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April 14, 1945

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By Les Redman 11-22-78

By Tony A. Ruffin

Dr. H. D. Smyth
Palmer Physical Laboratory
Princeton University
Princeton, New Jersey

Dear Smyth:

Your second letter on the history arrived just a few days ago, needling my conscience on my failure to answer the first. I had given your draft to Feynman shortly after it arrived. He has been very busy pinch-hitting for a man with a broken leg and doing three jobs at once, thereby doing even less work on the draft than I have done. After seeing your second letter I got the draft back and thought it was high time I let you know what my thoughts were.

It was hard for me to understand just what had been agreed to between General Groves, Dr. Conant and you on the contents of the revised chapter. One thing I gather is that there will be a lot less about ordnances and assembly, and since this had been one of the parts which I felt had been somewhat inadequately covered, I am not quite so worried about the draft as I was. In general, my feeling about it was that it presented a somewhat spotty and, perhaps for that reason, a somewhat distorted view of the efforts of the Laboratory, particularly on the ordnance side. I thought the part on the theoretical work was just about perfect and that the general account of nuclear physics was quite good. What I had said to General Groves and to Dr. Conant was that it could not be regarded as an authoritative and complete history of this project; and I have recently come to feel that we have need for such a history for more than one reason.

Perhaps the most general error of fact in the draft was the statement that we only started taking implosion seriously after the discovery of 410. The decision to go into this program and the decision to give it over-riding priority was made late in 1943, and as a result of this, George Kistiakowsky came here, we started building plants and firing sites and began to learn where the troubles lay. It is possible that, in view of the discouragements we encountered in the first eight months of 1944, we should

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indeed have abandoned the implosion program had not the discovery of ^{240}Pu and our total inability to think of promising alternative assemblies forced us to continue it. I don't think I should have taken that line, and the evidence of the past months has shown that it would have been a terrible mistake; in the past months I think we have had the fundamentals of the implosion licked, and the future in this field looks bright.

Another point which I found somewhat critically misleading is the remark that the Ordnance Division was concerned primarily with gun assembly. It had, and still has, the all-important and difficult job of making a weapon out of this thing, of fusing and designing it in such a way that it can be used in combat and can be effective. I take it that you won't be saying much about this aspect of the work, but you should certainly know that it is a very large set of problems and will increasingly be so.

I would like now to go over the draft picking out certain points, minor and major, where I thought a correction might be desirable. This will not be complete and some of the corrections may seem trivial to you, but I think you should have them:

Page 2, line 9. "December" should be "October".

Page 3, line 11 (from bottom). "January" should be "November".

Page 4, line 8. "April" should be "March".

Page 4, line 11. The University of Minnesota should be included as an important contributor.

Page 9, line 10. "Mass" should be "radius".

Page 12. This is the point I have discussed above. The history of implosion was roughly this:

It was proposed by Hedderneyer at the April conference, and some arguments were given by him to show that it would give a faster assembly than the gun; work was carried out on exploratory basis which gave misleadingly hopeful looking results. The matter was considered again by von Neumann in the Fall of 1943; he expressed the opinion that the implosion would work better with high charge to mass ratio, and might avoid the necessity for extreme purification of plutonium, at least for very small gadgets, because it would give such a rapid assembly. The compression on the material resulting from high velocities was then pointed out by Teller and investigated by Bethe. After much struggle and argument the implosion project was adopted with over-riding priority late in 1943. The later history I have outlined above.

Page 14, line 2. This I have corrected above.

Page 15. In the last paragraph Feynman thinks one should say that one assumes that the neutrons have the same velocity and ~~are~~ are

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scattered isotropically, rather than suggesting that there are systems like this in nature.

Page 23, line 5. I believe this is not true. There was some discussion of which isotope was responsible, but there were no arguments that anybody believed until the measurements were made by Segre.

Page 24, Par. 2, line 6. I don't recognize this statement. We have thought that the high energy branching ratio was fairly low, and all numbers given by us were estimated upper limits. We still think it is low.

Page 25, line 5. "Quarter" should be "third".

Page 25, line 15. I would use the word "surprise" rather than "setback", but that is a matter of judgment.

Page 25, Par. 2, line 3. "Nitrate" should read "sulfate".

Page 25, last line. "5 kilowatts".

Page 27, line 10. We don't ask the metallurgists for foils. This is a function of Dodson's group.

Page 32, par. 3, line 1. The 3000 ft/sec figure always referred to 19. The 25 velocity was not set until firm limits on the spontaneous fission of the isotopes enabled us to take 1000 ft/sec. This occurred early in 1944.

Page 33, line 6 (from bottom). "Several thousand feet" should be replaced by "about 1000 feet".

Page 34, Par. 2. Kistiakowsky and Bacher's divisions were set up late in October, 1944 with Kistiakowsky in charge of explosives and Bacher in charge of Gadget design and implosion studies. The division has not been sharp.

Page 37, line 5 (from bottom). "Constructed" should be "installed".

Page 41, line 6 (from bottom). This doesn't have to be clear to you; I think it is true: at least we have all the NDRC talent, much of the British talent, and much of the talent of industry available to us, and we are working quite closely with the Bureau of Ordnance. As you probably know, our relations with Army Ordnance are poor but this is the only lacuna that I can think of.

Page 42, line 10. The theoretical behavior is well known, but we are not sure that the theories are right.

Page 42, Par. 2, last sentences. The tank is on a lot faster schedule than Hanford. It is arriving tomorrow night at the test site.

I realize that these comments may seem rather useless and rather cantankerous to you. They are not meant so, but just to be sure that there are no minor errors of fact that should be a blemish on your account.

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I hope that when the revised draft is written you will pay us another visit. I am sure that with another talk we could get the bugs out of it; and I think you would be interested to see how rapidly things have moved. In any case it would be a great pleasure to me if you could come.

With all warm greetings,

Sincerely yours,

J. R. Oppenheimer

cc: *my file J.R. Oppenheimer*

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