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October 29, 1954

IN REPLY  
REFER TO:

Professor H. A. Bethe  
Post Office Box 451  
Ithaca, New York

Dear Hans:

Carson has shown me your article on the history of the H-bomb and I would like to make some comments. Let me say at first that I think that it is an excellent idea and I consider it as a splendid exposition of much of the truth. Perhaps, due to the unfortunate or even tragic, in a sense, publicity of the last weeks, one good thing that is happening is that the truth is coming out, slowly but surely.

Your article is excellent -- if anything it contains a few understatements of the case. (The well-known feature of the "big lie" is that it imperceptibly forces everybody to adopt some of the framework or terminology intended by the "enemies" and forces an impression of "defense" on the situation which should be really reversed.)

Let me say, on a few examples, what I mean:

1) It could be argued (with much less exaggeration than constantly present in Edward's frantic propaganda), that if one had followed Edward's approaches proposed by him during the war as alternatives to the now existing fission bomb and pushed, fortunately unsuccessfully, for first priority during the wartime years -- the fission bomb would not have been made at all during the second World War. This, of course, you know better than anybody else and while I realize that statements about it are very hard to make because of classification, nevertheless this is an important fact and perhaps constitutes the psychological background partly explaining all his nefarious activities after the war.

2) In connection with the "second part of Method D," it seems to me that in reality it is merely one of the applications of the first part of Method D, as you call it; in fact, the paper which described the Method D had many similar proposals together with a detailed plan for calculations with some, admittedly rough, estimates and drawings of the possible objects.

3) All the objects that now exist are variations on the same theme and while they make things cheaper and bigger, there is no question of any revolution in military application, but indeed the whole group is more of the same thing stemming very directly from the fission bomb development. An entirely different impression is being forced on the public.

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4) It seems to me that, with tremendous modesty, you play down the very essential role of the actual scientific work of developing the so-called ideas, the enormous number of calculations, all the studies of the general physics of the processes, the engineering planning, all combined with the necessity of predicting and avoiding "side effects," anyone of which could ruin the success of the device. This work which Carson and you have planned, directed and executed is indeed much more important than the mere sketches of the thing which, as we now know, are subject to terrific instabilities in design. (An attempt is made by Edward to describe the Los Alamos part as providing "hardware.") One proof for the vital importance of such care is the success of the Livermore experiments. Wouldn't it be a good idea to give an inkling of the immensity of the project? The fantastic rapidity with which it was brought to a successful conclusion by indicating the vital importance of the work which Carson has done in this connection? After all, the effort was essentially a cooperative one and its planning is one of the most impressive examples of speed and success that I know of in all the history of technology.

By the way, it seems to me it would be hard to exaggerate the importance of the contribution made by Fermi in the decisive switch from the original, hopeless approach towards opening one's mind for the necessity of really different ideas.

Indeed, being forced as it were, on the defensive we ourselves adopt phrases like: "the development was not slow; there was no dragging of the feet," etc., etc. In truth the exact opposite should perhaps be firmly asserted. One of the things which continues to amaze me is in what unbelievably short time, starting from a couple of reports written on the "knee," a fantastic number of computations and plans were executed and with what thoroughness and precision the thing was finally made.

One should not forget that, if the first experiment in this line had a success like, say, the Livermore thing, the whole program could have been stopped or delayed perhaps for an indefinite time. So accusations of delay should not merely be refuted but reversed. Indeed, it was never clear to me whether Edward's activities had not in reality protracted the thing? His insistence on Method A and unwillingness to admit the validity of the results of the calculations discouraged, perhaps during several years, a search for alternative methods. It is objectively true that accidents and luck play an enormous role in the development of these things.

In short, what I want to say is that I admire your account, its conciseness and restraint and I am merely trying to point out that, so to say, even more than what you indicate is true.

I have been East now for a period of eight days visiting in Chicago where I saw Fermi in the hospital; an account of that to you will have to wait for when we see each other. I also saw Rabi in New York and Johnny von Neumann in Princeton. Had a long talk with Oppenheimer, too.

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I understand you had a very pleasant vacation in Europe and would like to hear about it. Would like to see you and Rose very much. When will you come here? There are so many things to discuss!

With best greetings to you and the family from all of us,

Yours as ever,



SU:11

P. S.: Excuse my being so repetitious above. Sometimes I wonder whether most of the story could not be summarized as follows: Until early in 1951 there was nothing that looked, after a more thorough analysis, at all promising. There was a great deal of solid exploration going on -- and all the political noises or machinations did not change a whit in this. Then, after some promising, but different, directions were discovered, the thing was accomplished with unbelievable rapidity. Whether all the propaganda and all the misrepresentations, all the frantic noises accelerated or delayed the discovery of the working method will not be argued here.

S. U.