Chapter Four

Making Light of the Light Elements

Although itself a significant technical obstacle to the H-bomb project, Los Alamos found computing as only one of several critical problems. Other problems arose, as well, and weapons scientists acknowledged them at various times. Von Neumann, Teller, Wheeler and others early on established computing as a technical problem that stood in the way of understanding the Super configuration's feasibility. Nuclear materials were also a bottleneck to the hydrogen weapon program, yet Los Alamos's scientists recognized this problem later than they had the computing obstacle. Tritium in particular, from the time Konopinski had suggested incorporating this isotope into the Super theory, was a latent obstacle to the H-bomb program. After the Soviet Union detonated its first atomic weapon in 1949 tritium scientists began to view tritium as a serious critical problem facing the American thermonuclear project.

Although the Russian A-bomb test represented in the United States a political event outside of the AEC technological system -- this event nevertheless forced both scientists and policymakers to reconsider the AEC's pace and the intensity of nuclear weapons research. Only then the Commission called its materials production facilities into question. After President Truman instructed the AEC to explore further the hydrogen weapon in 1950, and when Ulam and his colleague's calculations began to show the ignition problems facing the Super, the tritium problem became blatant. Consequently, the Committee for Weapon Development demonstrate with reasonable certainty that the 1945-1946 ENIAC calculations were wrong.

In 1949 the AEC found itself unprepared to begin a program of large-scale tritium production in part because its predecessor, the MED, had not constructed any facilities specifically for this purpose. Instead, fission weapons had been the MED system builders' priority during the war. These weapons demanded Pu^{239} and U^{235}, thus T production had not been built into the wartime production infrastructure. Also, in addition to the AEC's having inherited a materials production system already oriented nearly exclusively towards the creation of fission weapons materials, the Commission's scientific advisors did not recommend any drastic changes in the production part of the system in the latter 1940s.

Why did tritium remain unrecognized as a critical problem by weapons scientists for several years? First, before 1949 the Joint Committee and American military leaders had few reasons to criticize the AEC and its weapons laboratory; the rate at which the Commission developed nuclear devices appeared sufficient. Second, because Los Alamos poorly understood both the Super and Alarm Clock theories, tritium remained a latent, or unobvious critical problem. Third, Teller, Metropolis, Frankel, and Turkevich all far underestimated the amount of T necessary to ignite the Super. On one hand, the Hanford reactors could produce a few hundred grams of in a few years as long the Commission would be willing to sacrifice the fission program. On the other hand, the AEC could not produce a few thousand grams of T.

The AEC production system could not immediately make a technical response to the Russian atomic bomb, and neither could Los Alamos. In the year after the Russian test the laboratory did not determine the feasibility of a thermonuclear weapon -- no one proved the Super or Alarm Clock viable or not with definite certainty. However, Ulam and Everett, and von Neumann and his team employing the ENIAC began to show the tritium problem as an outstanding obstacle to Los Alamos's H-bomb program. If tritium proved a formidable obstacle to the thermonuclear program, either the AEC would have to alter its production system drastically to meet the enormous tritium requirements of a Super, or scientists would have to circumvent the tritium problem. The former approach would be the Commission's responsibility, and the latter Los Alamos's. The Laboratory, not the AEC, solved the dilemma of finding the fastest approach to a hydrogen bomb, and in this way Los Alamos remained ahead of, but was also constrained by the larger technological system, Los Alamos's staff bypassed the tritium problem, although only after spending several years pondering just how much T the Super would require.

Detecting Tritium

In twentieth century physical science, nuclear transmutation studies produced thousands of radioisotopes of commonly known elements. While

Henry Cavendish identified hydrogen as a distinct substance by Henry Cavendish in 1766, and named by Antoine Lavoisier, its radioisotopes remained undetected for another century and a half. One such radioisotope and low-energy beta emitter, tritium, or hydrogen-3, was identified in the 1930s although it's discovery involved the work of several well-known scientists including Lord Rutherford, Luis Alvarez, Ernest Lawrence, and others.

By this time Harold Urey had already isolated the stable isotope of hydrogen (for this work he won the Nobel Prize), deuterium (H^2), using an electrolytic method to isolate deuterium oxide or heavy water from natural water. Consequently, Lord Rutherford thought that "triterium," as he referred to it, could most easily be isolated from heavy water. Rutherford then bombarded heavy water with a beam of deuterons accelerated by Cockroft-Walton accelerators. Two products resulted, both with mass number 3: Tritium, and Helium-3 (He^3).[288]

Rutherford mistakenly thought that tritium was the stable isotope and Helium-3 the radioactive one. Subsequently, the Cavendish Laboratory persuaded the Norwegian Norsk Hydro heavy water plant to concentrate tritium oxide by the electrolytic process, from which the Cavendish received 11 grams of the remains of 13,000 tons of heavy water. Rutherford, and

[288] John L. Heilbron and Robert W. Seidel, Lawrence and his Laboratory: A History of the Lawrence Berkeley Laboratory, Volume 1, (Berkeley: University of California Press, 1989), 368-369.

Francis Aston, inventor of the mass spectrometer, could not find tritium in the sample.[289]

American scientists investigated tritium as well. Young Berkeley physicist and colleague of Lawrence, Luis Alvarez, recognized Rutherford's mistake in his conclusion that tritium was stable. In 1939, using Lawrence's cyclotrons, Alvarez and a graduate student found radioactive H^3 in the product of D-D reactions passed into an ionization chamber. Pleased with the Berkeley Radiation Laboratory's discoveries in 1939, Lawrence wrote that "Radioactively labeled hydrogen opens up a tremendously wide and fruitful field of investigation in all biology and chemistry."[290]

Tritium would also be of tremendous consequence for nuclear physics, especially after the war. Like computing, nuclear materials, their properties and rate and ease of production affected the course and pace of thermonuclear weapons research and development. Unlike computing, materials production remained for the most part outside of Los Alamos control. Whereas the Laboratory's own employees and consultants initiated many computer-building projects machines procurement efforts in the postwar years, materials production had already been set up an integral part of the larger technological system.

Production of fuel for nuclear weapons became a technical cornerstone of -- along with the most expensive parts within -- the MED system early on in the Manhattan Project, with Groves the system builder behind the facilities

[289] Ibid., 370.

geared towards the manufacture of Plutonium and Uranium-235. Lawrence came up with the production process for U^{235}; the electromagnetic uranium separation plant built at Oak Ridge was based on Lawrence's cyclotron construction at Berkeley.

From the beginning of the Manhattan Project, Lawrence had a stake in the materials production portion of the project. The idea of going ahead with a large uranium production plant can be traced back to 1941 when the National Academy of Sciences Committee on Uranium -- made up of Lawrence, Compton, Van Vleck, and several others -- recommended this. Furthermore, Bush had assigned Lawrence sole scientific responsibility for developing a large-scale means of separating isotopes, and he wasted no time in taking actions towards his own interests.[291]

In December 1941 Lawrence convinced the S-1 Committee to give him $400,000.00 to convert the 37-inch cyclotron into a mass spectrograph for separating U^{235}. Initially the Scientific Planning Board did not know which of several types of proposed uranium separation methods to support. Besides electromagnetic separation, other potential means of separating U^{235} from U^{238} included gaseous diffusion, a hydrogen-water exchange process, and thermal diffusion. Lawrence convinced Conant, however, that the electromagnetic method for separating uranium constituted the "best bet" for producing fissionable material in the interest of time, by the end of 1944.[292]

[290] Ibid., 372; Quote in Heilbron and Seidel, Lawrence and his Laboratory, 373.
[291] Hewlett and Anderson, The New World, 36, 49-50.
[292] Ibid., 52, 104.

With his typical enthusiasm, Lawrence won Groves's support as well, after convincing the General that electromagnetic separation of Uranium was the best method. After Groves's appointment as military head of the atomic project in September 1942, Lawrence courted Groves's for his support of the electromagnetic separation method by giving the General a tour of the 184-inch cyclotron under construction at the Radiation Laboratory. As Hughes has pointed out, Groves and Lawrence reacted "sympathetically" to each other from the start, and thus Groves agreed to build an electromagnetic plant at Oak Ridge, awarding the construction contract to Stone and Webster.[293]

Almost immediately after Groves assumed military leadership of the atomic project, he negotiated contracts with at least a half dozen of the U.S.'s largest industrial corporations. Plutonium production on an industrial scale, like uranium separation, required industrial-size facilities in the form of nuclear reactors. To build these reactors Groves brought the du Pont corporation into the MED system because he had worked with the company previously in the construction of military explosives. Spreading the system far and wide geographically, Groves chose Hanford, Washington, as the site for du Pont to begin construction because of the region's isolation, its far distance from Oak Ridge, and its proximity to the Columbia River because the reactors required a large source of cool water.[294]

Du Pont built three piles, each producing 250,000 kilowatts of heat. Together, the piles consumed about the same amount of water as a city of one

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[293] Hughes, American Genesis, 407-408.

million people. The reactors' operators produced plutonium by irradiating uranium slugs, then removing them to adjacent separation plants where the Pu was extracted chemically via a bismuth-phosphate process. However, Hanford produced little Pu^{239} before the end of 1944, in part because of xenon "poisoning" of the piles, a phenomenon identified by John Wheeler. Du Pont corrected the problem by providing excess uranium for the piles, overriding the poison effect.[295]

Hanford managed to produce enough Pu^{239} by spring 1945 for the Fat Man device. This was the only "standard" set for Pu production during the war. Hanford's only purpose was to satisfy Los Alamos's requirements for an implosion bomb. No one established materials production standards beyond the wartime effort, and this bothered Groves. The U-235 plant and the Hanford Pu^{239} piles were, according to Hewlett and Anderson, Groves's most urgent concerns in 1945 and 1946. As with the MED's other facilities such as Los Alamos, the MED built the materials production plants solely for the war, with little thought given to their purpose for the long-term.[296]

The Army's corporate contracts with the major production facilities were supposed to terminate six months after the end of the war. With no successor to the MED operating in 1946, Groves extended the operating contracts with Carbide and Tennessee Eastman at Oak Ridge, and tried to negotiate a similar extension with du Pont at Hanford. Du Pont did not want

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[294] Hewlett and Anderson, The New World, 115, 105, 184.
[295] Ibid., 216; Hughes, American Genesis, 401-402.
[296] Hewlett and Anderson, The New World, 624.

to continue to run Hanford, however, and thus Groves approached General Electric, whose leaders agreed to take over operating the Hanford reactors and perform plutonium recovery.[297]

Cyclotrons or Reactors?

Like Groves, system builders at Los Alamos gave thought to materials production early on. Los Alamos's interest in tritium production stemmed from Teller and his colleagues' work on the Super theory in 1944. Oppenheimer initially entertained the idea of producing tritium, and in May 1944 met with Groves and Crawford Greenewalt, a du Pont chemical engineer who acted as a liaison with the Metallurgical Laboratory, to discuss T production. In reporting the meeting to Samuel Allison as the Metallurgical Laboratory, Oppenheimer mentioned that he, Groves, and Greenewalt agreed:

> [I]t would be wise to divert the excess k (reproduction factor) of the Hanford pile to the production of tritium, which is, as you know, a material very likely to prove most useful to us. I am formally requesting of the Metallurgical Laboratory that it advise the du Pont Company on methods of accomplishing this.[298]

Oppenheimer recommended to Allison that lithium be introduced into the channels in the pile to obtain tritium, yet the director of Los Alamos knew well, along with Groves and Greenewalt, that they could not jeopardize the normal operation of the piles.[299] Producing Pu would remain first priority to meet the accelerated implosion weapon program at Los Alamos.

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[297] Ibid., 628, 692.

[298] Letter from J. Robert Oppenheimer to Samuel K. Allison, May 27, 1944, 635 Los Alamos, Box 19, Folder 5, LANL Archives.

[299] Ibid.

Allison visited Los Alamos in late summer of 1944, where he observed that Oppenheimer, Teller, and Bretscher appeared the most eager among their colleagues for increased tritium production. By 1945 Bretscher headed the F-3 group, experimenting with tritium as fuel for the Super and measuring cross sections of the isotope. From Los Alamos, Bretscher himself corresponded with Allison, explaining that for further work in studying D-T interactions, including the absolute cross section, angular variation of alpha particle distribution and the variation of total alpha particle yield with bombarding energy, the production of tritium should be put on a "more permanent and efficient basis." Furthermore, Bretscher continued, virtually nothing was known about T-T interactions, and studies of this, as well, would require more T production.[300]

Allison considered Bretscher's requests -- after the war's end. In August Allison wrote that "all work on tritium is part of the post-war effort," and he would look into the possibility of producing T at Hanford.[301] Still the question of where Tritium would be produced remained open after the war, and Lawrence took advantage of this, suggesting that the Berkeley cyclotrons could be used to produce tritium. Lawrence had his Radiation Lab colleague Robert Cornog estimate costs for this process as compared to doing so in a reactor. If produced in a pile, Cornog estimated that tritium would cost an exorbitant $40,000.00 per gram. In a pile, reported Cornog, the most desirable

[300] Letter from Samuel K. Allison to H.L. Doan, September 28, 1944, 470.1 Tritium, Box 16, Folder 7, LANL Archives; Letter from Egon Bretscher to Samuel K. Allison, May 12, 1945, 470.1 Tritium, Box 16, Folder 7, LANL Archives.

material could be formed by the capture of pile-formed thermal neutrons by lithium:

$$L^6_3 + n^1_0 \rightarrow He^4_2 + T^3_1 + Q_1$$

In a specially modified cyclotron, Cornog estimated it would cost the same per gram of T as creating it in a reactor.

Any way that tritium could be produced would be very expensive.[302] Despite this, Los Alamos requested some small quantities of tritium right soon after the war. At this time the Clinton Laboratory at this time produced some tritium designated for Los Alamos. Meanwhile, Clinton entertained a new proposal made by some members of the Metallurgical Laboratory and the Institute for Nuclear Studies at the University of Chicago involving modification of the Hanford piles to produce H^3. Farrington Daniels, head of the Metallurgical Laboratory, promised Bradbury that the first quantities of H^3 produced at Hanford would go to Los Alamos, in connection with possible military use of the isotope.[303]

Production System

As David Hounshell has stated, Hanford stood out as the largest single construction project of the war, and the biggest component of the wartime

[301] Letter from Samuel K. Allison to R.L. Doan, August 24, 1945, 470.1 Tritium, Box 16, Folder 7, LANL Archives.
[302] Memorandum from Robert Cornog to Ernest O. Lawrence, September 11, 1945, 470.1 Tritium, Box 16, Folder 7, LANL Archives; Letter from Cornog to Lawrence, September 11, 1945, 470.1 Tritium, Box 16, Folder 7, LANL Archives.
[303] Letter from Norris Bradbury to Col. A.V. Peterson, November 30, 1945, 701 Tritium, Box 16, Folder 7, LANL Archives; Letter from Farrington Daniels to Norris Bradbury, February 13, 1946, 470.1 Tritium, Box 16, Folder 7, LANL Archives.

system. Du Pont and its contractors employed about 60,000 people and created a city almost overnight. As one of the most expensive system components that the MED built, Groves had a vested interest in keeping the facility operating after the war.[304]

When Congress introduced the May-Johnson legislation, Groves reported to Secretary of War Robert Patterson that the delay in getting legislation passed was a "constant source of embarrassment to his operations." A year earlier, Groves had appointed a Committee on Postwar Policy, made up of W.K. Lewis, a chemical engineering professor from MIT, Rear Admiral E.W. Mills, Assistant Chief of the Bureau of Ships, Henry Smyth, and Richard Tolman of the NDRC. By December the Committee made several technical recommendations to Groves: the Government should make arrangements for continued development and operation of the existing plants for U^{235}, and continued study and operation of the graphite piles for the manufacture of Pu. In addition, the Committee stressed that future research should focus on improved production piles giving consideration to alternative moderators and coolants such as heavy water.[305]

Not surprisingly, the Committee on Postwar Policy sought Oppenheimer's advice on making its recommendations to Groves. Yet, by

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[304] David A. Hounshell, "DuPont and the Management of Large-Scale Research and Development," in Big Science: The Growth of Large-Scale Research, eds. Peter Galison and Bruce Hevly, (Stanford: Stanford University Press, 1992), 236-261.
[305] "Notes of a Meeting in the Office of Secretary of War Concerning Atomic Energy Legislation," September 28, 1945, RG 77, Harrison Bundy Files Relating to the Development of the Atomic Bomb, 1942-1945, National Archives Microfilm Publication M1108, Roll 5, Files 65-71 (Hereafter H-B Files); "Report of Committee on Postwar Policy," December 28, 1944, RG 227,

the latter half of 1944 Oppenheimer got in touch directly with Tolman about the future of nuclear weapons. To those working at the Laboratory, Oppenheimer relayed, it seemed a "reasonable assumption that we will succeed in making some rather crude forms of the gadget per se, but that the whole complex of problems associated with the super will probably not be pushed by us beyond rather elementary scientific considerations." Oppenheimer continued:

> I should like . . . to put in writing at an early date the recommendation that the subject of initiating violent thermo-nuclear reactions be pursued with vigor and diligence, and promptly. In this connection I should like to point out that gadgets of reasonable efficiency and suitable design can almost certainly induce significant thermo-nuclear reactions in deuterium even under conditions where these reactions are not self-sustaining, and that it is a part of the program of Site Y to boost the yield of gadgets by this method . . . . it is of great importance that such boosted gadgets form an experimentally possible transition from the simple gadget to the super and thus open the possibility of a not purely theoretical approach to the latter.[306]

Any long-term plans for fusion bomb development would depend on the establishment of long-range plans for the MED's production plants. The future of these plants was one of several topics discussed at the May 31, 1945 meeting of the Interim Committee on Postwar Planning, where Lawrence forcefully recommended that a plant expansion program be pursued and at

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Office of Scientific Research and Development (Hereafter OSRD), S-1 Files, Files of Richard C. Tolman, Box 6, Folder titled "Postwar Policy Committee File Report," NARA II.

[306] Letter from Oppenheimer to Tolman, September 20, 1944, RG 77, MED Records, Box 61, File "Post War Policy Committee Correspondence," Entry 5, NARA II.

the same time a sizable stockpile of bombs and material be built up. Research, Lawrence professed, would go on unhindered.[307]

His views not unheard, Lawrence's colleagues on the Scientific Panel to the Interim Committee, Oppenheimer, Fermi, and Compton, made numerous technical recommendations for the postwar period. In addition to suggesting that thermonuclear research continue, the panel cited pile development as crucial, particularly in the case of "breeder" reactors to produce fissionable materials.[308]

Between the Scientific Panel's views and his own convictions, Groves managed to keep the production plants operating in the transitional period from summer 1945 through 1947. When considering the physical condition of the Hanford plants in the postwar period it becomes more apparent, historically, that the MED's facilities were not set up as permanent fixtures. Both the Oak Ridge and Hanford facilities required constant maintenance to keep up steady production of materials. In the case of Hanford, Hungarian physicist Eugene Wigner predicted that graphite would expand when subjected to heavy neutron bombardment, severely shortening the life of the piles. Hanford's reactors had already greatly deteriorated by 1947.[309]

Hughes claims that the role of General Electric during the AEC's early years reveals the labyrinthine character of this system. Because General Electric took over operations of a facility designed and built by another

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[307] Minutes of Interim Committee Meeting, May 31, 1945, RG 77, MED Records, Microfilm Collection M1108, NARA II.

company, and Hanford fell into disrepair by 1947, General Electric's slow managerial approach to solving Hanford's problems forced the Commission to both maintain a stable level of materials production, and also learn to manage corporate contractors to keep the system in balance.[310]

By spring 1947 the Commission and GAC planned to build the weapons stockpile based on Pu fuel, thus set additional reactor development at Hanford as a high priority, along with the development of the "redox" process for recovering Pu, proposed by Seaborg and some of his colleagues. Redox would recover Uranium as well as Pu from the irradiated slugs, and would help provide additional U to feed the reactors. The GAC wanted to construct five new reactors over the course of two years, yet the Committee feared that the existing units at Hanford would not last for that duration of time. Therefore, Hewlett and Duncan have argued, the new reactors would not truly provide an overall increase in Pu production.[311]

"Practicable" Investigation but a Fantastic Venture

The GAC's plans for reactor improvement had been based on Bradbury's postwar atomic weapons program. In the tenuous period between the end of the war and the AEC's takeover of the MED's facilities, Bradbury assured the Laboratory Coordinating Council and those scientists who chose to remain at Los Alamos that weapons development and stockpiling would continue with a focus on more reliable weapons, modifications in fusing, and

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[308] "Recommendations on Future Policy," June 16, 1945, in JCAE declassified General Subject Files, op. cit.
[309] Hewlett and Anderson, The New World, 630; Hewlett and Duncan, Atomic Shield, 145.

a careful program of gadget testing. I discuss the postwar fission program in greater detail in Chapter Five.[312]

Bradbury wanted the current Mark (MK) III Fat Man bomb stockpile to total fifteen. To meet this stockpile number would require continuous production of Pu at Hanford. The Hanford piles, however, could not produce enough plutonium for this fission stockpile, much less produce tritium for thermonuclear research.[313]

While Bradbury only gave brief mention to exploring the feasibility of the Super to the Coordinating Council meeting in October 1945, he discussed the issue in detail in a letter to Groves several weeks later. Certain types of investigation into the Super appearing "practicable" [sic] would be carried out in the postwar. Aside from studies of the compressibility of H^2 using shock velocity measurements, and studies of very fast jets:

> Experimental physics studies involving p-D scattering,; T-D cross-sections; properties of the 14 Mev neutrons from the T-D reaction and particularly their scattering by [D] and light elements in general; general problems of neutron scattering particularly on the very light and very heavy elements, scattering of alpha particles in [D]; and the T-T cross sections . . . [314]

Even such a limited experimental program would require some tritium, thus Bradbury asked Groves to push current discussions towards production in existing tritium piles to the extent of at least 1 cc gas per day.

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310 Hughes, American Genesis, 426.
311 Hewlett and Duncan, Atomic Shield, 62; Hewlett and Anderson, The New World, 630.
312 Bradbury presentation to Coordinating Council, October 1, 1945, op. cit.
313 Ibid.
314 Letter from Bradbury to Groves, November 23, 1945, 471.6 Weapons, Box 17, Folder 1, LANL Archives.

This amount would at least be sufficient to sustain the fundamental experimental research essential to the Super program. It would not, however, constitute enough T production for a Super test. Bradbury commented that at this rate of T production, "about 5000 years would be needed to accumulate enough tritium for a single test." For a serious effort to build a Super, T production would need to proceed at a rate of five to ten liquid liters per year.[315]

As opposed to the Laboratory director's practical view on thermonuclear weapons, Teller already had a theoretical production schedule for tritium worked out, and the T production figures Bradbury presented to Groves were based on Teller's recent estimates for a time and production scale for a Super construction program. Teller had informed the Laboratory director:

> If a Project comparable to this [wartime] Project were given adequate personnel and equipment then between one and two years from its inception it would be ready to employ one liter of liquid tritium in preliminary experiments. If liquid tritium was thereafter available at the rate of about 0.5 liters per month, about 1-2 more years might be required to make the final satisfactory model. Such a program, with tritium in the amounts indicated, has a high probability of success.[316]

For Los Alamos to obtain such amounts of tritium, however, even Teller acknowledged this as a "fantastic" venture given present supplies.[317]

Given the lack of every kind of nuclear materials in 1945, Bradbury had no intention of asking Groves to lead an effort for massive tritium

[315] Ibid.
[316] Ibid.

production. Neither did Bradbury make this request of the AEC in 1947. While the GAC wished to "strengthen" thermonuclear work at Los Alamos for the sake of re-invigorating the Laboratory, it did not call for an outstanding tritium production effort to go along with the former proposal. On one hand, by spring 1947 the GAC pondered a "considerable expansion in Plutonium production to bring it up perhaps to more than three times what it now is . . ."[318] Tritium, on the other hand, would have to wait:

> We have come to the point of realizing that recommendations about the 'Super' have little meaning unless one or two people that we know can be gotten in to worry about it. The theoretical problems are such that they could bring the breadth and interest that Teller has brought.* To compensate for his enthusiasm, we feel that until this is done, progress in other directions won't be possible. We have tabled -- rather postponed -- recommendation on further Tritium production until we understand a little bit better about it.[319]

In principle the GAC did not discourage Los Alamos from preparing for a thermonuclear research, and recommended that Los Alamos should include a thermo-nuclear [sic] experiment in one of its upcoming test series to look for the existence of a fusion reaction in the interior of an otherwise standard levitated fission model, or in other words, a Booster bomb.[320] Although, as noted in Chapter Three, the Booster may have had several inventors, Teller clearly pushed the hardest to test the device since it involved igniting D-T, even if it would not ultimately prove the Super's

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[317] Ibid.
[318] Draft Minutes, Third Meeting of the GAC, March 28-30, 1947, RG 326, DOE Archives, Secretariat Files of USAEC, Box 337, Folder 1-3-47, declassified.
[319] Ibid; Asterisk in original - (* to the Super problem).
[320] Draft minutes, Fifth Meeting of the GAC, July 28-29, 1947, RG 326, DOE Archives, Secretariat Files of USAEC, Box 337, Folder 1-3-47, Vol. 1, [This Document is Secret-RD].

feasibility. According to Teller, the Booster would be valuable for the fission program because it would increase the efficiency of a low-power fission implosion device by a factor of ten or more, and it might serve as an alternative to levitated weapons that still required elaborate initiating mechanisms. A Booster test would consume tritium, but by 1948 Teller saw no reason why enough T for a single Booster could not be produced by Hanford in a three to four month period. Of course, this meant that Hanford would have to operate at a level of half a kilogram of Pu per day and that nearly all the existing neutrons in the reactors would be made available for T production."[321]

In 1948 the GAC had little grasp of the rate and scale of materials production. In early June 1948, the GAC met in Washington, DC to consider, among other things, the Booster bomb. Probably in response to Teller's May report, "On the Development of Thermonuclear Bombs," Oppenheimer told the rest of the Committee that perhaps two years would suffice to produce enough tritium for a simple test of thermonuclear principles, and somewhere between this time and five years to obtain enough T to detonate a full-scale Booster bomb. More optimistically, Fermi stated that five years was perhaps too long, especially from the point of view of tritium production. Instead, he thought it reasonable to consider a production rate of about ten grams per year.[322]

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[321] Ibid., 39.
[322] Tenth Meeting of the GAC, June 4-6, 1948, RG 326, DOE Archives, Secretariat Files, Box 11217, Folder 9, 1-3-47, Vol. 2, [This Document is Secret-RD]. LA-643, op. cit.

The Committee concurred that the "urgency" to discuss thermonuclear weapons arose not from the Los Alamos program itself but from the point of view of tritium. Fermi wanted to allow Los Alamos to perform an experiment with tritium because the cost of T only amounted to about one kilogram of Pu. However, many other demands were being made on Hanford, and Fermi thought that this facility and Los Alamos might have to consult each other directly with respect to the amount of radioactivity that could be devoted to T production. The rest of the Committee agreed with Fermi, and decided to encourage Los Alamos to proceed with the design of and experimentation on a Booster weapon.[323]

Unlike the Super, the Booster device was a conservative design in terms of materials expenditure. One of the individual results of the 1945-46 ENIAC calculations indicated that the main charge of D would ignite with relatively little T. In 1948 Teller and Foster Evans, attempting to reexamine the ENIAC problems analytically, concluded that this particular problem's result was wrong. In his May report, Teller recommended an increased number of grams of T be placed in the booster, which would be compressed by a fission initiator and help further the process of ignition in the Super. Thus, the total volume of T now stood beyond double the 1946 predictions.[324]

Besides the difficulties in calculating the Super's ignition, and uncertainties as to the amount of tritium needed for this, Los Alamos's

[323] Ibid.

[324] LA-643, 9-10; The obvious vagueness in my description of the specific amounts of nuclear materials examined the ENIAC problems is due to classification of the amounts in grams.

scientists had doubts as to the optimal physical arrangement of T in a weapon. No one knew if the current Super design would be the most optimal arrangement for successful ignition. Indeed, a completely different mechanical arrangement within the Super might be better, as Teller himself suggested in his 1948 report.[325]

The staggering problems of the physical design of the Super combined with the growing realization that T presented a massive obstacle to the thermonuclear project likely prompted Teller and others to consider circumventing these bottlenecks altogether. Teller devoted much attention to the Alarm Clock in his 1948 report, describing this device as employing only normal U^{238} and D, in a configuration very different than that of the Super. Although like the Super, the Alarm Clock required a fission bomb to start a reaction, the latter apparently did not need any tritium for ignition, and may have held theoretical appeal for that reason. In addition, the Alarm Clock design appeared as an attempt to get around the problem of avoiding one of the most serious obstacles to the Super involving radiation and the heat-content of the fuel. Teller himself called the Alarm Clock a "simpler design," yet noted that it too would be a very difficult feat to accomplish.[326]

In 1948 the Laboratory did make a commitment to a test of thermonuclear principles, yet it would still have to use a small amount of T. The CWD proposed testing, among other configurations, a Booster weapon that the group estimated would consume a minuscule amount of T. The

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[325] Ibid., 10.

version of the Booster that the CWD considered in summer 1948 meant simply a fission implosion weapon with D-T placed in its center. This, and a test of what would become the "George" device (described in Chapter Three) would have to suffice for the 1951 tests, since the group agreed that no possible means existed to test either a Super or Alarm Clock at this time. The CWD then, agreed that Los Alamos would have to convince the AEC to produce T for the Booster.[327]

Glitches in the System

The AEC's production system remained out of line with the theoretical weapons program throughout 1948. Darol Froman and Bradbury had attended a meeting in Chicago in late October with representatives from Argonne, Oak Ridge, and Hanford to discuss tritium production. Froman reported back to Los Alamos that the staffs of the various laboratories were not clear about their respective responsibilities for tritium production. At that moment, Hanford had several T production problems. For example, the Hanford slugs from which tritium was extracted leaked in the reactor piles; the more recent slugs placed in the piles were made on an assembly line rather than handmade as the original lithium-fluoride slugs had been.[328]

Hanford representatives at the Chicago meeting agreed to set up a new tritium extraction plant in the first half of 1949; this facility would have a capacity to handle proposed production amounts up through 1951 or 1952.

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[326] Ibid., 13, 19.
[327] Meeting of CWD, August 6, 1948, op. cit.

However, the AEC had not set any exact figures for production, nor had they established rules for the required purity of tritium, decided upon the process for isotopic separation of T from hydrogen impurity, or decided what laboratory would undertake the development of this process.[329]

Frustrated with the Commission, before the Chicago meeting closed Bradbury suggested that a "tritium czar" be appointed, preferably from Argonne, because that laboratory had the most central locale among all of the AEC's research facilities. The "czar" would follow the research, development, and production of tritium and inform the Commission about which directives to send the various laboratories. Shortly after the Chicago meeting, Bradbury, Froman, and Manley met with Brigadier General James McCormack, head of the Commission's Division of Military Application, his Deputy, Navy Captain James Russell, Walter Williams, the AEC's chief engineer for reactor construction, and Arthur Peterson of the Commission's production division, again to suggest appointing a "tritium czar" to oversee the "whole picture of tritium production."[330]

The AEC never appointed a "tritium czar" to reign over H^3 production, and Bradbury and his staff undoubtedly found the AEC's apparent lack of directive in weapons-grade materials production frustrating, because the pace of material production in the large system affected Los Alamos's ability to plan for tests, and design and develop new and improved weapons. Even

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[328] LAB-ADWD-6, CWD minutes of Meeting on November 4, 1948, LANL Report Library, [This Report is Secret-RD].
[329] Ibid.

though the Commission made some progress by the end of 1948 in procuring reactor feed materials, fissionable materials, and other special products, and was actually ahead of schedule for these operations, Los Alamos's leaders interacted directly with the production components of the system when they could, and of course, do so in a way that would benefit the New Mexico laboratory.

In May 1949, Froman and Manley met with representatives from Hanford and Argonne, to work out a production schedule for tritium that Arthur Peterson had already set up for Hanford to produce about 20 grams of T by July of 1950. Hanford and Argonne wanted anxiously to receive a directive from the Commission for a T production schedule, yet when this did not come, Hanford and Argonne's representatives asked Manley himself if Los Alamos thought T production should continue at the rate established by Peterson and what kind of policy should be adopted regarding production following the period after 20 grams had been delivered. Manley and Froman both replied that if Los Alamos's staff found T experimentally valuable then they would probably ask for increased production of the isotope. If the scientists found T of little value then Los Alamos would likely call for discontinuation of its production. However, the two Los Alamos leaders

[330] Ibid.

could not see an overriding priority for more than 20 grams of tritium by the end of 1950.[331]

If the AEC appeared slow in establishing demands upon its production plants, others wanted a voice in this part of this system. Nichols, who had been promoted to the rank of General headed the Armed Forces Special Weapons Project, wanted to renew the campaign for military control of atomic energy. Only weeks after Los Alamos, Argonne, and Hanford discussed the near future of tritium production, the military asserted its own demands for a "substantial" increase in materials production far beyond the AEC's existing construction plans and the abilities of the current installations. Kenneth Nichols, along with other members of the military community, found support for their demands in JCAE Chairman McMahon, and Committee staffer William Borden, both of whom believed the U.S. could never have enough nuclear weapons. I explore the military's role in more depth in Chapter Five.[332]

Tritium availability, of course, influenced the CWD's deliberations over what models to choose for the 1951 tests. Los Alamos seemed pessimistic about the near future of tritium production as it tried to plan its weapons tests, and eliminated some proposed models altogether. For

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[331] Hewlett and Duncan, 178; LAB-ADWD-33, Memorandum from Froman to Bradbury, "Meeting of May 7 on the subject of Tritium Production," May 7, 1949, B-9 Files, Drawer 102, LANL Archives, [This Document is Secret-RD].
[332] Hewlett and Duncan, Atomic Shield, 181-183.

example, although Teller wanted a test of the Little Edward device, the CWD projected that Los Alamos wouldn't be able to get enough T for it by 1951.[333]

While some models got scrapped, scientists proposed other new ideas when considering how to conserve tritium expenditure. Gamow, whom Bradbury had asked to come to Los Alamos to help with theoretical thermonuclear work, regularly attended the CWD meetings towards the end of 1949. In November he proposed a variation on the large fission detonator purported to ignite the Super, which he named the Cat's Tail. Gamow theorized that the Cat's Tail needed less T than had been assumed in the ENIAC Super problems, but could not guarantee this.[334]

The June 1950 CWD meeting where Ulam had presented his group's hand calculations for ignition of the Super was a solemn one for Teller and those who had high hopes for the runaway bomb. While the Ulams, Everett, Elliott, and Houston had applied themselves to several weeks of work on desk calculators, tritium was their essential concern. Ulam's group found:

> [I]f tritium is used in the uncompressed state then the bomb [the Super], even if feasible, will require, as a conservative estimate of today, the equivalent of 100 or more kilograms of plutonium . . . .[335]

Ulam outlined his calculations to the CWD, which included Bethe, on a visit for the summer, de Hoffman, Gamow, Mark, Teller, Manley, Froman, Hammel, and chemist Eric Jette, among others. The fusion system Ulam's

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[333] LAB-ADWD-26, CWD minutes of Meeting, January 28, 1949, LANL Report Library, [This Report is Secret-RD].

[334] LAB-ADWD-31, CWD Minutes of Meeting, April 12, 1949, LANL Report Library, [This Report is Secret-RD]; ADWD-80, CWD Minuteds of Meeting, November 9, 1949, LANL Report Library, [This Report is Secret-RD].

group had calculated, including the D, was assumed compressed yet the amount of T within the T-D mixture was equivalent to a large amount of tritium at normal density. Even assuming this amount of T, Ulam concluded that high enough temperatures to detonate the cylinder could not be reached. Bethe estimated that at the minimum, an enormous amount of tritium in the uncompressed state would be necessary to ignite the Super, if this could be done at all. Thus, the CWD concluded that this idea would be completely uneconomical and that only compression might make the Super feasible.[336]

The GAC echoed Ulam's report in a letter from Oppenheimer to the AEC in November. By now estimates for the necessary tritium for a Super had risen even more, and the "lower limit for this [Super] model" stood in the "range of 3 to 5 kilograms."[337]

McMahon, Borden, and a Program of AEC Expansion

By the time Ulam presented his calculations to the CWD the AEC system underwent sweeping political change. As already mentioned in Chapter 3, the political impact of the Soviet Union's first atomic test on the United States' nuclear weapons program has been for the most part investigated by Bernstein and Galison, Hewlett and Duncan, Hansen, Rhodes, Herken, and York, and thus I will not interpret (1) the Soviet test itself, (2) the GAC's disapproval of an accelerated thermonuclear program on moral terms,

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335 LAMD-411, Weapon Development Committee,Minutes of June 21, 1950 Meeting, LANL Report Library, [This Report is Secret-RD].
336 Ibid.
337 GAC declassified report quoted in Hansen, Swords, III-148-149.

(3) Truman's announcement directing the AEC to continue with H-bomb research, and, (4) the scientific advisors' wavering stances on this subject.

Instead, I will concentrate on (a) the AEC expansion program and the JCAE's significant role in fostering such a major expansion of the entire system, (b) Los Alamos's own efforts towards expanding the AEC system, and, (c) several individual scientists who attempted to act as system builders in the period after the Soviet atomic test.

The AEC expansion program was only one result of the political discussions surrounding the Russian atomic bomb, but the technical changes demanded of the large system bore on Los Alamos most directly. From the time the hydrogen bomb became a political issue -- in fall 1949 -- Los Alamos needed three years to design and test a full-scale thermonuclear device -- one that had questionable value as a weapon. Still, the time it took the Laboratory to produce this device is, as I indicated in Chapter One, relative because the whole process occurred within the AEC system, which brought many factors to bear on this project. The size and complexity of the AEC system in the postwar has not only puzzled scholars but has also led them to ask the wrong historical questions, such as "Why was the H-bomb delayed," instead of more probing questions such as "Why was the project exceptionally complicated?" a query that better addresses the black box of nuclear weapons science, and can lead to a better understanding of why and when scientists developed certain fusion weapons models.

While Los Alamos remained in the hands of the newly formed AEC after World War II, the Commission answered to a higher authority -- several U.S. Congressmen, whose roles came to the forefront of the system after the American detection of the Russian atomic bomb. Rhodes, and Hewlett and Duncan have examined how the Russian atomic test in 1949 caused the JCAE to become alarmed about the state of the AEC's weapons facilities. To some of the JCAE's most prominent and outspoken members, particularly McMahon and Borden, technological solutions to the arms race represented the only option. For McMahon and others on the Joint Committee, the most effective technical solution came in the form of a "super" weapon.

With McMahon at its helm, the JCAE held power over the Commission since the former provided funding for the AEC's projects. As soon as the Committee learned of the Soviet test, it began to push the AEC's Commissioners for development of a thermonuclear weapon. By the week of September 23, before Truman had publicly announced the Soviet test, the JCAE began meeting to discuss possible responses to the Soviet test. Leading the discussion, Borden and his staff came up with a list of twenty-three "possible methods," to hasten the AEC's production of atomic devices. Among these suggestions, the Committee recommended bringing du Pont back into the system to increase materials production at Hanford, and increasing the number of staff members at Los Alamos. In addition, the Committee suggested that an entirely new pile area be built at a site other than Hanford, and that accelerated procurement of raw materials was imperative.[338]

The Joint Committee wanted an "all-out" effort on a hydrogen weapon, and by September 29 listened to testimony from several of the AEC's leaders on the prospects of doing this. The Commission prepared for this as best as it could. Carroll Wilson, the AEC's general manager, testified that Los Alamos was already working towards thermonuclear-related tests with plans for the Booster. Wilson saw this device as, "a step toward a possible thermonuclear bomb," and at this point would require all the of the Commission's attention and Los Alamos's concentration to demonstrate by 1951.[339]

Lilienthal had Wilson explain to the JCAE and McCormack that although the Commission planned to sponsor a test of thermonuclear principles, a full-scale hydrogen weapon would require several years to develop; Los Alamos simply did not know how to construct a workable hydrogen device. In addition to delivery problems, which I review in the next chapter, Wilson reported that a major hydrogen bomb program would likely require far more tritium than the Commission had, in addition to exceeding what could be produced by the AEC's reactors over the next few years. Producing large quantities of T would require reactors producing far more free neutrons than any facility existing or planned for Pu production.[340]

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[338] Herken, The WinningWeapon, 303; Rhodes, Dark Sun, 379.
[339] Rhodes, Dark Sun, 379.
[340] Hewlett and Duncan, Atomic Shield, 372.

According to Rhodes, AEC Commissioner Sumner Pike subsequently explained to the Committee in detail the troubles the AEC's tritium production problems. The Super would require far more reactivity than the AEC had in any working pile or even those they had considered building in the near future. Thus, although thermonuclear "experiments" had been officially sanctioned by the GAC, an active thermonuclear weapon project had not been a part of the Commission's agenda even for the long term.[341]

The Joint Committee members failed to see all the complications associated with hydrogen bomb development. If a thermonuclear project in the form of the Super went forward, a another technical choice would follow. Choosing the Super would severely disrupt the system of fission weapons development already now established within the AEC. In 1949, producing tritium meant not producing plutonium, or at least cutting fabrication of the latter material to a fraction of its former level.[342]

Making tritium in a graphite reactor like those at Hanford meant that the natural U^{238} slugs would require replacement with U^{235} slugs. As Rhodes has described, U^{235} fissions with neutron capture rather than transmuting to neptunium and then plutonium. Thus, although using U^{235} would increase T production, it would decrease the amount of Pu produced. Pike explained this in terms of cost, stating that producing tritium in terms of Pu that could otherwise be produced would be 80 to 100 times higher -- gram for gram. For

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[341] Rhodes, Dark Sun, 380.
[342] Ibid., 380.

every kilogram of T that the U.S. produced, it would cost between eighty and one hundred grams of Pu, and consequently many fission weapons.[343]

McCormack's suggested to Pike and Wilson that a program for building reactors specifically for tritium production be started immediately. Pike, too, thought that at some point construction on new reactors would have to commence, particularly if the 1951 Booster test proved successful. Hereafter, the AEC would embark on a large plant expansion program.[344]

Borden and McMahon equated bigger with better in the case of nuclear weapons. With little knowledge about the technical details of the Super and likely no understanding of the complexity of the theory, the Joint Committee members had an almost obsessive confidence in the Super weapon. Rhodes has described Borden as prone to utopian fantasies as, for example, when he envisioned the new thermonuclear weapon as being delivered by a state-of-the-art nuclear powered airplane, Yet due to their lack of understanding of what a thermonuclear project involved, Borden and the other Committee members were prone to fall for the ideas of Lawrence, Teller, and Alvarez.[345]

Can Berkeley Produce Tritium?

After the Russian atomic test, Berkeley chemistry professor Wendell Latimer found himself convinced that the hydrogen bomb effort needed serious attention because the Soviet's were working on their own version of this. He in turn convinced his colleague Alvarez, and by early October

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[343] Ibid., 380.
[344] Hewlett and Duncan, Atomic Shield, 372; Rhodes, Dark Sun, 380.
[345] Rhodes, Dark Sun, 380; Hewlett and Duncan, Atomic Shield, 372.

Alvarez and Lawrence contacted Teller to find out how much progress on thermonuclear research had been made.[346]

Meeting in Los Alamos with Teller, Ulam, Gamow, and Manley, Lawrence and Alvarez learned that their colleagues now gave a workable Super good odds if tritium were made plentiful. However, in fall 1949 calculations determining the Super's prospects remained far from complete. Nevertheless, Lawrence and Alvarez wanted to relay this optimistic news back to Washington, and offered Teller their assistance in promoting acceleration of the H-bomb's development. Teller suggested that they could be of the most help if they would try to convince the entire Commission to support additional reactor development, particularly a heavy-water moderated tritium production reactor.[347]

Lawrence probably could not have found a better excuse for approaching the AEC, because he personally wanted to further his own construction efforts at Berkeley. While Lawrence whole-heartedly supported building a thermonuclear weapon, Teller's request gave Lawrence a window to involve Berkeley in tritium manufacture just at the time the AEC considered plans to expand.

Lawrence's solution to the tritium bottleneck was simply more technology, hence Lawrence tried to capitalize on the tritium versus plutonium problem when he and Alvarez arrived in Washington on October

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[346] Rhodes, Dark Sun, 382; Luis W. Alvarez, Alvarez: Adventures of a Physicist, (New York: Basic Books, 1987), 169-170.
[347] Hewlett and Duncan, Atomic Shield, 376.

8. When Lawrence and Alvarez met with McCormack, Paul Fine, a physicist in the Commission's division of military applications, and Kenneth Pitzer, the AEC's director of research, they began to try to convince the Commission that it should sponsor a heavy water-moderated production reactor at Berkeley.[348]

Lawrence and Alvarez's social calls did not stop on Sunday. The following day, in addition to speaking with MLC secretary Robert LeBaron about their proposal, they met with the AEC Commissioners individually, and with McMahon, Borden, and Carl Hinshaw of the Joint Committee. The two Berkeley professors appeared convincing and more importantly said what the Congressmen wanted to hear -- an H-bomb could not wait.[349]

Lawrence was so confident about the results of the meeting that when Alvarez returned to Berkeley, Lawrence already appointed him director of the new reactor project. In the meantime Lawrence remained in Washington and looked up Kenneth Nichols in Washington, attempting to convince him to in turn convince the JCS to establish an official military requirement for a thermonuclear weapon.[350]

McMahon had promised Lawrence and Alvarez that he would create a special subcommitee on the Super to look into the possibility of its development. McMahon also wanted to find out directly from Los Alamos's staff their views on the Super's prospects. The subcommittee, consisting of

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[348] Ibid., 376.
[349] Hewlett and Duncan, Atomic Shield, 377; Rhodes, Dark Sun, 384.
[350] Hewlett and Duncan, Atomic Shield, 377; Rhodes, Dark Sun, 387.

JCAE members Chet Holifield, Melvin Price, Henry Jackson, Hinshaw, Borden, and Walter Hamilton, flew to Los Alamos to meet with Bradbury, Robert Kimball, then Associate Director of the Laboratory, Carroll Tyler, the AEC's area manager, Paul Ager, the AEC's area coordinator, and Everett Hollis, the AEC's Deputy General Counsel.[351]

After describing the state of the fission program, Bradbury told the JCAE members about the Laboratory's plans for the upcoming 1951 test series and thus the Committee members realized that in practice the thermonuclear program so far essentially consisted of the Booster. Still, the Booster, Los Alamos's director emphasized, represented a "departure from all previous weapons," and could be considered "a new field, that of igniting light atoms to form heavier atoms." By now, the laboratory had already proposed a design for the Booster:

> . . . [I]ncluded a small amount of D-T . . . detonated by a high explosive . . . [with] the shock wave traveling to the center . . . thus releasing the necessary neutrons. These in turn start the fission process in the U-235 and plutonium. The heat from this reaction, in turn, will set off the tritium and deuterium which combine to form helium. The heat yielded by this reaction in turn will act as a booster to the remaining unfissioned U-235 and plutonium in the core. Thus a higher degree of utilization of material is expected to be achieved.[352]

Bradbury went on to describe the "ultimate in weapons" as the Super, yet it would be a long time in the making; Los Alamos's original idea prior to the Soviet A-bomb test included an orderly, step-by-step process to develop a

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351 Memorandum to the Files from Walter A. Hamilton, "Inquiry into the Aspects of A Superweapon Program," November 8, 1949, JCAE declassified General Subject Files, Box 60, 1-2, [This Document is Secret-RD].

thermonuclear by about 1958 or 1960. Now, Los Alamos's leaders had to move the schedule up, and if the Booster proved successful, Bradbury had already decided to try to have the Laboratory "yield a proven Super weapon by mid-1952." Nevertheless, the AEC would have to produce between 50 and 500 grams of T for a test of the Super.[353]

The JCAE subcommittee did not return to Washington from Los Alamos but went on to Berkeley for an unofficial meeting with Lawrence, along with his colleagues Donald Cooksey, Edward MacMillan, Isadore Pearlman, and Robert Thornton. Lawrence argued to the subcommittee that the Super was feasible, and now that the Soviets had an A-bomb the AEC could afford to lose no time in getting started with an H-bomb. Lawrence, however, wanted to speak with the subcommittee more about the subject of tritium, and outlined three methods by which the U.S. could manufacture the isotope in large enough quantities for a full-scale Super test by 1952. Although he failed to mention exactly how much tritium could be produced, Lawrence felt that in addition to the construction of heavy-water piles, perhaps a modification of the Materials Testing Reactor (MTR) at Berkeley would be in order.[354]

Having already picked a tentative location just over the hill from the Radiation Laboratory, Lawrence advocated constructing either a giant cyclotron or particle accelerator that would fire particles at a block of lead or

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[352] Ibid., 5-6.
[353] Ibid., 7-8.
[354] Ibid., 13.

thorium, this action would, according to Lawrence, free 22 neutrons for each particle injected into the block. These neutrons then would be available for irradiation of the necessary lithium to produce tritium. Lawrence estimated the cost of this at $10 million.[355]

MacMillan, as eager as Lawrence to see an accelerated hydrogen weapon program, advised the subcommittee that the AEC should adopt a philosophy of "a production pile in every backyard," prompting Hamilton, who sat at the meeting taking notes, to later describe Lawrence's and MacMillan's discussions at the October 28 meeting as "A cross between hysteria and a tremendous enthusiasm."

On the same day on the East coast, the GAC began its meeting scheduled for the next few days, to discuss numerous issues including the Super, and a possible AEC expansion program. Lawrence wanted to participate in this meeting as well, and therefore sent Serber in his place to promote the idea of building a heavy-water reactor at Berkeley. Serber had left Los Alamos for the Radiation Laboratory after the war to work for Lawrence, whom Serber would later describe as "a benevolent dictator." Serber himself did not want to become involved with work on the Super, believing that "it wouldn't work under any circumstances."[356]

Regardless of Serber's personal opinion of the feasibility of the Super, he was obligated to relay Lawrence's ideas for getting Berkeley involved with

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[355] Ibid., 13-14.
[356] Hewlett and Duncan, Atomic Shield, 381-382; Author interview with Serber, November 26, 1996.

the AEC's plans for expansion, and according to Hewlett and Duncan, explained to the GAC the advantages of building a large neutron-producing reactor at Berkeley. Fermi, however, critiqued the idea by stating that Berkeley had absolutely no experience with reactors. Historical evidence indicates that Serber told the Committee that Lawrence merely wanted to see more reactors built, even if it meant undertaking this work himself.[357]

The October 28-30, 1949 meeting of the GAC is best known among historians for its members' nearly unanimous decision to recommend against going ahead with a full-scale thermonuclear weapon program. The Committee made its decision on two bases: technical and moral. The technical reasons the GAC cited reflected of the modest state of the AEC's production facilities and on Los Alamos's work on the Super throughout the 1940s. The Committee report read:

> No member of the Committee was willing to endorse this proposal [a super bomb]. The reasons for our views leading to this conclusion stem in large part from the technical nature of the super and of the work necessary to establish it as a weapon . . . .[358]

Testing a Super, which the Committee regarded as the only possible experimental approach to determine the device's viability, would require producing several hundred grams of T, a feat beyond the Commission's present capabilities.[359]

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[357] Hewlett and Duncan, Atomic Shield, 382; Rhodes, Dark Sun, 396.
[358] GAC Report to the AEC, October 30, 1949, 3, Box 1217, Folder "GAC Minutes," RG 326, DOE Archives.
[359] Ibid.

The Committee had never endorsed a large program of thermonuclear weapons research for Los Alamos, and consequently, aside from the tremendous experimental effort necessary to set up such a program, the GAC noted that the New Mexico weapons laboratory's theoretical studies of the Super were still incomplete. However, as Rhodes acknowledges, it is important to recognize that the GAC made its decision against a crash program to build the *Super* configuration. The Committee did not consider any other type of weapon in its October meeting. Morally, the majority of the GAC opposed the Super because it could be a weapon of "genocide," as the GAC pointed out: Limitless deuterium fuel added to the device meant limitless explosive yield.[360]

This GAC meeting had not been the first occasion where the Committee had recommended against a large and immediate program to build a Super based on technical grounds. In June of 1948 the GAC reported to the Commission that the "problem of Tritium production" was directly related to the development of thermonuclear weapons. Only the Booster weapon appeared capable of being developed rapidly, within two to five years. Consequently, while not encouraging a major Super or Alarm Clock project, the Committee recommended to the Commission that Hanford be directed to produce 10 grams per year -- enough to suit Los Alamos's needs for a test of the Booster.[361]

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[360] Rhodes, Dark Sun, 400; Italics mine.

[361] Memorandum for the File from J. Kenneth Mansfield, "Extracts from GAC Reports Relating to Thermonuclear Program," May 28, 1952, in JCAE declassified General Subject Files, Box 59,

In its technical considerations, the GAC's decision to forego Super development in 1949 did not constitute a departure from previous recommendations the group had made regarding the H-bomb. Yet upon reading the GAC's 1949 decision, McMahon reportedly became outraged, and took up his own cause for a hydrogen bomb construction effort and for an expanded AEC program, writing directly to President Truman urging him to support an increased H-bomb effort, and using the Joint Committee's influence to gain increased political and military support for this program. The Commission did not formally meet to discuss the GAC's recommendation and the Commissioners' present their personal opinions until several days after the GAC meeting. The Commissioners divided in their views: Lilienthal, former Wall Street investor Sumner Pike, and physicist Henry Smyth stood against accelerated Super development; financier and Navy Rear Admiral Lewis Strauss and attorney Gordon Dean in favor of it. Unable to come to an agreement, the Commissioners referred the issue to Truman for a final decision.[362]

McMahon wanted to see for himself the state of the AEC's weapons production facilities, visiting Los Alamos, Hanford, and other areas during November. Both McMahon and Borden met with John Manley at Los Alamos, who agreed with the GAC's decision on the Super. In their account of this meeting, Hewlett and Duncan stated that then Robert LeBaron,

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NARA; GAC Report to David Lilienthal, June 6, 1948, [report of Tenth Meeting], JCAE declassified General Subject Files, Box 34, [This Document is Secret-RD].
[362] JCAE Chronology, 15.

Chairman of the MLC, joined the conversation in the afternoon. Teller, who also joined the meeting, discussed the difficulties involved with understanding the Super, yet assured the visitors that the chances for this theory to work were greater than fifty percent. Manley observed that Teller only reinforced McMahon's and LeBaron's already-formed prejudices in favor of a Super project.[363]

After the Los Alamos meeting and McMahon's tour of the AEC facilities, the Senator intended to have the AEC embark on a major expansion program, since this constituted a necessary step towards developing a hydrogen weapon. A major expansion of the AEC system had been already suggested explicitly by the Joint Chiefs of Staff, and implicitly by Lawrence, Teller, and Strauss, who had suggested that the Commission take a "quantum jump" towards the Super.[364]

Teller wrote a letter in early October, probably intended for his Los Alamos colleagues and the AEC, remarking that "If the Russians demonstrate a Super before we possess one, our situation will be hopeless." To prevent this, Teller outlined a program including increased T production at Hanford through loading of enriched uranium slugs, using Chalk River to produce tritium, and building new piles oriented towards tritium manufacture.[365]

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[363] Hewlett and Duncan, Atomic Shield, 391-393.
[364] Memorandum to D.E. Lilienthal, S.T. Pike, H.D. Smyth, and G. Dean from Lewis Strauss, October 5, 1949, JCAE General Correspondence Files, Box 58, [This version of the memorandum is labeled "Secret" although a declassified version of this exists]; This letter is reprinted in Strauss, Men and Decisions, 216-217.
[365] Memorandum to the File from John Walker, September 12, 1952, Appendix A, JCAE declassified General Subject Files, Box 59; Teller's original letter, titled "The Super Bomb and the Laboratory Program," was filed in Los Alamos as report number LAMD-166, October 13,

Teller's letter bordered on frantic as he equated the Super with political superiority over the Russians:

> It is my conviction that a peaceful settlement with Russia is possible only if we possess overwhelming superiority. We do not now possess such superiority. The most promising prospect to acquire a great lead is by early development of a Super bomb . . . . It is quite possible that the Russians will possess a Super bomb in a short time.[366]

Manley also wrote a letter on the same day as Teller, to express his more conservative and realistic view on the subject. Manley forewarned that:

> Whatever statements the National Military Establishment or the Atomic Energy Commission have made or may make concerning the effect of the detonation of a Russian bomb, the Laboratory should admit at least to its own personnel that the current Laboratory program has not been geared to such an event in 1949.[367]

The Laboratory, Manley revealed, had been assuming that a Russian atomic weapon would not appear until 1952. Therefore, Manley recommended that Los Alamos should no longer operate on the basis of assumed time scales for Russian technical developments, and the Laboratory needed to strengthen its position. Here, Manley referred to the overwhelming lack of technical staff at the Laboratory, an issue I present in Chapter Five. [368]

While Manley made his recommendations internally at Los Alamos, others in the system worked to strengthen their own positions. By the time the GAC reaffirmed its statement on the Super in December 1949, the Joint

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1949, but was missing from the LANL Report Library in 1996. The letter from Manley was also filed as part of this same document. I have therefore quoted from Walker's interpretation of Teller's letter.

[366] Walker memo, September 12, 1952, Appendix A, op. cit.

[367] Ibid.

Chiefs of Staff had formally announced that the U.S needed to possess a thermonuclear weapon. Still, the GAC recommended that Los Alamos continue to work on thermonuclear weapons at the pace it had been doing so over the last year. Truman essentially overturned the Committee's recommendation in January, 1950.[369]

Truman's announcement obligated the AEC and Los Alamos to pursue a stepped-up thermonuclear program, but the Laboratory could do little to increase the pace of hydrogen weapons work without an exponential increase in the AEC's supporting materials and other production plants. The JCAE had no reservations about funding an expanded AEC program.[370]

The production system needed revamping almost entirely to support building a Super. Paul Fine tried to appraise the condition of the AEC's production plants in relation to constructing a Super. Hanford, he noted, could probably produce enough of the isotope for one of the 1951 thermonuclear principles tests, but for a full scale Super test by 1952 several new reactors would have to be completed at a cost of $150 million.[371]

By the time Truman had made his announcement regarding hydrogen bomb work, Lilienthal had resigned from the AEC, which had begun making plans for an expansion program. Pike, acting in Lilienthal's place, wrote to McMahon in March 1950, suggesting to McMahon that the cost of refitting the Hanford reactors with slugs to produce T would lie between $2 million and $5

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[368] Ibid.
[369] Hewlett and Duncan, Atomic Shield, 395-396; Mansfield Memo, May 28, 1952, op. cit.
[370] Hewlett and Duncan, Atomic Shield, 370.

million. Still, Hanford alone could not produce enough T for a test of the Super. Refitting the Hanford piles, as Hafstad soon informed the JCAE, would mean replacing the natural uranium slugs with U^{235} fuel slugs, and target slugs made of lithium, where T would be formed.[372]

The Commission took Lawrence's idea to build a production reactor at Berkeley as seriously as had the JCAE. Pike, writing to McMahon, explained that the Commission assumed that it would have to produce on the order of 1 kilogram of T per year. To do this so quickly would require entirely new means of producing T. The Commission considered several alternatives to modifying the piles at Hanford, including a high current linear accelerator at the Radiation Laboratory, heavy-water reactors, and a production Materials Testing Reactor, all intended for tritium manufacture.[373]

Although the Commission needed to work out its plans for an expanded program to meet the tritium requirements of a Super, by early April it had approved a short-term program with Los Alamos's needs in mind for the 1951 tests, and at least one of the Hanford piles would be charged for tritium production.[374]

The Problem of Attaining a Nuclear Reaction Involving the Light Elements

Although the Commission had to undertake an expansion program, finding the solution to the Super problem fell to Los Alamos. When

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[371] Ibid., 397.
[372] Letter from Pike to McMahon, March 1, 1950, JCAE declassified General Subject Files, Box 57, NARA; Hewlett and Duncan, Atomic Shield, 401.
[373] Letter from Pike to McMahon, March 1, 1950, op. cit.
[374] AEC Meeting No. 375, [Minutes], February 28, 1950, DOE Archives, RG 326, [No location noted], [This Document is Secret-RD].

Bradbury submitted his 1950 proposed program for Los Alamos to Carroll Tyler in December 1949, he informed the Commission that Los Alamos intended to continue the fission program at the same pace as in 1949, and augment research "on the problem of attaining a nuclear reaction involving the light elements," by 1952.[375]

Bradbury submitted his 1950 proposal before President Truman's announcement, but indicated that those in New Mexico stayed well aware of the debates taking place in Washington over the thermonuclear program. The Laboratory's members generally agreed, the director reported, that the questions being posed about the Super's practicality, military value, engineering, stockpiling, and morality would not be answerable until Los Alamos had a better theoretical and experimental thermonuclear program underway. Only then, the director advised the Commission, could many of the issues surrounding the Super "be resolved without recourse to hypothesis or wishful thinking."[376]

The director did include in his proposal some figures related to an enlarged H-bomb program: a request for 250 grams of T for a thermonuclear test in 1952; and, funding for an expansion of the Laboratory's staff by about 200 individuals in 1950, and 200 more in 1951.[377]

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[375] AEC Meeting No. 363, [Minutes], February 2, 1950, Doe Archives, RG 326, [No location noted], [This Document is Secret-RD]; Document submitted to Carroll Tyler from Bradbury, December 9, 1949, "Los Alamos Scientific Laboratory Technical Program for Calender Year 1950," DOE Archives, RG 326, Box 4944 (635.12) Los Alamos, Folder 7, (1-13-47), [This Document is Secret-RD].

[376] Bradbury to Tyler, December 9, 1949, op. cit.

[377] Ibid.

The Commissioners asked their scientific advisors to comment on Los Alamos's plans for that year, and when the GAC met early in 1950, its members suggested that the Laboratory include a test of the second part of the Super problem -- a study of propagation of the detonation into D -- to provide a test of the Super's overall feasibility, in addition to a test of D-T thermonuclear initiation. Bradbury noted in his 1950 proposal that the GAC did not believe that the "electronuclear machines," the MTR, or any other proposed reactor would meet the Los Alamos's suggested T requirements on the time schedule. For the AEC to approve the Los Alamos program, then, meant that Los Alamos would have to accept less tritium than it requested, or the Hanford would need conversion into enriched pile operation.[378]

Despite the GAC's comments, Bradbury nevertheless modified the Laboratory's program for 1950, stating that research pertinent to thermonuclear weapons would be accelerated, and several proposed lines of development related to the hydrogen weapon would be evaluated that year. For this work, Los Alamos would need to receive 40 to 50 grams of T by the end of 1950, and 250 to 350 grams by the latter part of 1951. The more tritium available, the more flexible the experimental thermonuclear program could be. Finally, the laboratory would now need to expand its staff by 300 people in 1950, and 150 more in 1951.[379]

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[378] GAC Report of Meeting 19 to Lilienthal, Febrary 1, 1950, JCAE delassified General Correspondence Files, Box 34, NARA.

[379] Document transmitted to Tyler from Bradbury, March 10, 1950, "Los Alamos Scientific Laboratory Technical Program for Calender Year 1950," DOE Archives, RG 326, Box 4944, (635.12) Los Alamos, Folder 7, (1-13-47), [This Document is Secret-RD].

The revised program Bradbury submitted to Tyler was idealistic. In practice the Laboratory compromised with other facilities in the system for materials production. In compromising, Teller and Froman held a meeting on T production with several representatives from Oak Ridge, Hanford, and other plants. Nevertheless, Los Alamos remained, as far as nuclear materials went, subject to the limitations of these other facilities. [380]

Hanford might employ less that one pile to produce 40-50 grams of T per year, although Froman learned that in principle Hanford could go to a "so-called full scale production schedule" employing one entire pile. If the cooling water in the temperature could be raised safely, and faster flows obtained, tritium could possibly be produced at the rate of 90 grams per month.[381]

Hanford never adopted this demanding T production schedule, probably because by the end of 1950, the feasibility of the Super had become questionable and, other means of producing massive quantities of tritium began to appear more promising. In April the GAC recommended to the Commission that for long-term T production, heavy water reactors were the least wasteful and would not deplete the AEC's reserve of fissionable material, that a knowledgeable industrial contractor such as du Pont be asked

[380] ADWD-100, Memorandum to Bradbury from Froman, February 10, 1950, "Tritium Production," LANL Archives, B-9 Files, Drawer 102, [This Document is Secret-RD].
[381] Ibid.

to build these facilities, and that Lawrence's proposal to build an accelerator for T production be taken seriously.[382]

At Los Alamos, Bradbury no longer took the Super very seriously. He reported to Tyler in November 1950:

> The concentrated research and investigation in this field over the past year has shown that the probability of early, practical success along the lines originally conceived [The Super] is considerably less than might have been anticipated earlier. Furthermore, practical success along those lines, if it can be attained at all, without new and presently unforeseen conceptions, must be regarded as more distant.[383]

On the other hand, the Laboratory continued with plans to go ahead with the Greenhouse test series, including the "George" and Booster "Item" devices. Until Los Alamos tested these devices, and the two parts of the Super problem were definitively solved, Bradbury could not give the AEC an accurate figure for the amount of T the laboratory would need in the coming year.[384]

On the same day that Bradbury submitted his proposal to Tyler, he hosted the AEC, and LeBaron and the MLC at Los Alamos, and explained that he viewed the Super as dubious mainly on economical terms. Over the course of 1950, the amount of tritium required and the device's overall

[382] Mansfield Memo, May 28, 1952, 9, GAC Meeting 20, April 1, 1950, op. cit.
[383] Document transmitted to Tyler from Bradbury, November 17, 1950, "Los Alamos Scientific Laboratory Technical Program for Calender Year 1951 and Fiscal Year 1952," DOE Archives, RG 326, Box 4944, (635.12) Los Alamos, Folder 7, (1-13-47), [This Document is Secret-RD].
[384] Bradbury to Tyler, November 17, 1950, op. cit.

projected cost had increased at such a rate that it would put off a test until at least 1954.[385]

Over the course of 1950 Teller's Family Committee reported that greater and greater amounts of tritium would be needed for a Super. A month after Ulam had formally presented his group's calculations predicting a poor chance for igniting the Super with less than nearly a kilogram of T, the Family Committee took up the issue. They concurred that setting off a "conventional" Super without compression of the main charge would require even more than a kilogram of T. The Committee noted that up until the present, the Laboratory had been planning for a test following the 1951 thermonuclear principles tests, where they would try to ignite large masses of D-T simply as a "fuze." However, the Committee agreed, given the predictions of the amount of T needed for such a test, it would be wasteful.[386]

Teller and Wheeler subsequently filed a large report on the status of Los Alamos's thermonuclear project with McCormick and the GAC in August, acknowledging tritium as an outstanding bottleneck to the Super. The most recent estimates, Teller and Wheeler reported, showed that the uncompressed amount of T required to ignite uncompressed D, stood on the order of "a kilogram or more but not of the order of tens of kilograms." Rationalizing, Teller and Wheeler suggested that a great expenditure of T could be justified by how little deuterium cost comparatively:

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[385] Draft Memorandum to Chairman of the AEC, "Notes on the AEC-MLC-LASL Conference on Tuesday, November 14, 1950," November 17, 1950, DOE Archives, RG 326, Box 4944, (635.12) Los Alamos, Folder 7, (1-13-47), [This Document is Secret-RD].

> Thermonuclear weapons were given a new look in February 1950. At that time, a review was made of the means to get bombs with yields of the order of a thousand time that of conventional weapons. By far the most promising plan called for ignition of a [large] amount of deuterium . . . ("Super Bomb") by a smaller mass of deuterium-tritium mixture. Tritium is very expensive, one kilogram costing the same number of Hanford neutrons as 80 kg of plutonium. . . . Nevertheless . . . the relatively low cost of ton-amounts of deuterium, led to the decision to work intensively on the problem of deuterium ignition.[387]

While Teller and Wheeler continued to hold the torch for the Super, they also reported to McCormack and the GAC that over the last two months Teller had come up with a modified Alarm Clock. However, like the Super, this version of the Alarm Clock needed a great deal of tritium for ignition. Still, little work on this idea had been carried out.[388]

Not the GAC, but the Joint Committee, expressed grave concern by the end of 1950 that the AEC failed pursuing an increased production program fast enough. While Truman had approved expenditure for two new heavy water reactors the previous June, and an additional three by October, at the newly chosen Savannah River, South Carolina site, Borden still did not feel that the AEC did not make an "all out" plant expansion effort.[389]

The Commission had managed to bring du Pont back into the system to build the Savannah River facility, and initiated construction on the heavy-water reactors by early 1951, but Los Alamos had not yet established a

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[386] ADWD-163, Minutes of Family Committee meeting 17, July 20, 1950, LANL X-Division Vault, [This Document is Secret-RD].
[387] LAMD-443, "Part I, Status of Thermonuclear Development," prepared by Edward Teller and John Wheeler, August, 1950, 6, [This Document is Secret-RD].
[388] Ibid., 43-46; Bethe Chronology, 12, op. cit; Hansen, Swords, III-38.

requirement for the Commission for any definitive amount of tritium. Instead, Bradbury had only been able to give estimates of what the Laboratory might need for both the 1951 tests and a subsequent test of the Super. Bradbury could not provide the AEC accurate estimates for tritium since the estimated amount needed for the Super kept increasing over the course of 1950. Thus, some of the AEC's perceived sluggishness in plant expansion stemmed from Los Alamos's theoretical Super program itself.[390]

"Great Progress in Showing Lack of Knowledge"

At the October-November 1950 GAC meeting, held at Los Alamos and already mentioned in Chapter Three, Carson Mark gave a general description of Ulam and Everett's, and the recent ENIAC calculations on the first part of the Super problem. In Ulam and Everett's first D-T mixture problem, the temperature dropped without propagating. The second hand calculation also began with the same mixture of D-T but this time with more of the latter isotope in the central zone. Again, the temperature of the D outside dropped without propagating.[391]

Mark, with von Neumann, described the ENIAC's treatment of these problems. They explained that in the first run, the team stopped the problem after 8 zones when it looked like the reaction in D was not progressing, However, Mark noted that in this problem there were indeed too many

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[389] JCAE chronology, 22, 26, op. cit; Draft of document of William L. Borden, "The Case for Further AEC Expansion," December 16, 1950, JCAE declassified General Subject Files, Box 4, NARA; Hewlett and Duncan, Atomic Shield, 525.
[390] Hewlett and Duncan, Atomic Shield, 531.

unknowns, such as the effect of inverse Compton on the large central zone. The group tried other variations with problem, such as varying combinations of D-T, and more and more tritium overall. Although the team did not carry any of the variations out to completion, all the problems indicated that no reaction would start in the deuterium.[392]

Theoretical problems aside, Teller knew well that the Super -- as Los Alamos envisioned it from 1946 -- embodied more practical obstacles than just the means of calculating it, materials, and thermodynamic and hydrodynamic effects. When Libby asked Teller whether or not "purely theoretical considerations would be sufficient to decide on the feasibility of the Super," he responded, "There has been great progress in showing lack of knowledge as a result of the extensive calculations to date. Further progress by this method won't be made if people work on something else or if machines are not available." Teller may have honestly believed that D-T would burn, but professed that greatest uncertainties remained in the area of "radiation engineering." The best arrangement for the Super remained to be seen, and although Teller thought that D-T would burn, he felt at least certain that small amounts of "tritium will not be enough to start a pure deuterium Super unless new tricks come into the picture."[393]

Teller's response to Libby reflected Los Alamos's confusion regarding the Super; even if the term "radiation implosion" had been coined already,

[391] Minutes of Meeting of the Twenty-Third Meeting of the GAC, October 30, 31, and November 1, 1950, Los Alamos, NM, DOE Archives, RG 326, Box 1217, AEC-377-GAC, Folder 10, [This Document is Secret-RD].

earlier that year within the Family Committee, it held no meaning yet as to making a full-scale thermonuclear weapon work. An alternative path towards a hydrogen bomb, Teller thought, would only encounter the same problems such as tritium and difficulty of calculations. He also emphasized this to the GAC:

> [It] is completely misleading if one thinks about a Super at all in the sense of having a design, a design with such walls, a design with no walls, or a design which is a cylinder or a design which is a long slab. Any of these things and many more complicated things may be fitted into the picture as soon as we catch our breath either because tests are finished or because we can get more help.[394]

If "tricks" were the key to making a workable hydrogen bomb design, then Teller dismissed an important "trick" at this meeting -- compression of the deuterium. Bethe had already mentioned this at the CWD earlier that year, and now, Fermi suggested to the GAC that if propagation of deuterium did work, then compression would improve the situation. Teller responded that while one might think of "tricks," compression was not one of them.[395]

Compression of the Issues, and Circumventing the Tritium Problem

Compression actually played a role in the Classical Super theory, yet not in a manner conducive to making the design work. Thus, the Greenhouse George test, undertaken a few months after the GAC's November 1950 meeting, had been set up as an "experiment," Teller explained, to heat, compress, and ignite a D-T mixture like one that would be

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[392] Ibid.
[393] Ibid.
[394] Ibid.
[395] Ibid.

used in a Super. On the other hand, Teller stated many years later, the notion of compressing pure D itself represented an "obvious solution" that had been raised many times before 1951 when Teller, Ulam, and de Hoffman combined their ideas. Teller claims that prior to 1951 he ignored the thought of compressing D, dismissing it as unimportant or unworkable.[396]

Teller's, Ulam's, and de Hoffman's individual contributions to the discovery of a viable thermonuclear device have been examined in several studies, including Rhodes's Dark Sun, York's The Advisors, and Hansen's The Swords of Armageddon, and thus I will not contribute to the debate over who invented the first workable American hydrogen bomb.[397]

Bethe has called the Teller-Ulam configuration an accidental choice, but this "accident" seemed partly the result of the George test, which used x-radiation from a fission bomb to compress and ignite D-T. Still, the final arrangement that Teller, Ulam, and de Hoffman proposed in 1951 for a full-scale hydrogen bomb test constituted a much more elaborate configuration than George. Teller, Ulam and de Hoffman's ideas were, according to Bethe, "completely novel concepts in this field."[398]

Teller has also dismissed the novelty of radiation implosion, calling it an "important but not unique device in constructing thermonuclear bombs," and that the "main principle of radiation implosion was . . . stated in a

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[396] Teller classified lecture, March 31, 1993, op. cit.
[397] For more on this, see: Rhodes, Dark Sun, 455-472; York, The Advisors, 75-80; Hansen, The Swords of Armageddon Volume III, 159-183.
[398] Hans A. Bethe, Memorandum on the History of the Thermonuclear Program, May 28, 1952, 7, op. cit; RS 3434/100, SC-WD-68--334, F. C. Alexander, Jr., "Early Thermonuclear Weapons

conference on the thermonuclear bomb in the spring of 1946." Still, one of the most important characteristics of the Teller-Ulam device that its inventors overlook in their personal reminiscences is that the new design did not employ tritium.[399]

Over the course of 1951 Teller, Ulam, de Hoffman, and according to Rhodes, physicists Arnold Kramish and Max Goldstein, refined their ideas into a preliminary design. Before Teller and Ulam filed their March 9, 1951 report describing the new thermonuclear configuration, the Hungarian contacted Borden, complaining of sluggish progress within Los Alamos's H-bomb program, in part due to the small number of "first-rate theoreticians " that the Laboratory recruited for the project. Work on the Super configuration carried out over 1950, Teller informed Borden, indicated that this idea was "not as promising as it once looked." Because he and his colleagues had focused so intently on the Super, Teller relayed to Borden, "Los Alamos was obliged to overlook, in large measures, several other interesting possibilities," which no doubt included the Teller-Ulam configuration.[400]

Even if he and others had "overlooked" the Teller-Ulam design, throughout most of 1951 Teller became increasingly agitated at Bradbury and Froman for not immediately launching a program to develop the Teller-

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Development: The Origins of the Hydrogen Bomb," May 1969, Sandia Laboratories, 15, [This Report is Secret-RD], op. cit.

[399] Memorandum to the File from Walker, "Thermonuclear Program -- Dr. Teller's Answer to the Bethe Chronology," August 15, 1952, JCAE declassified General Subject Files, Box 59, NARA.

[400] Rhodes, Dark Sun, 467; Memorandum to the Files from Borden, "Conversation with Dr. Edward Teller," February 9, 1951, JCAE declassified General Subject Files, Box 58, NARA.

Ulam bomb. Although Los Alamos had committed to perform the Greenhouse tests in June 1951, and preparing for this occupied most of the Laboratory's time in the first half of that year, Teller pressured Bradbury to create a new, separate thermonuclear division, that Teller would lead.[401]

Teller wanted this because he believed that thermonuclear work had "so far been dispersed in several divisions which have heavy commitments elsewhere." Bradbury and Froman opposed the idea of a new thermonuclear division. However, Teller still retained von Neumann's and Wheeler's support, since both wanted a greatly enhanced thermonuclear program. Even before proposing the establishment of a new division to Bradbury, Teller and de Hoffman both traveled to Washington to complain of the lack of effort at Los Alamos towards thermonuclear development. Sans Teller, de Hoffman informed Dean that Manley, Holloway, Jetty [sic], and probably Bradbury advocated a leisurely approach to the hydrogen bomb project; likewise, Teller and de Hoffman told Strauss that the Los Alamos program was not "all out" and thus did not live up to the President's directive. By March 1951 Teller and de Hoffman both threatened to leave.[402]

Froman tried to compromise with Teller, offering to set up a small group on the order of twenty-five people, who would be primarily responsible for hydrogen bomb work. Teller would not agree to this arrangement, and over the summer of 1951 threatened to resign from Los

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[401] Rhodes, Dark Sun, 473.

Alamos several times, although he did not actually do so until Bradbury appointed Holloway as head of the thermonuclear program to design and construct Mike.[403]

Teller admitted to Kenneth Mansfield in a private conversation that Teller himself felt responsible in some part for the "more hopeful attitude exhibited for the 'super' program." Still, he proceeded to complain about Bradbury, saying that the Laboratory director ordered that work on the H-bomb should:

> . . . proceed in such a fashion that one model should either be proven or disproven before research was directed towards another. This would have meant working on a classical model until it was adjudged a success or a failure, and then only turning to others.[404]

Teller chastised his Los Alamos colleagues:

> Dr. Teller felt, however, that this one-thing-at-a-time approach was gravely in error, and he suspected that Los Alamos would use a confession of failure upon the classical model as a justification for abandoning or cutting down to trivial proportions the entire H-bomb program.[405]

Los Alamos, Teller lamented to Mansfield, was rapidly taking on all the features of a monopolistic and secret bureaucracy at its worst. The laboratory leadership -- namely Bradbury and Holloway -- constituted the biggest problem, had become "less and less adventurous scientifically," and

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[402] ADWD-250, Memorandum to Bradbury from Teller, "Plan for Setting up a Separate Thermonuclear Division," March 24, 1951, DOE Archives, RG 326, Box 1235 (635.12) LASL, Folder 33 (1-13-47); Anders, Forging the Atomic Shield, 116-177.
[403] Anders, Forging the Atomic Shield, 132.
[404] Memorandum to the File from Kenneth Mansfield, August 28, 1951, "Conversation with Dr. Teller," JCAE declassified General Subject Files, Box 58, NARA.
[405] Ibid; Anders includes several of Gordon Dean's diary entries in Forging the Atomic Shield regarding Teller's complaints about Bradbury and Los Alamos's leaders.

now regarding their main mission as protecting the Laboratory from outside criticism. Thus, the laboratory would only embark upon projects almost certain to be successful.[406]

Teller appealed to the JCAE for approval to set up his own Laboratory, later founded at Livermore, California. As Rhodes has argued, Teller did not want to give up the Super, which he claimed looked much more optimistic than a year before due to the results of Greenhouse, and another set of revised D cross sections. On the other hand, Teller did not bring up the tritium problem with Mansfield, or the news that Los Alamos was indeed preparing to set up a program to develop the Teller-Ulam configuration. Teller also did not mention that Los Alamos was not socially, technically, and administratively prepared to undertake a large-scale thermonuclear research, development, and test program before completion of the Greenhouse series.[407]

Although Teller had been excited by the prospect of an H-bomb that did not use tritium, he lost interest in it. When Bradbury appointed Holloway head of the hydrogen bomb project in September 1951, the Laboratory had already made a commitment to develop Teller's new proposal, having described two tentative designs to the AEC. Paul Fine relayed to Walker that "the importance of these decisions should not be over-estimated . . . . The decision to build the . . . [new design] . . . means that tritium is probably not

[406] Mansfield memo, August 28, 1951.

going to be necessary." With Los Alamos's turn towards the Teller-Ulam device, scientists reduced tritium from a critical problem to one of simply obtaining enough material for a boosted fission weapon.[408]

The same month that he resigned from Los Alamos, Teller's mother and father were interned in a Hungarian detention camp. Borden expressed his fear to Walter Smith, then Director of the Central Intelligence Agency, that he hoped the Soviets did not realize they had Teller's parents. If they did, they might impose "mental torture upon our number one expert on the H-bomb."[409]

"It would be impossible to run a laboratory if you had no Dr. Teller's and it would be equally impossible to run one if you had all Dr. Teller's,["] Max Roy lamented to Mansfield in late August 1951. However, although Roy admitted to Mansfield his opinion that "95 per cent [sic] of Dr. Teller's ideas are crazy," the Hungarian still "served a very useful role in stimulating other minds to action."[410]

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[407] Mansfield memo; For more on Lawrence Livermore National Laboratory and its weapons programs, see: Sybil Francis, "Warhead Politics: Livermore and the Competitive System of Nuclear Weapon Design," (Ph.D. Dissertation, Massachusetts Institute of Technology, 1995).
[408] Memorandum to the Files from John Walker, October 10, 1951, "Conversation with Mr. Paul C. Fine, Technical Assistant, Division of Military Application, AEC, and the undersigned on October 9, 1951 regarding the thermo-nuclear weapon," JCAE General Subject Files, Box 62, [This Document is Secret-RD].
[409] Letter from William Borden to Walter Bedell Smith, September 28, 1951, JCAE declassified General Subject Files, Box 58, NARA.

One Technology or Another: The System Was Not Ready for an H-bomb

In The Swords of Armageddon, Hansen cites several reasons to back up his argument that Los Alamos took a long time to develop a hydrogen weapon, and I review these reasons in the conclusion of this dissertation. Hansen acknowledges that "the requirement for tritium was crucial and ultimately decisive," and he cites numerous references to this problem throughout his work. Indeed, tritium played a crucial role in the fusion bomb program, however, this critical problem may also be seen as one of the most important factors that highlights the weapons design laboratory's place within the AEC system.[411]

Here, when focusing on a particular obstacle to the thermonuclear project, the term "critical problem" is preferable to "reverse salient" because the former better applies to specific identifiable hindrances or bottlenecks at, as MacKenzie points out, the *micro* level. On the other hand, reverse salient is more applicable on the *macro* level, where a problem holds up the growth of the entire system. In the case of the postwar H-bomb project, tritium, or computing as well, did not hold up the growth of the large AEC system as a whole (where the fission weapons endeavor grew slowly but steadily) as much as they affected the course of thermonuclear weapons development alone.[412]

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[410] Memorandum for the Record from Ken Mansfield, "Los Alamos Opinions of Doctor Edward Teller," August 29, 1951, JCAE declassified General Subject Files, Box 58, NARA.
[411] Hansen, Swords, III-87, 183-189.
[412] Donald MacKenzie, "Missile Accuracy: A Case Study in the Social Processes of Technological Change," in Bijker, Pinch, and Hughes, Social Construction, op. cit, 195-222.

Nuclear materials as an obstacle to the hydrogen weapon program came out of the AEC system that Los Alamos depended on. How did scientists resolve the tritium bottleneck resolved? Hughes has noted that in the history of technological change conflict occurs between or among technological systems.[413] Likewise, conflict may develop within a system itself as it grows, and different social, economic, or technical portions of systems may compete or clash with one another. In this case, both occurred.

Conflict developed within the AEC system after the U.S. detected the Russian atomic bomb. The political conflicts within the system became obvious as leaders of the postwar nuclear weapons and energy research system took opposing positions in regards to development of hydrogen bombs. Although perhaps a short-term bottleneck in itself, the Joint Committee and American military leaders quickly overrode the GAC's decision not to endorse large-scale research on the Super. The GAC's based its decision for the most part on technical considerations, not least among them the projected amount of T that the Super would need to work. Thus, technical conflicts grew from latent to critical in the system.

Technical conflict in the form of nuclear materials appears as a key factor in hindering the postwar H-bomb program, considering that once scientists replaced the Super with the Teller-Ulam configuration as the fusion design of choice, the GAC became less opposed to thermonuclear weapons development. According to Teller, and historians who have examined the

[413] Hughes, Networks, 106.

June 1951 GAC Princeton meeting records, the Committee quickly supported the new idea and encouraged Los Alamos to go ahead with it. Teller recounted to John Walker that right before the Princeton meeting began, Wheeler held another meeting. When informed of the Teller-Ulam configuration, Oppenheimer supposedly remarked how "wonderful" the idea looked. Subsequently, at the main GAC meeting the Committee encouraged Los Alamos to go ahead with the Teller-Ulam design. Galison and Bernstein have confirmed the tone of the GAC's optimistic mood in their interpretation of the meeting's minutes, noting that the Committee viewed the Teller-Ulam configuration as "a certainly interesting, possibly encouraging line of attack."[414]

With the GAC's consent, at least this particular social component of the system fell into agreement with further research and development of thermonuclear weapons. However, by this time Los Alamos had completely circumvented the tritium crisis that by now had plagued the thermonuclear program for several years. If the Teller-Ulam design constituted the "trick" to overcoming the tritium problem, it represented a successful but frightening solution that brought the system back in line, in that the Commission's less-than-adequate tritium production facilities no longer mattered.

In spring 1950, Bradbury and Froman had asked some of their fellow scientists to comment on the revised Laboratory program before submitting it to Tyler. One reviewer -- probably Teller -- had asserted that the quantities of

[414] John Walker, "Memerandum to the File," January 13, 1953, JCAE declassified General Subject

T available at future dates might well prove to be the determining factor in the rate of progress of the hydrogen bomb program, and that in a "period of relative scarcity of tritium," Los Alamos needed to focus on theoretical and experimental studies of the ignition of D. The Laboratory did not stand in a position to be able to do much more than that.[415]

While tritium was scarce due to the AEC's inadequate production system, on the other hand Los Alamos could not give any clear estimates of the amount of tritium it would require to construct and test a Super prior to 1950. Here, the computing and tritium problems crossed. Scientists and hand computers completed only a few calculations for the first part of the Super problem in the 1940s. The dubious accuracy of this work stemmed at least partly from the computing bottleneck.

The ignition calculations' inaccuracy also may have also been partly Teller's fault, or to some degree arose from the Hungarian's enthusiasm for the Super. Did Teller cheat in his calculations, as Serber later suggested? "Cheated" is too strong a description for Teller's calculations, especially since Teller did not himself perform most of the calculations for the Super's ignition and propagation in the postwar. More likely, Serber also recalled, Teller was always "overly optimistic, and he never made an honest estimate" in his theoretical work on the Super. Fellow scientists such as Metropolis,

Files, Box 58, NARA; Galison and Bernstein, "In any Light," 323.

[415] Memorandum submitted to Bradbury and Froman on "Laboratory Program Draft of March 3, 1950," No author, No Date, LANL Archives, B-9 Files, Folder 635 - Lab Program, 1948-1950, Drawer 176, [This Document is Secret-RD].

Frankel, Turkevich, and others, were, according to Serber, "biased by Teller's enthusiasm."[416]

Max Roy accurately described the Hungarian physicist as having a talent for stimulating others' creativity. In a way, Teller had to do this since he was only at Los Alamos as a visitor between 1946 and 1949, and he had to encourage others to perform hand calculations on the Super problem in the postwar. Ironically, Teller's own absence from the Laboratory indicated other bottlenecks to H-bomb development, including a labor shortage at Los Alamos, and a lack of housing for personnel. Tritium and computing were not the only critical problems standing in the way of a thermonuclear device; other problems arose both within the AEC system and from outside of it -- particularly in the American military establishment. I analyze these problems in the next chapter.

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[416] Author interview with Serber, November 26, 1996.