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

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[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

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[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

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[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

[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.

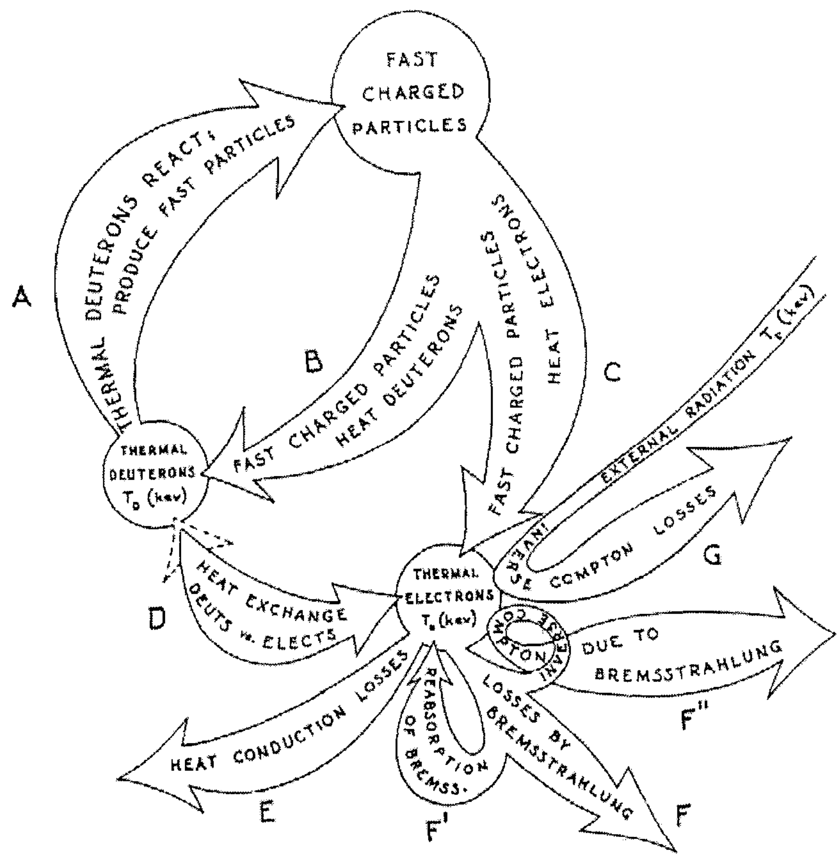

Figure 1: Drawing by George Gamow. Harris Mayer, LAMS-1066, "Daddy Pocketbook: A Summary of Lectures by Edward Teller," January 25, 1950. [This Document is Secret-RD]. Drawing is declassified.

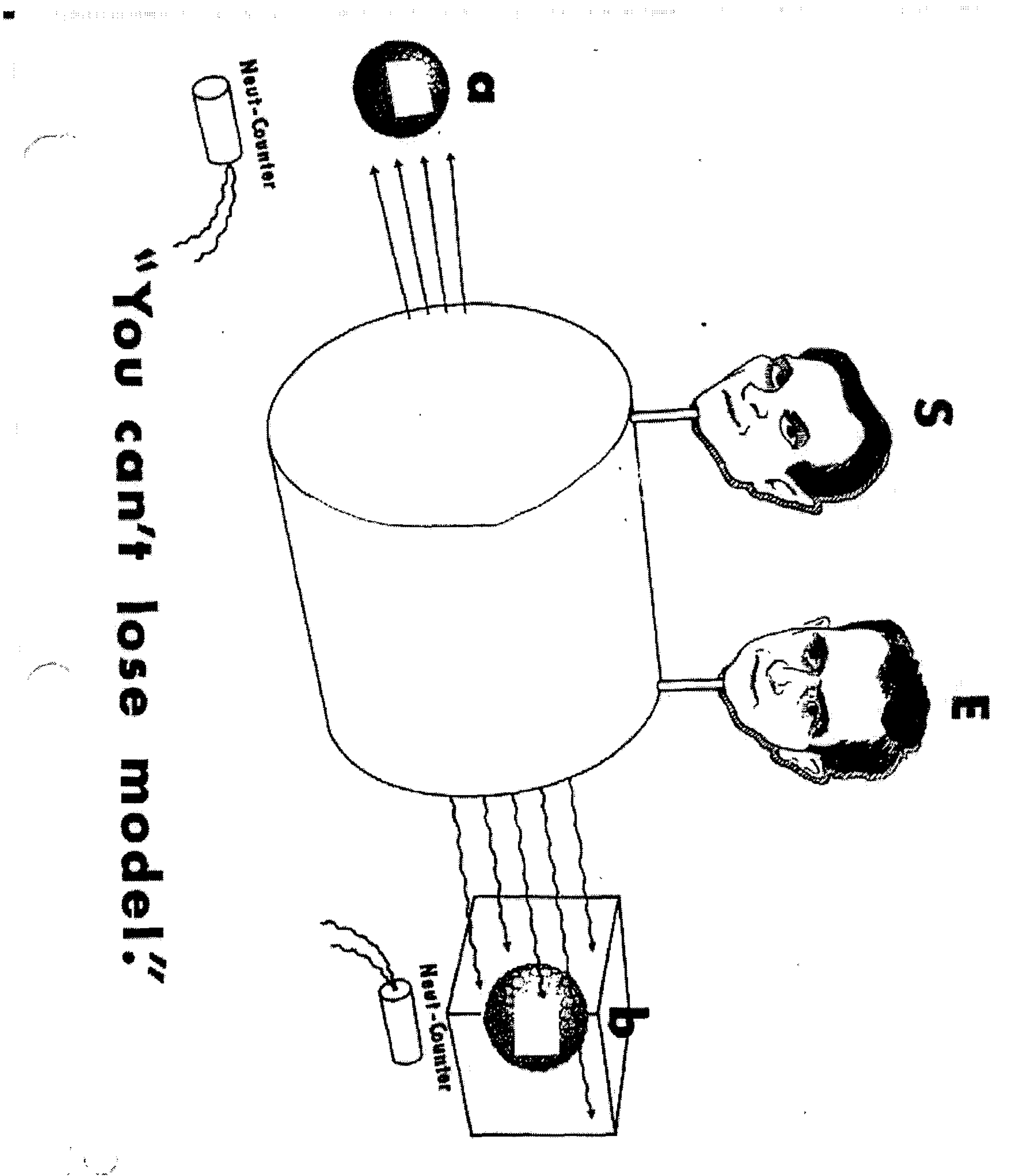

Figure 2: Drawing by George Gamow. LAB-ADWD-25, "Proposals in the Direction of the Super," January 14, 1949. [This Document is Secret-RD]. Drawing is declassified.

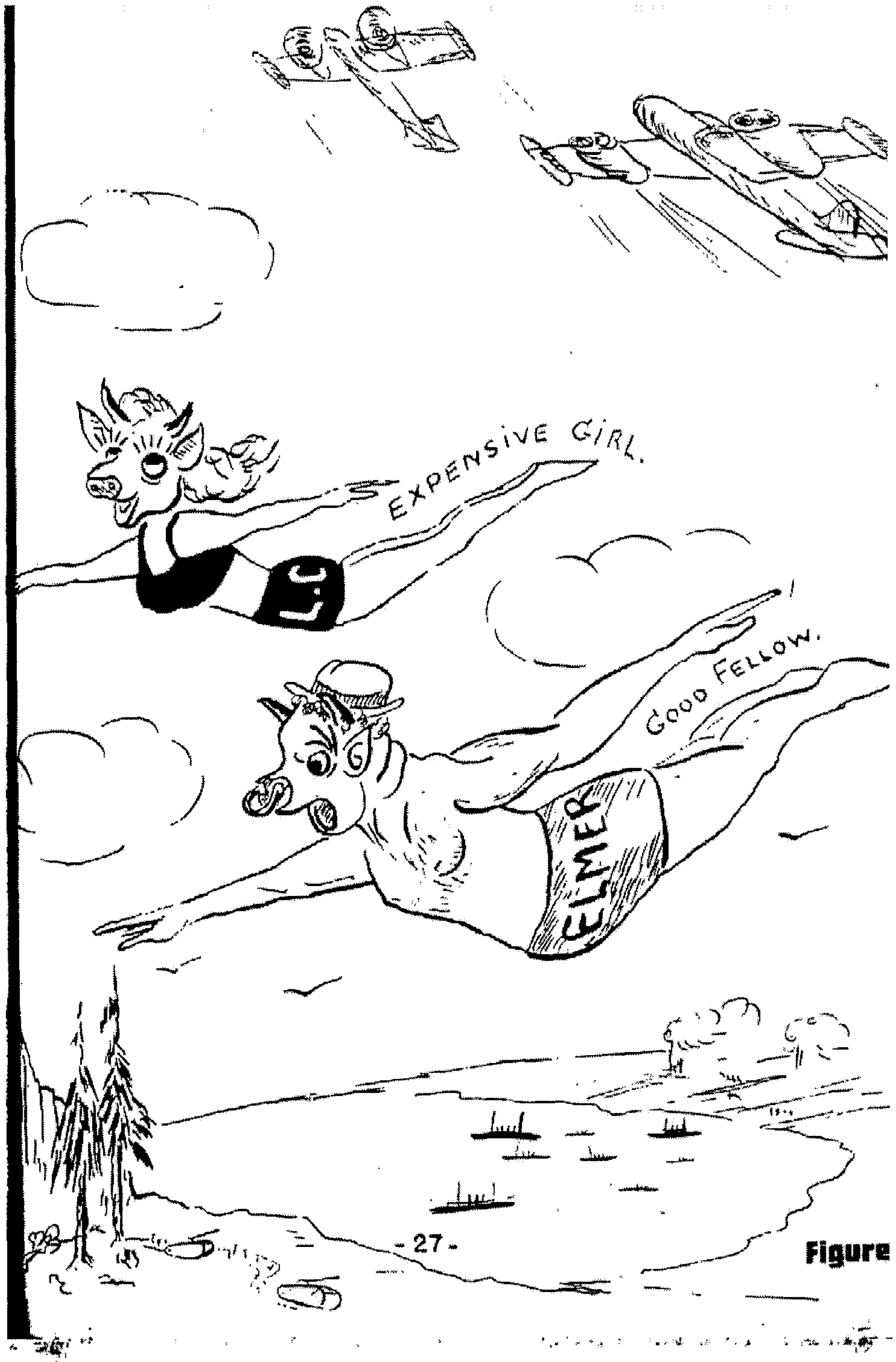

Figure 3: Drawing by George Gamow. From: George Gamow, LA-1194, "Targets, Bombs, and Delivery Methods," September 20, 1950. [This document is Secret-RD]. Drawing is declassified.

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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