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Norris Bradbury and Edward Teller
A Fission-Fusion Reaction
R. A. Meade

A stands for atom; it is so small
No one has ever seen it at all.

B stands for bomb; the bombs are much bigger,
So, brother, do not be too fast on the trigger.

H has become a most ominous letter.
It means something bigger if not something better.[1]

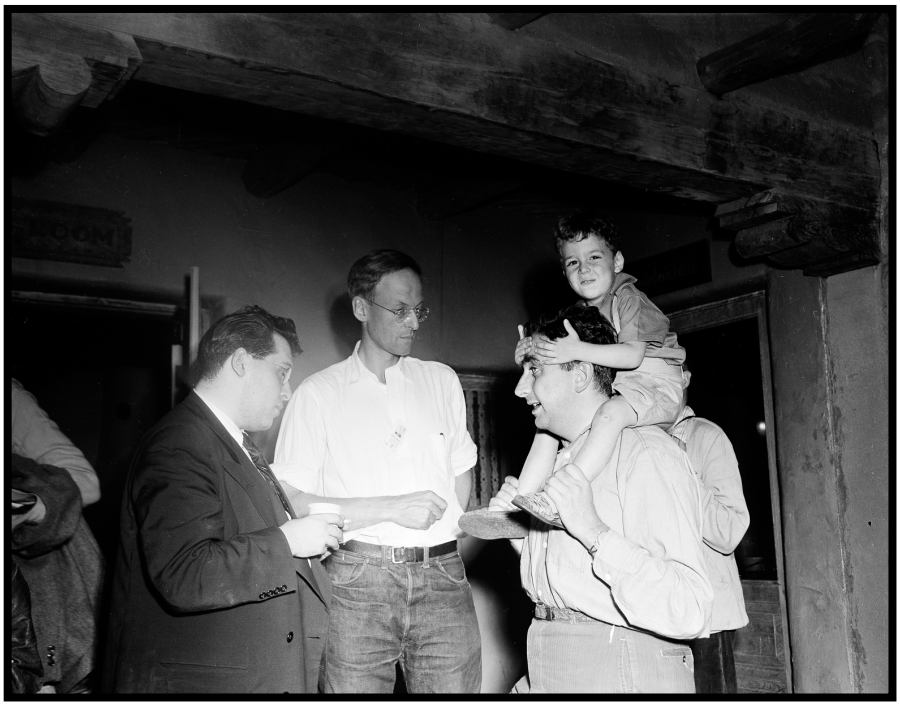

Figure 1: Paul Teller on his father's shoulders.

Los Alamos 1945

On a bright, sunny day in October 1945, a boisterous and jubilant crowd assembled in front of Fuller Lodge to watch as the Army-Navy "E" Award, a prestigious national honor given for "Excellence in Production" during World War II, was presented to the Los Alamos Laboratory. Major General Leslie Groves, the commanding officer of the Manhattan Project; Navy Commodore William S. (Deak) Parsons, the wartime leader

[1] Poem written by Edward Teller for his son, Paul. Reprinted in Life Magazine, September 6, 1954, p. 74.

of the Laboratory's Ordnance Division; Robert Sproul, the President of the University of California; and J. Robert Oppenheimer accepted the award on behalf of the Laboratory.

Although the war was over and Los Alamos had emerged from behind a curtain of secrecy to world-wide notoriety, Oppenheimer, in his last official act as Laboratory Director, sounded a note of caution in a short speech, saying, *"It did not take atomic weapons to make war terrible. It did not take weapons to make man want peace, a peace that would last. But the atomic bomb was the turn of the screw. It has made the prospect of future war unendurable. It has led us up those last steps to the mountain pass; and beyond there to a different country."*[2]

Figure 2: Oppenheimer, Groves, and Sproul with the "E" Award. The accompanying "E" flag has just been raised.

Despite Oppenheimer's eloquence and somber warning, his speech had little meaning for Norris Bradbury. As Oppenheimer's successor, he faced the daunting task of keeping the Laboratory, now without a mandate, operational until the President and Congress decided on the nation's postwar nuclear policy. Bradbury knew, however, that when Los Alamos was given a new mission, that mission would be the replacement of the crude experiments that were Little Boy and Fat Man with better fission weapons.[3] That belief became the foundation for Bradbury's management of the Laboratory in the years immediately following the end of the war, but it came at the cost of placing him at odds with Edward Teller.

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[2] J. Robert Oppenheimer, *Manhattan District Reunion*, (Los Alamos: Los Alamos National Laboratory, 1993), 1.

[3] At the end of the war, the United States had only one atomic bomb, or, to be more precise, the unassembled parts for one bomb and no concrete plans to produce any more.

Oppenheimer's speech also had little meaning for Edward Teller, who was beginning a public campaign to force the rapid development of the hydrogen bomb.[4] When Bradbury chose to focus the Laboratory's work on fission weapons, Teller told *Time Magazine* that the situation at Los Alamos was "catastrophic."[5] Teller's criticism of Bradbury and Los Alamos did not abate until 1954, two years after the first successful hydrogen bomb test. During that year, Teller, after testifying against Oppenheimer, seemingly took full credit for the hydrogen bomb. A *Life* Magazine said, among other things, that Teller, "*by an almost fanatic determination, kept the idea of an H-Bomb from dying of pure neglect."*[6] With that one sentence, *Life* described the essence of the *"fission-fusion"* relationship between Bradbury and Teller. Los Alamos did all the work and Teller given all the credit.

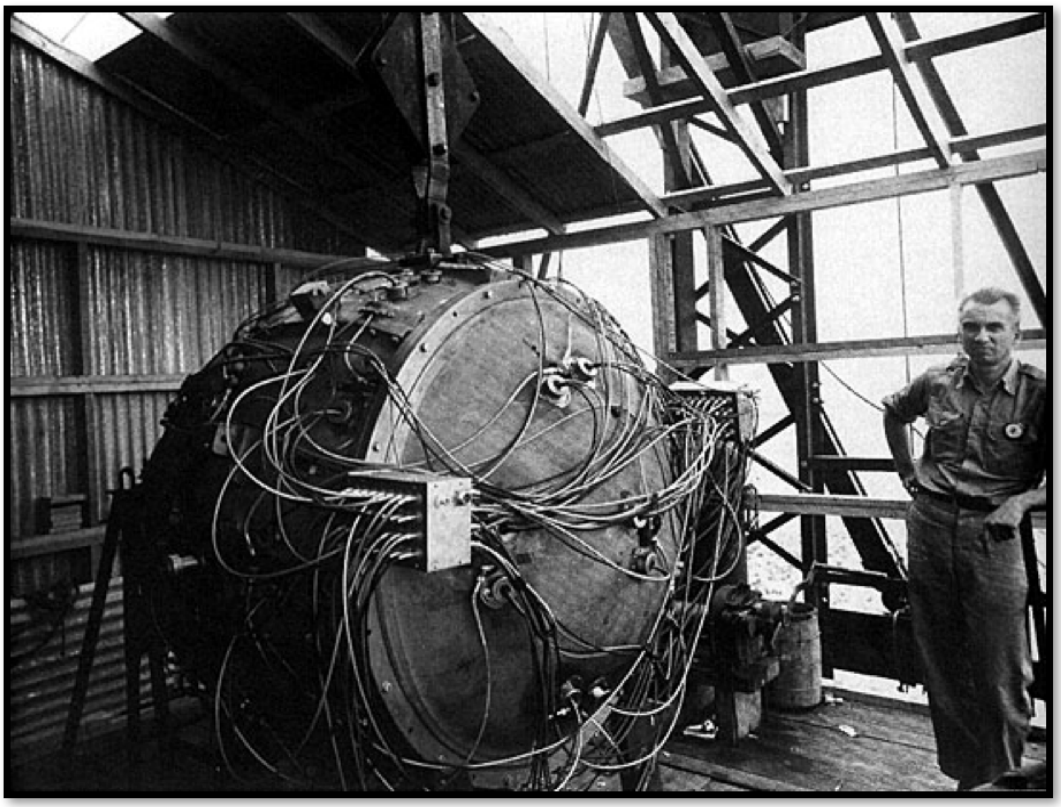

Figure 3: Bradbury and Trinity Gadget

[4] The terms "super", "hydrogen bomb", and "thermonuclear device" are used interchangeably.

[5] Time Magazine, February 25, 1946.

[6] Robert Coughlin, "Dr. Edward Teller's Magnificent Obsession." Life Magazine, September 6, 1954, p. 61-74.

The Hydrogen Bomb – National Politics

In July 1949, at the request of the Joint Chiefs of Staff, President Truman created a special subcommittee of the National Security Council (NSC) *"to assess the rate of progress being made in our atomic program."* The subcommittee - Secretary of State Dean Acheson, Secretary of Defense Louis Johnson, and AEC Chairman David Lilienthal - quickly determined that the nation's nuclear infrastructure was inadequate, particularly with respect to the production of fissionable materials and recommended that the nation's atomic program be accelerated.[7]

Figure 4: NSC Special Subcommittee.

Despite the subcommittee's findings, there was no sense of urgency or panic. The United States had, after all, an atomic bomb monopoly. That monopoly, however, ended suddenly on August 8, 1949, when the Soviet Union detonated its first atomic bomb, codenamed Joe-1. President Truman announced the Soviet detonation on September 23rd, saying: "*I believe the American people, to the fullest extent consistent with national security, are entitled to be informed of all developments in the field of atomic energy. That is my reason for making public the following information. We have evidence that within recent weeks an atomic explosion occurred in the U.S.S.R. Ever since atomic energy was first released by man, the eventual development of this new force by other nations was to be expected*."[8]

[7] Harry S. Truman, Years of Trial and Hope, (New York: Double Day, 1956), 302; McGeorge Bundy, Danger and Survival, (New York: Random House, 1988), 203

[8] http://www.atomicarchive.com/Docs/Hydrogen/SovietAB.shtml; Furer,114.

In the wake of Joe-1, Lewis Strauss sent a memo to his fellow AEC commissioners saying "*the time has now come for a quantum jump in our planning … that is to say, that we should now make an intensive effort to go ahead with the Super.*"[9] Strauss' memo "*sparked a secret debate within the government about whether or not to initiate a crash program to develop the hydrogen bomb."*[10] Senator Brien McMahon, Chairman of the Joint Committee on Atomic Energy, called for "*a crash program to develop the super."* Teller told colleagues at Los Alamos that "*It seems that the Russian rate of progress is at least comparable to, if it does not exceed, the rate of progress in this country*."[11]

Figure 5; Brien McMahon, D-Ct.

One of Strauss'AEC colleagues, David Lilienthal disagreed. Such a weapon, Lilienthal noted, would indiscriminately kill too many innocent people. Hence, there was no military need or justification for the hydrogen bomb. Oppenheimer also thought the hydrogen bomb was not needed for national defense. The growing stockpile of fission weapons was more than sufficient to protect the country. [12]

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[9] Lewis Strauss, Men and Decisions (Garden City, NY: Doubleday, 1962), 217.

[10] Dean Acheson, Present at the Creation: My Years in the State Department (New York: W.W. Norton, 1969), 344; Gordon Dean, Forging the Atomic Shield (Chapel Hill: The University of North Carolina Press, 1987), 35; Herbert York, The Advisors: Oppenheimer, Teller and the Super bomb (Stanford: Stanford University Press, 1976), 45; Strauss, 222**.**

[11] Edward Teller, To Technical Council Members, LANL Archives, October 12, 1949.

[12] Dean Acheson, Present at the Creation (New York: W.W. Norton, 1969), 344 – 346; Dean**,** 18.

Figure 6: Lewis Strauss.

Seeking to blunt the opposition to the hydrogen bomb, Strauss asked Admiral Sidney Souers, the executive director of the National Security Council, if the President knew about the possibility of a hydrogen bomb. Souers didn't know and told Strauss he would ask Truman. As Souers recalled many years later, "*I asked him [the President] if he had any information on it. He said, 'No, but you tell Strauss to go to it and fast.'*"[13] Strauss subsequently sent Truman a letter saying, "*I believe that the United States must be as completely armed as any possible enemy,"* and urged the President to *"direct the Atomic Energy Commission to proceed with all possible expedition to develop the thermonuclear weapon."*[14] Truman did not respond to Strauss, but did ask Lilienthal, Acheson, and Johnson to once again act as a subcommittee of the NSC, this time to advise him on "*whether and in what manner the United States should undertake the development and possible production of super atomic weapons … and whether and when any publicity should be given this matter.*"[15]

[13] *Sidney Souers Oral Interview*, Truman Library.

[14] Strauss, 219-222.

[15] Acheson**,** 346; Bundy, 212.

Figure 7: Truman and Sidney Souers.

At their first meeting, Lilienthal opposed the hydrogen bomb and Johnson, supported it.[16] Acheson, the swing vote, slightly favored building the hydrogen bomb because, he believed, regardless of what the United States might do, the Soviet Union would not delay their development of a super bomb. Equally compelling said Acheson, *"the American people simply would not tolerate a policy of delaying research in so vital a matter."*[17] The meeting adjourned without a consensus.

Anxious to bring the matter to a quick conclusion, Acheson prepared a set of four recommendations that he hoped both Lilienthal and Johnson would endorse.[18] The first recommendation called for the President to *"direct the Atomic Energy Commission to proceed to determine the technical feasibility of a thermonuclear weapon, the scale and rate of effort to be determined jointly by the Atomic Energy Commission and the Department of Defense.*" The second recommendation gave the President the option of deferring the final development of the hydrogen bomb pending a possible reexamination "*as to whether thermonuclear weapons should be produced beyond the number required for a test of feasibility."* The third recommendation directed "*the Secretary of State and the Secretary of Defense to undertake a reexamination of our objectives in peace and war and of the effect of these objectives on our strategic plans, in the light of our probable fission bomb capability and possible thermonuclear bomb capability of the Soviet Union."* The fourth and final recommendation called for "*the president [to] indicate publicly the intention of this Government to continue work to determine the feasibility of*

[16] Omar N. Bradley, A General's Life: An Autobiography by General of the Army Omar N. Bradley (New York: Simon & Schuster), 515.

[17] Acheson, 349.

[18] Ibid, 348.

a thermonuclear program, and that no further official information will be made public without the approval of the President."[19]

Acheson presented his recommendations to Lilienthal and Johnson on January 31, 1950.[20] Johnson objected to the wording of Acheson's second recommendation. He did not want any encumbrance placed on the production of weapons. After some debate, both Acheson and Lilienthal agreed to excise the paragraph. Once this was done, all three, including Lilienthal much to Acheson's' surprise, signed the recommendations. Lilienthal had decided not to directly oppose Acheson and Johnson, choosing instead to register his personal reservations about the hydrogen bomb directly with Truman.[21]

Undersecretary of Defense Stephen Early, who attended this meeting as an observer, suggested that the President announce his decision at a press conference.[22] Accordingly, a press release was prepared for the President saying that as Commander-in Chief, he had "*directed the Atomic Energy Commission to continue its work on all forms of atomic weapons, including the so-called hydrogen or super-bomb."* It concluded that this work was and would continue to follow American objectives until a satisfactory plan for international control of atomic energy is achieved.[23]

Secretary Johnson, who had a scheduled meeting that day with the President, suggested using his appointment to present the subcommittee's report to Truman. "*The heat was on,"* said Johnson, "*and every hour counted in getting this matter disposed of*." At 12:35 pm, Acheson handed the President the Committee's recommendations, which Truman started to read. Acheson also told Truman that Lilienthal wished to make a statement. Shortly after Lilienthal began expressing his misgivings, Truman cut him off and signed the recommendations. "*Further delay,*" said Truman *"would be unwise."* Later that day, Truman issued the prepared press release.[24]

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[19] David E. Lilienthal, The Journals of David E. Lilienthal, Volume II (New York: Harper & Row, 1964), 624.

[20] Ibid.

[21] Acheson, 349.

[22] Early had been Roosevelt's press secretary and, for a brief time in 1950, also served as Truman's press secretary.

[23] David Lilienthal, The Journals of David E. Lilienthal, Vol. 2, 626-633; Dean Acheson, Present at the Creation, 348-349; and Harry S. Truman, Public Papers 1950, #26; and Harry S. Truman, Years of Trial and Hope, 309.

[24] Even as Lilienthal, Johnson, and Acheson were meeting, Truman, according to General of the Army Omar Bradley, had already made up his mind to pursue the Super. Bradley, who met privately with the President on three occasions in January to discuss the hydrogen bomb, recalled in his memoirs: "*Truman was deeply troubled because AEC Chairman David Lilienthal was a humanitarian whom Truman greatly respected. But Truman had a way of seeing things clearly and going to the heart of the matter. If the Russians proceeded with the H-Bomb and we did not, and it worked, we would find ourselves in an intolerably inferior military posture. To Truman, it was as simple as that.*"

The Hydrogen Bomb – Los Alamos

In response to the President's directive, Bradbury returned the Laboratory to its wartime regime of six-day weeks, not to begin work on the hydrogen bomb, but to accelerate the work that had been ongoing since 1943. When Oppenheimer organized Los Alamos in 1943, he recruited Teller to head the Hydrodynamics of Implosion and Super Group (T-1). Although never intended to be the focus of the Laboratory's research program, work on the Super was included in the Laboratory's technical plan because, "*its potentialities were so great that research on it could not be neglected completely*."[25] And so it was immediately after the war. Work on the hydrogen bomb continued, but could not, in Bradbury's mind, be subordinated to the improvement of fission bombs and the nation's nuclear stockpile.

To meet the President's mandate for an accelerated program, Associate Director Darol Froman, proposed replacing the Laboratory's methodical computation and modeling techniques with a program of "*frequent real tests*" that could more rapidly confirm component designs.[26] This change in operating procedure required a continental test site to augment the Pacific Proving Ground (PPG). The PPG's great distance from the continental United States, its complicated weather patterns, the difficulty of protecting it during times of international crisis, and its limited amount of real estate made conducting *"frequent real tests"* there impossible. The AEC identified five possible continental sites: The North Carolina Coast, the Gulf Coast of Texas, the Dugway Proving Ground (Utah), the Wendover Bombing Range (Utah), the Alamogordo-White Sands Guided Missile range (Trinity Site), and the Las Vegas Bombing and Gunnery Range.[27] The AEC chose the Nevada site. Bradbury concurred, believing that tests as large as fifty kilotons could be safely detonated in the Nevada desert. The National Security Council approved the choice on December 15, 1950, followed by the President on December 18th.[28] Just a few weeks later, on January 27, 1951, Los Alamos carried out

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[25]Edith Truslow and Ralph Carlisle Smith, Project Y: The Los Alamos Story (Los Angeles: Tomash Publishers, 1983), p. 14-15; In the Matter of J. Robert Oppenheimer: Transcript of Hearing before the Personal Security Board (Washington: The Atomic Energy Commission), p. 325. Teller, who thought work on atomic bombs was pedestrian, argued that Los Alamos should focus on the hydrogen bomb. But, as Hans Bethe later said of Teller, *"He was on my staff. I relied – and I hoped to rely very heavily on him to help with our work in theoretical physics. It turned out that he did not want to cooperate. He did not want to work on the agreed line of research that everybody else in the laboratory had agreed to as the fruitful line. He always suggested new things, new deviations. He did not want to do the work [that] he and his group [was] supposed to do in the framework of the theoretical division. So that in the end there was no choice but to relieve him of any work in the general line of development of Los Alamos, and to permit him to pursue his own ideas entirely unrelated to the World War II work with his own group outside of the theoretical division. This was quite a blow to us because there were very few qualified men who could carry on that work*

[26] *LAB-J-W* 103.

[27] David Lilienthal to the Chairman of the Military Liaison Committee, September 20, 1948. LANL Archives; Terrance Fehner and F. G. Gosling, Origins of the Nevada Test Site (Washington, D.C.: United States Department of Energy, 2002.).

[28] Ibid.

the first of five tests in Nevada under the code name Operation Ranger. Not only did the comparative ease and speed of the Ranger Operation confirm the utility of having a continental test site, the tests also confirmed that using the Nevada site offered quicker resolution of many thermonuclear design issues.[29]

The PPG continued to be used for high yield tests. In 1951, four such tests were conducted at Eniwetok Atoll under the codename Operation Greenhouse. The first two Greenhouse shots, *Dog* and *Easy,* were significantly improved implosion devices. At 81 kilotons, Dog was the largest yield fission device to date. The third test, *George*, ignited the world's first thermonuclear fire.[30] As Los Alamos weapon engineer Jay Wechsler remarked, "*The George shot, the design of which resulted from the crash program on the H-Bomb, confirmed that our understanding of the means of initiating a small-scale thermonuclear reaction was adequate."*[31]The final Greenhouse test, *Item*, proved the principle of boosting, the technique "*of using a fission bomb to initiate a small thermonuclear reaction that increases the efficiency and use of the fissile material."*[32] The hydrogen bomb was not possible, commented Hans Bethe, until a fission device energetic enough to light a thermonuclear fire was developed. Boosted fission bombs could do so.

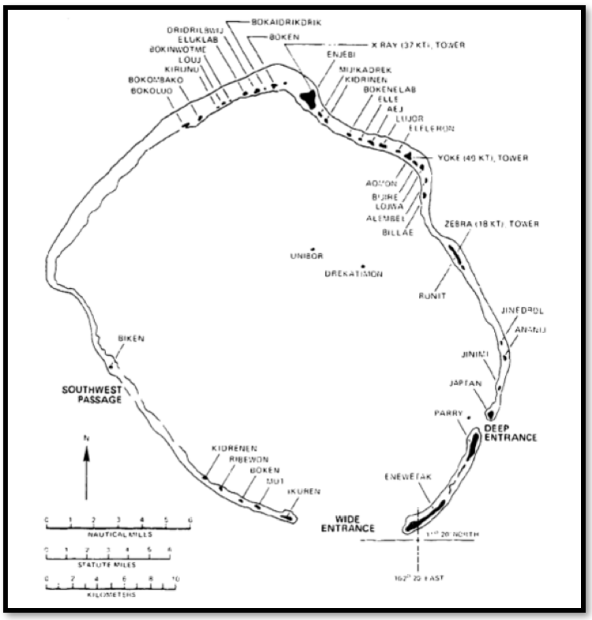

Figure 8: Enewetak Atoll.

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[29] By general agreement with the Military Liaison Committee, large yield tests would continue to be conducted in the Pacific.

[30] Atomic Energy Commission, Draft Report to the President on the Status of Thermonuclear Program, LANL Archives, February 26, 1951.

[31] *Los Alamos Science, Winter/Summer 1983*, 159-163.

[32] York, 23; and Carson Mark, "A Short Account**,**" 9.

Despite the success of both the *George* and *Item* tests, the problem of the Inverse Compton Effect remained. A thermonuclear fire could be lit, but could not be sustained. The answer to this problem came to Edward Teller in a fit of anger. While stewing over the needling of a colleague, it suddenly occurred to him that the radiation produced by atomic explosion had mass that could be channeled and used to compress deuterium thereby enhancing and sustaining a thermonuclear reaction. This concept, radiation implosion, was proved by the 1952 Mike test.

Immediately after Mike, the Department of Defense issued a national military requirement, codified in the Emergency Capability Program (ECP), for a deliverable thermonuclear weapon by 1954. Los Alamos designed and produced four ECP weapons, one of which entered the stockpile untested. The remaining three were proof tested during the 1954 Castle test series.

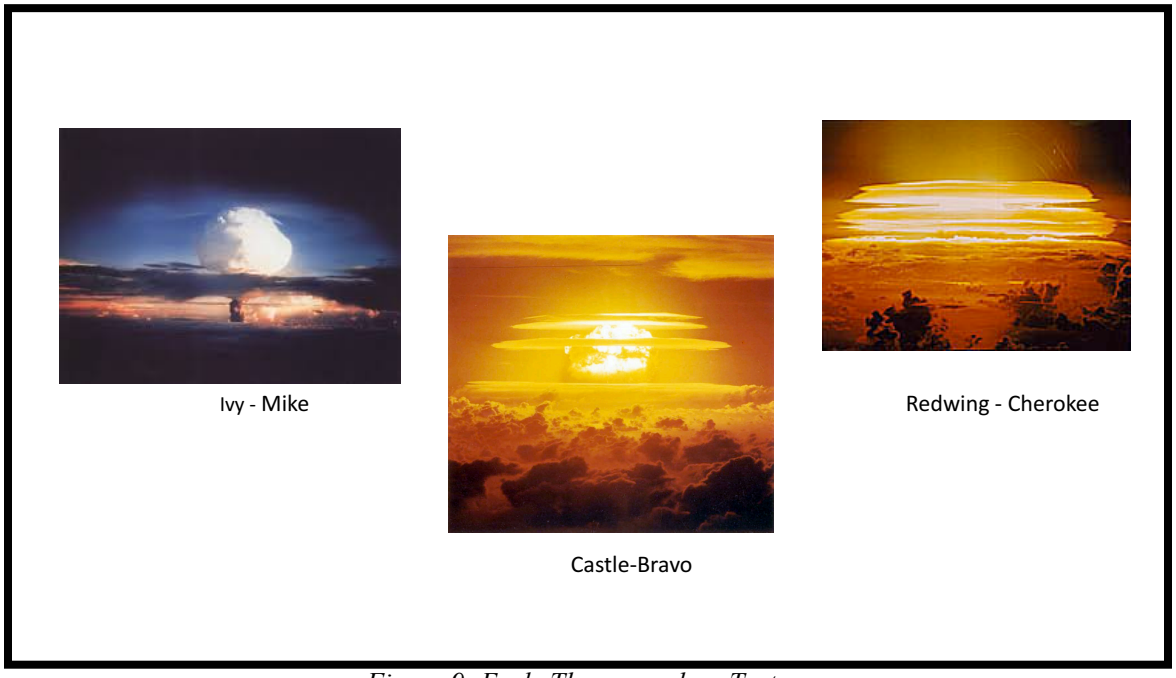

Figure 9: Early Thermonuclear Tests.

1954 – The Oppenheimer Hearing

In November 1953, a year after the Mike test, William Borden, a former staff member of the Joint Committee on Atomic Energy, sent a letter to FBI Director J. Edgar Hoover saying, in part, that "*more probably than not J. Robert Oppenheimer is an agent of the Soviet Union."* With respect to the hydrogen bomb, Borden also said of Oppenheimer that, "*(a) He was remarkably instrumental in influencing the military authorities and the Atomic Energy Commission essentially to suspend H-bomb development from mid-1946 through January 31, 1950; (b) He has worked tirelessly, from January 31, 1950, onward to retard the United States H-Bomb program; (c) He has used his potent influence against every postwar effort to expand capacity for producing A-bomb material; (d) He has used his potent influence against every postwar effort*

directed at obtaining larger supplies of uranium raw material; and (e) He has used his potent influence against every major postwar effort toward atomic power development, including the nuclear-powered submarine and aircraft programs as well as industrial power projects."[33] Borden's letter is generally accepted as the catalyst for the infamous 1954 security hearing.

Another catalyst was Teller, who is believed to have influenced Borden's thinking. One historian, Priscilla McMillan, has written that As early as March 1950, Teller sought out Borden *"to suggest that [the] slow going on the hydrogen bomb was Oppenheimer's fault for discouraging men from working on the problem."* Although Oppenheimer was the *cause celebre* of the 1954 hearings, Bradbury and Los Alamos were, by extension, clearly on trial as well.

Teller literally, and ironically, testified against Oppenheimer during the Castle tests of deliverable hydrogen bombs. Said Teller in response to two questions in particular:

> ***Question:*** *"To simplify the issues here, perhaps, let me ask you this question: Is it your intention in anything that you are about to testify to, to suggest that Dr. Oppenheimer is disloyal to the United States?"*
>
> ***Answer:*** *"I do not want to suggest anything of the kind. I know Dr. Oppenheimer as an intellectually most alert and very complicated person, and I think it would be presumptuous and wrong on my part if I would try in any way to analyze his motives. But I have always assumed, and I now assume that he is loyal to the United States. I believe this, and I shall believe it until I see very conclusive proof to the opposite."*
>
> ***Question:*** *"Now, a question which is the corollary of that. Do you, or do you not, believe that Dr. Oppenheimer is a security risk?"*
>
> ***Answer****: "In a great number of cases I have seen Dr. Oppenheimer act – I understood that Dr. Oppenheimer acted – in a way which for me was exceedingly hard to understand. I thoroughly disagreed with him in numerous issues and his actions frankly appeared to me confused and complicated. To this extent, I feel that I would like to see the vital interests of this country in hands, which I understand better, and therefore trust more. In this very limited sense I would like to express a feeling that I would feel personally more secure if public matters would rest in other hands."*

[33] In the Matter of J. Robert Oppenheimer, 837-838.

The Oppenheimer hearings placed Norris Bradbury in a difficult position. As the director of Los Alamos, he was responsible to the AEC, Oppenheimer's accuser. Also, as the director of Los Alamos, Bradbury believed strongly that the Laboratory had performed its work in the best interests of the nation, particularly with respect to the development of the hydrogen bomb. As might be expected, he bristled at the implication that both he and the Laboratory were mere pawns in a political game. He was relieved when the AEC confirmed that: "*Neither in the deliberations by the full Commission nor in the review of the Gray Board was importance attached to the opinions of Dr. Oppenheimer as they bore upon the 1949 debate within the government on the question of whether the United States should proceed with the thermonuclear program. In this debate Dr. Oppenheimer was, of course, entitled to his opinion.*"[34] Bradbury thanked Strauss for this declaration of support, saying to Strauss in an effusive letter, "*May I take this occasion to express my strong personal appreciation of the fine stand which the entire Commission took with respect to the exclusion of the hydrogen bomb question in the Oppenheimer case. Although I have not yet had the opportunity to discuss this with many members of my senior technical staff, it is my impression that this exclusion meets with unanimous agreement and that it will go far to allaying the deep concerns which this affair has unfortunately aroused. It is my own belief that this action by the Commission goes very far towards answering the specific point brought out in the statement which the Commission received from a rather large number of Los Alamos personnel. It is unlikely, however, that they will take the occasion to say so!"*[35]

1954 – Bradbury

If Bradbury thought the Oppenheimer Hearings had cleared the air about the hydrogen bomb, he was mistaken. Teller's criticisms were packaged and printed in *Life Magazine* and the book, *The Hydrogen Bomb.* Bradbury took vehement exception to the *Life* magazine article, which said, among many other things, that Los Alamos did not work hard enough or fast enough to develop Mike. As Bradbury detailed in an extensive set of notes, *"The statement regarding "dying of pure neglect" is false. The number of people involved in the over-all thermonuclear question has steadily increased at the Los Alamos Scientific Laboratory since 1946. The first form of one of the ideas recently tested at Eniwetok was suggested at the Los Alamos Scientific Laboratory (not by Teller) in 1946 although no techniques to exploit it were then known. Long prior to the public debate of 1951, the staff of the Los Alamos Scientific Laboratory had been exploring all avenues of maintaining or increasing this country's technical lead in both the fission and thermonuclear fields.*" Bradbury's comments are contained in Attachment A.

As Bradbury told United States Senator Clinton P. Anderson (D-NM), *"Much of the presently-appearing distortions of the technical history of the development of the thermonuclear weapon series by the Los Alamos Scientific Laboratory appear to center around the personality of Edward Teller. Unfortunately, Dr. Teller's career can be*

[34]TWX: From USAEC Washington, D.C. to USAEC Dr. Norris Bradbury, Director LASL, June 9, 1954.

[35] Bradbury to Strauss,

viewed in a light which is not entirely flattering, and your committee probably should be aware of this aspect." In particular, Bradbury told Anderson *"that Teller was asked by me to remain at Los Alamos and head its theoretical work. This he declined to do on the basis that I could not agree to testing 12 fission weapons a year, nor could I agree to abandoning the fission program for the thermonuclear. He also believed, apparently, that Oppenheimer did not support the Laboratory and, therefore, he did not wish to associate himself with it. In commenting on these points, it may be noted even at the Laboratory's present strength and with a continental testing site, it has only recently been possible or wise to test as many as 12 fission bombs a year, and that such a promise in 1946 would have been futile and untrue. Secondly, it may be noted that the country had but two extremely inefficient and bulky fission weapons in 1946, and that no way was known to use the output of the entire Oak Ridge plant except in gun-type weapons. To have diminished the emphasis of the fission field in 1946 would have placed this country enormously behind in numbers and flexibility of atomic weapons over the ensuing five years."*[36]

When *the Hydrogen Bomb* book was published, Bradbury went to great lengths to blunt the many issues raised by the book's authors, including veiled comments about the loyalty of Laboratory staff members. As Bradbury issued a press release saying, "*The imputation of disloyalty to that now large group of scientists and technicians who are fundamentally responsible for every nuclear weapon, fission and fusion, that the United States has in its stockpile, who are responsible for the atomic weapons leadership that this country presently enjoys, and who are dedicated to the continuance of this leadership, is a tragic, if not malevolent, thing. The motives behind these accusations of Los Alamos are unclear; their bases are faulty and irresponsible information necessarily obtained from those who do not and cannot know the classified facts; and their effect on the Laboratory would be wholly disheartening were it not for our knowledge that the facts warrant the full confidence of the Nation in our accomplishments over many years."* Bradbury's full comments are contained in Attachment B.

In addition to the press release, Bradbury held a press conference hoping to counter the many claims in the book that Los Alamos had not done its job properly. In the course of the question and answer session, Bradbury summed up, in a low-key and slightly disingenuous fashion, the *fission-fusion* relationship between himself and Teller. Said Bradbury, *"in all frankness Edward and I differed and, I presume not once but a number of times over the best way to administer the thermonuclear program … In general, Edward and I disagreed on the best way to make the most rapid progress and if there is irritation on that score it probably arose primarily form that source."* Despite his attempts to set the record straight, Bradbury's efforts had minimal effect, changing very few minds about the development of the hydrogen bomb. See Attachment C for the full transcript of this press conference.

[36] Bradbury to Anderson, September1, 1954.

Retrospective

Although the intensity of the hydrogen bomb debate lessened after 1954, it still survives today, all too often in the simplistic disguise of Teller versus Oppenheimer. Bradbury, who even more than Oppenheimer, tussled with Teller over the development of the hydrogen bomb, is largely unknown. And, it was unfortunate that much of the hydrogen bomb debate, including the Oppenheimer hearing, took place long after the hydrogen bomb, itself, became a reality. Once boosting and radiation implosion were discovered, the hydrogen bomb was developed with remarkable speed. As Bradbury told the Chairman of the Joint Committee on Atomic Energy in 1969, "*It would have been truly impossible to have stopped Los Alamos from studying, working on, and exploring the problem with all of the facilities available to us. One cannot tell scientists not to think!"*

Appendix A

LIFE
TIME MAGAZINE
September 6, 1954

FROM the testimony *In the Matter of J. Robert Oppenheimer* and the miles of comment on it in the press, it is well known that at the end of 1949 a great secret debate occurred in the government and among the atomic scientists about whether this country should try to build the hydrogen or "super" bomb; that Dr. Oppenheimer and the strong faction he led opposed and temporarily prevented it; and that another, smaller faction defeated them with the result that the H-bomb was achieved in 1952—barely nine months before the Russians fired a hydrogen device of their own.

Untrue. The progress on thermonuclear systems never stopped at Los Alamos. During the unfortunate public debate, Los Alamos was at work on the thermonuclear problem as well as on the fission problem. The debate did not "temporarily prevent" anything. Los Alamos was always vehement regarding the necessity of work in this field. Scientists could not have been prevented from thinking about the problem, and a negative presidential decision could only have prevented full scale nuclear testing. The achievement of the H Bomb was the result of hard work by Los Alamos, and not the result of the "defeat" of one "faction" by another.

To the best of our technical knowledge, the Russian hydrogen device had a far smaller yield than our own systems and employed a fairly primitive thermonuclear principle, first tested successfully, although with other materials, by Los Alamos in an experiment at Enivetok in 1951.

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Very well: it can be said at once that many people at Los Alamos and Washington deserve a substantial share of it. Teller's role nevertheless was unique, indispensable and decisive. Without it the chances are quite strong that the U.S. would not have the H-bomb in deliverable form today. In that event, in the well-informed judgment of President Eisenhower, "Soviet power would today be on the march in every quarter of the globe." Teller not only produced the brilliant idea which converted the H-bomb from a monstrous "gadget" to a versatile weapon. He also, by an almost fanatic determination, kept the idea of an H-bomb from dying of pure neglect.

"Substantial share" of the credit is poor reporting of accomplishments over many years in both the fission and thermonuclear fields which resulted in a Presidential Citation to the Laboratory this year - a fact unmentioned in the article although it was the first such citation ever awarded to any laboratory.

That the United States has H Bombs in deliverable form today is probably due far more to the persistent and super-human efforts of the Los Alamos Scientific Laboratory than it is to the dubious paternity of Teller. With one exception, every nuclear test with which Teller has been directly associated and responsible has been a technical failure, and no system which he has promoted

The key ingredient of the H-bomb is not, therefore, a certain unmentionable combination of ingredients. It is, instead, the even more complex compound comprising Edward Teller. He cannot be defined, but at least he can be described.

Most of his calculations are not quite accurate—not because he lacks respect for accuracy but because he lacks the patience to spend his own time rounding off the figures. A friend says,

Teller succeeds, not only by the high average level of his ideas, but by producing them in unparalleled volume, thereby making his mistakes rapidly and becoming more and more expert. But until he or someone else can demonstrate

independently of Los Alamos has so far been worth stockpiling.

The statement regarding "dying of pure neglect" is false. The number of people involved in the over-all thermonuclear question has steadily increased at the Los Alamos Scientific Laboratory since 1946. The first form of one of the ideas recently tested at Eniwetok was suggested at the Los Alamos Scientific Laboratory (not by Teller) in 1946 although no techniques to exploit it were then known. Long prior to the unfortunate public debate of 1951, the staff of the Los Alamos Scientific Laboratory had been exploring all avenues of maintaining or increasing this country's technical lead in both the fission and thermonuclear fields.

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The key ingredient of H-Bomb development is probably not Teller but hard, patient work by many people at Los Alamos. However, this is necessarily a matter of opinion.

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Hydrogen bombs and fission bombs are not made by "order of magnitude guesses" and by "calculations that are not quite accurate". Crash programs that depend upon this sort of theoretical background fail - as the three experiments of the Livermore Laboratory have failed under Teller's direction because the work was neither sufficiently detailed, patient, nor accurate.

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Teller's insistence in 1951 was on a "crash program" of his own definition. To "make mistakes rapidly" in the development of the MIKE shot could have set the thermonuclear program back ten years. Tests in this field require months of preparation by the Los Alamos Scientific Laboratory, the AEC, and the Armed Forces. Had the first

At the University of Chicago, where he taught for several years, his fellow physicists now measure enthusiasm in "Tellers," with Teller himself, of course, being the basic criterion comparable to the velocity of light. Degrees of enthusiasm are measured in millionths of "Tellers," called "micro-Tellers."

He had been assigned to Los Alamos' theoretical division, headed by his friend Dr. Hans Bethe, Cornell's great physicist. As Bethe has testified, ". . . I hoped to rely very heavily on him to help our work in theoretical physics. It turned out that he did not want to cooperate. He did not want to work on the agreed line of research. . . . He always suggested new things, new deviations. So that in the end there was no choice but to relieve him of any work in the general line of the development of Los Alamos, and to permit him to pursue his own ideas entirely unrelated to the World War II work. . . ."

Teller was transferred to the F Division where Enrico Fermi presided over "advanced development." There, with Dr. Emil Konopinski and a few others, he devoted himself during the rest of the war to the super—to "my baby," as he had begun to call it. And by the end of the war he and his group had succeeded in working out some of its most intricate problems. He believed—as he was to testify later—that a concerted effort on the part of the other senior scientists could dispose of the rest by 1947. He had been led to suppose that this would occur: that once the A-bomb had been tested successfully, the great human and technical facilities of Los Alamos would converge on the super.

test failed, the resulting congressional clamor would have been deafening.

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It probably did not occur to either the author or his editors that this statement was not necessarily intended as a compliment to Teller.

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The philosophy of Teller during the war has mystified many people. Had he been willing to turn his attention to fission bombs, they might have been developed months earlier and the war ended that much sooner. In this instance, at le[illegible] his fanatical obsession worked to the immediate detriment of this country's objectives.

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Instead, to his great dismay, after Hiroshima he found the laboratory disintegrating. One Los Alamos scientist remembers, "I was away for several months and got back after Hiroshima. There was a terrible sense of shock. I didn't recognize anybody. Everyone was wrapped up in petitions for world government, disarmament, internationalizing of the atom, and so forth." To this emotion, which Teller to some degree shared, there was, of course, added the normal reaction that affected scientists as well as GIs: the feeling that the war was over and it was time to get back home and pick up the threads of old lives. Teller appealed to Oppenheimer for help, but he was among the most eager to leave. At last, with great reluctance, Teller decided to join the exodus himself.

EARLY in 1946 he called a meeting of the leading members of the Los Alamos staff to summarize for them all that he and his associates had learned—a meeting known as "The Final Conference on the Super."

Remarks of this nature have characterized Teller (and TIME) since the war. In the February 25, 1946 issue of Time Magazine, Teller is quoted as saying, "The situation there," (Los Alamos) "was 'catastrophic'. Only a handful of scientists remained...." This statement was made after Teller had personally decided to leave. He was then actively recruiting and proselyting among younger Los Alamos scientists, attempting to persuade them to leave Los Alamos and come to the University of Chicago where he was then attached. He was successful in a number of instances. The quoted statement was, of course, completely untrue. The situation was not catastrophic although it was difficult in the immediate post-war period. Nevertheless a number of loyal scientists were working diligently to build a permanent, strong and vigorous laboratory. The Laboratory itself was preparing for the Bikini tests which were successfully carried out. The actions of Teller, his proselyting, and his statements were invidious, confusing and disheartening. They were not the actions or the statements of a man determined that this nation should have the best possible atomic weapon laboratory.

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There were a number of conferences on the theoretical and thermonuclear problems of the so-called super bomb during 1946. The first was called during April of that year - not by Teller but by the Los Alamos Scientific Laboratory. ~~Further conferences were held during the summer~~. None was ever called the "final" conference, and any such interpretation must have existed in Teller's mind.

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And in mid-1948 he wrote, "I believe that we should cease to be infatuated with the menace of this fabulous monster, Russia. . . . We must work for something. We must work for world government . . . [and] concentrate for the time being on establishing a common government with our friends and potential allies."

Nevertheless, the optimism he professed was qualified with private fears. Even during the war he had mistrusted the Russians. There were stories his parents told of oceans of men sweeping across the frontier during World War I, dying and endlessly replaced. The native prejudice faded when he grew up and traveled in the world, and, he recalls, he regarded Soviet Communism as "an experiment of interest and possibly of some merit." But the purge trials disillusioned him.

These are curious statements. Teller begins to sound like one of the long parade of "reformed" Communists. Nothing so remarkable (for its time) as the mid-1948 statement was even attributed to Oppenheimer.

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The result of this introspection led him to return to Los Alamos—"to do something I knew about"—at first for short periods as a consultant and finally on a leave of absence. This was 1949.

Teller probably did return to Los Alamos of his own choice, but he required the AEC to make a special plea to the University of Chicago, and at one time even asked that the President request that he do so. Teller's enthusiasm for working on atomic bombs has only recently been strong enough to carry out on his own. In 1946 when he thought Oppenheimer was dubious, he was unwilling to commit himself on his own beliefs. Someone else always had to take the lead.

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The AEC commissioner supported these views of their Advisory Committee: not once but twice they rejected the super.

The "super" was never "rejected". The general debate was over the meaningless and undefinable words "crash program".

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Teller tried them: all turned him down. Almost everywhere he encountered either indifference or active hostility.

It has been suggested that this was because of Oppenheimer's presumed views on the thermonuclear development. At least in some cases, it is known that the basis was a purely personal disinclination to work anywhere near Teller.

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There were technical discouragements too: some of the earlier calculations were repeated more thoroughly and put the whole project in doubt. Even a year later, when the first "thermonuclear device" was approaching the test stage and someone asked Teller, "Will it work?" he had to admit that he didn't know. "But you didn't know *that* five years ago," the questioner pointed out. "True," Teller answered, "but now we don't know on much better grounds."

Moreover, even if this "device" worked, there was no way in sight of developing it into a really practicable weapon. Ordinary hydrogen atoms, although they fuse in the sun, cannot be made to do so under any conditions attainable on earth. Teller's calculations involved the use of special "heavy" forms of hydrogen called deuterium and tritium, and these had to be kept liquefied by means of cumbersome refrigeration equipment. The result was less a bomb than a "contraption," as Oppenheimer has called it, which could be carried in a ship's hold and thus conceivably be used against enemy ports, but which was too big to be carried in any airplane built or planned.

During the latter part of 1950 Dr. Stan Ulam, a Los Alamos mathematical physicist, was working on a paper on certain theories indirectly related to this problem. Teller got into a conversation with Ulam about it. Not long afterward something they had discussed touched a spark.

Dr. Norris Bradbury, director of Los Alamos, and his division heads adopted a production schedule for "Mike," as the new device was code-named, which to them seemed efficient but to Teller seemed much too conservative.

There is an understandable technical confusion here. The practical performance of the thermonuclear device originally known as a "super-bomb" became increasingly dubious with the passage of time and more refined calculations made by better calculating machinery. The first thermomuclear experiment in 1951 had nothing to do with a "device" or a weapon. It was an attempt to see if a limited thermonuclear reaction could be produced under the only conditions which seemed at the time to be relevant to further pursuit of the fundamental problem. The thermonuclear reaction was itself completely trivial and, in accordance with expectations, produced less than 0.01% of the total energy released. Since it had no relation to a weapon, it would have been nonsensical to carry it in a ship's hold or anywhere else. In contrast, the MIKE device of 1952 which relied upon a different approach from that which seemed the only line of attack when the 1951 experiment was designed, might have been used in the fashion described. As is pointed out elsewhere, Teller had nothing to do with the actual design of the latter system.

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The field in which Ulam was working was directly related to the subsequent Teller suggestion. If Teller is the father of the H-Bomb, then Ulam should at least be given the credit for putting the gleam in his eye, if not for describing to him the actual facts of life.

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The design and construction schedule (not "production") never seemed to anyone "efficient". It was a mad rush from beginning to end. One finally modified component was actually shipped by air from Los Alamos less than a week before the scheduled shot date. Incidentally, while it is now ancient history, as the scheduled date approached there was great Washington consternation that it was so close to election day. Many individuals who, a year before had been urging "hurry" now urged "wait a while"! Due in part to the insistence of the LASL it was shot on schedule.

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"It is true that I am the father in the biological sense that I performed a necessary function and let nature take its course.

The word "nature" might better and more truthfully be replaced by "hard work at Los Alamos".

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Following the success of Mike, there was an effort, mainly at Los Alamos, to simplify its construction still more and to develop it into a family of weapons comparable to the A-bomb family. This has been entirely successful.

This statement is almost classic in its avoidance of clarity while still being strictly truthful. A much simpler statement would have been: "Following the success of its MIKE shot, Los Alamos successfully simplified its construction and developed a family of weapons comparable to the A-bomb family."

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Sitting in his room recently at Los Alamos, where he was spending a fortnight helping on still more and newer weapons problems, he discussed what he hoped could be his future.

This statement is not true. Teller had been sent to Los Alamos by Livermore on their behalf. He was not helping the Los Alamos Scientific Laboratory on "still more and newer weapon problems". Indeed, his primary preoccupation during his visit was with his relation to the Oppenheimer case and no one recalls hearing him discuss anything else. This is probably understandable.

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Appendix B

HELEN T. REDMAN
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LOS ALAMOS SCIENTIFIC LABORATORY
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LOS ALAMOS SCIENTIFIC LABORATORY
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Technical Information Office
Los Alamos 2-6811

September 24, 1954

NOTE TO EDITORS: The following statement was made by Norris E. Bradbury, Director of the University of California's Los Alamos Scientific Laboratory, at the beginning of a press conference held in his office at 9:15 a.m., MDT, on Friday, September 24, 1954.

In late 1945 a small group of courageous and loyal scientists and technicians undertook to continue the post-war operation of the Los Alamos Scientific Laboratory. These men believed that atomic weapons development had barely begun, that other countries would develop such weapons, and that the safety and security of the United States--if not of the world--depended upon the technical lead of this country. These men had the courage to stay at Los Alamos in the face of an uncertain future. The Atomic Energy Commission did not then exist. Job offers from universities and industry poured in upon them. Their home-towns seemed preferable to the strange surroundings of an isolated military post. The most senior scientists of the war days felt that their responsibilities required their return to their university posts. Younger men were leaving to return to school or other jobs. But some men stayed--and built a laboratory.

These men did not make demands nor require promises. These men stayed and built the greatest weapons laboratory this country has ever known. These men stayed and developed the greatest array of powerful and flexible atomic weapons of any country in the world .... developed them faster, developed them where they were urgently needed and requested by the Armed Forces .... developed them to fit the productive resources of the newly established Atomic Energy Commission. They stayed and built a laboratory that developed EVERY SUCCESSFUL THERMONUCLEAR WEAPON THAT EXISTS TODAY. Others left, but these men stayed and worked, and many others came to join them.

What these men accomplished cannot be told in detail, for these facts are classified TOP SECRET. These men do not talk. They believe in deeds, not words. But these deeds earned for the Los Alamos Scientific Laboratory the only Presidential Citation ever awarded to any laboratory for its extraordinary success in the development of both fission and fusion weapons, and its contribution to the collective security of the Nation and the free world.

What these men accomplished was this: They built a laboratory from 1200 employees in 1946 to 3,000 employees in 1954. They brought back many of the senior wartime staff members as consultants, frequently for months at a time. They worked and thought and had ideas. In the fission weapons field, they advanced development from the few primitive wartime weapons to weapons enormously more powerful; to weapons enormously cheaper; to weapons so enormously more efficient that only a small fraction of the bomb load, and a small fraction of the number of planes, and a small fraction of the cost in fissionable material were required. They multiplied the atomic capability of this country in so many ways that not even billions of dollars spent in active material production would have been equivalent.

Nor was the Laboratory idle in the thermonuclear field. The wartime efforts of a small group of men in the Laboratory were summarized in the 1946 conference. Later in that year, the basic idea for one of the present patterns of thermonuclear weapons arose, although no way to exploit it effectively could then be seen. An elaborate program of basic research, both theoretical and experimental, was undertaken in order to provide both the necessary fundamental data for the basic calculations as to whether the "super" bomb would work at all, even if it could be ignited.

THERMONUCLEAR WORK NEVER STOPPED. Basic nuclear data was obtained, TOP SECRET theoretical studies on thermonuclear processes were carried out, the great electronic brain, the Maniac, was being built with such calculations in mind, and simultaneously the necessary practical studies of materials and potential engineering problems were conducted. All this is in the official record of the Laboratory's work during the period from 1946 to 1951. Thermonuclear work grew as the Laboratory grew. By 1949 the design and understanding of fission bombs had proceeded far enough to permit studies of their application to thermonuclear systems to be undertaken. Even before the Russian Bomb was fired, the Laboratory was working on the detailed design of an experiment employing thermonuclear principles which would answer some (but far from all) of the basic questions regarding thermonuclear systems. Still later events suggested the addition to the Greenhouse program of even a more elaborate experimental approach. In March 1950 the Laboratory went, on its own volition, on a 6 day week for almost 3 years to speed its developments while it was further expanding its scientific staff.

Had the Laboratory attempted to exploit the thermonuclear field to the exclusion of the fission field in 1946, what would have happened? Hypothetical history can only be an educated guess, but the guess in this case is almost certain. The fission weapons stockpile would have been but a fraction of its present size. The essential fission techniques required for practical thermonuclear weapons would not have been developed. Discouragement would have nagged at those who worked in a field without the means

for practical accomplishment, and the program--and the Laboratory--might have died.

Rather than delaying the actual accomplishment of thermonuclear weapons, the Los Alamos Scientific Laboratory has, by its insistence on doing necessary things first, demonstrably provided the fertile soil in which the first feasible ideas could rapidly grow, and demonstrably did develop such weapons, and probably, but not demonstrably, did so years ahead of any other course which could have been pursued with the facilities and people available. Technically, the development of fusion weapons is so inextricably allied with and dependent on the development of fission weapons, that great success in the former had to follow success in the latter.

The assertion that the Los Alamos Scientific Laboratory was reluctant to work in the field of thermonuclear weapons is false. Although the thermonuclear program is referred to in every program of the Laboratory from 1945 on, some statements are of particular significance.

In a TOP SECRET letter to the Atomic Energy Commission dated December 9, 1949, three months after the Russian explosion, the Laboratory stated over the signature of N. E. Bradbury:

> "We propose to augment to the greatest extent possible the effort devoted to research on the problem of attaining a nuclear reaction involving the light elements. The goal of this effort will be an experimental test...."

The goal stated, of course is classified, but was attained even earlier than we then believed possible. In another section of the same letter the statement is made:

> "The importance of these questions (thermonuclear) makes, in our opinion, an understanding and test of the basic phenomena at the fastest practicable rate imperative. Then, and only then can the many issues be resolved without recourse to hypothesis or wishful thinking."

In another letter to the AEC dated November 17, 1950, over the signature of N. E. Bradbury, the Laboratory's position was stated unequivocally:

> "....the importance of arriving at firm conclusions regarding the application of these or other potential thermonuclear techniques to military use makes it imperative that vigorous work be continued in this field."

The letter, classified TOP SECRET, went on to give several pages of description.

At every stage from 1946 to the present time, the fission and fusion programs--both in basic research and in practical application--were pursued with the maximum appropriate emphasis, with care, with precision, and with success. What "might have been" is idle speculation. What would have happened to World War II if the Manhattan District had started work in 1939?

The imputation of disloyalty to that now large group of scientists and technicians who are fundamentally responsible for every nuclear weapon, fission and fusion, that the United States has in its stockpile, who are responsible for the atomic weapons leadership that this country presently enjoys, and who are dedicated to the continuance of this leadership, is a tragic, if not malevolent, thing. The motives behind these accusations of Los Alamos are unclear; their bases are faulty and irresponsible information necessarily obtained from those who do not and cannot know the classified facts; and their effect on the Laboratory would be wholly disheartening were it not for our knowledge that the facts warrant the full confidence of the Nation in our accomplishments over many years.

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Following is the text of an unclassified letter written to Norris E. Bradbury, Director, Los Alamos Scientific Laboratory, by Lewis L. Strauss, Chairman, Atomic Energy Commission:

"September 22, 1954

"Dear Norris:

"When we spoke on the telephone last week and discussed the forthcoming book by Shepley and Blair, which the authors had submitted to the Commission for security clearance, I told you that I had had no more influence to prevent its appearance than I have been able to exercise in respect to articles which have appeared attacking me untruthfully.

"There should be no doubt in your mind, however, that the Commission estimate of the Los Alamos Laboratory, long under your able direction, is properly reflected in the recent and unique Presidential Citation to the Laboratory which I recommended to the President and which he saw fit to award.

"Sincerely,

(Signed)

"Lewis L. Strauss"

APPENDIX C

LOS ALAMOS SCIENTIFIC LABORATORY
of the
University of California

LASL 54-39
Technical Information Office
Los Alamos 2-6811

September 24, 1954

PRESS CONFERENCE
Held in Office of the Director
Los Alamos Scientific Laboratory
September 24, 1954

PRESENT: LOS ALAMOS SCIENTIFIC LABORATORY:

Norris E. Bradbury, Director
Ralph Carlisle Smith, Assistant Director for Classification and Security
Frank Waters, Information Adviser

AEC REPRESENTATIVE:

Richard G. Elliott, Information Director, Santa Fe Operations Office, Albuquerque

NEWS MEDIA REPRESENTATIVES:

Robert McKinney	The New Mexican	Santa Fe
Joseph Lawlor	The New Mexican	Santa Fe
Al Glanzberg	The New Mexican	Los Alamos
Robert A. Brown	Albuquerque Journal	Albuquerque
Bernice Burkhardt	New York Times Denver Post	Los Alamos
Gordon Beach	United Press	Santa Fe
John Curtis	Associated Press	Santa Fe
Robert Y. Porton	KRSN, Los Alamos KOAT, Albuquerque	Los Alamos
William Spack	KRSN	Los Alamos
Jack Hardwick	KRSN	Los Alamos
E. J. Vanderwood	KVSF	Santa Fe

HAND DISTRIBUTION: Photos of meeting

Prepared Statement (copies mailed separately to all news media representatives)

DR. NORRIS E. BRADBURY:

Good morning. There is a great deal of peraphernalia scattered about the room. This is not because the session is at all formal--in fact, it is my hope it will be quite informal, and that we can discuss things within the limits of classification and security as much as you have time for. However we do want to keep a record of what was said, and what you said in some cases, just because these things are good to maintain and someone else might be interested in them sometime.

This business of having a press conference is something quite unusual for Los Alamos Scientific Laboratory. As a matter of fact, this morning is the second occasion on which I have ever been involved in any press conference here. The first one was back in late 1945 or early 1946, shortly after I took over this job and at that time I didn't actually call it. I think it was called by the local commanding officer. That was eight years ago. This morning we're having the second. You will therefore find me somewhat unskilled in the arts of press conferences, but we can make up for that I think by being informal.

The reason for the conference this morning is, as I think all of you are aware, quite specific. Ordinarily the Laboratory does not talk about its program, does not talk about its accomplishments, does not try to take credit for these accomplishments. This is in accordance with the requests of the Atomic Energy Commission, and with the policy and with the philosophy of the Laboratory. However, there has recently appeared a rather extraordinary document by a pair of relatively unknown individuals, in which such extraordinary and fantastic comments, imputations, speculations about Los Alamos and its activities, its accomplishments, and its people are made, that I felt that we could not rely upon our traditional "no comment" and that it was important for you, as representatives of the press, and for the Laboratory that the facts in this particular matter be set forth and set forth correctly insofar as they can be within the requirements of the Atomic Energy Commission personnel and news personnel.

Generally speaking, all newsmen would like to write an interesting and newsworthy story. On the other hand, the loyal citizen with access to classified information is in an impossible position. He cannot talk about those things which might be exciting but which are classified. The only people therefore who can talk, basically, are people who do not know the facts, who imagine the facts, who speculate and surmise, who depend upon leaks, rumors and information that may or may not be malevolently inspired. To rebut this is extraordinarily difficult in many cases. It is very much as if I picked out one of you and said, "You are a Communist," and you said "I am not," and I said "Prove it," and you'd find it extraordinarily difficult to do. In exactly the same sense the accomplishments of the laboratory are Top Secret, and when they are impugned there is no way to drag out the record and say, "Here are the facts." In talking to you this morning, and in the statement we have handed out, there are some aspects of the Blair-Shepley document--they are not classified--that are equally malicious and equally false. Some of these, a few of them, I propose to deal with on a point by point basis, so that you can get a flavor of the general

accuracy of the document in other areas where I will be forced to say "I'm sorry I can't talk" because the information is classified.

Now a few words about the prepared statement which already is in your hands or will be given to you shortly. It is, I am afraid it will seem to you, a somewhat emotional statement. I wrote it and I think you must understand in part why it may be somewhat emotional. Los Alamos has been my life for ten years. When this community, when this Laboratory, when the people here in are attacked in this extraordinary and unwarranted fashion, without basis and without foundation, I think you can understand that I react to this, to these accusations, in something more than a calm fashion. In any event if the statement is emotional, I apologize for it, and we can now proceed as calmly as you wish in the course of this meeting.

Turning to the Shepley-Blair document itself, let me make a few general comments. I assume most of you have had access, in fact if you hadn't you wouldn't be here, to the particular magazine in which the book was excerpted. Actually the book itself has only last night gotten into my hands, and I have had no more than an opportunity to thumb through it. As far as I can tell, essentially all of the sensational and may I say mis-statements appear in the magazine, and the text of the book itself contains essentially, again as far as I can tell in a hasty reading, only miscellaneous material which has been drawn from essentially published sources. The editors of the magazine have, I suppose, naturally drawn the more sensational, more controversial statements and passages and chapters from the book and printed them. So I think if we confine our discussions this morning to the magazine--I have copies and I assume you have--we can go through it in as much detail as you wish.

One other point that I would like to make about my comments, and your questions, and my replies: I would like to confine myself to matters which have to do with either me, people at Los Alamos, or Los Alamos affairs. There are a great many situations, incidents and discussions in the book, and in the magazine, which have to do with events and people with which I was not directly connected. I would like to have my remarks differ from those of the authors. I propose to talk strictly about those things which I know directly, personally and factually. I would prefer not to comment on matters which get beyond my immediate interest and knowledge. With this introduction, let me turn to the article itself and draw your attention if I may to some typical, if rather trivial points.

The first one that I would like to discuss because it is trivial, and happens to be totally unclassified, adds a sort of a flavor to the general document. This appears in a footnote on the bottom of Page 103. The text, incidentally, as you are well aware, is liberally sprinkled with footnotes, I presume to give an impression of detailed technical accuracy. It's unfortunate that such a large fraction of these footnotes seem to be totally false. This particular footnote starts out by saying that "Los Alamos personnel who supervised the preparation of the movie were careful to see that no scenes included Edward Teller," and so on.

Let's look at that statement in detail because it's a trivial remark, and it happens to be false in almost every word. In the first place, there are indeed movies made under the auspices of the Joint Task Force, documentary films, for each of the operations. These are ordinarily classified, highly classified, but in this particular case, the case of the Mike operation, a releasable version was prepared from the original film. Los Alamos personnel did not supervise either the preparation of the original Top Secret document, nor the preparation of the released version. The former, script and all, was prepared by an Air Force subsidiary of the Joint Task Force, Lookout Mountain Laboratory. The script was read here for let's say technical, I won't quite say technical accuracy, but to see that no serious technical misstatements were made, and that was the limit of our contact with it. We had no control over what was in the film other than from the point of view of technical accuracy.

The part of the film referred to was taken in this room. The table which is in front of you was actually facing the other way, and I was standing over there. Perhaps as some of you are aware, in the outer office there are pictures of a number of people prominent in the atomic energy work here during the war. Among those pictures are pictures of Edward Teller and Mr. Oppenheimer. They were there then, they are there now. One of the Air Force technicians, I presumed, as I had no knowledge of this, took the picture from the wall there and put it up here to lend an attractive background. They didn't like the atomic bomb pictures in the background and thought Mr. Teller would look better, I presume. I was not in the room during the course of the morning, and had no idea of what was being set up on the stage. It looked worse, incidentally, with lights and cameras, than it looks now. Mr. Teller himself at the time of the taking of that picture had left Los Alamos several months before. He wasn't here. He had nothing to do with the Mike shot. Why should he have been in the picture? He was a thousand miles away, or farther, and the picture, the scenes, were taken here where the Mike shot had been designed and prepared and by whom it was fired. There was no deliberate omission. It simply was not part of the picture.

In a previous story, in a previous picture on Operation Greenhouse, when similar documentaries were made--this one was classified and never released--Edward Teller was here at that time and extensive scenes, showing Mr. Teller describing let's say basic principles, were included. This particular scene (referred to in the paragraph above) survived final editing though the released version was dealt a few excises. A typical example of snide reporting and writing over a situation which had no basis, no real basis in fact or fancy.

Let me invite your attention to another, and perhaps rather more unpleasant piece of writing, again somewhat characteristic of the technique of this article. This one appears on Page 96. Again a footnote. We can deal with the text later on if you wish. In the footnote, roughly in the left-hand column in the middle of the page, there appears a statement: "In Washington, Congressmen who asked the question of the AEC, who on the Los Alamos staff is working full time on the Super, were never able to get an answer" and so on. Then they

go on to say that "The roster of theoreticians at the weapons laboratory actually declined during 1950." The latter statement I would like to deal with first, because it is purely and simply false.

Let me give you the numbers of theoreticians at the Laboratory as a function of time. Theoreticians are hard to describe. There are theoreticians who, let us say, are capable of originating ideas, originating problems, carrying out broad areas of scientific theoretical investigation. Naturally to assist them a supporting staff of mathematicians and computors is required. I'll talk about both. I'll talk first about theoreticians in the broadest and purest sense. The idea men--the people who have the ideas, who work them out, provide the directions in which the laboratory is going. In 1946, our low point after the war, when it was being suggested that the Los Alamos Scientific Laboratory fire 12 bombs a year, we had 8 theoreticians. In 1947, 12. In 1948, 14; in 1949, 22; in 1950, 35; in 1951, 45, and so on up to something of the order of 60 now. The number never declined. These people in turn were supported by between two and three times as many technical staff, computors and mathematicians. In 1946 the entire theoretical division comprised 23 people. A fine time to start a test program of 12 bombs a year! Indeed we were unable to reach a sensible firing number approaching that until roughly 1951 and from then on. At the present time the total staff of the theoretical division numbers around 160 people. The particular statement then, is deliberately and specifically false.

Turning to the first part of the footnote, -- Mr. Wheeler as an example. Indeed Mr. Wheeler came to Los Alamos on a leave of absence for a year. He then thought, and we concurred, that he could do more for us if he returned to Princeton, took a contract with us, and obtained the part-time services of many graduate students in Physics who were available to him there, and were not available here. So Prof. Wheeler returned to Princeton, took a contract with us, which we paid for, which we supported, which we directed, and his results were reported back to us, and by this technique added a considerable number to the staff of people working on the thermonuclear program. Again the statement in a literal sense is true, but the implication is false. Nordheim, again, was here for a year but returned time and time again, summers for consulting, and so on--the continuing contact--in contact now, in contact then. Von Neumann, who it said was in and out, has been a consultant of this Laboratory since 1944, not just at that time but regularly. He is a consultant now, he was a consultant then. He spends a great deal of time here. He has a great many other responsibilities in the national scene.

Let me turn to another curious piece of illogic. On Page 97, at the bottom of the page, we read the statement, "The staff of the Laboratory was busily engaged in preparations for the forthcoming Nevada test, and could find some justification for its unwillingness to get involved in Teller's work."

ROBERT MCKINNEY: Excuse me, Dr. Bradbury, where is that?

DR. BRADBURY:

On Page 97, left-hand column, near the bottom of the page, the next to last paragraph. The key words are "unwillingness to get involved in Teller's work." But 10 lines farther on, on the top of the next column, Dr. Alvarez is saying it would cause so much disruption if we stopped the thermonuclear work at Los Alamos because people there are so heavily involved in it. The authors failed to see the extraordinary contrast between what they were saying just 10 lines apart.

One of the more unpleasant statements appears on Page 94. Near the bottom of the page, left-hand column. This is a very extraordinary statement about Los Alamos being "soft" on Communism. Los Alamos "loaded with Communists and former Communists." "Loaded" is the word. Let's examine the facts, because these facts are known to you. They are unclassified facts. Everyone knows of Fuchs. How did Fuchs come to Los Alamos? He was sent by the British, approved by the United States Government, approved by the Army. Approved by no one at Los Alamos, nor disapproved--it was not our business. Greenglass: sent to Los Alamos by the United States Army, neither approved nor disapproved by anyone at Los Alamos. The two Communists, the two Communists at that time. Now the former Communists. You know these people, too. I trust you will do them no disservice, since I mention their names, but I want you to know the facts. David Hawkins: Instructor at the University of California, worked at Los Alamos in the Personnel Office, not a Communist at the time he was here. To everyone's knowledge who knew him, a honest, hard-working, loyal citizen, against whom no one has ever made a traitorous accusation.

Mr. "A." I would like to call him Mr. "A." He is referred to in a long incident in the next column. I would like to come back to this incident later. I would like to call him Mr. "A" for the moment, because his name as far as I know has not been bandied about in the public press although doubtless it may be known to anyone who seeks it. Mr. "A," I'll call him, had been a Communist, had become fed up with the lies and pretense of the association, abandoned all connection, and again had been a loyal, diligent, and hard-working citizen at Los Alamos, for four years. Against him no one had then made, or has yet made, any accusation of disloyalty.

One final individual, Frank Oppenheimer, again a short time at Los Alamos, left shortly after the war, again a former Communist, again one against whom no one has made accusations of disloyalty. Now what does the word "loaded" mean to you? Or to me? I will agree that one spy is one too many, and that two spies are two too many. But the word "loaded," in a publication of this sort, means one thing to the reader--but here are the facts.

I would like to go a little bit more into the case of Mr. "A," as I called him, which appears in the next column on Page 94 in some detail with a lot of artistic verisimilitude. Mr. "A," as I have said, was a technical editor on this project, diligent, hard-working. He came in 1944. Sometime in 1948--I don't remember the precise dates--we heard from the Commission that it was concerned about his clearance. At that point we immediately took him off the new classified work that he was doing, and confined his access to classified material that he had already been or had to do with. Later, it developed that he had

already seen or had to do with. Later, it developed that he had omitted on his Personnel Security Questionnaire the fact that in the early 40's he had been a member of the Communist party. At this point the Commission demanded that his clearance be removed, and that he be fired from the Hill.

The Laboratory did protest this decision, and a number of us went to Washington on this basis: Here is a man who had, as far as we can tell, expiated his Communist attachment. He has worked hard and loyally, he has had access to classified information. Is it the right thing for the country to fire this man on this basis? If it is the right thing for us to fire this man, let us do it then in such a way that he can get another job, and let us put him on unclassified work until he can find another job somewhere else. Because this man had worked here, worked hard and faithfully: this was the point of our visit to Washington. The Commission heard us, patiently and sincerely. However, they decided against us that the man must leave immediately. We swallowed hard, we came back to Los Alamos and went to work. We had a job to do. There were no scars here. No scars whatsoever. The technical task before Los Alamos has always been paramount.

If I can take a recent case, the Oppenheimer case: As all of you are aware, a great many people in this Laboratory felt very strongly about this matter. But there is something we feel much more strongly about, and that is the job this Laboratory has to do. The task that this Laboratory has for the safety of this country, means more to us than any personal feelings we may have about individuals. Perhaps that's about enough of detail points to pick out from this. I recognize it's always, let's say a potential criticism, to pinpoint detail and say this particular point is wrong and that particular point is wrong. I have picked out just a few of the obvious falsehoods in this article merely because they give you a flavor of the other falsehoods particularly regarding a technical program, particularly regarding the philosophy, feelings, and enthusiasms of people here. These are characteristic. They are repeated time and time again in other issues. To go through all of them would take us forever.

May I correct one statement I made. Mr. "A" came to Los Alamos in 1943, so his services were five years and not four years. I would like to be accurate.

Well, this is perhaps enough of my own comments here. I would now like to put myself at your disposal. This is an informal session and if I can't answer your questions because of classification I will say so, and if I can't answer them because I don't know, I will say so. And, in all frankness, you should feel no hesitancy in asking any question regarding the matter in this text that concerns Los Alamos that comes to your mind.

QUESTION AND ANSWER SESSION

ROBERT MCKINNEY: Dr. Bradbury, to keep the continuity, can we continue on the subject of Mr. "A." Were you present at this meeting in Washington?

DR. BRADBURY: Yes, I was.

ROBERT MCKINNEY: Did Admiral Strauss make this speech which is attributed to him?

DR. BRADBURY: He did not.

ROBERT MCKINNEY: Did anybody make that speech?

DR. BRADBURY: No one made such a speech.

ROBERT MCKINNEY: No one discussed any bank cashier or anything of that nature?

DR. BRADBURY: Not at that time. Let me say, not in our presence.

ROBERT MCKINNEY: Is it possible that this could have been made by somebody else at some other time? And that these reporters heard of it?

DR. BRADBURY: I haven't the faintest idea where these reporters picked up this story. I've heard this story, perhaps Smitty can correct me, I think attributed to Secretary of Defense Wilson at one time, in connection with the Oppenheimer case. I think I have also heard it attributed to Chairman Strauss, in connection with the Oppenheimer case.

QUESTIONER: In the Oppenheimer transcript isn't there a very similar comparison? One of the witnesses who.....

DR. BRADBURY: It was a story current about the time of the Oppenheimer investigation.

ROBERT MCKINNEY: This article, Dr. Bradbury, says the Los Alamos staff members added they would relieve the Atomic Commissioners of the responsibility in that matter, so the Los Alamos staff members were present including you. And that the Commissioners were happy to buck the burden of reply to Lewis Strauss.

DR. BRADBURY: The statement of responsibility involved is false. That particular statement is deliberately and specifically false.

ROBERT MCKINNEY: The entire speech was never made in your presence?

DR. BRADBURY: It was not made. Again it lends a certain artistry to the statement, but it doesn't happen to be true.

WILLIAM SPACK: Dr. Bradbury, can you comment on the main accusation that Los Alamos dragged its feet on the thermonuclear program?

DR. BRADBURY: Frank, is my statement in these people's hands? I think that particular problem is dealt with there at some length. I would really appreciate it if at this moment we perhaps took a slight recess and you read the statement. Perhaps this would be useful to us....

FRANK WATERS: Yes. It's on page 2, the middle of page 2. (Dr. Bradbury's prepared statement, previously handed to all present).

DR. BRADBURY: It might be useful to us to just take a look at the statement and see what it says. It may suggest some further queries to you. Let's take a five minute break then.

ROBERT MCKINNEY: Could we take a longer break, Dr. Bradbury? It's now 9:30 and some of us may wish to phone the early remarks and statements to our papers.

DR. BRADBURY: Perfectly OK with me. Use the time as you wish, any way you wish.

FIFTEEN MINUTE INTERMISSION

DR. BRADBURY: One of my staff has just called me and suggested that there is one addition I should make to this release, in the interest of precision. On the first page of the statement a sentence in capitals begins, "Every successful weapon that appears today." To be precise we should say "in the free world," and I presume he is right. I don't know exactly what is happening in Russia.

Now I would like to start the discussion off by a few more remarks of my own, having to do with the general problem of how atomic weapons get done anyway, how research is conducted, and how the broad area of basic research every so often peaks up into specific weapon development. In reading this text you may have the impression that the atomic bomb is made by one man with a bright idea and a faithful assistant tagging along behind. This is far from the case. The ideas which go into a new weapon development come out of discussions between numbers of people: theoreticians, mathematicians, physicists, chemists and so on. It's a team job. Somebody has a bright idea, but it doesn't seem to be so bright as he talks to somebody else. He adds a little bit to it and takes something away from it, and then the idea grows and somebody else hears about it, and in talking these ideas over in seminars and studies and in documents gradually it builds up a concept of a way to go.

Now the particular case of what, let us say, has been called the new idea, widely attributed and sometimes correctly to Mr. Teller, that appeared in 1951. Actually, what are the facts? The basic problem that faced us in both the fission field and thermonuclear field had been the subject of thought by people ever since the first fission bomb was successfully detonated. The underlying idea, the underlying problem, -- I can't discuss what it is for it's classified, -- the underlying attack dates back to 1945. Ulam was working directly on the problem of how to make a

thermonuclear reaction, and he had an idea. It turned out that his idea was discussed; that idea bifurcated or trifurcated and had various directions in which it might go. And indeed the direction in which it might go that was suggested by Dr. Teller turned out to be the most profitable direction to explore. But this was characteristic of the development of ideas. It doesn't spring out by somebody sitting at a desk with his feet on it; it springs out from lots of people talking and thinking and working.

The development of weapons is the same way. The whole thermonuclear field as it has been developed at Los Alamos is that of people working in the theoretical division, people working in physics, people working in mathematics, people working in chemistry and metallurgy, people working in high explosives, across the board, working toward a common team objective. That's why it is so hard to answer your question as to how many people work in the thermonuclear program. In a sense, everyone. How many people work in a fission program? In a sense, everyone. The fields are tied together. No progress in atomic weapons is made by one man alone. In this day and age and during the war, the only way these great team jobs get done is by groups of people working together, cooperating, directing their entire efforts toward a goal. It is not the result of a single man or a few men having a bright idea, and carrying it out and suddenly out in Eniwetok or Nevada appears an atomic bomb. It can't be done that way. It's a team effort and the team is Los Alamos Scientific Laboratory. These are the facts of how atomic weapons are developed. This matter is dealt with to some extent in my testimony in the Oppenheimer case and I don't think I need to belabor it more.

Now I am open to whatever questions you wish to ask.

GORDON BEACH: This article quotes a strong...

DR. BRADBURY: If you will refer to the page it will help me.

GORDON BEACH: Throughout the article there is a feeling that the scientists were ashamed of what they had done, and didn't want to go any farther. Is there any basis to this?

DR. BRADBURY: I cannot speak for people not at Los Alamos, but at Los Alamos I think the philosophy is correctly described in my statement. We believed very strongly and very firmly that the atomic weapons would not stop with the first use of these weapons in World War II, that atomic weapons would be developed elsewhere in the world, and that this country would have to maintain its lead. Of course the basic objective of atomic weapons development is not to kill people, but to keep this country in so strong a position that a war will never happen. Indeed the atomic weapon business is curious in that its fundamental objective is to put itself out of business.

ROBERT MCKINNEY: Dr. Bradbury, the preface of this magazine article says, "The authors who had access to official sources spent months of research on the problem." Can you outline what official sources were available to them?

DR. BRADBURY: I can speak only for official sources available at Los Alamos. The authors had no official sources whatsoever at Los Alamos. They did not see me so I do not know, but it is my understanding that they spent a few hours here one evening, a number of months ago.

ROBERT MCKINNEY: (reading) "Many of the officials concerned have read advance copies of the book and furnished corroborative data to the authors."

DR. BRADBURY: No copy of this was seen in Los Alamos until the magazine was published.

ROBERT MCKINNEY: Mr. Salisbury's telegram to the New Mexican would seem to answer it for Washington.

WILLIAM SPACK: What do you think the justification was, Dr. Bradbury, for Dr. Teller's feeling that there was hostility here for the thermonuclear development program?

DR. BRADBURY: I wish you wouldn't use the word justification. That's a hard question to answer, therefore.

WILLIAM SPACK: Can we change it then? Your prepared statement here shows that the thermonuclear program was a continuous thing, that development and research was done through the war and thereafter. Did anything take place that, to your mind, could have accounted for his feeling that Los Alamos was dragging its feet?

DR. BRADBURY: Not that I know of. I think however that it appears in my testimony in the Oppenheimer case that in all frankness Edward and I differed and, I presume, not once but a number of times over the best way to administer the thermonuclear program. Maybe I can use an analogy here. As I said earlier, any weapon development, including the thermonuclear program, is a team effort. The situation in the thermonuclear field was as if you were in charge of a platoon, on a dark night, and didn't know where the enemy was. If somebody ordered you to go out and attain this objective, one way would be to send out the scouts and patrols and find out where the enemy is and how to contact him. Another way would be to say, "Full speed ahead, boys, let's charge." The latter method in the atomic weapon business is very likely to let you fall on your face. In general, Edward and I disagreed on the best way to make the most rapid progress and if there is irritation on that score it probably rose primarily from that source. Now may I say here again, purely for the record, that my personal relations with Mr. Teller are friendly and continue so. I was in telephone conversation with him even yesterday.