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UNIVERSITY OF CALIFORNIA
LOS ALAMOS SCIENTIFIC LABORATORY
(Contract W-7405-ENG-36)
P.O. Box 1663
Los Alamos, New Mexico

May 23, 1952

In reply
refer to: T-ALLA

Mr. Gordon Dean, Chairman
United States Atomic Energy Commission
Washington, D. C.

Dear Commissioner Dean:

From conversations with Dr. Teller and with members of the G.A.C., I understand that many important members of the Department of Defense are gravely concerned that the Russians may be engaged in a major effort to develop the H-bomb and that they may actually be ahead of us. I fully share the first concern, and I believe that this possibility is the one compelling reason for our present rapid development of this weapon. On the other hand, I think there are good arguments for the belief that the Russian project has at least not reached a more advanced stage than ours.

It is obviously difficult for me to assess the Russian progress, especially since I have no access to intelligence reports. However, in the Fall of 1945, I made an estimate, purely on the basis of reasoning that the Russians could probably have an atomic bomb in about five years. This estimate, published in the pamphlet "One World or None", proved rather accurate, and I may, therefore, be permitted to make another guess. (Similar estimates were made in 1945 and '46 by many of my colleagues who had actually worked on the Manhattan Project and were repeatedly published in the Bulletin of Atomic Scientists and elsewhere. General Groves and Dr. V. Bush predicted times up to twenty years.)

Undoubtedly, the Russians were very much helped in the development of their fission bomb by the information given them by Dr. Klaus Fuchs. This





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removed much uncertainty from their thinking, especially concerning the actual assembly of the bomb. I believe they have saved at least two years due to Dr. Fuchs. They still had to make a great industrial effort to construct plants for the production of fissionable materials, and a sizeable experimental and engineering effort to perfect an implosion bomb. Their quick success in these tasks showed that their technology is at a very high level, and that they considered atomic bombs important.

It has been pointed out that Fuchs also gave to the Russians our information on thermonuclear bombs as of 1946. It has been argued that this information would lead them in a rather straightforward way to a successful hydrogen bomb. This conclusion, I believe, is incorrect because the H-bomb designs for which we now expect success are almost exactly the opposite of those proposed in 1946.

To substantiate this point, I enclose a brief history of our thermonuclear program to date. As you are undoubtedly aware, the "super bomb" envisaged in 1946 was based on a self-propagating nuclear reaction in deuterium. However, calculations in 1950 showed that this reaction probably cannot take place at all, or if it can, that it is highly uneconomical and impractical. If the Russians have followed the 1946 line of development, we can only be happy because they would have wasted a lot of effort on a project without military significance.

In 1951, Teller discovered an entirely new approach to thermonuclear reactions. I believe that among all scientists in the United States, he was the only one who could have made this discovery, due to his ingenuity and his persistent belief in thermonuclear reactions, in spite of the severe setback implied by the 1950 calculations. Even with Teller, the discovery was largely accidental. The new approach used high densities of deuterium rather than high temperatures and was based on two separate discoveries, (a) that high densities would be useful and (b) that they could be achieved by a radiation implosion. Whether this approach will actually be successful, only the test late this year can show. Whether the same accidental discoveries have been made in Russia, it is entirely impossible to judge.

In any case, the Russian thermonuclear development cannot have been straightforward, and we have no basis on which to predict the present status of their art, in contrast to their development of fission weapons where such a prediction could be made in 1945. We have, however, one very strong piece of evidence: there has not been any successful large-scale thermonuclear test in Russia because otherwise we would surely have observed it. The thermonuclear bombs we are now designing are exceedingly complicated, and many of their design parameters cannot be fixed by theory or preliminary

experiment alone, as was fortunately the case with fission bombs. Nobody, I believe, can arrive at a successful hydrogen bomb without one or more full-scale tests. We can, therefore, safely assume that the Russians are not as much as half a year ahead of us. I further would trust our own technical ability enough to believe that the time from the first successful test to a practical bomb which can be delivered, would be somewhat shorter for us than for them.

It is, therefore, my opinion that a continuation of our efforts at the present rate would assure us of a safe margin over the Russians. In the enclosed memorandum, I show that the progress of our work on thermonuclear weapons, since the first Russian bomb explosion and especially since Teller's discovery of the new approach, has been about as rapid as was technically feasible. I would expect that this will continue to be the case.

Clearly, no amount of work can assure us of a lasting monopoly in this field. On the contrary, if we now publicly intensify our efforts we shall force the Russians even more into developing this weapon which we have every reason to dread.

Yours very sincerely,

/s/H. A. Bethe

Hans A. Bethe

HAB:li

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# A REVIEW OF THE HISTORY OF THE THERMONUCLEAR PROGRAM

Hans A. Bethe

May 28, 1952



It seems appropriate at this time to review the history of our thermonuclear program, in order to correct two apparently widespread impressions which I consider erroneous. These are (1) that the progress of this program, since the Presidential directive of January 1950, has been slower than was technically feasible, and (2) that the Russians may have been able to arrive at a usable thermonuclear weapon by straightforward development from the information they received from Fuchs in 1946.

This historical sketch might perhaps be better written by a member of the Los Alamos Scientific Laboratory who would have more direct knowledge than I have. However, I may have the advantage that I have been equally exposed to the views of the management of the Los Alamos Laboratory and to those of Dr. Edward Teller who has been my personal friend for twenty-five years. Moreover, I have kept in continuous close touch with the work here and have participated in it part of the time.

In the summer of 1946, the following facts on thermonuclear reactions appeared to have been established by the work of Dr. Teller's group during and after the war:

[redacted]

(a) In a sufficiently [redacted] a reaction could take place and could propagate, given sufficient initial temperature [redacted]

(b) A self-sustaining and propagating reaction in [redacted] seemed a likely possibility.

(c) If successful, such a reaction could deliver energies equivalent to 1000 fission bombs and more, from a device weighing not much more than an ordinary fission bomb and containing mainly cheap materials. This device is now known as the [redacted]

(d) To initiate a reaction in [redacted] were useful, and it was believed that amounts of [redacted] might be sufficient for the purpose.

(e) The initial heating of [redacted] to the required temperature appeared perhaps as [redacted]

This was approximately the state of affairs when Fuchs left Los Alamos on June 15, 1946. From then until the end of 1947, rather intensive



[redacted]

theoretical work on thermonuclear reactions was done, especially by Teller, Richtmyer and Nordheim. In particular, the Alarm Clock, a device consisting of [redacted] was invented and investigated.

After the summer of 1947, work on large-scale thermonuclear reactions was curtailed, first because no idea for a thermonuclear weapon seemed to exist that offered great and immediate promise, and second because it was felt that the Los Alamos Laboratory, with its limited scientific personnel, could not carry this work in addition to its more immediate responsibilities of improving fission weapons. However, by the middle of 1948, Teller had invented the booster, in which a fission bomb initiates a thermonuclear reaction in a moderate volume of a mixture of T and D, and this reaction in turn serves to enhance the yield of the fission bomb. Substantial theoretical work on the booster was done in 1948, and on the basis of this work it was proposed in the Fall of 1948 to include the booster in the next weapons test. In the first part of 1949, a more thorough theoretical investigation of the booster was carried out. A recent test in Nevada [redacted] demonstrated the practical usefulness of the booster [redacted] Calculations have shown that the yield [redacted]

In September 1949, the first Russian bomb created a changed situation. As an answer, Dr. Teller recommended the acceleration of the thermonuclear program. The Los Alamos Laboratory which had in the meantime been greatly strengthened by the addition of new personnel, accepted Teller's suggestion immediately and enthusiastically. The joint recommendation by Teller and the Laboratory led, after considerable discussion, to the Presidential directive of January 1950, which in turn put the effort at Los Alamos on full scale.

The main progress which Dr. Teller had made prior to his recommendation concerned the initiation of the reaction (point e).



Apart from this, the program was resumed in 1949 on the basis of the theoretical assumptions of 1946. In particular, even as late as Spring 1950, Dr. Teller, in a memorandum to the G.A.C., estimated the amount of

Entirely separate from the main theoretical effort which was implied by Dr. Teller, [illegible] which only one assistant and one computer to help him, Dr. S. Ulam undertook the important task of determining more accurately the amount of T required. His results were spectacular: [the amount was calculated to be [illegible] or the equivalent of the [illegible] of present design.] More detailed and thorough calculations by other members of the Theoretical Division of Los Alamos confirmed Ulam's estimates. These results were entirely opposite to the 1946 assumption (d),

[redacted]

made the economic soundness of the H-bomb program highly questionable.

In the summer of 1950, Fermi and Ulam showed by an approximate calculation that there can probably [redacted] This conclusion, which was contrary to thesis (b) of 1946, has since been strengthened by the drop in experimental cross sections resulting from the accurate measurements of Tuck and his group in 1951. The calculations of Fermi and Ulam, however, were not definitive, and the final decision about the feasibility of a thermonuclear reaction in liquid deuterium will only come when a full-scale machine calculation on this problem is carried out which takes into account all important physical processes. Such a calculation has now been prepared. Even if [redacted] should turn out to be feasible, it will remain impractical and uneconomical.

Barring surprises from such a calculation, the theoretical work of 1950 had shown that every important point of the 1946 thermonuclear program had been wrong. If the Russians started a thermonuclear program on the basis of the information received from Fuchs, it must have led to the same failure. ]+

In spite of the apparent failure of the program, it was decided in the fall of 1950 to proceed with the planned thermonuclear experiment at Eniwetok in the Spring of 1951. This experiment which proved fully successful was designed primarily to confirm proposition (a) of the 1946, the burning of D-T, about which there had never been serious doubt. In addition the experiment was to try out [ one of several possible mechanisms which might be used to provide [redacted]

[redacted]

turn out to be feasible. In this particular mechanism,

It was largely accidental that just this mechanism was chosen. In one of the alternatives, Another alternative was the proposed by Teller in Fall, 1949, which by many members of the Laboratory was considered a more promising scheme. The accidental choice of proved fortunate because it led to a theoretical consideration of as well as of



The former line of work demonstrated that the this was perhaps not very remarkable. However, after several months, it occurred to Teller to me the bold extrapolation to the He was able to show by an approximate calculation that at a this would be the case even if

If this idea of it was necessary to Here again a fortunate

[redacted]

ccident intervened: in December 1950, Ulam had suggested [redacted] to use the [redacted]

[redacted] This idea was conceived entirely independently of the hermonuclear program, and its aim was to use fissionable materials more conomically. Ulam's idea was to utilize [redacted]

[redacted] Several months later, when Teller recognized [redacted] he importance of [redacted] he suggested that [redacted] ight be capable of achieving the required [redacted] This led to our present concept of thermo-



nuclear reactions.*]

This concept, then, came about by a series of accidents, the accidental choice of one particular device for the Eniwetok test rather than two others, the [ingenious extrapolation by Teller [redacted] just at the right time. None of these three steps was at all an obvious, logical development which would occur in every thorough scientific investigation of the problem. On the contrary, the results of the calculations

[redacted]

[redacted]

[redacted]

of Ulam and Fermi in 1950 (which were logical steps in the program) would have led nearly every scientist to give up the thermonuclear program altogether. Only Teller's persistent belief in the practicability of thermonuclear reactions led to our present, completely novel concepts in this field. It would be a most remarkable coincidence if the Russian project had taken a similar course.

[redacted]



[redacted]

It was immediately clear to all scientists concerned that Teller's new suggestion provided for the first time a firm basis for a thermonuclear program. Without hesitation, Los Alamos adopted the new program. The G.A.C. held a meeting on this subject in Princeton in the middle of June 1951. The meeting was also attended by the members and the manager of the AEC and by a considerable number of Staff Members and consultants of the Los Alamos Laboratory. The meeting was unanimously in favor of active and rapid pursuit of work on the [redacted] with a test to be prepared as soon as it was clear what exactly was to be tested.

[redacted]

However, in September 1951, when the initial calculations had shown promise, disagreement arose between Teller and the rest of the Los Alamos Laboratory as to the date for a full-scale test. Los Alamos proposed November 1952, whereas Teller demanded a date four to six months earlier. It will be shown in the following that Teller's date could not have been met.

The theoretical work started immediately, in June 1951. Four major problems had to be solved, concerning

(1)

(2)

(3)

(4)

The second of these problems was clearly the crucial one and received first attention from Los Alamos. Beginning already in the Summer of 1951, results were obtained from machine calculations and were very encouraging. Concerning (4), results on the efficiency of were obtained at Los Alamos in Fall 1951, and reasonably definitive calculations on were made by the Matterhorn

Project in Spring 1952. By combining the results of problems (2) and (4), it now appears probable that the [redacted] will work.

Problem (3) turned out much simpler than had been anticipated. It should be noted that this is a complete reversal of the position in 1946 when [redacted] But equally important in making the problem "easy" is the much better understanding of fission weapons which the Los Alamos project had acquired in the meantime.

Problem (1) was initially considered the easiest of all. In March 1952 unforeseen difficulties appeared, connected with the [redacted] These difficulties could only be minimized by a very major redesign of [redacted] This redesign came at the latest moment compatible with meeting the test date of November 1952; had Teller's test date been accepted, redesign would have been impossible and the test would very probably have failed.



I believe it is obvious from this outline that the theoretical program on [redacted] has proceeded at maximum speed from the moment this device was conceived. This rate of progress was only possible by the extensive use of high-speed computing machines which did not exist a year earlier. The conception of [redacted] itself was a matter of inspiration

[redacted]

and it was, therefore, unpredictable when it would occur; in my opinion, it is remarkable that it occurred so soon after full-scale theoretical work on the hydrogen bomb was resumed.

At present, roughly 75 percent of the work of the Staff Members of the Los Alamos Theoretical Division is devoted to thermonuclear reactions, and in addition the entire work of the Matterhorn Project. It should be noted that in this field the theoretical work determines the overall progress to a much greater extent than anywhere else.

Engineering of [redacted] for test was started in October 1951. This early start duplicated war-time procedure, and, just as during war time, put a considerable strain on both the theoretical and the engineering work because theoretical progress necessitated frequent changes of design. Preparation of observations for the test was an equally difficult problem and was begun as soon as [redacted] was conceived.



It is impossible to predict whether the test of [redacted] will be successful. On purely theoretical grounds, success may be expected, but the action of the device is so complicated that failure at one point or another would not be surprising. In case of success, the yield may be anywhere [redacted]

Even in case of success, [redacted] as now designed is too heavy (80 tons) to be practical. Reduction in weight to about 20 tons, and in diameter to about 65 inches may be possible by:

[redacted]

(a) engineering with smaller safety factors,

(b) reducing the volume of the [redacted] and

(c) using [redacted]

Perhaps more promising than the [redacted] is the "Alarm Clock." This device was invented on August 31, 1946, two and a half months after Fuchs left Los Alamos. In its original form, it consisted [redacted] Intensive calculations on this device were carried out by Nordheim, Richtmyer and others from the time of its invention to the end of 1947. The results were not very promising, even with the aid of [redacted] of the "Alarm Clock," and, therefore, the calculations were stopped.

About in late summer of 1950, Teller suggested that [redacted] in the "Alarm Clock." This [redacted] showed up very well in theoretical calculations. [redacted] it gives promise for a practical thermonuclear weapon. Relatively small amounts of [redacted] may suffice to give a yield of about [redacted] and the weight and size specifications could be met with more assurance than in the case of [redacted] However, the theoretical probability of satisfactory working of the "Alarm Clock" is

far smaller than for [redacted] because of the likely occurrence of Taylor instability.

For the further development of thermonuclear weapons, tests of components rather than full-scale weapons appear fruitful. Thus it has been suggested to test the [redacted] cannot be calculated reliably. One or more tests of Taylor instability have been proposed in connection with the "Alarm Clock." Finally, for the purpose of reducing weight, it has been suggested to test the [redacted] In this work, the collaboration of another [redacted] with Los Alamos would be desirable.



The yields now expected for devices which might be carried by a plane are of the order of [redacted] as compared with perhaps [redacted] in 1946. The yield of a fission bomb in 1946 was 20 kilotons; at present, a 500-kiloton bomb is in the design stage and [redacted] seems entirely feasible at the expense of more fissionable material. The yields expected from feasible fission and fusion bombs have, therefore, come rather close to each other, and while fusion bombs now appear feasible, they have remained extremely complicated in comparison with fission bombs. In addition, the weight of fusion bombs of substantial yield is likely to remain substantially higher than that of a Mark-6 fission bomb. All these points will tend to reduce their practical usefulness as weapons.

[redacted]

It should also be noted that there will still be a long way from the test of [redacted] late this year to a weapon. Even if the "Alarm Clock" should be successful, this time can hardly be less than one and a half to two years. It is more likely that theoretical work, component tests and full-scale test will show the "Alarm Clock" to be less efficient than is now expected, and that changes of design and, therefore, time delays will be necessary.

I would summarize the history of the thermonuclear development as follows:

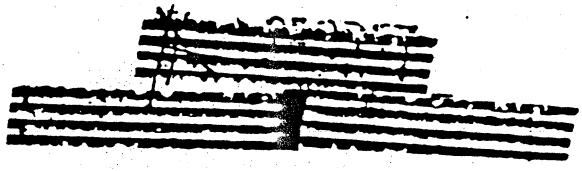
(1) The [redacted] as conceived in 1946 is probably not feasible, certainly impractical.

(2) There are at present only two promising ways to obtain large-scale thermonuclear reactions, namely [redacted]



(3) Development of a possibly practicable device could begin in earnest only after the invention of the radiation implosion which originated outside the thermonuclear program.

(4) The invention of [redacted] in 1951 was largely accidental. It is unpredictable whether and when a similar invention was made or will be made by the Russian project. The invention in our project could probably not have been accelerated by harder work. Since the time the invention was made, work has progressed at maximum speed.



(5) The "Alarm Clock" was invented after Fuchs left, [and became practical only by the inclusion of $Li^6$ (in 1950) and its combination with the radiation implosion.]

(6) The thermonuclear work at Los Alamos was never really interrupted. Between Fall 1947 and Fall 1949, the booster was developed which proved very important in its own right and proved closer to present design than the 1946 version of a full-scale thermonuclear reaction.



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