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Igniting the Light Elements: The Los Alamos Thermonuclear Weapon Project, 1942–1952

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Igniting the Light Elements: The Los Alamos Thermonuclear Weapon Project, 1942–1952

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Igniting the Light Elements: The Los Alamos Thermonuclear Weapon Project, 1942-1952

by

Anne Fitzpatrick

ABSTRACT

The American system of nuclear weapons research and development was conceived and developed not as a result of technological determinism, but by a number of individual architects who promoted the growth of this large technologically-based complex. While some of the technological artifacts of this system, such as the fission weapons used in World War II, have been the subject of many historical studies, their technical successors -- fusion (or hydrogen) devices -- are representative of the largely unstudied highly secret realms of nuclear weapons science and engineering.

In the postwar period a small number of Los Alamos Scientific Laboratory's staff and affiliates were responsible for theoretical work on fusion weapons, yet the program was subject to both the provisions and constraints of the U. S. Atomic Energy Commission, of which Los Alamos was a part. The Commission leadership's struggle to establish a mission for its network of laboratories, least of all to keep them operating, affected Los Alamos's leaders' decisions as to the course of weapons design and development projects.

Adapting Thomas P. Hughes's "large technological systems" thesis, I focus on the technical, social, political, and human problems that nuclear weapons scientists faced while pursuing the thermonuclear project, demonstrating why the early American thermonuclear bomb project was an immensely

complicated scientific and technological undertaking. I concentrate mainly on Los Alamos Scientific Laboratory's Theoretical, or T, Division, and its members' attempts to complete an accurate mathematical treatment of the "Super" -- the most difficult problem in physics in the postwar period -- and other fusion weapon theories. Although tackling a theoretical problem, theoreticians had to address technical and engineering issues as well.

I demonstrate the relative value and importance of H-bomb research over time in the postwar era to scientific, politician, and military participants in this project. I analyze how and when participants in the H-bomb project recognized both blatant and subtle problems facing the project, how scientists solved them, and the relationship this process had to official nuclear weapons policies. Consequently, I show how the practice of nuclear weapons science in the postwar period became an extremely complex, technologically-based endeavor.

Chapter One

Introduction and Literature Review: Why the H-bomb Still Matters

Historians have demonstrated several times how the practice of science in the 1930s and in the Second World influenced the character, style, and scale of modern American scientific practice since the 1940s. In the twentieth century, secrecy often characterized the ties between science, technology, and the military because so much federally-sponsored research was bound to defense interests. Further, science and technology for national security constituted a large portion of the federal budget from the Second World War to the present.

Unique to twentieth-century scientific practice, the growing networks of federally-sponsored laboratories (and their support facilities), and university and private contractors made up enormous systems of science and technology. Assessing these big networks is difficult because of their complexity, although a few scholars have tried. Historian Robert Seidel described the AEC multipurpose laboratories, their mission orientation, instrumentation, and multidisciplinary technical teams as a "system" of information manufacture. Historian Thomas Hughes went further, employing the term "technological system" to describe the AEC's predecessor -- the Manhattan District and its network of laboratories.[1]

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[1]Robert W. Seidel, "A Home for Big Science: The Atomic Energy Commission's Laboratory System," Historical Studies in the Physical and Biological Sciences, 16:1 (1986), 135-175.; Thomas P. Hughes, American Genesis: A Century of Invention and Technological Enthusiasm, (New York: Penguin, 1982), 383.

Building on his earlier work, Hughes first introduced the technological system thesis in Networks of Power (1983),[2] a study of the rise of the electric utility industry. The systems thesis -- a historical analytical framework -- allows for a view into scientific processes with an eye towards the technological products.

Hughes defines the "technological systems" that increasingly structure our environment (in his case study - electric power systems) broadly. They contain related, and interconnected, parts or components. Thus, the state, or activity, of one component influences the state, or activity, of other components in the system.[3] The components of a technological system can include physical artifacts, organizations, scientific texts, articles, universities, legislative artifacts, natural resources, and environment. A system has, of course, actors or human components as well, such as inventors, organizers, and managers. Among this last group, the "system builders" lead, and organize and develop components of the system.[4]

Adopting Hughes's terminology, the "system" of nuclear weapons research and development, at over fifty years old, is still going strong. Much of this network remains largely unexplored historically because the majority of nuclear weapons-related work was classified as "Secret-Restricted Data" under the Atomic Energy Acts of 1946 and 1954.

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[2]Thomas P. Hughes, Networks of Power: Electrification in Western Society, 1880-1930, (Baltimore: Johns Hopkins University Press, 1983).
[3]Ibid., 5.
[4]Thomas P. Hughes, "The Evolution of Large Technological Systems," in The Social Construction of Technological Systems: New Directions in the Sociology and History of Technology, eds. Wiebe Bijker, Thomas P. Hughes, and Trevor Pinch, (Cambridge, MA: MIT

This restricted nature of the systems that supported science and technology for military purposes has prevented attempts by historians to reconstruct accurately many scientific projects carried out by government institutions from the National Security Agency to the Department of Energy's (DOE) nuclear research and weapons laboratories. The DOE's laboratories, including Los Alamos, Livermore, Sandia, Argonne, Oak Ridge, and others have been the subjects of various historical, political, and even sociological studies. Most of these studies are limited in their scope partly due to access restrictions, but also because of the narrow single-disciplinary focus almost all of them take. In addition, the enormous nuclear weapons research and development complex is daunting, and therefore, histories of the DOE's facilities, and more importantly, studies of the nature of the overall system of nuclear weapons development await exploration.

Not surprisingly, the theoretical nuclear weapons design portions of the DOE system (those facilities most directly responsible for nuclear weapons research and development) -- Los Alamos and Livermore National Laboratories -- have received few historical treatments. Routine classification of historical documents generated at these facilities thwarts even the most persistent scholars. Nevertheless, the nuclear laboratories, their unique scientific and social cultures, and the weapons they provide for the military have consistently been the subject of scholarly interest.

Press, 1987), 51-52.

For many political scientists, historians, sociologists, and others, nuclear devices and nuclear power hold an intense if not morbid fascination. Nuclear weapons, and particularly the destructive potential they hold, are one of the most controversial and widely written-about technical developments of the twentieth century. Sociologists Donald MacKenzie and Judy Wajcman have argued, "All other effects of technology pall into insignificance besides the possible effects of nuclear weapons . . . . Understanding what has shaped and is presently shaping the technology that makes this possible is thus an urgent task." The classification constraints that barricade the meeting of this "urgency" have been kinder to practitioners of other social science disciplines who rely less on documentary or archival evidence than historians, which explains why nuclear weapons have been the subject of more political, sociological, and psychological studies than historical studies.[5]

For historians of science and technology, the classification standards have changed for the better over the last several years. Recent declassification of many documents from the nuclear weapons laboratories and DOE archives has allowed for the publication of a few new scholarly interpretations of the early atomic bomb program and a few attempts to examine the origins of the thermonuclear weapons program. As I assess in the following review of the work in these areas, a few scholarly histories of the Manhattan Project and wartime atomic bomb development at Los Alamos stand out, but definitive

[5]Donald MacKenzie and Judy Wajcman, eds., The Social Shaping of Technology: How the

analyses of the thermonuclear program from the wartime through much of the Cold War era have yet to face in-depth study. This dissertation is an exception to the "normal" process of historical research and writing because I had access to classified materials. This kind of access is not without scholarly pitfalls. For more about this, please refer to my "Bibliographic Note" on page 322.

Of those published studies that attempt to tackle the history of the American thermonuclear bomb program, most fail to answer the question of why the project entailed so many theoretical and engineering-related problems, and how weapons scientists solved them. Instead, such historical examinations of the project tend to frame their analyses loosely around the assumption that certain individuals involved in nuclear weapons policy decision-making somehow delayed work on the first American hydrogen bomb for several years following the end of World War II.

It is easy and convenient to argue that Los Alamos Laboratory and the AEC took a long time to develop a working hydrogen weapon, especially considering that scientists and engineers completed the first atomic weapons in about three years from the inception of the Manhattan Project until the bombings of Hiroshima and Nagasaki. Furthermore, many of the scientific participants in the fission and fusion weapon projects, along with the popular political and military figures of the postwar, criticized the AEC and Los

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Refrigerator Got its Hum, (Milton Keynes: Open University Press, 1985), 224.

Alamos numerous times for not developing a workable thermonuclear device before 1952.

This general historical perception that the hydrogen bomb took too long only reduces the H-bomb project's history to a political level. This perception also fails to account for the vastly complicated system of technology and its limitations, scientific networks, seemingly unsolvable problems in physics, and individual actors, in addition to political forces, together constituting the program that hydrogen weapons were developed within.

Asking why the hydrogen bomb project took what seems an abnormally long time, then, constitutes the wrong question, and a rhetorical one; analyzing the whole fusion bomb project in this kind of temporal framework cannot encumber the wide variety of problems the project faced. Instead, a more sophisticated historical may account for all the technical, social, and political problems involved in the project.

Approximately ten years that passed from the time physicists postulated a thermonuclear device in 1942 until the 1952 full-scale fusion bomb test. The length of time that passed is irrelevant when considering the problems affecting the pace and scale of the project. The problems remain unexamined in the history of science literature, as do several broad aspects about the project.

First, scientists posed the hydrogen weapon as a theoretical question before the start of the Manhattan Project. Although the U.S. successfully tested a fusion bomb in November 1952, it arguably represented only a proof-of-principle demonstration, and not a deliverable or practical weapon. The length of time the project took, then, is relative. Second, Why Los Alamos opted to construct and test this particular type of configuration (as opposed to a more weaponizable or deliverable type) first has never been clear historically. Third, the atomic weapon program shaped the thermonuclear program at Los Alamos. For example, during the war scientists believed that a fission device had to precede a fusion bomb, thus an atomic device required development and testing before any experimental work on H-bombs could begin. In this way, although the two projects cannot be analyzed historically independent of one another, they remain distinguishable, separate projects characterized by different theoretical problems and engineering considerations. Fourth and last, Los Alamos's leaders did not completely control the nuclear weapons program in the postwar period. Instead, the Laboratory and the nuclear weapons projects belonged to the large technological system of the U.S. Atomic Energy Commission, the agency ultimately responsible for the course of both atomic and thermonuclear weapons research and development.

While this dissertation aims to challenge the common single-disciplinary examinations by employing a variation of Hughes's technological systems approach, in it I also discuss the scientists -- particularly the theoretical physicists and mathematicians -- involved with the H-bomb project. I present a collection of several case studies of the enormous technological hurdles weapons scientists faced. I will elaborate more on the technological obstacles and on the dissertation's theoretical framework after reviewing several studies of the American nuclear weapons complex.

Because of the paucity of history of science and technology and science studies-oriented analyses of thermonuclear weapons, here I review studies from other fields of history and even other disciplines, including sociology and political science. The few academic historical studies that address nuclear weapons research and development vary as much in focus as in quality and accuracy. Therefore, I also review several items by journalists and weapons scientists themselves. I divide the literature review into the following categories: Los Alamos and the fission project histories; thermonuclear weapons studies; official and technical histories; political histories; sociological studies; and, scientists' accounts.

Although my dissertation aims to analyze the early Los Alamos thermonuclear weapons project, I also review a few Manhattan Project and atomic weapons histories to help establish some historical background for the later hydrogen bomb project.

Los Alamos and the Fission Project Histories

Since the end of World War II, numerous Manhattan Project histories have been published. To review them all would require several hundred pages. Historian Albert Moyer notes, "The fascination with the wartime development of bombs has extended to Oppenheimer's Los Alamos lieutenants and other soldiers in the Manhattan campaign -- not only publicly conspicuous physicists such at Bethe and Teller but also less prominent men such as Phillip Morrison, Leo Szilard, and Robert Wilson." Research on J. Robert Oppenheimer alone, Moyer asserts, became a "scholarly industry."[6]

Histories of nuclear weapons technologies take only a few pages to discuss. Reviewing publications concerning the wartime fission project, Seidel in 1990 stated that journalists, and popular and official historians produced most of the work on the atomic project, and dismissed the majority as "pot-boilers."[7] Yet a small number of scholarly, well-researched Manhattan Project histories exist, the best of which is Richard Rhodes's The Making of the Atomic Bomb (1986).[8] This work is unmatched in style and detail. Rhodes successfully narrates the technical and human elements of the atomic bomb effort beginning with the work of the Curies, Chadwick and other scientists working in turn-of-the-century Europe, and ending with vivid narratives of many of Hiroshima's victims. Rhodes's epilogue is essentially a summary of the thermonuclear program, carried on immediately after the war by Hungarian physicist Edward Teller and Italian physicist Enrico Fermi. Rhodes correctly relays that prior to 1945 the wartime fission program took precedence over work on the thermonuclear device.

In his epilogue to The Making of the Atomic Bomb, Rhodes's introduction to the fusion bomb project is, unfortunately, reductionist; he highlights the roots of Teller's so-called "obsession" with the Super, a result

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[6]Albert E. Moyer, "History of Physics," in Historical Writing on American Science: Perspectives and Prospects, eds. Sally Gregory Kohlstedt and Margaret W. Rossiter, (Baltimore: Johns Hopkins University Press, 1985), 163-182.

of the Hungarian scientist's childhood fear of the Russian communists. By portraying Teller this way, Rhodes sets the stage for his subsequent history of the U.S. hydrogen bomb program, which I review later in this chapter.

Lillian Hoddeson, Paul Henriksen, Catherine Westfall, and Roger Meade produced the best general history of Los Alamos's wartime technical program, Critical Assembly (1993).[9] The authors utilize many classified and formerly classified Los Alamos documents, and provide a view into wartime Los Alamos and its struggle to change its technical mission during the project, in particular the shift from the plutonium gun bomb to an implosion "gadget."[10] Agreeing with Rhodes, Hoddeson and her co-authors reveal that the fusion bomb project entailed only a small theoretical effort from 1943 through 1945.

Thermonuclear Weapons Studies

Except for Hoddeson and her collaborators, Rhodes, and Chuck Hansen, whose work I review later under the technical histories category, postwar nuclear weapons science and the weapons design laboratories remain for the most part untouched by historians of science and technology. This gap in the historical literature is especially obvious when considering that many journalists and other writers portrayed the thermonuclear project as a politically charged, fear-inspiring technological development whose main

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[7]Robert W. Seidel, "Books on the Bomb," Essay Review, ISIS, 1990 (81), 519-537.
[8]Richard Rhodes, The Making of the Atomic Bomb, (New York: Simon and Schuster, 1986).
[9]Lillian Hoddeson, Paul Henriksen, Roger A. Meade, and Catherine Westfall, Critical Assenbly: A Technical History of Los Alamos During the Oppenheimer Years, (Cambridge: Cambridge University Press, 1993).
[10] "Gadget" was used at Los Alamos during the war as a code-name for "bomb."

proponent, Teller, wanted only to develop weapons capable of completely destroying the Soviet Union. Indeed, the development of the fusion bombs were political, but not for the majority of the project's lifetime. Too often writers characterized the thermonuclear project broadly, and mistakenly, as the "Super" project. As a consequence, the project and even the scientists involved in it take on a modern mythical, and even fictional character. Teller's character, and the American H-bomb program, supposedly inspired film-producer Stanley Kubrick's Dr. Strangelove.[11] While little doubt exists that the American thermonuclear program had many cultural implications, its history is still elusive.[12]

The history of hydrogen bomb development remains haphazardly documented, thinly interpreted, and partly secret. No good scholarly interpretations of the fusion bomb project focus on Los Alamos and its role as the theoretical center for thermonuclear research. Furthermore, no scholars have cast an eye towards the technological artifacts themselves. In general, few authors have chosen to avoid the political reality and mythology surrounding the H-bomb. The first journalistic reports on the hydrogen bomb project propagated this sort of public misinformation in the early 1950s. I review them here.

[11] Dr. Strangelove or: How I Learned to Stop Worrying and Love the Bomb, directed by Stanley Kubrick, Columbia Pictures, 1964.
[12] For more on the cultural and social implications of nuclear weapons technologies, see Spencer Weart, Nuclear Fear: A History of Images, (Cambridge, MA: Harvard University Press, 1988) and Paul Boyer, By the Bomb's Early Early Light: American Thought and Culture at the Dawn of the Atomic Age, (New York: Pantheon Books, 1985).

In the ugly political climate surrounding physicist J. Robert Oppenheimer's security trial, journalists took up the H-bomb issue for the first time. Charles J.V. Murphy, an editor of Fortune magazine, published a short piece in 1953, "The Hidden Struggle for the H-Bomb." Dramatically emphasizing Oppenheimer's opposition to all thermonuclear weapons (which historically is incorrect), Murphy credits Teller as the sole genius behind the H-bomb's discovery, a test of which Oppenheimer and the AEC wanted to stifle. [13]

In a similar vein, James Shepley and Clay Blair, Jr., published the first full-length book on the origins of the hydrogen bomb, The Hydrogen Bomb: The Men, The Menace, The Mechanism, in 1954.[14] In this, they imply that Oppenheimer fostered a general hostility to thermonuclear weapons. In addition, their account of the technical problems within the project is scant and wrong in many cases. Historiographically, both Murphy's article and Shepley and Blair's book promote the idea that some individuals held up hydrogen weapons development. All three authors focus so much on the characters of Teller and Oppenheimer, respectively, as protagonist and antagonist for the H-bomb project, that these ideas have pervaded much of the subsequent literature on this history.

Forty years later these notions still prevail. As a follow-up to his earlier work, Richard Rhodes published a general history of the H-bomb

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[13]Charles J.V. Murphy, "The Hidden Struggle for the H-bomb," Fortune, May 1953, 109-110, 230.

[14]James R. Shepley and Clay Blair, Jr., The Hydrogen Bomb: The Men, The Menace, The Mechanism, (New York: David McKay Company, Inc., 1954).

project, Dark Sun: The Making of the Hydrogen Bomb (1995).[15] This pales in comparison to The Making of the Atomic Bomb. Although Rhodes's interpretation of Los Alamos's postwar thermonuclear program is fairly well-researched, and his ability to bring to life the human participants excels as usual, Dark Sun has several weaknesses. While I will review these weaknesses, I do not evaluate Rhodes's interpretation of thermonuclear devices proper because as a Los Alamos Laboratory employee, I am legally restricted by a DOE "no comment" policy regarding the accuracy of the technical content of Dark Sun, and cannot address Rhodes's technical descriptions of thermonuclear designs without losing my security clearance and facing other reprimands.[16] Nevertheless, I am free to discuss the many other aspects of Dark Sun that deserve commentary.

Rhodes presents an entertaining narrative, comprising three separate parallel stories. Only one of these tales actually relates to the American hydrogen bomb program. The others, one about Soviet espionage in the Manhattan Project, and another which is an attempt to analyze the Russian atomic bomb effort, have little relevance to the American thermonuclear project as Rhodes presents them. First, while fascinating in itself, Soviet espionage during the World War II did not influence the technologically original and independent Russian H-bomb projects. Second, Rhodes devotes a third of his manuscript to the Soviet fission weapons program presumably

[15]Rhodes, Dark Sun: The Making of the Hydrogen Bomb, (New York: Simon and Schuster, 1995).

in order to show how it influenced politically the expansion of the American H-bomb project. However, he never demonstrates this influence. Last, Rhodes's employed nearly all second-hand Russian sources, and he repeats much of what David Holloway covered in Stalin and the Bomb (1994).[17]

For the one-third of Dark Sun which addresses the American thermonuclear project, Rhodes relied heavily on interviews he conducted with retired weapons scientists. Undoubtedly Rhodes had to do this because he did not have the security clearance to view the classified documents at Los Alamos and other facilities which pertain to the thermonuclear program. However, a frequent problem with oral history is that human beings either forget entirely or re-invent memory, which is the case with some of Rhodes's interviewees. In sum, the small portion of Dark Sun that directly addresses the U.S. thermonuclear effort comes across as, in the words of historian Barton Bernstein, "bloated and desultory."[18] Rhodes's H-bomb story is incomplete. Combine this with the two other independent stories he presents, and by the end of the manuscript, Dark Sun burns out.

Rhodes poses and tries to answer the question of why the U.S. took a seemingly inordinate long time to develop and test a thermonuclear device. His main conclusion is a simple: Edward Teller's single-minded ambition and blind insistence on developing a multi-megaton weapon delayed the

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[16]Please see my "Bibliographic Note" on page 325 for a description of the DOE "no comment" policy.
[17]David Holloway, Stalin and the Bomb: The Soviet Union and Atomic Energy, 1939-1956, (New Haven: Yale University Press, 1994).
[18]Barton Bernstein, review of Dark Sun, by Richard Rhodes, in Physics Today, (January 1996) 61-64.

program. This is difficult to accept, however, because the thermonuclear program was too complex and involved numerous advocates besides Teller. It is easy to single out Teller as the thermonuclear program's driving force because he has been portrayed historically as having an unwavering commitment to this project. He did not always display such commitment to the project, even though he acted one of the most outspoken and politically savvy physical scientists in the postwar. Finally, Rhodes is not the first to suggest that Teller's blind ambition and attraction to scientific fantasy steered an entire research program on the course of disaster. Historian-turned-journalist William Broad drew the same conclusion in Teller's War (1992), where Broad compares Teller's zeal for the Super with his later obsession for the X-Ray Laser program, which Broad concludes describes as a failure.[19]

As Rhodes's chief antagonist, Teller is the dark, brooding "Richard Nixon of American Science." Thus, Rhodes leaves the reader with the impression that other reasons for the so-called lengthy time Los Alamos took to develop a thermonuclear device were insignificant.[20] This is historically far from the truth. Instead, the thermonuclear effort comprised a huge contingent of human endeavor, scientific networking, and the overcoming of technical and social hindrances.

[19]William J. Broad, Teller's War: The Top-Secret Story Behind the Star Wars Deception, (New York: Simon and Schuster, 1992).
[20]Rhodes, Dark Sun, 578.

Political History

The best published article addressing the political side of the thermonuclear project is Barton Bernstein and Peter Galison's "In Any Light: Scientists and the Decision to Build the Superbomb, 1952-1954."[21] The authors examine the shifting views of nuclear scientists-turned-policy-advisors, several of whom displayed inconsistencies in their moral and political attitudes towards thermonuclear weapons development. Galison and Bernstein debunk the common, oversimplified story that the split decision to go forward with hydrogen bomb research divided neatly into two separate scientific camps: a group of advocates led by Teller and Ernest O. Lawrence, and the opposing force led by J. Robert Oppenheimer and James Bryant Conant.

Galison and Bernstein succeed in treating the thermonuclear story on a political level, by, for example, including a detailed discussion of how Joe-1 (the 1949 Soviet atomic test) changed Washington's views. Their political analysis of scientific advocacy and opposition to building thermonuclear weapons is very good, but they do not examine the multifaceted technical problems faced by the Super program. However, as Galison and Bernstein acknowledge, this study is not a technical history. Like Rhodes, Galison and Bernstein had only limited access to technical documents regarding the thermonuclear program, which leads them to make a mistake in terminology

[21]Peter Galison and Barton J. Bernstein, "In Any Light: Scientists and the Decision to Build the Superbomb, 1952-1954," <u>Historical Studies in the Physical and Biological Sciences</u>, 19:2, (1989), 267-347.

often seen in literature on hydrogen bombs; that is, as their title suggests, the "Superbomb" constituted the main focus of Los Alamos's thermonuclear technical program from 1942 through 1952. Strictly speaking, this is not correct. The "Super" represented one of several proposed fusion devices in the postwar era -- the oldest type of a hydrogen device, although it has become a generic term in popular parlance for all kinds of thermonuclear weapons. By 1952, Los Alamos all but abandoned this configuration in favor of other pursuits. I will discuss a variety of proposed thermonuclear designs later in this dissertation.

In a similar fashion, Bernstein alone has written several excellent pieces related to the hydrogen bomb project. His "In the Matter of J. Robert Oppenheimer," (1982), is a lucid discussion of the events leading up to the Oppenheimer security case, an event that Bernstein calls a "classical tragedy."[22] Bernstein's emphasis on the characters involved in the case (and particularly their human flaws) lends great credence to the most influential portion of the technological system that was responsible for hydrogen weapons development -- the human system builders and powerful characters involved in this project. Although Bernstein does not delve into the history of the Super or thermonuclear projects in this piece, he does prove that the H-bomb issue allowed Oppenheimer's persecutors to win their case to revoke his security clearance.

[22]Barton J. Bernstein, "In the Matter of J. Robert Oppenheimer," Historical Studies in the Physical and Biological Sciences, 12:2 (1982), 195-252.

In 1983 Bernstein published "The H-bomb Decisions: Were They Inevitable?" in an edited collection of papers on national security topics.[23] Bernstein's paper is an attempt to analyze President Harry Truman's decision in January 1951 to order the AEC to accelerate the H-bomb project, by reviewing the controversy over this issue within the Commission and its General Advisory Committee (GAC). In doing this, Bernstein displays the social character of this conflict nicely, although he does not explore the technical problems equally intrinsic to the H-bomb controversy.

Bernstein's other relevant article, "Four Physicists and the Bomb: The Early Years, 1945-1950," (1988), provides a glimpse into four of the most important scientific advisors regarding nuclear weapons policy: Oppenheimer, Ernest Lawrence, Enrico Fermi, and Arthur Holly Compton.[24] Although the title is ambiguous because for Bernstein "the Bomb," refers to nuclear weapons of both the fission and fusion types, this piece is important in that it reveals the often inconsistent opinions on nuclear weapons which these four scientists displayed. Bernstein tends to emphasize the moral (and to a lesser degree some political) questions regarding fusion bomb development, while skirting other problems and issues surrounding project.

Several political histories exist that are related to, although not directly about, the hydrogen bomb project. Here I discuss a select few worth mentioning for their historical value and relevance to this dissertation.

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[23] Barton J. Bernstein, "The H-Bomb Decisions: Were They Inevitable?" in Bernard Brodie, Michael D. Intriligator, and Roman Kolkowicz, eds., National Security and International Stability, (Cambridge, MA: Oelgeschlager, Gunn, & Hain, 1983), 327-356.

Among these studies, Richard Sylves's The Nuclear Oracles (1987) provides a useful overview of the GAC, its members, and some of the large policy decisions they made.[25] Mostly a chronology of the GAC's meetings, this work includes an entire chapter about the GAC's role in the H-bomb controversy. While Sylves provides no interpretation of this controversy, he succeeds in demonstrating that in its early years, the GAC acted as a very influential group and ultimately had an important influence in the larger system.

Historian Gregg Herken's The Winning Weapon (1981) is mostly an interpretation of the presence of the fission weapon stockpile and its meaning for American foreign relations.[26] Although Herken provides a short discussion of Truman's 1950 H-bomb decision, the most valuable aspect of this work Herken may have provided unintentionally, where he reveals the lack of official policy on thermonuclear weapons while the U.S. maintained an atomic monopoly. This, too, is important to consider in the systems thesis.

Official and Technical Histories

Technical histories tend to focus on the products of the nuclear weapons programs but fail to examine the process of their invention. Still, the technical detail that such studies present is useful information. An unclassified technical history of nuclear weapon *designs* is Chuck Hansen's

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[24]Barton J. Bernstein, "Four Physicists and the Bomb: The Early Years, 1945-1950," Historical Studies in the Physical and Biological Sciences," 18:2 (1988), 231-263.
[25]Richard Sylves, The Nuclear Oracles: A Political History of the General Advisory Committee of the Atomic Energy Commission, 1947-1977, (Ames: Iowa State University Press, 1987).

U.S. Nuclear Weapons: The Secret History.[27] As with Dark Sun, I am prohibited from commenting on the accuracy of the technical content of Hansen's publications in terms of nuclear weapon design or workings according to the DOE's no comment policy on this book. In lieu of this, I will evaluate the not-so-secret characteristics of Hansen's secret history.

An aggressive researcher and well-known military historian, Hansen attempts in U.S. Nuclear Weapons to reconstruct the design of numerous devices from Fat Man bombs to ICBM's. Also in this work, Hansen includes a brief discussion of fusion weapons physics and thermonuclear test series from the Greenhouse series through Operation Dominic. Although U.S. Nuclear Weapons is not a political history, Hansen takes the liberty of condemning the entire nuclear weapons complex. Nevertheless, Hansen's focus on the weapons themselves allows for a very detailed narrative, with the workings of nuclear devices displayed in simple terminology. The actual science of weapons design and development, however, remains a mystery, or in the words popularized by sociologist Bruno Latour, a "black box."[28]

Hansen also produced a more recent and greatly expanded update to U.S. Nuclear Weapons in a CD-ROM format. In researching this, Hansen put the Freedom of Information Act (FOIA) to good use, citing many formerly

[26]Gregg Herken, The Winning Weapon: The Atomic Bomb in the Cold War, 1945-1950, (Princeton: Princeton University Press, 1981).
[27]Chuck Hansen, US Nuclear Weapons: The Secret History, (Aerofax, 1988).
[28]Bruno Latour, Science in Action: How to Follow Scientists and Engineers Through Society, (Cambridge, MA: Harvard Unversity Press, 1987), 2-3.

classified documents. This work, The Swords of Armageddon (1995),[29] is well researched and provides more information about nuclear weapons, and the political scene in Washington surrounding the H-bomb's development, than any other technical history. In addition, Hansen's mutli-volume history discusses the evolution of and innovation in nuclear devices up to the present day. However, he falls prey to the same assumption as Rhodes -- asking why American scientists acted so slow to design and test the first American thermonuclear device. In answering this question, Hansen concurs with Rhodes, placing most of the blame on Teller, without looking at the larger system within which Teller operated.

The organization which operated this large system is the subject of one set of official histories. The Atomic Energy Commission's historians produced a series of works on nuclear weapons R&D and reactor development from, naturally, the AEC's perspective. This series includes Richard Hewlett and Oscar Anderson's The New World,[30] Hewlett and Francis Duncan's Atomic Shield,[31] and Hewlett and Jack Holl's Atoms for Peace and War.[32] While the first and last works in this series address, respectively, the wartime and Eisenhower years, Atomic Shield (arguably the best volume in this collection), examines the early postwar period, the

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[29]Chuck Hansen, The Swords of Armageddon: U.S. Nuclear Weapons Development Since 1945, (Sunnyvale, CA: Chuckelea Publications, CD-ROM, 1995).
[30]Richard G. Hewlett and Oscar E. Anderson, Jr., The NewWorld: A History of the United States Atomic Energy Commission, Volume I, 1939-1946, (University Park: Pennsylvania State University Press, 1962).
[31]Richard G. Hewlett and Francis Duncan, Atomic Shield: A History of the United States Atomic Energy Commission, Volume II, 1947-1952, (U.S. Atomic Energy Commission, 1972).
[32]Richard G. Hewlett and Jack M. Holl, Atoms for Peace and War, 1953-1961: Eisenhower and

formation of the AEC, and its struggle to manage and maintain the odd conglomeration of weapons and production laboratories it inherited from the Manhattan Engineer District (MED), and the thermonuclear weapon project.

Atomic Shield has very broad scope and although not a history of thermonuclear weapons development proper, nor of Los Alamos, it chronicles the development of the AEC and its massive laboratory network, and the many parts crucial to the development of thermonuclear devices. Hewlett and Duncan acknowledge many hindrances to the thermonuclear weapons program, including tritium production, computing, raw nuclear materials, military demands and nascent technologies, and other factors. While this work is an excellent resource for anyone attempting an in-depth study of the AEC or Cold War nuclear weapons R&D, it lacks any critical theoretical framework, in a way that often characterizes official histories.

Hewlett and Duncan's interpretation of nuclear weapons science suffers from a philosophical positivism just coming under criticism by Thomas Kuhn and others at the time Hewlett and Duncan published Atomic Shield: The rise of big science was inevitable, and technology marched onward with its own momentum. Nevertheless, considering that this work is an official history, Hewlett and Duncan show remarkable sophistication in their effort, and they bring to bear on fusion development a host of technical

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the Atomic Energy Commission, (Berkeley: University of California Press, 1989).

and political factors that originated both within and beyond the boundaries of Los Alamos.[33]

A more recent and single-focused history of the AEC during Gordon Dean's chairmanship of the organization is Forging the Atomic Shield (1987), by Roger Anders, a former DOE historian. Anders includes a chapter on H-bomb development and the controversy over it, when Dean headed the AEC. Dean's personal perspective, representing the AEC, is useful, although there is no material present in Anders's book which has not been presented in some form in other histories.[34]

Staying within the borders of Los Alamos is David Hawkins's Project Y: The Los Alamos Story,[35] which focuses mostly on Laboratory organization and administration. Although bland, Hawkins wrote it to serve as the official history of wartime Los Alamos and thus its fatiguing style is understandable. A source for Rhodes, Hoddeson and her co-authors, Hawkins gives a concise but clear overview of Laboratory wartime policy on Super work, confirming that the thermonuclear project received less priority than the fission effort.

Sociology

Although no sociological studies of the Los Alamos thermonuclear project exist, sociologist Donald MacKenzie has explored an equally difficult

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[33]See: Thomas S. Kuhn, The Structure of Scientific Revolutions, (Chicago: University of Chicago Press, 1970).

[34]Roger M. Anders, Forging the Atomic Shield: Excerpts from the Office Diary of Gordon E. Dean, (Chapel Hill: The University of North Carolina Press, 1987).

[35]David Hawkins, Project Y: The Los Alamos Story, Part I, Toward Trinity, (San Francisco: Tomash Publishers, 1988).; Hawkins first wrote this between 1946 and 1947, and the published volume first appeared in 1961 as Los Alamos Scientific Laboratory report LAMS-2532 (Vol. I), "Manhattan District History: Project Y, The Los Alamos Project."

issue -- the relationship between the process of nuclear weapons design and supercomputing. Because I devote a significant portion of this dissertation to the role of computing in fission and fusion bomb development, I will briefly note MacKenzie's 1991 article, "The Influence of the Los Alamos and Livermore National Laboratories on the Development of Supercomputing."[36]

MacKenzie argues that the weapons laboratories, through the practice of computerizing nuclear weapons problems, contributed to the growth of high-performance computing in the 1960s and 1970s because of the increasing complexity of the calculations. Nuclear weapons did, to some degree, create a need for fast electronic computers before this time period -- even as early as 1945 nuclear weapons scientists recognized the value high-speed computing would have for hydrogen weapons calculations. Computing and computers played a significant role in the hydrogen weapon controversy, as MacKenzie suggests, since only with fast computers could the feasibility of the H-bomb be determined in a short amount of time (e.g. weeks instead of years) MacKenzie does not, however, elaborate on how computing's relationship to the Los Alamos H-bomb project.[37]

Participants' Accounts

Some of the Manhattan Project veterans and scientists who participated in postwar nuclear weapons work have published their own accounts of the Los Alamos thermonuclear program. Nuclear scientists' self-

[36]Donald MacKenzie, "The Influence of the Los Alamos and Livermore National Laboratories on the Development of Supercomputing," IEEE Annals of the History of Computing, 13, (1991), 179-201.
[37]Ibid., 186.

understanding of historical events plays an integral role in producing a coherent account of weapons design. Although this is not the whole story of nuclear weapons science, the scientists' accounts are worth discussing briefly. In addition, the discrepancies found between various scientists' accounts of the thermonuclear project help to reveal accurate sequences of events when compared with archival documents concerning the program.

Teller has written a great amount on the early thermonuclear program. One of his most enlightening pieces is "The Work of Many People," appearing in Science[38] in 1955. Some historians have speculated that Teller wrote this as a means of atonement for his role in Oppenheimer's security hearing only the year before. Regardless of Teller's motives, he credits a large number of Los Alamos personnel for their contributions to the thermonuclear effort. Nearly limitless in his praise of Los Alamos's staff, Teller applauds physicist Robert Richtmyer for his work on the Super weapon throughout the latter 1940s. Teller equally praises Oppenheimer's successor as Los Alamos Scientific Director, Norris Bradbury, for his determination to keep Los Alamos operating after the war. Teller gives an apparently accurate chronology of events in the Los Alamos thermonuclear program (which seems to jibe with similar ones given by Hans Bethe and Carson Mark, both of which I review shortly). Aside from this, Teller illustrates an important point missed in much of the popular literature on the H-bomb project -- it was indeed the "work of many people."

[38]Edward Teller, "The Work of Many People," Science, (121), February 25, 1955, 267-275.

Opinions can change with time. By comparing two of Teller's personal accounts of the hydrogen bomb, contradictions appear. Teller revised "The Work of Many People" for publication in his The Legacy of Hiroshima (1962).[39] In this later version Teller claims that the period from 1945 through 1948 saw almost no support for thermonuclear work.[40] His criticism of Bradbury is obvious, as Teller implies that the director did not want to support any H-bomb research in the postwar years. Teller glosses over the technical problems his original Super design embodied, hinting that certain individuals hostile to the thermonuclear effort caused its delay. Because of its overwhelming political slant, The Legacy of Hiroshima is of little use to the historian, essentially fizzling like it's author's Super theory.

Physicist Bethe presents his personal view on thermonuclear development in his "Comments on the History of the H-Bomb,"[41] originally published as a classified article in 1954. In explaining why the theoretical thermonuclear program went at a slow pace at postwar Los Alamos, Bethe emphasizes the Laboratory's uncertain future and mission at this time. Notably, the temporal judgment is ambiguous and, also contrary to Teller's account, Bethe asserts that "work on thermonuclear weapons at Los Alamos never stopped."[42]

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[39]Edward Teller, The Legacy of Hiroshima, (Garden City, NY: Doubleday, 1962).
[40]Ibid., 42.
[41]Hans A. Bethe, "Comments on the History of the H-Bomb," Los Alamos Science, (Fall, 1982), 43-53.; This piece was originally published as a classified article in 1954.
[42]Ibid., 46.

In his autobiography, Adventures of a Mathematician (1976), Stanislaw Ulam devotes a significant portion to the Super configuration and other thermonuclear work at Los Alamos. Ulam discusses what has been a huge source of controversy among the nuclear weapons science community, and a question that is still raised among historians of nuclear weapons: To what degree did Teller and Ulam each contribute to the workable thermonuclear configuration tested in 1952? While this is a worthy question, it is too narrow, as credit for what is often called the "Teller-Ulam" device belongs to more scientists than just Teller and Ulam.[43]

Priority issues aside, Ulam's account is deeply personal. In one passage, he describes young Teller upon first meeting him, as youthful, warm, and ambitious. Sometime during the war, however, Ulam sensed that Teller changed and wanted his own stamp on much of the essential work at Los Alamos. Ulam's description of the postwar Los Alamos Super program confirms Bethe's assertion: Work on thermonuclear devices had been going on efficiently and systematically from the end of the war through the late 194Os, as the subject of several scientists' theoretical efforts.[44]

Physicist Herbert York did not participate in the wartime atomic project, but worked at the University of California Radiation Laboratory with physicist Ernest Lawrence and Frank Oppenheimer, working on separating uranium isotopes. York participated in Operation Greenhouse in 1951 and

[43]Stanislaw Ulam, Adventures of a Mathematician, (New York: Charles Scribner's Sons, 1976), 149-150.
[44]Ibid., 210.

soon after became the first director of Lawrence Livermore Laboratory. York's account of the development of thermonuclear weapons, The Advisors: Oppenheimer, Teller, and the Superbomb (1976),[45] incorporates a general technical discussion of this program, a brief history of the Russian atomic bomb, and the well-known debate between the Atomic Energy Commission and its General Advisory Committee over the development of a hydrogen weapon.

York's account is factually accurate, but like so many other authors, he judges that work on the "superbomb" at Los Alamos went slowly between 1946 and 1948. Certainly, when compared to the period after 1949, Los Alamos's scientists worked less intensely on hydrogen weapons, and thus, "work" performed on the H-bomb is a relative quality.

York provides this background to set the stage for his actual goal in this study, an analysis of the arms race through counterfactual history: York concludes that if President Truman had followed the advice of the General Advisory Committee not to develop a thermonuclear weapon, and instead directed the improvement and further development of existing atomic bombs, international arms control would have been within reach.

York's assertion that President Truman's decision was pivotal in the effort to develop the H-bomb is, although correct, too simplified. Networks of individuals and groups strongly influenced Truman's thinking on the H-bomb issue. Many political leaders, organizations, and scientists had vested

[45]Herbert F. York, The Advisors: Oppenheimer, Teller, and the Superbomb, (Stanford:

interests in the thermonuclear project, including the Congressional Joint Committee on Atomic Energy and particularly its Chairman, Senator Brien McMahon. The Joint Chiefs of Staff, the Air Force, and Lewis Strauss of the Atomic Energy Commission, along with many scientific participants in the H-bomb project were also influential. As with his earlier decision to drop the atomic weapons on Hiroshima and Nagasaki, Truman's 1950 "decision" to continue work on this project reflected overwhelmingly the interests of these individuals and groups, which I discuss later in this dissertation.

Whether or not international arms control would have been attainable in 1949 is a difficult speculation and impossible to determine. Moreover, such speculation does not explain the numerous complications behind the H-bomb's development. One not-well-known short history that is centered around the technical problems facing thermonuclear development is J. Carson Mark's, "A Short Account of Los Alamos Theoretical Work on Thermonuclear Weapons, 1946-1950."[46] Mark served as T Division leader for most of the period covered in this paper, which he originally wrote in 1954 as a classified report. Like Bethe and Ulam, Mark asserts that many physicists completed a considerable body of theoretical work on thermonuclear weapons between 1946 and 1950. If the H-bomb work was hindered, Mark contends, several technical and non-technical bottlenecks that Hewlett and

Stanford University Press, 1976).

[46] J. Carson Mark, LA-5647-MS, "A Short Account of Los Alamos Theoretical Work on Thermonuclear Weapons, 1946-1950," (Los Alamos Scientific Laboratory, 1974).

Duncan also acknowledge -- tritium, computing, military technologies, and a shortage of labor, made up the stumbling blocks.

Although accurate in his account, Mark fails to explain that many of these bottlenecks originated outside of Los Alamos, in the larger system, although Mark deliberately emphasizes the Laboratory's theoretical program above all else. Moreover, Mark's piece is mainly a chronology of early work done in Los Alamos's T Division on thermonuclear weapons and, as a chronological reference it is valuable.

Physicist Robert Serber is best known for his role in the wartime atomic project, in particular for his work on neutron diffusion calculations and also for giving the introductory lectures on fission weapon theory when Los Alamos opened. Although he did not participate in the H-bomb project, in The Los Alamos Primer (1992) Serber gives a brief but lucid account of the origins of the Super thermonuclear theory and explains that early on, even before the war, the problems inherent in the theory were so complicated that it "never would work."[47] Still, Teller, and several of his other colleagues believed otherwise and even today there is still disagreement among nuclear weapons scientists as to the viability of this theory. For the most part the Super remained Los Alamos's "thermonuclear program" for many years, and its fate depended on the AEC.

Cultural Histories

No review of thermonuclear weapons studies would be complete without acknowledging two of the most widely regarded histories which examine the social-cultural effects of the nuclear age and weapons industry: Spencer 's Nuclear Fear (1988), and Paul Boyer's By the Bomb's Early Light (1985).[48] Although neither study attempts to deconstruct nuclear weapons as technological or engineering products, both Weart and Boyer examine in a broad sense nuclear weapons as images and modern mythologies in the American mind. As the American nuclear weapons complex gave prioritized hydrogen weapons production over fission devices, H-bombs no doubt became icons of the Cold War era.

A Technological System of Weapons Research and Development

Icons tend to remain surrounded by mythology, just as the historical literature has not represented nuclear weapons as technologies very well. Most studies concerning hydrogen weapons focus on the political and moral controversies surrounding their initial development. This is not without good reason as nuclear weapons remain one the most politically charged issues in international relations of this century. No literature, however, focuses on the scientific and technical processes of early H-bomb development to determine how who and what influenced the technological products, and why scientists chose certain weapons for developed and not others.

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[47]Robert Serber, The Los Alamos Primer: The First Lectures on How to Build an Atomic Bomb, (Berkeley: University of California Press, 1992), xxxi.
[48]See footnote 12 for complete references.

MacKenzie and Wajcman comment, "Social scientists have tended to concentrate on the effects of technology," and on the impact of technological change on society. But they argue that few social scientists have posed a "prior and perhaps more important question: What has shaped the technology that is having effects?" The case of hydrogen bomb development requires just this sort of inquiry.[49]

Instead of examining the thermonuclear project in terms of how long it took, this study explores the many factors that shaped this project from its proposal until the first full-scale H-bomb test. This study provides a potentially stronger historical analysis and may account for many other influences than time. Obstacles to this project abounded, yet they varied in degree of importance between 1942 and 1952. In order to reconstruct accurately the history of the early hydrogen weapons project, it is important to recognize when nuclear weapons scientists themselves first cited tritium, computing, lack of human labor, and other factors as critical problems to a hydrogen weapon. These bottlenecks came from different sources, for example: the AEC, Los Alamos, and the military.

Due to the complexity of the hydrogen bomb project, most historical studies have failed to account for all the different aspects of the project, because problems befalling the program did not appear sequentially; so many problems and events overlapped that some, particularly the more technological parts of the history, have never been acknowledged much less

[49]MacKenzie and Wajcman, Social Shaping, 224.

interpreted. While my historical approach is largely narrative I have chosen a case study-oriented chapter by chapter arrangement in order to elaborate several of the problems facing the thermonuclear project.

By viewing the American thermonuclear effort as part of a technological system more technical problems, as well as more politically or socially based issues, may be accounted for. Established officially in 1947, the AEC and its sprawling network that included laboratories, private industries, universities, and federal government constituted a large technological system. It had a precedent, though: the AEC became the successor system to the Manhattan Engineering District (MED) that General Leslie Groves established for the sole purpose of building a fission weapon.

According to Hughes, people within technological systems attempt to solve problems or fulfill goals. In his study of the electric utility industry, Hughes describes Thomas Edison and Samuel Insull as two important drivers behind the electrification of America. During World War II, the MED system had a clear, military-driven goal (with Groves at its helm), centered around a single mission of providing a limited number of fission weapons for the war effort. The AEC leadership's goals were not so well-organized and mission-oriented. In the postwar era, the AEC maintained a loose agenda regarding work on fusion weapons.[50]

Partly because of this lack of clear policy before 1950, most of the initiative to work on thermonuclear weapons problems came from Los

Alamos's scientists, and thus H-bomb work remained essentially confined to this one part of the system -- the Laboratory and mainly its Theoretical Division. Prior to 1950, a small group of theoreticians and Laboratory consultants led nearly all theoretical work on the Super and some technical alternatives to it. Only after the Soviet atomic test in 1949 did the top leaders of the AEC, Defense Department, and Congress start to bring official pressure to construct a hydrogen device. Although aware of many problems facing the H-bomb project as early as World War II, with a new political goal to attain a hydrogen device as soon as possible, scientists acknowledged the gravity of the technical problems facing the Super.

From its inception, Los Alamos's scientists held most of the direct scientific responsibility for the H-bomb project. Examining the thermonuclear project from the perspective of Los Alamos necessitates a study that focuses mainly on one of the AEC's laboratories and not the entire large system, as Hewlett and Duncan did in Atomic Shield.

In analyzing the Los Alamos thermonuclear program from the perspective of Los Alamos it would, however, be impossible to treat the weapons laboratory and those working within it as a completely independent entity from the AEC. Isolated only geographically, Los Alamos could not have functioned nor developed a workable thermonuclear weapon without the support of the AEC.

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[50]Thomas P. Hughes, "The Electrification of America: The System Builders," Technology and Culture 20, (1979) 124-61.

Indeed the system was crucial to thermonuclear research and development. In the course of adopting Hughes's systems theory as a historical framework, some of the terminology that goes along with this theory is confusing. As it evolves and grows, a system faces technical, political and social problems or barriers when attempting to reach its goals. The barriers and array of problems themselves become historical focal points when employing the systems framework because an important part of the historical story is how scientists and engineers solve these dilemmas.

When discussing systems' evolution and growth, Hughes employs the term "reverse salient." Reverse salients, on the other hand, refer to "an extremely complex situation in which individuals, groups, material forces, historical influences, and other factors play a part."[51]

Hughes argues that the appearance of a reverse salient suggests the need for invention and development if the system is to meet its builders goals and grow. Reverse salients draw attention to those components in a growing system that need attention and improvement. To correct the reverse salients and bring the system back in line, scientists and engineers may define the reverse salient as a set of "critical problems" which need solution. [52]

Although Hughes's concept of reverse salients is well-known among historians of technology, I prefer the term "critical problem" (which I will use

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[51] This term comes from the tradition of battle theory. It is used to describe a section of an advancing front or battle line continuous with other sections of the front, but which has been bowed back.; see Hughes, Networks, 79. Hughes notes that "reverse salient" became a household expression during World War I because of the struggle of the Germans to eliminate the reverse salient along the western front at Verdun.

[52] Hughes, "Evolution of Large Technological Systems," 73.

synonymously with the term "bottleneck") for the purposes of this study. Because I concentrate on one laboratory within the AEC system and its members' efforts towards thermonuclear bomb development, discussion of the critical problems which scientists and engineers faced, rather than their definition of reverse salients, is more appropriate for this study. The reverse salient idea -- which implies that the entire system is restrained or held back from growth -- is simply too broad for this analysis. In the case of hydrogen bomb development some very specific critical problems can be identified. [53]

Los Alamos scientists' recognition of specific critical problems in the thermonuclear project influenced the specific technological choices that weapons scientists made. Furthermore, critical problems were not just technical problems: as I will discuss later in this dissertation, people themselves can create or be part of a critical problem to a scientific program.

Goal of This Study

In the Los Alamos thermonuclear program, it is easy to identify several critical technical problems. Other, more socially-based problems present

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[53] Hughes, Networks, 81.; Although intriguing, the reverse salient idea is problematic and confusing as a tool for historical analysis. As historian Edward Constant has pointed out, Hughes does not explain how reverse salients are parsed into critical problems that attract the attention of practitioners.; Edward W. Constant, II, "The Social Locus of Technological Practice: Community, System, or Organization?", in Bijker, Pinch, and Hughes, Social Construction, 223-242.; It is not clear if the abstract reverse salient or the more concrete critical problem is identified first by practitioners, or which is more important in the overall system. The reverse salient, then, may be more useful as a metaphorical tool for picturing the progress of a large system, than truly representing a problem or glitch in the system. As noted earlier, accurate historical reconstruction of when scientists and engineers recognize a problem is necessary to avoid historicism. The critical problems themselves, may be more important for the historian to focus on than the more general concept of reverse salients, since the more concrete critical problems are simply easier to identify as hindrances to the goals of a technological system. In addition, compared with the reverse salient, the term "critical problem" is less restrictive when identifying individual problems within a system.

themselves more subtlety over time, but become apparent when examining the system builders and other important human characters in the H-bomb project, and the choices they made in weapons development. In this dissertation I examine case studies of several critical problems to the early Los Alamos thermonuclear program, particularly the Super project.

I demonstrate that the early fusion weapons program at Los Alamos entailed a drastically more complex scientific and technical endeavor than previous studies have revealed: Not only is the project impossible to explain simply in terms of government and scientific politics, but I argue that for the majority of the program's existence, thermonuclear weapons were a non-political issue. I demonstrate why the thermonuclear project was severely problematic technologically and socially in the senses of: how scientists themselves viewed the project; its high-level of secrecy; and the military's relationship to the project. I also show how decisions (such as President Truman's) regarding research and development of a hydrogen device cannot be broken down into strictly political issues such as a power-struggle between Teller and Oppenheimer, and the AEC Commissioners and the GAC.

On the other hand, technological determinism did not produce hydrogen weapons; technologies do not develop independently of their environment and social surroundings. One of the best aspects of the technological systems approach is that it emphasizes the role of humans in the development of technology, whether they are solving problems or creating them. Weapons scientists found solutions to technical problems within the context of their social environment. Solutions to the critical technical problems helped bolster the program's speed, and in one case, scientists discovered a labor-saving tool in computers.

Weapons scientists did not have to solve absolutely every critical technical problem that arose in order to develop a working hydrogen weapon. When they found unsolvable problems, scientists bypassed them or pursued new theories of fusion weapons. As I will demonstrate, in some instances the support technologies, such as reactors for example, which scientists had to work with in the 1940s led to shifts in the theoretical weapons program.

I will illustrate how the thermonuclear project may be viewed somewhat as an outgrowth of the wartime fission project, and in other ways evolved into a completely separate project governed by a separate technological system than the system originally established under the Manhattan Engineer District. The Super theory predated the MED system, and survived even when the fission device became the main goal of the Manhattan Project. Several Los Alamos personnel explored the Super configuration during and after the war, but the Super, and other H-bomb theories received scientific attention mostly within the AEC system.

Of those scientists who pursued thermonuclear research in the postwar period, the majority worked in Los Alamos's T Division, because prior to 1951 most work on the project was theoretical and mathematical. Therefore, many of the scientific characters I discuss in this study are either mathematicians or physicists. The theoreticians, however, did not build the weapons technologies, and I do not want to dismiss the importance of the many chemists, metallurgists, engineers, and technicians on the project -- although their role became crucial when the AEC and Los Alamos drastically re-oriented the hydrogen bomb program in 1951. Without all of the scientific and engineering personnel, the 1952 Mike test would not have been possible.

A study of the early American hydrogen weapons project allows for unique insight into the relationships between science and technology, and theory and experiments. Nuclear weapons design is a peculiar process that evolved in the Second World War, and is still undergoing evolution presently. The wartime fission project was initially theory-based, followed by engineering and testing. After the war this sort of progression in fission research and development was not so linear and one-directional. The hydrogen weapons program evolved in a similar way, but experiments preceded new theories in some instances, insuring a complicated science-technology relationship in the thermonuclear project.

Finally, in the course of examining Los Alamos's attempts to develop a thermonuclear device, I wish to shed light on the practice of a top secret and extremely "black-boxed" science, to understand what social, technical and political forces shaped early nuclear weapons technologies. In this study, I attempt to use as many of the original sources on thermonuclear weapons work as possible as the basis for an interpretive history of a traditionally closed scientific and technological enterprise.

Chapter Organization and Summaries

To explain the history of the early Los Alamos thermonuclear program in terms of the technological systems thesis, I have organized the remaining chapters into the following order:

Chapter Two summarizes Los Alamos's wartime atomic project, and it serves as a prologue to the subsequent examination of the Super project. During the war, Los Alamos Laboratory emerged as a unique component of MED system. Within this system, Los Alamos's scientists made a technological choice to build a fission weapon instead of a fusion device. However, by the war's end enough theoretical work had been done on the Super that weapons scientists recognized several technical obstacles to this type of thermonuclear device. Weapons scientists did not yet consider these technical obstacles critical problems. Scientists did not yet actively seek solutions to them; the fission device took first priority and would require development anyway to ignite the Super.

Also in Chapter Two, I discuss the wartime origins of one particular bottleneck to the fission (and later, fusion) program -- computing. During the war, "computing" meant hand calculations with Marchant, Friden and Monroe desk calculators, and later, IBM punched card machines. Scientists recognized that hand computers could not calculate a uranium gun device, and they solved this problem by seeking a different and partly automated technological solution. Employing punched cards in the fission program, though, suggested that they might be used for calculations related to the Super as well.

The next chapters consist of case studies of specific critical problems to the early thermonuclear program. I discuss the conception and evolution of the Super and other subsequent thermonuclear theories along with the origins of critical problems to the former. I show how scientists came to acknowledge and solve these problems, if at all. Notably, some critical problems did not always have direct or easy solutions, and the system builders and key participants deliberately had to change the goals of the system.

Chapter Three examines the origin of the Super theory and its early design. Although weapons scientists had little opportunity to work on this theory during the war, the idea survived. Before the war's end, Teller and others recognized that computing all the complex effects and processes for the Super (deemed the "Super Problem") would require machinery at least as complex as punched card machines. Weapons scientists used their own personal networks to make sure that new electronic computing technology would be available for the Super calculations.

After the war, work on the Super never completely stopped: Several scientists proposed a number of projects specifically to solve the Super Problem. Others conducted calculations with the dual purpose of benefiting both the fission and fusion programs. Still, in the postwar period many scientists believed that determining whether or not the Super would actually work required computer power that did not yet exist. In part, this lack of computational power helped initiate a computer construction project at Los Alamos.

Chapter Four traces a previously little-studied aspect of nuclear weapons design that grew into a serious critical problem for the thermonuclear program: Nuclear materials -- their availability, cost, ease of production, and efficient use were important considerations for weapons scientists from the war years on. In part, the wartime program changed from a plutonium gun device to an implosion gadget in the interest of efficient use of nuclear materials. In the postwar Super project, materials became an even bigger consideration, and emerged as one of the chief critical problems to this design. The Super needed rare and expensive-to-produce tritium in order to work. Not only would the Super consume more tritium than the amount available to the weapons laboratory, but for the AEC, producing this isotope constituted an arduous and expensive process. Moreover, few nuclear materials production facilities operated, and were limited in their capabilities; in the 1940s and early 1950s they could produce either tritium or plutonium, but not both. Plutonium fueled Los Alamos's fission weapons, the Laboratory's main technical focus in the postwar period. The Super project could not compete for precious nuclear materials with the already more well established fission program. Therefore, in a broad sense, the fission program itself became an obstacle to thermonuclear development in the 1940s.

Chapter Five looks at other less obvious, although important problems which bore upon the thermonuclear program. These problems originated both in and outside of the AEC system, and from within Los Alamos itself. The most blatant technical critical problem originated in the Armed Forces. In the 1940s, the Air Force did not possess a delivery vehicle for the Super. The original Super design was simply too large for any aircraft of 1940s vintage to carry. In addition, if an aircraft at that time could deliver a Super bomb, the plane and crew would likely be sacrificed due to the tremendous blast from the weapon. On the other hand, missile technology had not advanced far enough to carry a Super.

Other bottlenecks to the Super were not so technical. Regardless of the lack of military aircraft technology, no branch of the military specifically requested a thermonuclear device until the 1950s. With no customer for a hydrogen weapon, the AEC and Los Alamos placed thermonuclear development on a lower priority level than fission bombs.

The nuclear weapons laboratory had internal social problems, as well. Heading up a the weapons design portion of the AEC system, Los Alamos's leaders faced difficulties in the course of maintaining the Laboratory's immediate political goal after the war -- staying open. Los Alamos could only do this by focusing -- as I show -- on one, not several, technical products. The technical agenda within the laboratory, then, aimed to provide new and improved fission devices, not hydrogen bombs.

Other problems appeared. After the war Los Alamos suffered from a lack of personnel, as most senior scientists and many of their junior colleagues departed. In addition, the temporary wartime buildings at Los Alamos decayed rapidly after the war, and the community suffered for several years from a housing shortage when it became possible to hire new staff. As a result, few new personnel were available to work on projects like the Super. This problem went back to the AEC, which ultimately provided the funding for construction projects within the system. Last, besides Los Alamos's uncertain future at the end of the war, the Laboratory had to establish a new mission, having lost its wartime goal.

When the Laboratory managed to establish a new mission, Los Alamos's leaders and the GAC regarded fission weapons as having higher priority over fusion devices at this time. The Laboratory's mission again changed, though, in the wake of the Soviet atomic test in 1949. In Chapter Six, the conclusion, I review the case studies of critical problems to the H-bomb project. I also review Los Alamos's program transition from the Super as the preferred hydrogen configuration to the Teller-Ulam configuration. In doing this, weapons scientists handled the critical problems to the Super, along with responding to the official directive to produce a hydrogen weapon, by re-inventing the H-bomb, and choosing a new technology.

Furthermore in Chapter Six, I also argue that by analyzing the Los Alamos hydrogen bomb project in terms of technological systems, this history provides a balanced view of the technical and social factors, and also draws together many of the fragmented discussions of the political Super controversy, the role of scientists, and the desires of high military command, in order to give a more complete account of the practice of nuclear weapons science. Through this kind of analysis, I explain why other authors have posed the wrong question, "Why did hydrogen devices take so long?" in their respective studies of the project. Finally, I make suggestions for further studies.

Chapter Two

The Fission Bomb Had to Come First

In a 1958 review of Robert Jungk's then newly published Brighter Than a Thousand Suns, Hans Bethe was among those public figures, if not the first, to employ the term "big science" to characterize large-scale, government-sponsored postwar era American scientific and technical research and development. Since then the term "big science" has not only become commonly used by historians of modern science, but has itself been the subject of many studies, beginning with Derek de Solla Price's Little Science, Big Science (1963).[54]

Historians have often acknowledged the Manhattan Project as unprecedented in scale and budget, and as the beginning of big science in the United States. This attribution is misleading. Large-scale government and corporate sponsored research began to evolve in the 1930s at such Institutions as the California Institute of Technology, Stanford University, and the University of California at Berkeley. Physicist Ernest Orlando Lawrence promoted this type of research prior to World War II. Lawrence aggressively sought funding from private industry such as the Pelton Waterwheel Company, and from the Federal and California state governments for his

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[54] Hans A. Bethe, review of Brighter Than a Thousand Suns, by Robert Jungk, In The Bulletin of the Atomic Scientists, 14: (1958), 426-428; Peter Galison, "The Many Faces of Big Science," in Big Science: The Growth of Large Scale Research, (Stanford: Stanford University Press, 1992), eds. Peter Galison and Bruce Hevly, 1-17; Derek J. de Solla Price, Little Science, Big Science, (New York: Columbia University Press, 1963).

cyclotron work at Berkeley in the 1930s. Similarly and before the war, physicists at Stanford obtained resources from the Sperry company for work on microwave technology.[55]

In the early years of the twentieth century American physics needed a patron, and found one in industrialists. Historian Daniel Kevles described Lawrence as the "public personification of physics." Lawrence became involved in the atomic weapon project at its beginning, taking the initiative to build a large, 184-inch cyclotron in hopes that it might be useful in designing an industrial-scale Uranium separator. By 1942 Lawrence and his team at Berkeley understood the specifics of building an electromagnetic separator, experimenting with various magnets. AEC historians Hewlett and Anderson note that "Lawrence had swept his laboratory clean of the customary patient research into Nature's laws . . . he demanded results above all else." Moreover, Lawrence's style of scientific research influenced the character of the Manhattan District because the Berkeley physicist became involved early on in building the MED system, based on his cyclotron construction projects.[56]

The majority of big science conducted after the Great Depression had military purposes. Even though the Manhattan District and the postwar era nuclear weapons complex that evolved out of it made up no small part of

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[55] Galison, "The Many Faces of Big Science," 3; John Heilbron and Robert W. Seidel, Lawrence and His Laboratory: A History of the Lawrence Berkeley Laboratory,Volume I, (Berkeley: University of California Press, 1989).

[56] Daniel J. Kevles, The Physicists: A History of a Scientific Community in Modern America, (Cambridge: Harvard University Press, 1987), 271, 280; Hewlett and Anderson, The New World, 141.

this, large-scale research included numerous projects and organizations other than nuclear weapons development. Very large budgets characterized many postwar period research projects, although not all project managers found sponsorship in the American government. Instead, some projects received sponsorship from private industry. Furthermore, big science occurred in many different environments, for example, at public and private universities, at private corporations, and at federally-sponsored laboratories.

The American nuclear weapons complex, with its many design, production, and assembly facilities, in addition to private contractors, and academic and university affiliations, defies characterization merely by the all-encompassing phrase "big science." Furthermore, this phrase does not reveal the nuclear weapons laboratories' mission of turning out specific technological products for the military, nor the extent of their technological dimension. Finally, categorizing nuclear weapons work as merely big science is not an accurate description of this activity, since it does not help to explain the dynamics of changes within the weapons programs, nor the history of specific projects in this area, such as the early thermonuclear bomb program.

Any study of nuclear weapons development faces the intractable problem of the giant and labyrinthine character of the American atomic energy establishment. Secrecy aside, no study of reasonable length would be able to analyze in an integrative manner all of the numerous facilities and government and military organizations involved with nuclear weapons work at any given time. Therefore, focusing on case studies of specific projects and laboratories within the American nuclear weapons complex provides the most practical means of exploring this establishment.

As already suggested in Chapter One, I will employ Hughes's technological systems thesis as a general framework for analyzing several case studies of critical problems to the early thermonuclear program. In this chapter I will: (1) discuss the founding of the MED and its establishment as a technological system, and introduce several of the system builders, (2) show Los Alamos's founding as part of the MED, (3) highlight a case study of one of the most critical problems Los Alamos faced during the war -- calculating atomic weapons. The case study is appropriate for several reasons. First, mathematical calculations were necessary to predict the overall behavior of nuclear devices and the feasibility of proposed designs, which is why scientists began computations such as cross sections of nuclear materials even before settling Los Alamos. Second, during the course of the war nuclear weapons scientists came to view computing, in the form of punched card machines as a labor-saving technology. Scientists identified computations for nuclear weapons as a critical problem during the war. The final topic I discuss in this chapter is the AEC's founding and Los Alamos's place in this system. Los Alamos's leaders fought for the Laboratory's survival after the war, and also for autonomy in their weapons research and development projects. Understanding both the roots and evolution of the AEC and Los Alamos's place within the Commission provide a prologue to an accurate historical account of the thermonuclear weapons project.

The Manhattan District as a Technological System

In American Genesis Hughes characterizes the Manhattan District as a large technological system, similar in form to other large systems in the private sector; the relationships between scientists, engineers, and managers in inventing and developing the atomic bomb were analogous to relationships encountered in earlier innovative production at General Electric, AT&T, and DuPont. Hughes also attributes particular features to the Manhattan District that set it apart from these and other systems. According to Hughes, in the Manhattan Project the military played the role of system builder and the federal government sponsored the project, since no one system builder led the project. In contrast, large companies and public utilities such as the electric power industry that individual system builders such as Samuel Insull built up.[57]

Following Hughes, my interpretation of the Manhattan District's leadership would spot Brigadier General Leslie R. Groves as the most likely candidate for system builder of the MED, although Hughes argues the contrary, stating that Groves could not have fulfilled the role because he did not provide the inspired technical leadership given by, for example, Henry Ford in building his automobile empire. Furthermore, Hughes believes that Groves did not "elicit a collective creativity during the Manhattan Project similar to that of which Ford had stimulated at the Highland Park plant as the assembly-line system had evolved." Hughes argues that the problems facing

[57] Hughes, American Genesis, 383; Hughes, Networks of Power, passim.

the effort to build atomic weapons were too complex and the knowledge and skill needed to solve them too specialized for any individual to assume the singular role of system builder.[58]

If the military played the system builder in the Manhattan Project, then Groves clearly led the military in the effort to develop atomic bombs. The military provided a structural framework for the project; Groves organized the project in a military fashion, evident in Los Alamos's hierarchical structured with Oppenheimer in command. In addition, as Hughes correctly states, committees -- not individuals -- often made decisions about the Manhattan District. However, Hughes does not acknowledge that several important individuals stand out as recognizable leaders and system builders in the MED.[59]

Systems can have more than one builder. The size and scale the Manhattan Project would suggest that several system builders were involved in achieving the goal of developing atomic weapons. Towards this effort, several system builders emerged over the course of the war: Groves and Oppenheimer are the most well-known, but Lawrence built the system too. Each individual played different roles in the MED yet provided leadership in attaining the same ultimate technological goal, and had extraordinary influence on the course of the atomic project.

As the late historian Stanley Goldberg stated, "Most Manhattan Project retrospectives simply overlook Grove's importance." The General brought

[58] Hughes, American Genesis, 385.

many components into the Manhattan Engineer District, including the DuPont corporation in order to build plutonium separation plants, and Tennessee Eastman to operate the electromagnetic (Y-12) plant at Oak Ridge. As military head of the atomic project Groves made contracts with numerous industries to build plants and equipment for nuclear weapons research and development. Goldberg summarized, "Groves was despised and hated by many of those who had to work under him . . . . [H]e drove people mercilessly to get the job done." He both fostered and oversaw an all-out attempt to complete construction on materials production facilities and weapons design work and fabrication in only a few years.[60]

More directly responsible for Los Alamos and its technical program, Oppenheimer served as scientific head of the atomic project. Although Los Alamos operated hierarchically in a quasi-military fashion Oppenheimer allowed some research freedom as long as it did not hinder work on the fission weapons. Oppenheimer had to oversee several technical divisions with large numbers of staff members, as well as direct course of the project and alter its ultimate technical goals out of necessity to meet deadlines. Moreover, he had to coordinate Los Alamos's atomic weapon research efforts with the demands of the other parts of the MED system, and direct procurement of necessary technical equipment. Oppenheimer had to reorganize Los Alamos rapidly to best suit changing goals, thus rearranging

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[59] Ibid., 385-386.
[60] Stanley Goldberg, "Groves Takes the Reins," The Bulletin of the Atomic Scientists, (December, 1992), 32-39; Hughes, American Genesis, 392-402.

entire divisions and their personnel. While committees often made technical decisions at Los Alamos, Oppenheimer still had to direct all of the program changes, and maintain ultimate responsibility for all work, including the most crucial prerequisite to producing an atomic device -- calculating it.

Although Los Alamos could not have produced two different atomic devices within a short period of 3 years without all of its specialized divisions, the Theoretical (T) Division played an especially significant role because its members modeled the proposed weapons, and early in the project had to estimate mathematically calculable properties of fissionable materials. Oppenheimer depended on T Division's estimates of critical mass and efficiency, necessary prior to actual physical bomb design and the production of fissionable materials for the weapons.[61]

Initially, T Division had to estimate neutron diffusion. Hoddeson and her co-authors note that:

> . . . The members of T Division . . . had to create approximate numerical solutions and develop a sense of how the results depended on parameters, to enable extrapolation into new physical regimes. They had to balance the need for speed against the need for accuracy. . . . As illustrated in neutron diffusion calculations, T Division's primary strategy was to make the best possible calculations based on as many

[61] Hoddeson, et. al., Critical Assembly, 408; The critical mass is the amount of material from which neutrons disappear by leakage and nuclear capture at the same rate at which they are born from fissions that occur in the mass, which will just maintain a fission chain reaction; David Hawkins, Project Y, 4; Hansen, U.S. Nuclear Weapons, 13; "Neutron diffusion" is the way which neutrons distribute themselves in a critical mass of nuclear materials. "Efficiency" is the fraction of energy released in an atomic explosion relative to that which would be released if all the active nuclear material were transformed into explosive energy. Efficiency is calculated by dividing the actual yield by the predicted yield; Serber, Primer, 38; Hawkins, Project Y, 65-66, 77; Hansen, U.S. Nuclear Weapons, 14.

known factors as possible, employing extrapolation, approximation, and simplification. . .[62]

Neutron diffusion work had actually commenced the summer before Los Alamos opened, as these problems required solution before any engineering of the weapons could begin. The critical mass and efficiency calculations proved so difficult that T Division's scientists chanced to employ punched cards to speed their work. Scientists took a technical initiative on their own, without an MED's directive, to improving the way that wartime weapons calculation techniques.

Unlike Oppenheimer or Groves, Ernest Lawrence remains one of the least acknowledged MED leaders and system builders, particularly when it came to nuclear materials production. Lawrence's successful experience of aggressively securing federal and state government funding and corporate support for his cyclotron projects in the 1930s benefited his role in the atomic project. Seeking even more funding in the following decade, in 1941 Lawrence offered the Radiation Laboratory's services to James Bryant Conant, head of the S-1 (Section One) Committee of the Office of Scientific Research and Development (OSRD). In charge of studying the properties of uranium, the S-1 Committee took up Lawrence's' offer to experiment with separating U^{235} from U^{238} so that this process could subsequently be done on an industrial scale. Towards this effort, Lawrence vigorously recruited young physicists and graduate students to join the Radiation Laboratory. With the results of

[62] Hoddeson, et. al., Critical Assembly, 408.

work on the 184-inch cyclotron in hand Lawrence convinced Groves and the Stone and Webster engineers to begin construction on a giant, industrial-size electromagnetic separation facility (Y-12) at Oak Ridge, Tennessee in the fall of 1942.[63]

Calculating Atomic Devices: A Critical Problem for Los Alamos

Lawrence's cyclotron projects themselves did not just influence the style of the Manhattan District and the construction of the Y-12 plant. The location Lawrence had chosen to undertaken his early cyclotron work mattered too. Lawrence's Radiation Laboratory, at the up and coming Berkeley physics department, had already by this time became a mecca for young physicists. In 1929 Berkeley had attracted J. Robert Oppenheimer, who chose Berkeley over Harvard while simultaneously accepting a joint appointment with Caltech. Oppenheimer chose Berkeley as the site for a 1942 theoretical physics conference to discuss the theory of a fast-neutron reaction, and ponder the design of an atomic weapon.[64]

To the Berkeley summer conference Oppenheimer invited a group that he later nicknamed the "luminaries," who were supposed to "throw light" on atomic design. The participants included some the most well known scientists in the U.S.: Cornell physicist Bethe, Stanford theoretician Felix Bloch, Indiana theoretician Emil Konopinski, Hungarian physicist Edward Teller, Harvard physicist John H. van Fleck, and his former student Robert Serber.

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[63] Hewlett and Anderson, The New World, 141-143; Rhodes, Atomic Bomb, 376.

Knowing Oppenheimer since 1934, Serber had previously been appointed to Berkeley on a postdoctoral National Research Council fellowship, then subsequently, at the urging of physicist I. I. Rabi, went to the University of Illinois at Urbana to take a tenure track position, a rare opportunity during the Great Depression. Oppenheimer and Serber had become close friends and colleagues at Berkeley. A week after Pearl Harbor, Oppenheimer went to Urbana to convince Serber to return to Berkeley and join the theoretical conference the following summer.[65]

Serber had also known Lawrence during his first tenure in Berkeley. By the time Serber returned to Berkeley, Lawrence had already begun the calutron project to separate U^{235} from U^{238}. To assist in this, Oppenheimer had assigned several graduate and postgraduate students to work on magnetic field orbit calculations for Lawrence's electromagnetic separator. The most advanced members of this group included two post doctoral fellows, Eldred Nelson and Stanley Frankel, whom Serber put to work on improving the current state of neutron diffusion theory. The calculation of the exact amount of fissionable material needed for a weapon and of the efficiency of the reaction was a difficult but crucial task since the MED's selection of a production process for fissionable materials would depend on accurate estimates of weapons materials requirements.[66]

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[64] Hewlett and Anderson, The New World, 102; Rhodes, Atomic Bomb, 415.

[65] Serber, Primer, xxvii-xxviii.

[66] Hewlett and Anderson, The New World, 103; Author interview with Robert Serber, New York, NY, November 26, 1996; Interview transcription is held at the American Institute of Physics Center for the History of Physics, Niels Bohr Library, College Park, MD.

According to Serber, up until the issuance of the MAUD report no American scientists had published papers on neutron diffusion. If Oppenheimer's predecessor at the atomic bomb project -- Wisconsin theoretician Gregory Breit -- had performed any work on neutron diffusion, he had kept it so secret that no one else knew about it in 1942. Thus, Oppenheimer assembled a series of secret British papers by Rudolf Peierls, Klaus Fuchs, P.A.M Dirac, and others on the diffusion of neutrons through a critical mass, and on efficiency, by the time the Berkeley conference commenced, to serve as a basis for the luminaries' work that summer.[67]

Along with trying to improve the simple diffusion theory the British had used, to Nelson and Frankel also fell the assignment of estimating critical masses of uranium. The former task was a prerequisite for the latter. Qualitatively, the critical mass depends on the diffusion rate of neutrons out of an active mass as compared with the rate that they are generated in it. To calculate the critical mass requires a knowledge of the average way that neutrons distribute themselves in the mass. Ordinary simple diffusion theory is only valid in the range where the mean free path of diffusion particles is small compared to the dimensions of interest. An atomic weapon

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[67] Serber, Primer, xxix; Author interview with Serber, November 26, 1996; Robert Budwine has noted that one of the early British papers on calculating the critical mass of uranium was P.A.M. Dirac, "Estimates of the Efficiency of Energy Release with a Non-Scattering Container," BM-123 (MS D.4), December 1942 (sic); Citation in Robert Budwine, "Technical Chronology of the Development of Nuclear Explosives, Part 1 - Early Fission Explosives: 1942-1946," COPD-93-138, Lawrence Livermore National Laboratory (hereafter LLNL), November 1, 1993, 3, [This Report is Secret-RD]; The MAUD report was prepared by the British based on the theoretical atomic bomb work done by refugee physicists Otto Frisch abd Rudolf Peierls between 1940 and 1941. The MAUD report indicated that an atomic weapon was possible and estimated that a critical mass of ten kilograms would create an enormous explosion.

is more complicated; the number of neutrons in a given small region depends not only on that in adjacent regions, but on the entire distribution throughout the mass. Thus, Nelson and Frankel needed to employ an integral diffusion theory and find methods to apply it in a practical calculation.[68] Serber recounted:

> Nelson and Frankel did better [than merely improve on the British work] and wrote down an exact integral equation for the diffusion problem and found something about its solutions. . . [and in] the literature they found the Wiener-Hopf equation -- an exact solution for the case of flow in one direction. With that background they were in a good position to make accurate diffusion theory calculations.[69]

From the time he arrived in Berkeley in April until the summer conference started, Serber worked by himself on the theory of efficiency and hydrodynamics of the atomic explosion. When the conference began in earnest in July 1942, Serber, Frankel, and Nelson led off with a discussion of their efforts, confident that they understood well the physics of atomic weapons. The group thought that the chief difficulty in constructing an atomic weapon at that point involved building a gun of high enough velocity for the plutonium to assemble. Within two days, the entire group assumed they had nearly solved the fission problems, leaving Teller with the opportunity to present his idea for a thermonuclear device.[70]

Lore about the Berkeley meeting suggests that most of the conference was devoted to the theory of the Super. Serber confirmed this, stating, "It's

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[68] Hawkins, Project Y, 65.
[69] Robert Serber, "The Initial Challenge," lecture at Los Alamos National Laboratory (hereafter LANL), March 30, 1993, videocassette, [This document is Secret-RD]; Author interview with Serber, November 26, 1996.
[70] Serber, "The Initial Challenge."

true and its remarkable that we started out talking about [fission] and Teller brought up his Super. . . .This happened two days after the meeting started . . . everybody jumped on that since the A-bomb was a settled issue now."[71] However, by the end of the conference the participants concluded that an atomic device would constitute a significant scientific and technical effort. Although distracted by Tellers' idea, the group still settled on pursuing the atomic configuration because of several difficulties found with the thermonuclear weapon theory and because an atomic device would require development first to serve as an initiator for the hydrogen device, which they had named the "Super." Regardless of how much the Berkeley group found the Super intriguing, Bethe explained, ". . .the fission bomb had to come first in any case . . ."[72]

Like Serber, Bethe also remembered that because of Serber's, Frankel's, and Nelson's preparatory work, the theory of the fission bomb was "well under control so we felt we didn't need to do much." Therefore, the Berkeley conferees felt that they could spare extra time to theorize about this Super, and did not dismiss it as a possible line of research in the future. I discuss the Super theory and its origins, and the Berkeley Conference participants' reminiscences of it in the next chapter.[73]

Confident about the atomic gun weapon's feasibility, the Berkeley group reported to the S-1 Committee in August 1942 that a fission bomb was

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[71] Author interview with Serber, November 26, 1996.
[72] Rhodes, Atomic Bomb, 417; Hans Bethe quoted in Jeremy Bernstein, Hans Bethe: Prophet of Energy, NY: Basic Books, 1980), 73.

probable but would require a critical mass "6 times the previous [estimated] size[:] 30 kg U^{235}." Established in 1941 to supervise research on uranium, the S-1 Executive Committee, chaired by Conant, supervised all such work. Other members included Lawrence, Lyman Briggs, Arthur Holly Compton, Harold Urey, and Eger Murphree. Upon reading the Berkeley group's report, the S-1 Committee forwarded to head of the OSRD, Vannevar Bush, a recommendation that an atomic bomb could win the war. They also noted that a Super likely could be built at some point in the future.[74]

An atomic device remained the first priority of the Manhattan District, however, and when Los Alamos opened in 1943, Nelson and Frankel continued their work on neutron diffusion calculations for the laboratory's main technical objective, a gun weapon fueled by plutonium or perhaps uranium. In continuing their calculations, Frankel and Nelson ordered the same types of mechanical desk calculators they had used in California -- Marchants, Fridens, and Monroes. But they difficulties achieving any computational accuracy using these machines for calculations related to the gun weapon. Bethe recalled the numerical problems that several of T Division's members tried to solve:

> The first was neutron diffusion . . . . [T]o assemble the bomb by a gun, shooting . . . fissile material [together] . . . . very complicated shapes would result. We wanted to know how neutrons would diffuse in such a complicated assembly, in order to assess the probability that the chain reaction might start prematurely, and the bomb explode with less than the full yield. Even in the final assembly, we might have a cylinder of fissile material rather than a sphere, because this would be

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[73] Rhodes, Atomic Bomb, 417.
[74] Rhodes, Atomic Bomb, 420-421; Hewlett and Anderson, Atomic Shield, 75.

much easier to fabricate: we wanted to know how much of the energy yield of the bomb we would lose by this. All these problems were insoluble [sic] by analytical means, and while we could set up integral equations describing the process, they were too far complicated to be solved by the desk computing machines.[75]

Despite Nelson and Frankel's earlier work at Berkeley, Los Alamos scientists still found themselves facing the problem of finding a reasonably precise method of determining critical masses. Their results so far remained imprecise, even though at Berkeley the theoretical group had concluded that no significant gaps could be found in the theory of the fast-neutron reaction. In October, 1943, Frankel and Nelson reported that they could not find a way of transforming the integral equation for the infinite cylinder geometry into a form for which they had a solution. As Bethe described above, these problems could not be solved by the laboratory's hand computers, almost exclusively a group of women (many were scientists' wives) employing the desk calculators, under the supervision of New York University mathematician Donald "Moll" Flanders. Several members of this group, including Mary Frankel, Josephine Elliott, Mici Teller, and others, became exceptionally adept at hand computing and indispensable to Los Alamos's T Division.[76]

Even with the hand-computing group employing about 20 persons, the calculations for the gun device strained the mechanical calculators.

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[75] Hans Bethe, "Introduction" in Computers and Their Role in the Physical Sciences, eds. S. Fernbach and A. Taub, (New York: Gordon and Breach, 1969), 2; Serber, "The Initial Challenge."

[76] LA-31, "Multiplication Rate for Untamped Cylinders," October 18, 1943, [This Report is Secret-RD]; Hewlett and Anderson, The New World, 102; N. Metropolis and E.C. Nelson, "Early Computing at Los Alamos," Annals of the History of Computing 4, No. 4, October 1982,

Moreover, the desk calculators often broke down and were shipped back to their manufacturers for repairs. So many calculators broke down that young physicist Richard Feynman and mathematician Nicholas Metropolis began a trial and error method of repairing the machines, mainly by comparing the mechanical motions of a working calculator with a broken one. Metropolis remembered that he and Feynman even placed a sign outside their office door proclaiming their repair service, until the laboratory administration reprimanded them for not following the "proper" procedure of sending the machines back to the manufacturers for repair.[77]

How could the neutron diffusion problems be solved, and reasonably quickly at that? One of the Laboratory staff member's previous experiences at another scientific center proved useful for solving problems related to the gun device. Physicist Dana Mitchell had worked at Wallace J. Eckert's astronomy laboratory at Columbia University where laboratory staff used IBM punched card accounting machines to carry out astronomical calculations.

Eckert, one of the most famous figures in numerical astronomy at this time, had received his Ph.D. from Yale in 1931. Even before completing his degree, Eckert went to Columbia University as an assistant in astronomy, and began to build a small computing laboratory, supported by Thomas J. Watson of the IBM Corporation. In 1933 Eckert persuaded Watson to enlarge the laboratory that later became the Thomas J. Watson Astronomical Computing

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348-357.

[77] Metropolis and Nelson, "Early Computing," 349; Richard P. Feynman, Surely You're Joking Mr. Feynman: Adventures of a Curious Character, (New York: Bantam, 1985), 108.

Bureau. Eckert's laboratory employed IBM-made punched card machines for scientific calculations, and was one of the first to employ commercial punched card machines for basic scientific research.[78]

Herman Goldstine has stated that probably more than any other scientist, Eckert's demands for "emendations of the standard IBM machines to make them more useful for scientific work forced the company to develop an attitude of flexibility toward scientific users of machines." Eckert's desires to mechanize scientific calculations not only influenced IBM's technical strategies, but also likely inspired interest in electronic computers at the University of Pennsylvania and at the Institute for Advanced Study (IAS) at Princeton.[79]

Getting the Job Done on Time: Mechanization of Fission Calculations

After coming to Los Alamos, Dana Mitchell sat on the Laboratory's Governing Board which met weekly; Mitchell was also in charge of equipment procurement. When Bethe mentioned the difficult neutron diffusion equations, Mitchell recalled Eckert's laboratory, and recommended that Los Alamos try IBM 601 punched-card accounting machines (PCAM) for calculations of the behavior of the gun-type weapon. Mitchell estimated that a single calculation of the gun device would take six to eight months if carried out by the laboratory's hand computers. With the help of the IBM machines,

[78]Herman H. Goldstine, The Computer from Pascal to von Neumann, (Princeton: Princeton University Press, 1972), 109-110.
[79] Ibid., 110.

on the other hand, individual calculations might be carried out in three to four weeks.[80]

More specifically, the Laboratory ordered IBM machines for calculating critical masses of odd-shaped bodies in the fall of 1943. They could not arrive at Los Alamos fast enough. In January 1944, Oppenheimer urged that the IBM machines be rushed to the laboratory, stating that the card punches were essential for guiding engineering design; the card punches' results would be used in placing orders for materials whose fabrication would take months.[81]

The IBM machines did not arrive in Los Alamos until the spring of 1944. Because of the secrecy surrounding Los Alamos, the IBM corporation did not know the final destination of their machines, nor could they send an installation crew. The Army requisitioned an IBM maintenance expert (who had been drafted earlier) to Los Alamos in the meantime, but the machines arrived before him, only partially assembled. Feynman, Frankel, and Nelson finished assembling the machines using only the enclosed wiring blueprints.[82]

At this time, very few people at the Laboratory had any experience using IBM accounting machines. Persons who knew how to use punched card machines became a sought-after species at Los Alamos. Mathematician Naomi Livesay had expertise working with IBM machines at Princeton

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[80] Telegram from J. Robert Oppenheimer to S.L. Stewart, January 28, 1944, B-9 Files, Folder 413.51, Drawer 96, LANL Archives, [This Document is Secret-RD].
[81] Hawkins, Project Y, 81; Nicholas C. Metropolis, "Computing and Computers: Weapons Simulation Leads to the Computer Era," in Los Alamos Science 7 , (Winter/Spring, 1983), 132-141; Bethe, "Introduction," 2; Telegram from Oppenheimer to Stewart.
[82] Metropolis and Nelson, "Early Computing," 350.

Surveys; T Division hired her in February 1944, before the machines had arrived. Subsequently she supervised the military and civilian crews running the machines. Because pressure to complete work on the IBM machines steadily increased, in the summer of 1944 Livesay hired an assistant, Eleanor Ewing, who had been teaching mathematics at Pratt and Whitney, to help supervise the teams performing calculations on the machines.[83]

Not until summer 1944 did T Division's members solve the problems of calculating neutron diffusion and critical masses. At Berkeley, Nelson and Frankel had devised the extrapolated end-point method for studying neutron diffusion, although it was far too simple to use to model the complicated movement of neutron through the core of a bomb. In order to model neutrons with many velocities several T Division members tried a "multigroup method" of numerical approximation where they divided the neutrons into several groups, each containing neutrons of the same velocity, reducing the overall problem to a series of smaller, one-velocity problems. This represented a more realistic description of neutron diffusion in a weapon.[84]

Likewise, T Division members often approximated solutions to problems. Finding a suitable solution for critical mass calculations for the gun assembly required several approaches pursued by Bethe, Frankel, Nelson, David Inglis, Robert Marshak, and others. They had essentially solved this

[83] Personal communication with Caroline L. Herzenberg and Ruth H. Howes.

problem by July 1944. Several months earlier, Bethe and Feynman had developed an approximate formula for efficiency.[85]

In 1944, Los Alamos suddenly and abruptly changed its main technical goal. As mentioned earlier, when Los Alamos opened, its scientists concentrated on building a uranium or plutonium gun-type weapon, where two subcritical masses of fissile material would be shot together to form a critical mass. The Berkeley conferees and most of Los Alamos's members initially saw gun assembly as an achievable goal. During the summer of 1944, however, Los Alamos's focus shifted to developing an implosion bomb.[86]

Caltech physicist Richard Tolman suggested implosion as early as 1942, but the implosion method for assembling any fissile material constituted an extremely complicated shockwave phenomena. An implosion configuration basically consists of an amount of fissile material surrounded by high explosives. The explosives are detonated, creating shockwaves that travel inward and compress the fissile material into a super critical mass, creating a fission chain reaction. Although this presented a formidable problem, another Caltech physicist, Seth Neddermeyer, began a small implosion study program after Los Alamos opened. Los Alamos's technical focus began to shift in late 1943 after mathematician John von Neumann visited to lend his assistance to the project.

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[84] Hoddeson, et al., Critical Assembly, 179-180.
[85] Ibid., 183.
[86] Hoddeson, "Mission Change," 267.

A leading expert on shock and detonation waves, by World War II von Neumann served as a consultant to the Army Ballistics Research Laboratory, the OSRD, and the Bureau of Ordnance. Not surprisingly, he became involved with Los Alamos when Oppenheimer requested his help. Von Neumann studied Neddermeyer's small test implosions of cylindrical metal shells, and realized that implosion could be made far more efficient if one used a greater ratio of high explosive-to-metal mass, causing rapid assembly. In addition, the implosion scheme might use less active material and require less costly materials purification schemes.

Nuclear materials issues aside, the plutonium gun assembly had another problem. In the spring and summer of 1944, Emilio Segre's experimental physics group realized that spontaneous fission in Pu^{240} made the plutonium gun idea unworkable; it would not be fast enough to tolerate the added neutrons. Yet, given the state of the MED's production facilities, plutonium was the only material at that time that could be produced in large enough quantities for many bombs. A uranium gun bomb could be made by the summer of 1945, but probably only one. Thus, the Laboratory turned to implosion as the only practical means of utilizing the plutonium available in the summer of 1944.[87]

Generally, an implosion device works in the following way: A subcritical fissile core (in the war this meant Pu^{239}) is surrounded by a shell of high explosives -- part of a lens structure that focuses the blast into a

[87] Goldstine, The Computer, 177; Hoddeson, et al., Critical Assembly, 129; Hoddeson, "Mission

converging, inward moving front. Electrical charges detonate the explosives nearly simultaneously, so the resulting blast wave is relatively symmetric, causing an even implosion of the core and compression of the fuel. Due to this compression, the core becomes supercritical, and begins to expand outward, causing an explosion.[88]

Modeling these processes provided not merely a challenge, but in the summer of 1944 no one knew if implosion would work at all. But, with the change in the project already being considered by the Laboratory in spring 1944, the purpose of the IBM machines changed too, and T Division began preparing problems for the IBM machines in anticipation of modeling an implosion device.

Towards the new fission implosion configuration, Teller and his group in T Division assumed responsibility for developing a mathematical description of implosion, and calculated the time of assembly for large amounts of high explosives. Along with mathematician Nicholas Metropolis and Feynman, Teller calculated 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 scheduled to perform detailed calculations of an implosion weapon. Thus, Bethe sought a replacement for Teller.[89]

Change," 274-281.

[88] Hansen, <u>U.S. Nuclear Weapons</u>, 21.

[89] Bethe, "Introduction," 3; Hans Bethe, "Comments on the History of the H-bomb," op. cit., 43-53.

In March 1944, Bethe reorganized the Theoretical (T) Division in order to meet the urgency of the implosion program, and in July replaced Teller with Peierls as head of the theoretical implosion group. When Peierls first visited in February, he suggested a step-by-step method of solving differential equations based on his earlier calculational work on blast waves in air. Bethe recognized the importance of Peierls's suggestion and T Division based its implosion calculations on the same form as Peierls's blast wave equations.

Simulating the implosion device required detailed calculations of complicated implosion hydrodynamics. However, the Laboratory's hand computers could not solve the partial differential equations of hydrodynamics employing realistic equations of state applicable to high temperatures and pressures. By February 1944, T Division began to calculate the initial conditions for numerical integration of the implosion differential equations on the IBM machines. The numerical procedure for an implosion simulation, and a general approach to processing the cards through a sequence of machines, were worked out even before the IBM machines arrived. Metropolis and Nelson elaborated on the hydrodynamic problems:

> The numerical procedure evaluated the differential equation for a sequence of points covering one space dimension and then integrated ahead one step in the time dimension. Thus, a punched-card was established for each point in the first dimension, with a deck of cards representing the state of the implosion at a specific time instant . . . Each integration step of the partial differential equation corresponded to one cycle of a deck of cards through the machines . . . . About a dozen separate machine steps were involved in each integration cycle.[90]

[90] Hoddeson, et al., Critical Assembly, 160; Metropolis and Nelson, "Early Computing," 350.

After the IBM machines arrived and Feynman, Frankel, and Nelson assembled them, the card punch computational procedure needed checking out before implosion calculations could begin. Thus, Feynman and Metropolis organized a "race" between the hand computers and the card punches. For two days the hand-computing group kept pace with the IBM machines, as they tried to compute the first few integration steps of an implosion simulation in order to work any bugs out. By the third day, however, the tireless accounting machines pulled ahead and the group abandoned the race.[91]

A race of another sort continued. "Everything we did, we tried to do as quickly as possible," Feynman recalled. But in spring 1944 implosion calculations undertaken on the IBM machines went very slowly. To operate the machines, the army had recruited several high school graduates from all over the U.S. and sent them to Los Alamos. This Special Engineering Detachment (SED) arrived in Los Alamos knowing nothing about the purpose of the project or of their own duties of punching the cards and running them through the machines. One cycle took about three months to complete until Feynman obtained permission from Oppenheimer to inform the SED's about the purpose of the project. Excited about fighting a war, the SED's quickly invented their own programs to speed the effort, and completed about nine problems in three months. Feynman remembered:

> The problems consisted of a bunch of cards that had to go through a cycle. First add, then multiply -- and so it went through the cycle of

[91] Metropolis and Nelson, "Early Computing," 350-351; Feynman, Surely You're Joking, 109.

> machines in this room, slowly, as it went around and around. So we figured a way to put a different colored set of cards through a cycle too, but out of phase. We'd do two or three problems at a time.[92]

Implosion modeling began with simulating the detonation of the high explosive charge surrounding the bomb, computing the propagation of the detonation front through the charge, generating a shock wave when the detonation reached the tamper (a dense, inactive material surrounding the fissile core), propagating the shock wave through the tamper and active material, and reflecting the shock wave when it reached the center.[93]

The first implosion simulations explored different configurations of the high-explosive charge, tamper, and active material. Based on the results of these exploratory simulations, one particular implosion configuration, T Division chose what later became known as the Mark III, for detailed simulation. The Mark III represented the most practical road to an atomic device; when engineering construction on the actual implosion bombs began, engineers and technicians developed this configuration because it was the only one for which detailed data on its expected behavior existed.[94]

During the Manhattan Project the nuclear design process could not have happened in the reverse order. At this time, when nuclear weapons science was a new practice, its practitioners were exploring many unknowns. No one knew how a weapon would work, and the atomic project's success, measured ultimately in a successful fission bomb test, rested largely on

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[92] Feynman, Surely You're Joking, 111.
[93] Metropolis and Nelson, "Early Computing," 354.

theoretical mathematical estimations of a weapon's predicted behavior. The physical design of a weapon had to follow mostly from the theoretical work, although some of those performing the theoretical calculations for the Mark III no doubt had to consider physical limitations imposed on the weapons, such as, for example, the limited available amount of Pu^{239} to fuel the device. To some degree, T Division members had to tailor some theoretical simulations to fit within certain practical engineering parameters. Still, scientists performed much of the theoretical work during the war independently of experimental physical design aspects, considering that time was so crucial.

In a similar fashion, the theoreticians did not view the hand computers or IBM machines as experimental instruments. With the mechanical difficulties involved simulating implosion and given that T Division perpetually tried to accelerate these problems, there was little time for experimentation with the IBM machines. Frankel in particular caught the "computer disease" that physicist Feynman so acutely described: "The trouble with computers is you *play* with them . . . and it interferes completely with the work." Frankel stopped paying attention to supervising the card punch operations and the implosion calculations went too slowly. Bethe too recalled that Frankel became so enchanted with the machines that he forgot that the real aim of the project -- to solve the implosion problem. In order to speed the calculations, Bethe replaced Frankel with Metropolis and put

[94] Ibid., 354-355.

Feynman in charge of the entire IBM group. Frankel eventually ended up in Teller's group working on the Super theory. Under Feynman, Metropolis and Nelson, the whole IBM group of about two dozen machine operators and coders focused exclusively on implosion calculations.[95]

Not only did human folly affect the pace of work on the IBM machines, but so did the natural environment. Metropolis recalled that the machines were, for that time, relatively complex, each one containing several hundred relays as the primary computing element. The unpaved roads in Los Alamos and constant New Mexico dust caused intermittent errors -- at least one in every third integration step -- by sticking to the relay contacts. Luckily for the human operators, the computational procedure was very stable and insensitive to small mistakes; the operators had only to correct errors in the more significant digits.[96]

Over the course of the war, Los Alamos strengthened its ties with IBM. The laboratory needed machines with particular features that would speed the implosion calculations and accelerate the pace of weapons development. In May 1944 the laboratory requested that IBM custom-build triple-product multipliers and machines that could divide. Nelson himself traveled to New York in June to meet with IBM's vice president John McPherson to discuss in detail the new proposed machines. The new punched card models arrived at Los Alamos towards the end of 1944, and helped increase the pace

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[95] Feynman, Surely You're Joking, 109-110; Bethe, "Introduction," 5; Author interview with Hans Bethe, LANL, September 14, 1994; Interview transcription held at LANL; Hoddeson, et al., Critical Assembly, 307.

of the implosion simulations, while simultaneously increasing the need for more operators to run them.[97]

Many of the machine operators knew more about using the IBM equipment than T Division's scientific staff and consultants. Von Neumann took a great interest in the punched cards and learned their basic operation from Livesay and Ewing, who shared on office with him.[98] His experience with the IBM machines influenced his views on designing larger, electronic computers in which von Neumann became extremely interested at this time. Metropolis later wrote that von Neumann found wiring the IBM tabulator plugboards extremely frustrating:

> . . . the tabulator could perform parallel operations on separate counters, and wiring the tabulator plugboard to carry out parallel computation involved taking into account the relative timing of the parallel operations. He [von Neumann] later told us this experience led him to reject parallel computations in electronic computers and in his design of the single-address instruction code where parallel handling of operations was guaranteed not to occur.[99]

While in 1944 and 1945 the IBM machines represented the state-of-the-art in punched card technology, large, electronic computer projects got slowly underway at a few military and academic centers in the U.S. Keenly aware of these projects, von Neumann pushed his Los Alamos colleagues to consider the new electronic computers for the Laboratory's problems. In 1944 von Neumann informed T Division about Howard Aiken's Mark I computer at Harvard University. Although an electromechanical relay machine, it was

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[96] Harlow and Metropolis, "Computing and Computers," 134.
[97] Metropolis and Nelson, "Early Computing," 351.
[98] Herzenberg and Howe, op. cit.

still much faster and more precise than punched card devices. Von Neumann suggested that T Division loan one of its implosion problems to Aiken to run on the Mark I, and machine operators completed the problem in Spring 1944.[100]

Metropolis recalled that von Neumann kept Los Alamos's staff informed about "[p]rogress elsewhere in computing. . . . [c]ommunication of these new developments by von Neumann was initially informal, but as their profound implications became apparent, he was requested to present a series of lectures on them, showing the technical links between the separate independent developments." To Metropolis and other staff he also "described his computer of the future, outlining his single-address architecture, later implemented in the IAS computer" and other machines.[101] Von Neumann not only carried to Los Alamos news of computing developments, such as the Bell Telephone Relay-Computer, but he also inspired in T Division a contagious enthusiasm for large-scale computers and mechanizing weapons calculations.[102]

Despite the emergence of electronic computing, during the war the majority of implosion simulations occurred in New Mexico. At Los Alamos, by late April 1944, SED's completed the first implosion problem after about three months. The groups finished seven more IBM problems by the end of

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[99] Metropolis and Nelson, "Early Computing," 351.
[100] Ibid., 351.
[101] Ibid., 352.
[102] Letter from von Neumann to Oppenheimer, August 1, 1944, LANL Archives, MED Files, A-84-019, 310.1, T Division, Box 6, Folder 10.

1944, and seventeen in 1945, on three shifts, six day per week schedules.[103] Nearly from the time the IBM machines arrived at Los Alamos, they ran 24 hours a day to complete the implosion calculations on time. The results of the calculations showed that the Fat-Man type bomb could get a good energy yield with the fissile material strongly compressed in a spherically symmetrical implosion. The July 1945, Trinity test verified the calculations for the Fat Man design.[104]

The Emergence of Labor-Saving Technology

Los Alamos's employment of punched card machines gave a tremendous boost to the implosion calculations and undoubtedly helped to complete these problems in the face of military deadlines. Nevertheless, the IBM machines did not determine the outcome of Los Alamos's technical program; Los Alamos's scientists, not the card punches, held responsibility for developing an implosion device and determining the final design choice for the Trinity test. According to historian of technology Merritt Roe Smith, technological determinism -- the idea that technology is autonomous, and independent of society, yet it impinges on society -- has traditionally been one of the most influential theories of the relationship between technology and society. Technological determinism is deeply imbedded in American culture, with an intellectual heritage dating back to at least the eighteenth-century Enlightenment. Not surprisingly, much history of technology is laden with technological determinism, although such approaches have been challenged

[103] Metropolis and Nelson, "Early Computing," 351.

in recent years by the influence of the sociology of science and in particular the social constructivist schools of thought.[105]

Technology does not choose nor reproduce itself, although existing technical artifacts certainly act as preconditions for new and developing technologies. Technological choices represent a diverse array of human needs and values. They can also be representative of the particular culture or society in which they are developed.

The United States has often been characterized by historians as a society with a tendency towards building machines for automation, and for finding labor-saving technology. Hughes has described the United States as "technology's nation," a country of machine makers seeking a drive for order, system, and control. H.J. Habakkuk explored the American penchant for automation and employment of labor-saving technology in his American and British Technology in the Nineteenth Century (1962). Compared to the British, the US invented and adopted mechanical methods of labor more rapidly. There reasons for this originated in the cultural, environmental, and economic surroundings of the younger nation.[106]

According to Habbakuk, the United States had a scarce labor supply, thus manufacturers had to invent new technical means to make up for the labor scarcity. In addition, the U.S.'s over-abundance of land itself had several

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[104] Bethe, "Introduction," 8-9.

[105] Merritt Row Smith, "Introduction," in Does Technology Drive History?, eds. Merritt Roe Smith and Leo Marx, (Cambridge, MA: MIT Press, 1994), 2-3; Bijker, Pinch, and Hughes, "Introduction," 9-15.

[106] Hughes, American Genesis, 1; H.J. Habakkuk, American and British Technology in the Nineteenth Century: The Search for Labor-Saving Inventions, (Cambridge: Cambridge

effects: The U.S. had an independent and strong agricultural base, thus in order to attract labor away from agriculture, industries had to offer high wages; America had many forms of natural recourses from water power to minerals; the large American terrain meant that not only did agriculturists become creative in mechanizing work, but so did industrialists, unable to rely on others in close proximity.[107]

Industrialists employed all kinds of machines for a wide variety of tasks in the late nineteenth and early twentieth centuries, punched card machines not least among them. Herman Hollerith developed some of the first commercially used punched card machines at the end of the nineteenth century. The U.S. Census Bureau was one of the first large organizations to employ Hollerith's accounting technology in its mammoth task of tabulating statistics on the American population. Later, Hollerith developed a punched card system for the New York Central and Hudson River Railroad, and for the Pennsylvania Railroad Company to account their freight, scheduling, and statistics. Although successful in his punched card business, in 1911 Hollerith sold his small company to the Computing-Tabulating-Recording Company (CTR), which became the International Business Machines Corporation in 1924.[108]

Prior to Wallace Eckert's employment IBM's punched cards at his Columbia University laboratory, the only other situation where scientists

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University Press, 1962).

[107] Ibid., passim.

[108] Williams, Computing Technology, 253; Lars Heide, "Shaping a Technology: American

used accounting machines for basic research occurred in the mid 1920s at the National Almanac Office in London, where L.J. Comrie employed punched card machines to calculate the motions of the moon from 1935 to 2000. By the late 1920s and early 1930s when Comrie and Eckert just began to recognize the value of punched card machines for large scientific problems, calculating equipment of all kinds had been introduced into American businesses and industry and there had been firmly established as a labor-saving technology.[109]

Wartime Mission: Los Alamos Establishes an Approach to Problem-Solving

The introduction of business accounting machines into the wartime theoretical program to design an atomic weapon was a novel one for attempting to overcome a critical problem faced by Los Alamos. This sort of approach to problem solving reflected several characteristics of : (1) the wartime laboratory itself and, (2) its relationship to the Manhattan District. Wartime Los Alamos operated, according to Lillian Hoddeson, in a strict mission-oriented mode. Hoddeson describes the "mission-directed" laboratory as one where "scientific and technological research is oriented by a larger goal, the well-defined 'mission,' which typically is expressed in terms of a contribution to society reaching beyond the laboratory."[110]

From the beginning of the project, Groves, the military, and Oppenheimer imposed a strong mission orientation at Los Alamos. Projects

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Punched Card Systems, 1880-1914," (University of Odense, 1996), 5-6, 15-21, 23.
[109] Williams, Computing Technology, 254.
[110] Lillian Hoddeson, "Mission Change," 265.

in line with the goal of producing a practical military weapon received nearly unlimited funding and material support. Other projects out of line with the main goal of an atomic device (such as the Super) starved. Scientists had to meet Groves's deadline of producing a working weapon by summer 1945, therefore "pure" scientific research was not carried out during the war. In other words, as Hoddeson and her colleagues have shown, scientists had no time to "provide technical solutions based on full understanding of fundamental laws." Instead, scientists were forced to adopt alternative and inexact approaches to problem-solving, even approximating theoretical implosion calculations. Facing a strict deadline, Los Alamos scientists had to pay attention to practicality, and focus on the reliability of methods. Their objectives:

> [S]hifted from understanding to use, and from general conceptions to particular materials and apparatuses. This reorientation encouraged them to diversify their methodological toolkits with approaches typically employed by engineers and craftsmen, whose technical problems were anchored in concrete phenomena.[111]

Finally, Los Alamos had the additional characteristic of being organized like a military institution, enabling civilian division and group leaders to create an effective hierarchical research facility, where deadlines could be rigidly enforced and scientists directed towards particular work or technical goals.[112]

The Laboratory's relationship to the Manhattan District changed over the course of the war. Even though Los Alamos had been established as a

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[111] Hoddeson, et al., Critical Assembly, 4-6.

[112] Hoddeson, "Mission Change," 266; Notably, practical concerns originating in the MED still affected the final shape and component structure of the first fission weapons. An implosion or

theoretical center within the MED's growing system of contractors, production facilities, and universities, the Laboratory quickly began to establish a form of independence from the MED where weapons scientists adopted their own problem-solving approaches to weapons design without the explicit consent of the Manhattan District. T Division's choice of computing methods is one example of this independent approach. Furthermore, and in a more general sense, Los Alamos scientists and engineers had the autonomy to make technical changes to the atomic device as long as the final product still met the military requirement of a useable weapon and if the changes included considerations such as the efficient use of nuclear materials.

Like computing, Hoddeson's example of the "crisis" of spontaneous fission in plutonium demonstrates both Los Alamos's approach to weapons development and the evolution of the Laboratory's relationship with the MED. In 1944, when Segre's group realized that "production" plutonium from the Clinton reactor at Oak Ridge fissioned spontaneously at an alarmingly high rate, they concluded that using this material in a gun type device would cause it to predetonate, and thus "fizzle." In this instance, the technological limits forced a revision of the Laboratory's theoretical program. However, by this time Groves had already ordered construction of the large facilities at Clinton as well as Hanford to produce large amounts of plutonium. The Los Alamos Governing Board's decision to change the

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gun device had to be designed not only to fit in the bomb bay of a B-29, but the device had to be technical focus from a plutonium gun to implosion weapon stemmed partly from the huge investment already made in plutonium production. Los Alamos had little choice but to find a means of utilizing this material, but could still decide upon the style of weapon to develop.

Los Alamos constituted only one part of the Manhattan District, which employed thousands of workers, scientists, engineers, and managers. As Hughes indicates, the entire project was an industrial development-and-production undertaking dependent on scientific laboratories, such as Los Alamos, for essential technical data and theoretical understanding of many weapons-related processes. Groves had intended for the MED to function as a temporary organization, but it provided an organizational framework for any successor agency that would take control of atomic weapons development in the postwar period.

Likewise, the relationship that Los Alamos established with and its evolution into a partly autonomous facility of the MED set a precedent for how the weapons laboratory would relate to a new organization responsible for atomic energy. Still, with the end of the war came the end of Los Alamos's mission-orientation, and for several months the Laboratory lacked any well-defined technical goals to strive for. Moreover, the Laboratory's future and its overall value looked uncertain. This lack of mission created uneasiness for Los Alamos's remaining scientific staff and for its new leader, physicist Norris Bradbury.

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constructed to withstand a high-altitude drop.

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

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(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

[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

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[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

[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

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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

[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

[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

[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

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[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 Instititute 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,

[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

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[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

[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

[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

[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

[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,

[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

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[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.

[287] Hansen, US Nuclear Weapons, 11.

Chapter Four

Making Light of the Light Elements

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

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

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

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

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

Detecting Tritium

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

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

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

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

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

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

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

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

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

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[289] Ibid., 370.

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

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

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

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

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

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

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

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

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

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

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

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

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

Cyclotrons or Reactors?

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

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

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

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[297] Ibid., 628, 692.
[298] Letter from J. Robert Oppenheimer to Samuel K. Allison, May 27, 1944, 635 Los Alamos, Box 19, Folder 5, LANL Archives.
[299] Ibid.

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

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

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

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

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

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

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

Production System

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

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

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

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

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

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

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

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

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

Office of Scientific Research and Development (Hereafter OSRD), S-1 Files, Files of Richard C. Tolman, Box 6, Folder titled "Postwar Policy Committee File Report," NARA II.
[306] Letter from Oppenheimer to Tolman, September 20, 1944, RG 77, MED Records, Box 61, File "Post War Policy Committee Correspondence," Entry 5, NARA II.

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

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

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

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

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

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

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

"Practicable" Investigation but a Fantastic Venture

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

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

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

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

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

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

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

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

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

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

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

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

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

[315] Ibid.
[316] Ibid.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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[325] Ibid., 10.

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

Glitches in the System

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

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

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

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

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

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

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

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

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

[330] Ibid.

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

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

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

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

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

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

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

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

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

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

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

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

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

McMahon, Borden, and a Program of AEC Expansion

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Can Berkeley Produce Tritium?

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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[360] Rhodes, Dark Sun, 400; Italics mine.
[361] Memorandum for the File from J. Kenneth Mansfield, "Extracts from GAC Reports Relating to Thermonuclear Program," May 28, 1952, in JCAE declassified General Subject Files, Box 59,

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

[367] Ibid.

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

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

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

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

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

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

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

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

The Problem of Attaining a Nuclear Reaction Involving the Light Elements

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

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[371] Ibid., 397.

[372] Letter from Pike to McMahon, March 1, 1950, JCAE declassified General Subject Files, Box 57, NARA; Hewlett and Duncan, Atomic Shield, 401.

[373] Letter from Pike to McMahon, March 1, 1950, op. cit.

[374] AEC Meeting No. 375, [Minutes], February 28, 1950, DOE Archives, RG 326, [No location noted], [This Document is Secret-RD].

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

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

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

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

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

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

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[378] GAC Report of Meeting 19 to Lilienthal, Febrary 1, 1950, JCAE delassified General Correspondence Files, Box 34, NARA.
[379] Document transmitted to Tyler from Bradbury, March 10, 1950, "Los Alamos Scientific Laboratory Technical Program for Calender Year 1950," DOE Archives, RG 326, Box 4944, (635.12) Los Alamos, Folder 7, (1-13-47), [This Document is Secret-RD].

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

"Great Progress in Showing Lack of Knowledge"

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

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

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

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

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

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

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

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

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

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

Compression of the Issues, and Circumventing the Tritium Problem

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

[401] Rhodes, Dark Sun, 473.

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

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

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

Teller chastised his Los Alamos colleagues:

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

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

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

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

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

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

[406] Mansfield memo, August 28, 1951.

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

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

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

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

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

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

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

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

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

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

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

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[413] Hughes, Networks, 106.

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

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

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

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

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

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

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

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

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

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

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

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

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

[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

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[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]

[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, Atomic Energy Commission Weapons Program of the Los Alamos Laboratory, 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

[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

[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

[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

[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]

[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

[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

[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

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[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,

[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

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[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

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[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.

Chapter Six

Conclusion: The Super, The System, and Its Critical Problems

A decade passed between the time Fermi first proposed the idea of a fusion bomb until the Mike test. Compared to the wartime fission weapon project, Los Alamos appeared to take a considerably longer time to complete research and development of an H-bomb. It is difficult to compare the two projects, however, because the weapons technologies differed from one another excessively, and so did the systems they were developed in. In addition, the Laboratory focused on the Classical Super configuration for the majority of the time, with greater seriousness directed at it than towards any other theory. Up until 1951, the Super represented almost the entire Los Alamos thermonuclear program, with the Alarm Clock and Booster as the only theoretical alternatives. The length of time the U.S. took to develop and test a viable hydrogen bomb, too, is problematic in that it has taken on the form of historical myth. I will elaborate on this later.

The period of time that Los Alamos needed to develop and test a fusion device is relative historically. When comparing the time it took Los Alamos to develop a fission bomb as opposed to a fusion bomb it is necessary to consider the characteristics of each project, and the conditions surrounding their development: Compared with the gun and implosion bombs, a fusion weapon involved a much more complicated set of physical problems to solve, fewer people participated in this work, no deadline had been set, and no military directive for this project existed. Both the fission and fusion programs required massive material support structures such as large nuclear fuel production facilities. The strong wartime mission allowed for Hanford and Oak Ridge's respective completions within a couple of years to provide fuel for the Fat Man and Little Boy bombs. No such equivalent facility had been designed to produce tritium, on the other hand, during or after the war, when there existed no urgency to prompt such activity.

The respective technological systems in which the United States developed its first atomic and thermonuclear weapons differed disparately. Besides having the characteristics of a military mission and large industrial material support, Groves had set up the wartime fission program deliberately as a short term, intense, goal-oriented project, physically apparent in the temporary structures built at Los Alamos, Hanford, Oak Ridge, and other sites. This same short-term characteristic had, in the long term, the unintended consequence of being a hindrance to any large fission or fusion program in the postwar.

Critical problems for the thermonuclear project up until 1949 affected for the most part the Super, and to a lesser degree the Alarm Clock, because Teller and Richtmyer did not propose it until 1946. Critical problems present almost all along from the time Fermi proposed the Super theory included computing, tritium, and notably the fission program itself. From the time of the 1942 Berkeley meeting, the fission project took first priority under the

Manhattan District, and continued to take first priority, although on a much smaller scale, after the war ended.

Scientists early on in the war recognized computing (initially in the form of simple hand computers) as a critical problem to the fission gun-weapon program and thus it was not surprising that Teller and others quickly understood that computers would play a significant role in determining the Super's feasibility. The tritium problem, although technically originating in the AEC's materials production facilities' limits, depended on computing to some degree. Computer simulations (and hand simulations) gradually revealed the seriousness of the tritium critical problem. The ENIAC inconclusive results scientists' varying interpretation of them may have made the Super appear to require only a modest amount of T. Furthermore, the cautiously optimistic tone of the Super Conference seemed to have convinced enough of Los Alamos's scientific staff that a Super would require tritium on the order of only a few hundred grams.

Later mechanical and hand computations on the Super ignition problem brought the tritium critical problem to the forefront, along with the second half, or deuterium burning portion of the problem. Yet, computing itself was limited at least until Metropolis and his group completed the MANIAC. Prior to this, no machine had the ability to simulate a full thermonuclear calculation that could account for all the effects of the device and run such a problem in more than one dimension. Thus, the precise amount of tritium that could ignite the Super and the exact radius necessary for the deuterium cylinder to self-propagate and burn up entirely remained indeterminable, at least in a reasonable time. The problem, as von Neumann estimated, could have been solved by hand but only with a huge number of human "computers" working over several years.

This human labor-intensive effort as an alternative to mechanized machine calculations of the Super could not have happened given the AEC's modest efforts to rebuild Los Alamos and hence failure to allow Bradbury to hire the hundred or more human "computers" von Neumann had suggested would be necessary to solve this problem. Even after the AEC approved increased funding for Los Alamos to determine the Super's feasibility beginning in 1950, neither the Commission and GAC nor Bradbury ever put forth a proposal to initiate a large hand computer effort on the Super. However, Metropolis and Richardson and their team in T Division had the MANIAC underway this time and von Neumann pressured the IAS project towards faster completion. These high-speed computer projects apparently satisfied Bradbury, Froman, and the GAC as far as determining the Super's feasibility, which the Committee had already doubted the technical validity of the previous fall.

Stan and Françoise Ulam, Everett, and the rest of the group running hand calculations on the Super ignition problem revealed for the first time just *how* critical a problem tritium was to the thermonuclear bomb project as it stood in 1950. The Evans group's follow-up ENIAC simulation of this same problem seemed to have convinced most of those who examined this problem -- save Teller and Wheeler -- that no practical method of igniting the Super existed.

Had they been done by hand, the Super calculations would have been incredibly labor intensive. The labor force, furthermore, was not available, and moreover, a lack of full-time active participants in all theoretical work on the Super slowed the project all around. This problem tied to Los Alamos's housing shortage and ultimately back to the MED system which Groves had not established with long-term housing and staff needs at Los Alamos in mind.

Finally, the military and politicians put little pressure on the AEC to pursue intensely the various thermonuclear weapon proposals raised prior to 1949. The American military did not make taxing requests even for fission weapons from the AEC in the agency's early years, and fusion devices did not come under the consideration of the armed forces at all. Policy did not flow strictly in one direction, however. Partly because hydrogen bomb research was regarded as highly secret by the Commission, and because Los Alamos's work on this remained almost entirely theoretical in the 1940s, the AEC did not inform the MLC or AFSWP about Los Alamos's H-bomb research. The military had little knowledge of the project in the first place, and could not engage in discussion with the Commission about thermonuclear devices' values as a military weapons.

The American military's postwar weapons policies were strongly influenced by the state of international affairs at any given time. The Cold

War, however, did not begin to take on a strong sense of urgency until the latter part of the 1940s with events such as the Berlin Blockade, the gradual expansion of Soviet control over much of Eastern Europe, the Korean War's outbreak and the emergence of communism in China. Therefore, a 1940s and early 1950s military would be composed of both conventional bomb and fission-equipped fleets.

The Most Complex Physical Problem

Galison rightly and elegantly demonstrates in Image and Logic that the "Los Alamos Problem" was the most complex physical problem ever in the history of science when nuclear weapons scientists ran it on the ENIAC in 1945 and 1946. The Super Problem was indeed an incredible task for von Neumann, Teller, Ulam, Metropolis, Frankel, Turkevich, the Evanses, and others who contributed to this mathematical monster. Also, the Super problem's complex nature is important to consider on its own, because it represented a challenge.[505]

Fermi emphasized the Super problem's importance to the rest of the GAC very soon after the Committee had been formed, where the Super might serve as an attractor or theoretical "bait" to bring new physicists to the Laboratory or retain some of those already there. Although they never opposed it prior to 1949, nevertheless the GAC kept the Super and Alarm Clock projects at a bare bones level, never viewing these ideas with the same seriousness as they did the fission program. Under the technical

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505 Galison, Image and Logic, 693-694.

circumstances the AEC system had to operate after the war, a full-blown thermonuclear research and test program would only decimate the fission program.

To demonstrate the complexity of the Super problem, Galison cites Egon Bretscher, "For prediction, then, the primary requisite is a deep insight into the general properties of matter and radiation derived from the whole theoretical structure of modern physics." Given this, the problem could not have remained ignored by Teller and Los Alamos during or after the war due to its overwhelming theoretical appeal. However, the system determined the pace at which work on it would proceed.[506]

The Super problem was not entirely limited to the secret confines of Los Alamos and thus examining the development of the hydrogen weapon program in terms of a technological system allows for inclusion of many critical problems in an historical analysis of the project. The critical problems involved were not only diverse from one another, but they had even more diverse origins both in and outside of Los Alamos and the AEC system. Only through examining the foundations of the technological system set in place by Groves and others during the war and analyzing how it's nature and purpose evolved subsequently can the early American thermonuclear program be best understood and accounted for. Moreover, the technological systems thesis with an emphasis on critical problems as the theoretical framework for a historical study of the H-bomb program can encompass a

[506] Ibid., 694.

patois of social, technical, and political characters as bottlenecks along with human system builders that traditional political or technical histories cannot.

As I have demonstrated in this dissertation, the H-bomb project was mostly Los Alamos's responsibility, but was not subject solely to the Laboratory's internal policies regarding this program. Even if Bradbury asserted that policy flowed from the Laboratory to Washington and back in the postwar, Los Alamos's leaders did not dictate the fission weapons programs, which were subject partly to the technical conditions of the system, such as Hanford's production abilities at any given time, and partly to the AEC and GAC's approval of the Laboratory's yearly program proposals. Los Alamos's thermonuclear research and development program was similarly subject to conditions in the large system and thus the H-bomb project had a dependency on the technological system-nature of the nuclear weapons complex beginning in the Second World War.

The technological systems thesis emphasizing critical problems is a broader-reaching historical framework than others that explain, for example, the early American hydrogen weapons programs exclusively in terms of political motives. While there is no doubt that a lack of both official policy and presidential directive to build a thermonuclear weapon prior to 1950 certainly did not increase Los Alamos's and the AEC's efforts towards this project, this is only apparent historically when compared with official policy in the period after the Russian atomic test. Therefore, a mere "lack of policy" prior to 1950 as the chief cause for Los Alamos's failure to aggressively pursue thermonuclear weapons in the postwar does not suffice. Critical problems in the system, however, existed all along even if they became critical in the human participants eyes' at various times in the postwar years.

Early nuclear weapons development from the time of the Manhattan Project through the 1950s comprises a very complicated history -- too easily "black-boxed" by merely exploring the entire program in terms of political agendas and government nuclear weapons policies. Los Alamos, as part of this system, played by far the most important role in the theoretical and experimental research and design endeavor of hydrogen bomb development. Because the Super constituted the majority of Los Alamos's scientists' focus in the realm of fusion weapons from 1942 through approximately the next nine years, and because it was so complex, the technological systems notion is a necessary historical model to employ in order to "un-black box" the numerous technical critical problems that the project faced, besides the social-political ones.

Because the technological systems approach allows for an historical focus on the intricacy of the critical problems facing the hydrogen bomb within the system and the complexity of the system itself, it best shows how the problems inherent in thermonuclear weapons development, rather than falling into a common trap of examining the program using temporal assumptions. Some scholars have examined the history of the American hydrogen bomb program with an underlying initial assumption that the project took an excessively long time. Indeed, the ten year period between

Fermi's 1942 proposal and the Mike test is lengthy when compared to the atomic project, but in many aspects the fission and fusion projects were incommensurable, as I have tried to demonstrate.

In their analyses, Rhodes and Hansen make this temporal assumption, lending to the already overblown mythology surrounding the hydrogen bomb's development. Rhodes does so by asking why (as have other authors such as York in The Advisors) Teller failed to see the importance compression would play in the H-bomb prior to 1951. By asking this and also focusing on Teller's so-called "obsession" with the Super as a hindrance to the discovery of a viable hydrogen bomb, Rhodes has judged that the American thermonuclear program took longer than it ought to have.

Hansen poses the same question more blatantly than Rhodes, titling a section of Volume III of The Swords of Armageddon, "Why Did It Take So Long?" What Hansen is actually focusing on by posing this question is explaining why scientists failed to discover the principle of radiation implosion and the general "Teller-Ulam principles" earlier than 1951.

"After more than 40 years," Hansen states, "an enduring question about the discovery of radiation implosion is why the Teller-Ulam principles did not surface much earlier than they finally did." While his answers to this are worth reviewing, Hansen, like Rhodes, has posed a rhetorical question, assuming that the near-decade that passed between Fermi's Super proposal and Teller and Ulam's discovery constituted some sort of anomaly in the process of nuclear weapons science. Hansen has assumed that the work of

Teller and Ulam constituted a "relatively-late discovery of the application of radiation implosion to U.S. thermonuclear weapons."[507]

Hansen does answer his own question of "Why Did It Take So Long?", citing three main reasons. First and foremost, the "late" discovery of radiation implosion is attributed to:

> . . . the stubborn refusal of some Los Alamos weaponeers, Edward Teller foremost among them, to consider anything other than the 'classical Super' design . . . Even after it became abundantly clear that this idea would not work, Teller still clung to it tenaciously . . . This single-minded obsession, coupled with Teller's dominating personality and influence over the entire program, doomed the both the consideration and the viability of competing ideas.[508]

While several of Teller's colleagues including Bethe, Bradbury, Wheeler, Ulam, and Oppenheimer have publicly stated that Teller's narrow focus on the Super blinded him and other scientists to other possibilities for hydrogen weapons, there is no way of proving that Teller himself stood as the biggest obstacle to an H-bomb. By arguing this, Hansen (along with Rhodes) has tried to create an answer to the rhetorical assumption that a workable thermonuclear weapon was indeed "delayed." Assuming that fusion weapons were delayed in development and arguing that Teller or others somehow overlooked radiation implosion tends to mystify the history of thermonuclear weapons and even perhaps "black-boxes" the fusion technology more than it already is because of the secrecy surrounding this work. I will return to this issue shortly.[509]

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[507] Hansen, Swords, III-183.
[508] Ibid., 183-184.
[509] Ibid., 184-185.

To his credit, Hansen's other two explanations for the "relatively-late discovery" of the Teller-Ulam configuration are more tangible. The second reason Hansen cites is the lack of data available between 1945 and 1950 regarding the fusion cross-sections of D and T, information significant to Ulam's fuel compression proposal of January 1951. Thus, the D-T cross sections may have been another latent critical problem that went unrecognized for several years after the war. Teller's group measured a few unreliable D-T cross sections during the war. According to Teller the measurements were optimistic and made the outlook for the Super appear favorable. Not until after President Truman's 1950 directive, James Tuck took up the cross section problems and measured D-D, D-T, and D-He^3. At first Tuck reported that the new cross sections contradicted those taken during the war, lending pessimism to the Super's viability. Apparently, though, Tuck subsequently re-ran the measurements which "vindicated the earlier optimistic predictions of the MED days."[510]

The deuterium cross sections constituted only one piece of measured information related to the Super's feasibility, but they did not constitute nearly as large a problem as did full calculations of the "Super Problem." The third reason Hansen cites for the "delay" in the H-bomb is the lack of powerful computers. As I have reviewed the critical problem of computing extensively in this dissertation I will not elaborate on Hansen's discussion of this, but rather note his acknowledgment of the problem:

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[510] Ibid., 187; Memorandum to the File from Kenneth Mansfield, "Conversation with Dr.

> Between 1945 and 1951, theoretical understanding and mathematical formulation of the properties of many thermonuclear reactions far outstripped the computational abilities of existing electromechanical devices to verify these phenomena.[511]

Computing did constitute an overwhelming critical problem to the H-bomb project, and for the period from 1942 through 1951, seemed an obvious obstacle to the Super and Alarm Clock projects. However, as far as computing posing an obstacle to *any* form of thermonuclear weapon or arguing that it slowed the overall program down is incorrect. Not only was computing merely one aspect of the postwar technological system, but the Soviet Union carried out its own thermonuclear research and development program without the aid of high-speed computers, an issue I will return to.

Computers or no computers, making any assumptions about the length of time it took for the development of a workable thermonuclear device does not answer the questions of why the program was so problematic. Indeed, if scholars can argue that the American H-bomb program went excessively slowly or got delayed, then it is just as easy to argue the contrary by asking the equally rhetorical question: why did H-bomb development actually only take a short time, if one bases the argument strictly on the political policy issues surrounding the program?

If fusion weapons technology is black-boxed and one focuses on the absence of any official policy or directive to build the H-bomb from 1942 through 1949, then the lack of policy may have been the biggest obstacle of all.

Teller," August 28, 1951, JCAE declassified General Subject Files, Box 58.

[511] Hansen, Swords, III-188.

Considering that less than three years passed between Truman's 1950 announcement to continue work on hydrogen weapons and the Mike test, then the period from 1942 to 1952 seems brief. However, this assumption does not explain why the H-bomb project's technical difficulties, either.

The ten year period over which scientists conceived and developed fusion weapons cannot be judged simply in terms of policy nor strictly in terms of critical technical problems but in terms of both, along with the project's social surroundings. The technological systems thesis includes all these considerations. The demands on systems, historically, change over time. In the case of thermonuclear weapons development, the MED technological system set in place during the war was inadequate for supporting a large H-bomb program both during the war and for several years afterwards. The successor AEC system, although in principle a civilian-controlled organization separate from the MED, was in practice placed figuratively on top of the older system. The early AEC system could not support any form of well-organized large weapons science program, much less sponsor H-bomb development. Not until the AEC's leaders reoriented the system technically, in response to the Russian atomic detonation, was the Commission capable of handling a full-scale thermonuclear test project.

The notion of policy, too, goes back to Los Alamos in a sense thus demonstrating the complicated nature of the system. Bradbury, felt that policy in the early postwar unofficially originated in Los Alamos. Although Los Alamos did not make AEC policies, Bradbury and the Laboratory did have an unofficial implicit directive for Super research in the postwar, which made sense in light of the fact that no official policy existed to address H-bomb work. As already noted, Bradbury offered to support work on thermonuclear weapons as personnel became available, and as long as it did not interfere with the fission program. The unofficial policy towards fusion weapons technology, then, held it as secondary to fission development. The GAC agreed completely with this.

It becomes even more apparent that the fusion bomb program cannot be analyzed or judged simply in terms of policy (or lack of policy) as the driving (or hindering) force behind the thermonuclear project, because when an official policy had been finally established with the politicization of the H-bomb in the fall of 1949, not only did Los Alamos still require three years to develop and test an H-bomb, but in this period the sheer magnitude of the technical difficulties (or critical problems) became clear.

Many of the scientific and politician-participants in the history of the American H-bomb project did, notably, comment publicly on the issue of lost time in the thermonuclear project. The Joint Committee, particularly, in 1953 judged that time had been lost in the H-bomb project because no government directive had been established prior to 1951. The JCAE assumed that if a full-scale program had been started in 1946, a thermonuclear weapon would have been completed within a few years. Therefore, the JCAE's Sterling Cole asked several prominent nuclear weapons scientists including Bethe, Bradbury, Fermi, Lawrence, von Neumann, Rabi, and Teller, to offer their personal assessments of the amount of time the U.S. "lost" in hydrogen bomb development.[512]

Teller responded with the most criticism, stating that four years and five months were lost in H-bomb development beginning in September 1945 when "work on thermonuclear bombs was practically stopped," and did not resume until February 1950. Hansen has noted, however, that Teller failed to mention to Cole his departure from Los Alamos in 1946, and his 1947 suggestion to delay work on the Super for two years. He also failed to distinguish for the Congressman the differences between the Classical Super and the Teller-Ulam concept, nor did Teller mention the Alarm Clock or the Greenhouse tests.[513]

Some of the other scientists the JCAE queried only contributed to the myth of the how much time was lost on the fusion bomb project. Von Neumann and Eugene Wigner together in person told Kenneth Mansfield that "we would now be a good deal further along than where we actually are" when asked how much time would have been saved if the steps towards thermonuclear weapons taken in 1950 had been taken in 1946. Von Neumann thought that "we picked up in 1950 almost exactly where we left off in 1946," leading Mansfield to label the period in between as one of quiescence. The inaction during this period von Neumann and Wigner

[512] Ibid., 193.
[513] Ibid., 195.

attributed mainly to an indifference to the problem of exploring thermonuclear energy.[514]

Wheeler, too, told Cole that he thought the American hydrogen bomb effort seemed "shamefully inadequate," and that if the project had been started in 1946 instead of 1950, there existed no good reason why the project would not have been four years ahead of where it stood now.[515]

Other scientists the Joint Committee questioned such as Fermi, Rabi, and Bradbury gave less drastic judgments than Teller, von Neumann, Wigner, and Wheeler about the amount of time lost on the H-bomb. Hansen cites Bradbury's response to Cole, where the Laboratory director explained how the Classical Super would have been inefficient and unrealistic. He also noted the absence of high speed computing machinery in 1946 that would have handicapped rapid theoretical work on the Super. Only starting in 1951, Bradbury argued, the computing machines "essential to the calculation of the weapons systems of today have been in existence . . . and without them, our present progress would have been impossible."[516]

Give Us This Weapon and We'll Rule the World

The frequent discussions in the historical records regarding time lost on the H-bomb no doubt led scholars such as Rhodes, Hansen, and others to raise the question "Why did the H-bomb take so long?" However, not only is this question rhetorical, but the assumption that the fusion program got

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[514] Memorandum to the files from Ken Mansfield, "Conversation with Dr. John von Neumann," November 9, 1953, JCAE declassified General Correspondence Files, Box 60.
[515] Rhodes, Dark Sun, 527; Quotation in Rhodes, 527.

drawn out, delayed, and generally a long time in the making originated in the Air Force and with those such as Teller who were critical of Los Alamos and the GAC and Oppenheimer. However, it is easy to import this sort of judgment into a historical narrative given that so many participants in the scientific, political, and military portions of the H-bomb projects iterated this critique.

To their credit, by asking why the thermonuclear bomb project took a long time and blaming it partly on Teller's obsession with the Super, Rhodes and Hansen have revealed the social nature of the project. Although I do not argue that the entire system surrounding the thermonuclear weapons project was completely social or socially constructed, and indeed included many very complex technical components, the social aspects surrounding the H-bomb project appear to become more apparent in retrospect after the project became politicized. Teller's and Wheeler's departures from Los Alamos, the Matterhorn group's founding, and the foundation of Livermore Laboratory all constituted social-political events, and examples of scientific groups on the move for their own distinct interests.

Specific events such as those above may be best viewed in terms of what Bruno Latour described as enrolling allies in one's scientific cause, and building networks to bring scattered resources in science into one or a few central places. Although Latour's use of history is scant, he does help to place technology and science in a social context. In the case of early hydrogen

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[516] Letter from Bradbury to Sterling Cole, December 2, 1953, cited in Hansen, Swords, III-97,

weapons development this kind of social context is obvious especially after 1949.[517]

Other groups besides the JCAE became intimately involved with the H-bomb project following the Soviet atomic test. Not least among them was the Air Force in January 1952, when David Griggs from the office of the Chief of Staff of the Air Force asked Teller to brief Secretary of the Air Force Thomas K. Finletter on thermonuclear weapon prospects. Teller used this meeting to raise the issue of starting a second weapons laboratory, a project Teller apparently felt Gordon Dean did not support. Soon after this meeting, Teller approached the JCS, MLC, and NSC concerning the same topic to further press his case.[518]

Teller caused enough excitement in his meeting (by apparently emphasizing how the AEC's H-bomb program lagged) with the Secretary of the Air Force that Griggs later would report to John Walker and Bill Borden that the Air Force felt that there had been "almost literally criminal negligence in the hydrogen program -- in the five year delay in starting a large scale effort, in the failure to establish a second laboratory, etc." Allegedly, Finletter had become so convinced by Teller of the H-bomb's potential he got to his feet and said "give us this weapon and we'll rule the world." Furthermore, and of more consequence to the AEC, after this meeting the

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quotation in Hansen, Swords, III-97.
[517] Latour, Science in Action, op. cit., 162, 172, 180.
[518] Walker Memo, April 17, 1952, op. cit.

Air Force threatened to establish its own second laboratory unless the AEC did so first.[519]

"The hydrogen program has suddenly come to a boil," Walker and Borden told McMahon, after Teller's response to Griggs, who made Teller and Walker agree to refer to him only as "Mr. X," lest he be fired by officials in the Secretary of the Air Force's office since he apparently did not obtain official approval to speak with Walker. Griggs and some of his Air Force colleagues even personally contacted Fermi, Turkevich, Urey, Lawrence, and others about coming to work at the proposed second laboratory.[520]

Teller rallied the Secretary of the Air Force to his cause; by mid-1952 Finletter thought that the Air Force's philosophy now called for a complete changeover to H-bombs from A-bombs, for both strategic and tactical uses, as he predicted that the "real future competition with Russia would be in the thermonuclear field." In a sense, Finletter was correct because in the following year the Soviet Union did test a thermonuclear device. Just how "thermonuclear" this test was has been debated, but it had sorts of significance that I will discuss shortly.[521]

By 1952 Teller, and to a lesser degree others such as Lawrence and von Neumann, raised enough concern in Washington that criticism of the AEC

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519 Memorandum to Brien McMahon from John Walker and William Borden, April 4, 1952, JCAE General Correspondence Files, Box 59; Memorandum to the Files from John Walker, "Thermonuclear Matters and the Department of Defense," October 3, 1952, JCAE declassified General Subject Files, Box 59.

520 Walker and Borden Memo, April 4, 1952; Memorandum from Walker to Borden, "Thermo Nuclear Program," April 7, 1952, JCAE General Suject Files, Box 59.

521 Memorandum to the File from Bill Borden and John Walker, "Thermonuclear Program – Conference with Secretary Finletter," June 24, 1952, JCAE General Subject Files, Box 59.

became common. Conservative military analyst Bernard Brodie called Los Alamos a "national disaster" after visiting the Laboratory in February of 1952. He went as far as to charge that Los Alamos was made up almost entirely of third rate scientists, and described Bradbury as a "small man, not equal to his job."[522]

Brodie truly believed that "The hydrogen weapon offers us our only real hope of stopping the Red Army," because the military viewed it by now as an "area" weapon that didn't require precise delivery upon dispersed troops. When he tried to impress his belief in the H-bomb's importance upon Darol Froman at Los Alamos, Brodie claimed that Froman, as well as Bradbury, played down its military importance. He even recommended to the JCAE that the entire Los Alamos directorate be replaced.[523]

Teller succeeded in establishing a second laboratory, after obtaining many allies in the military and political arenas. Gordon Dean and the AEC had little choice but to support a second laboratory after the Air Force threatened to do this in Chicago if the AEC would not. In June 1952, the Commission proposed that the University of California begin managing the new laboratory in the same manner it did Los Alamos. By July the University of California accepted the AEC's proposal and Lawrence eagerly offered the Radiation Laboratory as a temporary home for the new weapons laboratory. Originally called Project Whitney, Herbert York led this effort by late 1952

[522] Memorandum to the Files from Kenneth Mansfield, "Bernard Brodie on the Hydrogen Bomb Program," March 13, 1952, JCAE declassified General Correspondence Files, Box 59.
[523] Ibid; Underlined in original.

directing the work of about seventy-two scientists recruited for the project. The members of Project Whitney planned to stage large-scale nuclear weapons tests by 1953.[524]

Not long after the AEC officially sanctioned the construction of the new Laboratory, Teller lamented to Walker that both the theoretical and practical difficulties in the thermonuclear field had been badly overemphasized. Teller informed Walker that:

> Perhaps . . . H-bombs are much easier, much simpler and much less complex than had been the universal assumption. This comment has also been made of our fission program. It is clear that atomic weapons have always been thought of in their semi-absolute sense, embodying exquisite tolerances and the most expensive engineering. Particularly in the H-bomb field we may have erred on our assumption of difficulty. The consequence of this error insofar as possible Russian achievements are clear to see.[525]

Suggestions for Further Study -- The Russian Los Alamos and Stalin's Technological System

Some of the most important and ironic considerations to include when examining the American H-bomb program were the Russian achievements. It is remarkable that Russia, as part of the Soviet Union, designed and tested an atomic weapon (albeit the design of the device was taken directly from the American Fat Man bomb vis-à-vis Klaus Fuchs) by 1949, and a hydrogen weapon by 1953. The latter, notably, was an independent creation. It is even more remarkable that a nation with no

[524] Memorandum to the File from John Walker, "Project Whitney," November 10, 1952, JCAE declassified General Correspondence Files, Box 60.
[525] Ibid.

tradition of indigenous science possessed the ability to catch up with the West as fast as it did in the twentieth century, and moreover, over the Cold War develop its own nuclear arsenal almost pacing that of the United States.[526]

David Holloway has to date completed the most exhaustive, accurate, and well-written history of the Soviet atomic project, Stalin and the Bomb (1994). Although the first Soviet atomic bomb, Holloway explains, had been a copy of the first American fission weapon, the first Soviet hydrogen device was an "original design." The main effort Soviet scientists made towards an H-bomb was the "Layer Cake" or Sloika design. It employed Vitali Ginzburg's idea of using lithium-deuteride fuel (instead of D-T) and Andrei Sakharov's notion of ionization compression of the fuel. It yielded only around 400 kilotons and was more fission, than fusion powered. It used solid-fuel, though, and could be far more easily delivered by aircraft than Mike.[527]

Holloway notes that it is somewhat a matter of taste whether the 1953 Sloika (Joe-4) test was a thermonuclear or boosted weapon, but importantly the Stalin and the USSR viewed as a true hydrogen bomb. Even more relevant, though, is what the Soviet's chose *not* to pursue in lieu of the Sloika and subsequent Teller-Ulam type weapons.[528] Sakharov wrote:

> "We devoted minimal thought and effort to the 'classical' device; we recognized the risks of neglect, but we were convinced that our strategy would pay off. Our resources were too limited to pursue both tracks

[526] For more on the history of the introduction of science into Russia, see Loren R. Graham, Science in Russia and the Soviet Union: A Short History, (Cambridge: Cambridge University Press, 1993).
[527] David Holloway, Stalin and the Bomb, 303; German A. Goncherov, "Milestones in the History of Hydrogen Bomb Construction in the Soviet Union and the United States," Physics Today, November 1996, 44-61.
[528] Holloway, Stalin and the Bomb, 308.

> aggressively. And in any case, we couldn't envision an approach that would radically improve the first choice." [529]

Not only did the Soviet thermonuclear weapon program take an independent course from the American project, but the way that weapons designers carried out weapons science differed in terms of technological sophistication, and the unique critical problems Soviet scientists encountered. Without the aid of fast electronic digital computers in the early 1950s, the Soviets nevertheless completed calculations for the Sloika test as Sakharov describes:

> The theoretical groups played a key role in the first thermonuclear test . . . The actual numerical calculations were performed by secret mathematical teams in several scientific research institutes in Moscow. . . . It was necessary first of all to develop calculation methods that would not be nullified by the small errors that were bound to occur, and that would still, without inordinate amount of work, yield sufficiently precise results. The computations themselves were straightforward, almost mechanical, but extremely time-consuming. At first, they were performed by brigades of human calculators; later by computers . . . . (our use of computers accelerated after 1953).[530]

The role that computers played in Soviet nuclear weapons development and in nuclear and high-energy physics remains open for study, along with the evolution of the technological system with the former USSR supported thermonuclear weapons research and development. Likewise, no scholarly history of the "Russian Los Alamos" -- Sarov (Arzamas-16) -- exists.

Besides computing, and the scientific dramatis personae of the hydrogen weapon project, the practice of the "science" of nuclear weapons

[529] Andrei Sakharov, Memoirs, (New York: Vintage Books, 1992), 183.

science in the Soviet Union resulted in technologies unique to that nation. The first Soviet hydrogen weapon is one example. Later, more advanced devices resembled the American stockpile but saw development under different social, scientific, and technical conditions; in other words, under the Soviet technological system headed by the Medium Machine-Building Industry, which was responsible for the overall Soviet nuclear weapons program.[531]

Just how much the Soviet system of nuclear weapons-making resembled its American counterpart remains unstudied. Moreover, if scholars are to make judgments about the length of time that bomb-building projects required, a comparative study of the two technological systems is necessary and even crucial, because the two systems provide a context for one another historically.

More Suggestions for Further Study

One aspect of the American H-bomb program that I have not addressed in this dissertation are weapons developed in the period immediately following the Mike test, when Los Alamos began a concerted effort on the Alarm Clock and subsequent, more easily deliverable designs.

Even before the Mike test, the Joint Committee grew aware that the Teller-Ulam idea did not represent the only path to a workable thermonuclear weapon. Walker reported to the JCAE after a visit to Los Alamos in September 1952 that the feasibility of H-bombs was not only

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[530] Sakharov, Memoirs, 156.

already established but also the TX-14, or new Alarm Clock, was underway at Los Alamos.[532]

Even as the Theoretical Megaton Group at Los Alamos hurriedly worked through the theoretical problems associated with the Mike test, the Laboratory and AEC committed themselves to exploring and developing the Alarm Clock, sometimes referred to as a "dry" device because of its fuel composition. Because of this it had a tremendous deliverability advantage over the gigantic liquid-deuterium fueled Mike type of weapon; the former constituted a much smaller device. The Mike type of device was, like the Classical Super, not practical because it's large size, mainly due to the huge volume of liquid D it contained.

The new Alarm Clock bore little resemblance to its predecessor with the same name proposed by Teller and Richtmyer in 1946. However, this project, like the Super, faced critical problems originating in the AEC system -- one in particular involved nuclear materials production.

Teller had unknowingly solved part of the deliverability problem associated with the Classical Super and original Alarm Clock by 1947, when he suggested using an alternative thermonuclear fuel to liquid deuterium. Although the technical details of the new Alarm Clock's design are restricted to this day, the concept suffered from bottlenecks to those facing the Super, particularly nuclear materials availability.[533]

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[531] Holloway, Stalin and the Bomb, 306.
[532] Memorandum to the Files from John Walker, "Status Report on the Thermonuclear Program," September 12, 1952, JCAE declassified General Subject Files, Box 59.
[533] Rhodes, Dark Sun, 306.

Even after scientists proposed the new Alarm Clock, not only did the problem of igniting it remain unsolved until Teller and Ulam's 1951 discovery, but the AEC had no plant to produce fuel for it. Not until 1951 did the Commission direct Oak Ridge Laboratory to begin to design a plant that would separate and produce lithium-6. By now, even as Richtmyer lead a team running calculations for the Alarm Clock on the SEAC, having completed about nine cycles by September, a test of the device would have to wait. Fermi advised Bradbury and others at Los Alamos that Li^6 production would push a test of the Alarm Clock back by two years. It did more than that, as the U.S. did not test an Alarm Clock until 1954.[534]

In part, the shortage of lithium explains why scientists chose to develop and test Mike before the Alarm Clock, even though the liquid deuterium-fueled device would never make a practical, deliverable weapon. After Los Alamos's scientists developed the Alarm Clock and introduced staged thermonuclear devices and other advanced designs, the AEC system grew, where both the number of nuclear materials plants and their output increased.

While Hansen has explored some of the evolution of the Alarm Clock device, and in great detail the evolution of nuclear weapons in general to the present day, the history of Los Alamos in the postwar as part of the AEC system has yet to be studied. Furthermore, the increasing role that computing played in, for example, the development of the Alarm Clock and dry, and

[534] "First Thermonuclear Meeting," September 10, 1951, LANL Archives, B-9 Files, Folder 334,

multistage thermonuclear weapons, and also in nuclear weapons science overall is a wide open field for historians, sociologists, STS specialists, and scholars from other disciplines.

Also waiting for historical examination is the evolution and growth of the AEC system itself over the course of the Cold War. This kind of study remains an overwhelming task for historians of science and technology. It poses a challenge due not only to its size, but also because of the classified nature of nuclear weapons physics and related work.

Classified or not, out of this system of laboratories, industry, and military interests came advances in computing, high-energy physics, experimental mathematics, and even entirely new scientific disciplines such as complexity studies. It was no coincidence that many of the origins of complex systems studies are traceable to Los Alamos, where Fermi, Ulam, John Pasta, and Mary Tsingou in the postwar began to employ the MANIAC for studies of nonlinear dynamics in 1953 -- work that grew directly out of mathematical treatments of thermonuclear weapons. A scholarly history of complexity studies at Los Alamos awaits exploration.[535]

Peter Galison has explored the history of the Monte Carlo technique, for example, and the impact it had on twentieth century high-energy physics. Still, more historical research on the American National Laboratories and the numerous mathematical techniques developed (neutron transport methods,

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Drawer 75, [This Document is Secret-RD].

[535] E Fermi, J. Pasta, S. Ulam, "Studies of Nonlinear Problems. I," LA-1940, Los Alamos Scientific Laboratory, May 1955.

for instance) within them that also impacted on twentieth century science and technology remain open for study. Finally, a comprehensive study of Los Alamos Laboratory on its own and as a part of the AEC system in the Bradbury years remains to be seen.

The technological system within which scientists worked to complete the H-bomb project, like Hughes's Edisonian system of electrical power, grew within a context of geographical, economic, political, and organizational factors. In the Edisonian system, old and new systems existed together in a struggle where the new system emerged as a result of the failure to solve a major problem in the old system. The AEC system evolved differently, though, in that the MED and AEC did not exist at the same time in a struggle for existence. Rather, Congress superimposed the AEC system on the older MED system, and likewise, Los Alamos also was based on a temporary wartime infrastructure. Thus, when critical problems for the H-bomb appeared, the system could not be brought in line to solve them right away.[536]

The inability to solve these problems frustrated Teller and his colleagues, and no doubt caused them to speak in terms of the H-bomb taking a long time. When Teller told Borden and Walker in 1951 of his resignation from Los Alamos, he gave his opinion that, "criticism for failure to have achieved a super weapon by this time should be shared by almost everyone

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[536] Hughes, Networks of Power, 79.

concerned," particularly the AEC and the way it had managed its materials production laboratories.[537]

Almost fifty years later, the notion of blame for not developing the H-bomb as fast as possible seems irrelevant and even absurd when the issue of nuclear nonproliferation is heard almost every day. The weapons -- although fewer in number than at some points in the Cold War -- and the system, now in the form of the U.S. Department of Energy, remain. Moreover, several other nations have attained nuclear weapons capabilities during the last fifty years, each as frightening and possessing the potential for horrible consequences as the Mike test.

Visiting Los Alamos in the summer of 1994, Edward Teller stated that:

> The question whether it [the hydrogen bomb] is horrible or not is an important one. But, if it is horrible, and to the extent that it is horrible, the conclusion should not be that we shouldn't develop it.[538]

The issue of whether or not the United States should or should not have developed the hydrogen bomb, although beyond the scope of this dissertation, remains a crucial and enduring question invoking strong emotions and political disagreement. Thus, understanding the origins of this problem, the nature of the system surrounding it, and the modern successor

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[537] Memorandum for the File from John Walker, "Lunch Meeting with Dr. Teller," October 3, 1951, op. cit.
[538] Author interview with Teller, op. cit.

systems that guided these efforts is relevant and significant for the human characters who intend to accomplish any successful, continuing efforts towards nonproliferation and disarmament.

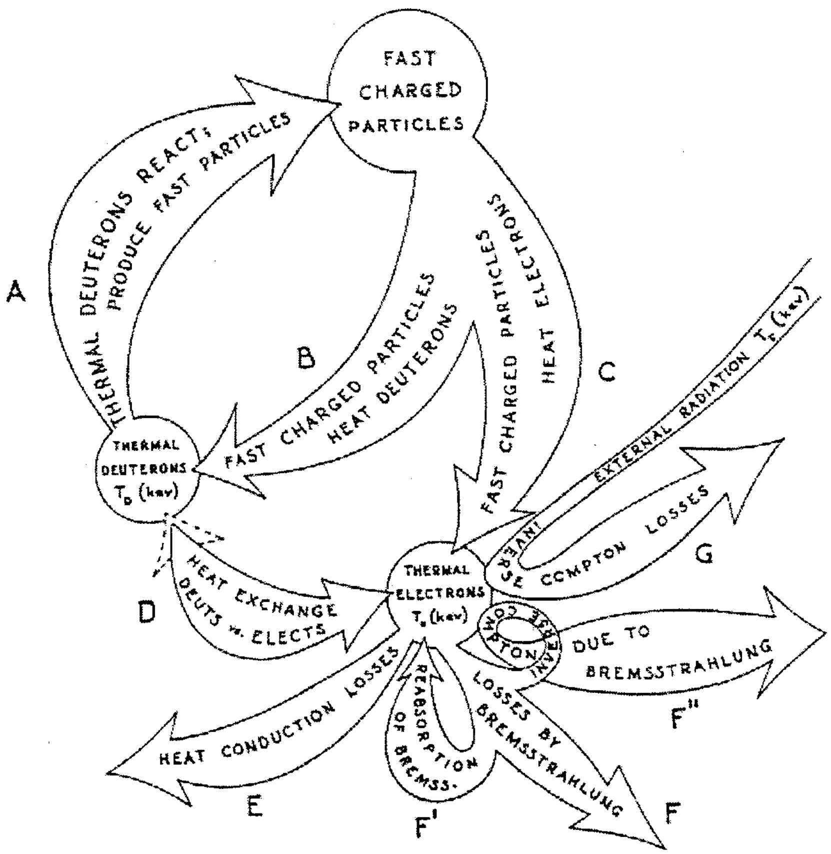

Figure 1: Drawing by George Gamow. Harris Mayer, LAMS-1066, "Daddy Pocketbook: A Summary of Lectures by Edward Teller," January 25, 1950. [This Document is Secret-RD]. Drawing is declassified.

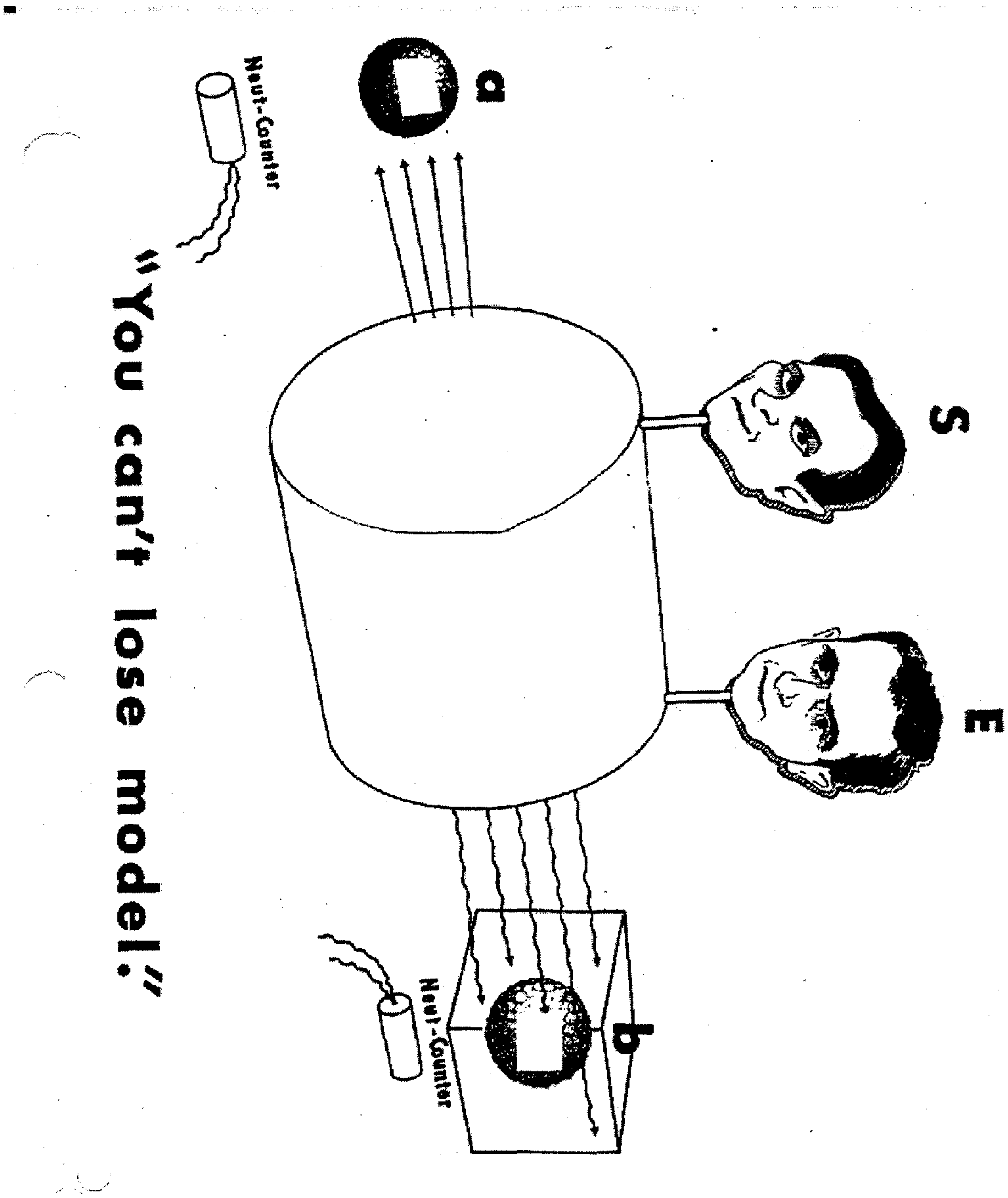

Figure 2: Drawing by George Gamow. LAB-ADWD-25, "Proposals in the Direction of the Super," January 14, 1949. [This Document is Secret-RD]. Drawing is declassified.

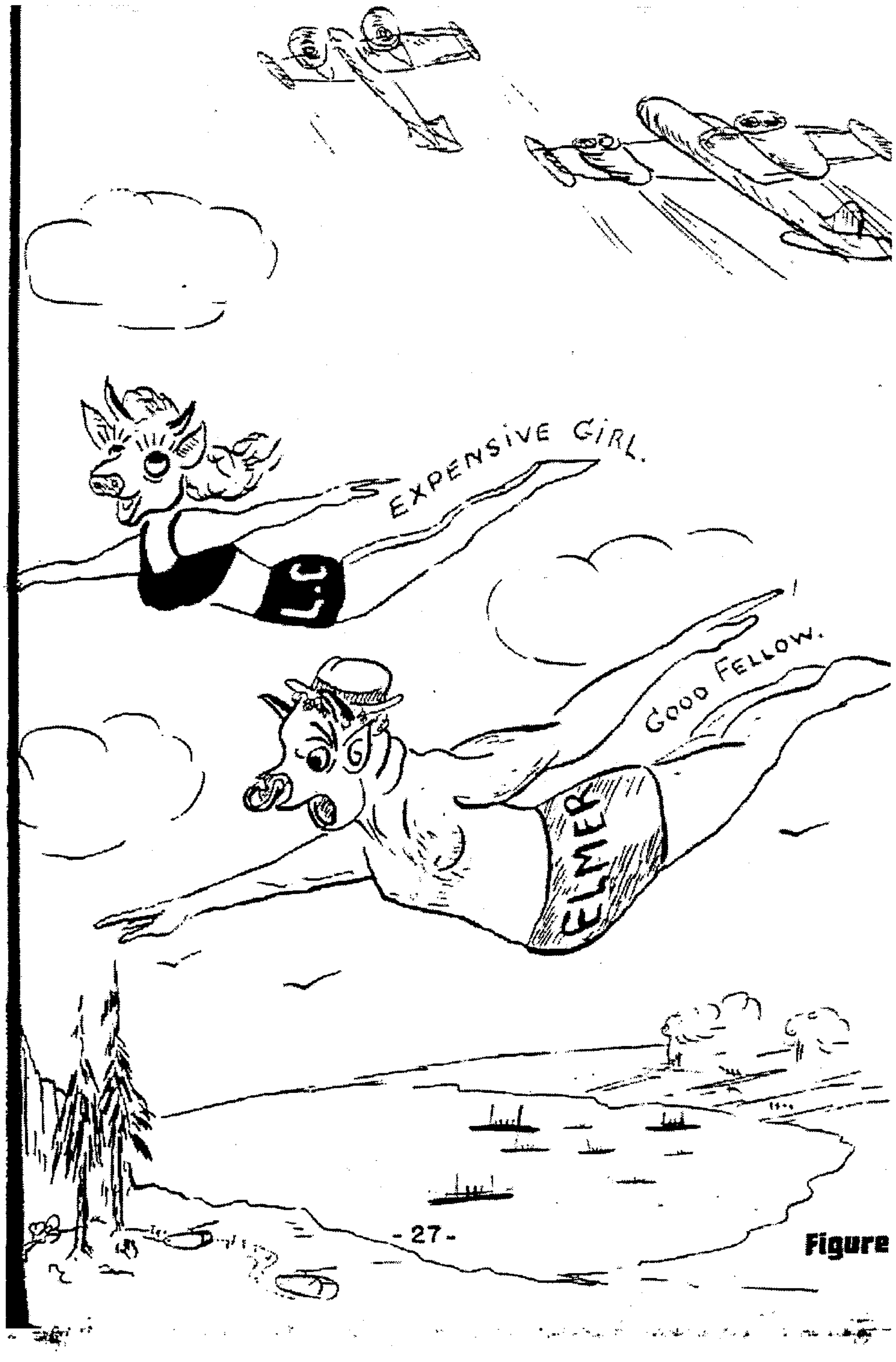

Figure 3: Drawing by George Gamow. From: George Gamow, LA-1194, "Targets, Bombs, and Delivery Methods,"September 20, 1950. [This document is Secret-RD]. Drawing is declassified.

Appendix A:

List of Acronyms

Government Agencies, Committees, Corporations, and other Institutions:

AEC...U.S. Atomic Energy Commission
AFSWP.......................................Armed Forces Special Weapons Project
CWD...Committee for Weapons Development
DOE..U.S. Department of Energy
DSWA...Defense Special Weapons Agency
GAC..AEC General Advisory Committee
IAS..Institute for Advanced Study
IBM...International Business Machines Corporation
LOC..Library of Congress
JCAE...Joint Committee on Atomic Energy
JCS..Joint Chiefs of Staff
LANL..Los Alamos National Laboratory
LASL...Los Alamos Scientific Laboratory
LLNL...Lawrence Livermore National Laboratory
MED..Manhattan Engineer District
MLC..Military Liaison Committee
NARA...National Archives and Records Administration
NSC...National Security Council
SED...Special Engineering Detachment
SAC...Strategic Air Command
TMG..Theoretical Megaton Group
USC...University of Southern California

Computer Names:

ENIAC...Electronic Numeric Integrator and Calculator
MANIAC.....................................Mathematical and Numeric Integrator and Calculator
PCAM..Punched-Card Accounting Machine
SEAC...Standards Eastern Automatic Computer
SSEC..Selective Sequence Electronic Calculator
UNIVAC......................................UNIVersal Automatic Computer

Bibliographic Note

Some of the sources cited in this dissertation are classified. Many are labeled "Secret-Restricted Data," (or "Secret-RD", or "SRD" for short), a category used by the DOE to signify written materials that in principle contain critical nuclear weapons data.

At the time I researched and wrote this dissertation I held a DOE "Q" clearance, and was a Los Alamos National Laboratory employee. Without these opportunities, I would never have been able to undertake this study because nearly all documented information associated with fusion weapons is SRD. The process of researching and writing this study became a proverbial double-edged sword: I could view SRD materials, yet I could not discuss specific details of weapons design. Furthermore, each chapter of this study had to be reviewed by a derivative classifier to insure it did not contain any classified information before it was released to the public.

DOE employees are bound by a "no comment" policy regarding certain published studies that address technical aspects of nuclear weapons design, thus I could not assess the technical accuracy of two of the most well-known published studies that analyze the nuclear weapons complex. To my initial surprise, classification extends even beyond references to American-made nuclear weapons; according to the DOE I could not comment on the technical details of Russian weapons, because of the threat of nuclear proliferation.

My hope is to make other scholars aware of the original sources available through the Freedom of Information Act (FOIA). All of the documents cited in this study are subject to FOIA, and thus are also subject to scholarly peer review. Often, much of what constitutes classified information is trivial, and consists of engineering-related details of nuclear weapons design. There are no fabricated report titles or numbers in this dissertation. In the DOE's classification system, SRD report titles are often unclassified. In a couple of instances I was asked by the derivative classifiers to leave out technical names of weapons, for which I substituted the generic term "special."

The assertions and conclusions I made in this dissertation are solely my own, and not those of Los Alamos National Laboratory or the U.S. Department of Energy.

No history is objective. Indeed, becoming a Los Alamos Laboratory employee altered some of my prejudices about politics, science, and technology and undoubtedly shaped the convictions in and scope of this study. Nevertheless, it was a fantastic experience.

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