THE INHERITANCE

CHAPTER 1

It was a dismal Thursday afternoon in Washington, the second day of January, 1947. Unnoticed among the hundreds of Government employees in the New War Department Building, ten men gathered at a conference table in a cramped office on the sixth floor. Looking southeast across Twenty-first Street toward Constitution Avenue, they could see the Washington Monument towering into the cold rain that was turning five inches of New Year's snow into a sea of slush. The four men on the far side of the table listened impassively as an aging graduate-school dean and a young Army colonel explained the intricacies of releasing wartime technical data without endangering national security.

The drabness of the surroundings and the pedestrian character of the discussion disguised the significance of the occasion. This was the first meeting of the United States Atomic Energy Commission since it had assumed control of the plants, laboratories, equipment, and personnel assembled during the war to produce the atomic bomb.[1]

Presiding that afternoon was the Chairman, David E. Lilienthal, a courageous lawyer and public servant who had risen to fame as head of the Tennessee Valley Authority. Nearby sat Sumner T. Pike, a hearty New Englander and experienced businessman who was returning to Washington after a term on the Securities and Exchange Commission. Next to him was William W. Waymack, an amiable gentleman farmer and newspaper editor from Iowa. The fourth Commissioner, Lewis L. Strauss, was a conservative investment banker with an interest in science and politics. Robert F. Bacher, the only scientific member, was on special assignment in Los Alamos, New Mexico, inspecting the nation's stockpile of atomic weapons.

Across the table was Carroll L. Wilson, a thirty-six-year-old engineer and former assistant to Karl T. Compton and Vannevar Bush. Just eight days before, President Truman had named him the Commission's general manager. With him were three other young men—Herbert S. Marks, the general counsel, George Fox Trowbridge, his assistant, and William T. Golden, administrative assistant to Commissioner Strauss. The elderly dean was Richard C. Tolman of the California Institute of Technology; the young Army officer, Colonel Kenneth E. Fields.

To the Commission the President had transferred "properties and an organization which, in magnitude, are comparable to the largest business enterprises of the country." In the days after the bombing of Hiroshima, journalists had described the more spectacular features of the Army's production plants, laboratories, and technical installations. Most American newspapers carried photographs of the huge buildings among the ridges of East Tennessee or the concrete monoliths in the desert of eastern Washington where materials for the bomb were made. Almost as many ran photographs of Robert Oppenheimer and other scientists who had constructed the first atomic weapons in a "super-secret" laboratory on an inaccessible mesa northwest of Santa Fe, New Mexico. The news services also described the research in the scientific laboratories which the Government had established at Chicago, Columbia, and California.

These, however, were just the highlights. Included in the transfer was a sprawling complex of men and equipment. The Army's transfer list ran to thirty-seven installations in nineteen states and Canada. With the facilities the Army would transfer 254 military officers, 1,688 enlisted men, 3,950 Government workers, and about 37,800 contractor employees. The entire project, representing a wartime investment of more than $2.2 billion, would cost an additional $300 million during the current fiscal year.

The Army could describe precisely the physical inheritance, but it was harder to measure the political and economic legacy, the temper of the times in which the new commission found itself. On New Year's Day, 1947, the President had taken a step toward ending World War II with a surprise proclamation recognizing the termination of hostilities. The sixteen months since V-J Day had been a chaotic period of transition. While Harry S. Truman struggled with the burdens of the Presidency, the nation moved from the flush of victory into the somber realities of reconversion and readjustment—inflation, strikes, and political strife at home and starvation, nationalism, and new power relationships abroad.

On the domestic scene, political fortunes were changing quickly. Franklin D. Roosevelt's death and the growing coalition between southern Democrats and conservative Republicans in Congress had undermined the majorities the President had carried to victory in 1944. In the face of such unpromising odds, his successor launched in September, 1945, a twenty-one-point reconversion plan as a return to the "progressive" tradition and the New Deal. Truman had to fight every inch of the way against the strong current of reaction to wartime controls, high taxes, and "government from Washington." A wave of strikes in major industries in 1946 alarmed business interests while the President's harsh action against John L. Lewis for his open defiance of the Government in the coal strike alienated some of the party's labor support. The Administration's efforts to extend price controls, rationing, and universal military training in peacetime met with failure. By the end of 1946, the Administration had made but two lasting achievements. One was the Employment Act, a mutilated version of the original Murray bill but still a step in the direction of economic controls. The other was the Atomic Energy Act, which passed only after a year of acrimonious debate by 9 votes out of 200 in the House of Representatives. Within the Administration itself, smouldering hostility ignited such explosions as the resignation of Harold L. Ickes as Secretary of the Interior and the dismissal of Henry A. Wallace as Secretary of Commerce. These awkward episodes, the President's failure to control Congress, and the inflation that occurred with the abandonment of price controls just before the November elections gave the Republicans their most effective slogan, "Had Enough?"

That 1946 would be a Republican year seemed certain, but few expected the landslide that occurred at the polls in November. Gaining twelve seats in the Senate and more than fifty in the House, the GOP took control of Congress and captured governorships in twenty-five of thirty-two non-Southern states. When the Eightieth Congress assembled on January 3, Arthur H. Vandenberg became president pro tempore of the Senate and Joseph W. Martin speaker of the House. Robert A. Taft, the Republican power in the Senate, stepped aside to permit Wallace H. White of Maine to serve as Senate majority leader. Taft was reserving for himself the chairmanship of the Senate Labor Committee, which would seek legislation to prevent recurrence of the strikes that had paralyzed the nation in 1946.

The character of the Senate, which would pass on the interim appointments to the Atomic Energy Commission, seemed transformed by the presence of such newcomers as Joseph R. McCarthy of Wisconsin, Henry Cabot Lodge, Jr., of Massachusetts, Irving M. Ives of New York, Edward Martin of Pennsylvania, Arthur V. Watkins of Utah, and John W. Bricker of Ohio. New members of the House included some young war veterans destined to make their mark: William G. Stratton of Illinois, Richard M. Nixon of California, and John F. Kennedy of Massachusetts.

Back in power after fourteen years, the Republicans were in a fighting mood as they talked of a 20-per-cent cut in federal taxes and appropriations, a drastic revision of the Wagner Labor Act, complete reorganization of the housing program, and a critical examination of reciprocal trade policies and foreign spending. Assuming that the Republican nominee in 1948 would succeed Truman as President, the principal aspirants were already engaged in a struggle for power. Robert A. Taft, Senate spokesman for the Midwest, seemed the leading contender. Thomas E. Dewey had bounced back from his defeat in 1944 with a resounding victory in the gubernatorial race in New York. Not out of the running were a dozen other hopefuls led by Harold E. Stassen of Minnesota, who had already announced himself a candidate.

Developments abroad looked no more promising for the Administration than did those at home. Though farm output in the United States hit a record high in 1946, millions of Europeans faced starvation. The devastation in Germany continued to tax the descriptive powers of American journalists. France was threatened by inflation and colonial unrest. The United Kingdom, for which the war had proved almost fatal, accepted indefinite rationing of consumer goods and transferred its coal mines to government ownership as a first step toward nationalizing basic industries.

The upheaval of war and the collapse of Europe stimulated political and economic aspirations throughout the world. In the Middle East, the British found themselves caught in the cross fire of Jewish and Arab nationalism. In India, Mahatma Gandhi sparked a new drive for independence. Southeast Asia was in tumult, and China remained an enigma. Despite a year of painful negotiation, General George C. Marshall had been unable to quench the fires of civil war which threatened to disrupt all Asia. In the Far East, only Japan, now under the firm hand of General Douglas MacArthur, seemed headed toward stability.

With the decline of Britain and France came the accelerated rise of the Soviet Union. In 1945, there had been confidence that somehow the victorious allies could establish a new era of peace and human freedom. But this dream grew dim in 1946, when countries bordering the Communist world from Germany to Korea felt the aggressive pressure of Soviet strength.

The United Nations had yet to prove itself. Secretary-General Trygve H. Lie warned that while the new association of states had laid a sound foundation on which to build peace, the state of the world left no room for "easy optimism." Certainly this judgment was borne out by the experience of the United Nations Atomic Energy Commission. After six months of negotiation, American delegate Bernard M. Baruch found the United States and the Soviet Union distressingly far apart on the requirements for effective international control.

The survival of western democracy seemed to depend upon the United States. Yet Americans were obviously uncomfortable with the responsibilities of world leadership. Europeans feared that the November elections might signal a withdrawal from overseas commitments. Their fears fed on Washington reports of a tighter attitude on foreign loans, of an end to support for the United Nations Relief and Rehabilitation Administration, and of open misgivings in the new Congress about the nation's foreign policy.

Undaunted by the universal uncertainty, Chairman Lilienthal prepared to enter the new world of atomic energy with an untried approach to public administration. The authors of the Atomic Energy Act themselves admitted that it was "a radical piece of legislation." They asserted that "never before in the peacetime history of the United States has Congress es-

tablished an administrative agency vested with such sweeping authority and entrusted with such portentous responsibilities. . . . The Act creates a government monopoly of the sources of atomic energy and buttresses this position with a variety of broad governmental powers and prohibitions on private activity. The field of atomic energy is made an island of socialism in the midst of a free enterprise economy." [2]

The Act was no more revolutionary, however, than the forces that produced it. Never before had man exploited a new dimension of power so suddenly. Though the first reactions to Hiroshima now seemed exaggerated and hackneyed, there was no gainsaying the words of one congressman who declared the control of atomic energy "a matter for the ages." The Commissioners understood as well as other Americans the predicament in which they found themselves. Chairman Lilienthal reportedly said at the Commission's first meeting in November: "I have taken the oath of office several times before in my life, but the last four words never had the meaning to me they have today. So I'd just like to begin by repeating them—'So help me God.' " [3]

To their task Lilienthal and his colleagues brought many talents. All had held important posts in the Government. Together they possessed a range of experience touching many aspects of American life, but only Bacher had an intimate knowledge of the wartime atomic energy program. For the others, as for most of their fellow citizens, their inheritance lay in the secret recesses of a military enterprise. In the guarded language of the Smyth Report they had a glimpse into the past. They could not, however, recapture the human experience that lay behind the stiff prose of the official report. Time had already blurred the anguished moments of blind decision, the chance event, the unpredicted accomplishments upon which the success of the project depended.

Few men besides Vannevar Bush, James B. Conant, and Leslie R. Groves knew more than a fragment of the story. It began early in 1939 with the discovery of the fission of uranium and the first efforts to win support from the federal government for nuclear research. At a time when Nazi threats had only begun to raise questions about American isolationism, proposals for co-operation between government and science had already reached the White House. The idea was so new that only British confidence that fission could influence the outcome of the war and Bush's skillful leadership gave the United States the beginnings of an atomic energy program by the eve of Pearl Harbor.

In 1942, the lack of reliable information about fundamental processes paralyzed efforts of the Office of Scientific Research and Development to select the most promising method of producing fissionable material. June brought momentous decisions dictated not by experimental evidence but by the desperate race for the bomb. Then came the painful transition from research to process development and from OSRD to Army control. Through the summer the project faltered in indecision and frustration until Bush won full Army support. In the meantime, Conant and his OSRD committee continued their search for the best route to a weapon. The prospects for the various approaches fluctuated from day to day. One process was dropped, another revived after a routine inspection trip by a reviewing committee, a third supported on the strength of an experiment not yet performed. The year ended with a climactic series of decisions which spelled out the nation's commitment to the atomic bomb as a weapon in World War II.

By 1943, the project had grown so rapidly in so many directions that no one individual could follow it. As the year began, earthmovers were already carving huge excavations out of the narrow Tennessee valleys for three plants and a new American city. Across the country a network of university laboratories and private contractors were designing and fabricating components to specifications unprecedented in mass-production efforts. Now, to follow the fortunes of the bomb, one had to observe physicists assembling vacuum tanks and high-voltage equipment at the University of California Radiation Laboratory, engineers laying precision-machined blocks of graphite within a concrete cube in Tennessee, chemists testing fragile pieces of porous metal in corrosive gases at Columbia University, scientists exploring the fundamentals of the fission process in New Mexico, and Army officers planning the transformation of a desert into an industrial city in the Pacific Northwest. Here one could feel the pulse of progress, share the moments of success and failure, watch hopes fade away one by one as Nature frustrated repeated attempts to solve the riddle of producing fissionable material and building an adequate weapon. No one who lived through the black days of June, 1944, could ever say that success was predestined.

But before the end of 1944, success was in sight. Engineers at Oak Ridge had devised an ingenious plan to operate the separation plants as a unit while taking maximum advantage of the peculiar capabilities of each. By the spring of 1945, the Oak Ridge complex was producing uranium 235 in significant amounts. The crisis at Hanford had passed, and increasing quantities of plutonium were being shipped to Los Alamos. Months of intensive research had made the bomb a certainty, though no one yet knew how powerful it would be.

Though the war was far from over in the summer of 1944, it was time to think about postwar arrangements, both domestic and international. Among the scientific men who shared this belief, Bush and Conant were in a uniquely favorable position to act. Believing that free interchange of scientific information under international auspices offered the only hope of averting a catastrophic arms race, they opposed any step that might bind the United States so closely to its British ally as to prejudice the chances for Russian co-operation. At Quebec in 1943, their views on a strictly limited form of Anglo-American technical interchange had prevailed, but in September, 1944, President Roosevelt and Prime Minister Winston Churchill

agreed on full collaboration in the military and commercial applications of atomic energy after the defeat of Japan. Though not aware of the President's commitment, Bush and Conant knew the trend of his thought. They alerted Secretary of War Henry L. Stimson to the danger and urged the importance of naming a high-level policy committee to advise on the whole sweep of postwar problems. Then the fates intervened in the guise of distracting issues, the stress of war, and the death of the President. Not until the first week in May, 1945, did Stimson appoint his Interim Committee.

When the Interim Committee met, Stimson had turned his full attention to the Far East. For him, the issue was not whether to use the atomic bomb but how to end the war against Japan. If the bomb would foreclose the prospect of a long and bloody conflict, he was disposed to use it. Aware of the threat that atomic energy posed for the future, Stimson urged Truman to tell Stalin of American hopes for future international control before the United States dropped the weapon in combat. The President might have followed Stimson's advice had not Russian conduct at Potsdam discouraged both men about the chances for fruitful co-operation. By that time, the Alamogordo tests had shown that the atomic arm was more powerful than anyone had dared hope. With Japanese leaders offering little reason to expect an early acceptable surrender, the President simply told the Soviet chief that the United States was at work on an unusually powerful new weapon and allowed nuclear operations to proceed against Japan.

The bombing of Hiroshima made atomic energy a topic for public discussion. For weeks, the Truman Administration groped for a policy on domestic and international control. Not until his message to Congress on October 3, 1945, did the President establish the bare outlines of such a course. Even then, his position on international control did not clearly emerge until the November meetings with the British and Canadian Prime Ministers in Washington.

Meanwhile, a combination of parliamentary maneuverings in the Senate, organized opposition among the atomic scientists, and second thoughts in the White House had defeated the War Department's bill to establish an atomic energy commission. An alliance of scientists and senators now took control of legislation. Senator Brien McMahon organized his Special Committee and introduced a new bill excluding the military services from any real voice in developing atomic energy. While the scientists' lobby and pressure groups focused public debate on the civilian-military control issue, McMahon fought a losing battle with the conservative majority of his committee. The turning point came in late February, 1946, when Senator Vandenberg introduced an amendment to strengthen the hand of the military in atomic energy policy. Although McMahon denounced the amendment as a threat to civilian control, it proved a blessing in disguise. It captured the imagination of the American public and gave the McMahon bill the popular support it needed to pass the Senate. A long battle, waged for the most part behind closed doors in the White House and in Congressional committee rooms, was still necessary to win the House. Almost a year to the day after the Hiroshima attack, the President signed the act establishing the Commission.

Hopes for international control rose in late December, 1945, when the Soviet Union accepted the Anglo-American invitation to join in asking the United Nations General Assembly to establish a commission on atomic energy. Early in 1946 Secretary of State Byrnes started policy studies which resulted in the Acheson-Lilienthal plan for an atomic development authority. Now high principle, now sharp political infighting ruled in Washington conference rooms and executive offices. The proposal that Bernard Baruch presented in June was compounded in almost equal parts of imagination, prudence, and yearning for a certainty that many men considered illusory. In the summer and autumn of 1946, the prospect of international agreement faded as the Soviet Union insisted on outlawing the weapon before investigating controls. Some Americans, seeking to cast any beam from their own eyes, criticized Baruch's tactics and the substance of the United States plan itself. Yet by the time Lilienthal and his four colleagues took over the nation's nuclear program, one fact stood clear: the United States had offered to yield its monopoly of atomic weapons. True, it insisted on abandoning its favored position gradually, but it did not demand the right to dictate the timing of the transition process.

On that bleak afternoon of January 2, 1947, the Commissioners were gravely aware of their responsibilities and their opportunities. Six weeks of preparation had taught them how vast was their inheritance and how little they knew about how it came to be. They expected to learn more as the months advanced; their natural feelings of personal inadequacy would diminish. But many years would pass, they thought, before the story of their inheritance could stand forth in ample detail and just proportion. The Commissioners could not anticipate that when the passage of a decade and a half made this possible, mankind would be edging into the new frontiers of space. They did not realize that atomic energy so soon would appear as merely the first of a continuing series of revolutionary demands that twentieth-century science would make on the capacity of human nature to adjust to the physical universe. Had they been able to see into the future, the Commissioners would have believed even more strongly in the surpassing importance of their task.

IN THE BEGINNING

CHAPTER 2

On the surface, there was nothing extraordinary about the first days of January, 1939. The American people were enjoying life's little diversions. They read in the newspapers that David O. Selznick had selected Vivien Leigh to play Scarlett O'Hara in *Gone With the Wind*, that Broadway was acclaiming Mary Martin as the season's musical-comedy find, that Brenda Frazier would make a $50,000 debut at the Ritz-Carlton. Many families contemplated a summer trip to New York's World of Tomorrow or San Francisco's Golden Gate International Exposition. Those in the market for a new automobile considered the Oldsmobile, advertised for as little as $777, or perhaps the Pontiac, listed at $862. Even business and politics, so long in turmoil, seemed to be returning to normal. The summer before, employment and production indices had begun to move up from recession lows. In November, 1938, the Republican Party won large gains in both Senate and House. Hard on President Roosevelt's failure to purge conservative members from his own party, the election returns suggested that the New Deal had run its course.

Yet no preoccupations of the moment, no mere redressing of the political balance, could still an underlying uneasiness caused by events abroad. Between 1935 and 1937, Congress had reacted to the signs of war in Europe and the Far East by passing a series of neutrality acts, laws which reflected a disillusionment with the results of American intervention in 1917. Hardly was the neutrality storm cellar complete, when the structure of international relations began to disintegrate at an alarming rate. In July and August of 1937, Japan expanded the incident at the Marco Polo bridge into a massive assault on China. During March of the following year, Nazi legions occupied and annexed Austria. Then late in September, 1938, Hitler's threats to take the Sudetenland intimidated Chamberlain and Daladier into appeasing him at the expense of Czechoslovakia.

Distressed by the Munich crisis, the President determined to make good use of the time that remained. Early in October, he announced an accelerated program of defense spending and projected plans for great increases in aircraft production. On Christmas Eve, his diplomacy bore fruit in the Declaration of Lima, which set up crisis machinery for assembling the foreign ministers of the American republics to take action in the common defense. Roosevelt was hopeful for a freer hand in countering the aggressors, but he recognized the difficulty of persuading Congress to revise the neutrality laws. Not that there was any significant pro-German or pro-Japanese sentiment in the United States. The absurd posturing of Hitler, the pogroms in Germany, the brutality of Japanese soldiers in China had forestalled that. But the overwhelming majority of Americans were resolved to take no action that might drag them into war.

The President's message to Congress on January 4, 1939, reflected the growing tension. With southern Democrats, whose support he needed for his foreign policy, no longer willing to follow his lead in domestic matters, Roosevelt rang down the curtain on the New Deal. He called for no new reform legislation and requested deficit spending only until recovery was complete. It was the international situation that now dictated the turn of events. In his address to Congress and in his budget message of the next day, Roosevelt recommended an augmented defense appropriation of almost $2 billion.

IMPACT OF FISSION

On a wintry afternoon twelve days after the President spoke to Congress, the liner *Drottningholm* was eased into its berth at New York. Aboard was the distinguished Danish theoretical physicist, Niels Bohr. Enrico and Laura Fermi were among those who met him at the pier. Their friend seemed to have aged perceptibly in the month since they had stopped off to see him at Copenhagen on their way to the United States from Stockholm, where the Italian physicist had accepted a Nobel Prize. There was good reason for Bohr's appearance. He was disturbed by the threat of war in Europe and by his knowledge of a recent scientific discovery of revolutionary implications. Late in 1938, Otto Hahn and Fritz Strassmann, working at the Kaiser Wilhelm Institute for Chemistry in Berlin, had discovered a radioactive barium isotope among the products resulting from their bombardment of uranium with neutrons. Hahn recognized the significance of this at once, but instead of proclaiming it himself, he chivalrously communicated his findings to Lise Meitner, an Austrian colleague who recently had been forced to flee Germany by the threat of Nazi racial laws. Fräulein Meitner and her nephew, Otto R. Frisch, concluded that the presence of barium meant that a new type of nuclear reaction had taken place—fission. They went to Copenhagen at once, where they advanced to Bohr the theory that the uranium

nucleus had split into two lighter elements in the middle range of the periodic table. Part of the enormous energy required to hold the component neutrons and protons together in the heavy uranium nucleus had been released. Meitner and Frisch outlined an experiment to verify their hypothesis. Some days after Bohr arrived in the United States, he received a telegram from Frisch announcing that laboratory results had confirmed the theory.[1]

The word spread quickly. Bohr went at once to Princeton, where he was to spend a few months at the Institute for Advanced Study. Physicists there were greatly impressed by the possible implications of the discovery. Isidor I. Rabi, in Princeton on sabbatical leave, rushed back to Columbia University the next morning to talk with Fermi. On January 26, Bohr and Fermi opened the Fifth Washington Conference on Theoretical Physics with a discussion of the exciting developments abroad. Press reports now flashed to centers of physics research throughout the nation. Soon American scientists had the full story, for the *Physical Review* of February 15 carried an authoritative account by Bohr.[2]

The discovery of fission was stimulating enough from a purely scientific standpoint, but the finding had such a galvanic impact because it pointed to the possibility of a chain, or self-sustaining, reaction. Physicists thought it highly probable that fission released secondary neutrons. Should these be effective in splitting other uranium nuclei, which in turn would liberate neutrons, it might be possible to generate a large amount of energy. If the process could be controlled, a new source of heat and power would be available. If it were allowed to progress unchecked, an explosive of tremendous force might be possible.

By 1939, American physicists were in a strong position to exploit the breakthrough. True, physics had been slow to develop on their side of the Atlantic. At the turn of the century, American physics had been graced by a few great names—Henry, Gibbs, Michelson, and Rowland—but these few could scarcely compare with European giants such as Maxwell, Kelvin, Joule, Rankine, Helmholtz, and Planck. College instruction had then been poor despite the efforts of a few universities to improve. Even textbooks were translations of European works. Not until 1893 was the *Physical Review* founded. Not until 1899 was the American Physical Society established. The discipline progressed rapidly, however, in the years prior to the first World War, and by the nineteen-twenties universities such as California, Chicago, Columbia, Cornell, Harvard, Johns Hopkins, and Princeton were offering good training to increased numbers of students. To obtain the best advanced instruction, it was still necessary to go abroad, particularly to Germany. Fortunately, many young scholars received Rockefeller-financed, National Research Council fellowships for this purpose. At European universities American students felt that they had a better, broader education than perhaps 95 per cent of their European classmates. Yet from their experience on the Continent they gained an inspiration, a feel for their subject, that was more important than any considerations of factual knowledge or technique.

In the nineteen-thirties, physical studies flourished in the United States. The quality of graduate work was high. Probably the depression helped encourage advanced study and postdoctoral research. There was little else to do. Certainly the National Research Council fellowships enabled scores of young physicists to establish the habit of research. Europe may still have had more giants, but it could not compare in the number of lesser known physicists. As the decade wore on, American scientific ranks gained further as some of the most talented Europeans came to the United States, seeking refuge from persecution in their homelands.

Theoretical studies experienced a healthy development, but the great strength of the United States was in experimental physics. This interest led naturally to large-scale equipment. Americans played a leading role in the development of the mass spectrograph, essential in studying the isotopic forms of the elements. In 1930, Ernest O. Lawrence, an imaginative young experimenter at the Berkeley campus of the University of California, constructed his first cyclotron. This contrivance, which whirled charged particles to tremendous speeds under the influence of a steady magnetic field and a rapidly oscillating electrical field, soon became a research tool of vital importance. In 1931, Robert J. Van de Graaff, a National Research Fellow at Princeton, developed his electrostatic generator, another device for producing a powerful beam of subatomic particles. By 1939, the United States was pre-eminent in work requiring elaborate and expensive equipment.

Eagerly, American physicists followed Chadwick's work, which reached fruition in 1932 with his discovery of the neutron. They noted Cockcroft's and Walton's experimental demonstration of Einstein's proposition that mass and energy were equivalent. They read avidly of Fermi's uranium bombardments and the efforts of Joliot-Curie and Savitch to interpret them. But Americans did more than observe. The *Physical Review*, rapidly evolving from a provincial journal into one of the world's great scientific periodicals, bulged with reports of their own experimental and theoretical investigations. Fascinated by the possibilities inherent in neutron reactions, Americans believed that the world was on the threshold of nuclear power and that everything waited on some self-perpetuating mechanism. At the very time of the Hahn-Strassmann breakthrough, Philip H. Abelson, a Ph.D. candidate at Berkeley, was pursuing a line of investigation that in a few weeks would have led him almost certainly to the discovery of nuclear fission.[3]

The first experimental task facing scientists in the early days of 1939 was to confirm the Hahn-Strassmann-Meitner results. This came rapidly in the United States, as elsewhere. The issue of the *Physical Review* that carried

Bohr's letter contained reports of corroborating experiments at the University of California, Johns Hopkins, and the Carnegie Institution of Washington. The next issue related experiments undertaken at Columbia just after Bohr's arrival in which Fermi and John R. Dunning, joined by a number of younger collaborators, further demonstrated the validity of the results obtained abroad.[4]

This was only the beginning. Scientists throughout the world launched a comprehensive effort to throw light on the phenomena of fission. They published nearly one hundred articles on the subject before the end of 1939. All the great centers of American physical research took up the challenge. In the realm of theory, the prime achievement was a study carried out at Princeton by Bohr and John A. Wheeler. Their work, published in September as "The Mechanism of Nuclear Fission," was rich in insights destined to aid many another scientist in the years ahead.[5] In the experimental field, nothing was more immediately significant than the work being done at Columbia on the possibility of a chain reaction. It was an investigation for which Morningside Heights was well fitted. Here at the Michael Pupin Laboratory was Dunning with the cyclotron and other equipment he had acquired for neutron-reaction studies. Here were Herbert L. Anderson, a gifted graduate student, and Walter H. Zinn, a physicist at City College who did his research in the Columbia laboratories. Here were Fermi, who had no intention of returning to his native land, and Leo Szilard, a Hungarian scientist who had come without benefit of a faculty appointment to work with Fermi. Fortunately, this team was under a sympathetic if somewhat conservative administrator, George B. Pegram. A physicist himself, Pegram was now dean of the graduate faculties.

The men at Columbia had seen from the first that the key to the self-sustaining reaction was the release of neutrons on fission of the uranium atom. Like physicists generally, they had guessed that neutrons were emitted. Their experiments, along with others conducted both in the United States and abroad, soon indicated that this indeed was true. Once the neutron question was settled, another rose to demand attention. Was a chain reaction possible in natural uranium? At Columbia and elsewhere physicists disagreed over which isotope fissioned with slow neutrons, neutrons which traveled at the energies known most likely to produce fission. Was it the rare 235, considerably less than 1 per cent of the natural element, or the abundant 238? Dunning thought 235 was responsible, while Fermi inclined toward 238. Dunning was impressed by the small fission cross section—the physicists' term for probability—of natural uranium. He thought it indicated only a small chance for a chain reaction. But if uranium 235 was the isotope subject to slow-neutron fission, and if it could be concentrated, he considered the chain reaction a certainty. Fermi accepted his colleague's reasoning, but even if U-235 should prove the key, he was content to try for a chain reaction in natural, unconcentrated uranium because of the extreme difficulty and expense of separating the isotopes. To settle this debate, Fermi and Dunning agreed on a co-ordinated investigation. The Italian would try for a chain reaction in natural uranium, while the American would acquire small samples of concentrated U-235 and see if his views on its susceptibility to fission were correct.[6]

Fermi's first effort to ascertain whether the conditions of a chain reaction existed in normal uranium was to measure the number of neutrons produced per fission. By the middle of March, preliminary experiments indicated that the average was two.[7] The next objective was to discover how extensive was nonfission absorption. Fermi, Szilard, and Anderson knew that neutrons might be captured without fission and produce a radioactive isotope of uranium, U-239. If this happened on an excessive scale, too few neutrons would live to propagate a chain reaction. The experimenters placed a neutron source in the center of a large water tank and made comparisons, with and without uranium in the water, of the number of slow neutrons present. These measurements led them to conclude that a chain reaction could be maintained in a system in which two requisites were met. First, neutrons had to be slowed to low, or thermal, energies without much absorption. Second, they had to be absorbed mostly by uranium rather than by another element. Fermi and Szilard had doubts, however, about the proper agent for slowing down, or moderating, the neutrons. It would have to be some material of low atomic weight. Neutrons, common sense indicated, would lose speed more quickly by collision with light rather than with heavy atoms. Water, which Fermi had used because it was two-thirds hydrogen, had exhibited a tendency to absorb neutrons. On July 3, 1939, the same day the editor of the *Physical Review* received the Columbia results, Szilard wrote Fermi to suggest that carbon might be a good substitute. Szilard saw heavy hydrogen in the form of heavy water as another possibility, for it had less tendency to absorb neutrons than ordinary hydrogen, but he did not know if it could be obtained in sufficient quantity. A few days later, he was so convinced of the advantageous physical properties of carbon that he thought the Columbia group should proceed at once with a large-scale trial employing a graphite moderator without even awaiting the outcome of experiments to determine its neutron-absorption characteristics.[8]

FIRST APPEALS FOR FEDERAL SUPPORT

Publication of the results of the absorption experiments in the summer of 1939 marked a temporary halt to intensive work on the chain reaction at Pupin Laboratory. Fermi departed for the University of Michigan to study cosmic rays. Anderson, his assistant, devoted his time to finishing his Ph.D. investigations, while Szilard, though full of suggestions for accelerating the

experimental work, concentrated on finding a way to alert the federal government to the significance of fission.

Actually, a branch of the government had already been approached. On March 16, Dean Pegram wrote Admiral Stanford C. Hooper, technical assistant to the Chief of Naval Operations, to say that Fermi, who was traveling to Washington on another matter, would be glad to tell Hooper of the experiments at Columbia. It was possible, Pegram wrote, that uranium might be used as an explosive that would "liberate a million times as much energy per pound as any known explosive." Pegram thought the probabilities were against this but that even the barest possibility should not be ignored. At the Navy Department the next day, Fermi talked for an hour to a group that included a number of naval officers, two civilian scientists from the Naval Research Laboratory, and several officers from the Army's Bureau of Ordnance. Fermi explained the Columbia efforts to discover whether or not a chain reaction could take place. He was not sure that the experiments would yield an affirmative answer, but if they did, it might be possible to employ uranium as an explosive. After some questioning, a Navy spokesman told Fermi that the Department was anxious to maintain contact with the Columbia experiments and undoubtedly would have representatives call in person.[9]

The most responsive of the listeners that afternoon were the scientists of the Naval Research Laboratory. They had a long-standing interest in a source of power that would permit protracted undersea operations by freeing submarines from dependence on tremendous supplies of oxygen. As soon as the news of fission broke in January, they had contacted the men at the Carnegie Institution who were checking the work of Meitner and Frisch. Just three days after the conference with Fermi, Admiral Harold G. Bowen, director of the NRL, recommended that the Bureau of Engineering help finance investigation of the power potential of uranium. The Bureau allotted $1,500 to the Carnegie Institution, which agreed to co-operate but for reasons of internal policy did not accept the grant. The NRL also approached Jesse W. Beams, a centrifuge expert at the University of Virginia, on isotope separation.[10]

The initiative for a new overture to the federal government in the summer of 1939 came in large part from Szilard, an impetuous, imaginative physicist who was at his best in goading others to action. The news of fission alarmed him, for he feared that it might lead to powerful explosives which would be dangerous in general and particularly so in the hands of Nazi Germany. Like many others, he hoped a bomb would prove impossible. But until this could be established, there seemed only one safe course: to pursue the work vigorously.[11] Szilard had been zealous on behalf of the Columbia experiments and had even borrowed money to rent radium for use in a neutron source.

Szilard was eager for some sort of federal action. At a June meeting of the American Physical Society in Princeton, he had consulted Ross Gunn, who, as the technical adviser of the Naval Research Laboratory, was at the center of the Navy's interest in the potential of uranium. On July 10, Gunn informed him that though the NRL was anxious to co-operate, restrictions on government contracts for services made it impossible to carry through any agreement that would be helpful.[12]

Frustrated, Szilard talked over the situation with physicist Eugene P. Wigner, also a native of Hungary. Szilard by now was convinced that the uranium-graphite experiment might quickly prove successful if only it could be carried out. More than ever, he thought it imperative to get on with the work. Besides, it was high time to take steps to keep the uranium ore of the Belgian Congo out of German hands. It occurred to the two physicists that Albert Einstein was the logical person to alert the Belgians, for he knew the royal family. They saw Einstein, who agreed to dictate a letter of warning, though to someone below that rank. Since this maneuver raised the propriety of communicating with a foreign government, Wigner suggested that they send the Department of State a copy with a note that Einstein would dispatch the letter in two weeks unless he received advice to the contrary. This, however, would do nothing to expedite research in the United States. Szilard believed that they should make some direct advance to the government in Washington. At the suggestion of Gustav Stolper, a Viennese economist and a friend of long standing, he went to see Alexander Sachs, a Lehman Corporation economist reputed to have ready access to the White House.

Quiet and unpretentious in appearance but curiously florid and involute in speech, Sachs prided himself on his skill in analyzing current developments and predicting the course of events. He specialized in "prehistory," he liked to say. Since 1936, when he had heard Lord Rutherford lecture, the work of the atomic physicists had intrigued him. Then early in February, 1939, while Sachs was visiting in Princeton, Frank Aydelotte, director of the Institute for Advanced Study, showed him a copy of a letter that Bohr had addressed to the editor of *Nature*. Sachs's excitement increased as the months went by and further experiments were reported. By the time Szilard called on him in July, he remembered some years later, he had already pointed out to the President the crucial character of the new developments. From Roosevelt, Sachs understood that the Navy had decided not to push uranium research, largely because of the negative attitude of Fermi and Pegram.

To approach the President successfully, Sachs believed it was necessary to counter the impression created by the Columbia physicists. This would require the testimony of a scientist more eminent than Szilard. The obvious solution was to enlist the name of Einstein. A letter should be prepared for his signature. Sachs could insure that such a communication, along with supporting scientific papers, received Roosevelt's attention.

The letter that emerged from conferences between Sachs and Szilard reported that recent work by Fermi and Szilard in America and by Joliot-Curie in France made a uranium chain reaction almost a certainty in the immediate

future. This would mean the generation of vast amounts of power and the creation of new radium-like elements. It was conceivable, though still not definite, that extremely powerful bombs could be constructed. These might prove too heavy to be dropped from an airplane, but they could be carried by boat and exploded in a port. The supplies of uranium ore in the United States were not extensive. Although there was some good ore in Canada and in Czechoslovakia, the Belgian Congo was the most important source. Something ought to be done to maintain contact between the Administration and the physicists working on the atom. Perhaps the President could assign someone, possibly in an unofficial capacity, to keep the appropriate government departments informed and make recommendations for action, particularly on raw materials. This agent might also seek to speed research by soliciting contributions from private individuals and by obtaining the co-operation of industrial laboratories. Closing the letter was a warning of German interest. The Reich had stopped the sale of uranium from Czechoslovakian mines.

At Sachs's request, Szilard drafted an accompanying memorandum. Seeking to explain more clearly the underlying science, the physicist stressed that a chain reaction based on fission by slow neutrons seemed almost certain even though it had not yet been proved in a large-scale experiment. Whether a chain reaction could be maintained with fast neutrons was not so certain. If it could be, it might be possible to contrive extremely dangerous bombs.

It was not hard to persuade Einstein to sign the letter, but before Sachs could take the completed dossier to Roosevelt, war broke out in Europe. Sachs delayed, for he wanted to present the case to the President in person, so that the information "would come in by way of the ear and not as a sort of mascara on the eye." He knew that Roosevelt, preoccupied with the international crisis and his fight to win repeal of the arms-embargo from a reluctant Congress, was unlikely to give the uranium recommendations adequate attention. But early in October, 1939, the time seemed more propitious, and Sachs arranged an appointment for the eleventh. At the White House, the President's secretary, General Edwin M. Watson, had called in two ordnance specialists from the Army and Navy, Colonel Keith F. Adamson and Commander Gilbert C. Hoover. After Sachs had explained his mission to them, he was taken in to see the Chief Executive. Sachs read aloud his covering letter, which emphasized the same ideas as the Einstein communication but was more pointed on the need for funds. As the interview drew to a close, Roosevelt remarked, "Alex, what you are after is to see that the Nazis don't blow us up." Then he called in "Pa" Watson and announced, "This requires action." [13]

This appeal for federal encouragement, if not support, of research touched a theme that went back to the Constitutional Convention of 1787. The powers expressly granted the general government seemed to imply a place for science, but just what this might mean awaited the resolution of constitutional issues that involved science only tangentially. As it worked out, Americans were slow to accept the idea that the federal government should have a permanent scientific establishment. Not until after the Civil War did a well-diversified corps of scientific bureaus evolve. By 1916, the process was largely complete. Since the several units had appeared at different times under widely varying auspices in response to the demands of society, there was no central organization. The emphasis was on applied rather than basic research.

This setup seemed reasonably well adapted to the day-to-day requirements of the government. All efforts had failed, however, to work out a satisfactory arrangement by which American science as a whole could serve in an advisory capacity in times of national emergency. The first attempt to achieve such an arrangement was the creation of the National Academy of Sciences. A group of scientists led by Alexander Dallas Bache made the Civil War the occasion for promoting their long-cherished plan to establish a self-perpetuating national academy which should serve the dual purpose of honoring scholarly attainment and of advising the government. Taking advantage of the end-of-session rush in March, 1863, they spirited the necessary legislation through Congress. Unfortunately, the wartime accomplishments of the National Academy were slight. Only through the efforts of Joseph Henry, the secretary of the Smithsonian, did the National Academy survive the crisis which saw its birth.

The first World War brought forth another effort to forge a working relationship between government and science. The National Research Council was organized in 1916 under the auspices of the National Academy to broaden the base of scientific and technical counsel. Not limited to members of the National Academy, the NRC sought the help of scientists generally, whether they were at work in government, the universities, private foundations, or in industry. Though it met the test of war by establishing co-operative research on a large scale and by serving as a scientific clearinghouse, it left much to be desired. Never financed independently, the only effective way it could obtain funds from the military was to have its scientists commissioned. It was further handicapped by losing to the services the initiative of suggesting projects. After the Armistice, the NRC evolved into an agency for stimulating research by dispensing Rockefeller and Carnegie money. Though this was useful enough, the council lost the capacity to serve as an active scientific adviser. In many ways, a more significant development of the war years was the establishment of the National Advisory Committee for Aeronautics, an independent board of both government and private members with functions less advisory than executive.

It was not surprising that a new effort at establishing efficient liaison between government and science emerged in the summer of 1933. Isaiah Bowman, chairman of the National Research Council, used Henry Wallace's

request for advice on the reorganization of the Weather Bureau as an opportunity to advocate a general review of government science. The result was a Presidential order creating a Science Advisory Board with authority under the National Academy and the NRC to appoint committees on problems in the various departments. This order named Karl T. Compton chairman. Compton, president of the Massachusetts Institute of Technology, promptly put subcommittees to work studying the government bureaus, but he had larger plans, plans which amounted to a New Deal for science. It was his idea that a large sum—in the final version $75 million in five years—should be spent to support scientific and engineering research. Programs would be formulated by the National Academy, the National Research Council, and a new advisory panel. Compton's dreams failed to win approval, apparently because of their scale and because of a reluctance to adopt a program that would support the natural sciences to the exclusion of other fields of learning. The Science Advisory Board itself did not survive for long. Thus was lost an opportunity not only to support science in the monetary sense but also to establish a rational basis for co-operation between the government and the great centers of investigation. There was still a hope that the National Resources Committee, which had its origin in the faith of social scientists in planning as the basis for sound governmental operation, might accomplish something. But although its science committee made a brilliant study of the federal research agencies and took the broad view that research was a basic national resource, it never gained the administrative position or the support from scientists that were essential for it to become an adequate instrument for mobilizing the nation's scientific strength.[14]

This, then, was the situation when Sachs talked with the President. Roosevelt's thinking must have been conditioned by the rather uneasy relations that had existed between the Administration and the scientific community. There was little basis for sentiments of mutual confidence. No adequate machinery was at hand. One alternative was to refer the matter to the National Academy of Sciences, but this was an unwieldy expedient, and there was little reason to believe it would be fruitful. Besides, every instinct would lead the President to conclude that security as well as policy dictated caution. Why not restrict consideration, for the present at least, to official circles? Whatever the reasoning, action came quickly. Roosevelt appointed an Advisory Committee on Uranium to investigate the problem in co-operation with Sachs. Its chairman was Lyman J. Briggs, a government scientist who had begun his career in 1896 as a soil physicist in the Department of Agriculture and was now director of the National Bureau of Standards. Other members were Commander Hoover and Colonel Adamson. This was a rational solution. Sachs later claimed he had suggested placing the Bureau of Standards in charge as a means of achieving a fresh view, a view uncomplicated by military prejudices. This may have been the case, but there was a more obvious explanation for appointing Briggs. This, after all, was a problem in physics. Why not have it investigated by the Government's physics laboratory?

Briggs called a meeting at the Bureau of Standards for October 21, 1939. Joining the committee members and Sachs were two Washington physicists—Fred L. Mohler of the Bureau of Standards and Richard B. Roberts of the Carnegie Institution—and three physicists of Hungarian origin—Szilard, Wigner, and Edward Teller. The latter three were invited at Sachs's initiative. Sachs also had arranged for Einstein to be invited but the shy genius did not accept. Szilard focused the discussion by pointing out that it seemed quite possible to attain a chain reaction in a system composed of uranium oxide or metal and carbon in the form of graphite. The principal uncertainty was the lack of information on the absorption of slow neutrons by the graphite moderator. Szilard and Fermi had devised experiments for measuring this. If the absorption cross section should be either small or large, they would know at once whether the chain reaction would or would not work. If they obtained an intermediate value, they would have to conduct a large-scale experiment. Some of those present were openly skeptical about the chance for a chain reaction, but the three Hungarians were optimistic. In a sequence that bordered on comedy, the meeting drifted into a discussion of government financing, which was not the immediate objective. As Szilard recalled it, Teller referred quite incidentally to the amount of money that researchers could spend profitably in the months ahead. Colonel Adamson made this the occasion for a discourse on the nature of war. It usually took two wars, he said, to develop a new weapon, and it was morale, not new arms, that brought victory. These sentiments moved Wigner, who had been fidgeting in his chair, to venture the opinion that if armaments were so comparatively unimportant, perhaps the Army's budget ought to be cut by 30 per cent. "All right, you'll get your money," Adamson snapped.[15]

The Advisory Committee on Uranium reported to the President on November 1 that the chain reaction was a possibility, but that it was still unproved. If it could be achieved and controlled, it might supply power for submarines. If the reaction should be explosive, "it would provide a possible source of bombs with a destructiveness vastly greater than anything now known." The committee believed that despite the uncertainties, the Government should support a thorough investigation. It urged the purchase of four tons of pure graphite at once and the acquisition of fifty tons of uranium oxide in the event that the preliminary investigations justified continuing the program. To provide for the support and co-ordination of these investigations in different universities, Briggs and his colleagues advocated enlarging their committee to include Karl Compton, Einstein, Pegram, and Sachs.

On November 17, Watson wrote Briggs that the President had noted the report with deepest interest and wished to keep it on file for reference.

The President also wanted to be sure that the Army and Navy had copies. There was no further word from the White House until February 8, 1940, when Watson told Briggs he intended to bring the report to the President's attention again. Was there anything Briggs could add as a personal recommendation? Briggs replied on February 20 that the Army and Navy had transferred funds "to purchase materials for carrying out a crucial experiment on a satisfactory scale." He hoped for a report in a few weeks. It would show "whether or not the undertaking has a practical application." These brief sentences referred to $6,000 that the military services had granted for the purchase of supplies for experiments with the absorption qualities of graphite. By the time Briggs answered Watson, both the President and his aide had departed on a trip that would keep them away from Washington until about the first of March.[16]

The little group that had sought to interest the President the preceding autumn was dissatisfied. Early in February, Sachs obtained a copy of the November 1 report from General Watson. Now he could see what was wrong, he wrote Watson: the paper had been too academic in tone to make its practical point. Sachs asserted that Einstein thought the situation looked even better than earlier. Sometime during the coming month, the economist announced, he would submit a new appraisal.[17]

Meanwhile, Joliot-Curie reported his measurements of a uranium-and-water system. The Frenchman's encouraging results stimulated Szilard to greater confidence in his own uranium-graphite approach. Rumors that the Nazis had secretly intensified their uranium research made action seem especially urgent. Again Szilard saw Einstein. Resorting to pressure tactics in the hope of forcing Government action, he showed Einstein a manuscript on a graphite system that he was sending to the *Physical Review* for publication. Einstein reported the new developments to Sachs. On March 15, Sachs relayed the communication to the White House. In view of the brighter experimental outlook, he asked, would the President be able to confer on the practical issues it raised?[18]

The first response was disappointing. Watson replied on March 27 that he had delayed until he could speak with Colonel Adamson and Commander Hoover. They had come in that afternoon, and Adamson had said that everything depended on the Columbia graphite experiments. Under these circumstances, Watson thought "the matter should rest in abeyance until we get the official report." Within a week, however, there was encouraging news from the White House. On April 5, the President thanked Sachs for forwarding the Einstein letter. He had asked General Watson, he said, to arrange another meeting in Washington at a time convenient for Sachs and Einstein. Roosevelt thought Briggs should attend as well as special representatives from the Army and Navy. This was the most practical method of continuing the research. ". . . I shall always be interested to hear the results," he said.

The same day, Watson sent Briggs a copy of the letter to Sachs and asked for suggestions "so that this investigation shall go on, as is the wish of the President." [19]

March, 1940, had brought a new interest in uranium. The development that touched it off was the conclusive demonstration that uranium 235 was the isotope that fissioned with slow neutrons. While Fermi had been investigating the chain reaction in natural uranium, Dunning had organized his attempt to determine the fissionable isotope. He had persuaded Alfred O. C. Nier of the University of Minnesota, the country's foremost expert on the mass spectrometer, to prepare small samples of partially separated U-235. Dunning and his co-workers at Columbia, Eugene T. Booth and Aristid V. Grosse, made the necessary measurements. In the March 15 and April 15 issues of the *Physical Review,* they presented definite confirmation of what so many had suspected was the role of the lighter isotope.

This was an event of profound significance. If uranium 235 could be concentrated, there seemed no question that a slow-neutron chain reaction was possible. This meant power. A bomb, however, remained highly doubtful. Some physicists already saw that a bomb depended on fission by fast neutrons. If they had to rely on slow neutrons, the metal would tend to blow itself apart before the reaction had gone far enough. It was questionable if the resulting explosion would have sufficient magnitude to justify its cost. The Dunning-Nier experiments indicated that uranium 238 would undergo fission under fast-neutron bombardment, but it did not seem likely that the heavier isotope would sustain a chain reaction. The cross section or probability of fission was too small. What about U-235? Might not it be susceptible to fission by fast as well as slow neutrons? Some physicists thought it was probable. If this were the case, there was a good chance of an explosive reaction in a highly concentrated mass of the lighter isotope. Still, it was only theory. All that was known definitely was that fast neutrons had a lower probability of causing fission in U-235 than slow ones. In the absence of samples substantially enriched in 235, physicists could not determine its fast-fission cross section experimentally.[20]

Whatever might be the possibility of an explosive, the first task was to prove the chain reaction. On March 11, Pegram sent Briggs advance word on the role of U-235. On April 9, Briggs reported to Watson that it was "very doubtful whether a chain reaction can be established without separating 235 from the rest of the uranium." He recommended an intensive study of methods of isotope separation. By this time, interest in uranium 235 had spread widely. It found a focus at the meeting of the American Physical Society in Washington the last week in April. There Gunn, Beams, Nier, Fermi, Harold C. Urey of Columbia, and Merle A. Tuve of the Carnegie Institution discussed its significance for the chain reaction. The next step, they agreed, was to separate U-235 in kilogram quantities. Of the various possible methods, the centrifuge alone seemed to offer much hope. They decided to try to acquire the funds necessary to determine its potential.[21]

On Saturday afternoon, April 27, the Advisory Committee on Uranium met at the National Bureau of Standards. Joining Briggs, Adamson, and Hoover were Admiral Bowen, Sachs, and four university physicists—Pegram, Fermi, Szilard, and Wigner. Einstein again had declined to attend. Of the scientists, Szilard was the most optimistic concerning the chain reaction, though he could say nothing very explicit about the prospect for an explosive. Sachs urged prosecuting the work more vigorously. If the Government was not disposed to undertake it, he favored trying to finance it from private sources. Sachs was impatient with Fermi's conservative position. If the United States would plunge ahead, he thought, the difficulties experienced in the laboratory would tend to disappear. The Advisory Committee agreed on the need for investigation, but it was ready to proceed on only a small scale and a step or two at a time. As Briggs reported to Watson on May 9, the committee did not care to recommend a large-scale try for a chain reaction until it knew the results of the graphite measurements at Columbia. These were expected in a week or two. If the large-scale experiment was undertaken, the Army and Navy should supervise it at one of the proving grounds. As for methods of separating isotopes, the committee favored supporting the investigations of scientists in various universities but did not favor attempting such studies on a secret basis.[22]

Briggs made some progress in May. He spent the first day of the month at Columbia. On the sixth, Pegram reported the consensus of a conference with his colleagues Fermi, Urey, and Dunning. If support could be obtained from the Navy or elsewhere, they favored tests on a laboratory scale to determine which method appeared best for concentrating substantial amounts of U-235. They proposed to enlist the principal isotope-separation specialists and launch the work in June, when the academicians among them could escape their teaching duties. On May 8, Pegram explained to Briggs what was involved in proving the chain reaction in a uranium-graphite system. On May 14, Pegram announced that Fermi and Szilard had found the absorption cross section of graphite encouragingly small.[23]

As the outlines of a sensible program emerged, pressure for action intensified. Sachs had no intention of leaving everything to Briggs. He argued the cause in May letters to Roosevelt and Watson. Now that Fermi and Szilard had determined the characteristics of graphite, it was time to move. The Nazis were overrunning Belgium; something should be done to safeguard the uranium ore of the Congo. The research program should have larger financial support as well as a better and more flexible organization. Perhaps a nonprofit corporation with official status under the President could make the arrangements necessary to further the work.[24]

More important were the repercussions of the talks at the American

Physical Society meeting. Gunn at once recommended to Admiral Bowen that the Naval Research Laboratory foster a co-operative research effort. Apprised a few days later of the Columbia proposals on isotope separation, Bowen asked Urey to organize an advisory committee of scientific experts to counsel the President's Committee on Uranium. Urey conferred with Briggs and soon had a list of physicists and chemists he thought would be helpful.

The stirrings at the Naval Research Laboratory were echoed a few miles to the north at a private center for scientific research, the Carnegie Institution. Tuve prepared notes for the information of his chief, Vannevar Bush. Though Tuve thought submarine propulsion appeared more practical at the moment than a bomb, he judged that the interests of national defense justified trying to develop the centrifugal system of separation. His recommendations led Bush to call a conference for May 21. The discussion convinced him that the centrifuge deserved support. Bush telephoned Briggs that he would wait to see what funds the Government furnished. If there should be a gap, the Carnegie Institution might step in.[25]

Briggs was pleased at Bush's assurances that his only purpose in calling the conference was to determine how the Carnegie Institution might be helpful. This kept the way clear for the scientific subcommittee. Briggs and Urey soon settled on a membership consisting of Urey himself, Pegram, Tuve, Beams, Gunn, and Gregory Breit, a professor of physics at the University of Wisconsin. This group reviewed the whole subject at the Bureau of Standards on June 13 and advocated support for investigations of both isotope separation and the chain reaction.[26]

ENTER THE NDRC

A new force now appeared on the scene—the National Defense Research Committee. An effort to organize American science for war, it owed its existence to Vannevar Bush. A shrewd, spry Yankee of fifty—plainspoken, but with a disarming twinkle in his eye and a boyish grin—Bush was well known for his original work in applied mathematics and electrical engineering. During the first World War he had worked for the Navy on submarine-detection devices. Though he then turned to teaching, his talent for invention did not atrophy. From his fertile brain came many ingenious innovations, including an essential circuit for the automatic dial telephone. In 1939, he resigned the vice-presidency of the Massachusetts Institute of Technology to become president of the Carnegie Institution, a post that put him close to the nerve center of the embryonic defense effort. Soon he moved up from member to chairman of the National Advisory Committee for Aeronautics, and when war broke out in Europe, he cast about for some way of organizing American science for the test that lay ahead. After discussions with Karl Compton, with President James B. Conant of Harvard, with President Frank B. Jewett of the National Academy of Sciences, and with his colleagues at the NACA, he evolved a plan for a committee that would have the same relation to the development of the devices of warfare that the NACA had to the problems of flight.

Early in June, 1940, when Nazi Panzer divisions were thrusting deep into France, Bush persuaded President Roosevelt to place him at the head of a National Defense Research Committee. Under the authority of the old World War I Council of National Defense, from which it was to draw its funds, the NDRC was to supplement the work of the service laboratories by extending the research base and enlisting the aid of scientists. Even more important, it was to search for new opportunities to apply science to the needs of war. It could call on the National Academy and the National Research Council for advice and on the National Bureau of Standards and other government laboratories for more tangible assistance. The NACA, already functioning well under Bush's leadership, lay outside the jurisdiction of the new agency. Not so the Committee on Uranium. It was to report directly to Bush, and the NDRC was free to support its work.[27] The NDRC did not owe its birth to uranium, but the pressure applied by those who had caught the vision of a chain reaction made Bush's organizational plan seem all the more attractive.

The new committee was an important factor in mobilizing the scientific resources of the nation. The NDRC did not have to wait for a request from the Army or Navy but could judge what was needed for itself. It was not limited to advising the services but could undertake research on its own. For the uranium program, its creation was an event of great significance. It freed uranium from exclusive dependence on the military for funds. More important, it rescued this novel field of research from the jurisdiction of an informal, *ad hoc* committee. By providing a place within the organizational framework of the defense effort of American science, the NDRC made it easier for nuclear scientists to advance their claims.

By the early autumn of 1940, Bush had reorganized the Committee on Uranium and adjusted it to its new place in the scheme of things. Guided by instructions from the President, he retained Briggs as chairman but dropped Commander Hoover and Colonel Adamson because the NDRC was now the proper channel for liaison with the military. To strengthen the scientific resources of the group, he added Tuve, Pegram, Beams, Gunn, and Urey. The new regime stressed security. One manifestation was the exclusion of any foreign-born scientists from committee membership, a policy adopted in deference to Army and Navy views and with at least one eye on future encounters with Congress. The other manifestation—arrangements for blocking the publication of reports on uranium research—originated with the scientists themselves. Szilard had sought in vain to accomplish this on an international scale back in February, 1939. In the spring of 1940, Breit sparked the establishment within the framework of the National Research Council of a reference committee to control publication of any research that had military significance. Uranium fell within its scope; indeed, the desire to control publication on fission phenomena prompted the ban.[28]

FORMULATING A PROGRAM

Though the NDRC would control the funds, it remained the duty of the Briggs committee to formulate a program. One of its concerns was uranium ore. There were no significant stockpiles in the United States, for the only commercial use of uranium was as a coloring agent in the ceramic industry. Of the 168 tons of oxides and salts American users consumed in 1938, only 26 came from domestic carnotite ores mined in the Colorado Plateau. The remainder was imported: 106 tons from the Belgian Congo and 36 from Canada. Early in June, 1940, Sachs urged Briggs to have someone make an overture to the Union Minière du Haut Katanga, the company that owned the Congo mines. He thought Union Minière might be persuaded to ship ore to the United States and, while retaining title, commit itself not to re-export without special permission. Briggs promptly authorized Sachs to make the necessary inquiries. The company showed no immediate interest in such a scheme, though later in 1940 its affiliate, African Metals Corporation, imported 1,200 tons of 65-per-cent ore and stored it in a Staten Island warehouse.[29]

Research, not raw materials, seemed the proper emphasis in June, 1940. Ore would become important when and if production was warranted, but with funds limited and with so little known about the defense potential of uranium, the Briggs committee did not deem it prudent to acquire large stocks of raw materials. There would be time enough when research had indicated the extent of the requirements.

The Committee on Uranium addressed itself to research on June 28. It accepted the findings of its scientific counselors that ample justification existed for supporting work on isotope-separation methods and for further efforts to determine the feasibility of a chain reaction in normal uranium. On July 1, Briggs gave Bush a report on his stewardship. He announced with gratification that the War and Navy Departments had approved a thorough study of separation. An allotment of $100,000 had already been made, which the Naval Research Laboratory would administer with the advice and assistance of the Committee on Uranium. That still left the chain reaction to be provided for. Briggs urged that the NDRC set aside $140,000 for two types of investigation: first, studies to determine more accurately the fundamental physical constants and, second, an intermediate experiment involving about one-fifth the amount of material judged necessary to establish the chain reaction.[30]

The NDRC approved the uranium recommendations in principle on

July 2 and asked Briggs to place them in definite form for consideration when funds became available. Briggs arranged for full presentations by Pegram and Fermi, and on September 6, Bush told him that the NDRC had agreed to assign $40,000. This was enough to finance the work on physical constants but not enough to undertake the intermediate experiment.[31]

RESEARCH: THE CHAIN REACTION

The chain reaction in natural uranium still had high priority despite the demonstration that it was only the lighter isotope 235 that contributed to slow-neutron fission. Many still thought that the expense made the isotope-separation approach impractical. To them it seemed essential to strive for a definite answer on unseparated uranium. If such a chain reaction did prove possible, to what use should it be put? In the summer of 1940, American scientists saw it first as a source of power. All of them, certainly, had thought of the possibility of a bomb. Some believed that in achieving a chain reaction they might gain understanding of what it took to make a bomb. But scientists in America did not direct their thinking primarily toward a weapon. When Pegram and Fermi outlined the research plans for the Columbia team in August, they listed their objectives only as power and large amounts of neutrons for making artificial radioactive substances and for biological and therapeutic applications.

More than a year of research had left the prospects for a chain reaction uncertain. The problem remained the same: to discover if enough of the neutrons produced by fission survived to keep the reaction going indefinitely. When one neutron produced fission, at least one of the neutrons emitted had to live to repeat the process. If this reproduction factor, which physicists were beginning to express by the symbol k, was one or better, the chain reaction was a fact. If it was even slightly less than one, the reaction could not maintain itself. In a uranium-graphite system there were three obstacles to a satisfactory reproduction factor. One was nonfission capture of neutrons by uranium. Another was their absorption by impurities such as might exist in the moderator. A third was escape from the surface. The larger the system, the less serious was the danger that the vital particles would be lost. This was so because the volume of the mass, where neutrons produced fission, increased more rapidly than its surface, where they escaped.

Fermi and his group at Columbia did not wait until the NDRC contract came through on November 1, 1940. First, they checked their work of the preceding spring on the neutron-absorbing characteristics of graphite. Their technique was to introduce a few grams of radon mixed with beryllium as a neutron source into a square column, or pile, of graphite a few feet thick. As the neutrons diffused through the column, they induced radioactivity in sensitive strips of rhodium foil that had been inserted as detectors.

This was work that Fermi especially enjoyed. Since the radioactivity in the rhodium was short-lived, the foil had to be placed under a Geiger counter within twenty seconds. Fermi would race down the hall to his office, where the counter had been placed to keep it from being disturbed by the neutron source in the laboratory, put the foil in place, and then delightedly tap his fingers in time with the clicking of the register. The measurements confirmed not only the suitability of graphite as a moderator but also led to a mathematical method for developing the life history of a neutron.

The second step for the Fermi team was to determine the average number of neutrons emitted by natural uranium when it absorbed a slow neutron. This was a value bound to be smaller than the number of neutrons emitted per fission, since not every absorption by a uranium atom produced fission. The experimenters rebuilt the graphite column to permit the insertion of a layer of uranium in a region where practically all of the neutrons had been slowed. Now it was easy to distinguish neutrons emitted by the uranium from those originated by the source. The value Fermi derived, 1.73, was so low that, although it did not rule out a chain reaction, it emphasized the necessity of keeping parasitic losses to a minimum. During the course of these experiments, Szilard brought forward the idea that if the uranium were arranged in lumps instead of being spread uniformly throughout the graphite, a neutron was less likely to encounter a uranium atom during the process of deceleration, when it was particularly susceptible to nonfission absorption. With heavy reinforcement from a new research group at Princeton, the investigators turned to explore the possibilities of Szilard's suggestion. By the spring of 1941, they had accomplished enough to gain a good understanding of the processes involved and of the arrangements most likely to minimize the unfavorable factors.[32]

While the basic work of measurement was proceeding, the physicists made plans to find out how large a pile with a given arrangement, or lattice, of uranium lumps should be in order to maintain a chain reaction. One way would have been to begin building a full-scale pile. When it started to react, they would know the necessary dimensions. If it should become impractically large without going critical, they could conclude that something was fundamentally wrong. But they had already rejected this crude and expensive technique, for it would delay reliable judgments until large quantities of materials had been amassed. A better method was to construct an intermediate-sized, or exponential, pile. This would make possible an informed, though not conclusive, opinion much earlier and at much less cost.[33]

It proved difficult to acquire suitable materials even in small quantities. Despite the co-operation of the Bureau of Standards, of Metal Hydrides, a producer of powdered metal alloys, and of the research laboratories of the Westinghouse and General Electric companies, the Briggs committee could find no dependable method of manufacturing either nonpyrophoric uranium powder or pure ingots. This disappointment forced the Columbia experimenters to turn to uranium oxide, even though the chances of success with this were less. Nor did it prove easy to acquire graphite low in boron, an absolute essential because of the strong neutron-absorbing characteristics of boron. By May, 1941, Briggs had placed orders for forty tons of graphite with the United States Graphite Company and for eight tons of uranium oxide with Eldorado Gold Mines, Ltd., of Canada. Not until these orders had been filled would it be possible to proceed with the intermediate experiment.[34]

The Fermi work at Columbia aimed at a uranium and graphite pile, but the Briggs committee considered other moderators as well. In November, 1940, Nobel Prize winner Arthur H. Compton, brother of Karl and chairman of the Department of Physics at the University of Chicago, suggested a beryllium moderator. Beryllium had not only the essential low atomic weight, he argued, but also the advantage that it would add rather than remove neutrons and thus contribute to a successful chain reaction. Two months later the NDRC let a contract for Samuel K. Allison to make the necessary measurements at Chicago. Meanwhile, it had not been forgotten that heavy water might be useful, both as a moderator and as an agent for removing the heat generated in a uranium-graphite pile. Early in 1941, Urey, the discoverer of heavy hydrogen, began to press for action. Urey, also the winner of a Nobel Prize, was interested in the experiments of Hans von Halban and Lew Kowarski. These co-workers of Joliot-Curie had fled to England at the fall of France with a few bottles of heavy water which constituted practically the world supply. Their studies now seemed to indicate a good chance of obtaining a chain reaction in a heavy-water and uranium-oxide system. Perhaps, Urey worried, the Germans were already ahead in this approach. Americans should study methods for producing large quantities of heavy water. By June, he had won Briggs's support and had done enough work himself to be able to submit a comprehensive report on the subject.[35]

RESEARCH: ISOTOPE SEPARATION

The big change in the uranium program after June, 1940, was the emphasis on isotope separation. The proof of U-235 fission by slow neutrons had dictated this second approach to the chain reaction. The scientists interested in isotope separation recognized the possibility of a bomb, but most of them, like the men working on normal uranium, were thinking mainly of a source of power.

Isotope separation appeared incredibly difficult. An isotope differed from its sister substances in mass—that is, in the number of neutrons in its nucleus—but not in atomic number. For most practical purposes, therefore, separation depended not on chemical methods but on some process involving a difference in mass. The task was especially troublesome in uranium, for the weight differential was slight, and uranium 235 was present in natural uranium in the ratio of only one part to one-hundred forty.[36]

To most scientists, the high-speed centrifuge seemed the best bet. The principle was as simple as that of the cream separator. Since the centrifugal forces in a cylinder spinning rapidly on its vertical axis acted more strongly on heavy molecules than on light ones, it was possible to concentrate them in the peripheral areas. If the principle was applied to the separation of a gaseous mixture of two isotopes, concentrations of the lighter isotope could be drawn off at the center and top of the cylinder. A high degree of separation could be attained by running these concentrations through a series, or cascade, of many such centrifuges. In 1919, Lindemann and Aston in England had suggested the centrifuge for isotope separation, but the early attempts had failed, for no one had been able to spin a tube rapidly enough. By 1939, however, Beams had developed at the University of Virginia a high-speed unit with which he achieved significant separation of chlorine isotopes. Beams had found some real difficulties. It was easier to spin a short tube than a long one, though long tubes were more efficient. Speed was limited by the strength of the rotating tube. Particularly troublesome were the vibrations encountered at certain critical velocities. It was necessary to accelerate rapidly through these zones before the machine shook itself to pieces. But no one thought these difficulties too serious. Purely mechanical, they would yield quickly to a concerted research effort.

Appropriately, Beams received an important share of the funds the Navy supplied for fiscal year 1941. He spun tubes of various sizes and tested methods of applying the principle. First, he worked with compounds of chlorine and bromine and with mixtures of gases. When uranium hexafluoride, the only gaseous compound of uranium, became available, he achieved concentrations of uranium 235. The yield, however, was not as high as theory had indicated. Another wing of the effort was located at Columbia under Urey, who had a long-standing interest in isotope separation. Its mission was to develop a centrifuge suitable for industrial operations, but Urey quickly concluded it was unwise to attempt specifications without additional exploration. Accordingly, he turned to Karl Cohen, an able young mathematician, and set him to work on theoretical calculations. By early 1941, Cohen had established a body of theory that made it possible to design a unit of encouragingly short length which the Westinghouse Electric and Manufacturing Company undertook to construct. Meanwhile, other workers at Columbia developed meters for determining the rate of gas flow. By May, the apparatus was about ready for the first experimental runs.[37]

Another possible method of separating the uranium isotopes was gaseous diffusion. A gas would diffuse through a porous barrier if there was high pressure on one side and low on the other. Since 1829, it had been known that the rate of diffusion was inversely proportional to molecular

weight. It followed that if a gas was a mixture of two isotopes, the molecules of the lighter would pass through the barrier more rapidly and be present, for a while at least, in concentration on the low-pressure side. If this process could be repeated many times, a very high concentration could be achieved. Aston had used the principle in 1931 to effect partial separation of neon isotopes. Later, Harkins, a University of Chicago chemist, applied the method to chlorine, while the German physicist, Hertz, achieved almost complete separation of neon isotopes by recycling the gas through many stages.

During lunch at the Carnegie Institution conference on May 21, 1940, George B. Kistiakowsky, a professor of chemistry, suggested gaseous diffusion as a possible means of separating the uranium isotopes. At the time, he was thinking of a diffusion apparatus that Charles G. Maier of the U. S. Bureau of Mines had developed for separating mixed gases. The conference believed that Kistiakowsky should be encouraged to investigate this and other diffusion methods. During the days that followed, Kistiakowsky concluded that the Maier system had certain grave defects, but he found the simplicity of the diffusion principle so appealing that he decided to investigate the old Hertz method. Early in July, Urey suggested the possibility of making barriers of a special glass the Corning Glass Company had developed. Having independently thought of the same possibility, Kistiakowsky initiated efforts to procure samples. On October 14, he reported findings from his research on glass barriers that he judged extremely encouraging.[38]

By this time, others at Columbia besides Urey had become interested in Hertzian diffusion—the physicists Dunning and Booth, the chemist Grosse, and a professor of mechanical engineering, Karelitz. Pressed by other work, Kistiakowsky bowed out, and in the winter and spring of 1941, Morningside Heights became the center of research on gaseous diffusion. Reinforced by Francis G. Slack from Vanderbilt, Columbia investigated a number of potential barrier materials with favorable results and moved on to more comprehensive studies.[39]

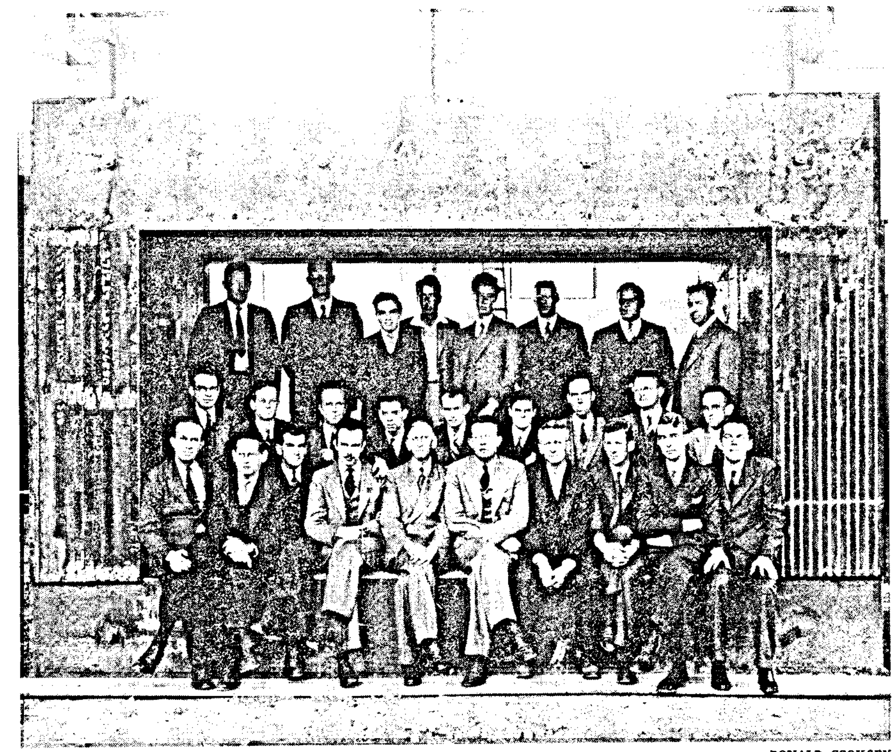
The advocates of gaseous diffusion recognized that they faced formidable obstacles. The barrier—filter might have been a better name—would need billions of holes with a diameter less than one-tenth the mean free path of a molecule, about one ten-thousandth of a millimeter. A material so delicate at the same time had to be strong enough to withstand a considerable pressure differential and the mechanical strains of assembly. Like the centrifuge, a gaseous-diffusion system would have to process the devilishly corrosive uranium hexafluoride. This meant it would be difficult to prevent deterioration of the equipment, contamination of the gas, and plugging of the barriers. No leakage of air into the system could be tolerated, for the water vapor would react with the gas to form uranium oxyfluoride, which surely would clog the barriers and halt the operation. Nevertheless, the gaseous-diffusion method seemed fundamentally sound. Though a plant producing one kilogram of U-235 a day would require several acres of barrier area and thousands of stages, the process would be continuous, not batch. It offered less likelihood of mechanical difficulty than did the centrifuge.

Of several other separation methods that scientists considered in the spring and summer of 1940, liquid thermal diffusion was the most significant. The first thermal-diffusion process to attract attention used gases, not liquids. It was based on the tendency of the molecules of a mixed gas, when confined in a container having a marked temperature gradient, to concentrate in either the hot or the cold region. Experiments at Columbia and the University of Minnesota quickly established that this process was impractical for large-scale separation. But Philip Abelson, now working at the Carnegie Institution, thought of trying liquid rather than gaseous thermal diffusion. On the recommendation of Briggs, who had discussed the matter with Bush, the Naval Research Laboratory furnished funds to finance the research. Abelson did the actual experimentation at the Bureau of Standards, where facilities were better. The Navy, which hoped to concentrate uranium 235 in order to develop a nuclear power plant small enough for submarines, became enthusiastic about the prospects of the method. At the suggestion of Gunn, Abelson transferred in the summer of 1941 to the Naval Research Laboratory, where higher steam pressures and superior shops were available.[40]

The weakest part of the Uranium Committee's research program was the study of fission by fast neutrons. Workers at the Carnegie Institution reported measurements of the susceptibility of natural uranium to fission by fast neutrons in July, 1940. When theoretical physicist Edward Teller saw them, he thought they indicated a possibility for fast-neutron explosions. But on the basis of the most likely assumptions, he estimated that a uranium sphere of more than thirty tons would be necessary for an explosion. This was not very encouraging.[41] About the same time, Briggs brought Gregory Breit, now working at the Naval Ordnance Laboratory, into the picture. Breit served informally at first, then as chairman of a subcommittee to co-ordinate theoretical investigations. These included the slow-neutron chain reaction. Fast-fission studies received very little attention and were clothed with the deepest secrecy. They failed to have any direct impact on the course of the American uranium program.

DISCONTENT AND REVIEW

The year 1941 opened on a note of strident controversy. Americans had thrilled at the desperate air battles fought the summer before in English skies. They had rejoiced in the victories of Royal Air Force fighter squadrons. Yet the defeat of the *Luftwaffe* had eased the tension only briefly. By the end of the year, British credit was approaching exhaustion. The U-boats tightened their grip. To keep Britain in the war required heroic measures.



DONALD COOKSEY

UNIVERSITY OF CALIFORNIA PHYSICISTS, 1938 / Staff of the Radiation Laboratory and associated physicists under the yoke of the 60-inch cyclotron magnet.

Left to right

row 1: J. H. Lawrence, R. Serber, P. C. Aebersold, F. N. D. Kurie, R. T. Birge, E. O. Lawrence, D. Cooksey, A. H. Snell, L. W. Alvarez, P. H. Abelson.

row 2: J. G. Backus, A. Langsdorf, J. G. Hamilton, S. J. Simmons, E. M. McMillan, R. R. Wilson, W. M. Brobeck, E. M. Lyman, J. J. Livingood.

row 3: D. H. Sloan, R. Cornog, M. D. Kamen, W. B. Mann, J. R. Oppenheimer, E. S. Viez, D. C. Kalbfell, W. W. Salisbury.

DONALD COOKSEY

SCIENTIFIC LEADERS OF THE S-1 PROJECT / Considering the feasibility of the 184-inch cyclotron project at Berkeley, March 29, 1940.
Left to right
Ernest O. Lawrence, Arthur H. Compton, Vannevar Bush, James B. Conant, Karl T. Compton, Alfred L. Loomis. Karl Compton and Loomis served S-1 only occasionally.

These took shape in January when the Administration introduced the Lend-Lease bill in Congress. Though the measure passed both houses in March by substantial margins, the fight that marked its course was among the most bitter ever waged on Capitol Hill. Had it not been urged as a means of keeping the United States out of war, the majorities might have been much less. With Pearl Harbor nine months away, it was difficult for the American people to face the fact that they probably would have to intervene by force of arms.

During the long weeks of February and March, weeks fraught with anxiety and torn by controversy, a small, yet significant number of American scientists became extremely dissatisfied with the slow pace set by the Briggs committee. They were convinced that the crisis required vigorous, even ruthless, research on uranium.

The central figure in the growing discontent was Ernest O. Lawrence. The inventor of the cyclotron—now the hard-driving director of the Radiation Laboratory at the University of California—was more alert than many of his colleagues to the darkening war situation. A man known for his talent as a promoter, he found himself pressed to do something not only by restive American scientists but also by Ralph H. Fowler, the British scientific liaison officer in Canada. By March, 1941, Lawrence was ready to ask questions, even if it meant going out of channels. But his concern was not strictly a political matter. More than anything else, this many-sided man was a physicist. His involvement stemmed directly from achievements in his laboratory.

The most spectacular was the discovery of elements 93 and 94. When news of uranium fission reached Berkeley, Edwin M. McMillan, a young assistant professor of physics, devised an experiment to ascertain the energies of the main fission fragments. In making his measurements, he noticed the presence of another product—a radioactive substance with a half-life of 2.3 days. Since there was no evidence of energy release, he concluded that it was not a fission fragment. It seemed reasonable to suspect that it was an isotope of element 93, produced by the capture of a neutron by uranium 238 and prompt subsequent decay. In the spring of 1940, McMillan and Abelson (who happened to be west on a vacation) confirmed this interpretation with a positive chemical identification. Element 93, McMillan suggested, should be named neptunium after the next planet beyond Uranus, for which the ninety-second element had been named. The two investigators suspected that neptunium decayed to form an isotope of a ninety-fourth element with a mass of 239. Unfortunately, they did not succeed in proving it, for their work was interrupted by Abelson's return to Washington and McMillan's departure to assist in the radar program at the Massachusetts Institute of Technology. But early in December, 1940, Glenn T. Seaborg, an instructor in chemistry at Berkeley, obtained McMillan's assent to continue the effort to find and identify element 94. A few days later, Seaborg, in collaboration with Joseph W. Kennedy, another instructor, and Arthur C. Wahl, a graduate student, bombarded uranium with deuterons—the nuclei of heavy hydrogen atoms—and produced an isotope of element 93. In this, they observed evidence of 94. Before the end of February, 1941, they established chemically that they had indeed found an isotope of a new element.[42]

The significance of these discoveries, which depended so heavily on the cyclotron and its utility in making samples of significant size, lay in the possibility that element 94 would prove fissionable. Physicists the world over were free to speculate on this possibility, for McMillan and Abelson related their work in the June 15, 1940, issue of the *Physical Review.* Even though they did not report finding 94, their evidence left little doubt that it had been present. It seemed likely that the new element would fission easily. Even before the McMillan and Abelson article appeared, Louis A. Turner of Princeton had predicted both the way it was formed and its characteristics. In fact, as early as the fall of 1939 Bohr and Wheeler had forecast that it would undergo fission with slow neutrons. If 94 should prove fissionable, what would it mean? At first it was viewed as a means of utilizing all the metal in a natural uranium pile. The theory was that neutrons from uranium 235 would convert uranium 238 atoms into 94, which would be subject to slow-neutron fission. Soon, the idea took hold that element 94 could be produced in a pile and then separated chemically. Thus it would be possible to obtain a substance perhaps as fissionable as uranium 235 without the tremendous expense of building isotope-separation plants.[43]

Yet all of this was mere speculation. No policy decisions could come until there was experimental evidence of 94's fission characteristics. During the Christmas season of 1940, a number of scientists saw the best way to proceed: use one of the Radiation Laboratory cyclotrons for manufacturing enough 94 to measure its nuclear properties. Emilio Segrè, who had worked with Fermi in Rome and now was a research assistant at Berkeley, suggested the possibility to Fermi and Pegram during a visit to Columbia. About the same time, the Uranium Committee asked McMillan to undertake the studies. Committed to radar research for the immediate future, McMillan suggested asking Seaborg to do the work and enlisting Lawrence's personal support. Actually, the idea had occurred independently to Seaborg and Kennedy, and they had already planned their experiments. Meanwhile, the British were becoming interested in 94. When Fowler learned that Norman Feather and Egon Bretscher had been working on it at the Cavendish Laboratory in Cambridge, he urged Lawrence to prepare samples and make the necessary measurements. Before this suggestion reached Lawrence, Seaborg's crew, augmented by Segrè, had set to work. They bombarded uranium, this time with neutrons, and found the 94 isotope, one with a mass of 239, that McMillan and Abelson had predicted. By the middle of May, 1941, their tests proved that 94 was subject to slow-neutron fission. Before the month ended, Seaborg reported to Briggs that 94 was about 1.7 times as likely as uranium 235 to fission with slow neutrons.[44]

The cyclotron thus opened an exciting new approach, but early in 1941, Lawrence began to toy with another idea that made action seem all the more important. His new 60-inch cyclotron was operating well; why not convert his 37-inch model into a super mass spectrograph? The mass spectrograph depended on the principle that the lighter particles in a high-speed beam of ions—positively charged particles—were deflected to a greater degree than the heavier ones as they passed through a magnetic field. The device had proved valuable in determining the relative abundance of the isotopes of a substance and in obtaining the extremely small samples of concentrated uranium 235 used to prove that it was the fissionable isotope. The mass spectrograph and the cyclotron had some striking similarities. Both required a high-vacuum chamber and a large electromagnet. Conversion should not prove difficult. Lawrence thought it would open the way to separate larger, purer samples of uranium 235 for study and, eventually, to derive the lighter isotope on a large scale.[45]

On March 17, 1941, Lawrence conferred with Karl Compton and Alfred L. Loomis at Compton's office in Cambridge. Loomis was chief of the National Defense Research Committee's Division 14, the focal point of research in radar, while Compton had special responsibility in the same area as the NDRC member in charge of detection, controls, and instruments. Lawrence had known both men for many years, and in the last few months had done yeoman service in helping them staff the radar organization. This raw New England morning it was nuclear research that concerned him. In his infectious way, he told of his excitement. He was sure there was a reasonable probability of results useful in the present emergency. True, fission was not his NDRC assignment, but his physicist friends had urged him to investigate.

That afternoon Compton telephoned Bush, then wrote him a letter amplifying his analysis of the situation. The British, he thought, seemed farther ahead despite the fact that American nuclear physicists were "the most in number and the best in quality." More disquieting was the probability of active German interest. But most disturbing was the functioning of the Committee on Uranium. It had put to work only a few American physicists, and these were becoming more and more restive. The committee, which seldom met, moved with painful deliberation. The program was so shrouded in secrecy that even those who were participating could not find out what was being done in areas so closely related that they might well influence their own experiments. Harold Urey, himself a member of the Uranium Committee, felt deeply frustrated. Some of the most promising lines of investigation had received so little attention that the chances of developing an application for use in the current emergency were impaired. Part of the trouble was Briggs, who not only had heavy responsibilities in several directions—he was a member of the National Advisory Committee for Aeronautics—but by nature was also slow, conservative, and methodical. It was hard to know what to do, Compton realized, but he had some suggestions. Bush might appoint

Lawrence his deputy for ten days or two weeks with full authority to explore. Lawrence might be able to provide an impetus, just as he had done with radar. Another possibility was to set up a parallel committee to consider possibilities hitherto neglected. Perhaps Bush could make sure of more vigorous administration if he gave Briggs a deputy who would be free to spend practically his whole time organizing the work.[46]

Two days later, Lawrence presented his plea directly to Bush in New York. Bush, whose temper had a low boiling point, did not conceal his irritation. He was in a difficult position. His responsibility covered the whole range of the contributions science might make to national defense. He was anxious to support the uranium program to the extent it seemed likely to have military significance in the near future, but he did not want to have to cope with pressure tactics that might dangerously warp the scientific effort. He believed that Briggs had done well in a situation which required a balanced, reasoned approach. He proposed to back the Uranium Committee in its decisions unless a strong case developed for his personal intervention. Still, Bush was aware that there was considerable justification for some of the criticisms that had been voiced. In a gesture well calculated to help Briggs, to contain Lawrence, and to capitalize on Lawrence's gifts and enthusiasm, Bush arranged for him to become a temporary personal consultant to Briggs.[47] It did not take long for Lawrence to effect some changes. Soon the National Defense Research Committee, on the recommendation of the Uranium Committee, voted funds to support work on elements 93 and 94 at Berkeley and to enable Nier at Minnesota to produce five micrograms of uranium 235 with his mass spectrometer. This last appropriation owed something, at least, to British pressure. Fowler had urged Lawrence to do what he could to get a sample of 235 for the English physicist John D. Cockcroft.

By the middle of April, 1941, Bush had decided to seek a review of the uranium program by a committee of the National Academy of Sciences. After sounding out Frank B. Jewett, its president, he arranged for the National Defense Research Committee to authorize a formal request at its April 18 meeting. His motivation was partly political, for he saw the importance of maintaining good relations with the academy. He had already gone to considerable lengths to see that Jewett was appointed to the NDRC. After all, the NDRC in a sense had usurped the functions of the academy. But more important, Bush felt the need of a dispassionate review by a group of competent physicists not deeply involved in the subject. How much emphasis, he wondered, was justified? A great deal of money could be spent on uranium, but so far as he could see, no one had uncovered any "clear-cut path to defense results of great importance." Jewett selected what appeared to be a strong committee headed by Arthur Compton of the University of Chicago. Other members were Lawrence of California, John C. Slater of the Massachusetts Institute of Technology, John H. Van Vleck of Harvard—all physicists—and William D. Coolidge, a physical chemist who had just retired as

director of the research laboratory of General Electric. Bancroft Gherardi, the retired chief engineer of the American Telephone and Telegraph Company, could not serve because of illness. The task of the committee, as Jewett defined it for Compton, was to evaluate the work already under way and to judge if larger funds and facilities and more pressure would serve the national defense.[48]

Compton's group met with Briggs, Breit, Gunn, Pegram, Tuve, and Urey at the Bureau of Standards on April 30. Briggs reported that the Committee on Uranium had been seeking to determine whether fission could be utilized successfully for explosive bombs, radiation bombs, and submarine power. Theory, he said, indicated that a bomb would require at least a ten-fold concentration of U-235 to be small enough to be carried in an airplane. Accordingly, a number of isotope-separation methods were under consideration. All methods for quantity separation would be difficult and expensive. When current studies revealed the most promising method, Briggs favored building a pilot plant. But Briggs spent most of the time discussing the Columbia work on the chain reaction in normal uranium. Compton came away with the impression that the Committee on Uranium was interested primarily in the generation of power. The National Academy delegation gained no clear understanding of the chances for a bomb. Compton and Breit were not able to get through to each other. A few months later, when an explosive was unquestionably the prime objective, Breit complained that the visiting committeemen exhibited only a polite interest in a bomb, while Compton declared with equal exasperation that he had been able to obtain only the barest outline of a report on the theoretical investigations into its possibility.

On May 5, Compton's committee met at Harvard. Here they discussed in some detail the relative merits of beryllium and graphite moderators and heard a report by Kenneth T. Bainbridge, a Harvard physicist just returned from an NDRC mission to England. The British, he said, took the uranium work very seriously and believed there was some possibility of developing an explosive within two years. In their efforts to establish a chain reaction, they were giving a great deal of thought to a heavy-water moderator. Halban, whose Cambridge investigations in this area they considered auspicious, was anxious to come to the United States to make closer contact with American research. In isotope separation the leading figure was Franz E. Simon of the Clarendon Laboratory at Oxford. He was hard at work on a gaseous-diffusion system and hoped to have a yes-or-no answer regarding it in July.[49]

Compton submitted a unanimous report on May 17 recommending a strongly intensified effort in the next six months. The committee saw military importance as depending on a slow-neutron chain reaction achieved with a heavy hydrogen, beryllium, or carbon moderator. It proposed three military applications. First came radioactive fission products which could be dropped over enemy territory. Since the development of such weapons would require about twelve months after the attainment of a chain reaction, they would not be available before 1943. In second place was an atomic pile which could generate power to propel submarines and other ships. Though this was a straightforward matter theoretically, the engineering difficulties were so great that it could not be important for three years after the first chain reaction. Listed last was the possibility of a bomb of enormous destructive power. It depended on a strong concentration of uranium 235 or of some other element subject to fission by slow neutrons. Viewing the problem optimistically, the committee thought it probably would take three to five years to separate adequate amounts of uranium 235. Possibly element 94, a potential substitute for the lighter uranium isotope, could be produced abundantly in a chain-reacting pile. Bombs made of 94 might be available twelve months after the first chain reaction, but they probably were some years away. All in all, Compton's group did not anticipate atomic explosives before 1945. It considered a chain reaction easy if enough uranium 235 were available, but to acquire this isotope in quantity meant erecting large, expensive plants of designs still undetermined. On the other hand, the prospects of a chain reaction in unseparated uranium were good. Perhaps eighteen months would suffice to achieve it.

What should be done? The National Academy committee saw the chain reaction in natural uranium as the most pressing concern. It recommended full support for the intermediate experiment on a uranium-and-graphite pile and for a pilot plant to produce heavy water. The committee urged emphasizing the beryllium project for the next six months. It favored continuing the isotope-separation studies but judged that they did not require such great stress. From an administrative standpoint, Compton and his colleagues had some improvements to suggest. The Committee on Uranium should be larger. It should pay more attention to the continuous interchange of research information. Only by sharing information and conferring on mutual problems could investigators make much progress. Finally, Halban should be brought over at once. He could help American research along and in turn take back to England information that might prove of value there.[50]

Bush and the National Defense Research Committee found the report hardly the solution to their problems. Its emphasis on power did nothing to allay fears that uranium had little military significance. Eventually atomic power would revolutionize naval warfare, but the NDRC's responsibility was to prepare the United States for possible involvement in the current war, not in some future conflict. The report mentioned bombs, but to discuss bombs in terms of slow neutrons was to admit one had no very clear idea if and how they could be made. And while Compton's review was good in regard to experimental physics, its bold estimates on weapons and time schedules were undocumented. Conant, a chemist and perhaps the most influential NDRC member, was decidedly unimpressed. Bush put it tactfully when he wrote Jewett that the NDRC, troubled at the thought of spending a large amount of

government money on uranium, wanted to know "how far and how quickly results could be put into practical use." He wanted the study reviewed by one or two first-rate engineers.[51] Jewett, himself a member of the NDRC, promptly added Oliver E. Buckley of the Bell Telephone Laboratories and L. Warrington Chubb of Westinghouse to the National Academy committee. Since Compton had left for South America, Coolidge acted as chairman.

On July 11, 1941, the reconstituted committee reported that it had reviewed the earlier recommendations from an engineering standpoint and could endorse them. The discovery that element 94 was subject to slow-neutron fission had strengthened them. The uranium program should have support, not on the basis of definite plans for applications but simply because a self-sustaining reaction was bound to have tremendous import. Nothing more had been learned about separation methods to indicate early success, but because even moderate increases in concentration were important, the effort should continue at its present intensity. Even more than in May, the chain reaction in natural uranium promised success. A demonstration was needed. This would reveal its potential and call forth such an increase in scientific and engineering effort that practical utilization would follow rapidly. The experiments under way sufficed for ascertaining fundamental data, but any practical appraisal demanded investigations on a larger scale. These should start immediately under a project type of organization devoted exclusively to proving the chain reaction and exploiting its possibilities for national defense.[52]

This report was no more helpful than the first. It placed no greater stress on the possibility of a bomb. This was strange, for Lawrence had prepared a "Memorandum Regarding Fission of Element 94," which Coolidge had attached as an appendix. The text mentioned the slow-neutron fission of 94 and referred to the appendix, but it did not point up Lawrence's observation that a fast-neutron chain reaction was likely if large amounts of 94 were available. Bush was disappointed because this second report did not give him the information he had requested. He acknowledged to Jewett that some additional information on physics had been furnished, but he had wanted engineering advice. What was the outlook for military applications? Should he decide to push ahead, what did he have to face in terms of time, of money, of difficulties? All these questions were taking on added significance, for Congress had cut funds. It did not seem likely that enough money would be available to finance a year's work.[53]

While the National Academy was reviewing the program, Briggs was preparing the Uranium Committee's budget recommendations. He made his plans in the light not only of the committee report of May 17, 1941, but of the uranium work in the United Kingdom. In April, 1940, the British had established a committee of scientists under the chairmanship of George P. Thomson of the Imperial College of Science and Technology to examine fission phenomena.

Operating under the code name, the MAUD Committee, this group had launched an effort quite similar to that in the United States except, as might be expected in a country fighting for its very life, it was pointed more directly toward a weapon. In the fall of 1940, the British established liaison with the American committee. Fowler and Cockcroft met with Briggs's group at the Bureau of Standards. Later a number of British papers, including minutes of the MAUD Committee, were sent to the United States, while certain American reports were delivered to Fowler for transmittal to England. Briggs was working on his budget when he received the minutes of a MAUD Technical Committee meeting held April 9, 1941. Two matters discussed were of much interest. First, Rudolf E. Peierls of the University of Birmingham believed that the fission cross section of uranium 235 was large enough to make practical the construction of a bomb of reasonable size. Second, Halban reported it would take a year to transmute one one-thousandth of the uranium 238 in a graphite-uranium pile into transuranic elements. It would be so difficult to extract element 94 in such low concentrations that a graphite pile did not appear at all interesting as a means of producing it.[54]

Briggs submitted his recommendations to the National Defense Research Committee on July 8. The basic objective, he stated, was to ascertain whether a chain reaction was possible. A second was to determine through intermediate piles at Columbia and Chicago and through associated theoretical studies the most promising dimensions, arrangement, and materials to be used in the full-scale experiment on power production. Finally, the aim was to continue work on separating uranium isotopes in quantity. Briggs now urged isotope separation primarily for military purposes. He argued that a ten- or twenty-fold increase in the concentration of uranium 235 was required to produce a chain reaction in a mass small enough to be carried in an airplane.

Briggs proposed a grant of $167,000 for the chain-reaction work. About two-thirds of this was to support studies in progress at Columbia, Princeton, and Chicago, while the remaining third was to finance a contract for the construction of a pilot plant and the development of suitable catalysts for the production of heavy water. The centrifuge project should receive $95,000 and the Columbia gaseous-diffusion experiments $25,000. Nier required $10,000 to analyze the isotope separation attained in the various trials and to improve the mass spectrograph so that better samples might be available for measuring nuclear properties. A number of miscellaneous investigations—on the chemistry of uranium compounds, on proposed separation methods, and on element 94—needed $30,000, and a like amount should be available to cover administrative expenses and theoretical studies.

These budgetary plans showed clearly the influence of British thinking. Investigation of element 94, which the National Academy committee had mentioned so hopefully, was to have only $8,000. Since this was to go to study its production "by bombardment of U-238 and the subsequent contribution of 94 to the chain reaction by fission," Briggs evidently was not impressed by the American suggestion to separate it and use it as a substitute for uranium 235. On the other hand, the separation of uranium 235, which the National Academy reviewers had de-emphasized, was to have almost as much financial support as the chain reaction. Heavy water, it was true, had received support from the Compton group, but this program also owed its existence to British optimism.[55]

Actually, by the time Briggs had drawn up his proposals, the National Defense Research Committee had lost authority to act. The NDRC had been a great step forward, but a year's experience had revealed certain imperfections. Because it was a research organization, it was not well adapted to fill the gap between research and procurement orders that engineers called development. Another disadvantage was that the NDRC ranked equally with the laboratories of the military services and with the National Advisory Committee for Aeronautics. There was no easy way to correlate the research of these three agencies. Moreover, there was a crucial need for stimulating research in military medicine.

The Office of Scientific Research and Development, which Roosevelt established by Executive Order on June 28, 1941, was Bush's effort to remedy these defects at a single stroke. Located within the Office for Emergency Management of the Executive Office of the President, under a director personally responsible to the Chief Executive, the OSRD was to serve as a center for mobilizing the scientific resources of the nation and applying the results of research to national defense. The NDRC would continue, but within the OSRD. Its function was to make recommendations on research and development. The OSRD directorship went to Bush, and Conant replaced him as chairman of the NDRC. The Committee on Uranium became the OSRD Section on Uranium, soon designated cryptically as the S-1 Section.

The uranium program had not been the primary inspiration for these organizational changes. Nonetheless, they had profound significance for its future. Now the work was under the protective arm of the President. Should Bush decide that an all-out effort was in the national interest, he could go directly to the White House for support. The prestige and power of the Presidency would sustain him in dealing with other agencies of the executive arm, particularly the military, on whose co-operation the success of the project would so heavily depend.[56]

TURNING POINT

July, 1941, was the turning point in the American atomic energy effort. Two factors made for a basic change in the attitude of those who bore prime responsibility. One was the better prospect for using element 94. At Berkeley, Seaborg and Segrè measured its fission cross section for fast neutrons on five micrograms obtained by bombarding uranium in a cyclotron. They derived the value, admittedly uncertain, of 3.4 times that of natural uranium. This was encouraging, the more so because Coolidge of the National Academy committee had sent in a report from Fermi which discussed specifically the technical problems involved in a uranium-graphite pile. For the first time, Bush told Conant, he had something like engineering data, and it seemed to be "good stuff." [57]

The other factor, more important in effecting a new approach, was news from Britain. On July 10, Charles C. Lauritsen, an NDRC armor and ordnance specialist just back from London, talked with Bush in his office at the corner of Sixteenth and P Streets. Eight days earlier, he had attended a meeting of the MAUD Technical Committee at which Chairman Thomson presented a preliminary draft report. Unanimously, the committee had recommended pushing a uranium 235 bomb project with all possible speed, the necessary isotope separation to be accomplished by the gaseous-diffusion method. Some days later, Bush and Conant received a copy of the draft report forwarded from the NDRC office in London on July 7. Its basic premise was the conviction that if pure 235 were available in sufficient mass, any neutron produced—not just slow ones—could cause a fission. Since the bulk of the neutrons would be fast, the chain reaction would develop so quickly that an explosion of tremendous force would take place. How much uranium 235 would be needed for an efficient bomb depended mainly on the probability of its fission by neutrons in the energy range of 500,000 to 1,000,000 electron volts. The magnitude of this cross section was uncertain, for when working with natural uranium, one could not be sure that some of the fission produced by neutrons with energies below 1,000,000 electron volts was not due to 238. Measurements made at the Carnegie Institution in the United States, combined with those of Frisch at Liverpool, indicated a minimum critical mass—the smallest size that would maintain a chain reaction—in the neighborhood of five kilograms. Since the nuclear element in a bomb would have to be larger than its critical mass for good efficiency, ten kilograms seemed a reasonable estimate of its size. Clearly, such a weapon could be carried in a number of existing airplanes. Besides, it could be ready in time for use in the present war, say, in two years.

There was a remote possibility, the drafters of the report believed, that element 94 produced in a slow-neutron pile could be extracted and used in a bomb, but they considered it a much surer bet to separate uranium 235. Thermal diffusion was impractical. It was out of the question to suppose that the electromagnetic method would yield even a few grams. Centrifuging was based on principles that long had been understood, but it required precision machinery of a type that so far had been attained only in a laboratory instrument. This left gaseous diffusion by far the most promising method of separating uranium 235 on a large scale. To judge from the report, Simon had gone further in developing a system at Oxford than Dunning and his col-

leagues had at Columbia. All of this indicated that British scientists believed uranium to have military significance worth a major effort. The best proof of their serious intentions was that they raised the question of whether the gaseous-diffusion plant ought to be built in England or in Canada or the United States.[58]

This report gave Bush and Conant what they had been looking for: a promise that there was a reasonable chance for something militarily useful during the war in progress. The British did more than promise; they outlined a concrete program. None of the recommendations Briggs had made and neither of the two National Academy reports had done as much.[59] The scientists at work in the United Kingdom were no more able or advanced than the Americans. Fundamentally, the trouble was that the United States was not yet at war. Too many scientists, like Americans in other walks of life, found it unpleasant to turn their thoughts to weapons of mass destruction. They were aware of the possibilities, surely, but they had not placed them in sharp focus. The senior scientists and engineers who prepared the reports that served as the basis for policy decisions either did not learn the essential facts or did not grasp their significance. The American program came to grief on two reefs—a failure of the physicists interested in uranium to point their research toward war and a failure of communication.

On July 18, 1941, the National Defense Research Committee recommended negotiating contracts to carry into effect the research proposed by the Section on Uranium. This, however, was only an interim measure. The United States was going to have to decide quickly whether it should launch the industrial effort necessary to manufacture the bomb. The immediate problem was to determine the direction and scope of the preliminary investigations and to win support for the program, which would be expensive and which had to be secret, from the only authority who could assure it, the President of the United States.

While Bush and Conant were making their first moves, news of the British intentions reached other American leaders. One channel was Marcus L. E. Oliphant, an Australian physicist working on radar at the University of Birmingham, who made a summer visit to the United States. At Schenectady, he saw William D. Coolidge, the author of the second National Academy report. Coolidge was amazed to learn that the British were predicting that only ten kilograms of pure 235 would be required and that the chain reaction could be effected by fast neutrons. So far as he knew, this information had not been available in the United States when he submitted his report. Oliphant's story, Coolidge told Bush, made a further study of separation by diffusion look more important than the work based on the action of "slowed down neutrons" that the committee had recommended. Oliphant also visited Berkeley. Lawrence was so impressed with what Oliphant told him that he insisted that the Australian prepare a statement summarizing the report of the MAUD Committee. About the middle of September, Lawrence attended the celebration of the fiftieth anniversary of the University of Chicago. One evening he met with Conant, Pegram, and Arthur Compton by the fire at the Compton home, reported on what Oliphant had told him, stressed the importance of element 94 as an alternate route to a weapon, and gave vent to his dissatisfaction with the slow pace in the United States. Conant, who already knew the British plans, put on a show of needing to be convinced. Then he sought to make Lawrence realize that an all-out effort, the only kind that would yield significant results, would take the next several years of his life. Compton listened avidly and went to bed that night seeing clearly the military potential of uranium.

Yet there was an even more important result of Oliphant's visit to Berkeley. It inspired Lawrence to begin plans for converting his 37-inch cyclotron into a giant mass spectrograph. First, he intended to produce samples. A visit to Minneapolis convinced him that Nier did not have the equipment to turn out the needed uranium 235 rapidly enough. But beyond this, Lawrence had his eye on electromagnetic separation on a larger scale. Physicists quite generally believed that what they called the space-charge limitation made this method impractical. They assumed that a large beam of positively charged ions, repelled from each other by their like electrical charges, would scatter hopelessly and disrupt any separating effect. Lawrence, however, suspected from his experience with cyclotrons that the presence of air molecules in the vacuum chamber would have a neutralizing effect. Sensing that he was on the right track, he determined to put his intuition to the test.[60]

Another courier through whom information from Britain reached American scientists was George Thomson himself, chairman of the MAUD Committee. On October 3, he delivered to Conant a copy of his final report. In essentials it was the same as the draft Bush and Conant already had. Thomson had discussed its substance freely with both the National Academy committee and with the Section on Uranium, though he had avoided telling them that the British scientists were exhorting their government to take up uranium in a big way. The Briggs committeemen were impressed with the optimistic British attitude toward the bomb, as Bush, Conant, Compton, and Lawrence had been. They listened intently to Thomson's discussion of gaseous diffusion. It confirmed their plans to send Pegram and Urey to investigate at first hand.[61]

ROOSEVELT MAKES A DECISION

Meanwhile, Bush and Conant were making progress. They took their initial step, strengthening the Section on Uranium, before the first of September, 1941. To the section itself they added Allison, Breit, Edward U. Condon of the Westinghouse Research Laboratory, Lloyd P. Smith of Cornell, and Henry D. Smyth of Princeton. They dropped Gunn in accord with the NDRC

policy not to have Army and Navy personnel as members of the sections, but they expected him to continue to perform a liaison function. Tuve they relieved so that he might devote his entire efforts to another vital war project, the proximity fuze. They retained Briggs as chairman and Pegram as vice-chairman. They established a subsection on power production under Pegram and one on theoretical aspects under Fermi, while they put Urey at the head of groups devoted to isotope separation and heavy water. They assigned panels of consultants to the subsections. Now the uranium work had more representative leadership and broader participation.

Next, Bush asked the National Academy committee to review the situation once more. He had already strengthened the committee by arranging for the appointments of Warren K. Lewis, the dean of American chemical engineers, of Harvard's George B. Kistiakowsky, one of the country's foremost explosives experts, and of Chicago's Robert S. Mulliken, an authority on isotope separation. Bush spelled out for Arthur Compton just what he wanted. Most important was information on a uranium 235 bomb, particularly its critical mass and its destructive effect. He also needed tentative design data on a gaseous-diffusion plant. Less important, he would like a review of available data about a heavy-water pile. He had no desire, he said, to limit the efforts of the reviewing committee, but one thing should be understood. The question placed before it was the technical one. What should or should not be governmental policy was outside its sphere. If the committee could come through with the information Bush had requested, he would have an independent check on the British work. He would have as well the data he needed concerning the scope and direction of an intensified American effort.[62]

Bush did not wait for Compton's committee to report before seeking support at the highest level. Urged on by Conant, whose initial doubts had vanished, he had already decided that the United States had to expand its research and discover what was really involved in building a production plant. Back in July, 1941, he had briefed Henry A. Wallace on the status of American uranium research. He wanted to keep the Vice-President informed, for he was one man sitting in high councils who had the scientific background to grasp the subject readily. Now, the morning of October 9, the same day he sent Compton his instructions, Bush conferred at the White House with Roosevelt and Wallace. He outlined the British conclusions, mentioning the amount of uranium necessary for a bomb, the cost of a production plant, and the time needed to achieve a weapon. He explained that this did not add up to a proved case but depended primarily on calculations backed by some laboratory investigation. Hence, he could not state unequivocally that an attempt would be successful. At some length the three discussed the sources of raw material, the little that was known about the German program, and the problem of postwar control. From this meeting emerged a number of understandings. Most important, Bush was to expedite the work in every possible way. He was not, however, to proceed with any definite steps on an "expanded plan"—on construction as opposed to research and planning—until he had further instructions from the President. When the time came to build, some direction independent of the current investigative framework would have to be devised. It would be best if the necessary construction were done jointly in Canada. Roosevelt, who understood that a great deal of money would be required, said he could make it available from a special source at his disposal. Bush was to prepare a letter that would serve to open discussion with Britain, "at the top." Finally, the President emphasized that Bush should hold the matter closely. He restricted consideration of policy to himself, Wallace, Bush, Conant, Secretary of War Henry L. Stimson, and Army Chief of Staff George C. Marshall. He told Wallace to follow up on any details that required attention.[63]

This White House conference was an event of prime importance on the journey that ended at Hiroshima and Nagasaki. Bush now had the authority, not to make a bomb, but to discover if a bomb could be made and at what price. When this investigation should point the way to a production program, he would need further Presidential sanction. Until then he had virtually a free hand.

Arthur Compton, who knew nothing of the October 9 decision, went to work at once on a third National Academy report. At Columbia he talked with Fermi and Urey. Fermi had estimated that the critical mass of uranium 235 could be as little as twenty kilograms, but thoroughly conservative in temperament, he would not exclude the possibility that it might be as high as one or two tons. These estimates did not give anything very definite to go on, but Urey, as well as Dunning, was optimistic about the chances for separating the isotopes. At Princeton, Wigner was confident that a uranium-graphite pile was a feasible method for producing element 94. And Seaborg of California, to whom Compton talked in Chicago, was confident he could work out large-scale chemical methods for extracting 94 from uranium bombarded in a pile.[64]

To focus the attention of his committeemen, Compton drafted a tentative report for discussion at an October 21 meeting at the General Electric Research Laboratory in Schenectady. This was a useful technique which produced a full review. Lawrence led off by reading Oliphant's summary of the MAUD Committee report. J. Robert Oppenheimer, a young Berkeley physicist who had been invited at Lawrence's insistence, took an active part in the talk on the physics of the bomb. Though he recognized the uncertainties in the case, he thought that about one hundred kilograms was a reasonable estimate of the uranium 235 needed for an effective weapon. Consideration of the methods of separation centered around a comprehensive review of the subject by Robert S. Mulliken, who had visited the various centers of research. From discussion at the meeting, written comments submitted later, and his own calculations, Compton fashioned a draft which, after some changes, became the final document. Lawrence had been concerned by the

tone of the Schenectady meeting. There had been a tendency to emphasize the uncertainties and the possibility that uranium would not be a factor in the war. This attitude was dangerous, he thought. "It will not be a calamity if when we get the answers to the Uranium problem they turn out negative from the military point of view, but if the answers are fantastically positive and we fail to get them first, the results for our country may well be a tragic disaster." His fears, however, were assuaged by the final document. He thought it "an extraordinarily good statement."[65]

The basic conclusion of the report, which Compton submitted to President Jewett of the National Academy on November 6, 1941, was that a "fission bomb of superlatively destructive power" would result from assembling quickly a sufficient mass of uranium 235. "Sufficient" could hardly be less than two kilograms or more than one hundred.[66] These were wide limits, but they stemmed from the prevailing uncertainty about the fission cross section of the lighter isotope, an uncertainty that could not be remedied until larger and better samples were at hand. Speed of assembly was essential. A bomb would be fired by bringing together parts, each less than the critical mass, into a unit that exceeded it. If a stray neutron triggered a reaction before the parts were thoroughly put together, a fizzle, not a powerful explosion, would result. Since uranium had some spontaneous fission, which meant that neutrons would be present, this was a possibility, but it did not seem likely to cause serious difficulty. The bomb would have tremendous destructive force, but just how much was still uncertain. Since only a small part of the energy locked in the uranium could be released before the mass blew apart and the reaction stopped, the available explosive energy per kilogram of uranium would be equivalent to no more than a few hundred tons of TNT. Besides, it was known that fast explosions involving small masses were less effective than slower explosions of the same energy involving larger masses. Following the judgment of Kistiakowsky in this matter, the report estimated that one kilogram would have a destructive effect equivalent in TNT to only about one-tenth of its available explosive energy. Though this was significantly lower than the estimate on which the British were proceeding, Compton, as he indicated in a footnote, thought even this might be too high.

The separation of isotopes could be accomplished in the necessary amounts, the National Academy committee concluded unhesitatingly. There was not enough information available to judge the British version of gaseous diffusion, but the Columbia system looked feasible, even though it might be slow in development. The centrifugal method appeared practical and was further advanced than gaseous diffusion. It was important to study both from an engineering viewpoint. Other methods deserved investigation, but these seemed further from the engineering stage. One possibility was the mass spectrograph. At the moment, it should be used to obtain samples for experimental work. Any estimate of the time needed to develop, engineer, and produce fission bombs could only be rough, but given all possible effort, they might be available in significant quantity in three or four years. The separation process would probably be the most time-consuming and expensive part of the work. This too could be estimated in only the roughest way, but something in the range of $50 million to $100 million seemed reasonable. Other costs in connection with producing the bombs would probably be around $30 million.

How should this analysis be translated into action? Certain work, comparatively modest in scale, had to be done immediately. Trial units of the centrifugal and gaseous-diffusion systems had to be built and tested. Samples of separated uranium 235 had to be obtained for tests on its nuclear properties. While this was being done, engineering of the separation plants should start in order that plans would be ready when more exact information about the requirements was known. So much for the tasks directly ahead. The time had come for some organizational changes. Since isotope separation was at the development stage, a competent engineer should direct it. His efforts, of course, would have to be co-ordinated with the research program. To make the best progress in research, the major tasks should be assigned to key men, men of ability and integrity who had proved themselves. They should have adequate funds to use according to their best judgment.

The report was clear, concise, and as unequivocal as the circumstances permitted. Yet it omitted altogether the possibility of using element 94 as a substitute for uranium 235. Compton had referred to it in the draft he prepared after the Schenectady meeting. Even if a 235 bomb should prove impossible, he had stressed the importance of proceeding with isotope separation and the chain reaction. This would lead to the development of valuable power sources and to the production of 94, "which may itself become a practical means of producing a fission bomb." But that section was dropped from the final report. Just why and at whose initiative was not apparent, but to omit it was good tactics. Bush had emphasized that he was primarily interested in the possibilities for a uranium 235 weapon. He and Conant felt justified in striving only for military results useful in the current war. It was the failure to point out definite prospects for a bomb that had so long delayed an intensive American effort. Nothing had happened to make general references to a source of power or of other interesting by-products impressive now. Element 94, which depended on the chain reaction, seemed to offer hope for a weapon, but in November, 1941, it was hope alone. In September, Fermi at last had made a test of the chain reaction with his intermediate pile, a graphite column eight feet square and eleven feet high, through which lumps of uranium oxide were dispersed in a lattice arrangement. Disappointingly, his measurements gave a value of only 0.87 for k, the reproduction factor. There was promise that by improvements in purity, in geometry, and in density of uranium, k could be raised above 1, but it had not been proved.[67] Until it was, there was no way to make large quantities of 94. Besides, it was not even known that 94 emitted neutrons on fission. Men like Compton and Lawrence might feel in their bones that the 94 program was practical, but their faith would not have seemed very convincing in the cold print of a report.

The National Academy report gave Bush information he needed. Though more conservative than the British recommendations in highlighting uncertainties, it confirmed the conclusion of scientists in England that uranium 235 could be separated and made into an effective bomb. The report made valuable suggestions on how to proceed and how to organize the work. Yet it was not responsible for the decision to go ahead with an intensive investigation. The President had made that decision on October 9. On November 27, Bush sent the report to Roosevelt, pointing out in a covering letter that he was forming an engineering group and accelerating physics research aimed at plant-design data. He also reiterated his understanding that he would await Presidential instructions before committing the United States to any specific program. The report required no action at the White House. Not until January 19, 1942, did Roosevelt send it back with a note in his own handwriting: "V. B. OK—returned—I think you had best keep this in your own safe FDR."[68]

PLANNING THE ATTACK

The main task now confronting Bush was to reorganize the uranium program for a quick decision on a production plant. The basic idea, which had taken shape before the end of November, 1941, was a product of the recommendations of the National Academy committee and of conferences with Compton, Briggs, and Conant. Those with Conant were especially important, for he and Bush had become a smooth team, each respecting and trusting the judgment of the other. In essence, the new organization called for a planning board to make engineering studies, for the designation of scientific personnel to lead the research effort, and for Bush himself as OSRD director to co-ordinate research and engineering.[69]

Working out the details came next. On the recommendation of Warren K. Lewis, Bush recruited Eger V. Murphree, a young chemical engineer now vice-president of the Standard Oil Development Company, to become chief of the Planning Board. Bush assured him that this was a temporary assignment only and instructed him to recommend other persons for the board, stressing that he wanted it staffed by chemical engineers of standing. That Bush should turn to chemical engineers was significant, for in this specialty the United States was strong. Thanks largely to Lewis' teaching at the Massachusetts Institute of Technology and to the demands of the oil industry in the years between the wars, the United States had a corps of engineers well grounded in basic physics and highly skilled in developing and operating large industrial chemistry complexes. Bush continued to keep the Uranium or S-1 Section informed. He had Briggs tell a December 6 meeting that though the committee's concern was science and not broad policy, it could rest assured that the matter would be pushed. Then Briggs outlined the organizational plans that were taking shape.[70]

It was more than just organizational machinery. Information kept rolling in that had to be weighed in determining the direction and scope of the effort. Urey, just back from England, sent Bush on December 1 a preliminary report on the British work. He and Pegram agreed with the British that the Simon method of gaseous diffusion looked most likely to succeed and should have first priority. The Columbia system was not far enough along to evaluate properly. It might become the best method, but no one thought this probable. The second best approach to isotope separation appeared to be the centrifuge. It promised, though, to be about twice as expensive and to require more time after it was built before it could turn out product. The French scientist Halban, Urey reported, was convinced that the best chance for a chain reaction useful for power production lay in employing metallic uranium and heavy water.[71]

Fully as significant as Urey's report was the thinking of Lawrence and Compton on the possibilities of element 94. Early in December, Lawrence sent Compton a letter reporting that measurements by Kennedy and Wahl at Berkeley indicated a spontaneous-fission rate in 94 no higher than in uranium 235. This was heartening, and Lawrence emphasized that 94 might prove the shortest route to a weapon. Compton saw Conant and Bush and made a strong case over lunch at the Cosmos Club for the chain reaction in natural uranium. After convincing them the objective was a bomb and not power, he won their support.[72]

Lawrence had suggested the importance of 94, but his primary concern was an electromagnetic process for separating isotopes. On his own initiative, first with laboratory funds and then with a grant from the Research Corporation, he had diverted some of his best men from the 60-inch and 184-inch cyclotrons and assigned them to converting the older 37-inch unit. Briggs promised financial support and sent Nier out to Berkeley to help. Nier sent back encouraging reports on the enthusiasm at Berkeley. He was convinced that the Radiation Laboratory would be able to achieve its immediate objective, the preparation of U-235 samples.[73]

In the rush of the first week after Pearl Harbor, Bush completed his plans. In letters of December 13 to Briggs, Murphree, Urey, Compton, and Lawrence he spelled out the details. The Planning Board, Murphree at its head, would be responsible for engineering aspects. It would make engineering planning studies and supervise all pilot-plant experimentation or enlarged laboratory-scale investigations. Its job, in short, was to see that the plans were available when the time came to enter the production phase. The active direction of physical and chemical research would be divided among three program chiefs. Urey would be in charge of separation by both the diffusion and the centrifuge methods and of heavy-water investigations. Lawrence would take responsibility for small-sample preparation, electromagnetic separation methods, and "certain experimentation on certain elements of particular interest involving cyclotron work"—less cryptically, element 94. Compton would be concerned primarily "with the fundamental atomic physics and particularly with the measurement of constants and properties." Translated, this meant his spheres were weapon theory and the chain reaction for producing 94. The S-1 Section would continue under Briggs, and all research chiefs, along with Murphree and Conant, would sit with it. The section would meet frequently to interchange information, review the work of the program chiefs, and co-ordinate the scientific attack.

In the new organization Bush assumed a central role. The Planning Board was to recommend contracts to him as director of OSRD. He would take responsibility for integrating the engineering studies with the fundamental research. Bush would place the contracts for scientific research on the basis of recommendations from S-1, though it was not expected that the full committee would have to pass on each one. Normally, these would be channeled through Conant. When quick action was required, any program chief could communicate directly with Briggs and Conant and the three make recommendations directly to Bush.[74] This arrangement made Conant, chairman of the National Defense Research Committee, more important than ever, but it left no place for the committee itself. A few days later Bush explained formally to Conant that the entire uranium program would henceforth lie entirely outside the NDRC. He had received "special instructions" which made this desirable. This was a reference to Roosevelt's insistence that policy considerations be restricted, apart from Conant and Bush, to Wallace, Stimson, and Marshall. To continue the uranium program under NDRC auspices would have extended knowledge of the effort to a degree that the President never was willing to permit.[75]

Three days after Bush's letters were sent to Briggs, Murphree, and the program chiefs, a top-level conference called by Wallace reviewed the whole situation, including the reorganization and the report of the National Academy committee. Joining the Vice-President at the Capitol were Stimson, Bush, and Director of the Budget Harold D. Smith, who was there at Wallace's invitation. General Marshall had been called away on other business, and Conant was down with a cold. The entire group felt that the fundamental physics and engineering planning, particularly the construction of pilot plants, should be expedited. (Later, after the conference, Smith offered assurances that if OSRD resources proved too low, he could supply money from funds the President controlled.) Bush stated his view that when full-scale construction started, the Army should take over. To this proposal no one objected. Bush favored assigning a well-qualified officer to the program at once so that he might become thoroughly familiar with it. Finally. Bush saw that all understood that relations with Great Britain on matters of general policy