Chapter Five

Fission before Fusion and the Rarity of Atoms

Although Teller, his Los Alamos colleagues, and the GAC recognized the two outstanding obvious technical obstacles to a thermonuclear weapon -- computing and tritium -- by the time of the first Russian fission test, other critical bottlenecks presented themselves from the end of the war through the time of the discovery of the Teller-Ulam design.

Several critical problems for the thermonuclear weapons project simply were not as blatant as the computing and nuclear fuel problems, and some even originated outside of the AEC system, in the American military complex. Other problems grew out of the early MED system only becoming apparent after the AEC's firm establishment. The temporary nature of the Manhattan District itself inhibited its inheritors from embarking on an ambitious fusion weapon project, or even initially, much expansion of the fission program.

This temporary character of the MED system became apparent in several ways, one of which included Oppenheimer's own ambiguous feelings towards the future of Los Alamos. Occasionally he expressed doubts as to the Laboratory's value in peacetime, while at other times he showed his support for continued weapons research in the postwar period. The loss of mission at the Laboratory after the end of the war certainly reflected the MED's temporary status. Hoddeson and her co-authors in Critical Assembly verify this. Comparing wartime Los Alamos with the postwar period, Hoddeson attributes a strong mission orientation to the wartime fission project. In the postwar era, the sense of mission almost entirely had vanished.[417]

If Oppenheimer expressed ambiguity about the future of atomic energy, several other system builders worked hard in the postwar era to assure that a new mission would be created for the Laboratory and also the for the larger system. Of the four scientific advisors to the Postwar Planning Committee, Lawrence had the greatest enthusiasm about building and improving nuclear weapons. As already mentioned, Bradbury struggled to assure the Laboratory a place in peacetime, while Groves tried to establish a permanent postwar nuclear weapons research and development complex that would succeed the MED. Other characters emerged as system builders as well. Kenneth Nichols, like Groves, stood out foremost among American military leaders pushing for an expanded nuclear weapons program, simultaneously trying to increase the American Armed Forces' influence on the fission program.[418]

The American military had little interest in and even little knowledge of the Super or Alarm Clock theories prior to 1949. Moreover, the aircraft employed by the military in the 1940s and early 1950s constituted yet another bottleneck to the fusion weapons project, which, like computing and tritium, changed from a latent to critical problem when hydrogen weapons became a political issue.

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417 Hoddeson, et al., Critical Assembly, 5, 389, 390-400.
418 Barton J. Bernstein, "Four Physicists and the Bomb: The Early Years, 1945-1950," Historical Studies in the Physical and Biological Sciences, Vol 18, Part 2, 1988.

Finally, human labor was another critical problem for H-bomb research and development that remained latent until 1949. Bradbury and the GAC envisioned calculating, designing, and testing a fusion weapon at least as equally labor-intensive as the wartime fission program. Yet with the loss of the wartime mission in 1945, came the loss of the Los Alamos scientific labor force. Whereas historian Daniel Kevles has argued that the MED "absorbed physicists like a sponge," after the war the same physicists, and numerous other scientists and technical staff, could not leave the system fast enough.

Primary Numbers

A staff shortage at the New Mexico laboratory affected not only the fusion but the fission weapons program as well. As many departed the isolated and secret confines of the Laboratory, Bradbury directed the weapons program towards a narrow trajectory of primarily advancing the wartime designs and, secondarily, exploring new fission configurations. Besides Lawrence, Arthur Compton, and a handful of other scientists, Bradbury was one of the few civilian participants from the wartime fission program with a strong determination to continue this work. Aside from the Pentagon and General Groves, Hansen asserts that little impetus existed to continue the U.S. nuclear weapons program right after the war, or to maintain the physical plants and technical staffs necessary to keep the program functioning.[419]

Hansen has also observed that at the end of the war America's atomic strength "would not be gauged just by the number of weapons in the nuclear

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[419] Hanson, Swords, II-8.

stockpile," but also by the yields that weapons could produce, and their adaptability to easy delivery to a target. This characterization further highlights how the nuclear weapons complex was a technological system. Any new and improved weapons that Los Alamos would develop would have to be delivered by existing aircraft, which in the immediate postwar meant the B-29. Thus, to make weapons appealing to the military and to insure an "atomic necessity" after the war, Los Alamos could only follow through to completion a limited number of styles of weapons.[420]

Delivery of atomic weapons both during and after the war remained limited by more than just the Boeing B-29 Superfortress, the only aircraft capable of this. The military introduced the B-29 for medium range missions in 1944; B-29's designated to carry fission weapons were structurally modified with an H-frame and hook to accommodate the 4-ton bombs, and wiring in the bomb bay for weapon fuses and monitoring equipment. Only 46 of these specially modified B-29s, code-named SILVERPLATE, existed at the end of the war, and according to David Alan Rosenberg, only 23 remained operational at the end of 1946. Not until 1947 did the Air Force begin deploying the B-50, essentially an advanced B-29. A year later Boeing delivered the first B-36 intercontinental bomber, but the Strategic Air Command (SAC) did not fully deploy these until 1951.[421]

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[420] Ibid., II-9.

[421] David Alan Rosenberg, "U.S. Nuclear Stockpile, 1945 to 1950," The Bulletin of the Atomic Scientists, May 1982, 25-30; Stephen M. Millett, "The Capabilities of the American Nuclear Deterrent, 1945-1950," Aerospace Historian, Spring, March 1980, 27-32.

Rhodes describes American military leaders' attempts at planning a postwar agenda for adopting nuclear technology as one of "cross-wired" confusion. While, on one hand, Groves desired a long-term system of nuclear production be set firmly in place, other high-ranking military leaders expressed less enthusiastic views towards nuclear weapons. General Carl A. Spaatz had commanded the Strategic Air Forces in the Pacific towards the end of the war, and in September 1945 headed of a board that ascertained the effect that atomic weapons would have on the postwar Army Air Forces. The committee, known as the "Spaatz Board," recommended in October 1945 that the U.S. Army Air Forces act cautiously in adopting the new fission weapons technology. Spaatz, along with U.S. Air Forces Major General Lauris Norstad, and Air Forces Lieutenant General Hoyt Vandenberg implied in their recommendations that atomic weapons would not have an overwhelming impact on the Air Force's "size, organization, [or] composition." Thus, Air Force plans for at least the next few years did not include drastic reorientation of its structure, aircraft, and personnel towards nuclear weapons.[422]

The Committee's apparent failure to embrace unquestioningly and immediately the new weapons technology was actually well founded, as the group blamed the Air Force's scant understanding of fission weapons technology on the MED's rigid secrecy policies. The certainty of fission weapons' future, too, appeared unstable to the Spaatz Board as it cited the

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[422] Rhodes, Dark Sun, 224, 226; John T. Greenwood, "The Atomic Bomb - Early Air Force Thinking and the Strategic Air Force, August 1945 - March 1946," Aerospace Historian, Fall, September 1987, 158-166; Quote in Greenwood, 160.

enormous cost of fissionable materials production and bomb development in general. In other words, the Board expected the nuclear weapons production system to remain modest in the postwar period, if it survived in the first place. Furthermore, Spaatz and his colleagues noted, only the few SILVERPLATE B-29s modified in wartime could deliver fission weapons at that time or within the next few years because significant size and weight reduction in weapons could not be foreseen. Coming to the conclusion that "The atomic bomb does not at this time warrant a material change in our present conception of the employment, size, organization, and composition of the postwar Air Force," the Spaatz Board placed nuclear weapons in an esoteric category.[423]

The Spaatz Board's view of fission bombs as "special weapons" of high cost and complexity would influence military thinking about nuclear weapons in the following years. They would be referred to explicitly as "special weapons" when Secretary of War Patterson and Secretary of the Navy James Forrestal, and Groves, established a joint Army, Navy, and Air Corps unit to organize military participation in the postwar nuclear weapons system and to develop military uses for atomic energy. Groves initially headed this organization, known as the Armed Forces Special Weapons Project (AFSWP), at its startup in early 1947. Groves saw this unit as important to establishing some military influence upon the realm of and control over atomic weapons.

[423] Greenwood, 160; Quote in Greenwood, 161.

Whereas the MLC (on which Groves also sat in 1947) interacted with the AEC on a policy level, the AFSWP did so more on an operational level.[424]

By early 1948 Kenneth Nichols replaced Groves as head of the AFSWP. Following in Groves' footsteps, Nichols not only sought military custody of atomic weapons in the postwar period, but expressed concern over the seemingly small numbers of weapons in existence following Hiroshima and Nagasaki. The U.S.'s nuclear weapon stockpile itself became a source of tension for the AEC as well as Groves, Nichols, and other military leaders, partly because the actual number of weapons present in the postwar nuclear stockpile was unclear and even subject to interpretation depending on how officials defined the term "stockpile."

Currently, the term "stockpile" refers to weapons immediately available for use in war. In the early years of atomic energy, however, Rosenberg has speculated that stockpile totals may have included all nuclear cores and non-nuclear assemblies, including conventional explosives, casings, fuses and electrical systems, for example. In mid-1946, the stockpile numbers in mid-1946 remained small. According to Rosenberg, "only nine implosion nuclear components and an equal number of Mark III 'Fat Man' implosion assemblies" existed. If this number included test weapons, then two of these were used in the summer Crossroads series. A year later, only thirteen implosion cores sat in the U.S. stockpile along with twenty-nine

[424] Greenwood, 160; Major General K.D. Nichols, USA(Ret.), The Road to Trinity, (New York: William Morrow and Company, 1987), 253; Hewlett and Duncan, Atomic Shield, 131.

mechanical Mark III implosion assemblies. In 1948 however, the number of implosion cores jumped to fifty and Mark III assemblies to fifty-three.[425]

Rhodes cites Jacob Wechsler describing the postwar stockpile as unassembled "piles of pieces," as opposed to weapons. Likewise, the AEC's Commissioners made a similar observation in January 1947 when they visited Los Alamos to see the state of the Laboratory. Bacher apparently became shocked by the lack of weapons and no inventory of those available. Lilienthal recalled a similar impression, remarking that the visit was one of the "saddest days of my life," when he came away with the impression that the Laboratory possessed only one or two operational bombs.[426]

Atomic Scarcity or Secrecy of the Postwar Stockpile

The stockpile numbers rose by the time of the Sandstone tests. Hansen notes that one of the most important results of Sandstone was the abolition of the "so-called 'doctrine of scarcity' that had dictated U.S. Air Force strategic war planning." The doctrine of scarcity:

> [A]ssumed that because of a shortage of raw materials and processing capability, and because relatively large quantities of fissionable material were required at great cost for each weapon, the U.S. would continue to have for quite some time -- possibly for as long as the next 10 to 20 years -- only a very limited supply of atomic bombs."[427]

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[425] Kenneth Nichols, "The Period of Atomic Scarcity," Sound Recording of Speech by Kenneth Nichols to Los Alamos National Laboratory, October 28, 1983; Quote from Rosenberg, "U.S. Nuclear Stockpile," op. cit., 26.
[426] Rhodes, Dark Sun, 282-284; Quotation from Rhodes, Dark Sun, 283.
[427] Hanson, Swords, II-26.

Still influential after the war but often wrong in predicting the future of atomic weapons, Groves even predicted in 1946 that the stockpile would continue to consist exclusively of MK III's up through 1950.[428]

Groves' successors believed him. Nichols labeled the entire period from 1945 through 1953 one of "atomic scarcity," claiming that the military thought that about ten to fifteen atomic bombs might have been available by the end of 1945. Nichols emphasis on the rarity of fission devices was honest at least up until 1948, given the difficulty in turning the MED's facilities over to the AEC, the decay of the Hanford reactors, and Los Alamos needing to rebuild. Yet, "scarcity" is a relative description of the stockpile, and Nichols may have truly believed that a stockpile number in the double or even triple digits was inadequate; he once expressed to then General Eisenhower that the stockpile should be in the thousands.[429]

Nichols likely meant the "scarcity" of completely assembled weapons, because if all the existing nuclear weapon components were counted, by the end of the 1947 the numbers constituted a significant stockpile. Yet Rhodes, drawing upon Hansen's research, cites a larger number of nuclear components available at this time than does Rosenberg: According to Rhodes, by the end of 1947 the Laboratory had fifty Mark series cores on hand.[430]

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[428] Ibid.
[429] Nichols Speech, op. cit.
[430] Rhodes, Dark Sun, 307.

Numerous other components were available also, including initiators and enough non-nuclear components to make over one hundred Fat Man bombs. At this time, Uranium Little Boy bombs remained part of the stockpile but comprised a very small part of it. In an emergency, then, Rosenberg states that the U.S. had fifty fission weapons on hand, although they would require assembly and delivery -- serious problems considering that it would take about a month to assemble even twenty bombs, and the number of available SILVERPLATE B-29 stood at thirty five. Furthermore, only twenty Air Force crews had been trained to handle atomic weapons.[431]

The Armed Forces' early policies towards fission weapons originated ultimately, above the AFSWP or MLC, at the level of the JCS, Strategic Air Command (SAC), and National Security Council (NSC). Although the nuclear war plans of these organizations are beyond the scope of this dissertation, their policies certainly influenced the AEC and subsequently Los Alamos in the direction of weapons development, towards which the Laboratory found itself ahead of schedule in fall 1948.[432]

Groves underestimated the progress Los Alamos would make in changing its weapons designs as the Laboratory began embarking on Bradbury's program of improvements in the Mark III and preliminary development of its successors. The biggest jump in the number of stockpile

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[431] Rhodes, Dark Sun, 307; Rosenberg, "U.S. Nuclear Stockpile," op. cit., 26.
[432] For more on the American military and planning for nuclear war, see: David Alan Rosenberg, "A Smoking Radiating Ruin at the End of Two Hours," International Security, Winter 1981/82, (Vol 6. No. 3), 3-38; David Alan Rosenberg, "The Origins of Overkill: Nuclear Weapons and American Strategy, 1945-1960," International Security, Spring 1983 (Vol. 7, No. 4), 3-71; Gregg Herken, The Winning Weapon, op. cit.

weapons components occurred after the Sandstone tests in spring 1948. "Atomic scarcity" then, had been eliminated by 1948. Hydrogen weapons, on the other hand, remained the scarcest weapons of all because of Los Alamos's emphasis on fission and eliminating any perceived lack of atomic weapons.[433]

Bradbury's program for Los Alamos was not hard and fixed, and remained open at least to suggestions from the MLC for specific kinds of weapons. Still, the Laboratory retained for the most part a conservative program that concentrated mostly on improvements to existing fission configurations. Late in 1948 Marshall Holloway made a summary of the state of the fission program, noting that the few Little Boy models currently in the stockpile had predicted yields of about 15,000 tons of TNT. The Nagasaki-type Fat Man Mark III, also in the stockpile, had a wider range of yield potential depending on the kind of fissile core and tamper assembly, or "pit," used in it. A type "A" pit, for example, would produce a lower yield than a type "B" pit. The first Mark IV to employ a type "C' pit, Holloway predicted would be placed in the stockpile in 1949.[434]

Although Los Alamos did not design the Little Boy, and Marks III and IV at the request of the military (the Mark IV had been proposed during the war at Los Alamos), the Laboratory was already speculating on other designs for specific tactical purposes, including a "light weapon," similar in shape and size to the Mark IV, but with a much larger potential yield. The Laboratory

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[433] Hansen, Swords, II-26.
[434] LAB-W-22, Memorandum from Marshall Holloway to R.W. Henderson, Technical Associate Director of Sandia, October 22, 1948, [This Document is Secret-RD].

also began to investigate a "very light" fission device, essentially a smaller version of the "light" weapon. In addition to these designs, the Los Alamos's staff considered a water-penetrating weapon affectionately named "Elsie," intended to weigh only around 3000 lbs. and give a low yield relative to other fission devices.[435]

Operation Sandstone would have taken place in 1947 but it was delayed until the next year, according to former fission designer Robert Osborne, because of Los Alamos's lack of staff. Not until 1948 did the Laboratory conduct the Sandstone series in April and May, to test three different types of new pits. In part, Sandstone represented Los Alamos's response to problems in other parts of the system, namely, the Laboratory took into consideration Hanford's limited Pu production capacity (as the piles deteriorated) since the purpose of these tests aimed to show that higher yields could be obtained from smaller amounts of fissionable material thus conserving Pu^{239} and U^{235}.[436]

Carson Mark claims that the individual tests, X-Ray, Yoke, and Zebra, led to immediate plans to change the military stockpile and even in the long term altered the characterization of stockpile production into an "assembly line" method, where the [Mark IV] would ". . . contain standard components that could be made by mass-production methods and could be put together by assembly-line techniques." Zebra in particular purported to ease the burdens on the materials production end of the AEC system because it was the first

[435] Ibid.

U^{235} implosion weapon, intended to make use of Oak Ridge's supply of weapons grade uranium, that was larger than Hanford's supply of Plutonium. Thus, the combined trends towards "mass production" of weapons and efficient use of nuclear materials would allow for a marked increase in the stockpile.[437]

Besides its implications for the fission program, preparations for the Sandstone series became time consuming for the understaffed laboratory, and themselves diverted attention away from hydrogen weapons work in 1947 and early 1948. For T Division, this meant performing countless hand and machine calculations of efficiency. As in the war, the problems for the Sandstone series completely occupied all of Los Alamos's IBM punched cards thus leaving no available time for any sort of thermonuclear-related problems.

The importance of punched-card technology to calculating improved yields and efficiency of atomic weapons grew in the postwar period, in part due to the lack of personnel available to make hand calculations. In addition, IBM improved its business machines. By 1948 Los Alamos had five new 602 calculators; but even as these machines arrived, T Division expressed an interest in IBM's new 604 -- an electronic calculating punch.[438]

If the IBM machines saved labor, the actual methods of use of the machines changed very slowly. Osborne makes a unique measurement of the

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[436] Osborne, "Theoretical Design," 4; Rhodes, Dark Sun, 320.
[437] "Bradbury's Colleagues Remember His Era," Los Alamos Science 7, Winter/Spring 1983, 29-53.

pace of the postwar weapons program circa 1945-1953 in his history of theoretical fission weapons design:

> For this period the best measure of the progress in implosion weapon design is the number of IBM problems completed each year. The capability of the IBM machines and the method of running problems remained essentially unchanged from April 1944 until the Model II CPC's were operational in May 1952.[439]

T-Division's work progressed very slowly right after the war, and according to Osborne, the IBM problems were the chief reason why. Design calculations capabilities developed during the war constituted: "Numerical solution of hydrodynamic equations during the implosion by IBM machines together with human calculation of discontinuities (i.e., the initial shock front)"; Serber-Wilson for neutronics; and, the Bethe-Feynman formula for explosion calculations.[440]

T Division's staff completed only one IBM problem in 1946, and two more in 1947. Even with the postwar labor shortage, T Division managed to run enough calculations in preparation for the Sandstone tests because many of the problems already completed during the war involved hollow pit designs. Although the Trinity test and Nagasaki bomb were composed of the more conservative but more reliable solid non-levitated Christy pits, scientists had already done some preparatory work for Sandstone by July.[441]

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[438] LAMS-646, T-Division Progress Report: 20 September 1947-20 October 1947, November 11, 1947, LASL, [This Report is Secret RD].
[439] Osborne, "Theoretical Design," 5.
[440] Ibid., 4-5.
[441] Osborne, "Theoretical Design," 4; LAMS-660, T-Division Progress Report: 20 October 1947-20 November, 1947, December 11, 1947, LASL. [This Report is Secret-RD].

Two years later, then, T Division members calculated, for example, "Problem X," a hydrodynamic calculation of the device planned for testing in the "X-Ray" shot at Sandstone. With the stockpile numbers growing faster and Bradbury's plan for improving fission weapons bearing some results, Los Alamos began to have a more firm mission. Also by this time, a new mission for Los Alamos finally became recognizable to others than Bradbury. The GAC announced in their February 1947 meeting that "the making of atomic weapons is something to which we are now committed."[442]

Yet commitment seemed the furthest thing the Committee had in mind when it came to the Super. Further reading into the early GAC's initial stance on the Super project indicate the group's feelings towards this project as one of technological bait for scientific personnel. In the GAC's second meeting Oppenheimer summarized the Committee's thoughts on the H-bomb, noting that it might be wise not to "have the super bomb pushed at Los Alamos," since perhaps instead "a really brave reactor program at Los Alamos would provide the new blood and incentive which would be successful."[443]

The GAC considered the Super a potential aid to strengthening the fission program, with which the military started to become at least more active, if not specific, in terms of requesting certain types of weapons for development. Groves, for example, told the GAC in 1947 that the military had been interested in a concrete-penetrating weapon for a long type since the

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[442] LAMS-673, T-Division Progress Report: 20 November, 1947-20 December 1947, January 8, 1948, LASL. [This Report is Secret-RD]; Draft Minutes of the GAC, Second Meeting, February 2-3, 1947, 5, US DOE Archives, Box 337, [declassified version].

"need may very well arise for such a weapon to strike at an extremely important underground installation."[444]

Likewise, from the military came other nonspecific requests for the AEC and Los Alamos to look into lightweight and subsurface fission devices. But they had yet to hear from the Armed Forces as to their establishing a requirement for a thermonuclear weapon. The military had at best scant familiarity with fusion weapons. General McCormack, meeting with the GAC in April 1948, expressed his confusion as to how to regard the Booster that Los Alamos was considering for inclusion in the 1951 tests. According to Oppenheimer, the GAC did not know the military evaluation of "need" for a thermonuclear weapon, and if they did, the Committee would be in a much better position to consider the future of Los Alamos in regard to weapon development. [445]

Military Need for an H-bomb?

Although in principal the AEC's science advisors expressed no objections to thermonuclear weapons in their early meetings, the GAC relegated the Super's military application as "remote." Although by 1948 the Committee did encourage Los Alamos to pursue the Booster for a test, since enough tritium would available for this for a test within two or three years,

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[443] GAC Minutes, February 2-3, 1947, 6.
[444] GAC Minutes, February 2-3, 1947; Draft Minutes, Sixth Meeting of the GAC, October 3-5, 1947, 11, US DOE Archives, Box 337, Folder (1-3-47), [declassified version].
[445] AEC 99, <u>Atomic Energy Commission Weapons Program of the Los Alamos Laboratory</u>, May 14, 1948, Appendix "A", Box 4944 (635.12), Folder 7 LASL, [declassified]; Draft Minutes, Ninth Meeting of the GAC to the Atomic Energy Commission, April 23-25, 1948, Box 11217, Folder 9, US DOE Archives [declassified].

the Armed Forces apparently saw little use for the Super throughout the 1940s.[446]

The military did not make outright, specific demands for fusion weapons for some time, even after the Soviet fission test. Moreover, John Manley questioned how the military could employ H-bombs, because they had not given the notion of the use of these weapons much consideration. As late as 1952 John Walker and Walter Hamilton reported that no H-bomb requirements had been submitted to the MLC, at least according to Committee member General Herbert Loper.[447]

Rosenberg asserts that the American military played a significant role in the "Hydrogen Bomb Decision" of 1950, in terms of the Armed Forces' multiple emergency war plans established in the latter 1940s, such as the "Halfmoon" operation, which included an air-offensive numerous atomic devices intended for Soviet cities. Because the American military had clearly established elaborate war plans that included atomic weapons by the time of the first Soviet fission test, and due to other causes, Truman was convinced that nuclear weapons would be "the centerpiece of future American strategic planning." The Spaatz Board seemed to have had little impact on war planning, since in 1947 the JCS requested that the AEC produce 400 fission devices by January 1951. Nevertheless, the Joint Chiefs referred to fission

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[446] Memorandum to the File from J. Kenneth Mansfield, "Extracts from the GAC Reports Relating to Thermonuclear Program," May 28, 1952, JCAE declassified General Subject Files, Box 59.
[447] JCAE interview of Los Alamos scientists, May 12, 1950, op. cit; Memorandum to the Files from John S. Walker and Walter A. Hamilton, April 17, 1952, JCAE declassified General Subject Files, Box 59.

bombs only. Moreover, given that Los Alamos still did not know how to build a workable H-bomb, the JCS and American military planners would logically not have been interested in this as readily available weapon.[448]

No open discussion arose among military leaders for establishing a requirement for hydrogen weapons until Ernest Lawrence went to Washington in fall 1949. In addition to pursuing the JCAE, Lawrence also asked Nichols, who acted as both head of the AFSWP and a member of the MLC, to ask the JCS to establish a formal military requirement for thermonuclear weapons. Through Nichols, Lawrence transmitted his, Alvarez's, and Teller's, strong advocacy for a serious thermonuclear program to influential military circles including the JCS. The MLC, too, essentially dominated the deliberations of the NSC working group on the thermonuclear weapon, because it included three MLC members: Chairman Robert LeBaron, Nichols, and Rear Admiral Tom B. Hill.[449]

Until this time the MLC remained the only military planning group with direct knowledge of the AEC's laboratories and their projects, as well as the conversations that went on at the GAC meetings. The MLC constituted the only military group with any exposure to the prospect of hydrogen weapons. Their stance on the military value of H-bombs was, not surprisingly, reflective of both the views of Strauss, Lawrence, and other civilian advocates, and of the technical status of the thermonuclear program

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[448] David Alan Rosenberg, "American Atomic Strategy and the Hydrogen Bomb Decision," The Journal of American History 66, June 1979, 62-87.
[449] Rosenberg, "Hydrogen Bomb Decision," 81; Hewlett and Duncan, Atomic Shield, 378.

in the postwar. The GAC's doubtfulness as to the Super configuration's viability and its reputed size channeled back into the military part of the system.[450]

The MLC and JCS viewed the hydrogen bomb, at least through 1949, as a psychological weapon and in terms of technological competition with the Soviet Union. Thoughts of actual military use of thermonuclear bombs held secondary importance although the MLC recommended that once developed, hydrogen weapons might serve offensively as substitutes for numerous fission devices. Likely influencing Truman's opinion on fusion research, the JCS recommended development of thermonuclear weapons in January 1950, regarding the "super bomb" as essentially only an extension of existing strategy. Even if H-bombs would not have immediate military use, their development seemed unavoidable to military system builders.[451]

"A Honey of a Design Problem and Delivery"

The possibility of H-bomb use had been raised several years prior to the Soviet atomic test. Marshall Holloway believed that the Super remained so far off in the future that it defied classification in 1948, yet he realized intuitively one of the problems it would encounter if developed. When he described the Super to Robert Henderson at Sandia Laboratories, Holloway noted that the Super represented "a honey of a design problem and delivery,"

[450] Rosenberg, "Hydrogen Bomb Decision," 81.
[451] Ibid., 81, quote 83.

because the warhead alone in theory weighed 30,000 lbs., was 30 feet long and 16 feet in diameter.[452]

The Super's massive dimensions and weight were, like the tritium and computing issues, latent problems until after Soviet fission test. When JCAE members questioned Los Alamos scientists in late October 1949 about the state of the project, Bradbury described the Super as potentially weighing 20,000 lbs. Because of this, its delivery would be limited to the B-36, yet the force from an H-bomb blast preclude the use of manned aircraft, since the plane itself would not be able to escape the blast. [453]

The kind of delivery vehicle necessary for the Super was no longer a latent critical problem by the end of 1949 when military leaders became more informed about the Super theory. The Special Committee of the NSC appointed by Truman to evaluate thermonuclear weapons remarked, in 1950, that anticipating the exact nature of a carrier for the Super would be impractical, because the weapon had not yet been developed. However, verging on technological fantasy, the Committee did suggest that some possibilities included a drone aircraft, a ship, and an improved B-36 with an underbelly weapon attachment.[454]

All of these suggestions comprised merely long-term speculation. The Super theory and military weapons delivery technologies of the time were incompatible. Hansen notes that General Omar Bradley, Chairman of the

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[452] Memorandum from Holloway to Henderson, October 22, 1948, op. cit.
[453] Hansen, Swords, III-80.
[454] Ibid., III-98.

JCS, sent a memorandum in January 1950 to Secretary of Defense Louis Johnson, explaining the JCS's views on the H-bomb. The Joint Chiefs viewed determining whether or not a thermonuclear explosion could be obtained as top priority, but they also recommended that thermonuclear production in any quantity be deferred until scientists determined the Super's feasibility, and the military assessed an appropriate carrier's feasibility.[455]

While the Air Force queried the Sandia Corporation for information about the Super's dimensions and technical details, Los Alamos could provide little information to Sandia and the Air Force regarding the Super since Bradbury and his colleagues knew so little themselves. Nevertheless, the Air Force wanted to at least explore the possibility of carrying fusion weapons by the time Truman announced that work would continue on hydrogen devices, and initiated Project EAGLE to modify B-47's as drone carriers.[456]

The JCAE, as well, took up the issue of deliverability of the Super within a few months after the Soviet fission test. In summer 1950 JCAE member Sterling Cole asked his fellow Committee member Bill Borden his views on whether or not construction of H-bombs was worthwhile in terms of the A-bombs that would be sacrificed in doing so. His mind already made up as to the value of the hydrogen bomb, Borden wrote off the deliverability problem as minor and gave Cole a technically optimistic and unrealistic reply.

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[455] Ibid., III-101.
[456] Hansen, Swords, III-115-116; Hansen has dicussed the issue of H-bomb deliverability and the Air Force's role in this extensively. For more on this see Hansen, Swords, III- passim.

Since the weapon's explosion would be so violent that it eliminated the use of manned aircraft for delivery, Borden indicated that the simple solution might be a drone B-36 and:

> . . . bombers of the type which have already crossed the Atlantic Ocean by remote control from a 'mother' ship are comparatively easy to visualize. Because delivery of an H-bomb would mean putting the equivalent of 'many eggs in one basket,' a specially designed jet-propelled carrier seems indicated, and the Air Force is actually working along these lines.[457]

Furthermore, Borden justified the H-bomb as a deliverable weapon in terms of its being less subject to aiming accuracy than its fission counterpart. Borden had little confidence in the Air Force's ability to hit targets with atomic weapons with consistent accuracy, and argued that a fusion weapon could miss it's target by up to fifteen miles yet still prove destructive.[458]

A little over a year later, when the Teller-Ulam configuration had been proposed, the general idea of an H-bomb still constituted a "big bomb" that would, like the Super, prove a challenge to deliver. Teller wanted to convince the JCAE that deliverability did not constitute an overwhelming problem. When Jackson and Mansfield interviewed Teller and Carson Mark in July 1951 to ascertain the status of the thermonuclear project, the Hungarian noted that both the Classical and radiation implosion types of weapons would weigh in the range from 10 to 20 tons. Teller reported that some thought had been given to using a C-123 cargo aircraft as a carrier, but

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[457] Memorandum from Bill Borden to Sterling Cole, July 24, 1950, JCAE declassified General Subject Files, Box 62.
[458] Ibid.

more likely was the possibly of manufacturing a more appropriate airplane capable of delivering the behemoth weapons. Moreover, thermonuclear devices, like postwar fission devices, would according to Teller, be reduced in size in the future, implying that the delivery problem would simply disappear.[459]

Teller had promoted this line of thought for a long time. When he wrote his report on the state of thermonuclear weapons in 1947, he predicted that the Super would require many engineering considerations. At that time he and his colleagues envisioned that the Super would use about one hundred cubic meters of liquid D as a charge. "Production and transportation of so much liquid Deuterium," Teller reported, "will be an extremely difficult engineering job," yet there existed no reason, he continued, why this could not be accomplished within a few years.[460]

Teller conceded that delivery of a super by aircraft -- at least in 1947 -- would work. He suggested other technological fixes: a boat or submarine might provide suitable alternatives to aircraft delivery. The Alarm Clock at this time did not constitute a lighter alternative to the Super: the version that Teller and Richtmyer had envisioned in 1946 appeared in theory capable of producing a billion-ton TNT equivalent explosion. It too could not be transported by air.[461]

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[459] Memorandum to the File from Kenneth Mansfield, August 28, 1951, JCAE declassified General Subject Files, Box 58.
[460] LA-643, 25.
[461] Ibid, 25-26.

"The Super," Marshall Holloway wrote in 1949, "had in common with the Booster and Alarm Clock, the requirement that a rather large fission bomb be used to detonate it." The large fission initiator was one hindrance, but the actual thermonuclear fusion portion of the device served as a bigger problem, as Holloway noted. Due to the nature of the propagation of the detonation wave in the Super, "the energy yield is determined almost entirely by the amount of liquid deuterium contained in the weapon. Because of this, Holloway was not so impressed by the Super's mass destruction potential, concluding that it represented "pure fantasy from the design standpoint, as well as a very difficult delivery problem."[462]

Less critical of the Super theory than Holloway, Teller's younger protégés often chronicled his ideas. Physicist Harris Mayer, a student of Maria Mayer although of no relation to her, wrote a summary of Teller's classified lectures on the Super. Titling his summary the "Daddy Pocketbook," Mayer completed this in 1950 when the Super's feasibility remained unknown, and how it would delivered to a target made for an even larger mystery. The Daddy's tremendous explosive power, Mayer reported, prohibited its delivery by ordinary manned bombers because the bombers themselves would be knocked out from the blast of the weapon they dropped. Apparently Teller had suggested that long-range guided missiles could provide a solution to the delivery problem. Northrup Aircraft was, for example, developing the subsonic "Snark" missile to carry ordinary fission weapons. With minor

[462] Marshall Holloway, LA-732, "Characteristics of Atomic Bombs," 12 April 1949, 41. [This

modifications, Mayer wrote optimistically, the warhead could be enlarged to accommodate a "Daddy" that weighed between 4700 and 5000 lbs.[463]

Designing a liquid deuterium-fueled Daddy that weighed roughly the same as a Mark III fission device seemed far-flung, since doing so contradicted the nature of the liquid D-fueled Super: in theory this weapon was, if it worked, limited in yield only by amount of liquid deuterium fuel it contained. Most of the weapon's bulk came from liquefied D. Thus, if the main portion of the Daddy were scaled down so as to fit inside a warhead, much of its "thermonuclear character" of a massive yield would have been foregone.

Where Have All the "Good Men" Gone?

In the period before the Soviet fission test, a substantial amount of scientific imagination along with individual theoretical efforts, rather than organized research, characterized Los Alamos's efforts towards the Super theory. This was not merely because of wishful thinking on the part of Teller, Mayer, and others, but also because overall so few scientists participated in nuclear weapons design.

After 1945 Los Alamos ended up nearly devoid of scientific staff. Hewlett and Duncan, Rhodes, and Hansen have all noted this in their respective narratives of the postwar AEC weapons programs, yet this human critical problem underscored the hydrogen bomb project from the end of the

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Report is Secret-RD].
463 Harris Mayer, LAMS-1066, "Daddy Pocketbook," January 25, 1950, 13-14, [This Report is Secret-RD].

war through 1950, when Los Alamos began hiring larger and larger numbers of staff as part of the AEC's expansion program.

T Division alone had been reduced from thirty-five senior theoretical staff members in 1945 to eight in 1946. One of the few T-Division members who opted to remain at the Laboratory after the war, Carson Mark, recounted that the numbers of staff in his division reduced to single digits in 1946, but they increased very slowly through 1948: T-Division had only twelve theoreticians experienced in weapons design in 1947, and fourteen in 1948. The rise in staff numbers at this point helps account for the marked rise in the fission stockpile at this time. Prior to 1949 consultants such as Bethe, Fermi, Teller, Frank Hoyt, Lothar Nordheim, and von Neumann each lent typically a few months per year to the Laboratory, but their part-time work at Los Alamos could not provide for intense work on the Super or Alarm Clock theories.[464]

"I think we are making progress, although . . . so slowly . . . . We hope to study the hydrodynamics of the Alarm Clock before too long," wrote Richtmyer to Teller at the very end of December 1946. Yet, "Because there are so few of us and because minute details [are] taking so much time, I fear that it will be some time before we can report any real progress along the lines we discussed when you were here." [465]

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[464] Osborne, Theoretical Design, 5; Mark, Short Account, 3, op. cit.
[465] Letter from Richtmyer to Teller, December 30, 1946, B-9 Files, 201 Edward Teller, Drawer 22, LANL Archives, [This Document is Secret-RD].

Human versus Machine Labor

The "minute details" Richtmyer referred to were calculations underway on the IBM machines, work almost entirely restricted to fission-related problems. Thus, T Division found itself doubly handicapped by the lack of staff and not enough computing power to make up for the former. "Manpower" for hand calculations no doubt decreased with the war's end as well, since those scientists' wives who had made up a majority of Donald Flanders's hand computer group departed with their husbands in 1945 and 1946. Mark and others hoped that improved computing capabilities might help make up for the Division's labor shortage and ease the workload of the staff when preparing for the Sandstone tests.

In March 1948 Mark complained to Bradbury in his monthly T Division report of a "shortage of help." Responding to this human shortage, the Division wished to standardize what he called some of the "necessary" calculations on the IBM machines. At this time IBM replaced the wartime 601's with 602's, making it possible to perform wider ranges of problems. Mechanizing fission problems served another purpose, Mark asserted, of relieving the T Division staff from boredom and routine work of running standard, repetitive fission simulations on the punched card machines that varied little from those done during the war.[466]

Mechanization of fission problems went beyond the simple punched card machines at the Laboratory: while the HIPPO program not only

[466] LAMS-694, T-Division Progress Report: 20 January 1947-20 February 1947, March 1, 1948,

purported to give a better understanding of the Super ignition problem in that it traced the course of events in the Trinity device, Mark characterized the giant HIPPO as an experiment by von Neumann and Richtmyer to try to completely mechanize implosion problems.[467]

As a subtle but still critical problem facing the thermonuclear program, Los Alamos's lack of personnel seemed to Bradbury unsolvable for the few years immediately after the war. Froman advised the Laboratory Director in spring 1947 that a new personnel policy should be established to increase the number and caliber of scientific staff. However, one of the problems preventing Bradbury from bringing more personnel to work on nuclear weapons was an acute housing shortage arising after the war.[468]

Both the Laboratory and the town of Los Alamos, like the rest of the wartime MED system, were not constructed as permanent facilities. Hewlett and Duncan dramatically described the physical condition of the town and laboratory facilities in July 1947 from the point of view of Carroll Tyler when he arrived to take his new post as head of the AEC's Santa Fe Operations Office. Los Alamos appeared ramshackle to Tyler:

> It was hard to believe that these crumbling temporary buildings surrounded by oil drums, cable reels, and mud-caked Army vehicles housed one of the world's famous scientific laboratories. . . . most of the town's 7000 inhabitants still lived in temporary wartime buildings. There were few paved streets, no sidewalks, and almost no private

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LASL, [This Report is Secret-RD], 6.

[467] LAMS-694, T-Division Progress Report: 20 January 1947-20 February 1947, March 1, 1948, LASL, [This Report is Secret-RD].

[468] Memorandum from Froman to Bradbury, "Los Alamos Laboratory Directive," March 24, 1947, B-9 Files, Folder 635 - Lab Program, Drawer 176, LANL Archives.

> telephones . . . . It was evident that living conditions in Los Alamos would not help to attract talented scientists to the Laboratory.[469]

Teller's demand to Bradbury that Los Alamos tackle the Super right away or plan for multiple fission tests was unrealistic. Bethe remarked that Los Alamos, "with its limited scientific personnel, could not carry this work in addition to its more immediate responsibilities of improving fission weapons." Human labor, then, made up an underlying, early, and long-lived critical problem to the thermonuclear project.[470]

The people of Los Alamos still felt the town's "temporary" character three years after the war's end. When the Technical Board met in February 1948 Carson Mark mentioned T Division's program set up for the IBM machines for the remainder of the year, explaining that he hoped about nine implosion simulations would be completed in that time. Mark wanted to increase the number of staff responsible for running the implosion problems, but Bradbury expressed reluctance to hire any more staff because of the housing shortage in Los Alamos. The implosion problems might be expedited, Bradbury suggested to Mark and the rest of the Technical Board, not by hiring more scientists but by employing the ENIAC for implosion problems.[471]

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[469] Hewlett and Duncan, Atomic Shield, 132-133.

[470] Bethe, "Comments on the History of the H-Bomb," 45; Bethe, "Memorandum on the History of the Thermonuclear Program," 3.

Back to the ENIAC

Mark and T Division did not hesitate, having already prepared several fission problems for ENIAC by now. Metropolis, Frankel, and Turkevich's Super ignition problem remained the only hydrogen bomb-related calculation carried out on ENIAC prior to 1949, reflecting the Laboratory's emphasis on atomic devices in this period.

"It was no mere accident that the first problem on the first computer [ENIAC] was the thermonuclear bomb," Peter Galison asserts in Image and Logic. Indeed, von Neumann's close relationship to the Laboratory, his thorough knowledge of the Super theory, and his intimate friendships with several of Los Alamos's scientists all contributed to ENIAC's employment for the "Los Alamos Problem." The use of the machine, and the results presented at the 1946 Super Conference may have prompted Ulam to consider what would become the Monte Carlo method. Yet scientists reserved the Monte Carlo method for fission calculations throughout the 1940s, in accordance with Los Alamos's priorities, and no one ran a Monte Carlo-based thermonuclear weapon problem on ENIAC until 1950, when Foster and Cerda Evans and their team used the machine, by then at the Aberdeen Proving Grounds, to check Ulam and Everett's hand calculations of the ignition problem.[472]

[471] Technical Board Notes, February 2, 1948, B-9 Files, Folder 001, Drawer 1, LANL Archives, [This Document is Secret-RD].
[472] Galison, Image and Logic, 694, 698-699, 720-723.

In January 1948 Los Alamos had prepared the first Monte Carlo problems for the ENIAC, essentially developmental techniques of using the ENIAC, but the computer was not ready for the Monte Carlo problems since it had recently been moved from Philadelphia to Aberdeen. Throughout March and April Carson Mark complained in his monthly reports about the delays encumbered by the fission program because of the slow pace of the ENIAC's conversion and "mechanical condition." The whole point of having fission problems run on ENIAC in the first place, Mark noted, was to speed up T Division's work by "mechanization" of calculations.[473]

Metropolis and Adele Goldstine carried out the first computerized Monte Carlo calculations on the ENIAC in late spring 1948, although they did this primarily for the purpose of checking techniques, and according to Metropolis, did not attempt to solve any type of weapons problem. When they found flaws in the trial run, they placed a second set of similar Monte Carlo calculations on the ENIAC but did not complete them until November. This latter series of problems constituted actual weapons calculations.[474]

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473 LAMS-694, T Division Progress Report: 20 January, 1948-20 February, 1948, March 1, 1948, 6. [This Report is Secret-RD]; LAMS-714, T Division Progress Report: 20 Februrary, 1948-20 March, 1948, April 2, 1948, 3, [This Report is Secret-RD].

474 LAB-ADWD-26, The Committee for Weapon Development: Minutes of Meeting, January 28, 1949, 1, [This Report is Secret-RD]; Metropolis, personal communication, September 16, 1996; LAMS-791, T-Division Progress Report: August 20, 1948-September 20, 1948, October 27, 1948, LASL, [This Report is Secret-RD]; LAMS-743, T Division Progress Report: 20 April 1948-20 May 1948, June 17, 1948, 3. [This Report is Secret-RD]; LAMS-753, T Division Progress Report: 20 May 1948-20 June 1948, July 13, 1948, 2, [This Report is Secret-RD]; LAMS-811, T Division Progress Report: 20 October 1948-20 November 1948, December 8, 1948, 2, [This Report is Secret-RD]; Metropolis, "The MANIAC," 459; Evans, "Early Super Work," 139; Aspray, John von Neumann, 239.

The problems included an investigation of the alpha for UH^3, a "hydride" core implosion configuration; another calculation related to a supercritical configuration known as the Zebra.[475] Machine errors prevailed in the hydride problem completed in early 1949, where the machine-values of the alpha appeared too high, compared with hand calculations. In preparing his monthly report for T Division, Carson Mark quipped that it was "evident that the ENIAC has not advanced beyond an experimental stage in doing serious computation for this project."[476]

A year passed before the Evanses and their team could employ the ENIAC to run additional calculations to study neutron diffusion in a hydride system (which by now had the code name "Elmer") only to find out that it would have very low efficiency. As a result, the Laboratory dropped the hydride from its program in 1950 when Froman reported that in the opinion of the Committee for Weapon Development, the hydride would be a "poor weapon." In addition, Los Alamos's scientists envisioned the hydride as big and awkward, as depicted by George Gamow in one of his many cartoons. Gamow irreverently drew Elmer as a human with a bull's head, making it appear unattractive and clumsy especially when compared to Elsie, that the

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[475] LAMS-791, 3; Like many other weapons ideas explored during war, the hydride was shelved in the interest of completing the Fat Man device on time.

[476] LAMS-868, Progress Report T Division: 20 January 1949-20 February 1949, March 16, 1949, 2, 8, [This Report is Secret-RD]; The "alpha" is the measurement of a fission weapon's efficiency, where fission chain reactions are counted to predict the rate of fission assembly before the core disassembles.

Russian physicist depicted as a woman (although encumbered with a bovine head) diving out of an airplane. (see Figure 3).[477]

Besides the hydride problems, Los Alamos continued running other implosion problems on ENIAC in the summer of 1949. By now, Los Alamos's human labor problem lessened in severity; in T Division Carson Mark had twenty-two full time theoreticians by the end of the year, but after September Teller, Wheeler, and others had a reason to bring the Laboratory's staffing situation to the attention of the JCAE.[478]

"Apparently Teller is the one most worried about the shortage of good men," Hal Bergman reported to Bill Borden in May 1950 when he interviewed most of Los Alamos's scientific leaders as to the status and future of the fusion bomb project. The lack of "Manpower," as Bergman categorized the problem, stood in the way of the Laboratory responding quickly to the President's directive. Teller, however, counted on more than just the numbers of staff.[479]

The Laboratory and AEC had agreed to schedule the 1951 tests for March, April, or May, but Los Alamos's leaders remained uncertain if they could make this deadline due to "insufficient manpower of the proper

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[477] LAMD-277, "Notes on Bomb Nomenclature for Handy Reference," March 28, 1950. [This Document is Secret-RD]; Chuck Hansen, Secret History, 39n; Memorandum from Darol Froman to Members of the Technical Board, February 6, 1950, [This Document is Secret-RD]; Cerda Evans, Foster Evans, Harris Mayer, Marshall Rosenbluth, LA-985, Report on Monte Carlo Hydride Calculations, November 7, 1949, 2-3, [This Report is Secret-RD]; LAMS-920, T Division Progress Report: May 20, 1949 - June 20, 1949, July 12, 1949, 2, [This Report is Secret-RD]; LAMS-868, Progress Report T Division: January 20, 1949 - February 20, 1949, March 16, 1949, 2, [This Report is Secret-RD].

[478] Osborne, Theoretical Design, 11; Mark, Short Account, 3.

caliber" present in New Mexico. Almost undoubtedly repeating Teller verbatim, Bergman commented about the nature of scientific problem solving, and about those who practiced science. Manpower constituted a unique critical problem to the H-bomb project because:

> Skill and imagination are needed to solve the 1001 problems which arise. Such solutions are frequently the result of 'intuition' resulting from the unabashed and uninhibited imagination of young scientists. It is worthy to note that many of the most famous scientists had their best ideas before the age of 30. The project at present does not have a superfluity of either prominent experienced scientists or bold, imaginative ones. And the project is still primarily in the 'theoretical' and 'lab' stage, rather than in the engineering phase. If time were not of the essence, solutions to the many problems might be arrived at in pedestrian manner.[480]

In March 1950 Froman and Bradbury had agreed, in response to Truman's announcement, to expand the Laboratory's work-week from forty to forty-eight hours along with hiring several hundred more staff members. These plans did not satisfy Teller, who informed Bergman that most of the new hires were new Ph.D's, of which about ten percent included theoretical physicists. The others included mathematicians, chemists, and technicians, which the program certainly required. Arrogantly, Teller mused that a dozen good lab men can be worth as much to the project as the rest of the 400 scheduled for hiring within a year. [481]

As far as "good" scientists went, Bradbury told Bergman, "Sure we could use a Bethe or two, but they don't come by the dozen." Thus, hiring

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[479] Bergman to Borden, "Thermonuclear Program at Los Alamos," May 12, 1950, JCAE declassified General Subject Files, Box 60.
[480] Ibid., 2.

new Ph.D's was practical and in essence the only choice. Agreeing with Bradbury, Froman thought that the manpower problem would take care of itself because of the many new hires due to arrive that summer.[482]

The manpower problem meant more than simply hiring new staff, but also raising scientific interest in the hydrogen bomb in the first place. Manley told Bergman that he personally believed that "good men" would avoid the project as long as security restrictions kept them ignorant of how far along the project had progressed. Circumstances differed in 1950 than in 1943 in that there was no world war going on. Bergman paraphrased Manley:

> . . . if we are in a desperate situation then the people will respond as they did before. But many good people have a moral repugnance to making weapons of mass destruction unless they are convinced it is necessary for national defense. Then they will do it.[483]

They did not come in droves as Teller had hoped, and he complained often to the JCAE about the troubles he encountered recruiting well-known scientists to the Laboratory. Teller did enlist his close colleague John Wheeler to assist at least as a consultant. When in summer 1950 they jointly reported to General McCormack and the GAC about the state of the hydrogen bomb program, they emphasized the manpower problem. "Theoretical Analysis," they asserted, "is a major bottleneck to faster progress in analyzing

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[481] Memorandum to Bradbury and Froman on "Laboratory Program Draft of March 3, 1950," B-9 Files, Folder 635, Lab Program 1948-1950, Drawer 176, [This Document is Secret-RD].
[482] Ibid., 3.
[483] Ibid.

thermonuclear weapons; and the bottleneck to the theoretical analysis is the shortage of the right men."[484]

Teller and Wheeler reported that about a dozen members of T Division had been working on the thermonuclear project, with their efforts divided evenly between the question of burning deuterium and preparation for the 1951 tests. Some, of course, worked only on fission problems. The two theoreticians appealed to the AEC to institute a change in recruitment policy for the Laboratory, because the number of theoretical physicists at Los Alamos, they claimed, had decreased instead of increased. Because the George test for D-T ignition had already been scheduled for 1951, the most important remaining problem T Division needed to address was the propagation of D, so far inhibited by the "severe limits set by insufficient manpower."[485]

Teller's recruiting problems began soon after Bradbury approved hiring new personnel. By April 1950 the Hungarian revealed to the Joint Committee his anxieties about recruiting new personnel to Los Alamos to work on the H-bomb; he had gone on a trip to several American universities in the late winter and spring and claimed to be "shocked at the icyness [sic]" on the part of younger colleagues towards the atomic energy program. Many of his younger colleagues did not want to join the atomic energy program because they did not want to have to worry about secrecy, loyalty programs, clearances, the FBI, and politicians. Some expressed concern over the moral

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[484] Teller and Wheeler, LAMD-444, Appendix I-A, op. cit., [This Document is Secret-RD].
[485] Ibid., 5-6.

issues surrounding an H-bomb. Dismayed, Teller reported a "decrease in respect and confidence, by the scientific brethren, of the AEC and the JCC."[486]

Frederic de Hoffman resided in Paris at the time Teller began his recruitment campaign, and the senior scientist wasted no time tracking down his protégé. Telegramming de Hoffman, Teller relayed that he saw little hope of getting any "prominent names" to come the Laboratory, and so far no additions had been made to T Division. "Please come back yourself [because] strenuous months [are] ahead," Teller pleaded to de Hoffman, who did return to the Laboratory later in the year.

Although Wheeler arrived at Los Alamos in March 1950 he stayed only through June 1951. Blaming Los Alamos proper for the lack of theoretical manpower, Wheeler left for the IAS to lead Project Matterhorn, with the intention of using the IAS machine and hiring several of his own theoreticians to explore thermonuclear weapons. Hewlett and Duncan have indicated that then Chief AEC Commissioner Gordon Dean viewed Wheeler's act as one of abandoning the Laboratory. Bradbury also opposed Wheeler's plans because he feared that Matterhorn would consume too much time and further weaken Los Alamos.[487]

Wheeler's decision to initiate Project Matterhorn at Princeton was no doubt tied to the rift that Dean observed growing between Teller, von Neumann, and Wheeler on one side, and Bradbury and Manley on the other.

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486 Memorandum to Bill Borden from Hal Bergman, April 27, 1950, JCAE General Correspondence Files, Box 4, NARA, [This Document is Secret-RD]; Apparently "JCC" was an abbreviation used for the Joint Committee of Congress.

Not surprisingly, Wheeler's departure for Princeton coincided with Teller talking of resigning from Los Alamos, asking Bradbury to set up a separate thermonuclear division, and approaching the JCAE about initiating construction of a second laboratory.[488]

Teller had threatened to resign and had apparently proposed a second laboratory as early as October 1949. Wheeler also had proposed building another weapons laboratory, but instead initiated Matterhorn as a means of carrying out theoretical work separate from but under contract to Los Alamos. Teller himself claimed to have opposed Wheeler's decision initially, saying that a theoretical study center on its own had little appeal and would be too limited, but probably because it would detract from prospects for a second laboratory, not Los Alamos, since he thought in 1952 that there existed a substantial chance that a series of "Wheeler groups" would be established, each working piecemeal on the hydrogen bomb problem.[489]

Competition with the Fission Program

Whereas Teller had been going cross-country recruiting new staff for Los Alamos throughout 1950, instead of contributing to the Laboratory's effort to determine the Super's feasibility, Bradbury and Manley had to balance increased work on the Super with maintaining the pace of the fission program. When the AEC and MLC met at Los Alamos in November 1950, Bradbury explained how the fission weapon program related to the

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[487] Mark, Short Account, 4; Hewlett and Duncan, Atomic Shield, 536.
[488] Hewlett and Duncan, Atomic Shield, 536.

thermonuclear program. The situation with the Super, he noted, contrasted "sharply with developments in the fission fields during the past several months." Instead, fission weapons now looked more attractive at the Laboratory since many improvements in them looked certain to happen in the near future.[490]

Deciding which program would have higher priority deeply concerned Bradbury, as it appeared that the fission and fusion technologies competed for resources such as IBM calculating machines, the ENIAC, manpower, and nuclear materials. Bradbury claimed that the Laboratory had given the Super problem first priority throughout 1950, resulting in inconclusive hand and machine calculations. For Bradbury this uncertainty translated to little progress made in the area of thermonuclear weapons, leading him to suggest that in 1951 the Laboratory program should "do first those things promising the greatest possible gain in minimum time whether for a fission or fusion weapon." For the Laboratory leadership, it seemed more important to follow such a policy to give the country more weapons with greater power quickly without serious interference to the Super project.[491]

Bradbury's revised Laboratory program for Tyler in March 1950 indicated a the technical choice that would need to be made between atomic

[489] Walker Memorandum, January 13, 1953, 12, op. cit; Memorandum to the File from John Walker, "Conversation with Dr. Edward Teller on the evening of Tuesday, April 15, 1952, April 17, 1952, JCAE declassified General Subject Files, Box 58.
[490] Draft Memorandum to Chairman of the AEC, "Notes on the AEC-MLC-LASL Conference on Tuesday, November 14, 1950," November 17, 1950, op. cit., [This Document is Secret-RD].
[491] Ibid.

and fusion weapons. As Los Alamos would commit to determining the feasibility of the Super over 1950, Bradbury warned Tyler:

> It must be clearly understood . . . that much less than the maximum progress which could be made in the fission weapon field will be made under these circumstances, and that this sacrifice would be made in the attempt to ascertain the thermonuclear weapon possibility at the earliest possible date.[492]

Exploring the feasibility of the Super not only meant a slowing of the atomic program, but a shift in approach to problem solving for the weapons laboratory. The "nature and philosophy" of the thermonuclear program would differ from those previously employed by Los Alamos, Bradbury informed Tyler. The Director likened the thermonuclear program to one of "experimental and theoretical necessity," and because Los Alamos would have to "gamble" on the chance of maximum progress, the planned 1951 tests at Eniwetok [Greenhouse] -- involving the expenditure of fissionable material -- would take the place of "extensive model testing and detailed theoretical calculations." The Super constituted a special case and required a "more empirical approach," than had the postwar fission program, yet Bradbury felt that "the chance of failure in such tests will be appreciably higher than that under the old philosophy."[493]

Thermonuclear Fallout

The drama surrounding the progressive falling out between Teller and the Los Alamos leadership has been told by many authors including Hewlett and Duncan, and Rhodes, Galison and Bernstein, and thus I will not analyze

[492] Bradbury to Tyler, March 10, 1950, op. cit.

this in great detail. 1951 marked a turnabout year for the Los Alamos hydrogen bomb project since, as noted in earlier chapters, the Laboratory considered the Greenhouse George and Item shots successful, and also adopted the Teller-Ulam design. The GAC endorsed the Laboratory leaders' decisions, supporting the Teller-Ulam idea, in contrast to the Committee's earlier opposition to the Super.

Rhodes describes how, in the wake of the Greenhouse tests, Dean called for a GAC meeting at the IAS in June to bring together all the experts on thermonuclear matters within the AEC system. Like Teller's increasing alienation from Los Alamos, the June Princeton meeting has been well documented in the historical literature, where Oppenheimer, Fermi and other GAC members appeared to have completely reversed their views on thermonuclear weapons development, and now completely supported the program.[494]

Besides Teller, Bethe, Lothar Nordheim, von Neumann, Wheeler, and Carson Mark attended this meeting.[495] Teller himself claims that he convinced the GAC of the importance of the radiation implosion design rather quickly at the June meeting:

> We reported it to the General Advisory Committee. Carson Mark reported, 'We now found that thermonuclear reactions can work and we can calculate them, and we have no further plans.' At that time I had the present method for the hydrogen-bomb already. Carson Mark and Bradbury ignored it. I asked to talk to people, Bradbury opposed my talking. I was allowed to talk because one member of the GAC, Smyth,

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[493] Ibid.
[494] Rhodes, Dark Sun, 475-476.
[495] Hewlett and Duncan, Atomic Shield, 544.

> just in the name of freedom of speech, said I could. In 20 minutes, the recommendation of the GAC had changed, The hydrogen-bomb project went ahead.[496]

Rhodes highlights that no one else who attended the meeting shared Teller's "melodramatic recollection" of it. Furthermore, there is no evidence that the GAC planned a priori to Teller's speech to recommend against the Teller-Ulam design. One of the more significant characteristics of the IAS meeting was that no moral opposition to fusion weapons arose as in the October 1949 GAC meeting. The GAC's failure to condemn thermonuclear weapons on moral terms this time around, in light of the technological system, translates back to the technical aspects of the project: The Teller-Ulam theory appeared far more plausible than the Super, and required no tritium. Oppenheimer, as Rhodes cites, thought that the difference between the Super and the Teller-Ulam device could be found in the technical promise of the latter idea, where he the GAC Chairman (although quoted far too often in the historical literature) described the radiation implosion theory as "technically sweet."[497]

Besides attending the IAS meeting, over the course of 1951 Teller spent probably more time attempting to recruit staff and conversing with the JCAE members, as he worked on the new hydrogen bomb theory. Teller complained so frequently, and due to his resignation from Los Alamos, that the GAC met later in the year in Washington, DC in early December, 1951, to

[496] Author interview with Edward Teller, August 4, 1994, Los Alamos, NM.
[497] Rhodes, Dark Sun, 476.

discuss not only the AEC's proposed expansion program but to allow Teller to present his views on creating a second laboratory.

Los Alamos, Teller proclaimed to the Committee, was not suited nor able to explore fully the possibilities of thermonuclear weapons. His recruiting campaign had been less successful than he had counted on, Teller explained, because the interest among physicists in hydrogen weapons remained low, in part because the nation was not in a "hot war" (obviously the Korean War did not constitute a hot war in Teller's mind) and also because of "unexpected rebuffs" from Los Alamos. However, he argued that competent scientists certainly could be recruited to the hydrogen bomb program if it would be carried out in a new, "flexible" laboratory. Los Alamos, Teller charged, had become rigid and not conducive to the success of a forward-looking group.[498]

Had Los Alamos become "rigid" in its approach to weapons research and development? Teller may have honestly thought so, but he placed all the blame on Los Alamos and its leaders, and failed to look at the Laboratory's shortcoming's in the context of the AEC system. Oppenheimer tried to assuage Teller by noting that Los Alamos worked from "test to test," which indeed seemed wasteful and frustrating. Oppenheimer, speaking for the entire committee, agreed that some lines of thermonuclear work as well as implosion development not scheduled for the 1952 tests, needed more

[498] Minutes of Twenty-Eighth Meeting of the GAC to the U.S. Atomic Energy Commission, December 12-14, 1951, 10-11, DOE Archives, RG 326, Box 1272, Folder 1, [This Document is Secret-RD].

serious thought. More importantly, the Laboratory's basic structure remained nearly the same as in 1943, and reflected an obsolete conception of the Laboratory's function. Thus, the Laboratory was still hindered by its temporary character as set by the wartime system, and its functioning had fallen behind in the system.[499]

System Errors: Humans Among the Critical Problems

Aside from the physical artifacts found in large technological systems, such big bombs, computers, nuclear fuel, and aircraft, human action within a system is, ultimately the most influential force at work in fostering a system's growth and influencing its direction, both on progressive and regressive trajectories.

Just as system builders promote the technological system's progress, human characters can themselves be hindrances to the system. Both Rhodes and Hansen have cited Teller's so-called "obsession" with the Super configuration as a major obstacle to obtaining a more viable thermonuclear device, in that his desire to develop a weapon of potentially unlimited yield blinded him to other designs that may have produced smaller yields but truly demonstrated the principle of fusion. Besides his apparent myopic focus on the Super, Teller contributed to the retardation of the thermonuclear program in other ways. He (a) called for a two-year delay in 1947 on work in the area of hydrogen weapons to let computing technology catch up with nuclear weapons theoretical work, (b) left Los Alamos in 1946 for four years,

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[499] Ibid, 27.

and, (c) prior to 1951 dismissed the idea of radiation implosion as "unimportant."[500]

Technological systems are, as Hughes asserts, bounded by the limits of control exercised by artifactual and human operators. The human operators ultimately set the degree of control and character of systems, that do not take on an independent life of their own. A crucial function of people in technological systems, "besides their obvious roles in inventing, designing, and developing systems, is to complete the feedback loop between system performance and system goal and in doing so to correct errors in system performance." Furthermore, system builders with political influence, like Teller and Lawrence, often attempt to solve critical problems associated with growth and momentum. Here, political influence became extremely crucial. Even if Teller did not single-handedly come up with the Teller-Ulam design, he, even more than Lawrence, made the political case for thermonuclear weapons before the JCAE. [501]

Borden, Mansfield, and other Joint Committee members repeatedly interviewed Teller from 1949 over the next several years to seek his opinion on Los Alamos's efforts towards fusion weapons, thus Teller had an opportunity to convince McMahon and his Committee that America needed H-bombs, which were possible, and that Bradbury and Manley had delayed the project.

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[500] Rhodes, Dark Sun, 579-580; Hansen, Swords, III-60.
[501] Hughes, Evolution of Large Systems, 54, 57.

In his conversations with the Joint Committee members Teller did not account for the several other problems faced by the fusion weapon program. All of them either resulted from or at least reflected a struggling technological system based on a predecessor system intended to be temporary. One of these problems included the military's slow adoption of nuclear weapons to fit with its strategic war plans and more literally fit with available aircraft. Because of their large size, nuclear weapons developed for the war did not easily suit available aircraft -- thus these types of technological artifacts stood at incommensurable points of development.

The military's role in the postwar AEC system appeared less certain than it had been in the wartime MED system -- an Army-based operation. The military had set up the AFSWP and MLC to encourage military influence in nuclear weapons policy and future, but they did not formulate weapons policy in any way for the Commission nor for Los Alamos. Recollecting the early postwar years at the Laboratory, Darol Froman even went as far as to claim that bomb design and research were two entirely separate things; the MLC had nothing to do with weapons design, the details of which never left Los Alamos and became known among the larger system, including the MLC.[502]

More than this, the MLC and Armed Forces had no interest in thermonuclear weapons in the 1940s. Fission weapons seemed enough to

[502] Arthur L. Norberg, Interview with Darol K. Froman, Los Alamos Scientific Laboratory, 1980, 56; For more about the postwar custody battle over nuclear weapons, see Nicah Stewart Furman,

satisfy the MLC, which did suggest that the Commission instruct Los Alamos to explore configurations such as the Elsie penetrating device, and in general encouraged work towards lighter, smaller weapons to ease the problems of delivery, and bring these technical artifacts in the system more in line with one another. Overall, the military posted only modest requests for modifications and innovations in postwar fission devices. Requesting that the AEC develop a Super was unrealistic on the part of the Armed Forces because they could not develop in parallel the huge drone bombers nor warheads necessary to carry this type of weapon.

John Manley admitted on more than one occasion that he had never been enthusiastic about the Super. Furthermore, he did not wish to adapt weapons to military needs; weapons development after Hiroshima and Nagasaki, Manley once told an interviewer, was made up of trivial changes as the amount of "bang per pound" of fissionable material. Froman concurred, saying that scientists came up with very few new ideas by 1948 that had not already been thought up during the war, and as Bradbury had tried to established as best he could a postwar mission, Los Alamos focused almost solely on improving the wartime devices up through the Sandstone test series.[503]

Although severely weakened by lack of staff, decaying facilities, and at best a tenuous postwar mission, Los Alamos had, according to Froman, a very

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Sandia National Laboratories: The Postwar Decade, (Albuquerque: University of New Mexico Press, 1990).

strong role in forming AEC policy up through the time the thermonuclear program became politicized. Bradbury agreed, stating that when in 1947 new weapons became the AEC and GAC's common interest, he told the Commission where Los Alamos went in terms of bomb development. Policy, Bradbury claimed, "flowed from here to Washington and then came back as official."[504]

If policy did flow from Los Alamos to Washington prior to the Super debate in 1949, then within that policy was an implicit decision on the part of Los Alamos's leaders to proceed with thermonuclear weapons research on a very slow, modest scale. Neither the GAC, AEC Commissioners, nor Joint Committee challenged Los Alamos about this prior to 1949. Furthermore, there is no evidence that the JCAE had been well informed as to the technical possibilities for a fusion bomb before this time, thus McMahon and his peers did not concern themselves with the Super or policy regarding it.

Bradbury, Froman, and Manley had practical reasons for choosing the technical paths they did after the war and not giving Teller the massive thermonuclear research program he asked for in 1945 when deciding to remain in New Mexico or leave for Chicago. The Laboratory was not equipped to embark on a large fusion weapons project because so few scientists remained to work on both the theoretical and experimental parts of this project. The "manpower" problem at Los Alamos was significant, so

[503] Arthur L. Norberg, Interview with John H. Manley, Los Alamos Scientific Laboratory, 1980, 64-65; Norberg interview with Froman, 49.
[504] Norberg interview with Froman, 52; Norberg interview with Bradbury, 58.

much that T Division had hoped to make implosion problems routine by mechanizing them completely. Neither Los Alamos's punched card machines nor the ENIAC sufficed and the human labor shortage plagued the Laboratory until the AEC called for a significant expansion of the entire technological system.

A last but not least problem that originated at the Laboratory in the postwar period, exacerbated by the AEC's failure to respond quickly, concerned the town of Los Alamos itself. As part of the MED, Los Alamos not been built to last, evident in the temporary physical structures on the Hill that the AEC did not begin to replace or add to with any effort until 1949. Los Alamos still suffered, then, from an obsolete system even after the new AEC system had been operating for a few years. Inadequate housing prohibited Bradbury's bringing large numbers of new scientists to the Laboratory; few new staff members prohibited large-scale work on the Super and Alarm Clock, or the discovery of other H-bomb designs. The AEC technological system and consequently its components had still to develop further.