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*Mem to Jontep  
from AEC  
5/18/52*

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-411A

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 make 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



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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:11

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