From MED to AEC

Bradbury had studied physics at Berkeley and also served as a Naval Lieutenant Commander during the war. In 1944 Groves had intervened to make sure that Bradbury would be transferred to Los Alamos from the Dahlgren Naval Proving Ground, since Oppenheimer had requested Bradbury's assistance with research on explosive lenses. Bradbury subsequently led the team that assembled the Trinity device. Preparing to return to California and academia, Oppenheimer recognized Bradbury as practical and committed to nuclear weapons work, and thus nominated him to take over directorship of Los Alamos in September 1945.[113]

While Bradbury never doubted that nuclear weapons would play an important role in the postwar period, Oppenheimer and some of his scientific colleagues expressed contradictory views on the future of Los Alamos and nuclear research at various times. Oppenheimer expressed guilt over the bombings of Hiroshima and Nagasaki, and in the fall following the end of the war, suggested to Acting Secretary of State Dean Acheson and Secretary of War Henry Stimson's aid George L. Harrison that many Los Alamos scientists objected to performing any further work on nuclear bombs.[114] A few months earlier, in August 1945, the Scientific Panel of the Interim Committee on Postwar Policy -- made up of Oppenheimer, Lawrence, Arthur Holly Compton, and Fermi -- which advised Henry Stimson on future nuclear

[113] Hoddeson, et al., Critical Assembly, 59.

policy, had expressed "[G]rave doubts that this further development [of nuclear weapons] can contribute essentially or permanently to the prevention of war."[115]

If the committee's attitude towards national policy on atomic weapons appeared cautious, their technical recommendations for the future of atomic energy and nuclear weapons seemed much more optimistic. Only two months prior to the August meeting of the Scientific Panel, the group had presented a letter recommending that problems of improving the fission bombs developed during the war might come under the jurisdiction of the ordnance organizations of the Army and Navy. Furthermore, the committee stated that:

> . . . the subject of thermo-nuclear reactions among light nuclei is one of the most important that needs study. There is a reasonable presumption that with skillful research and development fission bombs can be used to initiate the reactions of deuterium, tritium, and possibly other light nuclei. . . .[116]

The Committee also recommended that the Government should spend about a billion dollars a year after the war to support an active research program in nuclear energy.[117]

If Oppenheimer's feelings about the place of nuclear weapons seemed ambiguous, so did his attitude about Los Alamos's future, at least in the first half of 1945. During that spring Oppenheimer wrote to Groves confessing

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[114] Rhodes, Dark Sun, 204.
[115] Bradbury quoted in Rhodes, Dark Sun, 203.
[116] "Recommendations on Future Policy," June 16, 1945, in United States Joint Committee on Atomic Energy Records (hereafter JCAE), declassified General Subject Files, National Archives, Box 60.
[117] Hewlett and Anderson, The New World, 367.

that he was in the dark about "what plans have been discussed in high quarters for the future of gadget development in the country," and that many of the staff at Los Alamos regarded their positions as temporary war emergency jobs. Furthermore, Oppenheimer summarized:

> The whole organization, temper, and structure of Site Y laboratories is singularly unsuited for peacetime perpetuation . . . . [although] some members of the Laboratory could and should be persuaded to continue this work after the war, but I think that there will have to be a very great change in the way in which the Laboratory is set up and very probably an actual shift in its physical location.[118]

Oppenheimer told Groves of his desire to leave Los Alamos, mentioning that "The Director himself would very much like to know when he will be able to escape these duties for which he is so ill qualified and which he has accepted only in an effort to serve the country during the war." Oppenheimer explicitly advised some of his colleagues to leave, including his assistant John Manley who had worked on fast-neutron experiments during the war, and Teller, much to his dismay.[119]

Bradbury recalled that even by September 1945, there existed "no agreement as to what sort of future should be planned for Los Alamos." Bradbury described the uncertain situation:

> There was one school of thought which held that Los Alamos should become a monument, a ghost laboratory, and that all work on the military use of atomic energy should cease. Another group looked with increasing pessimism on the deterioration of our international relations and contended that Los Alamos should become a factory for atomic weapons. The majority agreed that, for the present at least, the United States required a research laboratory devoted to the study of

[118] Letter from Oppenheimer to Groves, May 7, 1945, in JCAE, declassified General Subject Files, National Archives, Box 41.
[119] Ibid.

fundamental nuclear physics and chemistry and their possible applications to military use.[120]

For about one and a half years after the war ended the Laboratory struggled for its existence. Bethe wrote, " . . . in 1946 the Los Alamos Laboratory was very weak," and it was not obvious that "there was any need for a large effort on atomic weapons in peacetime." In a similar tone, experimental physicist Raemer Schreiber noted that work at Los Alamos after the war came to a halt.[121]

Those scientists who opted to remain at Los Alamos during peacetime credit Bradbury with keeping the facility operating. Metallurgist Edward Hammel believed that Bradbury was absolutely crucial in holding Los Alamos together during this uncertain time: "What we accomplished during the war was based on basic research. If we were going to continue this whole business each division leader was specifically required to maintain a basic research program within his division, and that came from Bradbury."[122] Encouraging staff members to remain, Bradbury presented a research program to entice them -- improving the existing fission implosion and gun weapons.

Bradbury made up his mind that he wanted to stay at Los Alamos for the long-term, recalling his thoughts during the war, "If it [the fission project] works, if I get there, I'll never get out of it." Pragmatic in his style of managing the uncertain postwar weapons program, Bradbury tried to

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[120] Bradbury quoted in Rhodes, Atomic Bomb, 755.
[121] Rhodes, Dark Sun, 201.
[122] Hans A, Bethe, "Comments on the History of the H-Bomb," 45; Author interview with Edward F. Hammel, Los Alamos, NM, December 14, 1994.

reassure the remaining staff that there was much nuclear weapons research left to perform. Bradbury did his best to assure his colleagues that even if Los Alamos's exact role in the postwar was uncertain, federally-supported research in atomic energy problems would continue, because the Manhattan District would be taken over by a new legislation created-commission.[123]

Despite the lack, in 1945 and 1946, of a formal mission handed down from the military, Bradbury already had an agenda in mind for Los Alamos. Bradbury expressed concern about the crudeness of the wartime implosion and gun weapons. He reminisced, "We had lousy bombs . . . . a set which were totally wrongly matched to the production empire."[124] Los Alamos's self-imposed mission, Bradbury announced to his colleagues in the fall of 1945 -- in the absence of one handed down by the MED -- would be to improve the gun and implosion devices, making several changes especially to the latter, by developing many internal modifications, potentially changing the fusing and detonating methods, and creating a "levitated" implosion device. In addition, the Laboratory would begin to "engineer new weapons that embodied increased reliability, ease of assembly, safety, and permanence. . . [As] [m]uch as we dislike them, we cannot stop their construction now." Convinced that Los Alamos had to continue its wartime work, Bradbury believed that he had to work quickly to lure new staff to the Laboratory to replace the many who left in 1945 and 1946. In an interview many years later,

[123] Arthur Norberg, Interview with Norris E. Bradbury, Los Alamos National Laboratory Research Library, (Los Alamos Scientific Laboratory, 1980); Norris E. Bradbury, presentation given at Los Alamos Coordinating Council Meeting, October 1, 1945, reprinted in LAMS-2532

Bradbury recalled his opinion on Los Alamos's purpose, stating, "Look, we're basically going to be a weapons laboratory. I'm going to buttress it with all the basic research I can get to support that weapons research."[125]

Despite his confidence that Los Alamos could continue nuclear weapons development, Bradbury knew that his facility would be dependent on a larger federal atomic energy agency once such an organization had been established. Los Alamos's weapons program would depend somewhat, for example, on the rate of nuclear materials production established by the MED's successor. Bradbury wanted Los Alamos to have some freedom to create and design weapons of its own choice as it did in the war, and establish its own general research agenda. He feared that the successor to the Manhattan District would not allow the Laboratory any autonomy and instead would try and exert too much control. The new director asserted that he, ". . . was not going to let that AEC take us apart," and thus he and other remaining Los Alamos staff attempted to drew up a specific philosophy for the laboratory and establish long-term technical goals before Congress formally established the AEC in 1947.[126]

Well before the war's conclusion, scientific and military constituents alike began to plan for an agency to replace the MED in peacetime. The AEC was formed between 1944 and 1946 with input from several scientific, military, and legislative committees. Vannevar Bush, now serving on the

(Volume II), 113-125.
[124] Norberg interview with Bradbury.
[125] Bradbury presentation, October 1, 1945; Norberg interview with Bradbury.

Military Policy Committee, suggested that a Postwar Policy Committee be formed to consider the American government's future nuclear program. Headed by Richard Tolman, The Postwar Policy Committee recommended, based on interviews with "scientists representing the Manhattan Project's principal research centers," that the U.S. needed to maintain military superiority through atomic energy. Furthermore, the Committee suggested that the U.S. continue work on U^{235} separation, Pu^{239} and U^{233} production, and nuclear weapons development.[127]

Towards the end of 1944 Conant and Bush pressed Stimson to chair a new high-level advisory committee that included Bush, Conant, Karl Compton, Undersecretary of the Navy Ralph A. Bard, Assistant Secretary of State William L. Clayton, and former Director of War Mobilization James F. Byrnes. This group intended to focus on developing an international atomic energy policy and the U.S.'s own nuclear research and development policy and suggest legislation regarding it. Bush and Conant already had a preliminary scheme for a commission on atomic energy by July 1944.[128]

Bush and Conant initially conceived of a twelve-person commission appointed by the President, representing a mix of scientists, engineers, other civilians, and military representatives. With the assistance of two War Department lawyers, Kenneth C. Royall and William L. Marbury, the Interim Committee drafted an atomic energy bill in July 1945 that called for an

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[126] Norberg interview with Bradbury.
[127] Hewlett and Anderson, The New World, 325.
[128] Ibid., 367, 409.

organization of nine commissioners, with the commission having custody of raw materials and deposits, plants, production facilities, technical information and patents. Whereas Bush and Conant wished for a civilian-controlled commission, Royall and Marbury, true to the War Department and to Groves's own interests, drafted the bill to include strong military representation on the commission.[129]

1945 saw the proposal for a commission on atomic energy revised and countered aggressively. In Congress, legislators presented another version of the Royall-Marbury bill in fall 1945: Senator Edwin C. Johnson of Colorado, a ranking member of the Military Affairs Committee, together with Congressman Andrew Jackson May, introduced the controversial May-Johnson bill. Groves, Bush, Conant, as well as Oppenheimer, Lawrence and Fermi viewed the May-Johnson bill as acceptable, even though by this time the Army and Military Affairs Committee had been behind the redesigning of the bill to insure military control over nuclear research. Secretary of War Robert Patterson nicknamed The May-Johnson bill the "War Department's bill."

Probably wanting to hold the system together more than anyone else, Groves wanted to establish an organization to replace the MED as soon as possible. The Manhattan District had been set up as a temporary organization; now many of its contractors wanted to sever their ties from it. Groves also knew that his own authority as military head of the MED was

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[129] Ibid., 409-413.

fading. A new peacetime atomic energy organization, Groves hoped, would keep the large system of laboratories and production facilities operating in some sort of harmony. Although he never explicitly stated that the new commission should be led by military rather than civilian representatives, he felt that military experience with nuclear weapons during wartime would play a more important role in controlling nuclear research in the near future.[130]

In 1945 the details of the May-Johnson bill were not public, yet many scientists at Los Alamos, the Metallurgical Laboratory, and Oak Ridge heard unofficially that the bill would allow the military too much control over peacetime nuclear energy research. In addition, the bill provided for excessive security restrictions.

Scientists feared policy decisions made by persons without much technical understanding of the nuclear enterprise. Writing to Groves in August 1945, John Manley relayed the "gloomy" atmosphere felt by all at Los Alamos, as well as Chicago, about the future of control of atomic energy. Manley also lamented that there had been too little communication between those persons who had done most of the work and those who made policy. Manley warned that at this point, the Manhattan Project's scientists had little enthusiasm for government employment, and to them university offers

[130] Ibid., 425, 428-429, 413.

would be more appealing unless someone presented a more open policy concerning the future of nuclear energy.[131]

A few days after Manley wrote to Groves, several of the Chicago scientists began to organize an opposition to the May-Johnson bill. They found a senatorial supporter in freshman Senator Brien McMahon from Connecticut, who proposed another bill on atomic energy that called for a civilian controlled organization that focused on issues such as power production from atomic energy as well as its obvious military applications. Merely another new atomic energy bill did not satisfy McMahon. He also proposed before the end of 1945 another resolution in the Senate to form a new Joint Committee on Atomic Energy (JCAE), having himself appointed as its chairman. Together with McMahon, a number of other senators, the President, and the Chicago scientists managed to block the Army's attempt to rush the May-Johnson bill through Congress. Introducing it publicly in December 1945, McMahon's proposal sounded more reasonable to the civilian scientists: The commission would be led by five civilian appointees and it, rather than the military, would have control over production of fissionable materials and construction and stockpiling of nuclear weapons. Finally, McMahon's bill allowed for a more free flow of technical information than the May-Johnson bill had provided for, and the commission could finance private research in the physical, biological, and

[131] Letter from John Manley to Groves, August 30, 1945, LANL Archives.

social sciences.[132]

By the time President Truman signed the Atomic Energy Act (McMahon's bill) on August 1, 1946, it partially resembled the May-Johnson bill. Patterson had criticized McMahon's bill for having excluded military representation. Likewise, Groves argued that the Army and Navy should have a voice in regards to atomic weapons policy. Sympathetic to Patterson and Groves, Senator Arthur Vandenburg proposed an amendment to the bill that allowed for a Military Liaison Committee to the new commission. In addition, over the first part of 1946, the Senate Special Committee on Atomic Energy made several conservative amendments to the bill, although it added to the Commission's charter a scientific General Advisory Committee. The new Atomic Energy Commission would not become active until the beginning of 1947. Even then, it did not exactly resemble the now defunct MED system. Furthermore, the establishment of the new AEC system did not happen smoothly. As I will discuss more in Chapter Four, the AEC inherited the infrastructure of a system intended to be temporary for the sole purpose of a producing a few atomic devices for the war. The new AEC began operating without a goal-seeking agenda. Because of this, the Commission looked to Los Alamos and its GAC for recommendations to develop some form of postwar mission.[133]

[132] Hewlett and Anderson, The New World, 483.

New Life for Old Models: Establishing Los Alamos's Postwar Mission

Prior to the AEC's establishment, Bradbury had been hard at work setting down a technical agenda to present to the AEC by the time the Commission went into operation. Bradbury's Associate Director Darol Froman recalled that there "were very few new ideas [after the war] . . . that hadn't been thought up during the war." Thus, "improved" fission devices had been considered before the war's end, evident in some of the implosion problems T Division undertook in 1944 and 1945.[134]

The first several implosion calculations T Division performed on the punched card machines constituted "hollow pit" weapons, where a shell of active nuclear material made up the fissile core of the atomic device. Experiments with imploding hollow shells showed, however, that they imploded asymmetrically and thus scientists adopted Robert Christy's more conservative solid pit design for use in the wartime implosion weapon. T Division also ran calculations in March 1945 on an improved version of the Christy design -- the levitated core -- that in theory would achieve a higher energy when compressed and give a larger explosive yield. By this time the Christy design had been chosen for the implosion weapon, though, and the levitated core was shelved. By May, T Division seemed as though it were counting on weapons work to continue on some scale after the war. Bethe wrote in his monthly progress report, "Since many of the problems connected

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[133] Hewlett and Anderson, The New World, 499-502; Bradbury presentation, October 1, 1945.
[134] Arthur Norberg interview with Darol K. Froman, (Los Alamos Scientific Laboratory, 1980).

with the solid gadget have already been treated by the IBM machines a program was started to investigate other designs for future development."[135]

During the war, Los Alamos's Technical Board provided a forum for discussion of technical problems. It met a few times, was disbanded, then reorganized in 1945 to help direct the Laboratory's atomic stockpile research and development program. By the end of that year the Technical Board had agreed to plan for a test of improved fission devices -- smaller ones than those used during the war, in consideration of the Navy's carrier-based aircraft and guided missile projects.[136]

Before he had turned over the directorship of Los Alamos to Bradbury, Oppenheimer had expressed to the remaining staff his hope that a levitated implosion weapon would be completed in a year, and a new model of explosives design by the fall of 1946. The units produced, however, would have to match the rate of materials production during the coming year. In addition to such technical considerations, some social problems slowed the pace of the immediate postwar fission program.[137]

A serious problem that Bradbury faced after 1945 involved retaining personnel at Los Alamos while at the same time trying to recruit new staff. Because so many people left right after the end of the war, the weapons

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[135] Hoddeson, et al., Critical Assembly, 293; Robert K. Osborne, "Theoretical Design of Implosion Weapons Immediately Following the end of World War II," Defense Research Review, Vol. 1 (No 1), 1988, 1-31. [This Document is Secret-RD]; Hans Bethe, LAMS-260, "Progress Report for the Theoretical Physics Division for May 1945," Los Alamos Scientific Laboratory, June 20, 1945. [This Report is Secret-RD].
[136] Minutes of the Los Alamos Technical Board Meeting, July 18, 1946, LANL Archives, 001, [This Document is Secret-RD].
[137] Memorandum from Oppenheimer to All Division and Group Leaders, August 20, 1945.

program slowed to nearly a complete stop. T Division alone lost twenty-seven senior theoretical staff by 1946. Not surprisingly, in that year the remaining staff completed only one implosion problem on the IBM machines. As the laboratory struggled to establish a new mission and place in the postwar world, work on improving fission weapons limped along until the AEC's formal establishment and Bradbury could hire new staff, and until the military made more explicit requests for weapons stockpiling. I discuss the military's role in weapons development, along with the postwar fission program, in a later chapter.[138]

With no more urgency to produce workable weapons nor deadlines superimposed from above by the military, Los Alamos had more freedom to shift its focus towards more exploratory research. As World War II ended, many Los Alamos scientists expressed an interest in reviving more intensive work on the Super weapon. After July 1945, Bethe noted in his monthly report summarizing T Division's activities:

> [I]t seemed desirable that at least some members of the division take an interest in the Super-gadget. For this purpose a new group will be formed with Bethe as group leader. This group will work in closest collaboration with Teller's F-1, which for the past several months has cleared up several problems connected with the Super.[139]

Bradbury conceded, stating that the Laboratory would propose to perform experiments to answer the question "Is or is not a Super feasible?"

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[138] Bethe, "Comments on the History of the H-Bomb," 45; Mark, "Short Account," 3; Osborne, "Theoretical Design," 4-5; In addition to a personnel shortage, the 1946 Bikini "Crossroads" tests detracted from Los Alamos's work on improving the state of fission weapons, as these tests were essentially to determine the effects of atomic devices on Naval vessels.

[139] Hans A. Bethe, LAMS-273, "Progress Report of the Theoretical Physics Division for July 1945," Los Alamos Scientific Laboratory, no date, [This Report is Secret-RD].

Writing to the AEC in December 1946, Bradbury further elaborated that so far, theoretical calculations done to determine the feasibility of the Super did not decrease Los Alamos's expectations that such a weapon could be constructed, although an all-out effort at constructing and testing a Super remained beyond the capabilities of Los Alamos at that time.[140]

In Networks of Power, Hughes notes that a new system may emerge as a result of failure to solve a major problem in the old system. At the end of this process, he continues, the old and new systems exist at the same time in a kind of "dialectical tension," or "battle of the systems." This was not the case with the transfer of control of the American nuclear weapons complex from the MED to AEC. Instead, the MED essentially closed down after the end of the war. In 1947 the AEC's leaders had to pick up nuclear weapons research and development where it had been left off almost two years earlier. [141]

Hughes's argument that the military played the role of system builder of the Manhattan District is not wrong, although, as I argue earlier in this chapter, Groves did stand out as the MED's military leader. Likewise, after the war, Groves, more than any other military figure, strove to keep the system in place and force through legislation to establish a successor system.

The civilian-run AEC had a disorganized character when it began operating in 1947, because although its leaders intended to control many facilities such as Los Alamos, Oak Ridge, and others, the Commission's

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[140] Bradbury presentation, October 1, 1945; Letter from Bradbury to the Atomic Energy Commission, November 14, 1946, reprinted in LAMS-2532, (Vol. II), 215-224.
[141] Hughes, Networks, 79.

leaders had likewise to accept the many already established laboratories and facilities at face value. Thus, the Commission had to adapt to the remnants of the earlier MED, making for an awkward fit.

Perhaps, ironically, this lack of initial organization of the large AEC system combined with the characteristic of less pressure to build fission weapons gave Los Alamos's scientists some free time, for several months after the war, to explore the Super theory in more detail than they previously could. Moreover, the AEC professed no clear policy towards thermonuclear weapons development for several years after the war, while the GAC only referred to this in their meeting upon occasion and in vague terms. Therefore, Bradbury and Los Alamos made no promises to the AEC to develop an H-bomb in the postwar period, but on the other hand they did not completely stop work on fusion bomb theory throughout the remainder of the 1940s.

Several of Los Alamos's T Division members, and scientists from the Laboratory's other divisions such as Chemistry and Metallurgy, had managed to devoted several months' worth of work towards fusion weapons during the war. However, the Super remained the only hydrogen bomb theory as of 1945. Towards the Super, Teller and a few Los Alamos colleagues had devoted nearly all of their time from spring 1944 through 1945. The following chapter details the origins of thermonuclear weapons and early work done in connection with them.

Chapter Three

The Super and Postwar Computing: Machines Can Calculate, but Can Humans?

While the conclusion of World War II ended Los Alamos's mission to build an atomic weapon, it also allowed for shifts in the Laboratory's program and outlook for the future. Thermonuclear weapons made up a small but significant part of Los Alamos's postwar program, in which weapons scientists showed a vigorous and renewed interest almost immediately after the war's end. Given the intrigue the Berkeley conference participants had shown in Teller's Super proposal in 1942, a revived scientific focus on exploring thermonuclear weapons in 1945 was unsurprising.

Los Alamos's staff and affiliates' renewed interest in fusion weapons remained almost exclusively theirs. While the atomic project had been secret, fewer individuals even within the social network of the MED, and later, AEC systems, knew of the Super.

The secret nature of nuclear weapons work sets it apart from other systems that Hughes examined. Whereas Hughes argues that system builders in the 1880s, such as Edison, identified critical problems fairly readily in part because of inadequacies in patterns formed by the systems components and networks could easily be spotted, scientists could not identify critical problems in the AEC system so straightforwardly. According to Hughes, system builders of the 1880s could observe publications, file patents, and become very familiar with their competitors' inventions. System builders and scientists working within the AEC system could not do this, because so few individuals were aware of the Super theory. Therefore, critical problems were not open for widespread discussion, remaining hard to recognize, least of all solve.[142]

Hughes indicates that problems will not be seen by engineers and inventors unless they view the technology as a goal-seeking system. If the critical problems frustrate the system's growth, then the system builders try to alleviate the problems. In 1945 Los Alamos, however, no goal existed anymore as did during the war. If anything, Bradbury's biggest goal aimed to keep the Laboratory operating during the transition period from the MED to AEC. [143]

Los Alamos's scientists -- Teller, von Neumann, and others -- did truly recognize problems facing the thermonuclear project even before the end of the war, but did not regard them so terribly severe or critical that they thwarted the entire fusion bomb program; no defined goal to develop an H-bomb was set at this time, and not even a goal-seeking system within which Teller and others saw these problems. Furthermore, scientists gradually recognized the severity of the critical problems to the Super and other thermonuclear weapons theories over time. Computing was one of the earliest-appearing problems, which I examine in this chapter.

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[142] Hughes, Networks, 80.
[143] Ibid., 80.

Fermi and the Fusion Weapon: Origins of the Super

The idea of a thermonuclear weapon, where a fission chain reaction could be used to cause light elements such as hydrogen to fuse, was realized months before the Berkeley conference. According to Rhodes, in May 1941 University of Tokyo physicist Tokutaro Hagiwara publicly proclaimed that U^{235} might be used as a fission initiator for some quantity of hydrogen that in theory could produce a very large nuclear explosion.

In the same year that Hagiwara delivered his lecture on "Super-explosive U^{235}," a similar idea occurred to Enrico Fermi after lunching with Edward Teller in New York. While walking back to their office building at Columbia University, Fermi pondered aloud about whether or not an atomic weapon, already in prospect, might be used as a trigger for a deuterium (D), or H^2 weapon. In principle, a bomb that fused hydrogen to helium was far more economical and would produce a much greater explosion than a fission device. Deuterium, distilled from sea water, could be produced cheaply. In addition, theoretically a cubic meter of D ignited by an atomic device would produce an explosion on the order of megatons; a fission device itself would only yield an explosion in the kiloton range. Inspired by Fermi's suggestion, Teller took up the cause of exploring a fusion weapon.[144]

Not alone in his quest for long, others intrigued with a thermonuclear weapon soon joined Teller. Rhodes claims that when Teller first considered Fermi's suggestion, the Hungarian Physicist performed hand calculations and

[144]Rhodes, Dark Sun, 247-248; Rhodes, Atomic Bomb, 374-375.

concluding that deuterium could not be ignited by a fission bomb. Before the summer conference in Berkeley the next year, Teller changed his mind. After arriving at the Metallurgical Laboratory in Chicago early in 1942 and planning to work on the fission pile, Teller met Emil Konopinski. For the first few days in Chicago, both physicists had no formal assignments, thus Teller told Konopinski about his earlier calculations on igniting deuterium, and asked if the Indiana theoretician would help perform additional computations to further disprove Fermi's theory in time for the upcoming Berkeley conference.[145]

Teller claims that he and Konopinski initially set out to prove that igniting deuterium with an atomic bomb was a waste of time. The result of their calculations, however, led to the opposite conclusion:

> . . . [T]he more we worked on our report, the more obvious it became that the roadblocks I had erected for Fermi's idea were not so high after all. We hurdled them one by one, and concluded that heavy hydrogen actually could be ignited by an atomic bomb to produce an explosion of tremendous magnitude. By the time we were on our way to California, about the first of July, we even thought we knew precisely how to do it.[146]

According to several of those scientists invited to attend to Berkeley "luminaries" conference, Teller's Super idea dominated the discussions. Because Serber, Frankel and Nelson appeared to have the fission problems already worked out, the thermonuclear device became an easy source of distraction for the Berkeley conferees. Teller introduced the Super theory

[145] Teller, Legacy, 37-38; Edward Teller lecture at Los Alamos National Laboratory, "Origins of Thermonuclear Explosives: Super to Mike," March 31, 1993, Vidoecassette, LANL , [This document is Secret-RD].

only about 2 days after the conference began, by presenting the calculations he and Konopinski had completed just prior to the beginning of the meeting.[147]

"Edward Teller is a disaster to any organization . . . he always started bringing in all kinds of wild ideas," Serber recalled of the Berkeley conference. "He'd come in every morning with an agenda, with some bright idea, and then overnight Bethe would prove it was cockeyed." Still, at first the notion of igniting a mass of deuterium seemed simple -- so easy that by July Oppenheimer relayed news of the Super theory back to Compton and the S-1 Committee, who in turn relayed the idea to Bush.[148]

Bush, and subsequently Conant, took Oppenheimer's news seriously enough that they paraphrased the idea in a memo to Secretary of War Stimson in September. Referring to the thermonuclear idea as a "super-super" bomb, they relayed:

> Some of our theoretical physicists believe that it is extremely probable that the energy generated by the fission of the nuclei of '25' and '49' could under certain circumstances produce such a high temperature as to initiate a reaction which has never taken place on this earth, but is closely analogous to the source of energy of the sun . . . . A super bomb using heavy hydrogen (in the form of heavy water) and detonated by an atomic bomb using '25' or '49' would be of a different order of magnitude in its destructive power from an atomic bomb itself. We may therefore designate it as a super-super bomb.[149]

Some of the Berkeley conference participants did not feel as certain about the idea. Although as curious about the Super as any of the other

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[146] Teller, Legacy, 38.
[147] Teller, Legacy, 38-39; Serber, Primer, xxx; Author interview with Serber, November 26, 1996.
[148] Serber, Primer, xxx.
[149] Memorandum to the Secretary of War from Vannevar Bush and James B. Conant, "Supplementary memorandum giving further details concerning military potentialities of

members of the summer conference, Bethe displayed more skepticism than Teller or Oppenheimer about the Super's viability. Claiming that he "didn't believe in it from the first minute," Bethe reviewed Teller's initial calculations and found mistakes. Teller had ignored the problem of the inverse Compton effect, a cooling process where radiation would drain off energy at a rate that increased rapidly with temperature. For deuterium alone to ignite required a temperature of over 400 million degrees. The D-D reactions would proceed too slowly and fusion would not occur before the fission trigger destroyed the entire device. To try and salvage the idea, Konopinski suggested that tritium (H^3) be added to the deuterium to lower the ignition temperature. In addition, a reaction of tritium and deuterium would release about five times more energy than deuterium alone. However, tritium is extremely rare naturally, and too expensive and difficult to produce artificially.[150]

Nevertheless, the Berkeley group discussed Teller's proposal at length. Serber remembered that the rest of the conference was fun, conducted in a proposal and counter-proposal manner, with the whole group enjoying bantering ideas around. The Super remained a part of the discussion throughout the duration of the conference; Serber believed that the Super idea never was "laid to rest," because of Oppenheimer's informing the S-1

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atomic bombs and the need for international exchange of information," September 30, 1944, in JCAE declassified General Subject Files, Box 60, NARA.

[150] Teller, Legacy, 38; Bethe quoted in Rhodes, Atomic Bomb, 418-419; David Hawkins, Project Y: The Los Alamos Story, Part 1 - Toward Trinity, (San Francisco: Tomash Publishers, 1983), 86-87; Serber, Primer, xxx-xxxi.

committee about the possibility of a thermonuclear weapon. Serber thought at the time that Teller calculations about the Super's feasibility were over-optimistic. Although by the end of the conference the group finally settled on recommending the development of a fission device for the war effort, Teller remained hooked on the idea of developing a hydrogen weapon.

No Super for Wartime Los Alamos

Groves constructed only one laboratory to serve as the MED's weapons-design facility, and when Los Alamos opened in 1943 Oppenheimer expressed interest in supporting some thermonuclear weapons research, even if Serber and Bethe doubted about Teller's ideas. Others who came to Los Alamos showed interest in the Super, too. Teller recounted that in 1943, since Pu^{239} gun-weapons looked like "sitting ducks," Oppenheimer cast around for something really interesting beyond the fission project that would challenge the laboratory. The Super would provide that challenge.

In April 1943 several conferences were held at Los Alamos to teach new staff members about the purpose of the project and state of theory about building a fission device. During the meetings Teller led his own discussion about the Super, explaining to his colleagues that the effect of chain-reacting gadgets using expensive materials like U^{235}, Pu^{239}, and even U^{233} could be amplified by arranging them to initiate thermonuclear reactions in less expensive deuterium. A fission "detonator" then, could be used to set off a "charge" of inexpensive material such as deuterium. Teller knew, though, that the fission "gadget" represented a prerequisite to any "super-gadget." In addition, the latter theory needed theoretical analysis, to obtain an understanding of, for example, how energy would be transferred between the detonator and the charge, and how energy could be lost to radiation, shockwave, and conduction through the device's cold walls.[151]

Even if the Super could only be developed after an atomic weapon, from the time of the Berkeley conference Oppenheimer planned for a wartime research involving theoretical studies of the more powerful device. Consequently, and inspired by the Berkeley conference discussions, Teller returned to Chicago after the meeting ended and continued work on his thermonuclear calculations. John Manley, who also had been at Chicago in 1942, led a group that took measurements of D-D cross sections. Oppenheimer, Bethe, and Lawrence requested that a study be conducted on the Harvard cyclotron of the cross sections of deuterium and tritium (D-T) with lithium isotopes. Bethe wanted to see further studies, and with Compton's and Oppenheimer's approval, L. D. P. King and Raemer Schreiber began work at Purdue University on D-T cross sections. Later, Marshall Holloway and Charles Baker continued this work.[152]

Berkeley chemist Edwin McMillan, a co-discoverer of neptunium, recalled that Arthur Compton scheduled a meeting the week of September 19, 1942 at Chicago to further discuss the Super, as word from the recent Berkeley

[151] Memorandum for the File from John Walker, January 13, 1953, JCAE General Subject Files, Declassified Box 58; LA-4, "First Los Alamos Conference: Post-Conference Discussions," week of April 27, 1943, 12, 20, LANL Report Library, [This Document is Secret-RD]; A cross section is a measurement (in barns) of the probability of a nuclear reaction occuring. A barn is a measure of the area (10^{-24} centimeters^2) of the nucleus, used to measure neutron capture cross sections.

conference traveled East. The Chicago meeting differed from the Berkeley conference in that several chemists -- as opposed to theoretical physicists -- gathered more than likely to propose some kind experimental wartime program for producing nuclear materials for a Super. Chemist Earl A. Long, a former student of Herrick Johnston, also attended the Chicago meeting.[153]

Even before Oppenheimer, Serber, Teller, and others moved to Los Alamos, McMillan and Joseph Kennedy visited their colleague Johnston at Ohio State University, who had set up a project in 1942 for liquefying hydrogen. Johnston already had a contract with the War Department to produce liquid hydrogen; chemist Harold Urey -- a member of the OSRD Executive Committee -- had initiated this contract. While Johnston's plant began producing liquid hydrogen in February 1943, Oppenheimer apparently wanted additional facilities for this purpose and thus sanctioned construction of a similar hydrogen liquefier in New Mexico to produce liquid deuterium as part of the Super research program. Los Alamos completed construction of a Joule-Thompson liquefier by early 1944, a structure based partly on Johnston's design but more nearly a copy of a design created by chemist W. F. Giauque at Berkeley .[154]

While Johnston and his team continued under contract with the MED throughout the war to measure properties of liquid deuterium, the Los

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[152] Hoddeson, et al., Critical Assembly, 47; LAMD-154, Manhattan District History, Book VIII, Vol. 2, Project "Y" History - Technical, April 29, 1947, 22, [This report is Secret-RD].
[153] Memorandum to the File from John S. Walker, "Project Whitney," November 10, 1952, JCAE declassified General Correspondence Files, Box 60, NARA.

Alamos group, headed by Long, produced a small amount of liquid deuterium in the winter of 1944. At this time because the cryogenic group was the only one at Los Alamos devoted mainly to Super work, according to Teller its morale remained low. Even Teller, placed in T Division, worked mostly on fission-related calculations up until the spring of 1944. Partly because of Segre's discovery of the spontaneous fissioning of plutonium and the Laboratory's consequent reorientation towards an implosion weapon, the Laboratory's Governing Board began to de-emphasize Super research in the first half of 1944, as directed more work at the new fission implosion device. Teller, von Neumann, and others spending their spare time carrying out Super calculations, though, made several discoveries more devastating to the Super theory.[155]

Teller pushed Oppenheimer and Bethe for more intensive work on the Super throughout the fall of 1943, arguing that they revised cross section measurements for D-T and D-D upwards from those done earlier, and thus the Super would be ignitable at lower temperatures than what the Berkeley conferees had thought. Teller's thoughts involved more than the optimistic cross section measurements: he feared that the Germans had plans to use deuterium to build their own thermonuclear bomb, and Los Alamos needed to make a technical response to this.[156]

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[154] LAMD-154, 31; LAMD-160, Manhattan District History, Book VIII, Los Alamos Project (Y), Vol. 3, Auxiliary Activities, Chapter 3, Activities of the Ohio State Cryogenics Laboratory, July 15, 1946, passim, [This report is Secret-RD].
[155] LAMD-154, 31-32; LAMD-27, "Minutes of the Meeting of the Governing Board," September 9, 1943, 2, [This report is Secret-RD].
[156] LAMD-27, 2.

The Governing Board resisted Teller's urging, though, by recommending that not more than one full-time person be allowed to work on the Super theory, and suggested that either Teller, Konopinski, or Metropolis take up the task. Oppenheimer further directed that Bethe have no responsibility at all for thermonuclear research, since T Division's looming work on implosion calculations would be overwhelming enough.[157]

Teller and his group within T Division, which included Konopinski, Metropolis, and Jane Roberg, spent probably about half their time working on the Super theory during much of the winter of 1943-44, although they were supposed to focus on a mathematical description of implosion. With implosion's rising priority, Teller expanded the group by bringing in Mathematician Stanislaw Ulam, Geoffrey Chew, and Harold and Mary Argo. Still, the entire group worked on various theoretical problems related to the Super.[158]

By February Teller's group ran into trouble with the D-D Super theory. Teller, along with von Neumann and Roberg, proposed that perhaps a thermonuclear reaction could be started by placing deuterium or a mixture of deuterium and tritium inside of a fission bomb. However, as they pursued this idea, a major obstacle to the Super appeared in the form of energy dissipation. Teller, von Neumann, and Roberg reported to Bethe that the incredible speed of all the reactions inside the deuterium would make it difficult to deliver the energy needed to reach the ignition point in a short

[157] LAMD-27, 2.

time. Furthermore, the Compton effect would cause cooling of the hydrogen electrons by collisions with photons coming from the fission initiator, making it hopeless to try to start a thermonuclear reaction with pure deuterium in the initiator. On the other hand, Teller's group remained optimistic that a mixture of deuterium and tritium inside the initiator would work.[159]

Teller, Roberg, and von Neumann's discovery of just how greatly the inverse Compton effect would drain energy away from the Super provided a fateful blow to the wartime thermonuclear research program. When the Governing Board met on February 24, 1944, Teller described the newfound problems with the Super to Oppenheimer and the rest of the board members. Teller explained that in his original Super calculations he had overlooked inverse Compton cooling. Moreover, the entire Super theory needed much more detailed quantitative investigations, since many other phenomena about this idea remained not well understood. For example, in addition to the problem of Compton cooling, no one understood how the walls of the deuterium-filled vessel would cool the device.[160]

Teller's based his proposed solution to this problem on Konopinski's suggestion during the Berkeley conference; the addition of tritium to the deuterium would lower the ignition temperature of the Super, and a DT mixture could be used to create a "booster" that would in turn ignite the

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[158] Hoddeson, et al., Critical Assembly, 157, 204.
[159] LAMS-47, "Progress Report of the Theoretical Physics Division for the Month ending January 31, 1944," January 31, 1994, 14, [This Report is Secret-RD].

larger mass of pure deuterium. Although Teller felt certain that this scheme would work, the Governing Board did not want to support it because of the complexity of the theoretical problems surrounding the Super and because it would require tritium.[161]

The Board did not entirely dismiss the Super, however. Richard Tolman, who attended this meeting as General Groves's advisor, mentioned that although the Super might not be needed during the war, Los Alamos had an obligation to continue work on this for the long-term. The rest of the group agreed, and implicitly allowed for theoretical work on the Super to continue as long as it did not interfere with the fission program. Practically, though, work on Super-related problems stopped by the spring of 1944. Earl Long's group managed to test its hydrogen liquefier in April 1944, but the Governing Board halted all cryogenic work by September and dispersed the group to work on other problems.[162]

The theoretical Super research did begin to interfere with the fission program by the spring of 1944, because Teller increasingly devoted more time to this than to the implosion problems he and his group in T Division were supposed to work on. When Bethe reorganized T Division in March 1944 to focus more on the implosion weapon, he placed Teller in charge of a large group that included Konopinski, Metropolis, Roberg, Ulam, von Neumann, John Calkin, Chew, Mary and Harold Argo, and Robert Christy. Bethe

[160] LAMD-27, "Minutes of the Meeting of the Governing Board," February 24, 1944, 4, [This Report is Secret-RD].
[161] The tritium problem I discuss in more detail in Chapter Four.

charged them with doing calculations to produce a mathematical description of the hydrodynamics of implosion.[163]

Towards the implosion device, the group calculated the time of assembly for large amounts of high explosives. Along with Metropolis and Feynman, Teller determined the equation of state for highly compressed uranium and plutonium expected to result from a successful implosion. Teller declined, though, to take charge of the group that would to perform very detailed calculations of an implosion weapon to devote more time to the fusion weapon.[164]

Per Teller's request, in June 1944, Oppenheimer separated Teller and part of his team from the rest of T Division. Fermi arrived at Los Alamos in the late summer of 1944 from Chicago and remained for over a year. In September, Fermi set up a new division -- F Division -- to investigate lines of development other than fission devices. Teller and his group became F-1, responsible for all theoretical work on the Super. Group F-3, headed by Egon Bretscher, focused more on experimental studies with fusion fuels, and new measurements of the cross sections of the T-D and D-D reactions. By the following spring Teller reported that his group had focused on trying to gain a better understanding of the complicated processes such as the inverse

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[162] LAMD-27, 5; LAMD-154, 27, 32.
[163] LAMS-74, "Progress Report for the Theoretical Division for the Month Ending March 31, 1944," March 31, 1944, 2, [This Report is Secret-RD].
[164] Hoddeson, et al., Critical Assembly, 157; Hans Bethe, "Introduction," in Computers and Their Role in the Physical Sciences, op. cit., 3; Hans Bethe, "Comments on the History of the H-bomb," op. cit., 43-53.

Compton effect, and energy loss through thermal conduction out of the walls enclosing the container of deuterium.[165]

The more that Teller and his colleagues studied the Super theory, the more complicated it became as the group realized that numerous hydrodynamic and thermodynamic effects needing accounting for to have any understanding of how the device worked. Some of the alternative names Los Alamos's scientists gave the Super derived from this phenomena. For example, in principle this weapon had unlimited explosive yield and could "run away" depending on how much deuterium fuel it contained. Scientists accordingly called it the "runaway" thermonuclear device.[166]

Along with trying to merely understand how a Super might work, F-1 concentrated on how to ignite this weapon, as well as attempting to gain an understanding of an ideal ignition temperature of the device. This problem was formidable because calculating the ideal ignition temperature of the Super involved understanding the secondary reactions following the primary reactions in D-D, and the rate at which energy of the first reaction products dissipated to electrons and deuterons. Konopinski, Chew, Stanley Frankel, and Harold and Mary Argo tried working through these problems, but reported that they still did not know enough about the purely nuclear interactions between the heavy particles.[167]

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[165] Hawkins, Project Y, 188.
[166] F.C. Alexander, Jr., Early Thermonuclear Weapons Development: The Origins of the Hydrogen Bomb, SC-WD-68-334, Sandia Laboratories, May 1969, 10, [This Report is Secret-RD].
[167] LAMS-228, "F Division Progress Report for March, 1945," April 12, 1945, 3-8, [This Report is Secret-RD]; LAMS-238, "F-Division Progress Report for April, 1945," May 7, 1945, 8, [This

Teller and his group became aware by this time that the calculations for the Super were more complicated than even those of the fission implosion device. Teller realized before anyone else that advanced computing technology would be necessary to perform a complete analysis of the device. Hydrogen bomb calculations differ considerably from atomic-bomb calculations because the nuclear reaction products involve charged particles in addition to neutrons. Ignition of the Super required heating the material to a critical temperature rather than assembly of a critical mass.[168]

In the spring and summer of 1945, then, Teller realized that a critical problem stood in the way of simulating a Super -- how to calculate it. Believing that the complexity of the problems related to the Super exceeded the capabilities of hand computers, Teller followed Dana Mitchell's example of attempting to employ the only other calculating technology available at this time -- punched card machines.

Although the concept of using punched cards for scientific calculations was still very new, these machines represented in 1945 the most obvious and rapid technical solution to problems in the emerging field of nuclear weapons research and development. Eckert's laboratory had inspired this approach of using business machines for scientific calculations at Los Alamos, and Teller turned to Eckert for help with the Super calculations.

Report is Secret-RD]; LAMS-255, "F Division Progress Report for May, 1945," June 7, 1945, 2-3, [This Report is Secret-RD].

[168] Metropolis and Nelson, "Early Computing," 355.

Although Los Alamos was a secret laboratory, scientific networks between its staff and major research universities abounded during the war. Even though by 1945 Teller had been in Los Alamos for about two years, he still had close scientific ties at Columbia University, including mathematical physicist Maria Goeppert Mayer. In spring 1945 Mayer actually taught at Sarah Lawrence College because Columbia University refused to pay her salary. Nevertheless, Mayer was famous for her work on opacity studies, thus Teller sought her help with the Super calculations.[169]

The Los Alamos card punches may have been more convenient for an analysis of the Super than Eckert's machines, but the implosion problems completely occupied T Division's 601's. Thus, Teller asked Oppenheimer's permission to discuss the Super problems with Mayer, who would watch over Super calculations that would be placed on the punched cards at Eckert's IBM laboratory at Columbia. Teller justified the need for the machines at Eckert's laboratory:

> It is clear that the calculations about the Super will be of so involved a nature that the help of the IBM outfit will be needed if results are to be obtained in a reasonably short time . . . . It is my hope that by the end of May or beginning of June we should be in a position to have the calculation in New York started [and] Mayer could advise Eckert's group.[170]

Teller even volunteered Metropolis and Frankel to help run the calculations on the card punches in Eckert's laboratory. Teller wanted to start

[169] Opacity is a measurement of a substance's resistance to light, x-rays, and neutrons. If a nuclear material is very opaque, it is impermeable to radiation.
[170] Memorandum from Edward Teller to J. Robert Oppenheimer, April 9, 1945, LANL Archives, 201, Drawer 22. [This Document is Secret-RD].

machine calculations on the Super because he claimed that work on the physics of the Super was near completion or almost settled. His group's next immediate task would be to specify one or more designs so that calculations on them could proceed. By June, Stanley and Mary Frankel, Metropolis, who had transferred to Teller's group, and Turkevich began to develop a one-dimensional method, tailored for IBM calculating punches, to treat the initiation of detonation in deuterium.[171]

The group continued preparing the IBM calculations through the summer, with von Neumann joining temporarily in July to offer his assistance. However, the group could not mechanize the Super ignition calculations as soon as it would have liked. In October, Metropolis and Stanley Frankel still struggled to calculate the critical temperature distributions in various D-T mixtures, while the rest of the group tried to see if a D-T mixture could be detonated in direct contact with the gadget. Moreover, Teller and his colleagues did not even have a clear design for a Super specified, although in September 1945 they proposed a crude model for the weapon. Teller, Bethe, Oppenheimer, and Konopinski later filed a patent for this model.[172]

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[171] Memorandum from Teller to Oppenheimer, op. cit; LAMS-265, "F-Division Progress Report for June, 1945," July 6, 1945, 2-3, [This Report is Secret-RD].
[172] LAMS-272, "F Division Progress Report for July, 1945," August 9, 1945, 6, [This report is Secret-RD]; LAMS-304, "F-Division Progress Report for October, 1945," 2; [This Report is Secret-RD]; LAMS-298, "F Division Progress Report for September, 1945," October 18, 1945, 2,

Enter von Neumann

John von Neumann had long been famous as a mathematician when he became involved with Los Alamos. According to historian William Aspray, von Neumann became interested in electronic, digital, stored-program computers at a critical time, when they were being conceived to replace older calculating technology.[173]

Von Neumann introduced Los Alamos to the new computers of the postwar era. A chance encounter would hasten Los Alamos's exposure to this sort of technology. Herman Goldstine recalled:

> Sometime in the summer of 1944 after I was out of the hospital I was waiting for a train to Philadelphia on the railroad platform in Aberdeen when along came von Neumann. Prior to this time I had never met this great mathematician, but I knew much about him of course and had heard him lecture on several occasions. It was therefore with considerable temerity that I approached this world-famous figure, introduced myself, and started talking . . . . The conversation soon turned to my work. When it became clear to von Neumann that I was concerned with the development of an electronic computer capable of 333 multiplications per second, the whole atmosphere of our conversation changed from one of relaxed good humor to one more like the oral examination for the doctor's degree in mathematics.[174]

Goldstine remembered that soon after this meeting, he and von Neumann went to Philadelphia together so that von Neumann could see the ENIAC (Electronic Numeric Integrator and Computer) -- the project Goldstine co-directed -- under construction at the University of Pennsylvania's Moore

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[This Report is Secret-RD]; S-680-X in File 699096, Patents, Box 5, Folder 8, LANL Archives, [This Document is Secret-RD].

[173] William Aspray, John von Neumann and the Origins of Modern Computing, (Cambridge, MA: MIT Press, 1990), xv.

[174] Herman H. Goldstine, The Computer from Pascal to von Neumann, op. cit., 177, 182.

School of Engineering. First proposed by Moore School Engineers John Mauchly and J. Presper Eckert, Army Ordnance sponsored the machine. The Army had wanted a fast computer to calculate firing tables for new artillery. However, ENIAC was not finished by the end of the war, and had not been tested.[175]

With the war's conclusion, the ballistics tables that ENIAC was supposed to calculate became a lower priority than they would have been in wartime. Still, as computing historian Paul Edwards has suggested, ENIAC was a military machine, and von Neumann quickly found an appropriate first use for it.[176]

Metropolis and Nelson claim that von Neumann suggested using the ENIAC for the Super calculation "early" in 1945. Von Neumann had informed them at least of the existence of the ENIAC in January of that year, probably uncertain of exactly when ENIAC would be available to Los Alamos. Thus, Teller's F-1 group continued to prepare computations for the IBM machines at Columbia through the summer and fall of 1945. By the time engineers and technicians had almost completed the ENIAC at the end of the year, von Neumann had successfully arranged for Los Alamos to use the machine, and the Frankels and Metropolis began to prepare a calculation for the ENIAC to determine the conditions for successful propagation of a Super.

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175 Ibid.

176 Paul N. Edwards, The Closed World: Computers and the Politics of Discourse in Cold War America, (Cambridge, MA: MIT Press, 1986), 51.

Metropolis and Stanley Frankel had already visited the ENIAC in the previous summer to learn about it from Herman and Adele Goldstine.[177]

The "Los Alamos Problem" was the first full-length program ever run on the ENIAC; it attempted to predict whether or not the Super would ignite. The program ran for about 6 weeks from December 1945 to January 1946, although the ENIAC remained not quite finished. Anthony Turkevich recalled that Metropolis asked him to assist running the calculation, because putting the Super calculations on the ENIAC was laborious; the computer could not store any programs or retain more than twenty ten-digit numbers in its memory. Programming the large Super problems involved thousands of steps, each one entered into the machine through its plugboards and switches, while the data for the Super problem used one million punched cards.[178]

A giant itself, the ENIAC filled an entire room at the Moore School. The machine contained 18,000 vacuum tubes, 1500 relays, 70,000 resistors, and 10,000 capacitors. Although reliable when at their final operating temperature, the vacuum tubes tended to burn out when the machine was turned on and the tubes warmed up, thus the Moore School tried not to turn the ENIAC off unless absolutely necessary. However, when operating, the

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[177] Nicholas Metropolis, LA-UR-87-1353, "The Los Alamos Experience, 1943-1954," 5; Metropolis and Nelson, 352; E. Teller, E. Konopinski, and E. Fermi, LAMS-290, Super-Gadget Program, February 16, 1950; This Report was originally issued as a memo from the authors to Norris Bradbury, dated October 2, 1945, [This Report is Secret-RD]; Goldstine, The Computer, 214.

[178] Nicholas Metropolis, "The MANIAC," in A History of Computing in the Twentieth Century, eds. N. Metropolis, J. Howlett, and Gian-Carlo Rota, (New York: Academic Press, 1980),

tubes produced so much heat that the computer employed an internal forced-air cooling system to prevent it from catching fire.[179]

Igniting fires of another sort concerned the "Los Alamos Problem," described by Frankel, Metropolis, and Turkevich in a classified report. The entire calculation constituted a set of three partial differential equations, meant to predict the behavior of deuterium-tritium systems corresponding to various initial temperature distributions and tritium concentrations. Collectively, the calculations attempted to predict whether or not a self-sustaining nuclear reaction would occur and ignite pure deuterium.[180]

ENIAC was a powerful machine by 1945 standards and exceeded Los Alamos's computing capabilities. Nevertheless, ENIAC had only about 1000 words of memory, and Metropolis, Frankel, and Turkevich could only run a one-dimensional set of calculations. Even so, the problems used about 95 percent of ENIAC's control capacity. Because of the complexity of the Super problems, several effects were left out such as energy loss by the inverse Compton effect, the decrease in bremsstrahlung loss due to the presence of radiation, and the heating of cold deuterium by radiation from the hot deuterium. Russian physicist George Gamow once caricatured the flow of energy in the Super problem in a cartoon, to demonstrate the difficulties involved in understanding the device (see Figure 1).[181]

457-464; Turkevich personal communication with author, September 24, 1996, Los Alamos, NM; Edwards, The Closed World, 50.

[179] Williams, A History of Computing Technology, 285; Edwards, The Closed World, 50.

[180] S. Frankel, N. Metropolis, A. Turkevich, LA-525, Ignition of Deuterium-Tritium Mixtures: Numerical Calculations Using the ENIAC, March 2, 1950, 2, LASL, [This Report is Secret-RD].

[181] Metropolis, "The MANIAC," 74-75; LA-525, 24.

Metropolis, Frankel, and Turkevich described the philosophy behind the calculations as an "exercise" and means of testing the ENIAC. Although indeed the problems attempted to predict the proper temperature needed to start a thermonuclear reaction in deuterium, and to determine the amount of tritium necessary for starting such a reaction, the group concluded that much of the time spent on these early calculations should be "written off to education, and even to development of the use of the machine."[182]

The Los Alamos problem was the most complicated calculation of its time, but it did not truly answer the question of whether or not a Super could be ignited, much less propagate. Ulam gave his opinion on this calculation, stating:

> The magnitude of the problem was staggering. In addition to all the problems of fission . . . neutronics, thermodynamics, hydrodynamics, new ones appeared vitally in the thermonuclear problems: The behavior of more materials, the question of time scales and interplay of all the geometrical and physical factors became even more crucial for the success of the plan. It was apparent that numerical work had to be undertaken on a vast scale.[183]

Teller took the ENIAC calculations more seriously than did some of his colleagues, and convinced Oppenheimer to approve of a conference to review the results of this work. Even though work on fission weapons, much less the Super theory, had slowed dramatically, Teller and Oppenheimer agreed that the ENIAC's results, as well as F Division's work on

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[182] LA-525, 24.

[183] Metropolis, "The MANIAC," 74-75; Ulam quoted in Aspray, John von Neumann and the Origins of Modern Computing, op. cit., 47.

the Super, should be recorded in case anyone took up work on thermonuclear weapons in the future.[184]

Consequently, Teller hosted a secret conference in Los Alamos in April 1946, to review the ENIAC's results and to discuss the feasibility of constructing a Super. Of the thirty-one attendees at the conference, the majority had already left the laboratory for academic positions, and returned to Los Alamos only for this meeting. For three days, Teller, Bradbury, Metropolis, Frankel, Ulam, physicist J. Carson Mark, Soviet spy Klaus Fuchs, von Neumann and others, discussed a simple theory and a tentative design of the Super based on F-1's schematic diagram of September 1945. The group most likely chose this model not only because it was the only one existing at the time, but also as Edith Truslow and Ralph Carlisle Smith claim, for it's amenability to theoretical treatment as opposed to its engineering practicality.[185]

Although the Super conference purported to review the ENIAC calculation results, which Metropolis, Frankel, and Nelson did not initially describe as promising, the Super conference report came across as optimistic, indicating -- based on a minority of the individual ENIAC calculations -- that the Super would ignite with less than 400 grams of tritium present in its booster and primer parts. The Super conference concluded:

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[184] Edward Teller lecture at Los Alamos, March 31, 1993., op. cit.

[185]Stanley Frankel, LA-551, Prima Facie Proof of the Super, April 15, 1946, LASL, [This Report is Secret-RD]; LAMS-298, 5; Edith C. Truslow and Ralph Carlisle Smith, Project Y: The Los Alamos Story, Part II - Beyond Trinity, (Los Angeles: Tomash, 1983), 308; This piece was first published as Los Alamos Scientific Laboratory report LAMS-2532 (Vol. II).

> It is likely that a super-bomb can be constructed and will work. Definite proof of this can hardly ever be expected and a final decision can be made only by a test of the completely assembled super-bomb . . . . A detailed calculation would have to be undertaken to learn to what extent the thermo-nuclear explosion will propagate and how to obtain the best geometry.[186]

To Teller, one of the primary author of the report's conclusion, the critical problem of computing became more obvious after employing ENIAC; he acknowledged the machine's limitations, and recommended that attention be paid to developments in high-speed electronic calculators, and that thermonuclear calculations so far indicated that the complexity of the problems required at least an instrument like the ENIAC.[187]

Metropolis had brought Turkevich to Los Alamos in spring 1945 to help F Division. Although he considered himself "hired help" at the time, Turkevich remembered the Super conference, reminiscing that Philip Morrison took the occasion to celebrate the beginning of spring by throwing a bunch of lilies at the blackboard in the room where the conference was held. Turkevich described the tone of the conference as not terribly optimistic, mostly because of the ENIAC's unpromising results:[188]

Serber also attended the 1946 meeting:

> My main memory of it was that at the end Edward wrote up a report at the conclusion of the conference, and I found the report really incredible: The conclusion was that it was almost certain that it would work. I didn't want to discourage Edward from pursuing what he wanted to do, but I thought he should tell what was more close to the truth in the report, so we went over it and modified some of the more

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[186] E. Bretscher, S.P. Frankel, D.K. Froman, N. Metropolis, P. Morrison, L.W. Nordheim, E. Teller, A. Turkevich, and J. von Neumann, LA-575, Report on the Conference on the Super, February 16, 1950, LASL, 9-10, 31-32, 44-45, [This Report is Secret-RD].
[187] Ibid. 45.
[188] Turkevich personal communication with the author, September 24, 1996.

> extreme statements. I went back to Berkeley and a couple of months later and [when the report came, none of the changes were made] that we had agreed on.[189]

Several other attendees at the Super conference signed their names to the report, including Frankel, Turkevich, von Neumann, Froman, Metropolis, Morrison, Lothar Nordheim, and Bretscher. If they wrote the final report, Serber recalled, then Teller "certainly wrote the conclusion."[190]

Postwar Exodus, Other Thermonuclear Creatures

The Super conference marked the protraction of research not only on thermonuclear theory but on atomic weapons as well. Their wartime mission accomplished, most scientists wished to return to former or begin new academic positions. Teller departed for the University of Chicago in 1946 along with colleagues Fermi, Frankel and Metropolis. Bradbury, however, strove to rebuild Los Alamos and continue research on nuclear weapons and asked Teller to stay and become T-Division leader. Teller had wanted this position during the war, but Oppenheimer had awarded the job to Bethe. Now, bargaining with Bradbury, Teller claimed that he wanted to remain at Los Alamos, but only under the condition that the new director set up a vigorous thermonuclear research program, or at least step up the pace of fission weapons research and development by conducting a dozen Trinity-type tests per year.[191]

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[189] Author interview with Serber, November 24, 1996.
[190] Ibid.
[191] Hewlett and Duncan, Atomic Shield, 32; Teller, Legacy, 22.

Bradbury could not meet Teller's demand for a large thermonuclear research program because the new director had first to address more practical concerns, which included keeping Los Alamos operating, and finding new staff members to replace the droves that left. In the laboratory's technical program, Bradbury wanted improvements in existing "lousy" fission devices. Scientists and engineers designed and built the wartime implosion and gun devices to meet a deadline. This set of weapons was, in Bradbury's words, ". . . totally wrongly matched to the production empire." In the peace brought by the war's end, Los Alamos would increase atomic weapons' efficiency and yield, while decreasing their size and weight.[192]

Work on thermonuclear weapons did not completely stop at the end of the war. Bradbury approved of modest theoretical research on the Super, but placed it at a lower priority than fission weapons work. The Los Alamos Technical Board, essentially a policy-making body existing since the war, agreed that "We can go ahead with it [Super Research] as we have personnel available." The rapid departure of technical staff from the Laboratory acted to slow both the postwar fission program and more so hydrogen weapons research. Moreover, the fission program itself limited work on the Super and other thermonuclear theories. I explore these dynamics more in Chapter 5.[193]

Los Alamos indeed faced a social setback at the end of the war, nearly devoid of personnel by the end of 1946. Bethe recounted that Los Alamos was

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[192] Norberg interview with Norris E. Bradbury, 68-69.
[193] Teller, Legacy, 22; Handwritten notes from Los Alamos Technical Board Meeting, December 13, 1945, B-9 Files, Folder 001, Drawer 1, LANL Archives, [This Document is Secret-RD].

"very weak" in the period following the war, and not enough staff remained to work intensively on any weapons projects, least of all thermonuclear devices. F Division was dissolved completely in November 1945 as many of its members planned to leave in the next year. For the first part of 1946, Teller's group moved back into T Division. Even though he left for Chicago, by the summer's end Teller theorized about a second type of thermonuclear weapon, the Alarm Clock; Teller recalled he first proposed the Alarm Clock on August 31, 1946, the day his daughter was born.[194]

A small number of young scientists chose to remain at Los Alamos. Robert Richtmyer specialized in theoretical physics at Massachusetts Institute of Technology, and came to Los Alamos from the OSRD patent office in Washington, DC towards the end of the war to work in the Laboratory's patent office, only to move to T Division in 1946. According to Teller, Richtmyer kept thermonuclear weapons research alive after the war.[195]

Even after settling in Chicago, Teller visited Los Alamos every few months and worked closely with Richtmyer on the new Alarm Clock scheme. Richtmyer alone filed a report on this in the fall of 1946, having done hand calculations on its feasibility. Teller and Richtmyer alternately named this the "Swiss Cheese" weapon -- vaguely a "modified" Super. Although it purported to employ the same basic nuclear materials as the Super, Teller

[194] Truslow and Smith, Project Y: Part II, 307-308; Memorandum to the file from John Walker of a discussion with Dr. Edward Teller, January 13, 1953, JCAE declassified General Subject Files, Box 58, NARA.

[195] Teller lecture at Los Alamos, March 31, 1993; Author interview with Robert D. Richtmyer, March 4, 1997, Boulder, CO; Interview transcription held at American Institute of Physics Center for the History of Physics Niels Bohr Library.

likely came up with the Swiss Cheese configuration in the interest of conserving tritium, an issue I discuss further in Chapter Four.[196]

In addition to the Alarm Clock, Teller also pushed another scheme called the "Booster," basically an ordinary fission bomb with increased efficiency due to the timed injection of small amounts of tritium and deuterium gases into the hollow center of the fissile weapon core after it began to fission.[197]

As with the Alarm Clock, Teller did not come up with the Booster alone. Rather, this idea had several inventors. The name "Booster" did not appear until Teller put it down in report in 1947, but some form of this idea had been around for at least a couple of years. Turkevich had reported working on "deuterium boosted gadgets" as part of his assignment for F Division in June 1945. Carson Mark also claimed that at the end of 1946 he and Richtmyer theorized that it would be "fun" to put some D-T on the edge of a fission core, let it get compressed and hot, then see if any neutrons could be observed. According to Mark, Teller caught on to this idea and modified it by imagining putting the D-T in the middle, and named it the "Booster."[198]

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[196] R. Richtmyer, LA-610, "A New Thermonuclear System," November 15, 1946, 3, LASL, [This Report is Secret-RD]; Teller classified lecture at Los Alamos, 1993; Author interview with Richtmyer, March 4,1997; Teller has claimed that he thought up the name "Alarm Clock" to wake scientists up to the possibility of thermonuclear weapons; LAMS-448, "T Division Progress Report: September, and October 1-20, 1946," November 11, 1946, 2, [This Report is Secret-RD].

[197] E. Teller, LA-643, "On the Development of Thermonuclear Bombs," May 7, 1948, LASL, 29. [This Report is Secret-RD]; Hansen, US Nuclear Weapons, 13.

[198] LAMS-272, F Division Progress Report for July, 1945, August 9, 1945, 8, [This Report is Secret-RD]; LA-12656-H, Beverly A. Wellnitz, The last Vade Meacum: Conversations on Early Nuclear Test Devices, LANL, September 30, 1993, 47, [This Document is Secret-RD].

Teller tried as best he could to direct thermonuclear studies from Chicago, but was still only a consultant to Los Alamos. In this role, Teller spent his summers and many breaks in New Mexico between 1946 and 1949, occupying much of his time writing elaborate outlines for Super research and encouraging others to perform a variety of thermonuclear calculations related to the Super and Alarm Clock.

During his visit to Los Alamos in the summer of 1947, Teller held a review meeting with Richtmyer, Maria Mayer, and several other colleagues to discuss the Super, Alarm Clock, and Booster. In a classified report on this meeting, Teller noted that the functioning of the Super was very hard to calculate "because . . . [so many] variations in time and space must both be taken into account." The ENIAC calculation, Teller continued, had been based on many simplifying assumptions, the gravest where the sidewise escape of 14 million volt neutrons had not been taken into account. The ENIAC work remained the only large machine treatment of the Super, but in 1947 its results did not seem hopeful to Teller and his colleagues: More than 400 grams of tritium would be needed to ignite the Super.[199]

Teller also reviewed the status of the Alarm Clock, noting that hand calculations indicated that the energy required for one particular model's ignition would be roughly equivalent to one-million tons of TNT. As with the Super problems, simplifications were made to the Alarm Clock calculations, so no one could assess its feasibility with any certainty.

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[199] Mark, Short Account, 7-9; LA-643, 9-10.

Furthermore, both the Alarm Clock and Super appeared to require the development of a giant gun gadget to serve as a trigger for each.[200]

Finally, Teller called attention to another form of nascent technology, noting that any more detailed calculations of both the Super and Alarm Clock would require fast electronic computers. Although Teller urged had calculations for both the Super and Alarm Clock theories to continue, he hoped that "[e]ventual use of fast computing equipment may be speeded up if the theory of these bombs is not neglected in the near future."[201] Obviously this sort of work needed completion before either the Super or Alarm Clock could be designed and tested. As Teller penned this report in 1948, he recommended:

> I think that the decision whether considerable effort is to be put on the development of the Alarm Clock or Super should be postponed for approximately 2 years; namely, until such time as these experiments, tests, and calculations have been carried out.[202]

Computers of the Future

If thermonuclear weapons calculations required machines more powerful than ENIAC, then T Division's only choices for performing this work involved either waiting for adequate computers to become available, or to build its own machines.

Far from Los Alamos, construction of other computers proceeded slowly. Von Neumann began planning a high-speed, fully automatic digital

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[200] LA-643, 19.
[201] Ibid., 11, 19, 37-39.
[202] Ibid., 37.

computer at Princeton's Institute for Advanced Study (IAS) in 1945, hoping that when completed, it would be well-equipped to handle complex problems like thermonuclear weapons calculations. Other computers began to appear, as well. In New York, IBM unveiled its giant SSEC (Selective Sequence Electronic Calculator) in 1948; Presper Eckert and John Mauchly began planning the UNIVAC (UNIVersal Automatic Computer), and; in Washington, DC, the National Bureau of Standards began work on its SEAC (Standards Eastern Automatic Computer).[203]

For T Division, construction of these machines meant that perhaps a full calculation of the Super could be carried out. As the machines became available, Los Alamos farmed out calculations to the distant computing centers as long as equipment within the Laboratory's own fences remained inadequate. However, new computers did not seem to become available quickly enough, thus interest in building a fast computer at Los Alamos grew more serious towards the end of the 1940s. In 1946 Bradbury recommended that the Laboratory acquire an electronic computer.[204]

T Division in particular wanted its own electronic computer. Canadian-born Carson Mark had joined Los Alamos late in the war, arriving as part of the British team in 1944. He became T-Division leader in 1947, and ultimately held responsibility for theoretical work on both fission and

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[203] Aspray, John von Neumann, 53; Foster Evans, "Early Super Work," in Behind Tall Fences: Stories and Experiences about Los Alamos at its Beginning, (Los Alamos: Los Alamos Historical Society, 1996), 135-142; Michael R. Williams, A History of Computing Technology, (Englewood Cliffs, NJ: Prentice-Hall, 1985), 260-261, 362-363, 367.
[204] Memorandum from Norris Bradbury to Colonel H.C. Gee, November 7, 1946. DOE Archives, RG 226, Box 4944, Folder 7.

thermonuclear weapons. Later in life, Mark confessed that to many scientists the Super represented, "a theoretical wonder . . . . just bristling with problems." For this, Mark pushed Bradbury to allow T Division to build its own machine.[205]

Others pushed Bradbury, too. During a visit to Los Alamos in October 1946, Teller outlined an unofficial Laboratory program for the near future, stating that T Division needed to expand and perhaps some of the mathematical work on the Super be farmed out. He praised the laboratory's acquisition of the punched-card machines because they had truly expedited numerical work. Teller believed that within a year or two, efficient electronic calculating machines would be available rendering the accounting machines obsolete. Teller advised Los Alamos to obtain such electronic computers as soon as possible, since they would render the work of T Division more valuable.[206]

Soon after Teller outlined his recommendation, Richtmyer told Bradbury that the laboratory must emphasize the means and methods for nuclear calculations. Therefore, the laboratory should aim at "building or buying a really good electronic computing system" within the next 6 to 18 months, and that, "The planning of such a [computing] laboratory here,

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[205] Norberg interview with J. Carson Mark, 1980.
[206] Edward Teller, "Proposed Outline of Laboratory Program," October 1, 1946, LANL Archives, B-9 Files, Folder 635 "Laboratory Program," Drawer 176, [This Document is Secret-RD].

geared toward Los Alamos's problems, is an important item for national defense."[207]

By summer 1948, the Laboratory set plans to construct its own high-speed electronic computer. Bradbury wrote to von Neumann seeking his opinion on the Laboratory's plans to build a computer of its own. Von Neumann replied to Bradbury:

> I have just received your letter of June 24th, and I hasten to tell you that I am in complete agreement with the philosophy which it expresses. I think that with a small amount of good luck it should be possible to have a high-speed computing installation at Los Alamos in existence by the end of 1949 or the first half of 1950 . . . .[208]

1950 proved too optimistic a prediction. A desperate housing shortage in the town hampered Bradbury's bringing more staff to the Laboratory, which in turn delayed the Laboratory's computer project. Not until January, 1949 could Mark bring Metropolis back from the University of Chicago to build the Los Alamos computer, a device intended to replicate the IAS machine. Mark recalled:

> [Metropolis] was fascinated with the capability of the coming computing equipment. He had been working here during the war and [later] as a consultant on the hardest problems we had, which were to do with the basic operation of a thermonuclear device. He had worked on some of this material on the ENIAC and thus worked with the most advanced computers at the time. He was interested in the thermonuclear problems.[209]

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[207] Memorandum to N.E. Bradbury from R.D. Richtmyer, "Commentary on Proposed Directive from AEC," March 21, 1947, LANL Archives, B-9 Files, Folder 635,"Laboratory Program," Drawer 176, [This Document is Secret-RD].
[208] Letter to Norris Bradbury from John von Neumann, July 2, 1948, in John von Neumann (hereafter JVN) papers, Library of Congress (hereafter LOC), Box 14, Folder 10.
[209] LAB-ADIR-A-45, Technical Board Minutes, August 16, 1948, LANL Archives, B-9 Files, Folder 001, Drawer 1, [This Document is Secret-RD]; Author interview with J. Carson Mark, Los Alamos, New Mexico, January 19, 1996; Interview transcription held at LANL.

According to Mark, von Neumann kept "encouraging us to believe that his machine was going to be ready sooner than it actually was. . . ." [he was] ". . . hopeful and optimistic." The IAS machine came on line much later than von Neumann predicted, making some of the T Division staff more anxious to build one of their own. In addition, Metropolis wanted to build a computer at Chicago, but the University never allowed him to do so, making the prospect of constructing a machine at Los Alamos all the more appealing to him. "The arrangement we made for Metropolis," Mark recalled, "was specifically for him to come and build a copy of the Princeton machine."[210]

Metropolis did not leave Chicago by himself. Urged by Ulam and encouraged by Mark and Richtmyer, Teller soon followed Metropolis to Los Alamos to work full time and encourage a more concentrated effort on the Super. While Los Alamos waited for adequate computing technology to become available for Super calculations, the small number of permanent staff in T Division spent long hours doing simple hand calculations on various aspects of the Super and planning machine calculations for computers that did not yet exist.[211]

Taming and Mechanizing Large Animals: HIPPO and Baby HIPPO

Some machine calculations were created to benefit both the Super and fission programs. In October 1947, Richtmyer began to plan HIPPO -- a detailed machine calculation of the course of a fission explosion -- with the

[210]Author interview with J. Carson Mark, January 19, 1996.

hopes that Los Alamos would be better able to understand the atomic explosion process, and because a fission trigger would act to ignite a Super.[212]

True to its name, the giant HIPPO superseded the wartime fission studies in detail and accuracy. Wartime studies of the mechanics of the fission explosion led to the Bethe-Feynman formula for efficiency, but had to assume steady-state conditions and one-dimensional motion. Thus, uncertainties in the methods of calculations led to only minimal understanding of what went on in a fission explosion. Project HIPPO (subtitled "Mechanized Calculation of Efficiencies and other Features of a Fission Bomb Explosion) would give a greater understanding of the fission process. According to Carson Mark, HIPPO modeled the Trinity explosion, followed the radiation flow, and hydrodynamics and energy.[213]

Richtmyer remembered:

> They didn't know now to put things together, really . . . . There were several phenomena involved -- and for each one the mathematical methods were known partly or largely because of things done at Los Alamos under simplifying assumptions about the others. For example, if you have fissionable material uniformly distributed, moving, stationary, then you can compute the neutron multiplication. So there were these things, the problem was to put them together for the big computers just there.[214]

Richtmyer asked von Neumann to assist with this project with the hope that the IAS computer would soon be available, thus Richtmyer and his team -- which also included Klari von Neumann, Foster and Cerda Evans,

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[211] Stanislaw Ulam, Adventures of a Mathematician, (New York: Charles Scribner's Sons, 1976), 192-193.
[212] Author interview with Richtmyer, March 4, 1997.
[213] R.D. Richtmyer, LA-1282, Project HIPPO, August 10, 1951, 5-7.

and Herman and Adele Goldstine -- moved to Princeton. Richtmyer shared an office with Adele Goldstine and Klari von Neumann, and recalled how the two women satirically named the secret project:

> I had a habit of writing on the upper right corner of the blackboard cryptic notes to myself about things I had to do. On one occasion, I was away for about ten days, and when I returned, there was an additional note on the blackboard in imitation of my handwriting. It said 'fresh water for hippo.' In consequence, 'Hippo' became the code name for the project we were working on.[215]

Richtmyer and his team spent the summer of 1948 in Los Alamos using the IBM implosion calculations and approximate analytic theories of the early stages of a fission explosion to provide a set of initial conditions for the machine calculation. In the course of preparing the calculation, the team made several modifications of the now standard implosion calculation techniques to prepare the HIPPO problem for an electronic treatment. One of the most important modifications included von Neumann and Richtmyer's artificial viscosity treatment of shocks, a means to manage the problem of calculating on computing machines the progress of shock fronts in explosions and implosions.[216]

Because the IAS computer still awaited completion, Richtmyer and his group initially coded HIPPO for the ENIAC. Von Neumann, however, suggested that they use the newly completed SSEC, an IBM technological showpiece on public display on Madison Avenue in New York City. Von Neumann chose the SSEC over the ENIAC because the IBM computer had a

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[214] Author interview with Richtmyer, March 4, 1997.

greater capacity for instructions and number storage, even though it was sluggish. The monster calculation, then, would be run on a monster machine. Los Alamos did not make formal arrangements with IBM until 1949, and Richtmyer and his team did not begin to run HIPPO Problem I on the SSEC until March 1950. IBM personnel had never before prepared such a large and complex problem, and coding proceeded slowly. The program actually consisted of a set of two problems: HIPPO Problem I analyzed the Trinity test, and took about six months to complete; problem II modeled Little Edward -- a giant, high-yield multi-crit gun device proposed by Teller that was supposed to produce x-radiation to initiate the D-T mixture in the Super [217]

Soon after Richtmyer started planning HIPPO, physicist Rolf Landshoff began to work on a scaled-down version of the program, aptly named Baby HIPPO, for the IBM card punches at Los Alamos. Landshoff intended for Baby HIPPO to assist Richtmyer and his team with the larger calculation, but discontinued it in early 1950 when the larger HIPPO program began to run in New York. Baby HIPPO gave a picture of the events in the core and tamper of the Trinity device up to about halfway through the explosion.[218]

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[215] Author interview with Richtmyer, March 4, 1997; Richtmyer unpublished memoirs.
[216] LA-1282, 9.
[217] LAMS-900, "T-Division Progress Report: April 20, 1949-May 20, 1949," June 2, 1949, LASL, [This Report is Secret-RD]; R.D. Richtmyer, LA-1282, Project HIPPO, August 10, 1951, LASL, [This Report is Secret-RD]; Author interview wth Richtmyer, March 4, 1997.
[218] Mark, Short Account, 7-8; John Bond, LA-1442, Baby HIPPO, July 1, 1952, LASL, [This Report is Secret-RD]; Mark claims that in order to get HIPPO to run at all, Richtmyer and von

Monte Carlo

Besides those working on the HIPPO project, others also tried to find better ways to simulate a nuclear chain reaction. Stanislaw Ulam had departed Los Alamos in 1945 for the University of Southern California. However, Ulam became unhappy in Los Angeles and critical of USC's academic standards. In 1946 Richtmyer and Metropolis invited Ulam back to New Mexico and he returned to Los Alamos later that year. Not long after his return to New Mexico, Ulam began to formulate a new means of handling neutron diffusion calculations. Ulam had been ill with an inflammation of the brain while in Southern California. Confined to bed, during his recovery he enjoyed playing solitaire. According to Ulam, he:

> . . . noticed that it may be much more practical to get an idea of the probability of the successful outcome of a solitaire game . . . by laying down the cards, or experimenting with the process and merely noticing what proportion comes out successfully . . . . It occurred to me that this could be equally true of all processes involving branching of events, as in the production and further multiplication of neutrons in some kind of material containing uranium or other fissile elements.[219]

To estimate the outcome of these reactions, random numbers with suitable probability could be used to select by chance the fate of a neutron at each stage in the fission process. After examining the possible histories of a few thousand, one would have a good sample and approximate solution to the problem. After Ulam raised the possibility of using such probabilistic

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Neumann invented artificial viscosity, which, according to Mark, "is absolutely rock bottom input for everything done since." Mark quoted in Wellnitz interview, 73.
[219] Ulam, Adventures, 186, 196-197.

schemes to von Neumann in 1946, together they developed the mathematics of the Monte Carlo method. Repeated calculations with the computer could be used to estimate the outcome of these reactions. Therefore, von Neumann proposed an outline of a computerized Monte Carlo neutron diffusion calculation in a letter to Richtmyer in 1947, stating that the "statistical approach is very well suited to a digital treatment. . . . I am fairly certain that the problem . . . in its digital form, is well-suited for the ENIAC."[220]

Von Neumann believed that the ENIAC would be the fastest means for applications of these statistical sampling techniques that required long and tedious calculations. Moreover, in 1947 ENIAC was the only large machine available that von Neumann could try the Monte Carlo method out on. For neutron diffusion problems, Los Alamos recognized by the end of 1947 that "a computer at least like ENIAC would be necessary for applications of the Monte Carlo method." Throughout the latter half of the 1940s Los Alamos used ENIAC extensively in for Monte Carlo problems for fission weapons, which I will elaborate on in Chapter Five.[221]

Advanced Weapons, or a Large "Bang"?

Calculations related to the postwar fission program remained a higher priority than Super-related ones, and Los Alamos itself did not declare a

[220] Ulam, Adventures, 196-197, 199; R.D. Richtmyer and J. von Neumann, LAMS-551, Statistical Methods in Neutron Diffusion, April 9, 1947, 3-5.
[221] Nicholas Metropolis, "The Beginning of the Monte Carlo Method," Los Alamos Science 15, LANL, (1987), 125-130; Roger Eckert, "Stan Ulam, John von Neumann, and the Monte Carlo Method," Los Alamos Science 15, 131-141;. LAMS-653, Laboratory Annual Progress Report for 1947, December 3, 1947, 16; Peter Galison examines the creation of the Monte Carlo method in

formal research and development policy on thermonuclear devices. Nevertheless, in 1947 the newly organized AEC expressed a conditional interest in thermonuclear weapons, recommending that Los Alamos pursue studies of the Super and Alarm Clock theories in a leisurely and scholarly manner.[222]

The Commission's Scientific General Advisory Committee, headed by Oppenheimer and composed partly of several former Los Alamos scientists, met only for the second time in February, and hastened to try and formalize a plan for Los Alamos that would help strengthen the Laboratory. Fermi felt it "important to make Los Alamos healthy," and that the Super should be pursued as part of the Laboratory's long-term research. The General Advisory Committee agreed, and recommended that an emphasis at Los Alamos on problems associated with thermo-nuclear [sic] explosives would be stimulating work and helpful towards strengthening the facility.[223]

Although for the most part occupied with the Sandstone fission weapons test series up through most of 1948, Los Alamos attempted to plan for another test to see if thermonuclear burning could be obtained. In the fall of 1948, however, some members of the Laboratory felt that they knew so little mathematically about thermonuclear weapons, it would be difficult to

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his recent book, Image and Logic: A Materials Culture of Microphysics, (Chicago: The University of Chicago Press, 1997).

[222] Draft of Proposed Directive for the Los Alamos Laboratory, April 1, 1947, LANL Archives, B-9 Files, Folder 635, Drawer 126; Memorandum to the File from J. Kenneth Mansfield, "Extracts from GAC Reports Relating to the Thermonuclear Program," May 28, 1952, JCAE declassified General Subject Files, NARA, Box 59.

decide specifically which principles of thermonuclear burning should be tested in the first place. Landshoff, working on Baby HIPPO, explained pessimistically to his colleagues that the Super calculations were so complex, any realistic simulation would require the use of high-speed computing machines (better than the IAS computer and SSEC) which would not be available for some time. In addition, in 1948 few people understood how fast electronic computers worked. Finally, no one could guarantee that the new machines would be entirely able to handle the Super calculations.[224]

As in wartime, at postwar Los Alamos committees often made policy decisions. Some committees formed with specific weapons design tasks in mind, others for more exploratory purposes. The Committee for Weapons Development (CWD) fell into the latter category, formed by Darol Froman in summer 1948 to discuss long-term and "advanced" weapons ideas.

At the CWD's first meeting, Froman announced that the Laboratory must come to a decision on the number and type of test shots planned for 1951, and that this plan should be submitted to the AEC and GAC. Many of the members of the CWD were only consultants, including von Neumann and Teller, who proposed that for the 1951 tests, four devices should be

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[223] Draft minutes of the General Advisory Committee, Second Meeting, February 2-3, 1947, 3-4, 8, U.S. Department of Energy Archives, Record Group 326, Box 337, declassified.
[224] "Summary of a Discussion on Super-Weapons Policy," September 24, 1948, LANL Archives, B-9 Files, Folder 635, Drawer 126, [This Document is Secret-RD].

considered: a small gadget; a hydride weapon; the Booster, to obtain information about thermonuclear reactions; and, Little Edward.[225]

Teller made other suggestions at the August 1948 CWD meeting for the 1951 test series, including a special[226] implosion gadget which might serve as in initiator for the Super instead of the Little Edward device. The special implosion idea might require less active material than the Little Edward device, yet no one had any idea if either configuration would be able to initiate a Super.[227]

George Gamow was also a consultant to Los Alamos and had not only known Teller from George Washington University, but had been instrumental in bringing Teller from Europe to Washington, DC. At the request of Bradbury, in 1949 Gamow spent a sabbatical year at Los Alamos, to help with theoretical work on hydrogen weapons. Gamow joined the CWD, and liked the idea of initiating a Super with an implosion of active material. Grossly exaggerating this idea, Gamow, proposed the "You Can't Lose Model," with caricatures of Teller's and Ulam's heads protruding from the top (see figure 2).[228]

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[225] The hydride weapon was proposed during the war but was dropped. Feynman had worked on this idea, basically a fission device using UH^3; Minutes of Meeting: Committee for Weapon Development, August 13, 1948, 1-4, LAB-J-479, [This Document is Secret-RD].
[226] "Special" is a generic term used by the author in lieu of a classified name which more accurately describes this type of implosion gadget. The author has done this at the request of the Los Alamos National Laboratory Classification Review group.
[227] LAB-J-479, 6-7.
[228] George Gamow, My World Line: An Informal Autobiography , (New York: The Viking Press, 1970), 32; G. Gamow, "Proposals in the Direction of the Super," LAB-ADWD-25, January 14, 1949, LANL Report Library, [This Document is Secret-RD].

Also a member of the CWD, Ulam suggested that Los Alamos specify four weapons to test in 1951: the small fission weapon for military purposes; two other gadgets to give information on basic thermonuclear reaction processes; and a fourth gadget that could act as a mechanical initiator of a thermonuclear reaction.[229]

For a laboratory lacking personnel this plan was ambitious to carry off by 1951. Los Alamos remained considerably weaker than it had been during the war, and now dependent on the AEC for material support of the proposed weapons test programs. The CWD agreed that the projected 1951 tests could be accomplished only with genuine support from the AEC and a reasonable increase in the scientific and engineering personnel at Los Alamos. Los Alamos's survival was still an issue. The CWD agreed:

> Without very real support from the AEC in such items as speed in necessary construction, speed in clearance of personnel, ease in handling overtime work, additional housing, and in giving the aid of other AEC laboratories with respect to the production and treatment of tritium, this program would be difficult to accomplish. It is thought better to attempt a program which taxed the ability of the Laboratory to an extent just less than that which would produce a feeling of hopelessness rather than to attempt an easy program which would not attract the interest of many scientists presently in the Laboratory and outside of it.[230]

Echoing the GAC's earlier suggestion, the Laboratory leadership thought that research on the Super and other thermonuclear devices would at least provide an intellectual challenge for the Laboratory, and an incentive

[229] LAB-ADWD-25, 16.
[230] Ibid., 16-17.

for growth when Los Alamos struggled to find a permanent mission in the postwar period.

When the CWD met again in September a smaller number attended -- only Fermi, Froman, Holloway, and Ulam. Teller and others had to return to their respective institutes for the beginning of the academic year, but Froman had wanted to hold this meeting so Fermi could present his opinion on the test models proposed by Teller the previous month. Fermi argued that the Little Edward project seemed wasteful. Merely testing a multi-crit gun would only produce a large "bang," and if so, it should include some means of determining whether or not it would initiate a thermonuclear reaction in tritium and deuterium. The entire committee agreed that a test of this gadget in 1951 would require an appreciable strengthening of the laboratory and doubted its usefulness in a test.[231]

What Do Machines Know Anyway? Re-evaluating the ENIAC Calculations

Before any test of a means for initiating a Super could be carried out, T Division's members and consultants had to make headway into mathematical analyses of the Super's feasibility. In December, 1948 Ulam mentioned that he and von Neumann had a proposal they had been working on since September, to prepare a new Super calculation. Ulam described the philosophy behind this proposal:

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[231] Minutes of the Committee for Weapons Development, September 23, 1948, 1, LAB-ADWD-1. LANL Report Library, [This Document is Secret-RD].

> [It was] . . . essentially to make calculations which are rather detailed and precise in so far as the physical properties of the substances are concerned, i.e., properties which are essentially independent of the geometry in the sense that no particular detailed geometrical model is chosen for the Super. The object is to learn something about the feasibility of the Super in so far as the physical properties of the substances are concerned.[232]

When the CWD met again in January 1949, von Neumann had revised Ulam's report, adding that he was not certain whether such calculations could be made on the fastest machines that now existed -- ENIAC and the SSEC. Von Neumann tended to believe that this work could be carried out on one of these machines, but it would take about six months to complete. On the other hand, von Neumann remained optimistic that the IAS computer would be available by the end of 1949, and that on it the Super calculations would be completed in perhaps just a few weeks. Considering the other options open to Los Alamos, Ulam suggested that the Super calculations might be carried out by 50 or 100 hand computers over a six-month period.[233]

Los Alamos did not have enough hand computers to perform the tedious Super calculations. Although by 1948 several analytical studies and attempts at numerical solutions of the Super Problem (using desk calculators) had been undertaken, few answers emerged. A fast, electronic computer, then, might make up for the little human labor available for the Super calculation. In 1948 von Neumann had faith that the IAS machine would

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[232] Committee for Weapon Development, Minutes of Meeting, December 30, 1948, 1, LAB-ADWD-21, LANL Report Library, [This Document is Secret-RD].
[233] Committee for Weapon Development, Minutes of Meeting, January 11, 1949, 1-2, LAB-ADWD-23, LANL Report Library, [This Document is Secret-RD].

fulfill this role of labor-saving technology, and the Hungarian mathematician convinced several of his Los Alamos colleagues to work with him in preparing a Super calculation tailored for the IAS computer. Los Alamos physicists Foster and Cerda Evans (a husband and wife team), Metropolis, Teller, John and Klari von Neumann -- who had impressed the group with her extensive knowledge of coding and flow diagramming -- and Ulam began to prepare the logical layout of a machine calculation of a spherically symmetric model of the Super problem; this program would include all the effects left out of the 1945-46 ENIAC calculation. The new Monte Carlo technique inspired von Neumann and his peers, who believed that they could create a Monte Carlo procedure for the ENIAC that would account for different kinds of particles.[234]

Foster Evans recalled:

> We divided the problem into two parts: "hydrodynamics" and "particle physics." In the particle physics part, all of the thermonuclear reaction products and photons were treated by Monte Carlo . . . to determine where and at what rate their particles exchanged their energy in the plasma. In the hydrodynamics portion, the resulting heat exchange and motion of the plasma was calculated . . . . all of these processes take place continuously and simultaneously. In a numerical calculation, one approximates this by dividing time duration into small but finite intervals and space into small zones . . . . the capacity of the memory limits the number of zones one can use.[235]

By the time the group completed the layout of the problem, they realized that the Princeton machine would still not be ready, so the group

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[234] Mark, Short Account, 6; Letter from Ulam to JVN, May 16, 1949, LOC, Box 7, Folder 7; LAMS-673, "T Division Progress Report: 20 November, 1947-20 December 1947, January 8, 1948," 11, [This Report is Secret-RD].
[235] Evans, "Early Super Work," 138.

decided to trim the problem so as to fit on ENIAC, now at its final home in Aberdeen, Maryland.[236]

Ulam and University of Wisconsin mathematics professor, Cornelius Everett, decided that waiting for the Princeton machine would take too long. With slide rules and hand computers, Ulam and Everett performed simplified Super calculations based on the 1949 machine outline, whose purpose supposed to determine the amount of tritium necessary to make the Super ignite. Believing the problem was impossible to carry out with analytic methods, Ulam and Everett applied the Monte Carlo method, by hand, in a highly schematic and enormously time-consuming manner. Although admitting that the problem was nearly impossible to attack by analytical means alone, the two mathematicians tried to answer the question of whether or not the Super could be ignited using a mixture of half tritium and half deuterium. Ulam and Everett described the question they tried to answer:

> The physical problem is, of course, fundamental to the whole question of Daddy, namely, can one attain a sizable reaction in pure deuterium starting from a moderate amount of tritium and deuterium mixed together and ignited, by a suitable methods, from a fission bomb?[237]

By the end of February 1950, Ulam and Everett's results showed that Teller's previous estimates ranging between 300 and 600 grams of tritium

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[236] Ibid., 138.
[237] Evans, "Early Super Work," 138-139; Mark, Short Account, 8; C.J. Everett, S. Ulam, LA-1076, Ignition of a Large Mass of Deuterium by a Burning Deuterium-Tritium Mixture: Problem I, March 7, 1950, LASL. [This Report is Secret-RD]; Carson Mark, "From Above the Fray," Los Alamos Science 15, (1987), 33; Quotation in LA-1076, 5.

were not nearly enough to make the Super ignite. Ulam and Everett concluded that the Super model they had considered would be a fizzle, then discontinued the calculation.[238]

Ulam and von Neumann had been very close friends for a long time, frequently corresponding about personal issues, Super calculations, and ongoing computer projects. The two friends encouraged each other to continue trying to find solutions to the Super Problem. Ulam visited von Neumann in Princeton to discuss the hand calculations he and Everett had done, and Fermi later joined the conversation. Von Neumann concluded that the only possible solution was to increase the amount of tritium in the theoretical design of the Super. Still, this change would make the Super less attractive. Ulam returned to Los Alamos and broke the news to Teller, yet decided to try another hand calculation for the ignition problem. Ulam reported to von Neumann in March:

> . . . Everett has managed to formalize everything so completely that it can be worked on by a computer. Josephine Elliott (the queen of computers) has inherited another problem yesterday . . . Edward finally managed to organize a new committee - where he will be able to talk unimpeded about the [Little Edward] gun - essentially to himself. Very private impression [about the gun]: $100,000 and six months or more.[239]

Consequently, Elliott, Ulam's wife Françoise, and Joan Houston began a second calculation assuming several hundred more grams of tritium. Again, the results appeared very unfavorable -- the device would still not ignite. In

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[238] LA-1076, 5.

[239] Hewlett and Duncan, Atomic Shield, 440; Letter from Ulam to von Neumann, March 17, 1950, JVN papers, LOC, Box 7, Folder 7.

May 1950, Ulam reported to von Neumann that the future of the Super looked dim.[240]

Teller worried about the negative results of the hand calculations, and in June 1950 called a special meeting of the CWD, where Ulam reported that the hope for detonation of deuterium in the Super looked "miserable" -- the deuterium did not reach a very high temperature and then started to drop. Bethe also arrived in the summer of 1950 to consult on recent progress in fission weapons, and he too attended this meeting. Looking over the hand calculations, Bethe agreed that the prospects for igniting the Super were poor and would probably require a kilogram of tritium.[241]

Concerned about the negative results that both the hand and machine calculations displayed, Teller had already written to von Neumann in May 1950, lamenting that the laboratory found itself in a "state of phenomenal ignorance" about the Super, and that part of this ignorance could be attributed simply to the lack of fast computers. Von Neumann in turn wrote to Bradbury saying that he hoped the IAS would accelerate completion of its electronic computer, because it seemed "increasingly clear in connection with Los Alamos's requirements, especially in the current atmosphere of crisis, that radical measures to finish the computer were necessary."[242]

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[240] C.J. Everett, S. Ulam, LAMS-1124, Ignition of a Large Mass of Deterium by a Burning D-T Mixture: Problem II, June 16, 1950, LASL, [This Report is Secret-RD]; Hewlett and Duncan, Atomic Shield, 440.

[241] Hewlett and Duncan, Atomic Shield, 441; LAMD-411, "Weapon Development Committee, Minutes of June 21, 1950," 1-4, [This Report is Secret-RD].

[242] Letter to von Neumann from Teller, May 23, 1950, ADWD-140, LANL Archives, B-9 Files, Folder 635, Drawer 166. [This document is Secret-RD]; Letter to Bradbury from von Neumann,

In 1950 ENIAC was still the only other available electronic alternative to verify the hand calculations. Thus, the Evanses, the von Neumanns and others created another program for the ENIAC to determine how much tritium might be required to ignite the Super. This program, involving two separate calculations with various admixtures of tritium, ran in the spring and summer of 1950. The initial results agreed with the earlier hand calculations; the Super design looked unpromising and if at all possible, would consume far too much tritium. By the summer the Evanses, the von Neumanns, and others running the program abandoned it because Los Alamos's contract for time on the machine had expired and the results seemed so discouraging.[243]

Fermi arrived in New Mexico in the summer of 1950 and with Ulam, set up a calculation to explore the second half of the Super problem: If the Super could be ignited, which now seemed doubtful, would the burning of deuterium propagate and become self-sustaining? While Josephine Elliott and Miriam Planck performed the entire calculation, Fermi and Ulam supervised. They reported that although this was a crude set of calculations, the group made four attempts with different parameters. Each calculation predicted the Super would fizzle.[244]

July 18, 1950, LANL Archives, B-9 Files, Folder 635, Drawer 181. [This Document is Secret-RD].

[243] John Calkin, Cerda Evans, Foster Evans, John von Neumann, Klari von Neumann, LA-1233, The Burning of D-T Mixtures in a Spherical Geometry, April 23, 1951, LASL, [This Report is Secret-RD].

A Family of Weapons

While 1950 saw the Super's prospects diminish, 1949 had been a nemesis to American national security. The political impact of the 1949 Soviet atomic test on U.S. nuclear weapons policy has been analyzed by such historians and political scientists as Gregg Herken, Michael Evangelista, Barton Bernstein, and David Rosenberg to the point that I will not discuss this event. However, the more subtle impact that the first Soviet nuclear bomb had on Los Alamos is not well known. This event helped both to solidify Los Alamos's place in the large AEC system at the end of the 1940s, and to provide a more concrete postwar mission for the Laboratory.[245]

Up until 1949 the GAC, chaired by Oppenheimer, supported modest work on thermonuclear weapons at Los Alamos. In 1948 the group had little confidence in the Super's usefulness as a military weapons, they felt it "still necessary to inquire as to its possibilities." Oppenheimer suggested that it might be useful to encourage Los Alamos to pursue the design of the Booster bomb, since it had three possible consequences:

> [O]ne would learn about the concrete development of thermonuclear weapons, one would explore alternatives to present nuclear explosive materials, and one would take a step along a path leading to possible future development of more devastating weapons.[246]

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[244] E. Fermi and S. Ulam, LA-1158, Considerations on Thermonuclear Reactions in Cylinders, September 26, 1950, 3-4, 21, LASL, [This Report is Secret-RD].
[245] See: Herken, The Winning Weapon," op. cit; also see: Michael Evangelista, Innovation and the Arms Race: How the United States and Soviet Union Develop New Military Technologies, (Ithaca: Cornell University Press, 1988); Barton Bernstein, "The H-Bomb Decisions: Were they Inevitable?" in National Security and International Stability, eds. Bernard Brodie, (Cambridge: Cambridge University Press, 1983); David Alan Rosenberg, "American Atomic Strategy and the Hydrogen Bomb Decision," The Journal of American History 66, June 1979).
[246] In 1949 the GAC membership included, in addition to Oppenheimer, Fermi, Rabi, Glenn T. Seaborg, Conant, metallurgist Cyril Smith, Hartley Rowe, Hood Worthington, and Lee

Oppenheimer further suggested that for a test of thermonuclear principles involving the Booster bomb, a time scale of two years for a test might be in order. On the other hand, the GAC discouraged an all-out effort on the Super and Alarm Clock for several reasons I discuss in Chapters Four and Five.[247]

In its first years the GAC was a powerful advisory committee, especially when it came to formulating policy for the AEC's specialized laboratories such as Los Alamos. The early GAC was composed of several Los Alamos veterans who knew intimately what the weapons Laboratory's role had been within the Manhattan District. Moreover, the Committee itself was composed mainly of scientists. In contrast, among the five AEC Commissioners, Robert Bacher (also a former Los Alamos employee) was the only scientist. Thus, in handling technical issues the AEC relied on its scientific advisors. Moreover, as Richard Sylves has argued, the GAC was an elite group that not only represented Los Alamos in the 1940s and early 1950s, but the Committee devoted more time and effort to the AEC's specialized laboratory's than any other concern. The GAC tried to ensure that the laboratory's would thrive, and as part of this goal, tried to allow Los Alamos as much research freedom as practically possible, including theoretical work on hydrogen weapons.[248]

Dubridge. From 1947 through 1951 John Manley was the GAC's secretary; Draft minutes of Ninth Meeting of the General Advisory Committee to the Atomic Energy Commission, (hereafter GAC) April 23-25, 1948, 21, DOE Archives, RG 326, Box 11217, Folder 9, [declassified]; Quotation in Minutes of Tenth Meeting of the GAC, June 4-6, 1948, 20, DOE Archives, RG 326, Box 11217, Folder 9, [This Document is Secret-RD].
[247] Tenth GAC Meeting, 20, 26, 31.
[248] Sylves, The Nuclear Oracles, op. cit., 4, 18 114, 117.

Like the Soviet atomic weapon's impact on American nuclear weapons policy, President Truman's subsequent public announcement in 1950 and several of the GAC members' moral objections to continuing work on thermonuclear weapons development has been the subject of many historical and political analyses, and therefore I will not elaborate on these issues here. Truman's decision, however, did impact Los Alamos in that it encouraged the Laboratory to formalize plans to include thermonuclear principles tests in the 1951 series. In addition, in spring 1950 Los Alamos went from a five to six day work week.[249]

Bradbury had proposed to the AEC in the laboratory's planned program for 1950 that Los Alamos would indeed engage in "development of a super-bomb." The GAC responded to Los Alamos's program plan by recommending that the future thermonuclear initiation test be carried out, a study of the propagation of the detonation into pure deuterium be undertaken as well in order to provide a valid test of the feasibility of the Super weapon.[250]

Teller had wanted such a test for several years, and forwarded to Brien McMahon a letter that the Hungarian had written to Fermi at the end of war, explaining that any final doubt about the feasibility of the Super would be dispensed only with a test, and that the high chances of the Super's success warranted a large-scale program. As head of the JCAE, McMahon encouraged

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[249] For more on this see: Peter Galison and Barton Bernstein, "In Any Light," op. cit.

[250] Report to David E, Lilienthal from the GAC, February 1, 1950, in JCAE declassified General Correspondence Files, NARA.

a large-scale thermonuclear program, and saw to it that Los Alamos received supported in this effort.[251]

If the Laboratory wanted to embark on a stepped-up hydrogen weapons program it needed many more staff, Bradbury began to recruit vigorously in 1950. Teller and others tried to do the same on a more informal level. In February Teller contacted young Austrian physicist Frederic de Hoffman in Paris, urging him to come back to Los Alamos since there was "an enormous technical job ahead" with "strenuous months." The Laboratory had a difficult time attracting senior staff.[252]

Neither Oppenheimer nor Bethe wanted to work on thermonuclear weapons or least of all return to Los Alamos full-time. Perhaps because of Los Alamos's recruiting troubles, Teller wrote an article for the March 1950 issue of the Bulletin of the Atomic Scientists titled "Back to the Laboratories," which Richard Rhodes had called the equivalent of a want ad. In the same issue where Albert Einstein had a brief piece titled "Arms Can Bring no Security," Teller pleaded to his peers that "To the scientist . . . it should be clear that he can make a contribution by making the country strong."[253]

Teller wanted to recruit personnel specifically to sit on his new thermonuclear committee, better known as the "Family Committee," which Ulam had privately mocked to von Neumann. Bradbury asked Teller in

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[251] Letter to Senator Brien McMahon from Edward Teller, May 8, 1950, with attached letter from Teller to Fermi, October 31, 1945, in declassified JCAE General Correspondence Files, Box 58.
[252] Telegram to Frederic DeHoffman from Edward Teller, via State Department USAEC , February 15, 1950, LANL Archives, B-9 Files, 201, Drawer 22, [This Document is Secret-RD].

March to lead the "family organization," a code name for the variety of thermonuclear ideas that had been proposed over the last several years: "Daddy" equalled the large D-D Super -- the "daddy of them all." Scientists gave other animated names to the variety of thermonuclear-related proposals: "Sonny" referred to the Booster weapon; "Mother" was the cylindrical implosion idea; and, "Uncle" was another name for Little Edward. The Family Committee intended to prepare designs for the Greenhouse series and also to explore the array of thermonuclear possibilities. While Teller chaired the new committee, he would also report to Technical Associate Director Darol Froman.[254]

To the Family Committee Teller managed to recruit a few members from outside the Laboratory, including Konopinski, who had gone to the University of Indiana, and physicist John Archibald Wheeler from Princeton.[255]

While Los Alamos's leaders recruited more full-time staff to assist with thermonuclear research, Bradbury and other Laboratory leaders did not have a clear idea of what technical form a hydrogen bomb would take. The Super and Alarm Clock remained the only choices for Los Alamos to pursue but yet very little was known about either.

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[253] Rhodes, Dark Sun, 416-417; Edward Teller, "Back to the Laboratories," Bulletin of the Atomic Scientists, Vol. VI, no. 3, March 1950, 71-72.
[254] Memorandum from Bradbury to Teller, "Laboratory Matters," March 30, 1950, LANL Archives, B-9 Files, Drawer 22; Memorandum to the File from John Walker, "Status Report on the Thermonuclear Program, September 12, 1952, Appendix B, JCAE declassified General Subject Files, Box 59; Rhodes, Dark Sun, 416.
[255] Rhodes, Dark Sun, 416.

To get a better idea of just how little Los Alamos's leaders understood hydrogen device, the JCAE interviewed several individuals at the Laboratory to gain a first-hand assessment of the state thermonuclear program. Carson Mark summarized the state of theory and computations, explaining that the ENIAC did not calculate far enough for some of the problems, so Los Alamos had decided to build a "Maniac" which would do more complicated calculations. Building and programming the new machine presented an arduous task:

> While some of the mathematicians are figuring out how to build the machine, others are already at work figuring out the problems to give the machine. It takes longer to set up the problem than for the machine to work out the calculations with a 'memory' device of previous calculations.[256]

Mark continued explaining that Metropolis and his team were building the MANIAC (Mathematic and Numeric Integrator and Calculator) specifically to figure out whether or not the Super could be ignited and the deuterium would consequently burn. Given the recent hand calculations, Mark reported some of his colleagues as joking that "deuterium would make a good fire insulating material[!]" However, Mark and his colleagues had agreed that no one could be sure of the Super's fireproof qualities until a full electronic treatment of the Super problems was completed.[257]

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[256] JCAE interview of J. Carson Mark, May 12, 1950, JCAE declassified General Subject Files, Box 60, NARA.
[257] Ibid.

Greenhouse

Bradbury had proposed the formation of the Family Committee with the intention that the group would evaluate all the proposed thermonuclear designs and theories, including the Super, Alarm Clock, and Booster. With the approaching deadline for a 1951 test series, collectively code-named Greenhouse, the Family Committee directed most of its attention towards deciding what types of thermonuclear principles tests that would be a part of the agenda.[258]

The Family Committee picked up where the CWD had left off in the spring. The new group proposed a variety of designs for the upcoming tests, gradually ruling out those for which calculations predicted poor results. Finally, the Committee proposed testing a Booster weapon, and either a gun or implosion-type device to test thermonuclear initiation.[259]

Freezing of the designs for the Greenhouse tests depended in part on the IBM punched card calculations done in T Division. Over the spring and summer, Carson Mark regularly reported delays in the IBM work often due to a mere lack of people to run the problems. By fall 1950, with the HIPPO calculations starting to show results, the Committee decided to test the

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[258] LAMD-470, "Family Committee Minutes of Twenty-Seventh Meeting," November 15, 1950, LANL Report Library, [This Report is Secret-RD]; In addition to the thermonuclear experiments, two fission devices were tested in the Greenhouse series.
[259] ADWD-23-114, "Family Committee Minutes of the First Meeting," March 19, 1950, LANL Report Library, [This Report is Secret-RD].

special[260] implosion and Booster ideas, and drop the Little Edward gun design.[261]

The implosion and Booster represented, in the Committee's opinion, the most relevant to and hopeful for achieving thermonuclear initiation, although the latter device in practice only produced a larger "boosted" fission yield than an ordinary atomic device. Carson Mark recounted the Family Committee's reasoning behind testing the Booster in a secret interview in 1993:

> Starting in January 1950, with Truman's decision to go ahead with the H (hydrogen bomb) bomb . . . there was coming a test series, and some thermonuclear experience was a must in that test series. [This Booster] . . . got called Item . . . . [and] was earmarked for Greenhouse, and it was thermonuclear. It had the DT gas in the middle of a fissile explosive, where no energy could be transferred outside from it, but we used the fission to get the DT gas going, and that made fissions.[262]

The other thermonuclear-related test chosen by the Family Committee for the 1951 tests was very unlike the Booster. Mark, like Teller and several others, believed that if ignitable, D-T could in turn ignite deuterium, thus proving in principle that the Classical Super would work. Mark remembered that "The drawings [of the Super] did not really change from 1945 to 1951." To explore initiating the device, the Family Committee chose the implosion idea for one of the events in the Greenhouse series. Teller, Mark, and others

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[260] Here, as in footnote 220, at the request of the Los Alamos National Laboratory Classification Review group I use the generic term "special" to describe this configuration.
[261] "Family Committee Minutes of the Third Meeting," March 23, 1950, LANL Report Library, [This Report is Secret-RD]; ADWD-157, Family Committee Minutes of Fifteenth Meeting, [This Report is Secret-RD]; ADWD-197, "Family Committee Minutes of the Twenty-Fourth Meeting," October 5, 1950, [This Report is Secret-RD].
[262] LA-12656-H, 45.

anxiously waited to see if D-T could be placed outside of a fission initiator and radiation channeled to it, causing fusion reactions.[263]

The Thermonuclear Zoo

Even before the Laboratory began the Greenhouse tests, Los Alamos's leaders planned on conducting a full-scale thermonuclear bomb test by 1952. However, Los Alamos still had no reliable thermonuclear design to test since the Super's prospects looked so poor. With the hand calculations complete, and the ENIAC's confirmation of them, no one had any practical ideas for making the Super ignite or propagate. By this time, several proposed schemes for initiating the Super existed, but none would use a modest amount of tritium. Teller and John Wheeler labeled the spectrum of proposed initiator designs (including Gamow's "Cat's Tail," Little Edward, and others) "The Thermonuclear Zoo." Still not wishing to give up on the Super design, Teller and Wheeler appealed to the AEC and JCAE for support in hydrogen weapons research.[264]

"The research program required to come to a definite conclusion about the workability of any specific thermonuclear device is very great," Teller and Wheeler wrote to Brigadier General James McCormack, director of the AEC's Division of Military Applications. Teller and Wheeler argued that it was still impossible to say whether or not any thermonuclear weapon would prove feasible or economically possible, especially with the severe limits set by the

[263] Rhodes, Dark Sun, 457.
[264] E. Teller and J. Wheeler, "Thermonuclear Status Report: Part 1," August 1950, LAMD-443, LANL Report Library, [This Document is Secret-RD].

lack of theoretical manpower at Los Alamos. "There is some hope," the theoreticians continued, "of faster progress after completion of the high-speed computing machines now under construction," since theoretical analysis stood as the ultimate bottleneck to attainment of thermonuclear weapons. The bottleneck to theoretical analysis, in turn, was a "shortage of the right men." If not enough humans could be found to do the job, then perhaps the electronic computers scheduled for completion in 1951 and 1952 could prove both the ENIAC and Ulam and his colleagues wrong.[265]

Teller, Mark, and von Neumann joined the October-November 1950 GAC meeting, where Mark presented the hand computations and von Neumann the ENIAC's results indicating a dark future for the Super. Teller attended this meeting as well, and argued that the ENIAC calculations were too simplified to be accurate, and that future machine calculations that would include all of the thermodynamic and hydrodynamic effects within the Super might show more positive results. Fermi disagreed, believing that more detailed calculations would only confirm that the Super would fizzle. On a more practical level, Mark added that the machines with the ability to perform detailed calculations -- the Los Alamos MANIAC and its Princeton counterpart -- would not be ready until the next year. Subsequently Teller again took the floor and argued that the Super could be saved if only more theoretical work could be conducted, further criticizing that Los Alamos

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[265] E. Teller and J. Wheeler, "Thermonuclear Status Report: Part 1; Edward Teller and John Wheeler, "Scale of Theoretical Effort," August 1950, ADWD-184, in LAMD-444, Appendix 1-A, LANL Report Library.

lacked creative people as well as enough staff to perform the calculations. In the end, Teller proclaimed, only boldness, imagination, and determination would win.[266]

A few weeks later, Bradbury reported to a meeting of the AEC and its Military Liaison Committee that Los Alamos felt certain intuitively that a Super could be constructed, but the estimated cost would be daunting. In the last year the Laboratory had made Super research its first priority, with little to show for it. Now, with the upcoming Greenhouse tests, Bradbury thought it unwise to further pursue the Super.[267]

Many individuals at the Laboratory expressed their satisfaction with the Greenhouse "George" test because its results showed fusion reactions of 14 Mev neutrons when the special implosion device was detonated in March 1951 at Eniwetok; D-T could be ignited and perhaps used as a Super initiator. Still, the George test did not guarantee the propagation of deuterium in the particular manner that the Super design called for. To review the Greenhouse results, the Family Committee met for the last time in June. Teller proposed that the Super still should be investigated along with other designs such as the Alarm Clock, but above all encouraged his colleagues to pursue a new theory, postulated earlier that year by Ulam, Teller, and de Hoffman.[268]

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[266] Hewlett and Duncan, Atomic Shield, 530.
[267] Draft Memorandum of notes on the AEC-MLC-LASL Conference on Tuesday, November 14, 1950, DOE Archives, Germantown, MD, Box 4944, Folder 7.
[268] ADWD-271, "Family Committee Minues of Meeting 36," June 6, 1951, [This Report is Secret-RD].

Following the last Family Committee meeting, Bradbury reported to the AEC that prospects for the runaway Super had improved slightly since Mark and others had last reported to the Commission about it the previous fall. Over the winter the Laboratory had revised D-T cross sections upwards, and work on the inverse Compton effect indicated that this would not have such a devastating effect on the Super. Nevertheless, Los Alamos had come no closer to knowing whether or not the Super was possible or economically worthwhile. Bradbury continued:

> No significant progress can be expected prior to a full-scale MANIAC calculation. This calculation is being prepared by von Neumann and T Division personnel. . . . [and] In order to clarify the behavior of the inverse Compton effect, calculations have been proceeding at RAND under direction of de Hoffman.[269]

Last, Bradbury noted that Los Alamos would probably continue with a theoretical effort on the runaway Super "as is." Repeating Teller, Bradbury described the recent Greenhouse experiments as "successful" in both demonstrating that a thermonuclear reaction could be obtained from D-T, and a new promising technique of "radiation implosion" (from the special implosion device). Last, the newly proposed thermonuclear system appeared the most promising although least studied of all the designs. Therefore, T Division and John Wheeler would spend the remainder of 1951 examining the newly proposed design.[270]

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[269] Norris E. Bradbury to AEC, DIR-633, "Los Alamos Scientific Laboratory Thermonuclear Program," June 22, 1951, 1, DOE Archives, Box 1235, Folder 33, [This Document is Secret-RD].
[270] Ibid., 2.

As with the Classical Super, the new thermonuclear device had other names, such as the "Teller-Ulam configuration," and the "radiation implosion" bomb. Ulam independently discovered radiation implosion in the winter of 1951. Ulam recounted:

> In early January 1951, it occurred to me that one should employ an implosion of the main body of the device and thus obtain very high compression of the thermonuclear part, which then might be made to give a considerable energy yield. I mentioned this possibility, with a sketch of a scheme of how to construct it to Dr. Bradbury one morning. The next day I mentioned it to Edward, who by that time was convinced that the old scheme might not work.[271]

At first skeptical, Teller became excited by Ulam's proposal of a way to compress the thermonuclear fuel without destroying it first; this method Ulam named "hydrodynamic lensing." Rhodes claims that Teller, upon hearing Ulam's thoughts about compression, realized that x-rays from a fission trigger could be channeled and focused to compress and ignite a fusion fuel mass. In this manner, high temperatures could be avoided all together and the thermonuclear explosion achieved before the debris from the fission trigger destroyed the fusion part of the weapon. Ulam and Teller published a report on their ideas in March 1951.[272]

"Radiation implosion" itself was not novel in 1951. Teller had coined this term during one of the early Family Committee meetings to describe the process that went on in the special implosion device used to ignite a mass of D-T. In this scheme, by now already tested in the Greenhouse George event,

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[271] Letter to Glenn Seaborg from Ulam, March 16, 1962, LANL Archives, B-9 Files, 201, Drawer 22.

high explosives were used to implode nuclear materials, the resulting radiation was funneled to an adjacent mass of D-T, causing fusion. This idea for igniting a Super was based on a design von Neumann and Fuchs had supposedly proposed and patented in the 1946 Super conference. However, "radiation implosion" took on a different meaning after Ulam's 1951 discovery; Bradbury had perhaps described it best in his report to the AEC in June 1951, where he hailed this technique as a new means of "radiation engineering."[273]

Historically, the debate over Ulam's and Teller's specific contributions to the Teller-Ulam thermonuclear device has been examined exhaustively by Chuck Hansen. I will not explicate on this issue and instead will only summarize a few other discoveries related to the new thermonuclear device.[274]

In addition to some form of radiation implosion, Ulam also thought of the "staging" idea, where a fission primary would be used to set off a physically separated second (secondary) bomb. In the next few months after Ulam and Teller's discussion, Teller and his protégé de Hoffman presented a second crucial part of the new thermonuclear configuration. AEC historians

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[272]S. Ulam and E, Teller, LAMS-1225, On Heterocatalytic Detonations I: Hydrodynamic Lenses and Radiation Mirrors, March 9, 1951, LASL, [This Report is Secret-RD].
[273] DIR-633, "Los Alamos Scientific Laboratory Thermonuclear Program," June 22, 1951, DOE Archives, Box 1235, 635.12, LASL, Folder 33, [This Document is Secret-RD].
[274] LAMD-272, "Family Committee Minutes of the Third Meeting," March 23, 1950, 1, [This Report is Secret-RD]; LAMD-376, "Family Committee Minutes of Nineteenth Meeting," August 5, 1950, 2, [This Report is Secert-RD]; For a detailed discussion and evaluation of Ulam's and Teller's specific technical contributions to modern thermonuclear weapons technologies, see Chuck Hansen, ed., Swords III, 161-183; See also Herbert York, The Advisors, for yet another interpretation of Teller's and Ulam's discoveries.

Richard Hewlett and Francis Duncan claim that de Hoffman carried out all the mathematical work for this second part. In the final report where de Hoffman described this second part, along with Teller's and Ulam's collective ideas, de Hoffman signed only Teller's name. Teller and de Hoffman called this collection of new ideas the "Sausage," which at least appeared viable on paper. Even so, like the Super, the new system would have to be calculated and tested.[275]

Es geht um die Wurst

By September, Los Alamos began to tailor preparations for the 1952 test towards the new thermonuclear scheme. For this, Bradbury gave experimental physicist Marshall Holloway the responsibility for the entire thermonuclear research program, and for organizing a new committee, known as the "Theoretical Megaton Group."

Teller had desperately wanted control of the entire thermonuclear design and development program. Upon hearing that Holloway would lead this, Teller resigned from the Laboratory. On several previous occasions Teller had threatened to return to Chicago if the Bradbury and others did not take the Los Alamos thermonuclear program more seriously. However, Teller's Los Alamos colleagues had grown accustomed to taking such announcements in stride. Now Teller was serious. Oppenheimer had passed him over as head of T Division during the war, and now Bradbury failed to

[275] Hansen, US Nuclear Weapons, 49-50; Rhodes, Dark Sun, 470; Hewlett and Duncan, 541; E. Teller, LA-1230, The Sausage: A New Thermonculear System, April 4, 1951, LASL. [This Report is Secret-RD].

appoint him to another position he desperately wanted. Ulam recalled the tense situation in a letter to von Neumann:

> I see that you heard about a meeting we had in Los Alamos 10 days ago or so; it was one of the best! Edward indeed 'resigned,' - but I offer you odds of 25 to 1 that he will rescind it, persuaded by Freddy de Hoffman.[276]

Indeed, Teller did waver in his decision to leave Los Alamos, and changed his mind by late fall. By the end of the year, though, Teller departed with plans to build his own weapons laboratory. Teller was angry with Los Alamos, and Bradbury angry with Teller. Serber recalled that Bradbury became upset with Teller because he had mislead the director into believing that the Super calculations done in the 1940s were accurate. Serber felt that "Teller always cheated in his calculations . . . . He never made an honest estimate. [E]ssentially Bradbury threw Teller out" when the director "discovered the calculations for the Super had been misrepresented."[277]

Regardless of whether or not Teller cheated in the Super calculations, computations for the Teller-Ulam device would require as much difficult mathematical analyses as the former design. Von Neumann continued as a consultant to the Laboratory offering his assistance with theoretical work for the 1952 test, and with the farming out of thermonuclear calculations to a wide array of computing centers now becoming available. The TMG acted as a focal point for coordinating this work. Chaired by Carson Mark, the TMG

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[276] Memorandum for the record from Kenneth Mansfield, "Los Alamos opinions of Dr. Edward Teller," August 29, 1951, JCAE General Subject Files, NARA, [declassified]; Rhodes, Dark Sun, 471-472; Letter from Ulam to von Neumann, September 26, 1951, JVN Papers, Box 7, Folder 7, LOC.

began meeting in October 1951, and in a little over a year designed and tested a successful thermonuclear device. Mark described this period as coinciding with the long-awaited breakup of the "log-jam in computing resources," allowing is colleagues to complete the calculations by the fall of 1952.[278]

Besides von Neumann, Wheeler acted to expedite computations for the new thermonuclear device in May 1951. Wheeler had set up his own group back at Princeton University to calculate portions of the new thermonuclear configuration, arguing that Los Alamos still suffered from a lack of theoretical manpower. Wheeler code-named this secret project "Matterhorn-B" (B for bomb), which he intended to carry out on von Neumann's Princeton computer. The Princeton machine was still not ready, and Wheeler's group instead ran a series of calculations on the SEAC, using a distinct new series of codes to compute steady-state burning in the Sausage. By 1952, the two-dimensional hydrodynamic problems began to indicate the feasibility of the burning of deuterium in the Sausage. In September, Wheeler reported to Los Alamos that the Sausage scheme would probably burn very well.[279]

Besides Matterhorn, Los Alamos had to farm computational work calculations for the 1952 test out to other computing centers in part because T

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[277] Author interview with Robert Serber, November 26, 1996.
[278] Mark, Short Account, 11-12.
[279] John Wheeler, "Statement to FBI concerning Project Matterhorn," March 3, 1953, LANL Archives, B-9 Files, 201, Drawer 22, [This Document is Secret-RD]; Minutes of Theoretical Megaton Group meeting No.1, October 5, 1951, ADWD-3-18, LANL Archives, B-9 Files, Drawer 74, [This Document is Secret-RD]; Minutes of Theoretical Megaton Group Meeting No. 38, September 17, 1952, TM-77, LANL Archives, B-9 Files, Drawer 74, [This Document is Secret-

Division's own machine remained under construction for several years. Metropolis and engineer Jim Richardson and their technical team did not complete the Los Alamos MANIAC until the spring of 1952. Several T Division members used MANIAC immediately for radiation implosion calculations for the Sausage, hinting at success for the upcoming Ivy Mike test in November 1952, which yielded Teller's fantasy in the form of a 10.4 megaton explosion, and vaporized the Pacific island of Elugelab. Still angry with Los Alamos, Teller did not attend the event.[280]

Computing in Nuclear Weapons Science

Historian Peter Galison has shown how von Neumann compared the huge gap between "man" and computer hours needed to solve the Super problem.[281] In 1949 von Neumann reported to Ulam:

> . . . I tried for a while . . . [to finish] . . . a preliminary report on S [the Super]. I finished the flow diagram . . . . It now looks like a 24-30 hour problem for our future machine."[282]

"Our future machine" was, of course, the IAS computer. Von Neumann intended for this computer to provide a fast means of solving the Super problem, that otherwise would require an estimated 4 years to solve with hand computers.[283]

RD]; PM-B-37, "Project Matterhorn Final Report," August 31, 1953, 3, 30, [This Report is Secret-RD].

[280] Ulam, Adventures , 225.

[281] Peter Galison, "Computer Simulations and the Trading Zone," in The Disunity of Science: Context, Boundaries, Power, eds. P. Galison and D. Stump, (Stanford: Stanford University Press, 1996), 118-157.

[282] Letter from JVN to Ulam, May 23, 1949, JVN Papers, LOC, Box 7, Folder 7.

[283] Letter from JVN to Ulam, March 28, 1949, JVN Papers, LOC, Box 7, Folder 7.

Von Neumann, like Mark, Metropolis, the Frankels, Wheeler, and others, wanted to pursue a thermonuclear weapon on a theoretical level at least as much as Teller. While Teller was the most vocal among scientists encouraging the hydrogen project to continue in the postwar, he actually did few of the complex calculations for the Super. As Galison points out, Ulam and von Neumann both kept low public profiles in the debate over whether or not the United States should build a hydrogen bomb. Neither Ulam nor von Neumann opposed building this weapon.[284]

Von Neumann in particular is representative of three distinct human features of the early postwar thermonuclear project: First, nuclear weapons scientists' gradual recognition of computing as a critical problem to thoroughly understanding how -- and, more importantly, if -- the Super configuration would work; second, scientists' quest for computational technology not only as a means of conducting difficult calculations that could not be solved analytically, but also for machines to make up for a lack of humans to do this work; third, and last, scientists created personal networks extending between Los Alamos and universities, corporate and military computing centers, and other government agencies.

As Los Alamos evolved as an AEC facility, its human component stood out as most important. Los Alamos's staff and consultants took the first initiatives for exploring hydrogen weapons in the forms of farming out calculations to distant computing centers and initiating construction of its

[284] Galison, "Computer Simulations," 134.

own machine when the AEC leadership had not yet caught on to the notion of "scientific computing." Teller, Mark, Foster and Cerda Evans, John and Klari von Neumann, and others closely associated with Los Alamos recognized early on that computing could be used as a rapid and labor-saving means of simulating nuclear weapons.

Given Los Alamos's remote location, nuclear weapons scientists not only developed networks spanning large physical distances, but often fell back on long-standing professional relationships to find ways to hasten weapons calculations. Teller in particular took advantage of his professional ties with von Neumann, Mayer, Wheeler, and others to see that calculations for the Super problem received treatment as soon as the necessary technology became available. While Dana Mitchell's wartime connection to Eckert's laboratory signaled the beginning of Los Alamos's ties to large computing centers, von Neumann by far was the most influential in arranging for his colleagues to use the new electronic computers in such places as Philadelphia and New York, and in forging a permanent relationship between nuclear weapons science and computing.

The Laboratory's staff and consultants, not the MED, introduced Los Alamos to computing. Likewise, throughout the 1940s every initiative for hydrogen weapons theory and research came from Los Alamos, but not from the AEC. The General Advisory Committee's regarding the Super as an intellectual attractor for scientists hints of the AEC's initial uncertainties about establishing specific technical directives for its nuclear laboratories.

The AEC's struggle to establish a specific technical mission contrasts sharply with the wartime MED system and its clear directive to build an atomic bomb. Hoddeson and her co-authors have described the wartime fission program as directed from above by military objectives: Scientists were bound by strict deadlines and functioned in a mission-oriented mode. This sense of mission disappeared after the war, both at Los Alamos and in the larger system. The immediate postwar period lost the characteristics of immediacy and strong goal-orientation. Ironically, what was *not* explicitly specified by the AEC allowed Los Alamos to continue focusing on the Super configuration in the latter half of the 1940s. In other words, the AEC's failure to present a focused technical agenda for the laboratory to follow, combined with the GAC's subtle approval of thermonuclear research, permitted Los Alamos to pursue a small but steady theoretical program centered around a very sketchy hydrogen weapon configuration, and to seek labor-saving means of solving this problem.

In Networks of Power, Hughes notes that inventors and engineers in the emerging electric lighting and power industries of the late nineteenth century defined and sought solutions to critical problems in response to inadequacies in technological systems. Most inventions, Hughes asserts, result from efforts to solve critical problems. In Los Alamos's efforts towards hydrogen weapons development, Teller, Richtmyer, von Neumann, and Wheeler saw computing as an inadequacy at the weapons laboratory. Thus, von Neumann's rationale behind building "our future machine" at

Princeton was in part, motivated by hope that the Super problem could once-and-for-all be solved.[285]

Electronic computing evolved rapidly in the 1940s beginning with ENIAC's construction. To weapons scientists, new computers represented a technological short-cut to a problem that would otherwise involve years of human labor. Although the GAC regarded the Super as little more than an "interesting problem" for Los Alamos to pursue after the war, the Super project provided a motive for computer building at Princeton and at Los Alamos itself. Scientists transformed thermonuclear research from an intellectual pursuit to a tangible goal, however, when hydrogen weapons work became politicized in 1949. Consequently, for the weapons laboratory and particularly for T Division, the inability to compute a hydrogen device changed from a latent problem to a critical one.

As Galison indicated, in the postwar period the design of the first hydrogen bomb was the most complex physical problem ever carried out in the history of science. Solving this problem meant that not only the technological hardware to assist in this work required development to the point that it could handle such a complex problem, but new methods of utilizing the computing machines for weapons simulation had to be thought up as well. Von Neumann envisioned the Monte Carlo method simultaneously with powerful computers of the future, where the technology

[285] Hughes, Networks, 80.

would allow mechanized versions of the mathematical technique to proceed rapidly.[286]

Computers themselves bore not only on the pace of, but on the technical outcome of the early American thermonuclear weapons project. Because Los Alamos had been concentrating mostly on the Super configuration from the war through 1950, this theory still dominated weapons scientists' thoughts even after the Teller-Ulam device had been conceptualized. Moreover, because of Los Alamos's long-standing focus on the Super, the Laboratory knew more about D-D cross sections and the physics of deuterium than properties of other materials, and conservatively decided to continue experimenting with igniting D in the Mike test. Finally, the electronic computers available by the time Los Alamos scheduled a full-scale thermonuclear experiment allowed, for the first time, complex calculations of simulated burning deuterium to be carried out in a little over a year's time. Thus, the awkward and undeliverable Mike device became possible to calculate. Mike signified a vast departure from the Super in terms of the means of ignition of a thermonuclear weapon, but the 1952 test still involved the burning of a huge vessel of liquid deuterium.

While computing became a critical problem for Los Alamos, it was not so much the AEC's direct concern. Thus, the nascent concept of "scientific computing" was rapidly developed at the weapons laboratory where fundamental changes evolved in the way that nuclear weapons science was

[286] Galison, "Computer Simulations," 119.

conducted. Computers did not constitute the only important labor-saving components of the postwar system of nuclear weapons research, but they also allowed for much theoretical nuclear research based on estimation.

Nuclear weapons science was (and is) not an exact science; Chuck Hansen notes that there are still not truly a set of first principles, or completely known laws and equations of weapons physics. During the war Nelson, Frankel and Serber discovered that neutron diffusion problems related to a gun-bomb were unsolvable; fission problems only became more complicated as Los Alamos shifted its focus towards a fission implosion device, and in subsequent years, a thermonuclear weapon. Not accidentally, Los Alamos utilized business machines and later, electronic computers for approximating simulations of nuclear processes, especially in very difficult calculations such as the Super problem.[287]

Although calculating the Super's feasibility entailed understanding many different phenomena, a crucial part of the Super problem was the amount of tritium that the device would need to ignite. Tritium itself emerged as a critical problem that vexed the American thermonuclear program, constituting another bottleneck to the Super design especially. I examine this bottleneck in the following chapter.

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[287] Hansen, US Nuclear Weapons, 11.