In Any Light: Scientists and the Decision to Build the Superbomb, 1952-1954
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In any light: Scientists and the decision to build the Superbomb, 1952–1954

> If the development [of the hydrogen bomb] is possible, it is out of our powers to prevent it. All that we can do is to retard its completion by some years. I believe, on the other hand, that any form of international control may be put on a more stable basis by the knowledge of the full extent of the problem that must be solved and of the dangers of a ruthless international competition.
>
> Edward Teller to Enrico Fermi, 31 Oct 1945

> The fact that no limits exist to the destructiveness of this weapon makes its very existence and the knowledge of its construction a danger to humanity as a whole. It is necessarily an evil thing considered in any light.
>
> Enrico Fermi and Isidor Rabi, Minority GAC Report, 30 Oct 1949

IN THE FEW months from October 1949 to January 1950, the fierce but largely secret debate over the hydrogen bomb (or Super) came to a head. Here was a proposed weapon a thousand times more powerful than the atom bomb that leveled Hiroshima, just as that bomb was a thousand times stronger than the five-ton "Blockbusters" dropped on

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The following abbreviations are used: ACP, Arthur Compton Papers, Washington University, St. Louis, MO; AEC, Atomic Energy Commission; *BAS, Bulletin of the atomic scientists*; BC, Bush-Conant files, Records of OSRD, Record Group 227, National Archives; BP, Bethe Papers, Cornell University; CP, James B. Conant Papers, Harvard University; DOE, Department of Energy; *FRUS, Foreign relations of the United States*; GAC, General Advisory Committee, Atomic Energy Commission; HB, Harrison-Bundy files, Records of the Manhattan Engineer District, MED, RG 77, National Archives; JCAE, Joint Committee on Atomic Energy, U.S. Congress; LAR, Records of the Los Alamos National Laboratory, Los Alamos, NM; LP, Ernest O. Lawrence Papers, Bancroft Library, University of California, Berkeley; MED, Manhattan Engineer District; NA, National Archives; OP, J. Robert Oppenheimer Papers, Library of Congress; OSRD, Office of Scientific Research and Development; PSF, President's Secretary's File, Harry S. Truman Library, Independence, MO; RG, Record Group; SAC, Special Agent in Charge; SP, Lewis Strauss Papers, Herbert Hoover Library, West Branch, IA; SzP, Leo Szilard Papers, University of California, San Diego; UP, Harold Urey Papers, University of California, San Diego.

Europe. The debate reached into important aspects of foreign policy. At stake were military strategy, the balance of power between the Soviet Union and the United States, and the role of scientists in shaping the use of nuclear weapons. Could such a bomb be built? Would it dangerously intrude on the "conventional" atomic arsenal? Was it a moral weapon? Could the country in good conscience build such a device likely to be used against noncombatant populations? Could the country morally *not* construct the new weapon given American perceptions of Stalin's intentions and Soviet physicists' capabilities? Did scientists have an obligation to build it?

Even after President Harry S. Truman's public announcement on January 31, 1950 that America would try to develop the Super, issues in the scientific community persisted about the quest. Many scientists lamented the secrecy blocking public dialogue and the closed decision making. Some refused to work on the project, raising questions about morality and America's defense strategy; others, like Edward Teller, tried to recruit scientists, warning that America might lag dangerously behind the Soviets in a race to thermonuclear superiority.

Careers rose and fell on the issue of the H-bomb. J. Robert Oppenheimer, for a time the most powerful figure in the scientific-military leadership of the country, fought to block the device. This opposition came to haunt him in the security investigations of 1953/4 when the Atomic Energy Commission (AEC) withdrew his clearance and declared him a security risk. Harvard president James Conant, chemist and scientific leader, was another spirited opponent of the weapon, and by some accounts may have lost his quest for the presidency of the National Academy of Sciences because of his position on the bomb.[1] Teller, by contrast, pressed vigorously for thermonuclear weapons on scientific as well as strategic grounds; he successfully lobbied for the establishment of a new national weapons laboratory and ultimately became known as the "father" of the Super.

The H-bomb debate was markedly unlike the A-bomb debate. Virtually no scientist in World War II had challenged the development of the A-bomb before the defeat of Germany. Questions about the A-bomb did not erupt among Manhattan Project scientists until shortly before Hiroshima; even then few opposed the use of the bomb on Japan. The basic A-bomb debate occurred only after Nagasaki and V-J day.

1. Steve Heims, *John von Neumann and Norbert Wiener* (Cambridge, 1980), 482, n. 27; James Hershberg, "'Over my dead body': James B. Conant and the hydrogen bomb," to appear in E. Mendelsohn, P. Weingart, and M. Roe Smith, eds., *Science, technology, and the military* (Boston, 1989), 38–48.

In contrast, by 1949 the very possibility of the H-bomb had propelled many prominent scientists to probe the ethical issues its potential development raised. Their questions sometimes differed, and often their answers—at least as revealed in the extant records—were unsystematic, even fragmentary. Still, the probing moral and political questions the scientists posed during this period reached much further than any debate that occurred during the building of the atomic bomb.

Most of the major participants did not take consistent positions. During World War II, Oppenheimer and Enrico Fermi had easily endorsed the Super; Conant had sometimes expressed deep uneasiness but also supported it. In late 1945, however, Oppenheimer and Fermi, joined by Ernest O. Lawrence, shifted to oppose the Super on moral grounds. By early 1947, however, Fermi had reversed himself again, and began to urge work on the Super, a position he maintained until October 1949. From early 1947 to early September 1949, Oppenheimer and Conant, as government advisors and despite some doubts, had also endorsed the AEC's efforts to develop the weapon. Shortly after the first Soviet A-bomb ("Joe 1") exploded in August 1949, however, they again turned against the Superbomb—even if the Soviets were to build it. In 1949, Ernest Lawrence, in contrast, campaigned vigorously for the weapon. And Fermi, declaring the Super "necessarily an evil thing considered in any light," tried also to head it off while concluding, reluctantly, that America had to develop it if the Soviets did. Of all the participants in the debate, Edward Teller was the most consistent in his support for the new weapon.[2]

Only after the Soviets had detonated their first fission bomb and some advisors proposed a "crash" program for the Super did the hydrogen bomb become a *major* issue for American scientists. Arguing hard for and against the bomb with Oppenheimer, Conant, and Teller, as well as with Fermi and Lawrence, were physicists Luis Alvarez, Hans Bethe, I.I. Rabi, Arthur H. Compton, and chemists Glenn Seaborg and Harold Urey—as well as physicists John Wheeler, John Manley, Leo Szilard, Karl Compton, Robert Serber, Henry D. Smyth, Lee DuBridge, Robert Bacher, Cyril Smith, V.F. Weisskopf; mathematician/physicists John von Neumann and Stanislaw Ulam; chemists Wendell Latimer, Kenneth Pitzer; and others. On the

2. Teller's own views on the use of A-bomb, as expressed in Teller, with Allen Brown, *The legacy of Hiroshima* (Garden City, 1962), 13–14; cf. Teller to Leo Szilard, 2 Jul 1945, Gertrud Weiss Szilard and Spencer Weart, eds., *Leo Szilard: His version of the facts* (Cambridge, 1978), 208–209.

periphery of these arguments were Albert Einstein and Linus Pauling. The debate, which continued even after Truman's January 31, 1950 decision, forced many of the best American scientists of the 1940s and 1950s to take sides. It split the previously tightly-knit community of American nuclear physicists and chemists.

In treating the H-bomb history, this essay has three goals. First, we want to counter the usual assumption that the history of the hydrogen bomb can be parsed into a story of two unchanging camps, one for it led by Lawrence and Teller, and one against it directed by Conant and Oppenheimer. Such interpretations, encouraged by an over-reliance on the transcripts of the 1954 Oppenheimer "trial," distort and unduly polarize a more complex and interesting history of the weapon. Second, we will periodize the history of the H-bomb in such a way that the scientists' flip-flops make historical sense. Third, we use the arguments advanced by both sides to analyze the many levels—strategic, technical, and moral—of the debate, and to explore some of the relations among them. We are not seeking to analyze the Oppenheimer case, though our history does shed some light on that painful event. We will not attempt a technical history of the bomb—that is both beyond the scope of this study and the laws of classification. Nor will we pursue Herbert York's interesting hypothetical counterfactual investigation of what mght have happened in the arms race had the United States foregone the H-bomb.

1. WORLD WAR II AND THE AFTERMATH

War work on the Thermonuclear bomb

During a lunch in early 1942, Teller and Fermi discussed the idea of a hydrogen bomb in the context of the wartime effort. Bethe recalled, "we believed that the assembly of the fission bomb would be so simple that we could concentrate our main effort on the problem of thermonuclear weapons."[3] Although the optimism about the A-bomb soon faded, interest in the H-bomb continued in the Manhattan Project. From the beginning, physicists understood that a hydrogen weapon, were it possible, would have a destructive force limited only by the amount of fusionable material included. At a cost of cents per gram of explosive (not hundreds of dollars as in fissionable material), weapons might be designed yielding a force commensurate with that of the San Francisco earthquake. At first, Teller's equations exploited

3. Bethe, sworn testimony, "Statement of Hans Bethe," attached to Bethe to Samuel Silverman, 18 Mar 1954, BP, box 12, file, "Oppenheimer case."

only the roughest of approximations—and indicated that the bomb would work perhaps too well, since it might ignite catastrophically the light elements in the earth's crust. By the time of an Oppenheimer-hosted Berkeley summer conference in 1942, more realistic number crunching suggested that a Superbomb could be made without such dire consequences.[4]

The original idea of the Super was to fuse deuterium nuclei. Systematic theoretical work at Los Alamos began under Teller's direction in the fall of 1943, and the few physicists involved in the H-bomb project were soon joined by an engineering group that built a cryogenic laboratory to liquify deuterium. But to combine deuterium nuclei demanded a temperature of hundreds of millions of degrees. By February 1944 there was considerable concern at the laboratory that such temperatures were not practical because the deuterium would rapidly lose heat by a physical process known as "Compton cooling, (inverse Compton scattering)."[5] The only cure seemed to be to lower the "ignition" temperature by adding tritium, yet another cousin of hydrogen, which unlike deuterium, was extremely difficult to produce. The combined problems of tritium production and theoretical snags, as well as the need to develop the A-bomb, caused the laboratory leadership to set the Superbomb on a low priority during the remainder of the war.[6]

Oppenheimer's wartime decision to give the H-bomb project few resources disappointed and may have antagonized Teller, who insisted upon pursuing the Super. Hans Bethe very much wanted Teller's assistance on the A-bomb—Teller had contributed crucially to the physics of implosion associated with the plutonium bomb. But with the approval of Oppenheimer and Bethe (technically Teller's chief as head of the Theoretical Division at Los Alamos), Teller devoted nearly the entire last year of the war to working exclusively on the H-bomb while most others were laboring to produce fission weapons.[7]

Teller made progress. He reported to Washington on October 6, 1944: "the theoretical investigation of the super gadget has been carried out in considerable detail so that we can now predict with

4. Hawkins, "Toward Trinity," in D. Hawkins, E.C. Truslow, and R.C. Smith, *Project Y: The Los Alamos story* (Los Angeles, 1983).

5. JCAE report, "Policy and progress in the H-bomb program: A chronology of leading events," 5, Records of JCAE, RG 128, NA. William Borden planned and contributed to this document.

6. Ibid.

7. Bethe, "Comments on the history of the H-bomb," *Los Alamos science, 3* (Fall 1982), 44; Bethe, in AEC, *In the matter of Oppenheimer* (Washington, D.C., 1954), 325; for Teller's view, see Teller, *Better a shield than a sword* (New York, 1987), 70–71; and Teller, in AEC, ibid., 710–711.

reasonable probability that such a gadget will become feasible a relatively short time after the first successful fission gadget is produced." He estimated that approximately 100 scientists and technicians would be sufficient to carry out the task.[8] Oppenheimer also thought that the Super should claim attention after the war. "The subject of initiating violent thermonuclear reactions," he wrote to Washington on September 20, 1944, should "be pursued with vigor and diligence, and promptly [in peacetime]." A way station to a thermonuclear bomb might be a boosted fission weapon (an ordinary atomic bomb with a blast augmented by a small amount of deuterium and tritium at its core). It was not clear whether this "booster" weapon might even be developed during the wartime Manhattan Project, he said, "but it is of great importance that such...gadgets form an experimentally possible transition from a simple gadget [a fission bomb] to the super and thus open the possibility of a not purely theoretical approach to the latter."[9]

Advising Washington about postwar nuclear options on October 30, 1944, Enrico Fermi joined in recommending that the superbomb be pursued in peacetime. It was one of a few different ways, he explained, to "improve the explosive power [of nuclear weapons] by a large factor."[10] The recommendations of Oppenheimer, Fermi, and others persuaded a special, four-man Office of Scientific Research and Development (OSRD) Committee on Postwar Policy, chaired by physical chemist Richard Tolman and including Henry Smyth, to recommend in their report of December 28, 1944, the postwar development of thermonuclear weapons. H-bombs "of ten thousand fold greater power [than fission weapons] may even be feasible." Such weapons "would permit an enemy in a single day preceding declaration of hostilities to carry out an action which might be decisive for the outcome of a war."[11]

OSRD director Vannevar Bush and his deputy, James Conant, were also thinking in 1944 about the future prospects of the Super. Unlike Oppenheimer, Fermi, and the Tolman committee, however, both Bush and Conant explicitly worried about the likelihood of a postwar Soviet-American nuclear arms race and hoped that the specter of an H-bomb might persuade Secretary of War Henry L. Stimson and President Franklin D. Roosevelt to consider some form of international control of atomic energy. For Bush and Conant, the prospect of

8. Teller to Richard C. Tolman, 6 Oct 1944, file 471.6 (Super), LAR.
9. Oppenheimer to Tolman, 20 Sep 1944, in AEC (ref. 7), 954–955.
10. Fermi to Tolman, 30 Oct 1944, Tolman files, box 6, OSRD, RG 227, NA.
11. Tolman, chairman, "Report of Committee on Postwar Policy," 28 Dec 1944, Tolman files, box 6, OSRD Records.

the H-bomb was an added reason for seeking to avoid such an escalation. For them, as for Niels Bohr, what was needed was a wartime approach to the Soviets, and not bigger weapons, to guarantee the peace after the war. The counsel of Bush and Conant, like that of Bohr, failed in late 1944 and early 1945 as Roosevelt and Stimson maintained nuclear secrecy and continued to husband the options for future "atomic diplomacy."[12]

Despite the doubts and fears of Bush and Conant, which they concealed from the Manhattan Project scientists, none of the Los Alamos scientists—including Oppenheimer, Fermi, Bethe, and Teller—expressed any moral, scientific, or strategic doubts about investigating thermonuclear weapons with the goal of developing a Super. The wartime Manhattan Project, conceived in fear of a race against Nazi Germany, had taken on a larger momentum; its tasks already involved an exploration of fundamental aspects of nuclear physics, and recommendations to build even more powerful weapons to keep the peace after World War II, or, if peace failed, to win future wars. The Super, it appeared, would be an important part of that postwar effort.

After V-E Day, in May 1945, Conant shared with Bush his hopes and fears about nuclear weapons. He urged that the government should "back an all out research program for the super-duper [presumably the Super] as first priority [while] *at the same time* with equal priority push for an international armament commission. We have about 5-10 years to do both!"[13] Conant's counsel reflected his own uncertainties and confusion.

Conant's own uneasiness about nuclear weapons and their place in the postwar world had helped spur Secretary of War Stimson in early May to establish the blue-ribbon Interim Committe primarily to advise the Secretary of War on postwar atomic policy. On May 31, 1945, the Super was briefly the subject of discussion of the committee's Scientific Advisory Panel, made up of A.H. Compton, Fermi, Lawrence, and Oppenheimer. Oppenheimer said that the

12. Bush and Conant to Secretary of War, 30 Sep 1944, with memoranda, "Salient points concerning future international handling of subject of atomic bombs," and "Supplementary memorandum giving further details concerning military potentialities of atomic bombs and the need for international exchange of information," both 30 Sep 1944, HB, 77, Records of the MED, RG 77, NA. See Barton J. Bernstein, "The quest for security: American Foreign policy and international control of atomic energy, 1942–1946," *Journal of American history, 60* (1974), 1003–1009, and Bernstein, "Roosevelt, Truman, and the atomic bomb, 1941–1945: A reinterpretation," *Political science quarterly, 90* (Spring 1975), 26–32; Martin J. Sherwin, *A world destroyed* (New York, 1975), 86–116.

13. Conant to Bush, 9 May 1945, BC, his emphasis. Also see Conant, "Possibilities of a super bomb," 20 Oct 1944, in ibid.

Super would be more difficult to devise than a plutonium A-bomb and that it would take a minimum of three years. The Super, he estimated, might have an explosive force equivalent to 10 million to 100 million tons of TNT. Secretary of State Designate James Byrnes, in the words of the minutes, "expressed the view, *which was generally agreed to by all present*, that the most desirable program would be to push ahead as fast as possible in production and research [of all nuclear weapons] to make certain that we stay ahead and at the same time make every effort to better our political relations with Russia."[14]

In mid-June 1945, the Scientific Advisory Panel reported to the Secretary of War about the state of nuclear studies in general and thermonuclear weapons in particular. In the report, Oppenheimer overflowed with confidence about the project and the future: "Atomic bombs are weapons of overwhelming potential power; and this military development must be pursued on a commensurate scale. The ultimate possibilities of atomic power can revolutionize our economy: the raw material is available, and the technical problems can be solved." According to Oppenheimer and his collaborators, nuclear power would constitute a part of the national economy comparable to the combustion engine and electronics—Federal funding of a billion dollars per year "seems appropriate." The panel advocated breeder reactors, "fundamental studies" of nuclear physics, and applications to chemistry, biology, medicine, and industry.

It also advocated, as its first item, the development of thermonuclear weapons:[15]

> We believe the subject of thermo-nuclear reactions among light nuclei is one of the most important that needs study. There is a reasonable presumption that with skillful research and development fission bombs can be used to initiate the reactions of deuterium, tritium, and possibly other light nuclei. If this can be accomplished, the energy release of explosive units can be increased by a factor of a thousand or more over that of presently contemplated fission bombs.

The panel added that it might be possible to detonate "significant thermo-nuclear reactions" without the use of fission weapons, using ordinary high explosives. Looking into the future the four physicists concluded that the project of building a hydrogen bomb might be closely analogous to the Manhattan project itself: an amalgam of fundamental physics, industrial technique, and "novel, radical variations" in ordnance procedures. Los Alamos laboratory, they argued, should

14. Interim Committee, minutes, 31 May 1945, HB 100, RG 77, NA. Emphasis in original.
15. Oppenheimer (for the Panel) to George Harrison, 16 June 1945, BC.

make the project as high a priority "as is consistent with more immediate wartime commitments."[16]

The only caveat the panel placed on the expected postwar nuclear advances had to do with secrecy of two kinds. First, reflecting the physicists' frustrating encounter with military compartmentalization during the Manhattan Project, the panel argued that "almost all scientists [in the project] have indicated their despair of continuing to work effectively under conditions of extreme secrecy or extreme control;" such measures only "reduc[e] effectiveness by withholding from those who may have need of them essential facts, or essential insight." Second, the panel argued for international cooperation as part of an attempt "to use the present modest but not inconsiderable hegemony which we have attained to the benefit, not of this nation alone, but of all peoples."[17]

The argument against secrecy also involved the physicists' ethic. In the panel's words, "There are also more far-reaching things [than efficiency], in that the whole temper, spirit, value, and dignity of science is incompatible with secrecy. In the long run scientists will leave this field or will devote to it only the poorest part of their efforts if it is not carried out in an open way and in the full high confidence that knowledge is a good thing and its spread a good thing for humanity."[18] This sense of the intrinsic necessity of openness as a part of the "spirit of science" cut across the divisive issues raised by the Super; some supporters of secrecy opposed the H-bomb while some advocates of the Super militated for greater openness in the debate.

Considering the spectacular successes of the Manhattan Project, the frantic pace of development before Trinity, and the role of Oppenheimer, Fermi, Lawrence, and Compton in these efforts, it is understandable that future problems and doubts about the hydrogen bomb were at least temporarily pushed aside.[19]

After Hiroshima, 1945–46

After the atomic bombing of Hiroshima and Nagasaki on August 6 and 9, 1945, many physicists— foremost among them Oppenheimer— were plagued with doubts about the weaponry they had created. Teller recalls Oppenheimer's coming to his office on the day of

16. Ibid.
17. Ibid.
18. Ibid.
19. Cf. Conant, "Notes on the 'Trinity' test held at Alamogordo bombing range 125 miles southeast of Albuquerque [New Mexico] 5:30 a.m. Monday, July 16," BC; see journal of Jack Hubbard, 16 Jul 1945, copy provided by Ferenc Szasz.

Hiroshima to say that "we would not develop a hydrogen bomb."[20] Eight days after the bombing of Nagasaki, the Scientific Advisory Panel, in a report written by Oppenheimer, informed the Secretary of War of the "quite favorable technical prospects of the realization of the super bomb." Yet when the physicists moved beyond scientific-technical advice they now warned that a continuing nuclear buildup, and the development of new nuclear weapons, was not the road to world peace or national security. The panel's anxieties, reaching to the core of international political issues, led it to stress that reliance upon nuclear weapons could be a dangerous illusion. "The safety of this nation," in Oppenheimer's words, "as opposed to its ability to inflict damage on an enemy power—cannot lie wholly or entirely in its scientific or technical prowess. It can be based only on making future wars impossible."[21]

The panel expressed deeper worries in late September 1945 when it warned Washington against an escalating arms race and against the construction of thermonuclear weapons.[22] Summarizing at the end of its September meeting, panel member Compton put it in the starkest possible terms: "We feel that this development [of the H-bomb] should *not* be undertaken, primarily because we should prefer defeat in war to a victory obtained at the expense of the enormous human disaster that would be caused by its determined use."[23]

With these words the panel rejected in this case, the principle, often termed a scientific imperative, that scientists always had an obligation to try to discover nature's secrets and develop new knowledge. To the panel, the quest for the thermonuclear was not a matter of "mere" technology, but required basic scientific work, as they made clear in their report of September 1945. It showed that basic investigations were needed into the ranges and scattering of reaction products, the properties of high-energy neutrons, the production of large magnetic fields, conduction in ionized gases, and new, highly complex calculations. Second, though this was not mentioned in the report, many physicists were entranced by the prospect of creating a phenomenon on earth—for the first time—that before then had only existed in the heavens: the chain fusion reaction. But some knowledge appeared to be too dangerous to pursue at least for a decade or so.

20. Teller (ref. 7), 71.

21. Oppenheimer to Secretary of War, 17 Aug 1945, OP.

22. "Proposal for research in the field of atomic energy," report of the Scientific Panel to the Interim Committee, 28 Sep 1945, in AEC, "Thermonuclear weapons program chronology" (1955), 10ff, AEC Records, Department of Energy, Germantown, Maryland.

23. Compton to Henry A. Wallace, 27 Sep 1945, ACP.

Compton did mention that the Super might justifiably be reassessed in ten years. "Perhaps there may be then, an international government adequate to make its development under world auspices safe or perhaps unnecessary for further consideration.[24]

Dissenting sharply from the panel's position, Teller argued that other nations could design a research program that would lead them more or less simultaneously to the H-bomb and the A-bomb. The United States itself was now, Teller argued, in a position to build an H-bomb in five years and it should. Moral opposition to the bomb was "a fallacy."[25]

> If the development is possible, it is out of our powers to prevent it. All that we can do is to retard its completion by some years. I believe, on the other hand, that any form of international control may be put on a more stable basis by the knowledge of the full extent of the problem that must be solved and of the dangers of a ruthless international competition. The terrible consequences of a super bomb will not be avoided by ignoring or postponing the issue but by wise and provident planning.

The attitude expressed by the new Los Alamos leadership under Norris Bradbury, chosen director by Oppenheimer, seemed compatible with Teller's desire to explore the new weapon in order to exhibit its dangers. In his talk to the Los Alamos Laboratory Coordinating Council on October 1, 1945, Bradbury said that the American lead in nuclear weapons ensured the best chance of producing peace and that the laboratory should turn over to the commission, which would replace the MED, the best arsenal of nuclear weapons it could produce given the constraints of a reduced staff and resources:[26]

> The use of nuclear energy may be so catastrophic for the world that we should know every extent of its pathology. How bad *can* this bomb (if it were made a weapon) be? I shall return to this premise again in connection with the Super. One studies cancer—one does not expect or want to contract it—but the whole impact of cancer on the race is such that we must know its unhappy extent. So is it with nuclear energy released in this form...we must know how terrible it is.

Thorough testing would "convince people more than any manifesto that nuclear energy is safe only in the hands of a wholly cooperating world" and also "provide some intellectual stimulus for people working here. Answers can be found; work is not stopped short of completion; and lacking the weapon aspect directly, another TR[inity] might

24. Ibid.
25. Teller to Fermi, 31 Oct 1945, in LAR.
26. Truslow and Smith, "Beyond Trinity," in Hawkins, Truslow, and Smith (ref. 4), 362.

even be FUN."[27]

Bradbury concluded that fundamental experiments had to be conducted to answer the question, "Is or is not a Super feasible?" There was an intrinsic fascination in such experimentation,[28]

> We cannot avoid the responsibility of knowing the facts, no matter how terrifying. The word "feasible" is a weasel word—it covers everything from laboratory experiments up to the possibility of actual building, for only by building something do you actually finally determine *feasibility.* This does not mean we will build a Super. It couldn't happen in our time in any event. But someday, someone must know the answer: Is it feasible?

Despite Bradbury's commitment to the Super, the pace of weapons work in postwar Los Alamos was not rapid or expansive enough to include the vigorous fusion program Teller believed necessary. Disappointed by the postwar Los Alamos schedule, Teller decided to leave New Mexico and go to the University of Chicago to resume academic work in physics, while still hoping that Los Alamos would push for both a more energetic fission program and the Super.[29] Meanwhile, Oppenheimer was confessing his guilt in the highest places. He "came in my office," Truman complained to an associate, "and spent most of his time ringing [sic] his hands and telling me they had blood on them because of the discovery of atomic energy."[30]

By the winter of 1945/6, the division between Teller and Oppenheimer was emerging sharply. Teller, a brilliant Hungarian-born theoretical physicist who had emigrated from Germany in the 1930s, greatly distrusted the Soviet Union's intentions, and believed that improving nuclear weapons was essential to American security. Oppenheimer, a charismatic leader and an American-born theoretical physicist, had emerged from the war as the dominant scientist in the weapons community. He believed that the 1946 Acheson-Lilienthal plan for international control, a plan that he had helped craft, might well stop an arms race and constitute a keystone in the larger edifice of a peaceful community of nations.[31]

Despite the advice of Oppenheimer and others on the Scientific Advisory Panel, Los Alamos did explore prospects for the Super in

27. Ibid., 363.

28. Ibid.

29. AEC (ref. 7), 711–713.

30. Truman to Acheson, 7 May 1946, PSF. The date of the meeting is probably 25 Oct 1945; "The President's appointments Thursday, October 25, 1945," Truman Library.

31. See Bernstein, "Four physicists and the bomb: The early years, 1945–1950," *HSPS, 18:2* (1988), 231–251 for more on Oppenheimer in 1945–46.

1945 and 1946. The small Super group (F-1, later T-7) was formally relieved of its task of designing and testing fission bombs and authorized to turn entirely, for the first time, to thermonuclear weapons. By March 1946, Teller and von Neumann were eager to have a Los Alamos meeting on the prospects for the Super. They wanted an endorsement by their colleagues, in the words of Philip Morrison, "to initiate under the [Manhattan] District a sizeable development program."[32]

Morrison apparently feared that the leap to the Super might proceed as the result of purely scientific decisions. He wrote Robert Serber, who had also been a graduate student under Oppenheimer in the 1930s and a theorist at Los Alamos:[33]

> We have discussed the issues so raised at some length here. The general feeling is that, while one ought not to avoid facing the problem of decision on the technical feasibility of the new device—and in some ways this knowledge would prove of general value—it will be very bad policy if the decision is taken to proceed with this new and extraordinarily important work without a sharp decision at the highest level of policy making....The President and Secretary of State, and other persons adequate to establish national policy, must make the decision to begin in this field.

Morrison pleaded with Serber, despite "your natural inclination," to attend the conference, because "we are afraid that without the presence of people like you, we shall not be able to place [our arguments] strongly before the enthusiasts. Moreover, if we are ever to learn about this machine [presumably the Super], it will be because people as critical as yourself paid some attention to the ingenious but uncritical work done to date." Morrison sent similar pleas to Oppenheimer and Bethe.

The conference on the Super took place on April 18–20, 1946. Oppenheimer, Bethe, and Fermi did not attend; Teller, von Neumann, Serber, and the British physicist (and spy) Klaus Fuchs did. The meeting analyzed the bomb's theoretical possibility, destructive capabilities, needed ancillary experiments, and conceivable peacetime applications. Summarizing the conference proceedings, the committee (including Teller) in charge of writing the report concluded that the Superbomb could be built:[34]

32. Philip Morrison to Robert Serber, 28 Mar 1946, OP.

33. Ibid.

34. "Report of conference on the Super," LA-575 (unclassified, "sanitized" version), 44. Original version issued 12 June 1946; cover sheet dateline, 16 Feb 1950, Los Alamos Scientific Laboratory.

> It is likely that a super-bomb can be constructed and will work. Definite proof of this...can be made only by a test of the completely assembled superbomb.
>
> The detailed design submitted to the conference was judged on the whole workable. In a few points doubts have arisen concerning certain components of this design....In each case, it was seen that should the doubts prove well-founded, simple modifications of the design will render the model feasible.

Along with this optimism came the realization that such an effort "would necessarily involve a considerable fraction of the resources which are likely to be devoted to work on atomic developments in the next few years."[35] At the time there were only about nine A-bombs in the arsenal.[36] Meanwhile the possible costs of the H-bomb had escalated because of an increase in the expected need for tritium. Given the H-bomb's possibly extraordinary costs and enormous destructive potential the committee felt it "appropriate to point out that further decision[s] in a matter so filled with the most serious implications as is this one can properly be taken only as part of the highest national policy."[37]

A slow quest, 1947–49

Soon after the failure in later 1946 to achieve Soviet-American agreement on international control of atomic energy, higher authorization arrived, though not yet from the President. In February 1947, partly at the urging of Fermi, who had abandoned his September 1945 opposition to the H-bomb, probably under the impact of the Cold War, the newly-created Atomic Energy Commission decided that America should take the next steps towards the development of the Super. Oppenheimer initially opposed Fermi's proposal, arguing in the words of the minutes, "it is conceivable that because of the prejudice against weapons among our colleagues, it might be wiser to steer clear of this subject and not ask to have the super bomb pushed at Los Alamos."[38]

Oppenheimer's opposition was relatively weak, probably because of his own growing doubts about the possibility of a Soviet-American accord. In a sworn statement a few years later, Bethe recounted that early in 1947 Oppenheimer had, "much earlier than I or most of my

35. Ibid., 46.
36. ERDA chart to Bernstein (n.d.).
37. Ref. 34.
38. Draft minutes of the General Advisory Committee (GAC), 2–3 Feb 1947, AEC Records, DOE.

colleagues, [seen] that the Russian attitude was absolutely rigid and that it made [an] international control system impossible."[39] Overridden by others in the February meeting of the AEC's General Advisory Committee (GAC) meeting, Oppenheimer uneasily supported work on the H-bomb.[40] The recent failure of the American (Baruch) plan for international control of atomic energy had inaugurated a new phase in the hydrogen bomb debate, in which the more intense pressure of the Cold War eroded, though it did not stop, sentiment against the H-bomb.

In late March 1947, at a meeting of the GAC, composed of nine scientists including Fermi, Oppenheimer, Conant, Rabi, Seaborg, Smith, and DuBridge, Fermi emphasized that the thermonuclear work "needed a theoretical physicist of Teller's ability but with a high degree of pessimism concerning the possibilities of thermonuclear devices." Presumably Fermi's remarks meant that physicists worried that Teller was too committed to assess the feasibility of the weapon objectively. With the apparent agreement of Fermi and other GAC members, Oppenheimer proposed a low priority for the exploration of the Super, contending that the matter could not be resolved in the next five years but could be fully analyzed sometime between 1952 and 1967.[41]

That summer of 1947, after the announcement of the Truman Doctrine and the Marshall Plan, a Los Alamos study determined the amount of fission required to ignite deuterium; the Super, this report concluded, was probably feasible scientifically. The program recommended continued research on the Super, increased studies on the light elements, further work on the booster using both deuterium and tritium, and work on the initiation of the thermonuclear reaction.[42] During the next year, it became clear that a technical way-station was needed before constructing a "true" hydrogen bomb: a device that could explode large amounts of fusionable material with a small fission trigger. At the meeting of the GAC in June 1948, Oppenheimer proposed that Los Alamos should step up work on such a booster. It would ignite heavy hydrogen, and therefore test the nuclear reagents eventually needed for the Superbomb; and it would enhance the

39. Ref. 3.
40. Ref. 38.
41. Fermi, paraphrased in GAC draft minutes, 28–30 Mar 1947, AEC Records, DOE.
42. Original Los Alamos report issued Sep 1948; recommendations approved Dec 1948 after consideration by Military Liaison Committee and the AEC from "Historical statement, Appendix 'B,'" 10 Nov 1949, attached to AEC report to Truman; 9 Nov 1949, AEC 262/2, AEC Records, DOE.

fission process by contributing additional energetic neutrons. According to the minutes, Conant "was disturbed" about pursuing any way-station because he seemed fearful of the Super's great power. Fermi countered that it was unwise "to remain in ignorance of the possibilities."[43]

Despite Conant's objections, the GAC recommended pushing ahead with the booster and with an increase in tritium production which was necessary for the booster. The GAC expected that this weapon could be developed in two to five years. "Its success," Oppenheimer explained, "would not mean an immediate radical improvement in weapons but [it] would give some new options to the National Military Establishment."[44] Since scientists did not know how to construct an H-bomb and the military had not stated that it desired such a weapon, none of the GAC or AEC members urged a more vigorous Super program.[45]

The thermonuclear research program and its prospects were top-secret matters. Though a former Truman administration official had publicly mentioned the possibility of a Super and Teller had publicly hinted at it,[46] neither comment attracted much attention. The President himself seemed unaware of work on the bomb and scientists usually did not talk of it outside their own small community. They felt barred from discussing the issue publicly and sometimes, under restrictive need-to-know rules, even from talking about technical problems with their colleagues. Nuclear physicists often chafed under such rules; but they loyally conformed to them. The *Bulletin of the atomic scientists,* which often criticized secrecy, actually imposed self-censorship and decided not to publish speculations about the H-bomb lest they encourage the Soviets to enter the race.[47]

The low-key program of 1947/8 met the needs and goals of Bradbury and many Los Alamos scientists, but it disappointed Teller, and spurred Compton to urge more vigorous action. In September 1945, as

43. As paraphrased in GAC minutes, 4–6 June 1948, AEC Records, DOE.

44. Ibid.

45. Ref. 42. Panel on Long Range Objectives, "The long range military objectives in atomic energy," 18 Aug 1948, AEC (ref. 22), 17–19.

46. John J. McCloy, public address extracted in *BAS, 3* (Jan 1947), 5, and Teller, "How dangerous are atomic weapons?" *BAS, 3* (Feb 1947), 35–36. Tolman told AEC chairman David Lilienthal that McCloy's article, originally in *Infantry journal,* was "a serious breach of security." Tolman to Lilienthal, 4 Jan 1947, box 34, Records of the Special Assistant to the Secretary of State for Atomic Energy Matters, Department of State Records, RG 59, NA.

47. Editorial, "Secrets will out," *BAS, 6* (Mar 1950), 67–68. J.H. Rush later complained that the *Bulletin* itself had used secrecy to block democratic dialogue when its editors "deliberately suppressed discussion of fusion bombs," *BAS, 6* (May 1950), 138.

a member of the Scientific Advisory Panel, he had recommended that Washington not seek to develop the Super. But by 1947 or 1948, more fearful of the Soviets and eager to enhance America's nuclear arsenal, Compton reversed himself. He believed strongly that the Super "should first be in our hands." His earlier moral objections had given way under the impact of growing Soviet-American hostility.[48]

AEC plans for 1949, despite fears in the West triggered by the Czech coup and the Berlin blockade, embodied no crash program. Work was to proceed in the development of the booster, with the ignition mechanism, and with theoretical work on the dynamics of light elements, such as hydrogen and lithium. As AEC Chairman David Lilienthal stated in late July 1949, "In regard to thermonuclear assemblies, theoretical studies are continuing at Los Alamos at a pace which does not interfere seriously with more urgent elements of the laboratory program."[49] Most probably the H-bomb issue never came sharply before Truman before September 1949. It was just one among many nuclear weapon systems being pursued; if he had been informed of it, he could easily have forgotten it.[50]

Even Teller had given up hope for a vigorous quest for the Super. On August 23, 1949, in discussing subjects for a meeting on weapons programs at Los Alamos in September, Teller put the H-bomb as the last consideration on his list.[51] Indeed, whatever their disagreements, both Bethe and Teller agreed that practically no work was done on the Super between 1947 and 1949. In 1952, Teller recalled that between 1947 and 1949 only three senior physicists had been on the project: Robert Richtmyer for eight months, Lothar Nordheim for a month, and Teller himself for about two months, along with two or three helpers doing calculations for a full year.[52]

By August 1949, the hydrogen bomb had passed through three distinct stages. In the first, the weapon was inextricably bound to the fate of the atomic bomb. The second stage began after Hiroshima, and was marked by a groundswell of reaction against the possibility of an arms race and widespread support in the scientific community for an international accord. A third stage started in early 1947 with the failure of the Baruch plan, and this period was characterized by a quite modest continuation of support from leading scientists.

48. Compton to Gordon Gray, 21 Apr 1954, ACP. Compton could not recall whether his urging occurred in 1947, which seems early, or 1948.

49. David Lilienthal to Military Liaison Committee, 27 Jul 1949, JCAE report, 20.

50. R. Hewlett and F. Duncan, *A history of the United States Atomic Energy Commission*, vol. 2, *Atomic shield, 1947–1952* (University Park, PA, 1969), 374.

51. Teller to Bradbury, 23 Aug 1949, LAR.

52. Teller, "History of the thermonuclear program," 14 Aug 1952, cited in JCAE Report, 15.

2. SHOULD THE BOMB BE BUILT?

Joe 1 and the push for the Super

The pace of weapon builders and politicians was abruptly altered in September 1949 after an American plane flying over the North Pacific picked up radioactive debris indicating that the Soviets had detonated their first atomic weapon. Although physicists had repeatedly predicted that the Soviets could build an A-bomb in about five years after Hiroshima, the detonation shocked the political and military establishment. "This means that we are in a straight race with the Russians," Under Secretary of State James Webb told the Cabinet.[53] Capturing the sense in Washington of crisis, AEC Chairman David Lilienthal wrote in his diary, "the Russian bomb has changed the situation drastically, and...the talk of our having anticipated everything and following the same program we had before is the bunk."[54] "Joe 1" marked the beginning of a fourth stage in the debate—more intense and bitter than before.

In late September 1949, galvanized by the Soviet A-bomb, the Joint Congressional Committee on Atomic Energy raised the necessity of a much increased thermonuclear effort. The Joint Committee, which served as a legislative watchdog and tried to shape weapons-development policy by pressuring the AEC and the President, had frequently pushed for larger programs than the budget-conscious Truman wished to endorse. The Joint Committee's September agenda delineated twenty-three possible methods to improve nuclear weapons including an all-out H-bomb effort.[55] That same week, the AEC's General Advisory Committee refused to plunge into new programs or to yield to popular fears. As Oppenheimer explained in a secret report, "We felt quite strongly that the real impact of the news of Operation Joe lay not in the fact itself, but in the response of public opinion and public policy to the fact. For this reason and because we ourselves doubted our wisdom in foreseeing this response, we wanted to postpone making any recommendations based on the new situation." He expected that the GAC would meet again in early December 1949 to

53. Matthew Connelly, Cabinet meeting minutes, 23 Sep 1949, Connelly Papers, Truman Library.

54. David E. Lilienthal, *The journals of David E. Lilienthal,* 2 vols. (New York, 1965), *2,* 580.

55. JCAE minutes, 29 Sep 1949; JCAE(ref. 5), 20–21.

appraise the situation.[56]

Pressures for the H-bomb compelled a meeting in late October.[57] On October 5, 1949, Admiral Lewis Strauss, an AEC Commissioner and Herbert Hoover Republican, advocated "a quantum jump in our planning...that is to say, that we should now make an intensive effort to get ahead with the super. By intensive effort, I am thinking of a commitment in talent and money comparable, if necessary, to that which produced the first atomic weapon. That is the way to stay ahead."[58] Strauss brought the memorandum to Senator Brien McMahon, chairman of the Joint Committee; to insure the memo's impact, Strauss also routed it to the National Security Council Executive Secretary, who passed it to the President a few days later.[59]

Independently of Strauss, Lawrence, Alvarez, and chemist Wendell Latimer at Berkeley seized upon the Super as the appropriate response to the Soviet A-bomb. They feared that the Soviets might be ahead. Campaigning avidly for the Super, Latimer found strong support among other chemists—Kenneth Pitzer, who was on leave from Berkeley while serving as the AEC's director of research, and Harold Urey and Willard Libby at Chicago.[60] Alvarez later recalled that he urgently wanted to see Lawrence when he heard of the Russian bomb, in order to spur him to action; when the two did meet, Alvarez was pleased to find that Latimer had already persuaded Lawrence of the pressing need to accelerate work on the hydrogen bomb.[61]

Lawrence became a powerful advocate. As the Cold War developed, Lawrence had come to stress the need for a large defense system; but until early October 1949, Lawrence had paid little, if any, attention to the Super for he was deeply involved in his own lab and in promoting big machine physics. His advice to Washington on weaponry had concerned radiological warfare, which could be used against noncombatants or troops, and he had urged a stepped-up program. The shock of the Soviet A-bomb destroyed any ethical objections he may still have had to the H-bomb. With his great prestige as a Nobel laureate and as the director of the Berkeley Radiation Laboratory, he became a powerful campaigner for this new weapon.[62]

56. Oppenheimer to Lilienthal, 26 Sep 1949, OP.
57. Lilienthal to Oppenheimer, 11 Oct 1949, in AEC (ref. 22), 22a.
58. Lewis Strauss to AEC, 5 Oct 1949, in AEC (ref. 22), 22a.
59. JCAE (ref. 5), 27.
60. Latimer, in AEC (ref. 7), 659.
61. Alvarez interview with Galison, 16 Jan 1988.
62. On Lawrence, see Bernstein (ref. 31); Childs, *An American genius: The life of Ernest Orlando Lawrence* (New York, 1968), 384–420; and J.L. Heilbron, R.W. Seidel, and B.R. Wheaton, *Lawrence and his laboratory: Nuclear science at Berkeley, 1931–1961* (Berkeley, 1981), 61–63.

Lawrence and Alvarez arranged to meet at Los Alamos on October 7, 1949 with Teller, physicist George Gamow, and physicist John Manley, who was the lab's associate director, and mathematician Stanislaw Ulam. According to Alvarez' diary, the four men "gave project [a] good chance if there is plenty of tritium available."[63] Journeying on to Washington, Lawrence and Alvarez talked with Senator McMahon on October 10. According to a memorandum in the Joint Committee files, Alvarez and Lawrence

> expressed keen and even grave concern that Russia is giving top priority to the development of the thermonuclear super-bomb. They pointed out that the Russian expert, Kapitza, is one of the world's foremost authorities on the problems involved in light elements. This fact, along with the logic that Russia might experience great difficulty in competing with us in the production of "conventional" atomic bombs, means that she has every incentive to concentrate on being first to acquire the super-bomb.

Lawrence and Alvarez warned that the Soviets "may be ahead of us in this competition. They declared that for the first time in their experience they are actually fearful of America's losing a war, unless immediate steps are taken on our own super-bomb project."[64]

McMahon urged Lawrence and Alvarez to push for the Super-bomb. They continued their campaigning among the AEC commissioners, and encountered resistance only from Lilienthal. As Alvarez recalled, Lilienthal "turned his chair around and looked out the window and indicated that he did not want to even discuss the matter." Lawrence and Alvarez next lobbied GAC member Rabi, who was "happy at our plans" to build a heavy-water reactor to produce the neutrons for the tritium that the Super seemed to require. Rabi said, in Alvarez' later paraphrase, "It is certainly good to see the first team back in....You fellows have been playing with your cyclotron and nuclei for [four] years and it is certainly time you got back to work." Returning to Berkeley in mid-October 1949, the two physicists found strong support for the reactor program and the H-bomb among Serber, Edwin McMillan, Robert Thornton and GAC member Seaborg. When Alvarez discussed the plans with Lee DuBridge, president of Caltech and also a GAC member, and Robert Bacher, a former AEC commissioner, "they had no objections and I [Alvarez] felt they were impressed with the seriousness of the situation, and thought we were doing the right thing."[65]

63. Alvarez, in AEC (ref. 7), 775.

64. Memorandum on the Lawrence-Alvarez luncheon, 10 Oct 1949, cited in JCAE (ref. 5), 27.

65. Alvarez, in AEC (ref. 7), 777–778, 781.

John Manley—though later a vocal opponent of the weapon—agreed that Los Alamos had to speed up its efforts, as he stated to the laboratory on October 13: "The Laboratory should admit at least to its own personnel that the current Laboratory program has not been geared to such an event [the Russian A-bomb] in 1949." Instead, according to Manley, Los Alamos had assumed that the Russians would not achieve the atomic bomb until 1952, a year after the scheduled American explosion of the boosted A-bomb. "The Russian achievement should teach us at least one thing: that our state of ignorance of their efforts is so nearly complete that we should no longer assume any time scale for their developments but rather choose our action so as to strengthen our position as rapidly as possible *and* maintain a rate of progress limited only by our resources for a relatively long period of time."[66]

The campaigning continued throughout October. Teller reported, in Alvarez' words, that they "could count on" Bethe. Lawrence discussed the reactor project and the Super with Senator William Knowland, a Republican member of the Joint Committee whose political support could be valuable in securing funding.[67] Meanwhile Lilienthal angrily complained in his diary that Lawrence and Alvarez were "drooling over" the Super.[68] John von Neumann wanted the Super. "I believe there is no such thing as saturation [having too many weapons]. I don't think any weapon can be too large. I have always been a believer in this."[69]

A major meeting at Los Alamos was to take place on October 19. In preparation, Teller circulated an open letter, "It is essential for us to develop a Super Bomb at the earliest possible time or else be able to say with reasonable confidence that the Super is not feasible," he wrote. "It seems that the Russian rate of progress is at least comparable to, if it does not exceed, the rate of progress in this country....If the Russians continue to make actual progress faster and if we lose the atomic armament race, it will make little difference whether the reason has been the particular brilliance of Russian scientists or the exaggerated caution and thoroughness of our group....If the Russians demonstrate a Super before we possess one, our situation will be hopeless." For Teller, the situation demanded an "all out" effort, if Los Alamos "can marshall the necessary support from Washington for a really vigorous program."[70]

66. Open letter from John Manley, cited in JCAE (ref. 5), 28–29.
67. Alvarez, in AEC (ref. 7), 782.
68. Lilienthal (ref. 54), *2*, 577.
69. Oppenheimer, quoting von Neumann, in AEC (ref. 7), 246.
70. Teller to Technical Council Members, "The Super Bomb and the laboratory program," 13 Oct 1949, Los Alamos Records.

Some key Washington figures heard the message and began to envision the Soviets arriving first at the explosive use of fusion. On October 17, 1949—echoing Lawrence, Alvarez, and Teller—Senator McMahon warned the AEC: "There is reason to fear that Soviet Russia has assigned top priority to development of a thermonuclear super-bomb. If she should achieve such a bomb before ourselves, the fatal consequences are obvious. In my opinion, American efforts along this line should be as bold and urgent as our original atomic enterprise."[71]

Opposition to the Super

Opposition was swift in coming, grounded on diverse arguments. The leader of the opposition was James Conant. Early in October he wrote Oppenheimer that the bomb would be built "over my dead body," a strong phrase for a man described as having "red tape in his veins."[72] He liberated others, including Oppenheimer, to reexamine issues and express their negative judgments on the H-bomb. Oppenheimer privately deplored the increased push for the H-bomb, calling Lawrence and Teller "promoters," and complaining that the Joint Chiefs had joined the Joint Committee in believing that the H-bomb was *the* answer to the Soviet A-bomb. Because of the Soviet detonation, Oppenheimer reported, Bethe was seriously considering returning full time to Los Alamos to work on the Super. Oppenheimer acknowledged the technical problems of the H-bomb, saying that he was unsure whether "the miserable thing will work" and that perhaps it will be so cumbersome that it could only be delivered by "ox cart." "It seems likely to me," he complained, the quest for the H-bomb will "even further...worsen the unbalance of our present war plans," which he deemed already too reliant on nuclear weapons. "It would be folly to oppose the exploration of this weapon. We have always known it had to be done; and it does have to be done, though it appears to be singularly proof against any form of experimental approach. But that we become committed to it as the way to save the country and the peace appears to me full of dangers."[73]

Teller visited Bethe in Ithaca to urge him to join the thermonuclear project. But after discussions with V.F. Weisskopf, Bethe decided to stay out. "I had a very long and earnest conversation with Dr. Weisskopff," recalled Bethe, about "what a war with the hydrogen

71. McMahon to AEC, 17 Oct 1949, in AEC (ref. 22), 23.

72. Quotes from, respectively, Teller, in AEC (ref. 7), 715 and I.I. Rabi, 8 Jan 1976, interview by William Tuttle, courtesy of Tuttle.

73. Oppenheimer to Conant, 21 Oct 1949, in AEC (ref. 7), 242–243.

bombs would be. We both had to agree that after such a war even if we were to win it, the world would not be...the world we want to preserve." Apparently Bethe had a similar talk with physicist George Placzek, a Czech emigré and former collaborator of Teller's.[74] Placzek, Weisskopf, and Bethe were so overcome with the excitement and urgency of the issue that Placzek and Weisskopf each took home the other's coat, and Bethe missed his plane from New York. At the end of October, Bethe wrote to Weisskopf, obliquely referring to their conversations: "Your discussion with me last week-end was most wholesome. I transmitted this spirit to several members of the AEC...[Eugene] Wigner, on the other hand, is more of Teller's opinion, although with less enthusiasm. I felt very much better after talking to you and Placzek."[75] While Bethe may have been relieved, Teller was distressed that Bethe would not be joining the hydrogen bomb project.

Opposition crystallized at the GAC meetings of October 28–30, 1949. The AEC had asked the members to answer the following questions. Should the AEC concentrate on building up the stockpile of fission bombs? Or should the United States pursue the Super, which would "conflict with the [fission weapons] in terms of demand for neutrons?" Since the principal alternatives to the Super were "improvements in size, weight, and manageability of [fission] weapons[,] how should these possibilities be evaluated in relation to the [Super]?....Is it clear that the United States would use a 'super' if it had one available? What would be the military worth of such a weapon, if delivered? Would it be worth 2, 5, 50 existing weapons?" What would the Super cost in terms of scientific talent, facilities, and money?[76]

Eight of the nine (Seaborg was in Europe) GAC members converged on Washington for this crucial session, the seventeenth since the committee was appointed in December 1946. Seven of the eight men had served together for nearly three years as original members of the committee: Oppenheimer, who had been repeatedly selected as chairman of the group, usually on Conant's nomination;[77] Conant; Fermi; Rabi; DuBridge; metallurgist Cyril Smith; and engineer Hartley Rowe, a United Fruit executive who had served in World War II first

74. Bethe, in AEC (ref. 7), 328.

75. Bethe to Weisskopf, 31 Oct 1949, box 9, file "Fan mail," BP.

76. Sumner T. Pike, Acting AEC Chairman, to Oppenheimer, 21 Oct 1949, in AEC (ref. 22), 22c.

77. On the chairmanship, see Rabi to DuBridge, 13 Dec 1946 and reply, 17 Dec 1946; DuBridge to Conant, 17 Dec 1946 and reply, 20 Dec 1946, file 167.1, DuBridge Papers, Robert A. Millikan Library, California Institute of Technology.

as a division chief under Conant in the National Defense Research Committee and then in the war's last year under Oppenheimer at Los Alamos. The eighth member was physicist Oliver Buckley, director of Bell Laboratories, who had joined the GAC in mid-1948.

Oppenheimer was their accepted leader and he usually crafted their reports. Conant was their most prestigious member, whose fame and power were rooted in his Harvard presidency and not in his earlier accomplishments in chemistry. Fermi was their greatest scientist: laconic, cautious, reluctant to err. Rabi was admired for his intellectual and personal shrewdness. Cyril Smith, though British-born, never seemed to suffer the fears of being an emigré as did Fermi. Rowe and Buckley, the representatives from industry, often deferred to their colleagues.[78]

On Friday, October 28, with most GAC members present, George Kennan, director of the State Department's Policy Planning Staff, discussed the Soviet situation. After he left, Bethe stressed the substantial technical problems in developing the weapon. Serber, who had become the reigning theorist at Berkeley's Radiation Laboratory, emphasized the need for Lawrence's new project of a large neutron-producing reactor, but "disassociated himself from Teller, Alvarez, and the Super. Already convinced that the Super as then conceived would never work, Serber was pleased that he did not have to discuss the subject."[79]

On Saturday morning, the committee, joined by all five AEC commissioners, heard from military representatives. The discussion turned to the Soviet threat and to whether the United States would "launch an atomic attack on [the Soviet Union] if she moved into Europe, if we knew this meant Russian bombs on London, say[.]" The military men were unsure. "A close question, they said," Lilienthal recorded—"meaning, I guessed, that we wouldn't." The prospects of a Super, he wrote in his diary, "made the eye[s of the military men] light up; but [the] chief value of such a weapon [is] 'psychological.'" Or so said General Omar Bradley, chairman of the Joint Chiefs of Staff (JCS).[80]

Apparently the military men had not reached a consensus on crucial strategic issues: Was the Super a necessary supplement, or ultimately an alternative, to fission bombs? Were there appropriate Soviet targets for the Super? Since the H-bomb would be far more powerful than A-bombs, would that added power be a valuable, or necessary, offset to compensate for bombing errors? Or was it simply, as General

78. See GAC minutes from founding through 28–30 Oct 1949; on Fermi and Smith, see Bernice Brode to Oppenheimer, 15 Apr 1954, OP.

79. Hewlett and Duncan (ref. 50), 381–82.

80. Lilienthal (ref. 54), *2*, 580–581.

Bradley had suggested, that the H-bomb would be a useful additional deterrent—it would be psychologically unacceptable for Russia to develop the weapon and for America not to have it? If there was a trade-off in deploying scarce talent and materials to pursue the Super, what cost in lost fission bombs would be acceptable?

Alvarez, who had not been invited to the GAC meeting, stationed himself at the AEC building's entrance and watched the participants come and go. During the Saturday lunch hour, he joined Oppenheimer and Serber. Until then, Alvarez had assumed that Oppenheimer would support it, and indeed, he said later, he could not understand why anyone would oppose it.[81] (Alvarez recalled that Oppenheimer had recruited him to the Manhattan Project, specifically to work on the Super.)[82] Alvarez had an uncomfortable lunch. Oppenheimer said that the United States should not build the bomb because if it did, the Soviets would; but if it did not, the Soviets would not. To Alvarez' dismay, Serber, whom Alvarez had until then considered a supporter of the Super, agreed with Oppenheimer. Deeply disappointed, Alvarez decided to return promptly to California. "I felt that the program was dead," he later explained.[83]

Lilienthal summarized part of the GAC's Saturday afternoon deliberations in his diary:[84]

> Conant flatly against it [the Super] "*on moral grounds*." Hartley Rowe, with him: "We built one Frankenstein." Obviously Oppenheimer inclined that way. Buckley sees no diff[erence] in moral question x and y times x, but Conant disagreed—there are grades of morality. Rabi completely on [the] other side. Fermi, his careful enunciation, dark eyes, thinks one must explore it and do it and that doesn't foreclose the question: should it be made use of? Rabi says decision to go ahead will be made; only question is who will be willing to join in it....Conant replies: but whether it [the decision] will stick depends on how the country views the moral issue.
>
> Conant makes firm point at outset: Can this be declassified—i.e., the fact that there is such a thing being considered, what its effect will be, if it could be made successfully, etc.? I said [the] President certainly could

81. Alvarez, in AEC (ref. 7), 785. In this testimony of 1954, Alvarez characterized Oppenheimer's attitude to the bomb before October 30 as "lukewarmness," but in an interview (with Galison) on 15 Jan 1988, Alvarez said that he believed that Oppenheimer would be a strong supporter. Alvarez did not expect Oppenheimer to oppose the venture.

82. Alvarez, interview with Galison, 15 Jan 1988.

83. Alvarez, in AEC (ref. 7), 785–786.

84. Lilienthal (ref. 54), *2*, 581; Conant's "on moral grounds" was deleted by Lilienthal in the published version, but appears in the manuscript diary, 29 Oct 1949, Lilienthal Papers, Seeley Mudd Library, Princeton University. Emphasis added.

> announce it if he wished to. (Privately, [I] doubt if he would—then the arms race fat would be in the fire. He's more likely to say: well, move along but don't say anything about it.) Cyril Smith strong for the Conant point....Conant says: "This whole discussion makes me feel I was seeing the same film, and a punk one, for the second time."

Perhaps feeling guilty about Hiroshima and Nagasaki, Conant was also deeply troubled that the H-bomb would seem *the* solution to the Soviet A-bomb and thus block his efforts for bigger military budgets and a larger American conventional buildup in Europe. He hoped that the administration would rely upon ground forces and tactical A-bombs, not the H-bomb. The Super's great power had unnerved him in late 1944 and early 1945, and he had never comfortably endorsed the GAC's earlier approval, from early 1947 to September 1949, for the quest for the weapon.[85]

On Sunday, October 30, the GAC assembled its report. It endorsed the buildup and diversification of fission weapons (including tactical A-bombs), preparation for radiological warfare, and continued work on the booster, but objected to the Super for several reasons. First, the committee criticized technical aspects of the bomb having to do with its asymmetric configuration and the impossibility of conducting meaningful experiments short of a full test. Second, the committee questioned whether the Super was a wise use of scarce resources. "If one uses the strict criteria of damage area per dollar and if one accepts the limitations on [likely bombers], it appears uncertain to us whether the super will be cheaper or more expensive than the fission bombs." Financially it was a gamble. Third, because of its vast destructive power, the hydrogen bomb was "not a weapon which can be used exclusively for the destruction of material installations of military or semi-military purposes...; [it] carries much further than the atomic bomb itself the policy of exterminating civilian populations." Such moral considerations went further than purely technical worries, as was evident from the committee's final recommendations, since it believed that there was a better than fifty percent chance of developing the weapon within five years.[86]

Six members of the General Advisory Committee—Conant, Oppenheimer, DuBridge, Rowe, Smith, and Buckley—advocated *total* renunciation of the weapon:

> Let it be clearly realized that this is a super weapon; it is in a totally different category from an atomic bomb. The reason for developing such

85. Conant, in AEC (ref. 7), 387. Hershberg (ref. 1), typescript, rejects the idea that Conant felt guilty about the A-bomb.

86. GAC Report, 30 Oct 1949, AEC Records, DOE.

> super bombs would be to have the capacity to devastate a vast area with a single bomb. Its use would involve a decision to slaughter a vast number of civilians. We are alarmed as to the possible global effects of the radioactivity generated by the explosion of a few super bombs of conceivable magnitude. If super bombs will work at all, there is no inherent limit in the destructive power that may be attained with them. Therefore, a super bomb might become a weapon of genocide.

The GAC majority was consciously repudiating the scientific imperative and warning against the pursuit of a form of new knowledge. They also argued that developing the bomb would turn world opinion against America. Moreover, the United States could respond with A-bombs were the Russians to attack; the H-bomb was not necessary for deterrence or use. But most importantly, the United States was faced with "a unique opportunity of providing by example some limitations on the totality of war and thus of limiting the fear and arousing the hopes of mankind."[87]

Fermi and Rabi, the minority on the GAC, had moved away from their Saturday pro-bomb positions and now argued for a provisional renunciation of the bomb, contingent on Soviet restraint. "It would be appropriate to invite the nations of the world to join us in a solemn pledge not to proceed in the development or construction of weapons of this category. If such a pledge were accepted even without control machinery, it appears highly probable that an advanced stage of development leading to a test by another power could be detected by available means." But, concurring with Oppenheimer and the majority, the two physicists went far beyond a technical assessment when they found that no desirable peace could issue from a war won with such weapons. Fermi and Rabi too judged the bomb to be "a weapon which in practical effect is almost one of genocide," and considered that adequate retaliation could always be meted out by atomic weapons. "It is necessarily an evil thing considered in any light."[88]

Despite the harsh moral language, the Fermi-Rabi minority was actually offering a middle way between the stark "yes" and "no" alternatives represented by Lawrence and Teller on the one side and the GAC majority on the other. In this middle way, the United States could simply agree with the Soviets on a pledge even without on-site inspection, rely upon atmospheric and seismic detection to monitor Soviets cheated. The Soviets presumably could not produce a Super without testing, and American detection was very likely to pick up the evidence of that cheating. Thus Fermi-Rabi considered their compromise to be safe.

87. Ibid.
88. Ibid.

The GAC report, with its majority and minority annexes had some serious problems. The majority had argued that *unconditional* renunciation was safe, but the minority (Fermi and Rabi) had argued for *conditional* renunciation, thus implying that if Russia developed the Super it was essential to America. Neither group drew issue sharply with the other nor adequately explained its thinking. Why was the bomb necessary to America if the Soviets developed it—for strategic, or diplomatic, or psychological reasons? Nor did the eight GAC members explain how they could have supported the slow quest for the Super since early 1947, and then, suddenly, after the Soviet A-bomb triggered alarm, could have retreated from their earlier endorsement. The GAC responses had been, in effect, to recommend no H-bomb program after Strauss, McMahon, and others, frightened by the Soviet breakthrough, demanded a "crash" program.[89]

The GAC members were men who had long lived with the nuclear arms race and, in some cases, with guilt about Hiroshima and Nagasaki; they were being asked to address issues that reached to the core of their assumptions, careers, and beliefs. In a rushed weekend, even when aided by discussions over the years about the H-bomb, they could not fully grapple with such matters. They had struggled, nearly reached consensus, and in their spirit of opposition to the bomb they could find satisfaction. They "had reached a meeting of sensibilities," according to Oppenheimer in a later interviewer's paraphrase.[90]

The day after the reports were filed, Lilienthal telephoned Conant to congratulate him on the results. "Without Conant's unswerving opposition [to the Super]," Lilienthal said in the words of the official AEC history, "the committee's report might well have favored it."[91] Oppenheimer later emphasized Conant's influence in explaining why his own position had changed between October 21 (when he thought it would be folly to oppose the Super) and October 30 (when he helped craft the majority opinion for unconditional renunciation). As Oppenheimer recalled, in an interviewer's words, "Conant said he just wouldn't have this [thermonuclear weapon], and pointed out that a firm stand could be expected to meet with the approval of various groups, churches."[92] Conant's leading role was confirmed by Bethe,

89. Bernstein, "The H-bomb decisions: Were they inevitable?" in Bernard Brodie et al, eds., *National security and international stability* (Cambridge, MA, 1983), 334–336.

90. Warner Schilling, "Interview with J. Robert Oppenheimer," 11 June 1957, OP.

91. Hewlett and Duncan (ref. 50), 385.

92. In the same interview, Oppenheimer suggested "that it was a mistake to go along [with Conant]." He recalled that when his secretary saw the 30 Oct 1949 GAC report, "she was surprised, noting that this was not the position he had taken [earlier]....She also correctly predicted that this would get him in a lot of trouble." Schilling (ref. 90).

von Neumann, DuBridge, and Rabi.[93]

Among the GAC members only Seaborg, who missed the October meetings, did not oppose the H-bomb. In mid-October he wrote Oppenheimer: "Although I deplore the prospects of our country putting a tremendous effort into this [the H-bomb], I must confess that I have been unable to come to the conclusion that we should not."[94] His weak words of support would not have changed opinions at the meeting. Seaborg later explained that "I wrote it in the way I did because I did feel that in order to persuade Oppenheimer I would have to present the argument in a conciliatory way."[95]

After the GAC report, Oppenheimer talked about matters with John Manley, GAC secretary and associate director of Los Alamos. Despite the general enthusiasm at Los Alamos to develop the Super and his own earlier support for it, Manley was sympathetic to the GAC position. Oppenheimer confided that he was worried, in Manley's paraphrase, "whether Lilienthal had enough drive, stamina and courage left to get enthused about [blocking] the super-bomb and to carry it through first with his fellow Commissioners."[96]

Lilienthal, assessing the support for the Super, complained in his diary: "Reports from Los A[lamos] and Berkeley are rather awful: the visiting firemen [members of the Joint Committee] saw a group of scientists who can only be described as drooling with the prospect and 'bloodthirsty.' E.O. (Lawrence) quite bad: there's nothing to think over."[97]

The secret debate continues

In his diary, Manley noted that McMahon held a meeting on November 1 with the AEC commissioners ("a rather violent discussion"), learned about the GAC report, and very much opposed it. McMahon was also seeking to meet with Teller, who had earlier conferred with some members of the Joint Committee and helped

93. "Both von Neumann and I remember that Dr. Conant was the first member of the GAC who was strongly opposed to the hydrogen development." Bethe to Samuel Silverman, 18 Mar 1954, box 12, file "Oppenheimer case," BP. Rabi and DuBridge also testified to Conant's leadership at the crucial October GAC meeting, Tuttle, interview with Rabi, 8 Jan 1976, and also with DuBridge, 17 Mar 1976, courtesy of Tuttle.

94. Seaborg to Oppenheimer, 14 Oct 1949, AEC Records, DOE. This letter might have been read, or shown, to some GAC members before the meeting formally started. Bernstein, interview with Cyril Smith, 1986; and Philip Stern with Harold Green, *The Oppenheimer case: Security on trial* (New York, 1969), 144.

95. William Tuttle, interview with Glenn Seaborg, 24 Mar 1976, courtesy of Tuttle.

96. John Manley, Diary, 31 Oct 1949, AEC Records, DOE.

97. Lilienthal (ref. 54), *2*, 582.

convince them of the need for the Super.[98] Teller seems to have suspected a conspiracy. He complained that a conference, scheduled for Los Alamos late in the autumn, had been called off and that there were "mysterious actions in the GAC and even higher places." "What disturbs me most," Teller wrote von Neumann, "is that apparently Enrico [Fermi] is at least temporarily convinced that the action of the GAC is reasonable. The really fine and unanimous enthusiasm which was building up in Los Alamos [for the Super] is now checked at least temporarily."[99]

On November 9, after a number of earlier discussions, the AEC met to consider its advice to the White House. The commissioners split, 3–2, the majority being in opposition to the weapon. All five agreed to send Truman a unanimous statement of general considerations including the call for public discussion of the issues. They also provided individual opinions. Like the GAC, Commissioner Henry Smyth, a physicist, argued that America's "general standing in the world would be worsened by our development of 'Supers.'" He stressed that the military advantage to the Soviets if they did develop the weapon was "doubtful," and pleaded for new negotiations for international control of atomic energy. He recognized that a negative decision on the Super might have to be reconsidered as events changed.[100]

Strauss and Smyth had a sharp exchange; Strauss posed written queries and Smyth penned his retorts on the same page. Strauss asked: "1- In the light of Russian success thus far is there a reasonable presumption that a super is within their capabilities?" Answer: "yes." "2- If the answer to 1 is yes is it reasonable to assume that they may undertake such a project?" "Yes." So, Strauss asked what for him was the clincher: "If the answers to 1 & 2 are in the affirmative, can we afford not to have such a weapon in our arsenal?" Smyth scribbled, "I don't know," and then, "If the answer to 1 & 2 is yes can we afford the international situation for which we are in part responsible[?]" To Strauss, this was, as he labeled the page, Smyth's "oversimplification."[101]

Strauss, Teller, Lawrence, and McMahon worked to reverse the tide of opinion. Rabi later regretted that the GAC members had gone home immediately after their October meeting, and he charged that

98. Manley (ref. 96), 1 Nov 1949.
99. Teller to John von Neumann, 9 Nov 1949, box 5, von Neumann Papers, LC.
100. "Views of H.D. Smyth," 9 Nov 1949, in AEC (ref. 22), 46–47; cf., Smyth to Strauss, 21 Sep 1953, SP, Herbert Hoover Library, West Branch, IA. Unless otherwise cited, all Strauss papers were in his son's possession when we used them.
101. "Oversimplification," n.d. (about 9 Nov 1949), SP.

the bomb's scientist proponents had "def[ied] the rules" by lobbying for the weapon.[102] If confronted with Rabi's charge, the pro-bomb scientists would have pointed out that Oppenheimer was also lobbying, that unlike him they did not have formal conduits to power and had to establish informal ones, that they never violated secrecy rules, and that the Super was, in their judgment, essential to America's security. Thus, their meetings with McMahon and Strauss, as well as others, seemed to them both necessary and proper.

A welcome addition to the pro-bomb forces was Karl T. Compton, chairman of the Department of Defense's Research and Development Board and former president of MIT, who wrote to the President on November 9 to urge development of the Super. Rebutting the GAC report, Compton argued that the Soviets could not be trusted, that abstinence without inspection was foolish, and that America must have the weapon, if it could be created. "Our own national security and the protection of the type of civilization we value...require us to proceed." Unlike Conant and some of the other GAC members, as well as the five AEC commissioners, Compton urged that the President's decision be kept secret and implied that there should not be any public discussion of the issues.[103]

Pressure for the bomb continued to develop. By mid-November, as Manley painfully discovered, McMahon considered the GAC report "suicidal." McMahon favored some form of ultimatum to the Soviets: the United States might attack them if they tried to develop the Super. And Robert LeBaron, chairman of the Pentagon's Military Liaison Committee (MLC), which served as a link between the AEC and the Joint Chiefs, complained to Manley that there had been a Superbomb program all along, and that now, when the issue was "a speed-up," the GAC suddenly said no. LeBaron "could not understand," in Manley's diary paraphrase, "why there was so much fuss about speeding up a program which would have been a normal course of action anyway."[104]

LeBaron admitted that military analysts had still not thought deeply about how the Super might be used and what its actual strategic benefits, if any, would be over the existing fission bombs. But he stressed that "the existence of a weapon," in Manley's paraphrase, "always brought forth new ideas as to how it could be used and that only now many thoughts as to [the H-bomb's] advantage...for tactical use in preventing troop concentration were being generated." LeBaron

102. Tuttle, interview with Rabi, 8 Jan 1976, courtesy of Tuttle.

103. K.T. Compton to Truman, 9 Nov 1949, reprinted in Lewis Strauss, *Men and decisions* (Garden City, 1962), 440.

104. Manley (ref. 96), 15 Nov 1949.

reported that one of the three air force generals on the MLC believed, as Manley summarized it, "it was perfectly clear...that the military had to have this weapon."[105]

On November 23, the Joint Chiefs finally expressed their *formal* opinion: America should proceed with the H-bomb. It would be "intolerable," they argued, if the Soviet Union developed it and the United States did not have it. The hydrogen bomb could be a useful deterrent, add flexibility to planning for war, and would be cheap enough in terms of money, materials, and industrial effort. They believed that it would be more efficient than fission weapons in the use of ore and industrial facilities, and in damage per bomb. In direct opposition to the GAC, they asserted that such considerations "decisively outweigh the possible social, psychological and moral objections" to the H-bomb.[106]

Two days later, AEC Commissioner Lewis Strauss made a similar, but stronger, case to the President. The "United States must be as completely armed as any possible enemy," Strauss declared. The Soviet Union, "a government of atheists[,] is not likely to be dissuaded from producing the weapon on 'moral' grounds." He suggested that the Soviets might already be ahead and criticized the GAC's concern about morality, pointing out that some of its members before Hiroshima—he meant Oppenheimer and Fermi—had urged pursuit of the Super. In short, war was horrible, but America had to be prepared. "Our arsenal must be not less well equipped than with the most potent weapons that our technology can devise." For Strauss, the maximal commitment to the development of the H-bomb was the highest moral obligation that the scientific-military community owed to the nation.[107]

One of the most forceful rebuttals to the GAC came from Senator McMahon, who wrote to the President about the putative distinction between atomic and fusion weapons:[108]

> There is no moral dividing line that I can see between a big explosion which causes heavy damage and many smaller explosions causing equal or still greater damage. Where is the valid ethical distinction between the several Hamburg raids that produced 135,000 fatalities, the single Tokyo "fire" raid that produced 85,000 fatalities, and the Hiroshima bomb that produced 65,000 fatalities? What, then, is the distinction

105. Manley (ref. 96). Manley's diary mentions General "Slater" but quite probably meant Major General David M. Schlatter.

106. Bradley (for Joint Chiefs of Staff) to Secretary of Defense Louis Johnson, 23 Nov 1949, in Department of State, *FRUS, 1,* 595–596.

107. Strauss to Truman, 25 Nov 1949, in *FRUS, 1,* 596–599.

108. McMahon to President Truman, 21 Nov 1949, in *FRUS, 1,* 588–595.

> between the 1,000 square miles which one super might scorch and the 1,000 square miles which 143 fission bombs might equally destroy? Is a given weapon to be adjudged moral or immoral depending upon whether it requires hours, days or weeks to take its toll?. . . .
>
> To me the notion that our possession of this weapon would harm our moral position makes no sense, provided that we offered to relinquish it in exchange for a just and enforceable system of control. Only the nation which rejected such an offer would occupy an indefensible moral position. Any idea that American renunciation of the super would inspire hope in the world or that "disarmament by example" would earn us respect is so suggestive of an appeasement psychology and so at variance with the bitter lessons learned before, during, and after two recent wars that I will comment no further.

Early in December the GAC met again and reiterated its position. In an attached memorandum by John Manley that carried the endorsement of the GAC majority, the committee called for "all effort" to eliminate atomic bombs. "We see no compelling reason [or] military necessity to make the achievement of this goal more difficult by undertaking to develop super bombs." Furthermore, the Manley memo minimized the military gains the bomb would afford, argued that American development would inevitably assist Soviet efforts towards the weapon, and contended that pressing the Super would hamper expansion of the A-bomb arsenal.[109]

GAC member Hartley Rowe wrote Oppenheimer in the same vein pointing to the absence of military advantage, the false security the bomb would provide, and the damage emphasis on thermonuclear weapons might do to conventional defense. Moreover, he said, "A democracy, of the type in which I firmly believe, cannot, in my opinion, be strengthened by the possession of a super-bomb."[110] Fermi, reached a similar position by another route: on purely technical grounds he favored the atomic bomb over the hydrogen bomb except for the large-scale destruction of heavy structures. Strategically, he argued, the atomic bomb "could be more selectively and flexibly employed," and American possession of the hydrogen bomb would not significantly increase our ability to inflict damage on the enemy.[111]

Lee DuBridge agreed: adding the superbomb to the nuclear arsenal would not augment "damaging power" as rapidly as one might think. The super was to deliver 1000 times the energy or (since the area of destruction goes as the two-thirds power of energy) the hydrogen

109. GAC Minutes, 2–3 Dec 1949, with attached statement by Manley, cited in AEC (ref. 22), 60–63.

110. Hartley Rowe to Oppenheimer, 3 Dec 1949, quoted in AEC (ref. 22), 63–64.

111. Fermi to Oppenheimer, 3 Dec 1949, quoted in AEC (ref. 22), 64–65.

bomb would destroy an area 100 times that of an atomic bomb. For any realistic target, thirty atomic bombs could probably do the job just as well. By DuBridge's reckoning, each Super would need the fissionable material of four fission bombs, along with tritium that would absorb reactor time otherwise capable of producing four more A-bombs. This meant that the Super would "cost" eight A-bombs. Given that there would be further costs in time and effort, his estimate was that a Super was only two to three times cheaper than the fission weapon per unit of area destroyed.[112]

Conceding that the delivery of one Superbomb would be easier than that of thirty atomic bombs, DuBridge nonetheless insisted that the escort force would need to be about the same for the two cases. Thus for reasons of resource allocation and delivery, the Super would not represent a substantial improvement over the atomic bomb. But what of delivery by ship? Here DuBridge felt that the advantage would not be America's since the Soviet Union had far fewer important port cities. Finally, DuBridge reiterated the GAC's basic moral point: "The superbomb, to a far greater extent than the fission bomb, is no longer in a class of a 'military' weapon....It is solely a weapon for annihilating large cities." This consideration led the Caltech president directly to questions of psychology, diplomacy, and ethics.[113]

Arguing on psychological grounds, DuBridge observed that the threat of the A-bomb did not bring the Soviets to accede to American demands. Nor did the United States suddenly lose its "psychological balance" after Joe 1. "Just what is the evidence that this increment in our destructive potential would grossly alter this situation? It might only add ammunition to the propaganda campaign being waged against 'U.S. imperialism' and 'war mongering.' On the psychological front this is a weapon which might easily backfire."[114] Diplomacy self-evidently had to be supported by military strength. "But strength also involves a combination of psychological and moral factors. Are our national objectives clear and justified? Are we offering hope to the world's people or something less?" An offer to renounce the hydrogen bomb, according to DuBridge, "might offer a hope. Even a slim hope would be worth some cost." To the obvious rejoinder that, since the country (and DuBridge himself) was already committed to the deployment of A-bombs, there was no philosophical stopping place, DuBridge responded:[115]

112. DuBridge to Lilienthal, 5 Dec 1949, group 184, box 8, file "hydrogen bomb," Records of the Office of the Secretary of Defense, RG 330, NA.

113. Ibid.

114. Ibid.

115. Ibid.

> One need not argue that an A-bomb is moral and a super is immoral. But whatever moral position we have come to occupy by virtue of our present program can only be worsened by making a great forward step in the production of weapons of mass destruction—weapons of terror. If our moral position is already bad why not make it better rather than worse? If it is good why not improve it?

December 1949 was the high-water mark for opposition to the Super. The GAC had stated and forcefully reaffirmed its opposition to the weapon; the bomb was in trouble technically; the chairman and two other members of the Atomic Energy Commission objected to it; and foes of the bomb included such major figures of American nuclear physics as Bethe, Weisskopf, Fermi, Rabi, and Oppenheimer. They were joined by two powerful university presidents and science advisors, Conant and DuBridge. Plans for the Super seemed doomed on every front.

Arguments for the Super

Up through mid-November 1949, the deliberations about the bomb were kept secret under AEC rules. Szilard vehemently opposed the bomb; but like Karl Compton and Hans Bethe, Szilard did not want a public discussion.[116] But whereas Compton worried that airing the matter would alert the Soviets to the American project and Bethe feared that a public controversy would push the Soviets into a race, Szilard believed that the American public was not prepared to exercise informed judgment. He wanted to delay a public discussion until the American government had reappraised Soviet-American relations and made a generous peace offer. In that context he hoped that the American people would prefer a great power settlement to the arms race. Szilard's typically idiosyncratic analysis won virtually no support among scientists.[117]

Secrecy was punctured on November 18, 1949 by a front-page story in the *Washington Post* inspired by a senator's accidental mention of the Super on a television program. In response, Truman formally imposed an order barring all government employees, as well as advising scientists, from speaking publicly on the subject. He wanted to make his decision on the Super without a public discussion and for building the bomb.[118]

116. Bethe, "The hydrogen bomb," *BAS, 6* (Apr 1950), 99–104, and "The hydrogen bomb: II," *Scientific American, 182* (Apr 1950), 21.

117. Szilard, draft of 9 Nov 1949, in Helen Hawkins et al., eds., *Toward a livable world: Leo Szilard and the crusade for nuclear arms control* (Cambridge, 1987), 76–78.

118. Truman to Sidney Souers, NSC Executive Secretary, 19 Nov 1949, *FRUS, 1* 587–588; Hewlett and Duncan (ref. 50), 394.

Truman appointed a three-man National Security Council (NSC) committee to advise a course of action. It was composed of AEC chairman David Lilienthal, who had already made clear his opposition to the bomb; Secretary of Defense Louis Johnson, who was inclined to follow the Joint Chiefs in supporting it; and Secretary of State Dean Acheson, who constituted the swing vote and was likely to endorse the bomb. It was primarily through Acheson, whom Truman greatly trusted, that foreign policy considerations entered most directly into the hydrogen bomb decision. Acheson had much to push him towards a recommendation for the new weapon. He believed that additional military power would enhance diplomacy, he was already under attack for "losing" China, and he might have been unwilling to subject himself to domestic attack for leaving America weak.[119]

Acting like a skilled attorney canvassing the important arguments, Acheson met privately with Oppenheimer, who had been a friend since their work together with Lilienthal in 1946 on an American plan for international control of atomic energy. After their session on the Super in Autumn 1949, Acheson had rejected Oppenheimer's advice, saying, in an associate's recollection, "I don't understand [him]. How can you persuade a paranoid adversary [the Soviet Union] to disarm 'by example'?"[120] Because of that disappointing conversation, Oppenheimer apparently despaired of halting the effort for the Super, and gave up his earlier plan of appealing directly to the President to oppose the weapon. With Acheson unreceptive to the GAC position, Oppenheimer expected that Truman would push ahead with the H-bomb.[121]

Acheson also asked George Kennan, departing head of the State Department's Policy Planning Staff, and Paul Nitze, his successor, for advice. Kennan, in turn, consulted with Oppenheimer, who found him a kindred spirit against the Super.[122] For Kennan the new weapon raised basic strategic and moral questions about America's implicit first-use policy for nuclear weapons.[123] Nitze pushed for the bomb. For Nitze, as for Acheson, both greater conventional military strength and an enhanced nuclear arsenal were desirable, and the H-bomb was

119. Bernstein (ref. 79), 338–346.

120. R. Gordon Arneson, "The H-bomb decision," *Foreign service journal, 46* (May 1969), 29.

121. Schilling (ref. 90).

122. Oppenheimer to George Kennan, 17 Nov 1949, OP.

123. Kennan, draft to Secretary of State, 18 Nov 1949, Atomic Energy lot files, Department of State Records, Department of State. This memo, obtained under the Freedom of Information Act from the State Department before the records were transferred to the National Archives, may now be in the Department of State Records, RG 59, NA.

the greatest of weapons.[124]

When the NSC committee met for the first time on December 22, 1949, Lilienthal was impressed with Acheson's openness, but not Defense Secretary Johnson's rigid pro-bomb position.[125] Partly because Acheson and Johnson mistrusted each other on other grounds, the committee did not again meet for over a month, while Truman himself was also moving toward a commitment to seek the H-bomb.[126]

Despite Truman's secrecy order, additional pieces of the H-bomb dispute appeared in the press, igniting a public debate from which members of the AEC, other members of the executive branch, and advising scientists were barred. When in early January 1950 the Alsop brothers wrote about secret deliberations on the H-bomb, Truman exploded. "I don't know where the "'Sop Sisters' got their information," he told Senator McMahon, "but evidently somebody thinks it is proper to talk to such lying scoundrels." The AEC recommended that the President order an FBI investigation, and McMahon charged the Commission with leaks.[127] None at the time blamed any of the advising scientists, who seem to have abided by the President's secrecy order.[128]

Pressure from the Joint Chiefs, the Department of Defense, and the Joint Committee was building up for Truman to endorse the Superbomb. Senator McMahon, complaining again of AEC leaks, privately informed Truman that at least one Democratic committee member threatened, unless the president endorsed the project, to "sound off on the floor" of Congress. Annoyed by such threats, Truman responded: "the very best plan for you and me to pursue is silence on the subject and to carry out the business in the best interest of the United States."[129]

On January 13, 1950, the Joint Chiefs of Staff, urged Truman to endorse the H-bomb on military, diplomatic, psychological, and moral grounds. Building on their briefer analysis of November 23, 1949, the Chiefs argued that the bomb would increase national security "as a

124. Nitze, draft, 19 Dec 1949, Nitze Files, Policy Planning Staff Papers, Department of State Records, NA.

125. Lilienthal (ref. 54), *2,* 613–614.

126. Acheson, memorandum to file, 19 Jan 1950, Acheson Papers, Truman Library; printed in *FRUS* (1950), *1,* 511–512.

127. Truman to McMahon, 5 Jan 1950, PSF; AEC Minutes, 5 Jan 1950, AEC Records, DOE; McMahon to Truman, 3 Jan 1950, PSF.

128. In April 1950, however, Strauss did privately charge Conant with leaking information. Strauss to McMahon, 17 Apr 1950, SP.

129. McMahon to Truman, 18 Jan 1950, and Truman to McMahon, 19 Jan 1950, PSF.

potential offensive weapon, a possible deterrent to war, a potential retaliatory weapon, as well as a defensive weapon against enemy forces." The estimated expense of $100–$200 million for developing a single weapon to determine feasibility was well within America's capacity and would not interfere substantially with other military programs. The Chiefs then moved to rebut the GAC's charge that the weapon would be used primarily against noncombatants and therefore was genocidal. "[We] do not intend to destroy large cities per se; rather, only to attack such targets as are necessary in war in order to impose the national objectives of the United States upon an enemy." America's possession of the weapon would greatly enhance its diplomatic power, and make allies and the American people more comfortable; for the Soviets to develop it and for the United States not to have it would be "intolerable." "In war it is folly to argue whether one weapon is more immoral than another. For, in the larger sense, it is war itself which is immoral, and the stigma of such immorality must rest upon the nation which initiates hostilities."[130]

To Truman the JCS report "made a lot of sense." Acheson thought similarly.[131] Pressure mounted for a pro-bomb decision. Former atomic energy advisor Bernard Baruch, Democratic Senator Tom Connally, chairman of the Foreign Relations Committee, and Republican Senator Kenneth Wherry, among others, all publicly pressed Truman to approve the Super.[132] Some influential scientists, most notably Arthur Compton and Harold Urey, who were not government advisors, also entered the public debate in favor of the bomb. Opposing the policy of secrecy, Arthur Compton called for a full public discussion.[133] Urey, delighted that American citizens had been informed of the possibility of an H-bomb, warned that the Soviets, if they alone had the weapon, would probably deliver an ultimatum: surrender or else. "Judging from our past decisions," Urey complained, "we have apparently decided to lose the armaments race."[134] Lilienthal in his diary correctly identified Urey's speech with the "'Lawrence-Strauss' line: if we don't get this super first, we are sunk." The speech will "stir

130. Bradley to Secretary of Defense, "Request for comments on military views of members of General Advisory Committee," 13 Jan 1950. Word was speedily transmitted to Truman; *FRUS* (1950), *1*, 503–511.

131. Acheson, memorandum to file, 19 Jan 1950, Acheson Papers, Truman Library; printed in *FRUS* (1950), *1*, 511–512.

132. *New York Times*, 28 Jan 1950, 1 (Baruch); 29 Jan 1950, 1 (Connally): 23 Jan 1950, 3 (Wherry); and *Philadelphia inquirer*, 31 Jan 1950, 1 (Tydings).

133. Compton, "Let the people decide!" *BAS*, *6* (Mar 1950), 74–75, reprints his public statement of late January 1950.

134. Urey, "Should America build the H-bomb?" *BAS*, *6* (Mar 1950), 72–73, reprints his public statement of late January 1950.

up the animals."[135]

Behind the scenes, the ranks of important officials opposing the H-bomb were thinning. On January 27, at a secret meeting with the Joint Committee, AEC Commissioner Smyth implied that he might now support it. Commissioner Sumner Pike, who had also opposed the weapon in November, told the same committee, in the words of a colleague, that he (Pike) "did not know what his views were."[136] These defections meant that only Lilienthal of the five AEC commissioners, eight of the nine GAC members, and many well-known scientists were urging Truman not to seek the H-bomb. It was a politically weak coalition facing the Joint Chiefs, the Military Liaison Committee, Secretary Johnson and the Defense Department, at least two and possibly four AEC commissioners, Senator McMahon and most of the Joint Committee, some congressional leaders, a number of prominent scientists (mostly from Berkeley, Chicago, and Princeton), and the bulk of physicists at Los Alamos.

The special NSC committee held its second meeting on Monday, January 31, 1950. With a reluctant Lilienthal, they unanimously recommended that the President proceed with the $100–$200 million Superbomb project, at least to the stage of a test. Acheson had wanted an explicit recommendation that the President defer all decisions on actual production of H-bombs but yielded to Johnson's objections and removed that provision. Secretary Johnson proposed that they deliver their report during his scheduled 12:30 appointment with Truman. According to Lilienthal, Johnson felt that "the heat was on in the Congress and every hour counted in getting this matter disposed of." At a seven-minute session that day, the President approved the recommendation.[137] Truman's decision, in view of his own inclinations, his concern about public and congressional opinion, and his respect for Acheson's analysis, was virtually inevitable.

Later in the day Truman issued his statement to the nation:[138]

> It is part of my responsibility as Commander in Chief of the Armed Forces to see to it that our country is able to defend itself against any

135. Lilienthal (ref. 54), *2,* 622; and Lilienthal, diary, 28 Jan 1950, Lilienthal Papers.

136. Dean, "Sequence of events leading to the decision on the 'super' bomb," n.d. (after 27 Jan 1950 but before the Truman decision of 31 Jan), AEC Records, DOE.

137. Lilienthal (ref. 54), *2,* 623–632; Special Committee of the NSC, Report, 31 Jan 1950, App. C, *FRUS* (1950), *1,* 518.

138. Truman, statement of 31 Jan 1950, *Public papers of the presidents of the United States: Harry S. Truman, 1950* (Washington, 1965), 138. The reference to continuing was inserted upon Lilienthal's recommendation to make clear that the AEC was not starting H-bomb work, and Lilienthal *may* have offered this revision to protect the agency against charges that it, and its scientists, had been inattentive to military strength. Lilienthal (ref. 54), *2,* 625.

> possible aggressor. Accordingly I have directed the Atomic Energy Commission to continue its work on all forms of atomic weapons, including the so-called hydrogen or super bomb. Like all other work in the field of atomic weapons, it is being and will be carried forward on a basis consistent with the overall objectives of our program for peace and security.

The announcement was so popular that many in the House of Representatives greeted it with cheers. A public opinion poll, stretching slightly before and after Truman's pronouncement, indicated great support (73 versus 18 percent). Significantly, the same poll also revealed that about half the respondents preferred to try to get a Soviet-American agreement before making a decision on pursuing development of the H-bomb.[139]

Truman's public statement did not mention that he was also calling for a high-level strategic review, which culminated in April in National Security Council document 68. His public statement intentionally failed to mention his order that the hydrogen-bomb program should remain enshrouded in secrecy. "I hereby direct that no further official information be made public on it without my approval," he informed Lilienthal.[140] Accordingly, the AEC soon issued an order barring all employees, including consulting scientists, from discussing even the non-classified, technical aspects of thermonuclear weapons.[141]

Reactions against Truman's decision

When Lilienthal took the news to the GAC on the afternoon of January 31, the meeting was, he noted, "like a funeral party—especially when I said we were all gagged." It was a double defeat for the GAC—on the H-bomb itself and on continued secrecy. Some asked whether they should resign, and Lilienthal urged them not to do so. "This would be very bad," he wrote in his diary, "though before long a number of them may, just because they feel their standing is impaired." Oppenheimer required special encouragement to stay on the committee.[142]

Many of the bomb's opponents rallied to the thesis that secrecy had destroyed their position. Former AEC commissioner Robert Bacher wrote to Oppenheimer that "The amount of speculation and real misinformation on the subject as a whole is most discouraging,

139. Polls of 28 Jan-2 Feb 1950, in *The Gallup poll: Public opinion, 1935–1971* (New York, 1972), *2,* 888.

140. Truman to Lilienthal, 31 Jan 1950, AEC Records, DOE.

141. Directives of AEC, 11 Mar 1950 and 17 Mar 1950, reprinted in *BAS, 6* (May 1950), 132. This order was later modified.

142. Lilienthal (ref. 54), *2,* 633; Oppenheimer, in AEC (ref. 7), 83.

and this seems to preclude any sensible discussion until this situation is corrected." Bacher implied that the decision might have gone the opposite way if there had been an open discussion of the issues. Conant and the rest of the GAC took the same position.[143]

Oppenheimer told a nationwide television audience that the major issues about arms policy "are complex technical things, but they touch the very basis of our morality." The nation had to proceed on the basis of open dialogue. "It is a grave danger for us that these decisions are taken on the basis of facts held secret...wisdom itself cannot flourish and even the truth not be established, without the give and take of debate and criticism. The facts, the relevant facts, are of little use to an enemy, yet they are fundamental to an understanding of the issues of policy."[144]

Szilard remained true to his peculiar position that it would have been unfair to confront the American people "with the question: 'Shall we or shall we not build hydrogen bombs?' for the large mass of the people...cannot be expected to give the right answer to the wrong question." The right question related to prospects for an overall Soviet-American settlement.[145] In a burst of black humor, Szilard drafted but never published a letter, ostensibly from inmates in a lunatic asylum, to dramatize the insanity of the H-bomb. "We are a group of people with nothing to do but think, day after day, year after year and we therefore think we can help you," his letter opened. It ended with, "We *got* to show him [God] that He cannot get away with [domination] any longer[;] we got to show him who the master is, and let[']s not stop until we show him that we can blow up what he created. On to the global bomb!....Hit them! Beat them! Mash them up! Smash them up! Freedom!!! HI! Hi! Hi!"[146] In despair Szilard publicly proposed plans for moving millions of Americans out of large cities, the likely targets for enemy nuclear weapons in a war.[147]

Lilienthal, having retired from the AEC, lashed out publicly, deploring what he called Szilard's "cult of doom." "This [relocation]," Lilienthal argued, "can't be done and every one knows it can't be done, so why scare the daylights out of every one?" Szilard replied that the recognition of the possibility of mass death was essential to

143. Bacher to Oppenheimer, 8 Feb 1950, OP.

144. Oppenheimer, in transcript of "Mrs. Franklin D. Roosevelt's simulcast on Sunday, [12 Feb] 1950, 4:00 PM EST, over NBC network," Lilienthal Papers.

145. Szilard, draft, 1 Feb 1950, in Hawkins et al. (ref. 117), 79; and Pauling to Szilard, 1 Feb 1950, SzP.

146. Szilard, unpub. draft, "We got to go on," 6 Mar 1950, SzP.

147. University of Chicago roundtable discussion, 26 Feb 1950, in SzP, and reprinted as "The facts about the hydrogen bomb" in *BAS, 6* (Apr 1950), 107–109.

changing policy. He was not, he said, one of the "oracles of annihilation," as Lilienthal had charged, but a man who acknowledged, and wished to warn his fellow citizens of, the possible death of millions.[148]

Expressing similar concern at the end of the meeting of the American Physical Society in New York on February 3, 1950, twelve prominent physicists, including S.K. Allison, Kenneth Bainbridge, Hans Bethe, C.C. Lauritsen, G.B. Pegram, F. Seitz, M.A. Tuve, and V.F. Weisskopf, lamented the movement toward the H-bomb and pleaded for a pledge against American first use of this weapon. "To create such an ever-present peril for all nations in the world is against the vital interests of both Russia and the United States. This bomb is no longer a weapon of war but a means of extermination of whole populations. Its use would be a betrayal of all standards of morality and of Christian civilization itself."[149]

Fear of such a genocidal war brought Albert Einstein together with Lilienthal, Bethe, and Oppenheimer on a television program conducted by Eleanor Roosevelt on February 12. By then Einstein, with his outspoken advocacy of a world government, had set himself far from the mainstream of American political life. In his view the powerful governments of the world had to establish a supranational judicial and executive agency "with power to settle questions of immediate concern to the security of nations."[150] From Einstein's viewpoint all lesser measures, such as temporary bans on development of individual weapons systems seemed thoroughly inadequate. The problem, Einstein contended, lay not in any single weapon system but in the Cold War itself. "The belief that it is possible to achieve security through armaments on a national scale is, in the present state of military technology, a disastrous illusion. In the United States, this illusion has been strengthened by the fact that this country was the first to succeed in producing an atomic bomb." Again and again since 1945, Einstein had argued that technological measures were taken when political ones were needed. As a result, "the arms race between the United States and the Soviet Union, initiated originally as a preventive measure, assumes hysterical proportions. On both sides, means of mass destruction are being perfected with feverish haste and behind walls of secrecy. And now the public has been advised that the production of the hydrogen bomb is the new goal which will probably be accomplished....The weird aspect of this development lies in its

148. Lilienthal, in *New York Herald Tribune* (about 1 Mar 1950), and Szilard reply, ibid., 4 Mar 1950, both reprinted in *BAS, 6* (Mar 1950), 109, 126–129.

149. S.K. Allison, K.T. Bainbridge, et al., "Let us pledge not to use H-bomb first!" *BAS, 6* (Mar 1950), 75.

150. Otto Nathan and Heinz Norden, eds., *Einstein on peace* (New York, 1968), 522.

apparently inexorable character. Each step appears as the inevitable consequence of the one that went before. And at the end, looming ever clearer, lies general annihilation."[151]

Bethe reiterated his judgment that the atomic bomb "still could be applied to military targets" while "the hydrogen bomb can only mean a wholesale destruction of civilian populations. We dislike the Russian system because of the means...it uses. It has a dictatorship; it suppresses human liberties; it disregards human dignity and human life. We believe in these values. Shall we defend these values by obliterating all Russian cities and their populations?" In Bethe's opinion there was only one reason for developing hydrogen bombs: the Russians might perfect one before the Americans and force the United States to surrender under threat of their use. At a minimum, Bethe argued, America should unilaterally proclaim that "we will never be the first to use hydrogen bombs."[152] Truman watched the TV show. Lilienthal summarized the President's comments: "about these scientists, he said, such as Einstein and the others on the show, we need men with great intellects, need their ideas. But we need to balance them with other kinds of people too."[153]

Two days after the television broadcast, Bethe wrote Weisskopf that opposition efforts were going well, "I had a long talk with Oppie who agreed very much with what we had done and were doing. He emphasized the necessity of keeping the issue [of the hydrogen bomb] alive and I very much agree with him. Can you help?" Bethe would write an article for *Scientific American* and try to place one in *Atlantic Monthly*. Weisskopf concurred with Bethe's analysis, "Thanks to you for the ends-means argument!!," and encouraged Bethe's efforts to clarify the questions: "I read your article about the H-bomb and I like it *very* much!...I know how much work, time, nerves, mental stability, etc., such an effort costs and we must be all very grateful to you that you did it. (This sounds sentimental but it is true.)"[154] Bethe had cast most of his analysis in a moral framework: "I believe that we would lose far more than our lives in a war fought with hydrogen bombs, that we would in fact lose all our liberties and human values at the same time, and so thoroughly that we would not recover them for an unforeseeably long time."[155]

151. Ibid., 519–522.
152. Transcript of simulcast (ref. 144).
153. Lilienthal diary, 14 Feb 1950, LP.
154. Bethe to Weisskopf, 14 Feb 1950, and response, 9 Mar 1950, box 9, file "Fan mail 1938–1950," BP.
155. Bethe (ref. 116), Manley (ref. 102) and Conant (ref. 21), respectively.

Like Einstein, Linus Pauling had never worked on weapons and now publicly called for a program to prevent war itself. He wanted to overturn Truman's decision. He proposed congressional hearings on the H-bomb and congressional appropriations to subsidize a research program on the causes and prevention of war. Stressing what he saw as the ultimate danger, Pauling asserted that the H-bomb "would kill...all. This problem of an atomic war must not be confused by minor problems such as communism versus capitalism."[156] Privately, he wrote to Szilard that he was "especially disturbed by the statement that Urey issued—that the solution of the problem that faces the world now is for the United States and other western nations to make hydrogen bombs, and become so strong that they can rule the whole world, forcing the eastern nations into submission." To move the international dialogue toward peace, Pauling suggested to Szilard that they should put together a set of questions, to be addressed publicly to Stalin and Truman, and signed by Einstein and a few others along with Pauling and Szilard.[157]

The vigor of the reaction against Truman's decision from leaders of the scientific community may have pushed Urey, the discoverer of deuterium, to step back from his earlier identification with the pro-H-bomb position. He told an associate, "I made one statement on this subject and since then I have tried to avoid it as much as I could. I do not want to be labeled the father of the hydrogen bomb and the best way I can think of to avoid this is to keep off the subject."[158]

But even as opposition to Truman's decision was gathering force quite another matter came before the public to consolidate the drive for the H-bomb.

The Fuchs case and the H-bomb

The day after Truman announced the "continuation" of the H-bomb project, he learned of the major atomic espionage by physicist Klaus Fuchs. A German emigré who had become a naturalized British citizen and worked at Los Alamos as a member of the British scientific group from 1944 to 1946, Fuchs had access to what American officials feared was crucial information about the Super.[159] Strauss

156. Pauling in *New York Times,* 14 Feb 1950, 16.

157. Pauling to Szilard, 1 Feb 1950, SzP.

158. Urey to Michael Amrine, 5 Apr 1950, UP.

159. J. Edgar Hoover to Tolson, Ladd, and Nichols, 1 Feb 1950, Doc 65-58805-586, Julius Rosenberg (Fuchs) Files, FBI Records; H. York, *The advisors: Oppenheimer, Teller and the Superbomb* (San Francisco, 1976), 69, claims that Acheson, Johnson, and Lilienthal knew about the Fuchs case on January 31, before Truman announced his decision. There is no contemporary evidence to support this claim and considerable contemporary evidence against it.

told FBI Director J. Edgar Hoover that the Fuchs case would (in Hoover's words) "very much reinforce the hands of the President on the strength of [his H-bomb] decision [and] it will make a good many men who are in the same profession as Fuchs very careful of what they say publicly."[160]

"The roof fell in today," Lilienthal wrote in his diary upon learning from Strauss of Fuchs' espionage. "As the President is reported to have said to Admiral Souers, 'tie on your hat' [when the news reaches the public]. It is a world catastrophe, and a sad day for the human race."[161] Meeting that day, the five AEC commissioners decided that it "appeared undesirable" to make a connection between Fuchs and the H-bomb, and thus the AEC's official news release of February 3 totally avoided the Super while mentioning that Fuchs had worked at Los Alamos.[162] But the press quickly made the connection and some papers blared that Fuchs had given away America's H-bomb secrets.[163] Senator Millard Tydings, Democratic chairman of the Senate Armed Services Committee, declared publicly that Fuchs would save the Soviet H-bomb program "perhaps a year or more" in developing the weapon.[164]

Oppenheimer doubted that Fuchs' espionage would help any Soviet H-bomb project. Unlike others in the government such as McMahon, Strauss, and Secretary Johnson, all of whom speculated on the basis of "worst case" assumptions, Oppenheimer believed that even though Fuchs had attended the April 1946 Los Alamos conference on the Super, he would have gained little useful information since virtually none was available. At a joint Defense-State meeting on February 27, 1950, Oppenheimer said that "if they [the Soviets] had been able to make any advances on the basis of information given them by Dr. Fuchs they were marvelous indeed."[165]

160. Hoover to Tolson, Ladd, and Nichols, 2 Feb 1950, Doc 65–58805–587, Rosenberg (Fuchs) Files, FBI Records.

161. Lilienthal (ref. 54), 634; Lilienthal, diary, 2 Feb 1950, LP. Single quotation marks added.

162. AEC minutes, 3 Feb 1950; and AEC press release, n.d. (3 Feb 1950), both in AEC Records, DOE.

163. *New York Times,* 4 Feb 1950, 1; New York, *Mirror,* 7 Feb 1950; New York, *Daily News,* 4 Feb 1950; *New York journal American,* 5 Feb 1950; all in Rosenberg (Fuchs) Files, FBI Records.

164. Tydings, quoted in Robert C. Williams, *Klaus Fuchs, atom spy* (Cambridge, 1987), 153.

165. State-Defense Policy Review Group, Minutes, 27 Feb 1950, in *FRUS* (1950), *1,* 168–175, on 173.

Despite Oppenheimer's analysis, fears that Fuchs had given away something vital about the H-bomb helped push Truman to agree secretly on March 10 to accelerate the thermonuclear program by endorsing preparations for production—without waiting for scientists to establish whether they could build a Super. Technically, Truman's decision of January 31 had deferred judgment on the new rate of expenditures for the H-bomb program. The Fuchs case pushed his hand and other factors, including a new bleak strategic assessment of *atomic* bombing,[166] forced it. By his secret order of March 10, he virtually committed America to mass production of the weapon if it could be created.

And just at that time public opinion shifted away from the H-bomb. In March 1950, an opinion poll showed 68 percent for, and 23 percent against seeking international control before proceeding; at the end of January, opinion had been almost evenly divided (48 for the bomb versus 45 percent against). At neither time, however, did Americans have much hope of achieving a Soviet-American agreement, and even in March over three-fifths believed the effort would fail.[167]

The Fuchs case did not alter the positions of Oppenheimer, Conant, Bethe, and others who had opposed the decision for the Super.[168] But it added to the worries of the Joint Committee and defense officials that America was in a desperate race with the Soviets for thermonuclear weapons. And it helped to recruit scientists, or so Smyth and Strauss anticipated. Strauss planned to show parts of Fuchs' confession on the H-bomb to Bethe and believed (in Hoover's words) "this would straighten Bethe out and in turn would have a salutary effect on the others [physicists]."[169]

166. Ad Hoc Committee to the JCS, "Evaluation of effect on Soviet war effort resulting from the Strategic Air Offensive," 11 May 1949, Naval Archives, Naval History Center (Washington, DC); and Weapons Systems Evaluation Group to Joint Chiefs of Staff, "Report on evaluation of effectiveness of Strategic Air Operations," 10 Feb 1950, JCS 1952/11, in 384.5 Russia (25 Oct 48) TS file, AF/OPD RG 341, NA. These reports are treated in greater depth by David Rosenberg, "American atomic strategy and the H-bomb decision," *Journal of American history* (June 1979), who interprets them as *central* to Truman's decision-making. Also see Bernstein, review of Blumberg and Owens, *Energy and conflict: The life and times of Edward Teller* (New York, 1976) in *BAS, 34* (May 1978), 52–53.

167. Gallup (ref. 139), *2,* 888, for the results of the first poll, 28 Jan-2 Feb 1950; and *Public opinion quarterly, 14* (Summer 1950), 372, for the second.

168. On Conant, State-Defense Policy Review Group, minutes, 2 Mar 1950, *FRUS* (1950), *1,* 176–182 (esp. 182); on Bethe, Bethe to Bernstein, 18 Jul 1984.

169. Hoover to Tolson, Ladd, Nichols, 10 Mar 1950, Doc 65–58805–611, Rosenberg (Fuchs) case, FBI Records.

3. BUILDING THE HYDROGEN BOMB

Recall to the laboratories

Teller seized on Truman's decision and Fuch's confession to rally his fellow scientists to return to the laboratory. "Our scientific community has been out on a honeymoon with mesons. The holiday is over." The crisis for him was as great as in 1939, when he and Szilard had tried to spur the American government to embark upon the A-bomb project in a dread race with Hitler. Now, the foe was Stalin:[170]

> The scientist is not responsible for the laws of nature. It is his job to find out how these laws operate. It is the scientist's job to find the ways in which these laws can serve the human will. However, it is *not* the scientist's job to determine whether a hydrogen bomb should be constructed, whether it should be used, or how it it should be used. This responsibility rests with the American people and with their chosen representatives.

Thus Teller's clarion call: "Back to the [weapons] Laboratories."

The call was directed perhaps first of all to Bethe, who, however, stuck to what he told Los Alamos director Norris Bradbury in February 1950: "It is morally wrong and unwise for our national security to develop this [hydrogen] weapon. In the future I will completely refrain from any discussions related to the super-bomb." If war broke out, he stated, that decision might change.[171] Teller also called at the Institute for Advanced Study, where he urged the young theorists to take the advice of the pro-bomb von Neumann, and also of Oppenheimer, who, Teller hoped, would assist the recruiting effort. Teller wanted Oppenheimer and von Neumann to explain "just how urgent the situation is and just how badly we need more physicists to help us."[172]

Bradbury also understood the importance of securing the good will of Oppenheimer. "One of the clearest difficulties...is the serious division of point of view of physicists on this matter and the absence of enthusiasm in men like Oppie, Bethe, Weisskopf and others whom they influence. When these things are coupled with General Public ignorance even in scientific circles the problem is far from easy."[173]

170. Teller, "Back to the laboratories," *BAS, 6* (Mar 1950), 71–72.

171. Bethe to Bradbury, 14 Feb 1950, Los Alamos Records.

172. Teller to Robert Karplus, to J.M. Luttinger, and to Oppenheimer, all 17 Feb 1950, OP.

173. N.E. Bradbury to Brig. Gen. James McCormack, typescript of teletype sent 27 Mar 1950, 12:22 pm, Smyth Papers, Box: "Coh-Los Alamos;" files "Los Alamos Sci Lab 1949–1954," American Philosophical Society Libary.

The situation as Bradbury confronted it at the end of March appears from a cable he then sent to Brigadier General James McCormack, an AEC official:[174]

> Believe strongest thing which could be done [to obtain theoreticians] would be to find some way to inform scientists generally of the real state of affairs in the H-bomb field and of the need for scientific rather than engineering assistance at this time. We recognize high level difficulties in this but believe that combined efforts of GAC and AEC might convince appropriate authorities that an authoritative AEC statement on these matters would do much more long range good than harm and would probably tell Uncle Joe very little that he does not now know or suspect.

Universities and laboratories were reluctant to release their best physicists. Bradbury had already pushed Smyth to get "very very high level encouragement for Wheeler and Teller."[175] "Very very high" meant Truman himself.

Bradbury's toughest task was to get the GAC to help him. Frustrated by the GAC's reluctance to back the project, the Los Alamos leader let General McCormack know that "we [at Los Alamos] feel that the GAC by virtue of having accepted the responsibility of their position have thereby a responsibility not to be neutral or apathetic to the Commission's Programs even in spite of what their personal feel[ing]s may be." In particular, Los Alamos wanted Kenneth Case, Karplus, and two others at the Institute for Advanced Studies. "Oppie," Bradbury noted, "has so far been 'neutral.' Could he be persuaded to do more than be neutral? Wheeler will be in Princeton early in April 1950. Could he and Oppie jointly interview these people and get some real push behind the problem?" A similar situation reigned at Harvard: Julian Schwinger "won't come as things stand now," but might if President "Conant indicated that it was appropriate to consider it favorably."[176] And other important theorists resisted the call.

Richard Feynman was "pretty definitely 'no' as far as we are concerned, but somebody ought to try to get him;" Hermann Feschbach's joining was "dubious," but worth exploring.[177]

Teller complained to McMahon's chief aide (William Borden), "the attitude of the members of the GAC has been a serious difficulty in our recruiting efforts, and I continue to wish for a clear-cut change of

174. Bradbury to McCormack, 27 Mar 1950.
175. Ibid.
176. Ibid.
177. Bradbury to Alvarez, 25 Mar 1950, box "Coh-Los Alamos," file, "Los Alamos 1949–1954," Smyth Papers.

heart, publicized at least in their closest circle. A man like Conant or Oppenheimer can do a great deal in an informal manner which will hurt or further our efforts."[178] Borden, building partly on Teller's charges, reported to his boss McMahon about "the great number of highly placed AEC people who strongly and bitterly opposed the H-bomb decision...Without question, these individuals are sincerely and loyally attempting to execute the decision that was made. The problem is whether or not, where feelings run so high and strong, they are capable of putting forth their best effort." Borden wanted people who favored the H-bomb put into AEC vacancies.[179]

Another trial of strength was then takng place between the two groups. Conant was running for the presidency of the National Academy of Sciences. He lost. Wendell Latimer of Berkeley, a strong supporter of the H-bomb, later told the FBI that he had helped defeat Conant because Oppenheimer, whom Latimer thought dangerous, "had Conant in his hip pocket" and promoted Conant's candidacy.[180]

Events from outside the scientific community again pressed hard on the physicists' deliberations. In late June 1950, war erupted on the Korean peninsula. On June 30, Truman committed ground troops to the war. Five months later, the Chinese entered the war with a massive number of troops, threatening to vanquish American-led UN forces. Propelled by these events and general fears of the Soviet threat, Bethe and other holdouts reluctantly agreed to consult on the hydrogen bomb project. Bethe still hoped that the bomb would prove scientifically impossible, and that the world would be saved from this escalation of the arms race.[181] But even though the technical prospects for the bomb were by no means clear, the pressure of the Korean war had begun to drive out open opposition to the new weapon. A fifth stage of the H-bomb story, one of at least surface consensus, had begun.

Los Alamos

In mid-1950, Teller's original ideas for the Super were encountering grave technical obstacles. Mathematician Stanislaw Ulam, whose work at Los Alamos had revealed just how difficult igniting the

178. Teller to Borden, 13 Apr 1950, JCAE records.

179. Borden to McMahon, 11 May 1950, JCAE records.

180. Latimer, interview with C.A. Rolander, 15 Mar 1954, Oppenheimer Files, FBI Records. On Conant, see his own "'If' Game," n.d. (about 1970), Conant Papers. Also see "Minutes of business session," 25 Apr 1950, Records of National Academy of Sciences, National Academy of Sciences, Washington, D.C.

181. Bethe, in AEC (ref. 7), 330–331.

hydrogen weapon would be, recalled, "Teller was not easily reconciled to our results. I learned that the bad news drove him once to tears of frustration, and he suffered great disappointment. [H]e certainly appeared glum in those days, and so were other enthusiasts of the H-bomb project." Von Neumann did not succumb to any such pessimism. "He never lost heart even when the mathematical results for the original approach were negative."[182] Fermi, though he earlier had opposed the Super, also gave scientific advice to solve the problems, and went to Los Alamos to work with Ulam on theoretical exploration of deuterium arrangements for the bomb.[183] But Oppenheimer "seemed rather glad to learn of the difficulties."[184]

Ulam, a Polish emigré who had worked on the Manhattan Project and then returned to Los Alamos in 1946, later stated that he never had any moral doubts about the effort to develop the Super. "I did not feel it was immoral to try to calculate physical phenomena," he wrote. "Whether it [the H-bomb] was worthwhile strategically was an entirely different aspect of the problem—in fact the crux of a historical, political, or sociological question of the gravest kind—and had little to do with the physical or technological problem itself." Ulam added that even a simple mathematical calculation could be applied to systems with terrible consequences. Since no one could reasonably argue that "the calculus was bad," Ulam felt one could not judge science by its application. But Ulam elaborated on this standard argument with an interesting reflection on the initiation of such potentially dangerous research:[185]

> I felt that one should not *initiate* projects leading to possibly horrible ends. But once such possibilities exist, is it not better to examine whether or not they are real? An even greater conceit is to assume that if you yourself won't work on it, it can't be done at all. I sincerely felt it was safer to keep these matters in the hands of scientists and people who are accustomed to objective judgements rather than in those of demagogues or jingoists, or even well-meaning but technically uninformed politicians.

During the heat of the debate in late 1949, John Wheeler was "quietly minding my P's and Q's [on sabbatical] in Europe,"

182. S.M. Ulam, *Adventures of a mathematician* (New York, 1976), 216–217.

183. E.M. Sandoval to Emilio Segrè, 17 Dec 1985 on LA 1158 "Considerations on thermonuclear reactions," 26 Sep 1950, courtesy of Segrè.

184. Ulam (ref. 182), 216.

185. Ibid., Ulam added three subsidiary arguments: an H-bomb's destruction differed in no essential respects from damage that could be inflicted by fission weapons; the Russians would build H-bombs anyway; and fusion bombs by their destructive power might render war less likely.

pondering problems in nuclear physics. Europe's political fragility seemed apparent to Wheeler—"a house built of cards that a puff of wind from the east could blow down." The AEC asked him to join the H-bomb project. "I certainly didn't want to do it. And I held off the decision. But I was going to Copenhagen for two weeks and staying in the house of Bohr...and at breakfast one day I told him that I was concerned whether I should go back to Los Alamos...I can never forget [Bohr's] phrase at breakfast time...'do you imagine for one moment that Europe would now be free of Soviet control if it were not for the bomb?'"[186] Wheeler left for Los Alamos.

Back in Washington a rift was growing over the proper balance between fission and fusion research. The GAC had three new members when it met in late summer and early autumn 1950 to consider the matter. Fermi, Rowe, and Seaborg had all left the committee when their terms expired; they were replaced by chemist Willard Libby (Teller's colleague at the University of Chicago), chemical engineer Walter Whitman of MIT, and Standard Oil president Eger Murphree. At the September meeting, most members doubted that the Super would work soon or at all. Recent work by Ulam and Fermi indicated that a self-sustaining reaction of deuterium would die out. In contrast, new work on fission weapons showed what the official history calls "striking progress." The military could soon have smaller fission weapons that would provide a wider choice of targets and many more bombs, perhaps "doubling the atomic stockpile." The GAC concluded that "preparations for testing thermonuclear principles in 1951 [should not] jeopardize work on fission weapons."[187]

At the GAC meeting of October 30 and November 1, 1950, the gloom about the Super deepened. In defense of the beleaguered project, Teller argued before the GAC that some uncertainties in the Fermi-Ulam calculations, when resolved, might improve prospects. Fermi replied that better data would probably only confirm the present bleak assessment. Teller "could offer little more than determination," according to the official history; and complained that Los Alamos lacked the people to do the calculations and the bold thinking. Teller "had no new ideas. In some way success would be grasped—how, he did not know."[188] New ideas were needed for ways to bring energy from the exploding A-bomb to the deuterium and tritium, and

186. Wheeler, interview with Jeremy Bernstein, typescript, American Philosophical Library, Wheeler collection; and with P. Galison and P. Hogan, 1987.

187. Hewlett and Duncan (ref. 50), 527–528; Also see GAC Minutes, 10–13 Sep 1950, AEC Records, DOE.

188. Quotations from Hewlett and Duncan (ref. 50), 529–530; GAC Minutes, 30 Oct–1 Nov 1950, AEC Records, DOE.

to continue the deuterium burning.[189] The GAC had taken a decidedly dim view of the technical state of play: "We have some misgivings as to the value and relevance of the intensive work which has taken place, and which it is proposed to maintain for the next months." The GAC further "note[d] with regret" that the thermonuclear program was placing such demands on Los Alamos that it was interfering with the fission program.[190]

Soon, nearly all work on the Super halted, as the whole theoretical team at Los Alamos began to focus on the "George" test of the booster scheduled for May 1951. There was a clear sense that even a successful test would not address "the paramount uncertainties which are decisive in evaluating the feasibility of the super." In the absence of crucial information about the design, it seemed that the AEC might have to fix the amount of tritium available for the eventual test. Unsaid, but clearly of concern, was that this amount of tritium might not be enough to make a thermonuclear device.[191]

Gordon Dean emphasized this tritium crisis when he testified before the Joint Committee in November 1950. Even if the bomb itself were not uncertain, he stressed, "There is a great deal of uncertainty as to what you would have to pay or be willing to pay for it, because you might find yourself...sacrificing a hundred or one hundred fifty orthodox bombs in order to get one hydrogen bomb."[192] The amount of tritium needed for a Super, already estimated as large in the fall of 1949, grew substantially larger in early 1950, and by early summer reached the point that the reactors needed to provide it themselves carried an estimated cost of a quarter of a billion dollars.[193] Against this backdrop of tritium cost, design problems, calculational difficulties, and clashes with the fission program, prospects for the Super looked grim indeed.

A-bombs looked better and better, especially when the Chinese Army began to rout UN troops in Korea. Truman himself implied publicly that he might use A-bombs there.[194] Expressing the sense of crisis, on December 6, 1950, Senator McMahon telephoned Gordon Dean, his former law partner and the recently installed chairman of the AEC. "I know you are fully conscious of living every day in a Pearl Harbor atmosphere, therefore, I would put myself, if I were you,

189. Teller and Wheeler, report discussed by GAC in meeting of 23 Sep 1950, cited in JCAE report, 56.

190. GAC report, 23 Sep 1950, cited in JCAE (ref. 5), 56–57.

191. GAC report, 1 Nov 1950, cited in JCAE (ref. 5), 57–58.

192. JCAE (ref. 5), 58.

193. JCAE (ref. 5), 52.

194. Truman, press conference, 30 Nov 1950, in (ref. 138), 727.

in a pre-Pearl Harbor frame of mind. Where are you going to be if they [the Soviets] hit tonight?" Dean responded that the AEC had done its work—"specifically on the [A-bomb] stockpile."[195]

In this climate of fear, a special panel chaired by Oppenheimer submitted a report on long-range objectives, emphasizing atomic bombs and downplaying the H-bomb. The Korean War, according to this report, revealed the importance of fission weapons—both in small wars (including tactical weapons) and in all-out war. Los Alamos should continue "its effort to reduce the dimensions of fission weapons and to increase their efficiency." Recent calculations "cast serious doubt on whether or not deuterium can, in fact, maintain an explosive reaction and...suggest that even if this turns out to be possible, the efficiency of the reaction will be rather low." The H-bomb was, at best, a long-range project, unlikely to be developed in the next five years. The Oppenheimer panel concluded: "Only a timely recognition of the long-range character of the thermonuclear program will tend to make available for the basic studies of the fission weapon program the resources of Los Alamos Laboratory."[196]

This advice further provoked Super supporters. Lewis Strauss, who had left the AEC in April 1950 but remained a vigilant observer of the program, complained that men who had earlier cited moral reasons for opposing the Super were now latching on to scientific objections. "These people are entitled to their ideas," he acknowledged, but not as advisors to the government. To rely on Oppenheimer and his associates was "analogous to giving a campaign into the hands of a commander who did not want to fight. Victory could hardly be expected."[197] Strauss told Gordon Dean that Oppenheimer was "sabotaging" the H-bomb program.[198] In less harsh terms, Frederic de Hoffmann complained: "Oppenheimer has discouraged, and very effectively, people from getting too enthusiastic about the program and would like to see it follow a rather leisurely course."[199]

195. Roger M. Anders, ed., *Forging the atomic shield* (Chapel Hill, 1987), 91.

196. The panel's report is summarized (with some quotations) in Hewlett and Duncan (ref. 50), 530–531; in Strauss, draft memorandum for the President, 5 Feb 1951, SP; and also in Richard Pfau, *No sacrifice too great: The life of Lewis L. Strauss* (Charlottesville, 1984), 131–132. The 10-man panel included Bacher, Alvarez, and C.C. Lauritsen, as well Admiral W.S. Parsons and three generals usually supportive of the H-bomb, McCormack, K.D. Nichols, and Roscoe Wilson. Alvarez later claimed that Oppenheimer had duped him and that he had not understood the report. Alvarez, in AEC (ref. 7), 788–789.

197. Strauss, draft memorandum (ref. 196.). Apparently, Strauss never sent this paper to Truman; no copy can be found at the Truman library.

198. Dean, "Memorandum of conversation with Lewis Strauss," 12 Feb 1951, AEC Records, DOE; also published in Anders (ref. 195), 117.

199. Dean, unpublished diary, 9 Feb 1951, AEC Records, DOE, paraphrasing conversation with de Hoffmann.

Dean did not defend Oppenheimer but stressed that matters were in the hands of people who did believe in the Super. "People at Los Alamos were most enthusiastic about the super at the time the Directive [Truman's decision of January 31, 1950] went to the laboratory." Dean admitted that the Los Alamos program was not an "all-out" project and even mentioned that, in theory, there were various ways of speeding work on the H-bomb: creating a new division at Los Alamos for the Super (this was soon done); putting an enthusiastic man in charge of the Super program (this was soon done); creating a second laboratory (this was later done over Dean's objections); and moving some of the work on bomb design to Princeton (John Wheeler was in the process of doing just this).[200]

Wheeler's Matterhorn B

By December 1950, Wheeler had been at Los Alamos for about eight months. Because of the frustrations of recruiting scientists to Los Alamos, and perhaps because of some of his own disagreements with people at the laboratory, Wheeler felt that he would do better to found a supplementary H-bomb program at Princeton. He received support from Oppenheimer for the Princeton plan, who gave what Wheeler gratefully called "fatherly advice."[201] To Allen Shenstone, chairman of the Princeton Physics Department, Wheeler argued that Princeton was a natural site for the project: "Thermonuclear research involves strong Princeton specialties–nuclear and atomic theory, ideas familiar from astrophysics, and hydrodynamics." Princeton's MANIAC computer would be "indispensible," and graduate students could only benefit: "Insofar as graduate students are going to have to get part of their training working on university sponsored war projects, it will be hard for them to do better than on the thermonuclear project for all-around range of ideas." All that was needed was Teller, and Wheeler urged that the University try to get him. "He is the heart and soul of the enterprise here....We ought to make strong efforts to get him if we are to undertake this work."[202]

Princeton could contribute to bomb assessment, theory, and primordial design by joining with efforts underway at the Institute for Advanced Study. Within a few weeks, Wheeler hoped to meet with

200. Dean (ref. 198).
201. Wheeler to Oppenheimer, 14 Nov 1950, included in letter of 22 Dec 1950, OP; also Bradbury, interview with Galison, 15 June 1988.
202. Wheeler to Shenstone, 8 Jan 1951, Lyman Spitzer Papers, Princeton University Archives.

Shenstone, Wigner, von Neumann, Oppenheimer, astrophysicist Lyman Spitzer and others who might be interested.[203] As Wheeler put it a few months later in a letter to Ernest Lawrence, "I have been dismayed at the small number of theoretical men engaged in [this work]. Seeing that the men won't come to the work, I have decided to take the work to the men."[204] And thus, with the AEC's enthusiastic approval, Wheeler arranged for an AEC contract with Princeton to enlist von Neumann, Spitzer, and a number of others in what came to be called "Project Matterhorn B," with "B" for bomb. From the time they moved to the Forrestal Center at Princeton in May 1951, their self-appointed task was to calculate the feasibility of specific bomb designs—not long-term solutions to problems that might arise later.[205]

"Bringing the work to the men" did not solve the recruiting problem. Above all, Wheeler wanted Richard Feynman to join the effort. Feynman had worked as Wheeler's student at Princeton before World War II, and the two of them had done important work on the theory of the electron before and after the atom bomb project. During the war Feynman had contributed brilliantly to the Manhattan Project; he would clearly be an invaluable asset to Matterhorn. Here is Wheeler's plea:[206]

> Dear Dick: I know you plan to spend next year in Brazil. I hope world conditions will permit. They may not. My personal rough guess is at least 40 percent chance of war by September, and you undoubtedly have your own probability estimate. You may be doing some thinking about what you will do if the emergency becomes acute. Will you consider the possibility of getting in behind a full scale program of thermonuclear work at Princeton through at least to September 1952?

Wheeler assured Feynman that Princeton was expanding into an important center of research on the thermonuclear project. Wheeler would have the help of Spitzer in recruiting; the university had provided a large building; and von Neumann, L. Goldstein and R.D. Richtmyer were going to pitch in on the computer simulation work using Princeton's MANIAC.[207]

To persuade Feynman, Wheeler offered two basic arguments. First, he pointed out that "Already without the benefit of thermonuclear oomph, atomic bombs form a major part of this country's war potential." During World War II, high explosive production peaked at

203. Ibid.
204. Wheeler to Lawrence, 12 Apr 1951, LP.
205. Ref. 202.
206. Wheeler to Feynman, 29 Mar 1951, Richard P. Feynman Papers, file 3.10, California Institute of Technology.
207. Ibid.

4 kilotons per day, yielding about the equivalent of 140 "old fashioned atomic bombs" in 700 days. Wheeler invited Feynman to "take any newspaper guess" as to atomic bomb output, and to reflect "if there is any justification for the hair-shirt philosophy of many nuclear physicists—'Nuclear physics is interesting; therefore we mustn't work on it in case of war; it's better to forget physics and tell the admirals and generals how to do tactical and strategic this-and-that.'" While such tactical reasoning might gain the country a factor of two over earlier production, the nation needed much more. "[W]hat business have the country's best physicists fooling around with a factor two when factors of five and twenty are at stake? If they do, the country may feel in return it ought to give atomic weapon development to the generals!...Princeton's job is to be [the] idea factory and do primordial design, Los Alamos to work in these fields, too, but also to carry things through all the practical stages right to the end. The shortage of people on the idea assessment and primordial design end is to me terrifying. You would make percentage-wise more difference [at Princeton] than anywhere else in the national picture."[208] Feynman declined.

Teller and Ulam's "new Super"

Everyone recognized the dire straits in which the Super program now was stuck. But just at this time of deepest discouragement, Teller and Ulam recast the Super problem in a startling new form. Apparently, in early 1951 Ulam went to Teller with an idea that Teller integrated with work of his own.[209] Though their original reports are classified, later comments give some sense of the nature of the crucially important Teller-Ulam innovation. With this step, many of the technical obstacles to the new weapon were, at once, eliminated.

On March 9, the two issued a secret report proposing that the "conventional A-bomb" be used to "compress materials to high densities." This was in marked contrast to earlier designs where the A-bomb's function was to generate extremely high temperatures, not densities. According to a later "sanitized" summary, "Teller modified this [antecedent deleted] to using the radiation, and this was the origin of 'radiation implosion' [ellipses in original] when Teller recognized the importance of high densities [ellipses in original] he suggested that radiation implosion might be capable of achieving the required extreme densities over large volumes."[210] It would seem that the

208. Ibid.

209. Teller, "The work of many people," *Science, 121* (25 Feb 1955), 271–273; Teller, "Shield" (ref. 7), 79; Ulam (ref. 182), 219–220.

210. JCAE (ref. 5), 62.

Teller-Ulam idea is really two ideas: using A-bomb-emitted x-rays to transfer energy from the A-bomb, and using that energy to *compress* the hydrogen isotopes rather than *heat* them. Such an interpretation seems to be confirmed by Bethe's statement that "The new approach [of Teller and Ulam] was based on two separate discoveries, (a) that high densities could be useful and (b) that they could be achieved by a radiation implosion."[211]

In less than a month, April 4, 1951, Teller had a new type of H-bomb on paper.[212] Though many theoretical tests remained to be undertaken, the new model was utterly different from the classical Super and its variants; it was what Oppenheimer later called a "technically sweet" solution.[213] Suddenly, the hydrogen bomb had become, at least in theory, a practical, comparatively inexpensive and virtually limitlessly powerful weapon. Up to this point technical, moral, and strategic considerations had been inextricably intertwined. Now the technical side of the issue looked solvable, and elegantly so. For some physicists, the now well-designed bomb was attractive in itself, for purely technical reasons. Work at Los Alamos raced forward. On May 9, 1951, the "George" booster test was successfully fired at Eniwetok. Teller and Lawrence, both in the South Pacific for the test, rejoiced. Lawrence had bet the worried Teller five dollars that the test would establish that heavy hydrogen had been ignited and a thermonuclear reaction started. It was a debt Teller was delighted to pay.[214]

About six weeks later, on June 16-18 in Princeton, Oppenheimer hosted a GAC-AEC conference to assess the thermonuclear program. In attendance were all five AEC commissioners and five of the nine GAC members; Carson Mark, Bethe, Bradbury, Teller, and others from Los Alamos; and Wheeler, and Fermi. Teller was not on the scheduled list of speakers. Furious that the Teller-Ulam configuration might not receive its due, he insisted upon speaking about it. Teller reported that Oppenheimer "warmly supported this new approach."[215] The GAC soon judged it "a certainly interesting, possibly encouraging line of attack" that could well lead to a thermonuclear test within a year.[216]

211. Bethe to Dean, 23 May 1952, CD 471.6, Office of the Secretary of Defense Records, RG 330, NA.
212. JCAE (ref. 5), 62.
213. Oppenheimer in AEC (ref. 7), 81.
214. Teller "Shield" (ref. 7), 80.
215. AEC (ref. 7), 720; Blumberg and Owens (ref. 166), 275–278; Hewlett and Duncan (ref. 50), 544–545.
216. GAC minutes, 11–13 Oct 1951, AEC Records, DOE.

The technical possibility of the New Super—indeed, for some its inevitability—consolidated opinions of the community of scientists privy to H-bomb work. Bethe, for example, had made it clear—even while opposing the bomb—that he would support work on the weapon if the Russians might get it first. With the H-bomb now technically possible, that race had begun. And, at last, Bethe joined the effort.

Suspicion of Oppenheimer

In memorable words, Oppenheimer explained during his loyalty-security hearing of 1954 that "when you see something" like the Teller-Ulam configuration "that is technically sweet, you go ahead and do it and you argue about what to do about it only after you have had your technical success. That is the way it was with the atomic bomb."[217] Others, however, doubted whether Oppenheimer had committed himself to the Super after the Teller-Ulam breakthrough.

In the fall of 1951, Teller complained to Oppenheimer at an international conference in Chicago that progress and leadership at Los Alamos on the Super were still inadequate. Teller believed that Marshall Holloway had to be removed, and that Fermi, Bethe, or Oppenheimer himself should take over the direction of the work. Fermi and Bethe were not available. Oppenheimer later claimed that he volunteered to approach Bradbury about returning to Los Alamos for this assignment. "I was content with my job and work at Princeton," Oppenheimer later said, "but I [did] communicate with Bradbury, and I called him and told him of the conversation and he gave no sign of wanting to have the ex-director back."[218]

Despite the opposition of Oppenheimer, Teller pushed for a second laboratory specializing in thermonuclear weapons. Teller had found Los Alamos unpleasant and scientists there found him difficult.[219] Perhaps because of this tension, he had spent only two weeks at the laboratory during the six months between mid-October 1951 and mid-April 1952.

Harold Urey, irritated about the neglect of Teller at Los Alamos, complained to the AEC; then he urged President Truman not to reappoint Oppenheimer to the GAC after his term ran out that summer. "Dr. Oppenheimer and those who follow him have opposed and I believe have obstructed the development of the hydrogen bomb in a most continuous and determined way even after you decided that it should be developed." Urey charged Oppenheimer with "conservative

217. Oppenheimer in AEC (ref. 7), 81.
218. Oppenheimer in AEC (ref. 7), 85; Bradbury in ibid., 480.
219. Ulam (ref. 182), 223; and Dean to Harold Urey, 17 Apr 1952, UP.

and often quite incorrect advice," and mentioned that some scientists believed that Oppenheimer had acted out of ugly motives.[220]

Urey may have been referring here to charges by chemist Kenneth Pitzer, a strong H-bomb advocate and former AEC director of research, who had recently returned to the Berkeley faculty. In April 1952, Pitzer had told the FBI's San Francisco office that Oppenheimer might be disloyal. In the FBI's words, Pitzer also said that "not only did Oppenheimer oppose [the H-bomb] personally, but that he attempted to influence other members of the Commission and other influential persons also to oppose it...even though Oppenheimer's position...was overruled by the President, Oppenheimer stayed on as Chairman and as such has been impeding progress on this work." When the FBI asked Pitzer for evidence, he stated (in the FBI's words) "that Oppenheimer has been instrumental in persuading other outstanding scientists not to work on this project." Pitzer hoped that Oppenheimer would not be reappointed to the GAC. When filing these charges, the San Francisco field office suggested that the Bureau provide this information to the President.[221]

That spring, Pitzer and Wendell Latimer of Berkeley, as well as chemist Farrington Daniels of Wisconsin, joined Urey in writing separate letters to Truman in a campaign against Oppenheimer's reappointment. Pitzer stressed that Oppenheimer "has remained very much in the driver's seat [as GAC chairman] and has dragged the brakes ever since" on the H-bomb. Not only had several GAC members "gone along with" Oppenheimer's efforts, according to Pitzer, but AEC commissioner Smyth had also been influenced by Oppenheimer.[222] Latimer warned Truman, "it would be a dangerous mistake to continue a man with this mystic pacifist philosophy," a man in "constant opposition...to the development of new weapons."[223] Daniels, a World War II aide to Arthur Compton at the Chicago Met Lab of the Manhattan Project, recommended Pitzer and Latimer as candidates to replace Oppenheimer on the GAC.[224] News of the campaign leaked to Conant. "Some of the 'boys' have their axe out for three [also Oppenheimer and DuBridge] of us on the GAC....Claim we have 'dragged our heels' on H bomb. Dubridge

220. Harold Urey to Dean, 24 Apr 1952, and Urey to the President, 2 June 1952, UP.

221. Special Agent in Charge (SAC), San Francisco, to Director, FBI, on "J. Robert Oppenheimer internal security—R," 5 Apr 1952, Doc 100–17828–273, Oppenheimer Files, FBI Records.

222. Pitzer to Truman, 4 Apr 1952, Official File (OF) 692B, Truman Library.

223. Latimer to Truman, 29 May 1952, OF 692B, Truman Library.

224. Daniels to Truman, 16 June 1952, OF 692B, Truman Library.

worries about Oppie!"[225]

At the end of May 1952, William Borden sent McMahon a report ("some late gossip—which I believe is authentic") on Oppenheimer. According to Borden, former commissioner Lewis Strauss had visited Truman to urge him not to reappoint Oppenheimer to the GAC, and Air Force Secretary Thomas K. Finletter was also maneuvering to block reappointment. "If—when you speak to the President about Oppie—you find it feasible," Borden advised, "you might wish to ask him to review with you substitute names because a man really must be on the complete inside of the program to know whether a given individual is on Oppie's team or on the team that wants to build H-bomb."[226]

As Borden knew, Air Force secretary Finletter, as well as top SAC generals, had come to distrust Oppenheimer, partly because of Oppenheimer's anti-H-bomb attitudes and his criticism of the Air Force's emphasis on strategic nuclear weapons. Antagonism had peaked in early 1952 with the top-secret Vista report, partly written by Oppenheimer, which emphasized preparation for tactical nuclear war and thus urged less dependence on strategic nuclear weapons. The report implied that the H-bomb was unimportant. The Vista group, headed by DuBridge who had opposed the H-bomb, and including H-bomb opponents Robert Bacher and Charles Lauritsen, both of whom were also at Caltech, had hoped to reorient American military policy, especially for Europe. To Finletter and other top Air Force officials, the report seemed to strike at the heart of the Air Force itself—big bombers and strategic nuclear weapons. The study made Finletter and his aides more suspicious of Oppenheimer's motives, led them to look into his leftwing past, and propelled the Air Force to try to bar him from top-secret documents.[227]

The GAC meeting on June 14, 1952, was the last for Oppenheimer, Conant, and DuBridge. Truman had chosen not to reappoint them and their six-year terms ran out that summer. "Finally Oppie, Lee DuBridge and I are through as members of the GAC!!," Conant wrote in his diary after the GAC meeing that day. "10½ years of almost continuous connection with a bad business now threatening to become really bad!!"[228]

225. Conant Diary, 9 May 1952, Conant Papers, Pusey Library, Harvard University.

226. Borden to McMahon, 28 May 1952, box 2, Records of the JCAE (ref. 5).

227. Garrison North to Finletter, "Dr. J. Robert Oppenheimer," 1 Jul 1952, Doc. 100–1728–1118, Oppenheimer Files, FBI Records. The Vista report (subtantially declassified) is in Modern military records, NA. On Vista, also see David Elliot, "Project Vista and nuclear weapons in Europe," *International security, 11* (Summer 1986), 163–183.

228. Conant Diary, 14 June 1952.

Four days after that secret GAC meeting, FBI director J. Edgar Hoover sent the White House a summary of a secret interview with Teller about Oppenheimer. In it, Teller stressed that Oppenheimer had opposed the H-bomb and claimed that he was still trying to block it. According to Hoover's report, "Dr. Teller stated that in his opinion, Dr. Oppenheimer is opposed to the H-bomb development since Dr. Oppenheimer had achieved greatness in atomic energy work and may have developed a resentment against further efforts which might be an improvement on what he has accomplished. [Teller] stated that it was his personal hope that Dr. Oppenheimer would be relieved of his responsibilities on the General Advisory Committee and all other responsibilites connected with military preparedness." Unlike Pitzer, however, Teller did not charge that Oppenheimer might be disloyal. Hoover summarized Teller's position this way: "Dr. Teller...does not believe that there are any disloyal thoughts or influences in Dr. Oppenheimer's opposition to the development of the H-bomb."[229]

Apparently Hoover sent similar warning letters to the AEC and the Attorney General. They excluded, at Teller's request, other charges that Teller had made about Oppenheimer: that he had sent Bethe to Los Alamos to determine whether the "H-bomb was feasible;" and that AEC Commissioner Smyth's opposition to the H-bomb was because of the "influence of Oppenheimer." Teller had told the FBI (in its words) "that should [this] information be disseminated to scientists, particularly Dr. H.D. Smyth of the Atomic Energy Commission, [Teller's] position with the Atomic Energy Commission in the H-bomb development would be untenable and he would have to give up his work with the Atomic Energy Commission."[230]

In August 1952, Teller reiterated his deep frustration over the pace of thermonuclear progress: "I believe that we have pursued the thermonuclear development throughout the past seven years at much too slow a rate; and even since the Presidential Directive progress has been slower and certainly narrower than is consistent with national security. Our only comfort seems to be that the Russians have not as yet given any evidence of possessing an effective thermonuclear weapon. [But] we may be at the beginning of an arduous program and

229. Hoover to Sidney Souers, 18 June 1952, Doc 100–17828–317, Oppenheimer Files, FBI Records. There is no copy of this letter available at the Truman Library in the President's files or Souers' correspondence file with the FBI.

230. W.A. Branigan to A.H. Belmont, "Dr. J. Robert Oppenheimer internal security—R," 10 June 1952, Oppenheimer Files, FBI Records. An interview on 15 May with Teller was also rewritten to exclude some information. See SAC, Albuquerque, to Director, FBI, 26 May 1952, Doc 100–17828–313, and "Dr. J. Robert Oppenheimer internal security—R," 27 May 1952, Doc 100–17828-[illegible numbers], Oppenheimer Files, FBI Records.

it is quite possible that the Russians have advanced much farther along the road than we have."[231]

The road to the Super

Beginning in the late spring of 1952, a special high-level State Department advisory panel was convened by secretary Acheson to explore long-term possibilities of arms control with the Russians; it included Oppenheimer, Vannevar Bush, and CIA Deputy Director Allen Dulles, with Harvard professor McGeorge Bundy as executive secretary. Contrary to Acheson's expectations, the panel sought to head off the first full-scale thermonuclear test, scheduled in the autumn, and indeed to bar all thermonuclear testing. Without such testing, the panel argued, the H-bomb could not be developed and America would be safer. Before this first test, there might still be a chance for a Soviet-American agreement to ban the bomb.[232] The panel's effort failed. By 1952, with America's first thermonuclear test scheduled for the autumn and most advisors persuaded of the desirability of an American H-bomb, the panel found no support in Washington for its recommendation. Even AEC Commissioner Henry D. Smyth had shifted and was sketching in 1952 various hopes for psychologically exploiting the forthcoming test at Eniwetok of the Teller-Ulam hydrogen device.[233] Secretary Acheson and military leaders wanted the weapon. They welcomed the strategic and political power they thought the bomb promised.[234]

Truman did try to delay the autumn Eniwetok test briefly, until after Election Day, to keep the issue out of the 1952 presidential campaign. But he failed because AEC advisors concluded that even a brief postponement would be too wasteful. The result was that the AEC, with White House approval, tried to impose a news "blackout" on the "Mike" test. The blackout failed, but the H-bomb never became an issue in the presidential campaign.[235]

Two weeks after the spectacular test of the world's first thermonuclear device, the United States successfully detonated the world's most powerful all-fission weapon ("King"). It had a yield forty times that

231. Teller memorandum, 14 Aug 1952, cited in JCAE (ref. 5), 78–80, on 79–80.

232. Panel of Consultants, "The timing of the thermonuclear text," n.d. (probably Sep 1954), *FRUS* (1952–54), *2,* 994–1008. Also Vannevar Bush to Conant, 29 Mar 1954, Bush Papers, LC. The other two panel members were Joseph Johnson and John Dickey.

233. Smyth to Executive Secretary, NSC, 18 Sep 1952, PSF 202.

234. Bernstein (ref. 79), 344–346, 349–351.

235. Anders (ref. 195), 226–231.

of the Hiroshima bomb. But while some of the bomb's designers had originally thought of King as a possible fallback superweapon, King's success in no way dampened enthusiasm for the H-bomb.[236]

The success of King and Mike did not reduce alarm among H-bomb advocates. As Strauss wrote the Joint Committee, it did not mean "*that we enjoy any lead time in the competition with Russia in the field of thermonuclear weapons.*" He stressed Fuchs' espionage, the Soviet capture of German nuclear physicists, and general Soviet perfidy to justify his assumptions about Soviet progress. Further, he explained, the American project had lagged because of its unpopularity among some physicists and the comparatively small size of the staff deployed on the work. Echoing the laments of Teller and Lawrence, among others, Strauss complained that the American H-bomb project never gained as strong a commitment as the World War II quest for the A-bomb.[237] No doubt he would have been at least equally alarmist had the test failed.

After Eisenhower's election, Strauss became the president's special advisor on nuclear energy and then, in July 1953, chairman of the AEC, replacing Gordon Dean. From these new positions, Strauss vigilantly watched over the Super program. On August 7, 1953, he sent Eisenhower a special memorandum noting that the Soviets had not tested fission weapons in nearly two years and suggesting that this hiatus might mean that they were jumping directly to the thermonuclear.[238] Within a week, the Soviets announced that they had exploded an H-bomb.

The official American announcement, issued by Strauss, played down the event: the test "involved both fission and thermonuclear reactions. It will be recalled that more than 3 years ago the United States decided to accelerate work on all forms of atomic weapons. Both the 1951 and the 1952 Eniwetok test series included tests involving similar reactions."[239] Strauss privately concluded that the Soviet test confirmed his old fear, shared by Teller and Lawrence among others, that America had to race for the Super. Strauss added to his sense of being right the pleasurable conviction that Oppenheimer and Conant had been wrong, dangerously wrong. He soon sent for Oppenheimer's security files.[240]

236. York (ref. 159), 84–85, 99–100; Bethe in AEC (ref. 7), 329–330, 333, 340; and Lilienthal in ibid., 400–401. For example, Theodore Taylor, one of King's designers, in John McPhee, *The curve of binding energy* (New York, 1974) and in interview with Galison and Hogan.

237. Strauss to William Borden, 10 Dec 1952, SP. Emphasis in original.

238. Strauss to Eisenhower, 7 Aug 1953, SP.

239. Strauss, statement of 20 Aug 1953, in *BAS, 9* (Jul 1953), 236.

240. Strauss, oral history, 138–139; J.A. Waters to Strauss, 12 May 1954, AEC Records, DOE.

Rabi, who had succeeded Oppenheimer as GAC chairman, also worried deeply that the Soviets would catch up and even jump ahead of the United States. He wrote Strauss: "It is not safe to assume that [the Soviets'] rate of progress is not even greater than ours, not only in the field of the so-called hydrogen bomb but also in the field of fission weapons....It may easily be true that in all too few years they may be equal or ahead of us qualitatively and even quantitatively." He did not have a solution or even a program. The issues, he stressed, reached beyond technical matters—better delivery vehicles, a larger supply of fissionable materials, and improved weapons design—to some kind of basic reconsideration of policy.[241]

Representative W. Sterling Cole, the new Republican chairman of the Joint Congressional Committee on Atomic Energy, urged Eisenhower to speed up the H-bomb program, improve air and sea defense against nuclear attack, emphasize the peaceful uses of atomic energy, and labor for world disarmament.[242] Eisenhower was already working on the "atoms for peace" plan, to be launched in December 1953, on the "massive retaliation" doctrine to be announced in January 1954, and on greater efforts for defense.

As Rabi, Cole, and Eisenhower knew, the American H-bomb program was speeding on to success. The 1952 Eniwetok test had provided information necessary for scaling down the huge thermonuclear device (the size of a house) to a deliverable bomb. In March 1954, in the "Bravo" test in the Pacific, the United States detonated the first true superbomb that could be carried in a plane. The bomb yield was 15 megatons, over a thousand times more than the Hiroshima bomb that had killed at least 80,000 Japanese. Exploding with nearly twice the force predicted, the Bravo test drenched with fallout some Japanese fishermen, producing radiation illness and one death.[243]

The Bravo test ignited international protests about nuclear testing and the development of the H-bomb, including a plea by Arthur Compton, an earlier supporter of the weapon, that America halt testing.[244] So troubled were Secretary of State John Foster Dulles and President Eisenhower by the protests that they seriously considered seeking a test moratorium with the Soviets. Unless the United States did so, Eisenhower suggested at an NSC meeting on May 6, 1954, the

241. Rabi to Strauss, 24 Aug 1953, SP.

242. Cole to Eisenhower, 21 Aug 1953, *FRUS* (1952–54), *2,* 1185–1188.

243. For an analysis of the Russian mixed fission-fusion bomb of August 1953, see York (ref. 159), 89–92.

244. Compton to White House, 31 Mar 1954, Official File 108A, Eisenhower Library.

world might continue "to think that we're skunks, saber-rattlers and warmongers." Supporting Eisenhower, Secretary of State John Foster Dulles said, "We are losing ground every day in England and in other allied nations because they are all insisting we are so militaristic."[245]

The GAC's warnings of October 1949, delivered to the Truman administration, were finding echoes in the early Eisenhower administration. Oppenheimer, Conant, and others had warned about the injury to America's reputation if it developed the H-bomb; Eisenhower and Dulles were upset that the American image had been damaged. They were not prepared to give up the H-bomb, but they briefly hoped that a test moratorium could improve the American image. Eisenhower worried, according to the NSC minutes, about "a future which contained nothing but more and more bombs." Wasn't there some way out?[246]

On June 23, 1954, five days before the AEC's decision that Oppenheimer was a security risk, Eisenhower learned that Strauss, Secretary of Defense Charles E. Wilson, and the Joint Chiefs vigorously opposed any halt to thermonuclear testing. The president reluctantly accepted their advice.[247] And thus the American-Soviet thermonuclear race continued unabated.

In November 1955, twenty months after Bravo, the Soviets exploded their first true hydrogen bomb. That test indicated, as Oppenheimer and Conant had believed in 1949/50, and as Oppenheimer and Bush had in 1952, that the United States had been ahead in the quest for the H-bomb. And the twenty-month gap before the Soviet test provided evidence for some like Herbert York (the first director of Livermore Laboratory) much later to argue,[248] that the United States may have missed an opportunity to head off the H-bomb race.

The meaning of the controversy for scientists

Laments such as York's and Bush's were linked intimately to the 1953/4 loyalty-security charges against Oppenheimer for opposing the H-bomb and to splits in the physics community, growing out of the H-bomb controversy that bitterly divided American scientists for over a generation.[249]

245. Quoted from paraphrase in NSC Minutes, 6 May 1954, *FRUS* (1952–54), *2,* 1423–1428.
246. NSC Minutes, 27 May 1954, *FRUS* (1952–54), *2,* 1455.
247. NSC Minutes, 23 June 1954, *FRUS* (1952–54), *2,* 1469–1472.
248. York (ref. 159), 94–110.
249. Blumberg and Owens (ref. 166), 364–365, 380.

The H-bomb was at the heart of the Oppenheimer security hearing: why had he initially opposed the weapon, did his motives make him disloyal or a security risk, and did he continue to oppose the Super even after Truman's pro-bomb decision of January 31, 1950? The Oppenheimer case had been triggered in November 1953 when William Borden, McMahon's former advisor, accused Oppenheimer of being a Soviet spy partly because of his opposition to the H-bomb. Most of the pro-Oppenheimer witnesses had earlier opposed the H-bomb (although most supported it in 1954): Conant, Fermi, Bethe, Buckley, Rabi, Bush, Bacher, DuBridge, Manley, Lilienthal, Kennan, and former AEC commissioner Sumner Pike.

No person opposed to the H-bomb testified against Oppenheimer. Henry D. Smyth, who had opposed the Super in 1949 and who as an AEC commissioner in 1954 served as one of Oppenheimer's "judges," found for him. Five of the 1949 pro-H-bomb people also testified for Oppenheimer—General McCormack, Karl Compton, former AEC commissioner Gordon Dean, Bradbury, and von Neumann—and a sixth, Arthur Compton, submitted a letter affirming Oppenheimer's loyalty.[250]

In contrast, all eight hostile witnesses had supported the H-bomb in 1949. Five were scientists: geophysicist David T. Griggs, former chief of research for the air force, Alvarez, Pitzer, Latimer, and Teller. Ernest Lawrence, who had been expected to give damning testimony, had fallen ill. Sickness and fear of offending Oppenheimer supporters or jeopardizing AEC funding for his laboratory, kept him from the hearing.[251]

Latimer and Griggs made the harshest statements, but Teller, because of his prestige, may have damaged Oppenheimer most. Like others, Teller argued that Oppenheimer had delayed America's development of the Super. According to Teller, if Oppenheimer had simply provided moral support in 1945 for the Super, many scientists would have stayed at Los Alamos and the bomb would have been developed "just about 4 years earlier." Echoing his secret FBI testimony of 1952, Teller told the hearing board when he was asked whether Oppenheimer was a security risk: "I feel that I would like to see the vital interests of this country in hands which I understood better, and therefore trust more."[252] These words made Teller a pariah

250. Compton to Gordon Gray, 21 Apr 1954, ACP.

251. Bernstein, "In the matter of J. Robert Oppenheimer," *HSPS, 12:2* (1982), 228–233; Alvarez interview with Bernstein, 12 Sep 1981; Bernstein, interview with Boris Pash.

252. Teller in AEC (ref. 7), 710, 714, 726–727; and SAC, Albuquerque to Director, FBI, "Dr. J. Robert Oppenheimer," 27 May 1952, FBI Records.

in much of the scientific community whose respect and affection he desired.

The splits in the scientific community widened when two journalists in late 1954 published a book on the H-bomb controversy that castigated Oppenheimer and Los Alamos for resisting the Super and celebrated Teller and the recently created Livermore Laboratory as America's savior in producing the thermonuclear weapon.[253] The book so upset Fermi that he criticized it in public, contending that "perhaps as much as 95 per cent of the H-bomb development took place at Los Alamos."[254] Los Alamos Laboratory director Norris Bradbury, who had quarreled earlier with Teller, condemned the book at a special press conference. It was not Livermore, he asserted, but Los Alamos "that developed EVERY SUCCESSFUL THERMONUCLEAR WEAPON THAT EXISTS TODAY."[255] Bethe believed that the book was part of the vendetta, inspired by some H-bomb loyalists, against Oppenheimer and the earlier H-bomb opponents. It "was strongly inspired by people around Edward [Teller], perhaps not by Edward himself, who wanted to destroy not only Oppenheimer but also his friends."[256]

In an attempt to restore unity, and encouraged by Strauss and Enrico Fermi, Teller soon began drafting his own conciliatory version of the H-bomb story.[257] Having heard of Teller's effort but not having seen it, Bethe wrote urging him on: "Only you can give such an answer in a manner which will *unify* the scientific community again" (Bethe's emphasis). If Teller was not willing to do this, Bethe warned that he would publish his own article, one that "must necessarily be divisive, and in fact must be hard on you." Teller's article "must be very clear. It must say in effect that there were no essential delays in the Los Alamos work after the President's decision in 1950, except those caused by our technical ignorance."[258]

Acrimony over the pace and direction of thermonuclear research was reflected in the way the participants themselves cast the history of recent events, even while the debate still flared. In fact, even *before* the explosion of the first thermonuclear weapon, a dispute began over the history of the project.

253. James Shepley and Clay Blair, Jr., *The hydrogen bomb* (New York, 1954).
254. *Newsweek* (18 Oct 1954), 88.
255. Norris Bradbury, statement in Los Alamos Scientific Laboratory press release, 24 Sep 1954, Enrico Fermi papers, University of Chicago. Capitals in original.
256. Bethe, quoted in Blumberg and Owens (ref. 166), 370.
257. Ibid., 374–376.
258. Bethe to Teller, 30 Nov 1954, Teller Papers.

In a "Memorandum on the History of the Thermonuclear Program" of May 28, 1952, Bethe had argued that the Teller-Ulam invention "came about by a series of accidents," none of which

> was at all an obvious, logical development which would occur in every thorough scientific investigation of the problem. On the contrary, the results of the calculations of Ulam and Fermi in 1950 (which *were* logical steps in the program) would have led nearly every scientist to give up the thermonuclear program altogether. Only Teller's persistent belief in the practicability of thermonuclear reactions led to our present, completely novel concepts in this field.

Later in the same document, Bethe contended that "the conception of [Teller and Ulam] itself was a matter of inspiration and it was, therefore, unpredictable when it would occur." For Bethe, the lesson was that there was no rational reason to have devoted more effort to the thermonuclear program than Los Alamos had. Even if supplementary funds and more personnel had been available, they would not have accelerated development. What was needed was Teller's "inspiration."[259]

In what at first may seem paradoxical, Teller viewed the situation oppositely: his idea had come as a *natural* outgrowth of earlier work. The implication was that more effort by Los Alamos would have led to the discovery much earlier, perhaps in 1946/7 or at the latest by 1950/1. In the first draft of his article Teller presented the Teller-Ulam design, as physicist James Franck complained to Teller, as a "natural consequence of the previous ideas." "People who are angry about your statement in the Oppenheimer case might construe [this argument] as a hidden attack against him," Franck warned Teller. Apparently Teller revised the article but implications of the "natural consequence" argument remained.[260] Ironically, Teller's opponents stressed his genius, first to defend the pace of the thermonuclear program and then to protect Oppenheimer from charges of delay. In contrast, Teller defended his discontent with the program by citing the naturalness and inevitability of his innovation.

Teller's article, published in *Science*, credited 51 scientists who had made contributions, but it could not mend the breach even though Teller avoided direct criticism of Oppenheimer and the GAC, praised Oppenheimer for his leadership during the Manhattan project, and lauded Bethe for his contribution to the Super. Teller pleaded, "the

259. Bethe, "Memorandum on the history of the thermonuclear program," 28 May 1952, AEC Records, DOE.

260. Franck to Teller, 16 Nov 1954, James Franck Papers, University of Chicago. Also see Teller to Strauss, quoted in Blumberg and Owens (ref. 166), 375–376.

development of the hydrogen bomb should not divide those who in the past have argued about it but rather should unite all of us who in a close or distant way, by work or by criticism, have contributed toward its completion. Disunity of the scientists is one of the greatest dangers for our country."[261] Teller concluded:

> We would be unfaithful to the tradition of Western civilization if we were to shy away from exploring the limits of human achievement. It is our specific duty as scientists to explore and to explain. Beyond that our responsibilities cannot be any greater than those of any other citizen of our democratic society....I am confident, whatever the scientists are able to discover or invent, that the people will be good enough and wise enough to control it for the ultimate benefit of everyone.

The argument of a scientific imperative, which continued to divide the scientific community, separated Teller and many of his allies from those like Oppenheimer and Conant, who had argued in the H-bomb debate for a restriction on research, the danger of some scientific knowledge, and the peril of producing the weapon. Such differences reached to the heart of issues about scientists' responsibility and moral obligation. Beyond the sharply felt differences, physicists in the H-bomb debate of 1949/50 generally shared two important beliefs—the *political* importance of physicists and the scientific and political dangers of secrecy. They attacked secrecy as antithetical to the development of scientific knowledge, which advanced through dialogue and challenge. Openness, most of the physicists believed, had served them well in peacetime physics; they had struggled for open discussion at Los Alamos and the Rad Lab; and the free presentation of ideas was essential in a democratic society.

Advocates and critics of the H-bomb believed that scientists did make the critical difference in shaping the Truman administration's final decision for the superbomb. Yet, it is questionable whether the physicists' moral, political, and strategic arguments made a decisive difference. Certainly it was the physicists who brought the *possibility* of a thermonuclear weapon to the attention of the policymakers, and scientists on both sides emphasized that the decision about the hydrogen bomb was a crucial one. Truman's secrecy order had barred them from going beyond the Joint Committee, the Department of State, the Joint Chiefs, and the Department of Defense to plead their case to the people. Inside the government, the triumph of Teller, Lawrence,

261. Teller, "The work of many people," *Science, 121* (25 Feb 1955), 267–275, and quotation on 275; cf. Teller "Shield" (ref. 7), 66–84, on 75, for reopening the dispute with Oppenheimer. The dispute is treated in Teller, "Seven hours of reminiscences," *Los Alamos science, 7* (Winter/Spring, 1983), 190–195; and Bethe (ref. 7), and Bethe, *Los Alamos science, 3* (Fall 1982), 43–53.

Alvarez, and their colleagues on the H-bomb, and the defeat of Oppenheimer, Conant, Fermi, Rabi, and their associates, may well have been dictated by the needs and values of President Truman and Secretary of State Acheson.[262] This was a point that Oppenheimer unhappily had understood during his H-bomb discussion with Acheson late in 1949; for it was then that he first recognized the inevitability of defeat on this issue and decided not to carry the GAC's anti-H-bomb report directly to the president.

4. SUMMARY AND CONCLUSION

Individual trajectories through the hydrogen bomb debate were complex. For example, Bethe evinced no opposition to the Super during World War II and the early Cold War, and he even took out a patent on the device in 1949. Then, shortly after the issue was revived following Joe 1, he objected to the weapon in the strongest possible terms, publicly expressing his discontent after the Truman decision in articles. In 1950, with the outbreak of the Korean War, he began to quiet his opposition and then, about the time that the Teller-Ulam idea became known, began consulting on the project. He then considered the production of the bomb to be a technical certainty. Judging that the H-bomb project had become a race between the United States and the Soviet Union, Bethe wanted to do what he could to help the Americans win.

Fermi also flip-flopped during the course of the debate. In October 1944 he recommended that the Superbomb be pursued during peacetime as part of a general improvement in nuclear capability. He reiterated this favorable verdict in the four-man Scientific Advisory Panel's report written shortly before Trinity. Just after Hiroshima, however, Fermi agreed with the rest of the panel that there was a grave danger of an arms race and in particular that the H-bomb presented a terrible danger. According to the summary of the panel's meeting in September 1945 drawn up by Arthur Compton, the four members argued that "this development [of the H-bomb] should not be undertaken primarily because we should prefer defeat in war to victory obtained at the expense of the enormous human disaster that would be caused by its determined use." Within about seventeen months, Fermi shifted back towards support of the thermonuclear project. In late October 1949 he switched again; with Rabi he composed the minority GAC report characterizing the H-bomb as "an evil thing considered in any light." Then, after Truman's decision, Fermi began

262. Bernstein (ref. 89), 343–351.

to work on the hydrogen bomb.

At the outset of World War II, Oppenheimer had supported immediate research on the thermonuclear bomb. With others involved in the Manhattan project, Oppenheimer thought that the A-bomb was a relatively simple matter, a prelude to the real work of building a Super. Towards the end of the war he participated in several studies that advocated the postwar construction of the hydrogen bomb as an important part of America's nuclear arsenal. Soon after the bombing of Hiroshima, Oppenheimer suddenly reversed himself, mystifying Teller and his allies in Berkeley. In 1947, Oppenheimer reluctantly shifted to endorse the Super. He strongly backed a variety of other forms of nuclear weaponry, including miniaturization of weapons, increase of explosive capacity, multiplication of the number of bombs, and the development of radiological warfare. Most significantly, Oppenheimer supported the development of the booster. When Oppenheimer became an outspoken member of the anti-H-bomb forces in the GAC decision of October 1949, he simultaneously had to defend the development of the nuclear arsenal and to exclude the Super as the only morally unacceptable device in the nuclear armory.

James Conant, friend and admirer of Oppenheimer since their World War II experience, had followed a slightly different trajectory. Most notably, Conant had expressed doubts about the H-bomb during World War II. Like Oppenheimer, by early 1947 Conant endorsed the efforts to expand and diversify the emerging American nuclear arsenal, including the development of the booster, and Conant uneasily supported the Super until September 1949, when he became a leading GAC opponent of this weapon. "Over my dead body," he had said.

These four principals were not the only scientists to shift positions. Ernest Lawrence had served on the Scientific Advisory Panel that had come out so strongly against the H-bomb in September 1945; but in 1949 he became a powerful voice for the weapon. Arthur Compton had been for the Super during World War II, abruptly moved against it after Hiroshima, and then in 1947 or 1948 urged an accelerated program. John Manley, Secretary to the GAC, had been for the bomb in early October 1949, but at the height of the debate he was outspokenly against it. Rabi too had switched back and forth: he was strongly against the bomb in 1949 but consulted for the project at Princeton under Matterhorn. So sudden was Robert Serber's switch that Luis Alvarez, a colleague at the University of California Radiation Laboratory, discovered Serber's change of heart only as the two physicists anxiously waited for the GAC to emerge from their fateful October 1949 meeting.

The gyrations of Bethe, Fermi, Oppenheimer, Conant, Lawrence, Compton, Manley, and Rabi reflected the deep problem of a generation of physicists suddenly plunged into a new world of political and scientific power. In a remarkably short time they had come face to face with moral, political, strategic, and technical problems that had few precedents before World War II, and little to glean from the experience of the Manhattan project, itself, where there had been neither the time nor the place to think through these issues. The obvious analogy—the atomic bomb—provided but a weak guide and left them unable to formulate a consistent stance.

Despite the scientists' many changes of position, it is possible to periodize the history into five stages; their many reversals then exhibit a certain order. The first stage began with the pre-Los Alamos "Super" meeting in the summer of 1942 at Berkeley, and continued until about the time of Hiroshima and Nagasaki. These first hydrogen bomb discussions took place as if thermonuclear weapons—not A-bombs—would constitute the primary goal of a wartime nuclear physics effort. Though it is hard to imagine in retrospect, the atomic bomb itself was seen as a relatively trivial technical task, one that might challenge industry but not scientists. The thermonuclear problem appeared at once more difficult and more interesting. With this understood, it becomes clearer why Oppenheimer, Fermi, Bethe, Serber, and others who later became opponents strongly endorsed the hypothetical superweapon in this first stage. Indeed, during this period, none of the participants made an ethical distinction between hydrogen and atomic bombs.

But with the mass carnage at Hiroshima and Nagasaki, the A-bomb's great lethal power became viscerally real to the physicists as it had not during the early phase of the project. The H-bomb history entered a second stage as physicists began, in the glare of postwar publicity, to confront what they had done. Rallying to the cause of international accords on atomic weapons, the scientific elite dampened their enthusiasm for the hydrogen bomb. Oppenheimer, Lawrence, Compton, and Fermi now argued strongly against the Super, while conceding its probable *technical* feasibility. Aside from Teller, few showed much interest in pushing full ahead on the hydrogen bomb. From August 1945 to early 1947 the thermonuclear was on everyone's back burner—again with the exception of Teller.

When the Baruch plan failed in December 1946, the intensifying Cold War introduced a third stage to the history. Even politically optimistic scientists were deeply troubled by worsening Soviet-American relations. Many concluded that a general improvement of the nuclear arsenal was necessary, though the place of the hydrogen bomb was uncertain partly because of technical problems in its design.

Fermi now spurred the quest for the bomb; Oppenheimer and Conant grudgingly supported it. Arthur Compton cancelled his opposition. At this time the H-bomb was the object neither of explicit hostility nor of concerted lobbying. It was one of many rearmament projects.

The explosion of Joe 1 in August 1949 initiated a fourth stage in the weapon's contentious development. The scientific community split: one side, led by Lawrence, Teller, Alvarez, Pitzer, and Latimer, viewed it as essential that the United States respond to the Soviet threat with an energetic program for the H-bomb. Their opponents, led by Conant and Oppenheimer, saw the Soviet detonation as the opening of an arms race that could only be halted by a redoubled effort to prevent development of the next generation of more powerful weapons on both sides. Of the anti-H-bomb GAC scientists, some were for conditional and others for unconditional renunciation of the weapon. When the debate became public in the last weeks of 1949, most of the well-known scientists who spoke up did so against the thermonuclear. Truman's decision for the bomb, combined with the Fuchs espionage case, weakened the opposition, but failed to generate enthusiasm for the thermonuclear program among prominent civilian scientists. Protests continued.

The final shift of sentiment came in a fifth period that played against the broad backdrop of the worsening Korean War. But the real turning point for most of the scientists came with the 1951 Teller-Ulam idea for radiation implosion. From an extraordinarily expensive, unwieldy, and possibly unworkable drawing-board hope, the bomb became a technical project with a definite program of development. Debate subsided in this stage as it became clear that further open opposition was futile. Many of the chief opponents now worked on the bomb, at least part time: Fermi, Rabi, and Bethe foremost among them. Even Oppenheimer, perhaps judging the idea "sweet," acknowledged that it had to be done. Bethe's reversal may be representative of many—he had always left open the possibility of working on the bomb if the Soviets seemed likely to get it. With the bomb now apparently technically feasible, there was a race on that he believed the United States had to win. What little opposition remained surfaced in a last-ditch, unsuccessful attempt in late 1952 to prevent the first full-scale thermonuclear test, and to conclude a Soviet-American test ban on H-bombs.

The shifting political currents of the wartime and Cold War years allowed and sometimes forced the scientists and their political overseers to consider the H-bomb far more carefully than they had the fission bomb. As a result, the record of their positions on a weapon in the process of development offers a diversity of technical, strategic, and moral reflections unique in the history of warfare.

Schematically, one can divide these arguments as follows:

For building the H-bomb	Against building the H-bomb
1. Power per dollar	1. Resource allocation
2. Strategic asset	2. Coastal vulnerabilities and false security
3. Diplomatic/psychological	3. Moral standing
4. Principle of double effect	4. Means/ends
5. Best weapon	5. Overkill leads to genocide
6. Continuum A-bomb/H-bomb	6. If A-bomb bad, why make worse
7. Technological imperative	7. Scientists' special responsibility

On a prosaic level, both sides adduced arguments about the best allocation of resources. The pro-bomb contingent saw more "bang per buck" in the H-bomb once the weapon went into production; the anti-bomb side contended that the diversion of effort, money, and facilities from A-bomb production to H-bomb work would cripple the effort to bolster the nuclear shield. A related split arose over the building of the second laboratory—some thought it would hamper Los Alamos from accomplishing its mission, others saw it as a necessary investment in thermonuclear weapons. In both cases, debate over the strategic function of the weapon within the nuclear arsenal was married to the distribution of the Atomic Energy Commission's resources.

Although in this essay we have only been able to touch briefly on thinking within the military, the superweapon offered the armed forces at least two clear strategic advantages. First, since a single weapon could destroy a city, only a single plane had to penetrate enemy defenses. Thus the likely success of a city-destroying bombing mission would be hardly diminished even under conditions of highly effective air defense. Second, even inaccurate bombing with an H-bomb would destroy almost any conceivable target. Countering the supposed strategic advantage, some scientists and politicians pointed out that the cities most vulnerable to air attack were those on the coasts. While the United States had almost all its population located near the oceans, the Soviets had practically none. Conant added that the Superbomb would weaken the West in another way, by offering a false sense of security that might lull the Western countries into not building up the conventional defense of Europe.

The Joint Chiefs, Teller, and the Berkeley group located a further argument in the twilight between diplomacy and war: a country that alone possessed the Superbomb would have a psychological and diplomatic advantage that no amount of other weapons could counter. And there was no reason to believe that American renunciation of the H-bomb would lead to reciprocal action by the Soviets—or to improved relations. For the opposition, such arguments were spurious. They saw the H-bomb as a weapon of such utter destructiveness that its possession would be inimical to the principles of democracy it was supposed to defend. As a result, America's construction of the weapon would undermine the legitimacy of its political goals both at home and abroad. Conversely, as the GAC argued, rejecting the H-bomb would put some bound on the totality of war, and so "limi[t] the fear and arous[e] the hopes of mankind." Renouncing the weapon would raise America's moral stature in the world.

In the context of the H-bomb debate, even the military felt obliged to present their position in language that was at least partly moral. The Joint Chiefs of Staff, for example, used what moral philosophers have termed the "principle of double effect" to defend their position. While they could foresee that the thermonuclear bomb would level an entire city, that was not the military's intention. According to the Joint Chiefs, to the extent that blame could be apportioned in the event of all-out nuclear war, it had to fall on the country that started the conflict—not on those who responded by whatever means in order to end it. Bethe sharply disagreed, arguing that the principal moral distinction between the United States and its Soviet adversary lay in the *means* they would use, rather than the *ends* they would achieve. Should the United States employ such a city-destroying weapon, they would forfeit that moral claim.

Should diplomacy fail, the pro-bomb side argued, what would the military and scientific community say to the American people? That we now face an implacable enemy armed with and ready to use hydrogen bombs and that we do not have them out of moral rectitude? How could the defense establishment—both in uniform and lab coat—not provide their fellow citizens with the best weapons that they could possibly design? The H-bomb opponents replied that the hydrogen bomb was not simply a military weapon, because it was of such overwhelming destructive power that it could have only one purpose: the total annihilation of a city of noncombatants. There were practically no military targets anywhere in the world that "needed" a bomb a thousand times greater than "Fat Man" or "Little Boy." Enhanced atomic bombs extended the kill radius of the fission bomb to several miles. As a result, Fermi and Rabi insisted, the Super was a weapon on the road to genocide.

McMahon responded to the GAC's reasoning with a slippery-slope argument in a forceful rhetorical question: On what conceivable grounds could a moralist distinguish the week-long destruction of a city by firebombs from a hundred-plane atomic attack or a single-plane thermonuclear strike? To whom could it matter whether death came from fifty fission weapons or one thermonuclear? The United States had used nuclear weapons already, twice, killing over a hundred thousand civilians; non-nuclear weapons had killed millions more. How could one use humanitarian arguments to advocate increased atomic armaments and oppose exploration of the Super? Responding to this "continuum" position, DuBridge issued an equally stark query: If the situation is bad with atomic bombs, why make it worse with a weapon a thousand-fold more destructive?

As difficult as these moral antinomies were, one of the most fraught and complex issues raised by the debate was the one of the so-called technological imperative. In effect, the pro-bomb side raised three distinct notions of such an imperative: abstract, political, and competitive. Teller frequently argued that technological advance was simply "out of our hands"–it was in the nature of science itself to pursue lines of inquiry, and no amount of hand-wringing could stop it. "It is the scientist's job to find the ways in which these laws [of nature] can serve the human will....It is not the scientist's job to determine whether a hydrogen bomb should be constructed, whether it should be used, or how it should be used." Teller's sense of obligation as stated here is an abstract one, a demand on the scientist's relation to natural laws and humanity. No amount of moralizing was going to stop someone, somewhere, with the backing of an appreciative government, from trying to simulate the energy of the sun in the confines of our planet. Furthermore, for Teller, to try to stop the pursuit of knowledge was not only futile, but immoral.

Obviously Teller also had more political concerns about the spread of Stalinism, and part of his argument was that scientists bore a responsibility to the American people to develop what they could in the field of nuclear weapons. What the elected government decided to do with that information was the government's not the scientists', responsibility. Bradbury put it into his striking medical metaphor: the physics community had an obligation to explore the implications of horrible weapons much the way an oncologist needed to display the pathology of cancer. No one should confuse the search for the dynamics of a disease with a desire to see it spread. According to this line of reasoning, no amount of solemn proclamations could ever carry as effective a warning to humankind as the reality–proved through design and testing–of a terrible new weapon. Demonstration of the power of the Super was the best way to insure that it would never be used.

Finally, there was a competitive form of the technological imperative: if the bomb became feasible then it would inevitably be made because of the larger political and military antagonism between the United States and the USSR. Given that it would be made in one of those two countries, the American scientific community had a specific national obligation to insure that their country had it first. These various forms of imperative gave different and complementary senses to the phrase that the "H-bomb *had* to be built" both as an injunction and as a statement of historical inevitability.

Einstein, Weisskopf, and many other scientists specifically challenged the passivity of such determinism. Rather than being obligated to an abstract concept of technical inquiry, these protesting physicists believed that by virtue of their knowledge, scientists had a special responsibility to oppose the next step of the arms race. Spurred by the H-bomb, Einstein put it this way: "In our time, scientists and engineers carry a particularly heavy responsibility, because the development of military means of mass destruction is dependent upon their work and activities."[263]

The debate engaged by these arguments has a particular interest in that it was insulated from larger industrial interests (unlike, say, disputes about aircraft development and procurement). Nor did the weapons laboratories (principally Los Alamos) have a powerful institutional role in the basic debate on the H-bomb. Bradbury and his associates generally favored the Super with a support constrained by their concern about resource allocation—but their power was easily offset by the more influential GAC. Even the military did not seem to lobby very vigorously for the hydrogen bomb before Truman's decision of January 31, 1950. The initial push for the H-bomb cannot be seen as issuing from the so-called military-industrial complex.

Instead, the H-bomb had its origin within the scientific community. One segment of that community kept the issue alive throughout World War II, and brought it—and kept it—in the ears of political leaders during the early Cold War. In 1949, the issue reached Truman, probably for the first time. He and Acheson, not swayed by the pressures from scientists from either side, made the basic decision—for the H-bomb.

The history of the H-bomb, and especially the debate, raises important and enduring issues. What is the proper role of secrecy as important weapons systems are under consideration? Should the press censor itself at the price of secrecy from the larger community of scientists and citizens? How are the twin principles of national defense and open discussion to be balanced? Who should have access to the debate? Another set of questions revolves around the moral

263. Nathan and Norden (ref. 150), 526.

issues of national defense. Moral as well as technical and military considerations were crucial for the Joint Chiefs, Strauss, McMahon, and Kennan, as well as for Teller, Oppenheimer, Bethe, Conant, Fermi, Rabi, Wheeler, Smyth, and DuBridge. Was the hydrogen weapon, as McMahon contended, necessary to "save ourselves" from foreign aggression or was it, as York later claimed, a tragic and needless head start into an unending arms race?

Then there is the matter of the scientists' moral responsibility. Should they override their individual principles and work on weapons systems they oppose, if constituted authorities desire it? Or should they withhold their talents, as Bethe initially did and Einstein always did, regardless of presidential will or public wishes?

At a depth without precedence the hydrogen bomb catalyzed a moral struggle within the elite of the scientific community over how to think about their role in the emerging national defense state. Debates over weapons since the H-bomb have been intense, but in none has the scientific community struggled so hard to create the right vocabulary and frame the right questions that would clarify these issues.

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