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CHAPTER FOUR: THE HYDROGEN BOMB

By

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Origins

It was the prospect of the uranium bomb that gave rise to the idea of the hydrogen bomb. In the years before World War II physicists had identified the nuclear fusion of light elements as the source of energy in the sun and the stars. Since fusion takes place only at temperatures of tens of millions of degrees, this research did not appear to have practical application.[1] Early in 1942 Enrico Fermi speculated, in a conversation with Edward Teller, that a fission explosion could be used to initiate a thermonuclear reaction in a mass of deuterium, one of the isotopes of hydrogen. Fermi and Teller understood from the outset that the explosive yield of a fusion bomb could be made indefinitely large, depending only on the amount of thermonuclear fuel it contained. Teller went on to examine the idea of a thermonuclear bomb and in the summer of 1942 presented his preliminary ideas to the Berkeley conference on the physics of nuclear weapons. When Los Alamos was established in the spring of 1943, work on the superbomb or Super (as the hydrogen bomb was known) was one of its main tasks.[2]

It soon became apparent, however, that the Super would be difficult to develop. Significant amounts of tritium – another, heavier, isotope of hydrogen – would be needed, but tritium occurs rarely in nature and is difficult and costly to produce. This lessened the promise of the hydrogen bomb as a wartime project. Besides, the development of the atomic bomb proved more complex than expected and so required almost all of Los Alamos's resources. Teller continued to work almost exclusively on the Super with a small group of collaborators. No significant breakthroughs resulted from this wartime research, but Teller remained firmly committed to the project. Oppenheimer supported the idea that vigorous development of the hydrogen bomb should begin once the war was over.[3]

Los Alamos held a conference on the Super on 18-20 April 1946, with thirty-one participants, including Teller and John Von Neumann, as well as Egon Bretscher and Klaus Fuchs from the British delegation. Teller described the design that he and his group had developed. The idea was to place a fission weapon at one end of a cylinder of liquid deuterium. The fission explosion would ignite the deuterium and the resulting thermonuclear reaction would propagate to the other end of the cylinder.

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[1] This is why fusion reactions are also known as thermonuclear reactions.
[2] Lillian Hoddeson et al, Critical Assembly: A Technical History of Los Alamos during the Oppenheimer Years, 1943-1945, Cambridge U.P., 1993, pp. 44-7; Herbert York, The Advisors. Oppenheimer, Teller and the Superbomb, W.H. Freeman, 1976, pp. 21-23; Peter Galison and Barton Bernstein, "In Any Light: Scientists and the Decision to Build the Superbomb, 1942-1954," Historical Studies in the Physcial Sciences, 1989, vol. 19, no. 2, 270-271.
[3] Hoddeson, op.cit. 203-204, 345-346; York, op.cit. 22-24; Galison and Bernstein, loc.cit. 271.

Known later as the "classical Super," this was a concept rather than a design. The two main problems were how to ignite the thermonuclear fuel and how to ensure that the fuel continued to burn once it had been ignited. Accurate mathematical models were needed to study the immensely complex physical processes involved. No definitive studies were possible in 1946 because the computers and computational methods required to perform the necessary calculations were only then being developed. The conference report nevertheless provided an optimistic assessment. "It is likely that a super-bomb can be constructed and will work," it said. "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."[4]

This optimism proved to be unwarranted. Development of the Super was an enormous challenge, as daunting in its own way as making the atomic bomb. The Maud Committee had made a convincing case in 1941 that the atomic bomb was feasible. The decision to make the atomic bomb entailed, however, the creation of a whole new industry to produce the materials needed for the bomb. It was the production of fissionable materials as much as – if not more than – the skill and ingenuity of weapons scientists and engineers that determined when the atomic bomb would be built. Nuclear industries were already in existence when the hydrogen bomb

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[4] Quoted in Galison and Bernstein, loc.cit 280; York, op.cit. 23-24; Lorna Arnold, Britain and the H-Bomb, Palgrave, 2001, 7-8 (this is the official history of the British project).

decisions were made. The hydrogen bomb did require new materials – deuterium, tritium, lithium-6 and their compounds – but the main challenge lay in the physics, which was vastly more difficult than the physics of the atomic bomb. The report on the Los Alamos conference pointed out that predicting the thermonuclear reaction required "a deep insight into the general properties of matter and radiation derived from the whole theoretical structure of modern physics."[5] Experiments with controlled fission had helped in making the atomic bomb. Experiments with controlled fusion were not possible, however, and the physical processes had to be simulated. Physicists had to work with mathematicians and electrical engineers to develop the computers and mathematical methods to carry out the simulations.[6]

Hans Bethe noted in 1954 that there were four requirements for a successful thermonuclear program: an idea; a team to carry out the idea; highly-efficient fission bombs; and high-speed computers.[7] In making the atomic bomb, the three countries examined in this chapter had gone some way to meeting these requirements. Each of them had created teams of weapons scientists and had developed fission bombs. The progress the three countries made toward making the hydrogen bomb was to a significant degree determined by their progress in developing computers and computational methods. Most striking of all was the fact that the three countries started from the same basic idea, thanks to the activities of Klaus Fuchs, who was member of the British wartime delegation at Los Alamos.

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[5] Quoted in Peter Galison, Image and Logic, University of Chicago Press, 1997, 693-694.
[6] Ibid. 45.

The two British scientists who attended the Los Alamos conference had been actively involved in the laboratory's thermonuclear work.[8] Bretscher had worked on calculations relevant to the Super. Fuchs had contributed an idea that was to prove significant in the development of the hydrogen bomb. In 1944 von Neumann had proposed placing a D-T (deuterium-tritium) mixture inside the active material in a fission bomb. He assumed that when the bomb exploded the D-T mixture would ignite as a result of heating and compression. The thermonuclear reactions in the D-T mixture would release fast neutrons, and these would increase the number of fissions in the atomic bomb and enhance its yield. This was an important step in the development of boosted fission weapons.[9] Fuchs took up this idea early in 1946, when he was investigating the possibility of using a fission bomb to ignite a cylinder of thermonuclear fuel. He modified von Neumann's idea by proposing that the D-T mixture be placed outside the uranium-235 in a beryllium oxide tamper, and the fission bomb and the D-T mixture encased in a radiation-impervious material. The atomic charge and the D-T mixture would be separate, and radiation from the former would be used to compress and ignite the latter. The resulting explosion would in turn ignite the cylinder of deuterium. This was the first time that the principle of radiation implosion had been suggested. On 28 May 1946 Fuchs and von Neumann filed a joint patent application for this idea.

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[7] Hans Bethe, "Comments on the History of the H-Bomb," Los Alamos Science, Fall 1982, 50.
[8] Arnold, op.cit. 7; German Goncharov, "The American Effort," Physics Today, November 1996, 46.
[9] A boosted weapon is one in which a fission bomb is used to initiate a small thermonuclear reaction so that the fast neutrons emitted by this reaction will increase the efficiency of the use of the fissionable

Bretscher and Fuchs soon returned to Britain to become division heads at the Atomic Energy Research Establishment at Harwell. Before leaving America, they reported on the Los Alamos conference to Sir James Chadwick, head of the British mission to the Manhattan Project. In 1947 and 1948 Fuchs gave the Soviet Union detailed information about the classical Super. Thus hydrogen bomb development in the United States, the Soviet Union and Britain had a common point of departure in the Los Alamos conference of April 1946. Each project led to the development of a two-stage hydrogen bomb using the mechanism of radiation implosion, which differed in fundamental ways from the classical Super. The United States first tested the two-stage weapon in March 1954. The Soviet Union followed in November 1955, and Britain in May 1957. Although each of the hydrogen bomb projects had a common starting-point in the classical Super and ended by testing the two-stage weapon, the path from the initial concept to the actual weapon was far from straightforward. In this chapter I explore the interconnections between the three projects by examining how they moved from the classical Super to the testing of the hydrogen bomb.

The United States Program to 1954

Many of the leading scientists at Los Alamos returned to universities after the war. The laboratory devoted most of its resources to improving fission weapons; work

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material. A superbomb uses a fission bomb to ignite a large mass of thermonuclear fuel in order to produce a very large explosion.

on the hydrogen bomb proceeded slowly.[10] Teller came up with a new and apparently simpler design in August 1946. This was the "Alarm Clock," which consisted of alternating layers of fissionable and thermonuclear materials. Los Alamos worked on both the classical Super and the Alarm Clock, but neither design looked promising to the laboratory's directors or to the Atomic Energy Commission (AEC). By the summer of 1949, "work on the super itself presented a very mixed picture," according to Herbert York.[11] Theoretical calculations had cast doubt on the feasibility of the classical Super. In spite of Teller's continuing enthusiasm, there was little support at Los Alamos or in the AEC for an intensive program.

The Soviet atomic bomb test on 29 August 1949 changed the political situation utterly.[12] The government knew that the Soviet Union was building the atomic bomb but had not expected it to succeed before the early 1950s. Washington was shaken; there was a general sense that something needed to be done in response. Lewis Strauss, a member of the Atomic Energy Commission, wrote on 5 October to the other four commissioners that "the time has come for a quantum jump in our planning (to borrow a metaphor from our scientist friends)…we should make an intensive effort to get ahead with the super." Ernest Lawrence and Luis Alvarez traveled from Berkeley to Washington to lobby for the Super, arguing that the Soviet Union might already have given top priority to the development of thermonuclear weapons. Senator Brien

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[10] York, Op.cit. 22-26; Galison and Bernstein, loc.cit, 273-283. Just how slowly has been a matter of controversy, since "slowly" is a relative term. For a defense of the laboratory see Carson Mark,
[11] York, op.cit., 26-27.
[12] On the push for the Super after Joe 1 (as the Soviet test was commonly known in the United States) see Galison and Bernstein, loc.cit, 184-188.

McMahon, chairman of the Congressional Joint Committee on Atomic Energy, echoed that warning when he wrote to the AEC on 17 October that "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." United States intelligence had no evidence that the Soviet Union was developing thermonuclear weapons. To the Super's advocates, however, the Soviet atomic test showed how rapidly the Soviet Union was progressing and how little the United States knew.

There was, however, strong opposition to the Super. The General Advisory Committee (GAC) of the AEC opposed development when it met on 28-30 October 1949.[13] Although it was an advisory body, the GAC carried great weight on scientific and technical matters; its members, apart from Oppenheimer, included Fermi, I.I. Rabi, and James Conant. At its October meeting it approved the build-up and diversification of fission weapons (including tactical weapons), endorsed preparation for radiological warfare, and supported the development of boosted fission weapons. This was not, in other words, a group of men opposed in principle to the development and production of nuclear weapons. But the committee unanimously recommended against developing the Super. It pointed to technical and theoretical problems with the existing concept and questioned whether such a weapon would be cheaper than fission bombs in terms

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[13] The GAC Report may be found in York, op.cit. 151-159.

of damage area per dollar. Besides, the Super carried "much further than the atomic bomb itself the policy of exterminating civilian populations."[14]

The majority of the GAC recommended that the United States should make an unconditional commitment not to develop the Super, since this would be "in a totally different category from an atomic bomb" and might become a "weapon of genocide." They argued that the "extreme dangers to mankind inherent in the proposal wholly outweigh any military advantage that could come from this development."[15] Fermi and Rabi took a different view, arguing that an American commitment not to develop the Super should be made conditional on a similar renunciation by the Soviet Union. "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," they wrote. "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."[16] The GAC Report gave expression to moral reservations that scientists who had helped to make the atomic bomb felt about the further development of weapons of mass destruction. The Fermi-Rabi proposal was particularly innovative in advocating the logic of mutual restraint that arms control would later try to institutionalize.

Three of the five AEC commissioners, including the chairman David Lilienthal, agreed with the GAC recommendation, but the Super's supporters responded with

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[14] Ibid 155.
[15] Ibid 156.

vigor. On 14 November Lewis Strauss wrote to the president that the recent Soviet test showed that the Soviet Union had the superbomb within its grasp, and that "a government of atheists is not likely to be dissuaded from producing the weapon on 'moral' grounds."[17] Senator McMahon was an equally forceful advocate. He sent a long and eloquent letter to Truman on 21 November supporting development.[18] The Joint Chiefs weighed in on 23 November with a paper arguing that "possession of a thermonuclear weapon by the USSR without such possession by the United States would be intolerable."[19]

Truman set up a three-man committee to advise him on the issue. Secretary of State Dean Acheson was the chairman; Lilienthal and Secretary of Defense Louis Johnson were the other members.[20] Lilienthal was reluctant to go forward, but Acheson and Johnson supported development of the Super. On 31 January 1950 the committee met with Truman for seven minutes. The president approved their recommendation at once and cut short Lilienthal's speech expressing reservations about the decision. He asked, "Can the Russians do it?" When all heads nodded, he said "In that case we have no choice. We'll go ahead."[21] Later that day he announced that he had directed the AEC to work "on all forms of atomic weapons, including the so-called hydrogen or

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[16] Ibid 159.
[17] Galison and Bernstein, loc.cit. 298.
[18] FRUS 1949, i, 588-595.
[19] Ibid 595.
[20] Ibid 597 [check]
[21] The Journals of David E. Lilienthal Volume Two: The Atomic Energy Years 1945-1950 Harper & Row, 1964, 632-633; R. Gordon Arneson, "The H-bomb Decision," Foreign Service Journal, May 1969, 29.

super bomb."[22] On the following day the New York Times carried the headline: "Truman Orders Hydrogen Bomb Built." The decision was applauded in Congress and in the press. On 10 March Truman took a secret decision to authorize preparations not only for a test, but for production of the weapon as well.[23]

Truman's decision did not result in rapid development of the superbomb, for the scientists still did not know how to build it. There was some confidence that it could be done. The GAC had estimated in October 1949 that "an imaginative and concerted attack on the problem has a better than even chance of producing the weapon within five years."[24] During 1950, however, the mathematician Stanislaw Ulam showed, using the Monte Carlo method, that Teller's earlier calculations had been mistaken, and that a very large amount of tritium would be needed to initiate a self-propagating thermonuclear reaction; this would make the Super an enormously expensive proposition. Further work by Ulam and Fermi cast doubt on whether a self-propagating thermonuclear reaction could take place in deuterium. By the autumn of 1950 the classical Super looked unworkable, and the program was in extreme difficulty.[25]

It was only in the early months of 1951 that a way out of the impasse was found.[26] Ulam proposed that the energy from a fission bomb be used to compress the

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[22] FRUS 1950, I, 513.
[23] Galison and Bernstein, loc.cit. 305-306, 310-312.
[24] York, op.cit. 154.

[25] Richard G. Hewlett and Francis Duncan, A History of the United States Atomic Energy Commission. Vol. 2: Atomic Shield, University of California Press, 1990, 439-441.
[26] Ibid 537-539.

thermonuclear fuel to a high density before the fuel was ignited. Teller suggested radiation implosion rather than mechanical shock as the mechanism for compressing the thermonuclear fuel. Fuchs had proposed in 1946 that radiation implosion could be used to a compress a D-T mixture that would then ignite a cylinder of liquid deuterium. Fuchs was working within the framework of the classical Super. On 8 May 1951 – after Teller and Ulam had formulated the new concept – Fuchs's idea was put into practice in the George test, in which the radiation from a large fission explosion was used to compress and ignite a small amount of thermonuclear fuel. Teller (as Bethe acknowledged in 1954) may have been influenced by his work on this design when formulating the Teller-Ulam idea. The Teller-Ulam configuration confines and uses the radiant energy from a primary atomic bomb to compress and ignite a secondary, physically isolated, core containing the thermonuclear fuel. Because the fission primary is physically separate from the thermonuclear fuel, bombs of this kind are sometimes referred to as two-stage or staged weapons. Neither of the key elements in this configuration – staging and radiation implosion – is present in the classical Super.

It was clear at once to the scientists in the program that the Teller-Ulam configuration made the superbomb feasible. Oppenheimer remarked three years later that "the program we had in 1949 was a tortured thing that you could well argue did not make a great deal of technical sense…. The program in 1951 was technically so sweet that you could not argue about that."[27] The AEC commissioners and the

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[27] United States Atomic Energy Commission, In the Matter of J. Robert Oppenheimer, MIT Press, 1971, 251.

members of the GAC met in Princeton in June 1951 with key scientists and consultants from Los Alamos and Princeton and gave their support to a vigorous effort to realize the Teller-Ulam idea.

The first test of the Teller-Ulam idea took place in the Mike test on 1 November 1952 when the United States detonated a thermonuclear device on the Enewetak atoll in the South Pacific. This was not a deliverable weapon, but a large and bulky assembly, weighing about 60 tonnes. The thermonuclear fuel was liquid deuterium, which had to be kept at temperatures colder than -250 degrees C, and therefore required a large refrigeration unit. The detonation, which was known as the Mike shot, produced an explosive yield equivalent to ten megatons of TNT – about 500 times more powerful than the plutonium bomb that destroyed Nagasaki.[28] In the spring of 1954 the United States tested six variants of the Super in the Pacific. The first and most powerful of these was detonated on Bikini atoll in the Bravo shot on March 1. This was a deliverable bomb and used lithium deuteride, a chalklike solid that was much easier to handle than liquid deuterium, as the thermonuclear fuel. The explosive yield of 15 megatons – more than 1000 times greater than that of the Hiroshima bomb – was larger than expected. A Japanese fishing-boat, which was about 80 miles from Ground Zero when the test took place, received a heavy dusting of radioactive debris. The 23 members of the crew soon contracted radiation sickness, and one of them died from it.

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[28] York, op.cit. 82-83; Chuck Hansen, US Nuclear Weapons: The Secret History, Orion Books, 1988, 56-60.

Five more tests of the hydrogen bomb followed in the Pacific over the next two and half months.[29]

The Bethe-Teller Exchange

The United States developed the hydrogen bomb during one of the tensest periods of the Cold War. The victory of the Chinese communists had altered the balance of power in the world and triggered recriminations in the United States. The Korean War added to the tension abroad and at home. The Soviet atomic bomb test had shown that sooner rather than later the United States would be vulnerable to nuclear strikes by the Soviet Union. American advocates of the superbomb argued that the Soviet Union might make thermonuclear weapons before the United States did. They had expressed that fear before Truman's decision to go ahead, and when Klaus Fuchs was arrested in London on 27 January 1950 they had new grounds for anxiety. News of Fuchs's arrest arrived too late to affect Truman's decision to develop the Super.[30] But Fuchs's confessions to the British and to the FBI raised the level of anxiety in Washington. He had given the Soviet Union detailed information not only about the fission bomb, including the design of the plutonium bomb, but also about American work on the Super up to June 1946 when he left the United States to return to Britain.[31]

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[29] York, op.cit. 85-87; Hansen, op.cit. 61-68; Neil O. Hines, Proving Ground, University of Washington Press, 1962, 165-195.
[30] Galison and Bernstein, loc.cit. 310-312.
[31] Fuchs made a confession to William Skardon on 27 January 1950 and a more technical confession to Michael Perrin on 30 January. Both are reprinted in Robert Chadwell Williams, Klaus Fuchs: Atom Spy, Harvard University Press, 1987, 180-194.

In the spring of 1951 Fuchs's confessions assumed a new significance when radiation implosion, which he had proposed as part of the mechanism for initiating the classical Super, became a key element in the Teller-Ulam configuration.

The United States had no information about Soviet work on the hydrogen bomb; as late as July 1953 – one month before the first Soviet thermonuclear explosion – the CIA reported that "we have no evidence that thermonuclear weapons are being developed by the USSR."[32] The advocates of the superbomb nevertheless criticized Los Alamos and the AEC for not doing enough to meet the Soviet challenge.[33] In May 1952 Hans Bethe responded to this criticism in a memorandum to Gordon Dean, chairman of the AEC. Bethe had headed the theoretical division at Los Alamos during the war and was still active in weapons development. He wanted to counter the impression that "the Russians may have been able to arrive at a usable thermonuclear weapon by straightforward development from the information they received from Fuchs in 1946."[34] He pointed out that the calculations done by Ulam and Fermi in 1950 had shown that the classical Super design discussed at the Los Alamos conference in April 1946 was unworkable. "The theoretical work of 1950," he wrote, "has shown that every important point of the 1946 program had been wrong. If the Russians started a thermonuclear program on the basis of the information received from Fuchs, it must have led to the same failure." Bethe argued that the discoveries that led to the Teller-Ulam

[32] Central Intelligence Agency, National Intelligence Estimate, NIE-65, FRUS 1952-1954, viii, 1189.
[33] Richard G. Hewlett and Jack M. Holl, Atoms For Peace and War 1953-1961, University of California Press, 1989, 34-37.
[34] Hans Bethe, "Memorandum on the History of the Thermonuclear Program," 23 May 1952. CD 471.6, Office of the Secretary of Defense Records. RG 330, NA.

configuration had been largely accidental.[35] It could not be assumed, therefore, that intensive work on the earlier ideas about the classical Super would lead in a straightforward way to the Teller-Ulam concept. While the Soviet Union might indeed be making a major effort to develop the hydrogen bomb, there was good reason to think that it had not taken the lead.

Teller responded three months later, claiming that the Soviet Union might well be far ahead.[36] He rejected Bethe's thesis that intensive work on the ideas of 1946 would not have led to the development of a workable design. He also disputed Bethe's characterization of the Teller-Ulam idea as "accidental." Modification of the earlier ideas, Teller argued, might have yielded practicable results. "Radiation implosion is an important but not a unique device in constructing thermonuclear bombs," he wrote. In other words, Soviet physicists could have come up with an alternative workable idea. Moreover, he continued, "the main principle of radiation implosion was developed in connection with the thermonuclear program and was stated in a conference on the thermonuclear bomb, in the spring of 1946. Dr. Bethe did not attend this conference but Dr. Fuchs did." Teller was concerned that if Fuchs had communicated the idea of radiation implosion to Soviet scientists, they might have hit upon the Teller-Ulam configuration before Teller and Ulam did so.

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[35] According to Bethe, it was an accident that radiation implosion had been chosen as the mechanism for igniting the D-T mixture in the George test. It was also an accident that Ulam had come up with the idea of using a fission bomb to compress the thermonuclear fuel, since this idea had been conceived independently of the thermonuclear program.

[36] Edward Teller, "Comments on Bethe's History of Thermonuclear Program," 14 August 1952. Records of JCAE, RG 128 NA.

Fuchs himself, it is worth noting, did not place a very high value on the information he had given to the Soviet Union about thermonuclear weapons. "His information concerning the H-bomb work in the United States at the time he returned to England was…best described as a confused picture," the FBI reported, in describing his views, after interviewing him in 1950.[37] Fuchs did not then understand the significance that radiation implosion would have in the Teller-Ulam configuration. Nor of course did Teller before the early months of 1951.

The Soviet Program to 1955

The physicist Yakov Frenkel wrote to Igor Kurchatov in September 1945 about the possibility of using an atomic bomb to initiate thermonuclear reactions.[38] He did not know that Kurchatov had already received intelligence reports of American work on the Super, including a summary of lectures in which Fermi had described specific features of the classical Super.[39] Kurchatov asked a group of physicists to look at the possibility of releasing energy from light elements. On 17 December 1945 Yakov Zeldovich made a report on the work of this group to the Technical Council of the Special Committee on the Atomic Bomb. Zeldovich concluded that "an explosion of an

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[37] "Foocase – Espionage ® Interviews in England with Fuchs," Hugh H. Clegg and robert J. Lamphere to Director, FBI, June 4, 1950, 30. HSTL, PSF.

[38] "Dokladnaia zapiska Ia.I. Frenkelia I.V. Kurchatovu o soderzhanii besedy s F. Joliot-Curie," in Atomnyi proekt SSSR Tom II Atomnaia Bomba 1945-1954 Kniga 2, Nauka, 2000, 330-332.

[39] G.A. Goncharov, "Khronologiia osnovnykh sobytii istorii sozdaniia vodorodnoi bomby v SSSR i SShA," in Nauka I Obshchestvo atomnogo proekta, Trudy mezhdunarodnogo simpoziuma, vol. 1, Izdat, 1997,237-238.

unlimited amount of the light element" was possible in principle.[40] Thermonuclear research could not claim a high priority, however, since the overriding goal of the Soviet nuclear project after August 1945 was to make the atomic bomb as quickly as possible. Nevertheless, in June 1946 a small group of theoreticians under Zeldovich's direction began to examine the possibility of releasing nuclear energy from light elements.[41]

Soviet thermonuclear research received a new impetus in 1948. Alexander Feklisov, Fuchs's MGB control in London, had asked him in September 1947 for information on the "tritium bomb." At that meeting Fuchs had told Feklisov in general terms about the work done in the United States up to 1946. Six months later, on 13 March 1948, he gave Feklisov a package of material.[42] According to his 1950 confession, Fuchs passed on information about

> "the T-D cross-section value before this was declassified, and he also gave all that he knew from his Los Alamos period on the methods for calculating radiation loss and the ideal ignition temperature. He also described the current ideas in Los Alamos when he left on the design and method of operation of a super bomb, mentioning, in particular, the

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[40] Ibid. 239. The paper was published in Soviet Physics: Uspekhi, May 1991, 445-446. It is not clear whether Zeldovich and his colleagues had access to the notes of Fermi's lectures. Their report coincided on some points with Fermi's notes, but contained original elements too.

[41] Goncharov, loc.cit. 239

[42] G.A. Goncharov, "Osnovnye sobytiia istoriia sozdaniia vodorodnoi bomby v SSSR I SSha," Uspekhi fizicheskikh nauk 1996, vol. 166 no. 10, 1098-1099.

composite fission bomb, the tritium initiating reaction, and the deuterium one."[43]

Russian sources now make it clear that Fuchs provided a detailed description, along with a diagram, of the classical Super design as it existed in the spring of 1946. He also included information on the two-stage radiation-implosion system for which he and von Neumann had applied for a patent.[44]

The Ministry of State Security forwarded this material to Stalin, Molotov, and Beria. On 10 June 1948 the government set up a special design group under Zeldovich to work on the hydrogen bomb at KB-11, the weapons laboratory at Sarov, and another group, headed by Igor Tamm, at the Physics Institute of the Academy of Sciences in Moscow.[45] Tamm recruited some younger physicists, including Andrei Sakharov, to work with him. In September-October 1948 Sakharov came up with a novel design. He proposed placing alternating layers of thermonuclear fuel and uranium-238 in a fission bomb between the core and the high explosive charge. He called this design the sloika (usually translated as "Layer Cake"). It bore some similarity to Teller's "Alarm Clock" but was arrived at independently by Sakharov. (Tamm's group did not have access to the intelligence materials.[46]) In December 1948 Vitalii Ginzburg, another member of Tamm's group, proposed that lithium deuteride -- a compound of lithium-6 and deuterium -- be used in the bomb, instead of deuterium and tritium. The merit of this

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[43] Fuchs's confession to Perrin, in Williams, op.cit. 192.
[44] Goncharov, loc.cit. 1099
[45] The decree "O dopolnenii plana rabot KB-11" is printed in Atomnyi proekt SSSR. Tom II Atomnaia bomba 1945-1954 Kniga 1 Nauka 1999, 494-495. At this time the hydrogen bomb received the designation RDS-6. RDS stands for "jet engine special" the name given to Soviet nuclear weapons.

proposal was that lithium deuteride would be easier to handle than deuterium and tritium and their chemical compounds. More importantly, the lithium-6 would produce tritium when bombarded by neutrons in the course of the explosion.[47]

KB-11 held a conference in Sarov in June 1949 to review the state of work on nuclear weapons.[48] The conference concluded that work should continue on both the Layer Cake design, proposed by Tamm's group, and the classical Super (the Cylinder, known in Russian as truba), which Zeldovich's group was investigating. For reasons that remain unclear, Beria was cautious about pressing ahead. It was only after Truman's 31 January 1950 announcement that Beria, on 4 February, asked his chief scientists and managers for reports on Soviet thermonuclear work and drafts of decrees on developing the hydrogen bomb.[49] He sent the draft decrees to Stalin for his signature on 26 February. In the accompanying letter he described the difficulty and the cost of developing the hydrogen bomb. He went on:

> "taking into account the fact that our enemies may have in their hands a new, very effective weapon, we consider it necessary and possible, in spite of the above-mentioned difficulties, to organize research and practical work on the development of the design of the hydrogen bomb and

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[46] Goncharov, loc.cit. 1099.
[47] Sakharov refers to this proposal in his memoirs as the "second idea" and the the "Layer Cake" as the first idea, though neither was so known at the time. On the Layer Cake see [Sakharov's memoirs]; David Holloway, Stalin and the Bomb, Yale University Press, 1994, 298-299; Gennadii Gorelik, Andrei Sakharov: Nauka i Svoboda, R&C Dynamics, 2000, 175-184.
[48] Goncharov, loc.cit, 1100.
[49] G.A. Goncharov, unpublished paper.

production of the materials needed for that (tritium, deuterium, lithium-6)."[50]

Stalin signed the decrees on the development of the hydrogen bomb and the production of tritium on 26 February 1950, the day he received the drafts. Both the Layer Cake (RDS-6s) and the Cylinder (RDS-6t) were to be developed. The Layer Cake was to have a yield of 1 MT, the Cylinder a yield of 1-1.5 MT.[51]

The United States Mike test of 1 November 1952 gave a new impetus to the Soviet project because it showed that the Americans were making progress. But it did not reveal information about the Teller-Ulam configuration, as some advisors feared at the time and some physicists later claimed.[52] In principle, analysis of the radioactive debris from the Mike test could have provided clues about staging and compression, but Soviet scientists did not conduct the kind of analysis that could have given them this information.[53] They pressed on with the Cylinder, which still presented problems, and with the Layer Cake, which they regarded as a workable design. On 15 June 1953 Tamm, Sakharov and Zeldovich signed the final report on the development of the Layer Cake. Two months later, on 12 August, the Soviet Union tested this design at the Semipalatinsk test site. The energy release was measured at 400 kilotons. Unlike the Mike assembly, the Soviet device was – or could be made into – a deliverable bomb, since it had the same dimensions as the first atomic bomb. The Layer Cake was not

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[50] Ibid.

[51] Ibid. It is interesting that the two designs were expected to be of almost equal explosive power. This may indicate that the distinction which later emerged between the designs was not seen at the time as being so important.

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based on a principle such as the Teller-Ulam configuration that would make it possible to build a bomb of almost unlimited explosive power. It was, however, different from the boosted fission weapon tested by the United States in 1951; it had more thermonuclear fuel and a greater proportion of its explosive yield came from thermonuclear reactions.[54] (It was what the British later called an "intermediate" bomb.)

Over two years later, in November 1955, the Soviet Union tested its first two-stage weapon. The path from the Layer Cake test of August 1953 to the November 1955 test was not a direct one. Sakharov believed that the Layer Cake could be modified to produce a yield of at least one megaton. In November 1953 he made a report to the government on the modified design. Two weeks later he was summoned to a meeting of the Politburo, which mandated the design and testing of the new version of the Layer Cake. Sakharov's proposed design proved unsatisfactory, however. In early 1954 the weapons institute at Sarov came to the conclusion that Sakharov's proposed modifications would not raise the explosive yield as much as he had claimed.[55]

By early 1954 the Soviet thermonuclear program was at an impasse. The Layer Cake, while successful, could not be modified to produce higher yields. Nor was Soviet work on the Cylinder any more successful than American work on the classical Super; by the end of 1953 research by different theoretical groups had demonstrated that it

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[53] Holloway, op.cit. 312; Goncharov, loc.cit. 1101.
[54] Holloway, op.cit. 305-309; Goncharov, loc.cit 1101.
[55] Sakharov memoirs; Holloway, op.cit, 313; Goncharov, loc.cit. 1101.

would not lead to a workable design.[56] The Soviet Union was in the same position in early 1954 as the United States in late 1950. Soviet scientists did not know how to make a superbomb; nor were they sure how to move forward.

The United States Bravo shot of 1 March 1954, with its explosive yield of 15 megatons, gave the Soviet thermonuclear program its final push. It soon became apparent that the Americans had solved the problem of designing the Super. Sakharov and Zeldovich and their teams now tackled the problem with new urgency. In March or April 1954 they came up with the idea of compressing the secondary thermonuclear core by means of the radiation energy from a primary atomic bomb. In other words, they hit upon the Soviet analog of the Teller-Ulam configuration.[57] Exactly how the Soviet physicists made this discovery remains unclear. The available documentary evidence provides no indication that they received intelligence information about the Teller-Ulam configuration. Nor were they led to it by radiochemical analysis of the Bravo test, since it appears that only the thermonuclear character of the test, and not the configuration of the device, was established by that analysis.[58] One participant in the Soviet research, Lev Feoktistov, has suggested, on the basis of a meeting he remembers attending at the time, that some information about Ulam's first formulation of the two-stage concept had reached the Soviet Union and helped lead to the new discovery.[59]

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[56] Goncharov, unpublished paper. Iu.B. Khariton, V.B. Adamskii, Iu.N. Smirnov, "O sozdanii sovetskoi vodorodnoi (termoiadernoi) bomby," in Nauka i obshchestvo…op.cit. 205-207.

[57] Sakharov refers to this in his memoirs as the "third idea."

[58] Goncharov, unpublished paper.

[59] .L.P. Feoktistov, "Vodorodnaia bomb: kto zhe vydal ee sekret?" in Nauka i Obshchestvo…op.cit. 229-230. In Ulam's early formulation it was the shock wave and neutron flux from the fission bomb rather than radiation that compressed the thermonuclear fuel.)

Whatever the process of discovery, it was immediately apparent to the Soviet physicists, as it had been to their American counterparts, that this was a breakthrough. They now devoted almost all their efforts to the two-stage design, which was tested successfully on 22 November 1955 at the Semipalatinsk test-site. The design yield was 3 megatons, but some of the thermonuclear fuel was replaced by passive material in order to halve the yield, which Soviet scientists estimated at 1.6 megatons. But the yield was not the main point: the significance of the test was that, as Sakharov wrote in his memoirs, it "had essentially solved the problem of creating high-performance thermonuclear weapons."[60] Like Bravo, this test too had tragic consequences. Because of a temperature inversion at the test site, the shock wave proved to be much more powerful than anticipated. A young soldier was killed at a distance of many kilometers when the trench he was in fell in and buried him. A two-year old girl was killed in a village outside the test site when the shelter she was in collapsed.[61]

Teller's fears about Soviet progress were not realized. Soviet scientists did not discover how to build the Super before the Americans did. They were no more able than their American counterparts to turn the concept of the classical Super into a workable design. Nor did they see in Fuchs's own ideas about radiation implosion the clue to what later became the Teller-Ulam configuration. Bethe was right to argue that Soviet scientists would not be able arrive at a workable thermonuclear weapon by straightforward development of the information they had received from Fuchs.

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[60] Memoirs, 193.
[61] Holloway, op.cit.313-317.

Fuchs's information was nevertheless extremely important for the Soviet program. He directed Soviet attention to the hydrogen bomb in 1945. The material he provided in March 1948 led to the setting up of Tamm's group, which quickly developed the concept of the Layer Cake. The Layer Cake was not a superbomb, but the test of 12 August 1953 allowed the Soviet Union to claim to have developed the first deliverable thermonuclear weapon. Besides, the Soviet Union did not have to start from scratch when Truman made his announcement in January 1950. Thanks to the independent development of some key concepts as well as the information received from Fuchs, Soviet physicists possessed a stock of ideas and concepts similar to those at the disposal of their American counterparts. It is true that the Cylinder (classical Super) proved not to work, but Soviet physicists had a set of ideas to which they could turn when they reached a dead end. The fact that they came up with the Teller-Ulam idea so quickly in the spring of 1954, apparently without any new intelligence information, suggests that the earlier work was important even though it did not lead directly to the Super. And if they did receive some hint about the Teller-Ulam configuration, as Feoktistov suggests, Soviet physicists were attuned to be able to understand and exploit it quickly. This suggests that the Teller-Ulam configuration was not quite as "accidental" as Bethe claimed, and that the ideas available to Soviet physicists in 1950 could be combined in such a way as to lead to the Super.

A Missed Opportunity?

An abiding question about this history is whether Truman missed an opportunity to halt the arms race when he authorized development of the hydrogen bomb in January 1950. If he had accepted the GAC's recommendation not to develop the superbomb, would the Soviet Union have reciprocated? If he had acted on the proposal from Fermi and Rabi to seek a thermonuclear test ban, would an agreement have been possible? The same question can be asked about the recommendation of a State Department Panel of Consultants on Disarmament in 1952 not to go ahead with the Mike shot and to seek a thermonuclear test ban.

Herbert York showed in 1976 that Truman could have accepted the GAC's advice without harming the national security of the United States.[62] If the United States had deferred a decision to develop the superbomb until after the test of the Layer Cake in August 1953, it would still have been able to test a multi-megaton weapon in late 1955 or 1956, at about the same time as the Soviet Union. York argued, further, that if Truman had taken the GAC's advice, the Soviet Union would probably not have been able to develop a two-stage weapon by November 1955; that is because York believed that the Mike test had spurred on the Soviet program. In other words, American restraint might well have slowed down Soviet development of thermonuclear weapons. Even if it did not have that effect, there was little risk to the United States in exercising restraint.

Sakharov was skeptical about the possibility that Stalin might have reciprocated American restraint in the development of thermonuclear weapons. In the late 1940s, he wrote in his memoirs, Stalin and Beria "already understood the potential of the new weapon, and nothing could have dissuaded them from going forward with its development. Any U.S. move toward abandoning or suspending work on a thermonuclear weapon would have been perceived either as a cunning, deceitful maneuver, or as evidence of stupidity or weakness."[63] In either case Stalin's reaction would have been the same: to press ahead with thermonuclear weapons in order to avoid a possible trap or to exploit an American mistake.

A similar argument can be applied to the 1949 and 1952 proposals for a thermonuclear test ban. No inspection would have been required, since it was assumed that a thermonuclear explosion could be detected beyond the borders of the country that carried it out.[64] But some agreement, even if informal, would have been needed for mutual restraint. Stalin might have agreed to talks in the hope of influencing US policy. But the political context was not propitious, especially after the outbreak of the Korean War, and negotiations require at least a minimum degree of confidence that agreement is possible. It is hard to conceive of the aging Stalin, whose characteristic suspiciousness had grown even stronger, entering into talks in the expectation that a satisfactory agreement could be reached. Yulii Khariton, when asked whether a test ban might have been concluded when while Stalin was alive, answered no.[65]

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[62] York, op.cit.
[63] Memoirs, 199 [check]
[64] For example, the United States understood the nature of the Joe-4 test very quickly by analyzing the radioactive debris.
[65] DH interview with Khariton, 12 March 1988.

Nevertheless, the most recent evidence about the Soviet program suggests that American restraint might indeed have slowed down the Soviet program. At the end of 1949 the Soviet Union was, as Sakharov writes, ready to go ahead with the development of thermonuclear weapons. But Beria seems to have delayed seeking Stalin's authorization, perhaps because he did not trust his scientists and was fearful of making a promise he could not keep – as had happened with the atomic bomb. Whatever the reason for Beria's hesitation, it was Truman's decision that provided the impetus for the Soviet Union to move forward. Soviet progress might have been slower if the United States had decided not to go ahead. Slower progress in developing thermonuclear weapons on both sides might have allowed time, after Stalin's death, to bring the race to develop nuclear weapons under control.[66]

The British Program to 1958

Publication of the official history of the British hydrogen bomb program provides a further point of comparison and thereby throws further light on the U.S. and Soviet programs.[67] In spite of the information it had about the April 1946 Los alamos conference, Britain did not pay serious attention to thermonuclear weapons until the early 1950s. John Corner and Herbert Pike of the atomic bomb group did some work on the classical Super in the late 1940s and concluded that it was not a practicable design

[66] This assumes that political decisions really determined the pace and scope of research. Truman's decision did not have the immediate effect of speeding up research in the United States; perhaps Soviet research, which had acquired some momentum, would have gone ahead without Stalin's decision.
[67] Arnold, op.cit.

and would be too expensive for Britain to try to develop.[68] Nuclear weapons research focussed on the atomic bomb. Neither the government nor the weapons scientists assumed that Britain would proceed automatically to develop thermonuclear weapons. Britain exploded its first atomic bomb in October 1952, three years after the Soviet Union, but this success was overshadowed within a month by the United States' Mike test. This showed that weapons that were immensely more powerful than the atomic bomb could be developed.[69] Churchill asked Lord Cherwell in February 1953 whether Britain was technically and financially capable of producing a hydrogen bomb. Cherwell replied that "we think we know how to make an H-bomb."[70] That was not true, however. The scientists at the Atomic Weapons Establishment at Aldermaston were fully occupied with atomic bomb program and had not yet done sustained work on thermonuclear weapons.

Serious discussion of thermonuclear weapons began early in 1954. The initial impetus came from the government's desire to know what the implications of the new weapon would be for civil defense. This interest was reinforced in February when Sterling Cole, chairman of the Joint Committee on Atomic Energy, stated publicly that the United States now had a hydrogen bomb more powerful than Mike. The Bravo test of 1 March 1954 gave additional urgency to the British discussions, which took place against the background of anxiety and alarm caused by the United States tests in the Pacific. A series of consultations took place with the Chiefs of Staff, the Foreign Office

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[68] Arnold, op.cit. 41.
[69] Mike seems to have made a bigger impression on the British than on the Soviet leaders and scientists.

and leading nuclear scientists. The Defence Policy committee approved production of thermonuclear weapons on 16 June, and the Cabinet finally decided on 26 July "to proceed with…plans for the production of thermonuclear weapons in this country."[71]

A small group led by John Corner, head of the theoretical physics division, was formed at Aldermaston in April 1954 to work on the Super. The "astrophysics committee," as it was known, started by gathering all the information it could on "the 1946 position," including the papers and notes of Klaus Fuchs. At the time the British did not know how to design a superbomb. It was only two years later that the basic ideas came together – that "the light was switched on," in Corner's words. What happened during those two years is impossible to reconstruct with any certainty. "Progress towards the H-bomb was both incremental and uncertain, sometimes advancing and sometimes retreating," writes Lorna Arnold, the official historian of the British H-bomb project.[72] "Penney… produced a series of ideas and frequently changing sketches.[73] But where the essential ideas came from, how they were brought together, and how the design really evolved is something of a mystery." At the end of 1954 the scientists at Aldermaston were still quite unclear about the design of a superbomb. They were therefore instructed by the Chiefs of Staff to concentrate on developing a heavily boosted fission bomb with a yield of one megaton; the superbomb, whatever its design might be, now had a lower priority. These uncertainties did not prevent the government from making public, in the Defence White Paper published in

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[70] Arnold, op.cit. 37.
[71] Ibid. 39-57; PRO CAB 128/027 C.C. (54) 53rd Conclusions, 16 July 1954, p. 3.

February 1955, its decision to proceed with the development and production of thermonuclear weapons.[74]

By September 1955 Britain apparently had two of the key ideas. Penney outlined the elements of a staged bomb to a small group of scientists at Aldermaston in September 1955. He talked of two-stagers, which might have a yield of one megaton, and three-stagers, which would give larger yields, referring to the three stages in the latter bomb as Tom, Dick and Harry. According to Herbert Pike, who was at the meeting, "it was thought that a simple fission device was inadequate for compressing thermonuclear fuel but could be used to implode a much more powerful U235 device."[75] (The idea of a three-staged bomb was soon dropped, but the names remained – Tom for the primary, and Dick for the secondary.) In the same month Penney made a note to himself about the role that radiation transfer could play in compressing the thermonuclear fuel.[76] The prospects for the superbomb now looked more promising, and its priority was restored. The Chiefs of Staff agreed that it should be developed and tested in 1957 along with boosted weapons. Several more months were needed, however, for the Aldermaston scientists to come up with the British analog of the Teller-Ulam configuration. It was in April 1956 that the basic features of the superbomb

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72 Arnold, op.cit. 93.
73 William Penney was head of the Atomic Weapons Research Establishment at Aldermaston.
74 Statement on Defence, HMSO, CMd 9391, 1955.
75 Arnold, op.cit. 85.
76 Ibid. 86.

design – the fission primary, the secondary with its solid thermonuclear fuel, and radiation implosion – fell into place.[77]

Between May 1957 and September 1958 Britain conducted nine test explosions in the Pacific. A base was constructed on Christmas Island, and most of the explosions took place near Malden Island 450 miles away. Both boosted fission and two-stage devices were tested. A spherical thermonuclear bomb analogous to the Soviet Layer Cake was transported to Christmas Island but not tested. The first test of a two-stage bomb was conducted on 15 May 1957, with a yield of 300 kilotons; a modified version of the same design was tested on 19 June, with a yield of 200 kilotons. These tests demonstrated the principles of staging and radiation implosion, but the results were disappointing. On 8 November Britain tested a further modified two-stage device, which yielded 1.8 megatons. Another test in April 1958 produced a yield of three megatons.[78]

The United States did not give Britain direct assistance in developing thermonuclear weapons, since the McMahon Act was still in force. There were, however, a number of channels of indirect help. The first of these was Klaus Fuchs, whose notes and papers the British scientists studied carefully. Quite how helpful these were is unclear because they were later destroyed; we do not know whether Fuchs had developed new ideas about the superbomb after leaving the United States. It is apparent, however, that Fuchs's papers contained detailed information about the state

[77] Ibid. 93.
[78] Ibid. 236.

of research in the United States in 1946. Thus the difficulties the British had in designing the hydrogen bomb confirm Bethe's contention that the ideas of 1946 would not lead in a straightforward way to the Teller-Ulam configuration.

The second channel was Penney himself, who had worked at Los Alamos during the war and was well regarded in the United States as a specialist on weapons effects. (He spent the summer of 1946 in the United States working on the analysis of the Bikini tests.)[79] Penney picked up some ideas, in official and unofficial ways, on his trips to the United States. According to his Royal Society biography, "he kept in close touch with his American friends and was able to sit up drinking with them half the night. There is little doubt that he and he alone gleaned in this way much information about the American programme."[80] It is difficult to say how helpful this information really was. He certainly did not acquire a clear picture of the design of the hydrogen bomb. One of his scientists later commented that Penney did not get enough detail and sometimes misinterpreted what he had heard.

The third, and evidently most helpful, source of information was the analysis of radioactive debris from the American and Soviet test explosions. In 1952-53 the Americans had provided British scientists with "certain snippets of information," in Penney's phrase, to make possible joint evaluation of Soviet test explosions.[81] This was compatible with the 1948 Anglo-American <u>modus vivendi</u>, which allowed for

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[79] Cathcart?

[80] By Lord Sherfield (Roger Makins), who played a key role in Anglo-American nuclear relations. It is conceivable that the AEC may have wanted to help the British, notwithstanding the McMahon Act, and passed helpful hints to Penney. I know of no evidence for this, and the fact that Penney often returned with garbled information suggests that it is not true.

cooperation in the detection of "a distant nuclear explosion."[82] This limited information stimulated British thinking on the design of thermonuclear weapons in 1954. By arrangement with the United States, the British collected samples from the American tests in the Pacific in the spring of 1954. They also analyzed the debris from the Soviet tests of August 1953 and November 1955. For some reason, however, the analysis of the 1954 U.S. tests did not reach the Aldermaston scientists until late 1955. Nevertheless, the analyses of foreign tests – especially the United States Pacific series and the Soviet test of November 1955 – were important in the early part of 1956. These analyses apparently did not suggest new approaches but clarified and confirmed British ideas about the mechanism of the two-stage bomb.[83]

The Three Projects in an International Context

International rivalry was the major driving force behind the hydrogen bomb projects. Truman's decision to develop the Super was precipitated by the Soviet atomic bomb test. "Can the Russians do it?" he asked at the meeting on 31 January 1950. The possibility that the Soviet Union might make the hydrogen bomb was the most compelling argument for going ahead with the American program, and the advocates of the Super exploited it to the full. Beria made the same point from the other side in his letter of 26 February 1950 to Stalin: it was important to develop the hydrogen bomb because "our enemies may have in their hands a new, very effective weapon." Neither

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[81] Arnold, op.cit. 41.

the United States nor the Soviet Union wanted the other to develop the hydrogen bomb first, for fear of being subjected to political intimidation or even military attack. Britain's reasons were more complex: it wanted both to deter the Soviet Union and to influence the United States. The public rationale stressed the former goal, but ministerial discussions gave more weight to the latter. The British continued to cherish the idea that, notwithstanding their declining power, they could exercise a wise and restraining influence on American policy. According to the minutes of the Cabinet meeting on 8 July 1954, "at present some people thought that the greatest risk was that the United States might plunge the world into war either through a misjudged intervention in Asia or in order to forestall an attack by Russia. Our best chance of preventing this was to maintain our influence with the United States Government; and they would certainly feel more respect for our views if we continued to play an effective part in building up the strength necessary to deter aggression than if we left it entirely to them to match and counter Russia's strength in thermo-nuclear weapons."[84]

Important though international rivalry was, it does not explain the actual pattern of hydrogen bomb development. Thanks to Klaus Fuchs, each of the three projects had a common starting point in the classical Super design discussed at Los Alamos in April 1946. In each country the projects led to development of the same design: a two-stage weapons employing radiation implosion to compress and ignite the thermonuclear fuel. How far the designs may have differed from one another is impossible to say because

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[82] Margaret Gowing, Independence and Deterrence: Britain and Atomic Energy 1945-1952 Volume 1 Policy-Making St Martin's Press, 1974, 272.

their details remain classified. It is striking, however, that each country came up with the same design concept, even though that concept did not follow from the information they had in common – from what the British called "the 1946 position."

Teller's fear that Fuchs's information about the 1946 design might lead the Soviet Union directly to the superbomb proved to be unfounded. Bethe's characterization of the Teller-Ulam configuration as "accidental" is unsatisfactory too. When it became clear that the Soviet Union had tested its own version of the Teller-Ulam configuration, Bethe rejected the idea that it could have done so independently. He argued that the Soviet Union had obtained important clues to the design of the hydrogen bomb by analyzing the radioactive debris from the Mike test of November 1952.[85] Since Britain obtained useful information from analysis of the debris from the first American and Soviet thermonuclear tests this explanation is plausible, but it appears not to be true. Soviet scientists did not conduct the kind of analysis of the Mike test that would have provided them with useful information. Nor is there evidence that analysis of the American tests in the Pacific in the spring of 1954 played a role in the development of

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[83] Arnold, op.cit. 41, 91

[84] PRO CAB 128/027 CC(54) 48th Conclusions p. 4.

[85] DH interview with Bethe, 28 May 1982; Daniel Hirsch and William G. Mathews, "The H-Bomb: Who Really Gave Away the Secret?" Bulletin of the Atomic Scientists, January/February 1990 23-30; a longer version of this article was published as Fuchs and Fallout: New Insights into the History of the H-Bomb, Los Angeles: Committee to Bridge the Gap 1990. Isotope analysis would show that the thermonuclear fuel had been subject to a very high degree of compression. Analysis of the debris – and in particular of the effect on the fission primary of the tremendous neutron flux produced by the thermonuclear reactions – could also show that the primary was physically separate from the thermonuclear fuel. In other words, it would be clear that Mike was a two-stage device; it would not be clear from the analysis that radiation implosion was the mechanism used to compress the thermonuclear fuel. See Lars-Erik de Geer, "The Radioactive Signature of the Hydrogen Bomb," Science and Global Security, 1991, vol. 2, 351-363.

the Soviet hydrogen bomb, beyond establishing that they were indeed thermonuclear tests and thereby adding to the urgency of the Soviet program.

German Goncharov, who has written the most detailed analyses of the Soviet thermonuclear program, offers a different hypothesis. By early 1954 the Soviet weapons scientists had reached an impasse. The Bravo test showed that the United States had succeeded in developing a superbomb. Under pressure to match the American success, Soviet weapons scientists turned again to the concepts and ideas they had worked with over the previous eight years. In their intense search for a solution, Goncharov argues, they realized that radiation implosion, which they had learned about in Fuchs's original conception, could be used to compress and ignite the thermonuclear secondary in a two-stage weapon. Goncharov has no direct evidence to support this argument, but it seems plausible, just as it seems plausible that Teller might have been influenced by Fuchs's idea when he made his contribution to the Teller-Ulam configuration.[86]

Fuchs may have given the Soviet Union and Britain an unworkable idea when he informed them about the classical Super, but he focussed their attention on the superbomb with an indefinitely large explosive yield. And as Goncharov points out, the information he provided became part of the stock of ideas and data with which weapons scientists in the Soviet Union and Britain worked. Those scientists were already following in the footsteps of the Los Alamos scientists. In the years after World

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[86] It provides a way of understanding the role that Fuchs's information played in the development of the Soviet superbomb. (It also, incidentally, enhances the importance of Fuchs as a physicist as well as a spy.)

War II the Soviet and British scientists and engineers were developing copies of the Nagasaki bomb, about which they had detailed design information. The scientists who received Fuchs's information were committed to making nuclear weapons and intensely interested in what was being done in the other countries. In this setting Goncharov's argument makes sense. Scientists in the three countries were working with similar goals, similar experience, and similar concepts. This suggests that, to understand the early hydrogen bomb projects, it is important not only to take account of international rivalry. It is just as important to understand what the three countries had in common (even though what they had in common in this case was the product of international rivalry). They had teams of scientists and engineers who were capable of completing the immensely difficult task of designing and developing thermonuclear weapons. These scientists also studied the effects of the hydrogen bomb and duly impressed, in the tests they conducted, by just how destructive it was.

The Hydrogen Bomb as a Turning Point

The first thermonuclear tests had a profound effect on political leaders. Eisenhower, who had been briefed on the Mike test in November 1952, after his election, declared in his inaugural address in January that "science seems ready to confer upon us, as its final gift, the power to erase human life from this planet."[87] Khrushchev recalled tens years later that when he had become First Secretary of the Central Committee in September 1953 – one month after the first Soviet thermonuclear test – he had been briefed on nuclear weapons: "when…I learned all the facts about nuclear power I couldn't sleep for several days."[88] Georgii Malenkov said on 12 March 1954 that a world war with modern weapons would mean the end of world civilization. Three days earlier, on 9 March, Churchill had written to Eisenhower about the hydrogen bomb. Human minds recoiled from it, he wrote, but "the few men upon whom the supreme responsibility falls… have to drive their minds forward into these hideous and deadly spheres of thought."[89]

This sense of foreboding was very widely shared. The hydrogen bomb decisions had been taken in great secrecy by small groups of scientists, administrators and military and political leaders. Truman had announced his decision on the day he took it. Malenkov had made a public statement several days before the Layer Cake test in August 1953. Britain had made its decision public in February 1955, six months after it had been taken. But it was the tests that stirred public anxiety. Lawrence Wittner notes that a "rising sense of uneasiness about nuclear weapons emerged around the world" as a result of the development of the hydrogen bomb.[90] The bombing of Hiroshima and Nagasaki had generated a wave of anti-nuclear sentiment, sustained by scientists, pacifists, and advocates of world government. But the failure of international control, as well as the tensions of the Cold War, had weakened the anti-nuclear movement by

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[87] Hewlett and Holl, op.cit. 1, 34.
[88] Holloway, op.cit. 336, 339.
[89] Peter Boyle, ed., The Churchill-Eisenhower Correspondence, 1953-1955, University of North Carolina Press, 1990, 123-124.
[90] Lawrence Wittner,

the late 1940s. Soviet-led peace campaigns, which supported Soviet policy without question, generated controversies that further undermined the movement.

In 1954 the situation began to change, as information about thermonuclear weapons tests – and especially about the American tests in the Pacific – became available. The danger of radioactive fall-out from tests became the focus of anti-nuclear movements in many countries in the second half of the 1950s and the early 1960s. In July 1955 Bertrand Russell made public the Russell-Einstein manifesto, which he and Albert Einstein had written, calling for the abolition of war. Signed by a small group of scientists from Britain, France, Japan, Poland and Germany, as well as the United States, the manifesto declared that "the best authorities are unanimous in saying that a war with H-bombs might possibly put an end to the human race." Further international appeals were issued in the following years. Popular movements to ban the bomb gained considerable strength throughout the noncommunist world.

The development of the hydrogen bomb was a turning point in the Cold War and in the history of international relations. States were acquiring weapons that could, under certain conditions, destroy all life on earth. In each of the three countries, weapons scientists advised their governments that thermonuclear weapons could cause destruction of an unprecedented kind. A common understanding began to emerge among the political leaders of the nuclear powers that general war was unacceptable, that there was no political purpose that such a war could rationally serve. The question that then arose was what role this common understanding could play in the management of nuclear weapons and nuclear relationships.