 1950
Glenn T. Seaborg	December 12, 1946–August 1, 1950
Cyril S. Smith	December 12, 1946–January 10, 1952
Hood Worthington	December 12, 1946–August 1, 1948
Oliver E. Buckley	August 2, 1948–August 1, 1954
Willard F. Libby	August 7, 1950–September 30, 1954
Eger V. Murphree	August 7, 1950–August 1, 1956
Walter G. Whitman	August 7, 1950–August 1, 1956
John von Neumann	February 27, 1952–August 1, 1954
James B. Fisk	September 22, 1952–August 1, 1958
John C. Warner	September 22, 1952–August 1, 1964
Eugene P. Wigner	September 22, 1952–November 19, 1956

Source: R. G. Hewlett and Francis Duncan, Atomic Shield: A History of the U.S. Atomic Energy Commission, vol. 2, *1947/1952* (Berkeley: University of California Press, 1972), p. 664.

however, there was greater skepticism because of the technical uncertainties and the large expense. Moreover, they anticipated that the USSR would also move in the same direction and the result would be an arms race in fusion as well as in fission bombs. Would this not, they asked, bring about less rather than more national security? And would not a reasonable alternative to development of thermonuclear bombs be to announce that the United States would refrain from doing so if the USSR would agree to the same? This alternative—occasionally referred to as "to announce to renounce"—was seen by some members of the commission and the GAC, who were appalled by the power for mass destruction, as a realistic step toward a world disarmament treaty.

It was inevitable that the two very different courses of action—development or renunciation—would give rise to deep differences of opinion among members of the commission and the GAC and would alienate them from the joint committee and the MLC, where the consensus was for full-scale development. In the highly charged atmosphere of the time, some individuals on the commission and the GAC saw the pursuit of thermonuclear weapons as sinful and potentially in the same category as the mass slaughter of humanity by the Nazis. Others saw no essential difference between those weapons and the more powerful fission weapons that were already being pursued.

The joint committee had no reservations about the development of thermonuclear weapons and no patience whatsoever with the idea that an understanding with the USSR might be possible. An underlying source of tension between the commission and the GAC, on the one hand, and the Joint Committee, on the other, was the position taken by the joint committee that they and the president were elected to pass judgment on such questions based on the commission's and the GAC's technical advice, not on their ideas of global strategy or canons of morality.

This role was extremely hard for some members of the commission and the GAC to accept. Lilienthal, for one, felt that the United States was making a tragic error in giving up what he and others saw as a unique opportunity to achieve world disarmament. He was convinced that disarmament was in the best interest of the country, which was unscarred by a modern war, and that renunciation was worth a try in spite of the high probability that it would fail. Even before President Truman decided to go ahead with development of the hydrogen bomb, Lilienthal, weary in body and soul from the constant tumult of the previous three years, decided to resign from the commission. At the same time, Lewis Strauss, a strong proponent of development, felt his aim was accomplished when the president assented to this project and so resigned.

The development of the hydrogen bomb would require the construction of new plants for reactors to generate hydrogen isotopes and a new laboratory akin to Los Alamos. The method of achieving detonation of a hydrogen bomb required fresh ideas and difficult experiments to confirm them. There was much disagreement about the experiments and their validity, and the situation was worsened when Communist forces attacked South Korea on June 25, 1950, and the United States entered the Korean War. Some distinguished physicists—among them Oppenheimer—saw the technical problems presented by a fusion bomb as likely to be insuperable. Indeed, without the original, very clever work of Stanley Ulam and Edward Teller at Los Alamos, the hydrogen bomb as a true fusion weapon would not have emerged at that time. In Washington, there was fear of an expanded conflict, possibly another world war, and the urgency of increasing the stockpile of fission bombs and developing thermonuclear bombs intensified. Priorities shifted once again, and the GAC was pushed to estimate the prospect for the successful construction of a hydrogen bomb. The joint committee under Senator McMahon would tolerate nothing less than an all-out effort to produce the weapon. Expenditures that might have been questioned at another time were approved without a second thought. The commission concurred, and progress toward the creation of a hydrogen bomb was rapid. On October 31, 1952, a single hydrogen bomb measured at the equivalent of 10.4 million tons of TNT—or five hundred times the strength of the Hiroshima bomb—was detonated by the United States on Bikini Atoll in the South Pacific.

THE OPPENHEIMER INVESTIGATION

The passions that attended the development of the hydrogen bomb came to a bitter climax two years after the test. The first briefing of president-elect Dwight D. Eisenhower by the AEC was in early November 1952 in Augusta, Georgia. It was a secret briefing, ordered by President Truman, to acquaint the new president with the facts of the hydrogen bomb test a few days earlier. Eisenhower was stunned by the news and immediately inquired about plans to keep strict secrecy. His dealings with the Russians in the seven years since the end of WWII had convinced him that it was best to keep them off balance by maintaining secrecy. If there was an advantage to announcing the successful test of a U.S. hydrogen bomb, then and only then would Eisenhower go public. With that frame of mind, it was natural for Eisenhower to ask Lewis Strauss, who agreed, to serve as his special assistant on atomic energy. Eisenhower's outlook was also shared by most members of the joint committee and particularly by its chairman, McMahon, before his untimely death from cancer at age forty-nine, just prior to the hydrogen bomb test.

There was, however, considerable ambivalence contained in Eisenhower's position. He desired secrecy as far as the Russians were concerned, but he was also aware of the need to inform the American people of what had been done and of the international crises that the nation faced as a result. A panel to study possible U.S. proposals for disarmament had been organized by Dean Acheson, secretary of state under Truman, with Oppenheimer as chairman. The panel produced a report titled "Operation Candor" that circulated within the government, stressing the terrible consequences of an atomic war—a hydrogen bomb war—and outlining the advantages to the United States of seeking disarmament. Eisenhower very much supported "Operation Candor" and cast about for practical proposals that might be presented to the nation and the world based on its ideas. The president in essence sent a mixed message to those concerned with atomic weapons and national security: on the one hand, that it was prudent to build up the nuclear arsenal and, on the other, idealistic to promote disarmament.

Although completely loyal to Eisenhower, Strauss strongly opposed the ideas of "Operation Candor" and indeed to any release of information that might, in his view, benefit the Russians even in the most minimal of ways. Strauss and others who agreed, among them Commissioner Thomas E. Murray, still balked at Oppenheimer's unrelenting opposition to the hydrogen bomb and decided that an attempt should be made to block Oppenheimer's employment in any future consultative capacity for the government. They were backed by a report on Oppenheimer's activities compiled earlier by William L. Borden, then the executive director of the joint committee, who claimed that Oppenheimer was at the very least a security risk. When the USSR tested a bomb using hydrogen as part of its explosive material just nine months after the U.S. test, those suspicious of Oppenheimer asked whether he had used his influence purposely to delay the U.S. effort and allow the Russians to catch up. Still others questioned the advice that had come from other committees he had chaired. For example, Oppenheimer's advice to the Strategic Air Command initially cast doubt on whether hydrogen bombs could be built, and he suggested that its strategy be focused on other weapons.

The situation came to a head when Gordon Dean, on Oppenheimer's request and just a few days before retiring as AEC chairman, renewed Oppenheimer's consultantship to the AEC for the year June 30, 1953, to June 30, 1954. Assuming no opposition, Dean took the step without consulting his fellow commissioners or his successor, Lewis Strauss. This action set the stage for a profound tragedy.

A second report from Borden, by then a private citizen whose own prior record at the joint committee of keeping classified documents secure was far from spotless, went from J. Edgar Hoover, head of the FBI, and Strauss to Eisenhower and members of the National Security Council. It was accompanied by an FBI report on Oppenheimer dating back to 1942. This report was interpreted by some as indicating Oppenheimer's questionable behavior and by others his active disloyalty. Some suggested that Oppenheimer, who was abroad at the time, might be ready to defect. The issue of Oppenheimer's status and his access to classified information was brought to the president as an urgent matter, but Eisenhower took the only action available to him: he delayed it. Attempting to pacify all parties and treat Oppenheimer fairly, he nevertheless suspended Oppenheimer's clearance pending an investigation by the AEC, since the clearance involved concerned Oppenheimer's consultantship.

Soon afterward, Chairman Strauss and the new general manager of the AEC, General Kenneth D. Nichols, who had been General Groves's assistant throughout the Manhattan Project, met with an astonished Oppenheimer to offer him the opportunity to resign in order to forgo an investigation. That was an option that Oppenheimer refused. He feared such action would be a tacit admission that he was guilty of some crime that, as far as he was concerned, he did not commit.

The commissioners arranged for a board of three members from outside the government to review Oppenheimer's situation. The board was headed by Gordon Gray, a lawyer and former publisher, who had been assistant secretary of the army in 1947 and subsequently an assistant to the president of the United States until he became president of the University of North Carolina in 1950. The investigation began on April 12, 1954, and went on, for five full days each week, until May 6 of the same year. The board concluded that

Oppenheimer was devoted to his country but voted two against one that, because of bad judgment, he was a security risk and his access to classified material should be withdrawn. Four of the five commissioners concurred in this recommendation; the only exception was Henry Smyth.

The public—to whom Oppenheimer was a hero—was generally critical of the result of the investigation. The scientific and academic communities were divided: most thought he had been railroaded in an effort to put scientists in their place, but some believed there was justification for the verdict against him. This was the time of McCarthyism and of the Soviet spies Allan Nunn May, Klaus Fuchs, and the Rosenbergs. Emotions ran high, and rational argument ran low. Few if any recognized how similar the fates of Douglas MacArthur and Robert Oppenheimer were, both men of remarkable gifts and accomplishments brought low by their own arrogance. Gordon Gray was disturbed by the authority with which Oppenheimer placed his own judgment, as Gray put it, "over that of more responsible persons."[2] President Truman could have said precisely the same thing of MacArthur. At the conclusion of the investigation, Oppenheimer returned to his position as director of the Institute of Advanced Study in Princeton, New Jersey, where he remained until his death in 1967.

Rescinding Oppenheimer's clearance to receive classified information and serve as a government consultant had many repercussions within the community of American physicists. The investigation had pitted Ernest Lawrence and Edward Teller against Oppenheimer. Younger physicists found themselves passively taking sides, unable to resolve the conflict between their personal loyalties. For example, Robert Serber, who had a position at Berkeley, moved from California to Columbia University, in New York, leaving behind the academic institutions that were home to Oppenheimer, Lawrence, and himself. More generally, scientists were disabused of the notion fostered by WWII that they were indispensable and would always be handled with kid gloves by the government.

In the eight years between the creation of the AEC and the Oppenheimer investigation, atomic energy grew to be perhaps the most important part of the national defense. In that period, scientists had been introduced to the inner councils of government in peacetime and in turn the government had learned a bit about science and a lot about scientists. What emerged was a kind of love-hate relationship. Both sides recognized their mutual attraction and interdependence. It was clear that they would not be able to go completely separate ways.

The navy acted to establish close relationships with university scientists after WWII through creation of the Office of Naval Research.

Not all government science agencies had origins as tumultuous as the AEC's. The Office of Naval Research (ONR) began quietly in the navy and went equally quietly through Congress while the Atomic Energy Act was in the throes of debate.

At the start of WWII, The director of the Naval Research Laboratory was Vice-Admiral Harold G. Bowen, who had previously been the chief engineer of the U.S. Navy. The Naval Research Laboratory was concerned primarily with practical applications that might solve maritime problems. It was the product of a recommendation made originally at the end of World War I by a civilian advisory committee headed by Thomas Edison, and as part of that recommendation the Laboratory reported directly to the secretary of the navy. Admiral Bowen was one of the few high-ranking naval officers aware of the U.S. intent to develop the atomic bomb in 1940 and of the promise of atomic energy for ship propulsion. However, Bowen was forced out of his post in naval research in 1944 because he was difficult and had antagonized the civilian heads of the OSRD by insisting that naval weapons development should be kept out of civilian hands. His inability to work with others was one of the possible reasons that the navy was excluded from the Manhattan Project in the first place.

At the end of WWII, Bowen was determined to bring nuclear power to the ships of the fleet. He had pioneered the use of high-pressure steam power many years before and believed nuclear power would make the fleet largely independent of the land. Bowen looked for a base within the navy from which to pursue his goal and, having learned a lesson from his past experience, concluded that a new agency established to interact closely with civilian scientists might serve his purpose. As director of that agency, his thinking went, he would be in a position within the navy's hierarchy to undertake the development of nuclear propulsion for the fleet.

The road to the agency Admiral Bowen had in mind was not direct. He had been relegated to lesser posts during the war because of his abrasiveness, but his boldness in encouraging innovation was admired by James Forrestal, then secretary of the navy, and by Commodore Lewis L. Strauss, Forrestal's special assistant for research planning and nuclear affairs. Soon after the death of President Roosevelt, Forrestal ordered the creation of the

Office of Research and Inventions (ORI) and transferred to it the Office of Patents and Inventions, the Office of the Coordinator of Research and Development, and jurisdiction over the Naval Research Laboratory and postwar research planning. Admiral Bowen, plucked from relative obscurity, was appointed its chief. He quickly set about cultivating favor with scientists who were returning to universities from the OSRD and the Manhattan Project by offering ORI funds to support their research. The ORI promised freedom of choice and action to the scientists and their universities and was careful to live up to those promises. There would be no burdensome bureaucratic requirements as the price for its support. This was an opportunity too good to miss, and university scientists responded immediately.

Free to concentrate on nuclear propulsion, Bowen had two obstacles to overcome: he needed to convince General Groves that it was in the interest of the army and the nation to grant the navy access to the Manhattan Project, and he needed authorization within the navy for ORI jurisdiction over nuclear propulsion. Direct cooperation with the navy was not welcomed by Groves, but this simply gave added reason for Bowen and his supporters—Lewis Strauss; W. John Kenney, then undersecretary of the navy; and Admiral Luis de Flores, deputy chief of the ORI—to seek congressional authorization for the long-term stability and independence of ORI. President Truman signed the bill creating the Office of Naval Research, the new name for the ORI, without fanfare on August 3, 1946, just two days after signing the Atomic Energy Act.

The second obstacle to Bowen's organizational base for a nuclear navy—authorization within the navy—was too formidable for him to overcome. That issue was settled in favor of the Bureau of Ships, which would later designate Captain Hyman G. Rickover to direct the collaborative effort with the AEC for the development of naval nuclear propulsion systems. Admiral Bowen had failed again to attain a position from which he might direct the conversion of a steam-powered to a nuclear-powered navy. He had, however, been the catalyst that stimulated action by the navy and ironically the choice of Rickover to do that job was a good one. Bowen's legacy to the navy and to a legion of university scientists was the Office of Naval Research, an agency free to act under its congressional charter and the benign neglect of a navy preoccupied with national defense matters. Admiral Bowen went on terminal leave soon after the ONR was created and officially retired a year later.

The ONR staff that Bowen left behind, a mix of naval officers and civilians, was charged to support university-based science in a way acceptable to both academic scientists and their university administrations. Some believed in the need of the nation to have a basic research agency in place while Congress was debating the National Science Foundation; others thought it in the navy's interest to have as allies the scientists whose technical advances had been so important in the war. Still others saw the ONR as a science base for a future organization, possibly a future OSRD. These reasons, coupled with the support of the secretary of the navy, made the ONR an attractive agency to young would-be science administrators. Its first chief scientist was Alan T. Waterman, previously an instructor at Yale University and a member of the OSRD and later the first director of the NSF. Another of its chief scientists, Thomas Killian, became the first chief scientist of the Office of Army Research, and still a third, Emmanuel Piore, went on to become vice president in charge of research at the International Business Machines Corporation.

The ONR satisfied an important criterion specified by Bush: that it be far removed from the operational activities of the navy and independent of the chief of naval operations. It focused on maintaining close relations with universities, university scientists, and engineers by helping them financially to do the science they proposed. Little pressure was exerted to suggest projects or programs of primary interest to the navy. Less than 10 percent of the $86 million received by the ONR in the period 1946 through 1950 was spent on naval science applications. The remainder of the funds went for equipment and the support of basic research in a wide variety of fields. The organizational chart of the ONR in October 1946, shown in figure 3.5, illustrated the level of ambition of its administrators and the method used to create an office staffed at the top by a civilian scientist and a senior naval officer. The laboratories and other divisions on the line just below the chief of naval research were mostly inherited. One revealing item in the organizational chart was the nationwide extent of the branch offices, including a London office that successfully reported on European science and helped promote contacts among American and European scientists.

The ONR used its authority to place contracts with universities by following the precedent established by the OSRD. It paid universities the full costs of research contracts, including indirect costs that went to the university. A scientist who had prepared the research budget was not penalized by deductions for the indirect costs or by overly tight restrictions on expenditures. Again, following the OSRD pattern, the ONR did not advertise for bids

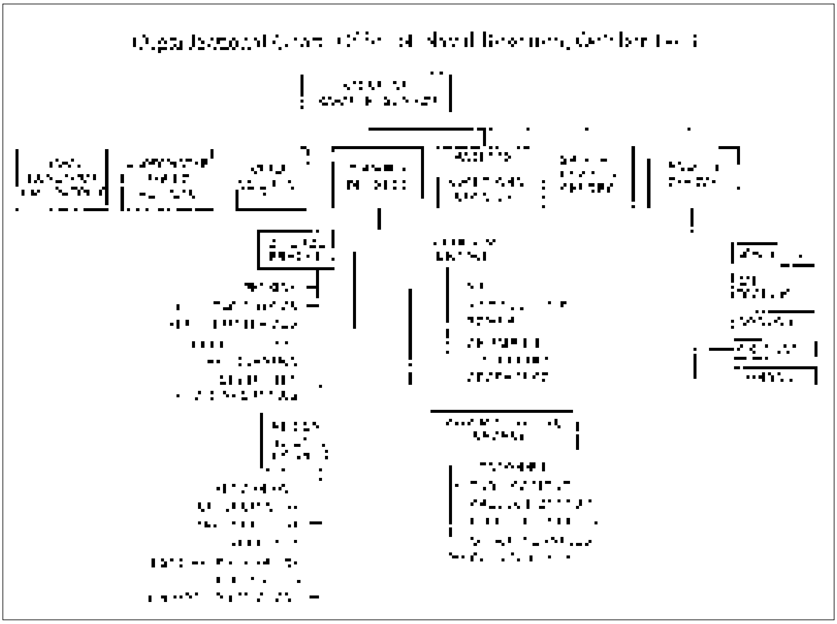

FIGURE 3.4. *Top*: Organizational chart of the Office of Naval Research, October 1946.

Source: Harvey M. Sapolsky, *Science and the Navy: The History of the Office of Naval Research* (Princeton: Princeton University Press, 1990), p. 49.

Bottom: Alan T. Waterman, chief scientist of the ONR and director of the National Science Foundation from 1950 to 1962.

Source: National Science Foundation.

but responded to research proposals from scientists. Proposals were ranked by the ONR program officers in the science and medical science branches after consultation with a peer group of scientists in those fields, with support going to those proposals that ranked high. The ONR introduced the idea of providing support for graduate students to help build a future research and academic base. Finally, a minimum of reporting was required by scientists. Since most of the research was unclassified, it could be published in the open literature; a reprint of a published article was often acceptable as a progress report.

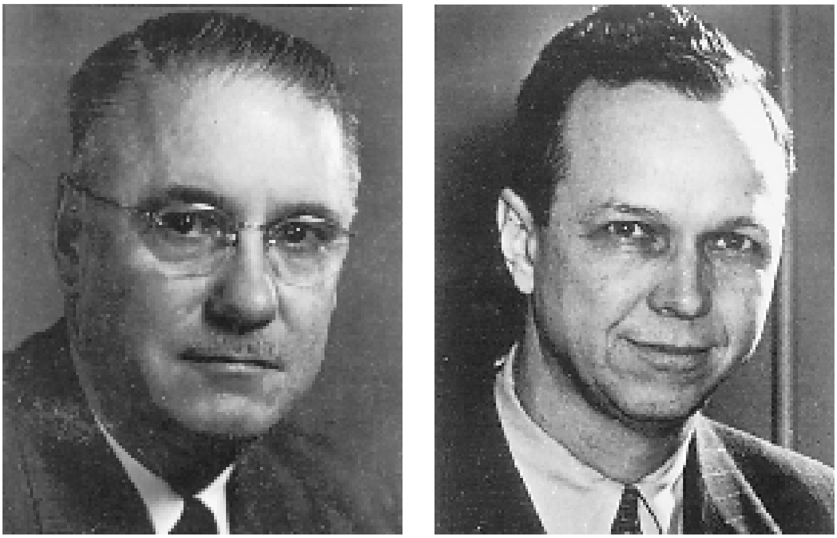

FIGURE 3.5. *Left*: Cassius J. Van Slyke, first chief of the Research Grants Office (later Division) of the National Institutes of Health, 1946–1948; director, National Heart Institute, 1948–1952; associate director, NIH, 1952–1958.

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. 21.

Right: Ernest M. Allen, scientist director, U.S. Public Health Service; assistant chief, Research Grants Office, 1946–1950; chief, Division of Research Grants, 1951–1960; NIH associate director, 1960–1963; grants policy adviser, Office of the Surgeon General, 1963–1968; deputy assistant secretary for grants administration, HEW, 1969–1973.

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. 57.

The ONR took responsibility for a number of contracts vacated by the OSRD, servicing seven hundred contracts by 1950. It made itself especially useful in funding expensive, large-scale equipment for universities and took the initiative in funding elementary particle accelerators built on university campuses to explore the burgeoning new field of high energy physics. Soon, however, the ONR found itself overcommitted financially and turned for help to the fledgling AEC. A joint ONR/AEC program began, and it proved to be an extraordinary example of cooperation between two newly formed agencies. Within a few years, high-energy accelerators known as synchrocyclotrons were constructed at the Carnegie Institute of Technology (now Carnegie-Mellon University), the University of Chicago, and Columbia, Harvard, and Rochester universities, each costing several million dollars. The original synchrocyclotron was designed and built at the University of California in Berkeley with the assistance of funds from the Manhattan Project. These accelerator laboratories established U.S. physicists at the forefront of that field and served thereafter as the training grounds of several generations of physics students.

The ONR played a vital part during the birth of government support of basic science and scientists after WWII. This was a period in which discoveries of new phenomena gave rise to new fields of science that needed encouragement, freedom of action, and the financial support that the ONR provided so well and sustained through a critical time. The ONR acted selflessly, operating apart from the mainstream of naval operations even during the Korean War, and was willing to seek help from the AEC to carry out its mission. It was respected and prized by scientists for the effective way it conducted its business and later venerated for the farsighted precedents it set for the future of the science establishment.

After many years of determined independence, the medical community overcame its fear of peacetime affiliation with the federal government.

Before the twentieth century, the U.S. medical community had a tenuous, arm's-length relationship with the federal government. This suited physicians, who were averse to any government interference that might come between them and their patients, and allowed the government to avoid entering a province reserved to the individual states. Nevertheless, there were areas in which the government, at first reluctantly and then more willingly, served a useful function in the field of medicine. It occasionally established a temporary agency to amass data and make recommendations with respect to sanitation, health reform, and control of infectious and chronic diseases. As medical science advanced, there emerged from this tentative beginning an increasing presence of the federal government in epidemiological studies and public health legislation. Thus the origin and evolution of the modern medical science agency, the National Institutes of Health, was the result of a gradual, trial-and-error activity over many decades that caused hardly a ripple of excitement in Congress or in the medical community.

Congress's ambivalent attitude toward medical science is shown by two early inconsistent actions at the end of the nineteenth century. The Marine Hospital Service (MHS), later renamed the Public Health Service (PHS), sought but did not receive a supplementary congressional appropriation to investigate the 1878 yellow fever epidemic in Mississippi. A year later, Congress began a far-sighted but short-lived experiment involving a centralized medical agency, the National Health Board, whose function was to provide funds for yellow fever research to university scientists. The situation was stabilized to some extent in the early twentieth century, after MHS bacteriologists successfully fought epidemics of plague in San Francisco and Hawaii and yellow fever in Cuba. The MHS Hygienic Laboratory was directed to serve as the national clearinghouse for medical scientific information, and the MHS surgeon general was authorized to coordinate the states' public health efforts. Despite these early federal actions, the funding of medical education and research prior to WWI was dominated by private foundations: the Rockefeller Institute and the Carnegie Institution of Washington. These two largely funded research within their own institutions, often called intramural research.

The ravages of the battlefields of WWI strengthened the government's resolve to aid medical research. Congress chartered the National Research Council (NRC) in 1916 to award contracts for "Military-Medico" projects to academic researchers. The NRC Medical Division consisted of fifteen representatives of scientific societies, six or eight members at large, and special committees of outside experts to help make awards, adjust budgets, and administer projects. This movement toward peer review was a departure from the traditional practice of providing funding to only the most experienced investigators, based primarily on their reputations. In 1918 the NRC was extended into peacetime and established as a permanent advisory committee to the federal government. With its annual budget of $800,000, the NRC dispensed 140 fellowships (not all in medicine) and after 1930 began a small grant program.

In the struggle to provide federal relief and recovery programs in the New Deal era of the 1930s, PHS Surgeon General Thomas Parran managed to get $2.5 million for public health research into the 1935 Social Security Act. Soon after, the PHS's Division of Scientific Research began awarding grants to academic scientists, known as extramural grants. The authority to make grants had been placed with the National Advisory Health Council (NAHC) at its creation in 1930, when a congressional act converted the Hygienic Laboratory of the former MHS into the National Institute of Health (NIH). The issue of how grant proposals to the NAHC were to be made and how they were to be evaluated rose again, this time with the assistant surgeon general, Lewis R. Thompson. Thompson was chief of the PHS's Division of Scientific Research and spoke in favor of medical subcommittees composed entirely of individuals outside the PHS who would recommend action based on scientific worth. By 1937 the elements were in place to promote concentrated attacks on single major diseases, as well as general support of research in nonfederal institutions, including fellowships for advanced study. Together, the mortality rate of troops in WWI and the postwar infant mortality prodded Congress to these actions. In the spring of 1937, the NAHC approved $31,520 for new projects. Later that year, Congress created the National Cancer Institute (NCI) and empowered the NCI council to make research grants-in-aid to cancer studies and to make loans of radium to hospitals and university medical centers. In the period from 1938 to 1940, the NCI received 137 applications for funds and awarded $200 million in extramural grants. Almost simultaneously, the OSRD was formed with the Committee on Medical Research (CMR) as one of its divisions.

At the time of the Allied invasion of Europe, three interconnected agencies were contending for control of federal funding for medical research after the war: the surgeon general of the PHS; the CMR, whose leadership included the NIH director; and the NRC's Division of Medical Research. The interlocking memberships of these committees and institutions kept the battle for control from getting out of hand. At the same time, the influence of Vannevar Bush, who was already looking toward the creation of a national research foundation to replace the CMR in peacetime, promoted a medical community free of nonmedical control. So strong was the desire for independence that one unrealistic suggestion expressed by a CMR advisory panel requested control of federally funded medical research without fund management by any federal agency.

Despite the controversy, by the summer of 1946 the residual university contracts in biology and medicine of the CMR were effectively transferred to the PHS. With its adjunct NIH, it took the position held by most of the medical community that research was inseparable from treatment and education. Equally important, the OSRD/CMR/NRC experience from WWII was also transferred. Federal support for biomedicine and medical research was firmly established as the vital prerequisite for the future of national health research and education systems. Furthermore, peer review was recognized as the effective means of assuring quality in research and satisfying the different interests of individual scientists, universities, federal agencies, and federal policy makers. Herein lay the origin of the extramural award system of the NIH, particularly its separation of review and management functions. Rules concerning the behavior and performance of outside consultants—no compensation except for documented expenses and no participation that might involve a conflict of interest—were products of wartime experience.

Of course, consensus did not mean complete agreement. Basic problems remained to be solved. One was inherent in the extensive nature of medicine, which requires supervision by a large number of divisions, committees, and subcommittees. Human afflictions and the basic biomedical sciences needing study have long been legion, and the number of researchers eager to pursue those studies has been nearly as large. This raised difficult questions of principle and organization. How could the vast number of diverse research proposals—good, bad, and indifferent—be handled fairly, competently, and promptly while adhering to the guiding principles? And how would narrow fields of research be satisfied? Moreover, how would the proper balance between the intramural and extramural programs of the PHS/NIH be devised? All agreed, however, that, first, the biomedical knowledge then available was inadequate to mount a direct assault on major diseases; second, that NIH support of broad, far-ranging research programs in the fundamental biomedical sciences was required; and, third, that the way to ensure high-quality sponsored research was to demand that it meet rigorous standards determined by expert peer review.

With the fate of the National Research Foundation yet in the hands of Congress, the director of the NIH, Rolla Dyer, set up a small section primarily to administer the forty-two outstanding OSRD contracts accepted as NIH grants and to carry out those duties as a sponsor of extramural research. The section was headed by Cassius J. Van Slyke, a senior surgeon from the Venereal Disease Division of the PHS, who drafted the regulations under which NIH would exercise "only the most minimal supervision"[3] over extramural researchers. Technical review of proposals would be delegated to advisory panels drawn from universities and medical schools. The NIH Research Grants Office (RGO) opened on January 1, 1946, with Van Slyke as chief and Ernest M. Allen, also from the Venereal Disease Section of the PHS, as his deputy. They were located in temporary quarters pending availability of a permanent location, with borrowed army office furniture and one secretary. It was a beginning much like that of the AEC, coincidentally on the same day.

By August 1947 the tiny RGO had been transformed completely as an organization. A year earlier, the National Mental Health Act had been passed. It authorized $10 million in grants to the states for facilities and research projects, the first permanent legislation authorizing grants to educational institutions to train health care manpower. One result of the act was to increase applications for grants and, as a consequence, to consolidate all five PHS divisions with research grant authority into one single office, the RGO. Soon thereafter, the RGO achieved division status within the PHS and became the Division of Research Grants (DRG), with administrative supervision over all programs of research grants-in-aid of the PHS. Of all the institutes, centers, and divisions (ICDs) that constituted the NIH, the DRG is the second oldest and the division most closely tied to the NIH's Office of the Director. The DRG became the focus of all activities of the extramural system of the NIH, receiving grant applications, assigning them to ICDs, organizing reviews by scientists, managing the information that tracked applications and the progress of awards, and reporting to Congress on critical concerns. The issue of research awards to individual researchers rather than to research groups remained to be settled over time and through the experience of the changing study (review) sections. The emphasis on extramural awards, however, established in the early days with formation of the RGO, did not change significantly. The instinct for autonomy ingrained in the academically educated medical community would ensure that the resources for research would be dispersed widely and not concentrated in a single giant federal medical establishment.

In 1947–1948, the DRG received 2,078 applications, of which 1,526 were approved. The FY 1951 budget for the NIH was $15.75 million, which was then

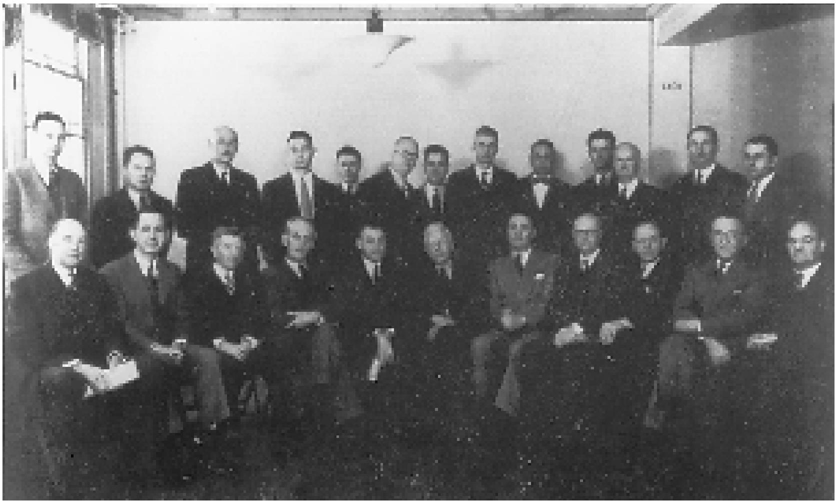

FIGURE 3.6. *Top*: NIH director Rolla E. Dyer (*seated center*), DRG chief Van Slyke (*seated, fifth from right*), and deputy chief Allen (*standing, far left*) with study section representatives, about 1947.

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. 29.

Bottom: Surgeon General Thomas Parran, USPHS, a key player in the formation of the National Institutes of Health.

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. 18.

decreased by 9.1 percent in a period of national retrenchment during the Korean War. It was thus a slow beginning, but by 1955 the budget of the NIH passed the $79 million mark.

The business of PHS/NIH was initially considered too specialized to warrant technical meddling by Congress in those early years. Perhaps more important, the goals, awards, and research itself of the PHS/NIH medical research system were delivered in terms the average citizen could understand. Moreover, the medical community was conservative and would not tolerate spending on the study of fringe ideas. No questionable medical research awards provoked righteous indignation in Congress. The one matter on which it took a definite position—namely, a broad, nationwide distribution of research funds and medical construction—was largely addressed by the widespread distribution of medical schools and hospitals. Where there was a geographical lack or weakness, the NIH stepped in with a remedial institutional grant.

There was, however, an area in which NIH succumbed to congressional pressure: it involved loyalty tests of grantees and revocation of awards without due process. As early as mid-1947 the NIH was required by the Federal Security Agency (FSA) to secure an affidavit of loyalty from "every incumbent employee."[4] The impetus at the time came in part from incidents at the University of Washington, where three tenured professors were fired for past association with the Communist Party, and in the AEC, where Senate investigators allegedly found security risks. The NIH director complied with the directive for regular employees but deferred to the independent PHS division, the DRG, on the question of NIH fellows and awardees. The DRG insisted that fellows and awardees were not employees and therefore not subject to government investigation or oath requirement. It argued that academic freedom should be respected, that universities held jurisdiction over their faculty who were fellows and awardees, and that loyalty oaths and investigations would lead to serious complications in the award system. This position was not firmly upheld by either the NIH or DRG, which gave way under pressure from the Senate. The division then required from all grant applicants an anti-Communist oath as a condition of any award, and the surgeon general soon extended that requirement, in addition to FBI clearance, to the study section consultants if their service exceeded ninety days.

If loyalty checks were mandatory for federal service personnel, then the PHS had to enforce the ruling for all its employees. The issue of loyalty oaths and investigations for NIH grantees was another matter, and it was fought bitterly by chiefs of the DRG, who capitulated only, as they thought, to protect the newborn NIH from complete disintegration. They saw loyalty oaths as a foretaste of the future, and they were proved right only a few years later, when McCarthyism drove rampant allegations of disloyalty against NIH award recipients. By then, however, the NIH was in no position to resist. President Eisenhower, whose instinct was to wait and see how much irrational, harmful behavior the public would tolerate, allowed his secretary of health, education, and welfare (HEW), Oveta Culp Hobby, to institute a policy denying support to grantees about whom the FBI had some form of derogatory information. Approximately thirty researchers were removed from their projects in mid-1954, among them, the famous, gifted Linus Pauling.

By August 1955 the nation tired of witch-hunters, Marion Folsom succeeded Oveta Culp Hobby as HEW secretary, and the NSF now stood firm to protect its awardees, a position endorsed by Eisenhower. The infamous Senate inquisition of the army had destroyed McCarthy and his Senate colleagues. This episode left its debilitating effects and personal anguish everywhere. It is summed up in a statement by C. J. Van Slyke, who, as NIH associate director for extramural affairs and a pioneer organizer of the Division of Research Grants, was responsible for terminating a number of grants between 1952 and 1955. Van Slyke's statement, given in an oral history interview in 1963, is a cry from the heart for those caught up in the ugly episode:

> Everything ran along fine until McCarthy started acting up, and then we would get instructions from our security officer that this grant headed by scientist number x, or a b, or whatever he was, would have to be terminated. Well, that of course would stop the research work and throw the whole team out of support just overnight, because they had to be stopped immediately. . . . I was the S.O.B. who said, "If you will wire me today that you would change investigators—and I couldn't tell why, I was not permitted to tell them that he was the subject of security questions—and you'll have to recommend somebody else. . . . " I swear I did that dozens of times. . . . We lived through those awful days of McCarthy influence without anything, save the protection I was able to give the research grant program from my desk. I can tell you a good many times I felt like chucking the job. I felt so unclean.
>
> A fellow had signed a petition or something, or had contributed two or three dollars to some cause. This just happened to be causes I would

> have contributed to, if I had any money. I would have thought that contributing to free Spain to get rid of the dictator, Franco, would have been a good thing because I'm opposed to dictators. . . . It was these kinds of people who got into trouble. . . . It was the most unfair sort of thing, and it wasn't until Mr. Folsom came in as Secretary that it stopped.[5]

The creation of the NIH and its evolution during the ensuing three decades was remarkable. It was a period in which the Korean War began and ended, the cold war was moving to its height, and the medical community and successive administrations and Congress had to agree on one unprecedented action after another that would sooner or later affect the lives of generations of Americans. Actions agreed on within the medical community broke with the past in trading their independence for the well-equipped, better-staffed, more productive laboratory that favorable peer review and federal funds could buy. In pre-WWII American medicine, this trade-off had few proponents. The majority of the community saw it as government control of medicine. Congress reflected the same view from the government side. It wanted no part of the responsibility for funding medical research or subsidizing medical education at the national level. These positions were not changed by discussion; they simply proved themselves to be irrelevant, given the amazing achievements made during WWII.

The development of the PHS/NIH system with its centerpiece, the Division of Research Grants, was typical of the medical profession of the time. They were determined to be fair and scandal-free and to hold to the guiding principle of peer review as the basis for awards, which in the main freed them from political interference. It was only a matter of time until the nation at large recognized the value of the institutes it had spawned. This occurred not long after the organization had taken on a semblance of permanence, when the Salk vaccine for poliomyelitis was developed in 1955. It is difficult to reconstruct today the dread this disease produced. In the summer of each year, parents of small children grew fearful, since this was apparently when the children were most susceptible. The dread was almost palpable, stimulated by the publicity that President Roosevelt's haven for polio victims in Warm Springs, Georgia, received each year through the March of Dimes. Americans breathed a collective sigh of relief when a vaccine was available to defeat this enemy. The accomplishment lost some of its luster when 204 new cases of polio were discovered among the 400,000 children who had been inoculated with NIH-licensed vaccine manufactured by a commercial company. Fortunately, the NIH and the Communicable Disease Center moved quickly to do its own rigorous safety tests of vaccine lots and were able to restore public confidence in the immunization program. After this experience, NIH field testing of a medicine on a large scale was no longer contracted to outside drug companies or private foundations.

By 1955 there was no question that what was initially an unproven expenditure of public funds for biomedical research and education was subsequently justified many times over.

Legislation creating the National Science Foundation was finally passed in 1950.

As early as 1942, the New Deal stalwart, Harvey Kilgore, introduced a bill to the Senate aimed at creating a national science foundation. When it failed to pass, he reintroduced it in 1943 and again in 1945. Senator Kilgore foresaw the need for a science agency that would support through grants and contracts both basic and applied science research. His foundation included the social sciences and required research funds to be dispensed according to a prescribed formula to achieve an equitable geographical distribution. Kilgore's agency was directly responsible to the president and the Congress through their authority to appoint and remove members of its management.

Vannevar Bush agreed on the need for a science foundation but disagreed with the strong populist flavor present in Kilgore's vision. In particular, Bush recommended supporting only the best basic research in colleges, universities, and research institutes; that research was to be identified by critical review of proposals prior to funding. Bush was strongly opposed to any formula for a geographical distribution of funds. He wished to keep the foundation as free as possible of political authority, even going so far as to remove appointment of the foundation's director from the president's purview. Bush thought that support for basic science research in the physical and mathematical sciences was the hardest to acquire and maintain at a steady level. Consequently, he excluded both applied science and social sciences from his proposed foundation because he felt that they were able to attract support separately in their own right.

Bush's ideas were presented to the Senate in a bill submitted by Senator Warren Magnuson on the same day that the Bush report was released by the White House. The stage was set for a contest between the Kilgore and Magnuson bills, with the White House favoring the former because it contained the requirement that the foundation be directly responsible to political authority, namely, the president. After two years of debate, much of it overshadowed by the stirring drama of the creation of the AEC, Congress sent a bill to the president that he promptly vetoed because it did not provide for presidential appointment of the foundation director and advisory board. The language of the vetoed bill finessed two other issues that divided the Kilgore and Magnuson visions of a foundation. The new agency was instructed to avoid undue concentration of its funds to prevent an overly unbalanced geographical distribution, and the term "other sciences" was added to the proposed list of sciences to allow for the inclusion of social and applied sciences at a later time.

The bill that the president finally signed in 1950 gave him executive control and was a reasonable charter for an enterprise that was completely new to the Congress. By that time, the AEC, ONR, and NIH had several years of experience sponsoring research of university scientists, but it was research that evolved more or less naturally from the functions of their parent service agencies: the weapons function of the AEC, the modernized navy, and the Public Health Service. The National Science Foundation (NSF) was intended to stand alone, with no attachments or obligations other than to support scientists to carry out the research they proposed. Evaluation of their work would be conducted by fellow scientists and no tangible end product was required. Consequently, the National Science Foundation was seen as a departure from the norm by a Congress intent on exercising detailed oversight of the agencies it created. No wonder that Congress took five years before it would approve the venture.

The agency that was created differed from Bush's report in two important respects, apart from the change of name from National Research Foundation to National Science Foundation. Two of the divisions—Medical Research and National Defense—in the original plan were omitted in the enactment of the NSF. There were several reasons for this. The five-year interval between the end of WWII and the creation of the NSF opened a window of opportunity for existing agencies to satisfy what they rightly regarded as important national needs that they were mostly qualified to fill. The AEC and the ONR had moved quickly to support physical and mathematical scientists and were therefore well established before the NSF could get started. At the same time, the surgeon general, representing the Public Health Service and much of the medical community, was reluctant to see biomedical research relegated to a single, untried agency. It was likely to be separated from clinical practice, which would be anathema to that community. This reaction had been anticipated in the report of the Committee on Medical Research that was appended to the Bush report. There, the CMR agreed with the need for a medical research agency independent of the other disciplines under the umbrella of the NSF but hinted that an agency separate from the NSF was also required. But the CMR did not explicitly break ranks with Bush on this issue in 1945. The young, single NIH, however, had an established relationship with the Public Health Service and was the designated heir of the CMR and its wartime contracts. It was structured to permit rapid growth by the accretion of related institutes that would focus the efforts of medical researchers and practitioners on both the research and clinical aspects of diseases.

The reasons for deletion of the Division of National Defense from the NSF were less direct but not mysterious. Influenced by the OSRD's successful program of wartime research and development, Bush recommended a division within the NSF that would conduct long-range research on military problems. Research on the improvement of existing weapons could be done best within the military establishment, but research involving application of the newest scientific discoveries for military needs would be done better by civilian scientists in universities and industry. Both kinds of military research could go forward side by side, and a close liaison between the two could be achieved. Bush emphasized the value of a broad independent program of basic research and that a healthy interaction between military and nonmilitary research would benefit civilian military research. Doubtless, he felt the same way about the Division of Medical Research. But the absence of an NSF or its equivalent immediately after the war left the military uneasy. The armed forces moved to fill the gap by direct contacts with university scientists they knew from the war whose peacetime research was now funded by the newly created AEC and ONR. And, again, they did not relish the idea of a civilian science agency independent of military control

Early in March 1951 President Truman nominated Alan T. Waterman to be director of the NSF. Waterman was formerly the chief scientist of the Office of Naval Research and an alumnus of the OSRD. He was instrumental in forming the ONR policy of funding basic research in universities and helped to negotiate the joint ONR/AEC agreement to do so in 1948. He was welcomed by a congressionally authorized budget of $150,000 for the NSF's first year (1951), although he had expected a number closer to the $15 million upper limit established by the founding legislation. The low budget was in part an aftereffect of the expenditures for the Korean War and in part a reaction to the larger budgets of the other already established science agencies.

Nevertheless, Waterman proceeded actively to bring the NSF together. He recruited a staff from the ranks of the ONR and universities, and, following the pattern of the OSRD, he moved the foundation frequently from one address in Washington to another as it grew. He quickly organized three of the four mandated divisions: mathematics, physical, engineering, and "other" sciences; biological science; and scientific personnel and education. The fourth, medical science, was held in abeyance because the NIH was already funding many of the proposals in biomedical sciences. A small nonclinical medical science program in the NSF was eventually absorbed into the division of biology.

The NSF initiated a nonrestrictive project grant system to respond to proposals. Following the pattern of the OSRD and the ONR and the precedents of the Public Health Service and private foundations, it moved to support the best research within as comprehensive a program as it could afford. A proposal was first submitted by an individual scientist to his or her own institution for more or less pro forma approval, after which it was sent to the NSF. The grant, if obtained, was awarded to the institution, not to the individual, to fix fiscal responsibility. Grants covered the direct costs specified in the proposal plus an additional 15 percent for indirect (overhead) costs. Proposals went to a program officer in the appropriate foundation division who was the scientist's direct, personal contact with the agency. Program officers read each proposal and arranged for their external reviews. The basis of selection of a proposal for funding was peer review. Reviewers from outside the NSF were asked to evaluate the proposal for originality, interest, feasibility, and cost.

Questions of taste and differences of interpretation of the criteria made reviewing less than an exact science. Nevertheless, peer review succeeded in choosing far more good and excellent proposals for support than mediocre or poor ones. The process also kept serious disagreements to a minimum since active scientists were judged by other active scientists. On the other hand, it opened the NSF to criticism on two fronts. The first revolved around the issue of elitism, which has always plagued programs based on peer review. The charge was that a chosen few were responsible for selecting another chosen few in a process that resulted in an exclusive, self-perpetuating network. It was argued that the network was difficult to broach by individuals from institutions with smaller reputations or interests different from the mainstream of research or by those who were less sophisticated in writing grant proposals. The second criticism had to do with the concentration of approved proposals among a relatively small number of universities whose faculty members were an integral part of the network. Some critics believed that this negated the intent of Congress to make the NSF a national agency. Purposely or inadvertently, they said, the system put an obstacle in the way of any university from the "wrong" part of the country that was seeking to improve itself in science, rather than providing encouragement to do just that.

The NSF acknowledged that there was some legitimacy to both criticisms. It established institutional grants to universities for the purpose of improving their status in different areas of science and began fellowship and traineeship awards for postdoctoral scientists and graduate students that allowed recipients to choose where they wanted to study. Many did go to the few outstanding universities, but many came from states throughout the union, and most states had at least one institution that attracted award holders. To a significant extent, this satisfied the desire for a wide geographical distribution of NSF funds.

A serious threat to the integrity of the NSF during its early years, as in the case of the NIH, arose when Senator Joseph McCarthy embarked on his nefarious and ill-fated Communist witch hunts. The Science Board of the NSF, unlike the NIH, elected to hold the line that awards of research grants would continue to be based on the competence of scientists and the merit of their proposals. No security checks would be required for prospective grantees, the board stated, because the agency supported only unclassified research and its awards were made to institutions, not directly to individual scientists. This courageous stand, taken in 1954, the same year that Oppenheimer's security clearance was rescinded by the AEC, worked and helped to protect the NSF and its award recipients. Two years later, President Eisenhower extended the NSF's policy throughout the government.

Another issue of the 1950s, primarily intellectual but with political overtones nevertheless, was concern for support of the social sciences—the "other" sciences in the NSF charter—and how to give them a proper place in the foundation. There was strong opposition to a social science division in the NSF but equally strong pressure for inclusion of some social science funding. Both sides had good reasons: those opposed alleged the difficulty of evaluating the quality of social science proposals and the waste that would be incurred from funding poor proposals; those in favor cited the fact that the social sciences were assuming an increasingly important place in American life and needed more and better study. The initial steps in the direction of inclusion were compromises. Anthropology, human ecology, and demography, all partially quantitative disciplines, were placed in the division of biological sciences, and proposals for research projects in those fields were reviewed. By 1955 a program of sociophysical sciences—mathematical social science, human geography, economic engineering, statistical design, and the history, philosophy, and sociology of science—was inserted into the mathematical, physical, and engineering division for the same purpose. By 1958 the board created an office of social science that brought all the social science disciplines together as parts of a concerted single research effort.

In 1956, the year before Sputnik, the NSF's appropriation was $40 million, which represented the growing respect of Congress for this obvious national asset.

As peacetime science research expanded, Congress acted to empower federal departments to conduct research consistent with their missions.

The four science agencies—the AEC, the ONR, the NIH, and the NSF—were required to encourage and financially support research in U.S. universities. This, however, did not fully satisfy the needs of long-established government departments for better understanding of the technical features of their missions. Government officials were aware that research in certain areas might improve the quality of service to the nation and possibly provide valuable new products and technologies. As a result, proposals to carry out research related to broadly defined missions were submitted to Congress, which approved them readily. These led to the establishment of new, permanent federal facilities, laboratories, and research stations whose purposes were to acquire data on long-term trends in phenomena that influence daily life.

A few examples indicate the direction of this activity. Early in 1948 the secretary of agriculture was authorized to establish laboratories for research and study of foot-and-mouth disease and other animal diseases that constituted a threat to the U.S. livestock industry. These laboratories complemented the private and state veterinary schools and were responsible for legislation to protect the public against diseases that beset both the animals and the humans who handled or consumed them.

In mid-1948 the Weather Bureau in the Department of Commerce was directed to study the causes and characteristics of thunderstorms, hurricanes, cyclones, and other atmospheric disturbances. Over the years, this research has led to better understanding of weather phenomena and to greater accuracy in predicting weather patterns.

A year later, funds were set aside for construction of new facilities for the National Advisory Committee for Aeronautics (NACA), including $10 million for wind tunnels at universities. These studies of flight were the basis of design for the jet aircraft that would traverse continents and oceans a decade later.

In July 1952 a congressional act authorized the secretaries of the army, navy, and air force to establish advisory committees and appoint part-time personnel necessary for research and development activities and to make five-year contracts with extension rights to carry out this program. The objective was to facilitate the performance of research and development in the armed services, but the authorization made allowance for participation and funding of university scientists and engineers, who were encouraged to engage in research only loosely related to the broadly defined military missions. The army and air force created new agencies to do this: the army through the Office of Ordnance Research (OOR) and the air force through the Air Force Office of Scientific Research and Development (AFOSRD); in most respects, these followed the precedent set by the ONR.

The agencies that emerged as auxiliaries to long-established government departments attracted young university scientists as trainees and older scientists as permanent staff. Their laboratories, like those of major industries, grew into an integral part of the scientific resource of the nation.

At first sight, the activity of the decade 1945–1955 appears to have been the product of infatuation with scientific research as the solution for the myriad problems facing the nation. It resembles a romantic interlude run wild, but when the individual actions by Congress are studied and the results evaluated after many years of experience, it is hard to find fault with either the early fascination with science or with its implementation. In short, the idea of funding science in universities and encouraging research in government agencies and industry was a good one. And the nation benefited.