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Brig. Gen. K. E. Fields, US AEC DMA

May 3, 1954

N. E. Bradbury

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THERMONUCLEAR HISTORY

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FOR R. C. SMITH'S APPROVAL BEFORE DISPATCHING

In the April 12 issue of TIME magazine there appears an extended discussion of the "hydrogen bomb" in which it is apparent that the editors of this magazine have a rather extraordinary misconception regarding the roles of AEC laboratories presumably involved in this development. This, of course, is not the first time that the Luce publications have erred along these lines (e.g. the May 1953 issue of FORTUNE magazine), and the history of their misinformation in this field goes back to 1946. It has never been obvious whether corrective measures were either appropriate or possible.

The present instance is, of course, so fantastically in error, particularly with respect to the relative roles and accomplishments of the Livermore and Los Alamos laboratories, that a non-trivial morale problem is arising at both laboratories. The persistence of the uncorrected Livermore legend in the press is now raising the question at Los Alamos as to whether or not the course of fusion weapon development as it has occurred over the last ten years and is occurring at present is really understood in all appropriate Washington areas, particularly the Congress. On the other hand Livermore personnel can only be embarrassed, as honest scientists, by the attribution of accomplishments to them which are not theirs, and also are concerned by the easily detectable exasperation of many of their Los Alamos colleagues with whom there has been up to the present as amiable relations as one could expect between two institutions whose activities were expected to be competitive in the weapon field.

The purpose of this memorandum, therefore, is to set forth as specifically as possible the situation in the thermonuclear field over the last few years as it has actually occurred at Los Alamos. All of this is no doubt well known to you and to the Commission, but it may be useful to summarize the situation as it has developed.

As you are aware, the thermonuclear field is a very old one, dating back to the early nineteen thirties. The development of the atomic weapon seemed to provide the first practicable means of attaining the necessary temperatures, and during the war years, Edward Teller while a member of the Los Alamos Scientific Laboratory devoted a considerable portion of his time to the direction of a small group of theoretical physicists in the exploration of this problem. At that time the thermonuclear weapon was only known in the abstract form of the classic "super" or run-away deuterium device and there was a fundamental question for which calculational techniques did not then exist, as to whether such a device if ignited would actually burn with a

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useful efficiency. (This question is now almost universally believed to be answered in the negative.)

At any rate, strong interest in this problem continued at Los Alamos after the war as the records of the various "super" conferences will indicate. However, it was imperative to devote our major efforts to the atomic weapon, both from the practical point of view of the national stockpile (particularly the effective use of U<sup>235</sup>) but also because it was apparent that any advance in the thermonuclear field could only follow a high degree of technical skill with fission systems. The accomplishments of the Los Alamos Scientific Laboratory in this field during the period 1946-1949 are well known to you.

Teller left Los Alamos in 1946 to return to academic work but remained as a consultant to the Laboratory and spent most of his summers there. He returned to Los Alamos on a full time basis in July 1949 on a year's leave of absence from the University of Chicago. With the occurrence of the first Russian atomic explosion we, of course, urged Teller to extend his stay at Los Alamos not only because interest in the potentialities of the thermonuclear field was at a high point, but the technical skills which had been acquired at Los Alamos in the fission field made the prospect of real progress seem more likely. An extension of his leave of absence was ultimately obtained with the assistance of the Commission.

The first significant and large scale accomplishment in this field occurred in 1951 in the GEORGE shot in Operation Greenhouse. As you will recall, this was the first instance in which a thermonuclear reaction had been made to take place external to a fission system. It was designed as a first step toward an initiator for a run-away super, and Teller was instrumental in the design and theoretical interpretation of this system. Shortly after its design it was realized that the technique could be extended to the principle we now know as radiation implosion. In this connection, full credit should also be given to Teller for his fundamental idea of using the radiation from a fission weapon as an effective means of compression of a thermonuclear system. The possibility of using one bomb to affect another fission or fusion system was, of course, a common concern to many people, and indeed, a closely related idea was first proposed at Los Alamos by Dr. Ulam who suggested the shock from a fission weapon for this purpose. Actually this suggestion led Teller to the idea of radiation implosion and both ideas were written up jointly and simultaneously by Ulam and Teller.

Following the GEORGE shot, it was obvious that the next step should be a full scale test of a real thermonuclear system.

At a scheduling meeting of the Technical Board of the

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Los Alamos Scientific Laboratory in September 1951, the tentative date for the MIKE shot was picked for the Fall of 1952. At this meeting Teller became extremely upset and asserted that if the date (obviously a year in advance) were not set to be no later than the Summer of 1952 he would resign from the Laboratory. Although the actual incident was trivial, underlying tensions had grown up primarily between Teller and myself but paralleled by similar reactions from my entire senior technical staff, regarding the most effective manner in which the thermonuclear program could be administered and accomplished, and were such as to make Teller's separation from the Los Alamos Scientific Laboratory probably inevitable. A number of sincere efforts were made by myself and other people to persuade Teller to change his mind and remain but these efforts were unsuccessful. In all frankness, Teller's volatility and sensitiveness tended to detract from the value of his theoretical intuition and ability and there was a general feeling, which I shared, that our progress would not be less rapid in his absence. He left Los Alamos in a reasonably friendly spirit; his relations with me personally since then have been friendly and agreeable although I presume we both feel some degree of personal disappointment in the other.

The theoretical design, development, and test of the MIKE shot was entirely an operation of the Los Alamos Scientific Laboratory, its consultants and sub-contractors, and Teller's contact with it was completely incidental and casual. He was not present at Eniwetok at the time of its firing, but his congratulations to the Los Alamos Scientific Laboratory, I believe, were thoroughly sincere and were received in complete friendliness.

The SHRIMP, HUFF, and other shots of the present program are, as you know, weapons (or weapon prototypes) also designed and developed entirely by the Los Alamos Scientific Laboratory, its consultants, and sub-contractors. It is my opinion that extraordinary credit is due our theoretical division for the ingenuity, skill, and persistence which they have displayed in this task.

Following his departure from Los Alamos in 1951, Teller intensified his campaign (which he had been conducting quietly for some time) for a second weapons laboratory. He returned to the University of Chicago and sought to have it established there. The Commission ultimately decided in the late Spring of 1952 to establish such a laboratory under the guidance of the Radiation Laboratory of the University of California, and located at Livermore, California.

Relations between the Los Alamos Scientific Laboratory and Livermore have been good. They have given us excellent assistance at CASTLE in certain measurements although they indicate that they do not wish to do this in the

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future. On the other hand, we have furnished them with copies of all our reports and documents, our personnel have been involved (at times seemingly endlessly) in the indoctrination of theirs; we have performed experiments for them, made primary bombs for them, and are now engaged in an extensive program of HE fabrication experiments for them. Teller has recently expressed the intent to spend several weeks at Los Alamos this summer.

There is one final matter which I should like to be certain is clearly understood. In 1950, as you well recall, there was extensive public debate (but not by Los Alamos) regarding the wisdom or morality of proceeding with hydrogen weapon development. Teller, of course, conducted a strong, personal, and somewhat publicized campaign along these lines. To the surprise and dismay of the Los Alamos Scientific Laboratory the GAC recommended against the program. This was essentially the first time in which we had found ourselves opposed to a point of view of so eminent a group of people. However, the LASL opinion - both mine, my senior staff, and as far as I am aware, that of all technical personnel, was to the effect that no decisions as to the wisdom or morality of stockpiling or using thermonuclear weapons could possibly be made in ignorance of the facts; and that the facts of such weapons were of fundamental importance and must be known by this country as soon as possible. It was our task to ascertain those facts as rapidly and as accurately as possible. Accordingly, we did not and, in my opinion, properly did not enter into the public debates as to whether this course was sound or moral. We did insist to the Commission that the investigative, experimental, and test program must proceed at the highest priority to provide the relevant facts with which to make future decisions.

An impression has apparently grown up in some quarters that the LASL was opposed to this program. On the contrary, we were not only most strongly in favor of its exploration, but had been, were, and are deeply and persistently involved in it. Not even a negative presidential decision, I believe, could have stopped the calculational, theoretical, or experimental effort, although it would have denied actual full scale tests. As a laboratory we privately deplored the FORTUNE statement and its exaggeration of a situation in which the LASL played a role almost exactly contrary to that ascribed to it by inference.

May I make it clear in closing that I am not making a specific request for public clarification of the role of Los Alamos in the events of the last three years. Official denial of those parts of the TIME story which are incorrect is not likely to be very impressive or to undo much of the damage which has been done. A conscious attention on the part of the Commission to more positive public statements in the future may be helpful, although I am far from sanguine that the problem is soluble and fear that it is inextricably related to the fact that two-weapon laboratories do, indeed, exist.

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My major purpose in describing this situation in some detail is to provide you with the basic facts in case such an account may be useful. These scientists, whose opinions the Laboratory values, know the accomplishments of the Laboratory and are unlikely to be seriously impressed by TIME, FORTUNE, or the speculations of the press services. The BOB appears to have been under some misapprehensions in 1950-1951, but it is unlikely that they still retain them at the present time. The clarity of understanding of the Joint Committee of Congress (and, indeed, congressmen in general) is far less certain and may be greatly influenced by what they see in public print. It is to the Commission that we must look to carry out the responsibility of making sure that no errors in understanding occur in these lofty areas, and hence my concern that there be as factual an account as possible in your hands.

Very truly yours,

Original Signed by N. E. Bradbury


N. E. Bradbury  
Director

NEB/as

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