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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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This dissertation is dedicated to Michael Henderson, nuclear weapons wizard and mentor to this historian.

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

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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

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[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

[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.

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[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

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[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.

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[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

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[21]Peter Galison and Barton J. Bernstein, "In Any Light: Scientists and the Decision to Build the Superbomb, 1952-1954," Historical Studies in the Physical and Biological Sciences, 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.

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[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

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[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

[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

[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."

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[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

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[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

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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

[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.