(31)

Teller

The Keyworth Company

April 9, 1987

Washington Harbour
Suite 360
3050 K Street, N.W.
Washington, D.C. 20007

Dr. Edward Teller
Hoover Institution
Stanford University
Stanford, CA 94305-6010

Dear Dr. Teller:

Enclosed is the book "How Democracies Perish" by Jean-Francois Revel and the transcript of a taped conversation on 9/20/79 between you and Jay that he promised to send you. On his list of things to send you, Jay wrote "Letter of autobiographical story" but doesn't remember what he meant. Do you know? If so, please let me know and I'll be glad to send it to you.

I did the transcription of the taped conversation between you and Jay immediately after you finished talking in 1979. (As you can tell, it was done on a typewriter and has not been retyped to incorporate Jay's handmade corrections.) It has been sealed and stored since that time and no one has seen it but Jay and me. We also have the tape and can consult it if you have any questions about the transcription.

With best regards,

Carol Lynch

Carol Lynch
Executive Secretary

Enclosures A/S

092079..ET

09/20/79 **Transcript of taped conversation between Edward Teller and Jay Keyworth concerning an important question in the wartime history of Los Alamos-- "How the idea of the implosion emerged." (092079..ET)**

20 September 1979

This is a short statement concerning an important question in the wartime history of Los Alamos. And that question is, "How the idea of the implosion emerged." When we started work the only program that existed was to assemble the parts of the bomb with the help of a gun. Within the first few weeks, and I don't know the precise date, one of the participants, Seth Naddermeyer ~~Medemeyer(?)~~ proposed a different approach. That was the approach of implosion. Seth did not have any real idea how important the implosion would become from the point of view of compressing the material. All he was after, and it was a very sensible point, that by this high-explosive method you can go ahead with the assembly faster and thereby decrease the probability of predetonation. And indeed predetonation was a concern of all of us at that time. I again do not remember the precise date, but perhaps a couple of months after Los Alamos was started up, it may have been around the first of June but it may have been sooner or later, Johnny Von Neumann came on his first visit and almost immediately he gave us a talk about a very remarkable effect with which we were not familiar at the time, but with which we are all familiar now--the formation of jets. He described that and described the theory of it and then suggested that the ^{235}U or plutonium may be assembled by the same mechanism. Actually toward the end of the war I together with some members of a group that I led tried to make calculations about that and see whether it would be an alternative. I don't think it is a viable alternative and we did it only for sake of completeness. After the lecture Johnny came home with me and I don't remember whether it was in the lecture, I think it was actually in the lecture, that one of us, and I think it was myself, mentioned to Johnny the approach of Seth Nedemeyer, the implosion approach which actually at that time not many people took seriously. Even as early as that we already had a bit of a manomania of pursuing one definite program and nothing else. Johnny got interested and in my home--one of those four-family barracks--we started to make crude calculations based on an exceedingly simple idea; namely, the obvious idea that solid matter is incompressible. Since the same amount of material had to go first through a shield of the surface $4\pi R1^2$ and then through a surface $4\pi R2^2$, R2 being smaller than R1, the material had to be speeded up so that the velocity should be proportional to $1/R^2$. From this he then proceeded to calculate, very accurately but without any particular art, not only the high velocities that one would obtain starting with the velocity in part of the high explosives, a quantity which at that time was unknown to me. But he also derived accelerations and the pressures that were needed. Now by the time he had that formula I remembered something. The pressure that one got that way went into the megabars and in fact on the incompressible assumption you would have reached easily 1000 megabars. At that point I said, "wait a moment." I knew something about geophysics and I knew that at the center of the earth at the pressure of meybe 5 megabars, I do not remember now and did not remember then the precise figure, but a few megabars iron is compressed 20 or 30% and that at the pressures which would occur if we assumed incompressible materials the materials in fact could not remain incompressible. It was also by that time very clear to me that a compression by a factor of two would greatly influence the critical mass and the conclusion was then evident that implosion would be an excellent method not only in order to avoid predetonation but also to save critical materials of which at that time we had very little, and the earliest time at which nuclear explosives could be used would depend on this compression in a decisive manner. Next day we discussed that quite generally, the idea started to catch on. It was taken by that time quite seriously but was not completely acceptable until at a somewhat later period when Segre observed spontaneous fission, which of course increased the danger of predetonation and made the implosion that much more desirable.

There is one point that should be well to remember, from the very beginning Johnny proposed and, of course, I had worked with him and participated in all these things, that the implosion should be started from a shell. There ensued long debates concerning stability and that this would be completely reliable. The eventual case design to have uranium to help the plutonium assembled, a subcritical amount, in a solid sphere and only implode the reflector upon it, the damper on it. That was a suggestion which was acceptable because of excessive caution, because we wanted to be very sure that there is no mistake but that what one could do much better had been generally realized and indeed it had been made plausible, although not foolproof, that the implosion would be symmetric enough even if it started from a thin shell. One last remark, the detailed calculations for the implosion taking into account compressibility were too hard even for Johnny. But by that time he knew enough about the handling of computers and the use of computers in solving partial differential equations so that he suggested, and the suggestion was accepted, to introduce into Livermore, into Los Alamos--sorry for the Freudian slip--clumsy IBM equipment by which the calculations should be carried out. I have to justify my Freudian slip, when after a lot of effort and years later Livermore got established my first proposal that was at once acceptable was that we must acquire the services of a computer before we did anything else.

Keyworth--Excuse me Edward, I have one question about this. I was unaware that the concept of a shell preceded the concept of a solid fissionable ball. Could you comment more on how this evolved?

Teller--The shell was the first concept from which we started. I suspect, in fact I'm practically certain, that Medemeyer had it, and Johnny's first calculations, crude calculations, were based on that very shell. Next day when we started to persuade people that this is how one should behave, it was the shell that was discussed. Of course from then on the problems became quite difficult because we had little idea about the equation of state that was to be used. We had only indications there. But already on the first day we began talking about compressions as high as 5 ~~feet(?)~~. A lot of the later discussion was concerned with the stability of the shell. I particularly remember it in one connection. I had the job, among others, to indoctrinate newcomers and one newcomer whom it was a great pleasure to indoctrinate a couple of years later was Enrico Fermi. And in telling him about the various things including this story of the implosion I pointed out to him that one of our chief worries was to be sure that in the implosion no instabilities developed and one of my challenges to him, which was in fact a challenge to all of us, is to come up with a really hard proof that there would be no instability, and Fermi coudn't do it, neither could anybody else. And that is why at a quite late date shortly before the first test we retreated to the so-called "~~crysty~~ Christy gadget" which started from the solid ball and there instabilities could in the end, even if they occurred, do only limited damage.

Keyworth--So here we are more than 35 years later with the role of instability still completely un-understood in primary design and untreatable mathematically. Hmm!

Teller--But we are familiar with it and if we are familiar with it we are less afraid of it, which may be right or wrong.

Keyworth--True, but our familiarity is entirely empirical, no more profound.

Teller--This will be a summary of the sequence in which the ideas about the hydrogen bomb have arisen. Of course it is incomplete because I talk only about the things in which I have participated. But as far as development is concerned, from the beginning to the (present?) I believe I have participated in most of the events as far as the hydrogen bomb in the United States is concerned. It started at Columbia and in this case it was an idea of Enrico Fermi that triggered it. We had usually lunch in the faculty club and as we came back from lunch to Pupin I remember that Fermi stopped just before Pupin and said, "Now, if the nuclear bomb works we can reproduce fusion as the (energy source?) in the sun, except that of course we would not use hydrogen but deuterium where the cross section is very much bigger." I gave it some thought and practically a week later we went to the Fermi's, Mitzi and I, went for a walk and on the walk I proved to Fermi that it was a bum idea.

Keyworth--What year was this, Edward?

Teller--That was, I am not quite sure, yes. I have to calculate backward. We came to Livermore, er, again, we came to Los Alamos in the spring of 1943. I believe to tell you about the discussions we had in Berkeley in the summer of 1942. This must have been in the fall of 1941. I proved to Fermi it could not work because in order to get decent cross sections we needed high temperatures and at those high temperatures the whole--practically the whole--energy would go into radiation. Therefore the reaction would certainly not work. Of course I talked only about the case of equilibrium, but that passed unnoticed, unnoticed by me and unnoticed by Fermi, and he accepted that it could not be done. A few months later, in the spring of 1942, the Met lab was established in Chicago. And I was put together in a room with Konopinski, told to work with him and we had absolutely nothing to do, no program for us. I was told, "do whatever you think is reasonable." So at that time I was still in the strange state that I thought what is reasonable is the same thing as what is fun. A state from which I had to retreat but which may have been more justified than most people believe.

At any rate at that time I thought it would be fun to write down this argument of Fermi and the proof that it could not be done so that the mistake should not be repeated, and I remember taking out the pad, you know with nothing on it, and I said now here are the calculations. And ~~Kirsky~~ (which is what we called Konopinski) made a very

~~Ksky~~
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serious face and said, "Yes, I see that you have used invisible ink." So I had to confess to him that the problem may never have been written down. But I tried to explain to him and the more I explained the less convincing it was. By that time I was conscientious enough to say that of course equilibrium would be established. And indeed by that time I had convinced myself of this in the case of uranium. That in the case of hydrogen equilibrium would take longer was clear, that it would take so much longer as to make a real difference, this was not clear. But after a discussion that lasted a week or two I was now conversely convinced and so was Ko~~y~~nopinsk~~y~~i that the Super (the hydrogen one) could be realized, probably by using an atomic explosion and sending a shock into liquid hydrogen, or rather liquid deuterium.

In the late spring of 1942 the job of theoretical leadership of the project which had been hel~~ped~~d by Gregory ~~Bright~~ Breit was transferred to Oppenheimer. Oppenheimer called a conference in Berkeley to which Bethe and I, (Blons, ?) Serb~~e~~er was there anyway and a few others including Van ~~Plecht~~ Vleck were invited. On my suggestion, ~~Kirsky~~ K'ski came along. And on the train from Chicago to Berkeley ~~Kirsky~~ K'ski) and I explained the matter of the Super to Bethe and convinced him. We were supposed to discuss the atomic bomb in Berkeley. We did so to a very small extent, I would say 90% of our discussions were concerned with the hydrogen bomb and we looked into all kinds of details. ~~Kirsky~~ K'ski and I had already developed the idea of an ignition temperature. A temperature where the energy production by fusion would exceed the energy loss by radiation. Approximately, we estimated, 20 kolo~~watt~~volts--which was correct. From our early calculations on atomic bombs we easily found that we could not produce temperatures as high as 20 kilo~~watts~~ volts. Our main concern in Berkeley was how we could up to this temperature and shock arrangements by which we could accomplish a higher temperature in the low density liquid hydrogen than was available as a temperature in the driver seemed to several of us the obvious answer and indeed the question was not trivially simple but did not seem to be unsurmountable. We went on from there and considered lots of other details and that was a cooperative effort between all of us, obviously very much including Oppenheimer. We did take into account questions like heat conductivity, high temperatures and liquid hydrogen densities, the possibility to cut down heat conductivity by magnetic fields, in other words we did go in several respects into some detail as early as the summer of 1943. I had to go back to Chicago, I was lent from the Met lab and ~~Kirsky~~ K'ski and I had to go back; incidentally, two other important points which we discussed, ~~Kirsky~~ K'ski came up with the idea that the d,~~d~~ t reaction would be much better than the d,d reaction. And we also realized that there would be a difference of temperatures between the deuteron ions and the electrons and that problem was solved by Ko~~r~~nopinsk~~y~~i in a very direct method and by me in a very much simplified method which I still am trying to use in discussions about this relevant energy exchange in controlled fusion. This problem, of course, is now quite unclassified. Approximately at the same time when ~~Kirsky~~ K'ski and I came back to Chicago, Oppenheimer (I think it was a little later) went to see Compton, told him about this development and as I then understood it from Oppenheimer used this unexpected development for establishing Los Alamos. The opinion at the Met lab was that everything could be done there. It needed a little bit of imagination to understand that making the fissionable material was not the only problem in atomic explosives. I remember that for instance Eugene Wig~~mann~~ner, when the plan for Los Alamos had been evolved and when I decided to go there, disapproved of it in every way, he said all the important things will be done here in Chicago; once we have the fissionable materials there will be no further problems.

Actually we got to Los Alamos in 1943 around the first of April and at that time no further progress had been made on the hydrogen bomb. Furthermore, after we got to Los Alamos it very soon turned out that indeed other problems would emerge, predetonation, implosion. And not long after we arrived Oppenheimer discouraged further efforts on the hydrogen bomb. He did not discourage it completely, for instance, measurements on the d,t cross section, which at that time were not well known, did proceed. Also, Oppenheimer did not object to my putting a considerable portion of my time into hydrogen bomb work and I think it was understood that two or three other people might help me with it. But that was about the limit. It was indeed difficult to make progress because Oppenheimer's objections to dividing our efforts in many directions had some real substance and I myself had been in the meantime quite interested in other topics; to mention only one, to calculate opacities, which at the time was generally neglected in any design work. Oppenheimer agreed that this was important, agreed that I should pursue it but would not allow anybody else to work on opacities but then proposed that a little project should be started wherever I wished, and it was done actually at Columbia, to work on opacities; incidentally, without telling the people, Maria Mayer and two young students, what the work was for except for the little point that when I had to tell them that they should calculate the opacities at 5 kilovolts some eyebrows went up, some eyebrows were raised. But as far as I know they never asked me any further question about it nor discussed it among each other. Maria afterwards told me that she was quite surprised by the atomic bomb. I am sorry that I did not ask her or didn't tell her, but she must have known it; I didn't, we avoided the topic. We tried to observe secrecy in every possible way, and of course, that means also in some less than completely (Sinmite?) ways.

¶ I am not sure but I believe that the next step in the hydrogen bomb considerations came as late as Christmas, 1944. Almost two years after Los Alamos had started. There was just too much else to do, and I had considered the theoretical problems as solved. The next step emerged when I visited Johnny von Neumann and I believe it was Christmas, 1944--I am almost certain of it--it might have been Christmas, 1943, but I don't believe so. And I tried to explain to him at that time the situation about the hydrogen bomb, he was very much interested because he even then planned to put the complicated processes that were involved on computing machines. That was his interest and he inquired step by step how it would go, and when he forced me step by step to explain, I suddenly realized that I had forgotten something, and all of us had forgotten, something very important which I called then and I think the nomenclature has remained, the inverse Compton effect, which of course is not inverse at all, except for the point that the electrons in the case of the hydrogen bomb, the classical hydrogen bomb, transfer energy to the radiation and help to fatten up the low frequency ~~quantum~~ quanta that had been plentifully emitted. When I realized that, on the basis of the most crude calculations, I felt that I should turn around and say again that the hydrogen bomb is not feasible, because too much energy would be lost to radiation. ¶ Actually the dilemma was quite clear. If you had relatively little liquid hydrogen it would blow apart before it would propagate. Of course the obvious design is to have a cylinder along which the burning of hydrogen would propagate; too little and it would extinguish itself by expansion, too much, too thick a cylinder, and the Compton effect would take over and would prevent the atomic nuclei to retain enough energy. By that time Oppenheimer has realized that he could use me most effectively by putting me on one side. I was effectively, essentially on my own initiative, excluded from the programmatic work and I was looking together with a few very goo people including ~~Kirsky~~ K'ski, including Harold Argo, who might remember those days, and Mary Argo, a few others and very much including Henry Hurwitz. Our job was to look at far-out ideas including the Super and we discussed that matter and then when I went on a trip I came back with some ideas how better to estimate the inverse Compton effect and that quite probably it would not be fatal anyway. Henry Hurwitz while I was away did the same thing, but much more thoroughly and the first formula how to calculate in detail the effect of the inverse Compton effect, I mean the influence of the inverse Compton effect, came from Henry Hurwitz. Incidentally my whole group was put under the supervision of Fermi who had come by that time to Los Alamos. He was interested in other things as well like putting together a small nuclear reactor, the water boiler in Omega Canyon; but he listened to us from time to time, so did Neils Bohr when he came to visit.

Incidentally one of the many jobs that we had and one I particularly liked was toward the end to evaluate the possibilities how a chain reaction could produce worldwide disaster and the paper, since declassified, by Ko~~r~~nopinsky, (Marvin) and myself proved that this could never happen. The same thing I then had to carry even farther to answer questions whether it could happen anyway if physics had different laws than we now knew, a very strange assignment, but of course these laws would have to be at least consistent with facts as we knew it and to try to let all of physics stand on its head and imagine a new set of laws was of course too big an undertaking which was not completely absurd because after all the advice of people like Fermi, Bethe, Oppenheimer, Bohr was available and was available for that very purpose. And incidentally while Fermi was not terribly interested in the Super, in the hydrogen bomb, in that question he took a great deal of interest.

When the Alamogordo test succeeded, Oppenheimer started to reorganize for the purpose of going after the hydrogen bomb and that is what I wanted and I was very happy to see that now people like Fermi and Bethe would begin to get really interested in the subject. On the day where Japan surrendered, Oppenheimer came in to see me and said that Los Alamos now has to be dissolved and he would not like to see any futher support for the hydrogen bomb. This was something that could not be changed except I got one concession, and particularly I think I got it from Bradbury who then after a few months took over from Oppenheimer. Namely that in the summer of 1946 we should have a conference for a final evaluation of the feasibility of the hydrogen bomb together with appropriate machine calculations. These had been carried out during the intervening year by Frankel, I think also Metropolis, but I am not sure. It was not Frankel alone, but I don't remember who else. And they found that with the data available the hydrogen bomb would easily work and that is how matters stood when they were put aside. I kept coming back for visits and kept on making other plans. In particular Richtmeyer and I discussed a strange layered structure consisting of fusion and fission layers which we for a peculiar reason called "alarm clock" and for which a few calculations were carried out. So alternative possibilities had occurred even before 1949.

¶ The real change came in 1949 and was really due to the Russian bomb and among other things to Ernest Lawrence and Louis Alvarez coming to visit me. They heard about the hydrogen bomb and they persuaded me that now I couldn't wait longer. At any rate at that time we did go to work and we planned a few tests. In particular, we planned the ~~Jog~~ George shot knowing, of course, full well that this would not show anything about propagation but it would really try out experimentally whether we have calculated rightly about radiation transfers, shocks, compressions. In that effort, incidentally, quite a number of other people, very particularly Johnny Wheeler then later Conrad Longmire, Marshall Rosenbluth, participated. Also at that time we fully realized that fusion with the use of tritium is quite easy. I proposed at that time the principle of boosting and to the best of my memory the man who made the first decent calculations on that proposal was Marshall Rosenbluth. You might want to check with him. We prepared for a test and in the meantime, however, Ulam called into question the results of the

(Cornelius Everett)

Frankel's calculations and in particular he called into question and rightly so whether there is a radius of the cylinder along which propagation should take place, which was thick enough so that disassembly will not be disastrous and thin enough so that the inverse Compton effect should not stop the reaction. Helped by one other man whose name I forget but who was very diligent, he demonstrated that actually the Super cannot work. A few weeks later the calculations that Johnny von Neumann had proposed for a long time and which were very much more detailed than the Frankel calculations fully justified Ulam's objections. It was decided that we should go ahead with the test anyway but the road beyond that was completely obscure. I also was told by Bradbury that there must not be any considerations beyond calculating the test results until the test plans were completed.

Now in the test there was first the ~~Jog~~ George shot to test our principles and second a boosting shot, the first booster. All this, I remember very clearly, was put to bed, that is the designs were frozen. On January 15, 1951, which happened to be my birthday, when that meeting ended with agreement on how to do the testing I said to Bradbury and now I want to go ahead with the next one, and Bradbury said it is too late, there is no point until after the test. I found that rather outrageous. The more so because by that time I knew how to solve the problem, I cannot tell you when I knew it, I think it was in December, 1950. It was not long before that January 15 meeting and I also know very accurately why I did not understand all this much sooner. The reason was that I had a scaling law, one obvious way how to change things was to compress the material but I knew that all processes that we ever discussed were due to binary collisions and that showed that what happened at one density with one speed with one linear dimension would happen at another density with another speed with other dimensions, but the same thing would happen and if it is impossible at liquid hydrogen densities it would be equally impossible at thousand times liquid hydrogen densities. Now this was obviously wrong and I don't remember when I realized that it was wrong. It was a very simple point that if you compress sufficiently then you could tolerate equilibirum and the first name of the new design was indeed the Equilibrium Super. And this to my mind was the decisive point. Of course I was not happy with this until I found why the scaling document is wrong formally, and it is of course wrong because equilibrium is established through absorption of light quanta that we have always neglected and absorption of light quanta is a three-body process and therefore it doesn't scale. After that there was no question in my mind how the Super should be made. We needed a primary, we needed a secondary, we needed a sparkplug, the secondary had to have cylindrical symmetry because that was the symmetry natural to the process. The energy transfer had to go by radiation in order to make it as close to simultaneous as possible. All this was clear to me. I don't know whether all of it before the 15th of January, I believe so. And I believe that very shortly after that event I also told the new story to Johnny von Neumann. Sometime in February Ulam came into my office and said "I have a way to make the Super. Let us compress the material." I said, "yes," and then he said well you know we could, for instance have here a nuclear explosion and then put around it some containers to make a star-like structure and put deuterium in here and they will be compressed by the shock and then it will work. And I told Stan, I said, "Stan, (That is) the simplest thing and it might work but I think I know something better. You should not compress mechanically, you should compress by radiation." He wouldn't take it so I said "all right." Stan could talk an awful lot and consume a lot of time and by that time we did not get along very well. I said, "Look I will put down both of these ideas into a paper and we both sign it." And in that paper I explained how, and for the first time I wrote it down, that compression would help, that you could compress by shock or else you could compress by radiation and that it was much better to compress by radiation than to compress by shock. I did not say whose idea was the one, whose idea was the other. We both signed it. Then I had explained all these things to Freddy DeHoffman who wrote a much more detailed description of how actually it should be done and I think that that report again Freddy and I signed.

¶ Now I would like to mention here, on the side, something that in a logical structure I should have mentioned earlier. At a much earlier time I realized that another good fuel for the thermonuclear reaction would be lithium-six deuteride and the reason is obvious and is known to everybody by now. That was acutally the summer of 1950. I realized that and I went to Ewing(Li) and I asked him, "could you separate the lithium isotopes," and he said that should be quite easy and why don't I talk with people in Oak Ridge and they had started on separating lithium isotopes as early as the fall of 1950. The actual, uh which shot was it, Mike shot, was liquid deuterium. It could well have been lithium-six deuteride because by that time we knew that in equilibrium of course it did not make any difference and that this would be a very good fuel. I tried to explain this idea to Darrell ~~Vroeman(?)~~ Froman, to Bradbury, they wouldn't listen. I explained it to the Chairman of the Atomic Energy Commission, he listened politely but as it turned out, in reality he did not listen.

Now I want to clarify one very important point because it has become since quite controversial. One of the things that has occurred only once in my life in connection with the hydrogen bomb is a question of priority, who thought about it first? In general, I am not interested and if the collaboration of Ulam and myself there would have been nothing more than what I have told so far, then I would have proceeded and happily acknowledged that this invention is due to Ulam and me. But, when the ~~Jog~~ George shot had been fired Ulam went around and talked to everybody in Los Alamos who would listem that the ~~Jog~~ George shot has proved that the hydrogen bomb could never work. He travelled to the East and carried on this message. Why he did that I don't know. That he did it is beyond question. Now you know the situation. Ulam did not have the idea, he did not write the paper, and when it came at last to the decision after ~~Jog~~ George shot, he declared that he did not believe it. To me the authorship in a paper or in a report does not mean a question of priority, it means a question of responsibility. If you have signed the paper, you should stand up for it, or if you don't stand up for it you should tell why you have changed your mind. And that to my knowledge Ulam never did. In the famous Princeton meeting where Bradbury tried to suppress the mention of this new idea I carried the ball alone. After the meeting, as is known to everybody, the decision has been made. The Chairman of the Atomic Energy Commission, who listened without listening, has later said, "Teller went to the blackboard and out of his own head produced this remarkable invention." The invention that I told him two months earlier. This to my mind is the story how in the United States the idea and the execution followed each other.

I might add, perhaps, one little point that should be of interest, particularly to people in Los Alamos. Regarding a student of Fermi's, very young at that time, came to visit in the summer in 1951 a short time after ~~Jog~~ George shot. I told him about all these things and I asked him to put down a concrete design with dimensions for the (concept?) and I told him that I did not want a deliverable weapon I want a proof of principle and I want it so hard that there should be the least possible doubt about it, because as a consequence of the hydrogen bomb controversy it was not clear at all if that first shot misfired that there ever would be a second shot. And if the Russians got it first we might have managed to deny that it meant anything. Now we are managing to deny that the Russians are ahead of us in military matters which to my mind is just as absurd.

So that first design was made by Dick Garwin, it was then criticized forward and backward, in the end it stood up to all criticism. The people who worked it out in detail were to my knowledge Marshall Rosenbluth and Conrad Longmire. I had left, I came back for a visit at Christmas and I found that the calculations came out just as I had expected, that the design remained unchanged. Bethe had come out and looked it over, and first tried to change it in some ways then gave in and said all right that will be a reasonable way to shoot it. And therefore, as far as I'm concerned the preparation for the hydrogen bomb was completed by Dick Garwin's design shortly after the famous meeting in Princeton.

The next interesting design that Los Alamos shot with great success was a lithium deuteride design which actually was worked out not in Los Alamos but by Johnny Wheeler, who had returned to Princeton, and in the first portion of the Matterhron project which in the meantime is defunct has made that design that Los Alamos has shot. There are quite a few people, ~~Klusky,~~ Kski Henry Hurwitz, my very good friend now Director or Chancellor of the Salk Institute, Freddy DHoffman, who wrote the first really detailed paper on the design, Johnny Von Neumann, Dick Garwin, they contributed. Ulam contributed by disproving the classical Super.

There is of course a post-script and this is unfinished business. Many years later some of my friends in Livermore and this time I did not mention the wrong name, and I don't even know who they are (and I'll try to get it and complete this record) found that both Ulam and Johnny Von Neumann were wrong. They, particularly Johnny Von Neumann, did the best job that could be done with computers at that time. Ulam's was more crude, it had to be because it was a hand calculation. The new calculations, and these old calculations were then very carefully repeated by Foster Evans Evers(?) and Cerda Evans Evers(???) and others and they all verified that the classical Super could not be done. The only difference why it worked in Livermore and did not work in Los Alamos was that we had better computers and therefore could zone more ~~findings.~~ finely. The obvious answer which was not obvious to my knowledge to anybody until the calculations were completed) The obvious answer is this, the inverse Compton effect does not become deadly because the photons instead of escaping sidewise can escape forward and backward. While the cylinder can be quite thick the detonation range remains thin. A point which could not be brought out without fine zoning. Even so, the calculations show that quite thick cylinders don't work and the work of the Super is indeed touch and go. That it works has been in the meantime verified, not only by calculations but by a reduced scale Livermore experiment in which somewhat compressed deuterium was used. The full-scale classical Super may yet work and I hope to God that the Russians don't get it first!

Keyworth--One question, the calculations that Von Neumann did, and before that Ulam did more crudely, were nothing but calculations to prove whether the runaway Super would propagate. They in no way addressed how the ignition occurred. Correct?

Teller--That I think is the case. They ignited with plenty of energy and I think there was no debate that we could ignite with plenty of energy because afterall we knew that with plenty of tritium we could have a low-ignition temperature and by spending enough tritium it was obvious that we could ignite whatever we wanted to ignite if only the propagation would then be maintained.

Keyworth--The mechanism of radiation coupling or hydrodynamic never arose?

Teller--It arose but it was rightly considered a solved problem, so it did not arise in a controversial sense.

/cm 12/20/79