

Long history

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If I may I will ~~not~~ start my account of the Chicago project at the point where I left off in the conversation I had with you about a year ago in Washington. <sup>let me</sup> ~~I have to~~ remind you that when the Chicago project was set up at the beginning of last year it was not intended that it should extend its activities into the field of designing power units. This part of the work was supposed to be handed over to Murphree and the Standard Oil Company of New Jersey. When I saw you in Washington I told you that there was a group of Standard Oil engineers at that time at Chicago who were trying to design a helium cooled power unit. I expressed to you my doubt that that group would be able to design a unit which was suitable for our purpose that I told you that I thought it more likely that Mr. Wigner would come forward with a scheme that might prove practicable. I will not repeat the account of the troubles which I gave you at that time. I had the impression that you listened sympathetically to my account, but ~~that~~ it also became clear from the conversation that you did not feel free to discuss with me the reorganization of the project which was then impending and so ~~it remained for me only to take note~~ <sup>I look</sup> ~~with regret~~ of the fact that your interpretation of the terms of secrecy which are imposed upon you did not ~~make~~ at the time make it possible for you to have a really <sup>useful</sup> ~~truthful~~ discussion of the question of how this work ought to be organized. With your permission I wish ~~now~~ to relate the sequel of the subsequent events at Chicago.

Rather soon after my conversation with you we learned that the War Department had been given the responsibility of building pilot plants and production plants. It was clear that it would be necessary to find one contractor or several contractors and we had many discussions in which we considered the suitability of various firms for this purpose. While these discussions were still going on the War Department placed a contract with Stone and Webster over the head of Dr. Compton. This was not the fault of General Groves who at that time was not yet concerned with our work and I do not know whose fault it was. We were supposed to design and build a power unit as a joint enterprise of the Chicago laboratory and Stone and Webster under the general supervision of the Army engineers. It was obvious to every man in this laboratory that this could not be done.

I immediately drafted a letter addressed to you dated July 3 which I am sorry to say I never sent, but which is in my files. In this letter I wrote "Dr. Compton reported on his return to a gathering of the group leaders of the Metallurgical Laboratory last Saturday, June 27, 1942. His report was followed by a discussion, but E. P. Wigner, E. Fermi, S. K. Allison refrained from making any comment. I thought it best to keep silent also. Subsequently I obtained privately an expression of opinion of all those within our group who have shown foresight in the past.

"To explain why our work would be slow if the proposed pattern were adopted would require a detailed presentation of the nature, scope and above all the complexity of our task. I should be glad to furnish such a description if desired."

This I believe is an example where there was complete unanimity in the laboratory to the effect that the proposed collaboration with Stone and Webster could not succeed. Anyone who would have taken the trouble to make inquiries regarding the available engineering staff of Stone and Webster would of necessity have had to come to that conclusion. Nevertheless it took ~~a number of months~~ <sup>October until</sup> until this point of view received official recognition and I am afraid those <sup>intervening</sup> months have to be considered as wasted <sup>from the point of view of obtaining a workable engineering design.</sup>

In the meantime work went on on building the powerless unit on the campus at the University of Chicago. The fact that in the first half of 1942 it was not Compton's responsibility to provide the materials for this unit made this work very difficult and I believe it is correct to say that we succeeded by the skin of our teeth. This success was due to <sup>a number of</sup> ~~three~~ lucky circumstances: First of all it so happened that Dr. Compton ~~was~~ knew personally the Mallinckrodt family and was thus able to arrange with them a rather unusual but very successful method for purifying the uranium on an industrial scale. Secondly, it so happened that I found through ~~entirely~~ <sup>unauthorized and uncalled for</sup> inquiries and some simple experiments <sup>made by Brush Be Co</sup> that commercial magnesium is a suitable <sup>reducing</sup> agent for <sup>producing</sup> ~~reducing~~ uranium and that this available material <sup>can</sup> ~~can~~ therefore be used in place of distilled calcium which is not available.

Thirdly, Dr. Spedding, a college professor, developed a successful industrial method for <sup>producing</sup> ~~reducing~~ uranium by magnesium and by turning his laboratory <sup>into</sup> ~~over~~ to a factory in an amazingly short time reached the production of two tons per day. He thus not only provided us the material which we needed, but also demonstrated that in the case of new processes professors can

successfully compete with industrial corporations even in purely manufacturing processes. After long discussions in the laboratory it was decided that a second power unit should be built in the Argon forest near Chicago primarily for the purpose of providing the chemists with a moderate quantity of the product. Everybody in the laboratory with the possible exception of myself was strongly in favor of this program. The construction had already started and the separation was scheduled to go into ~~production~~ action in May, 1943. In September 1942, at the time of the preparations for the experimental unit in Chicago had gone a long way, we were informed that against the advice of Dr. Compton it was decided not to build the chemical separation plant in the Argon forest but to build a chemical separation plant at X. I understand that at present there is hope that the chemical separation plant at X may be in operation in October and so the delay due to the shift in plans may not exceed five months. I may again add that the decision of this shift, if it was a mistake as most people in Chicago thought it was, must not be laid to General Groves, since General Groves did not take part in it.

General Groves first appeared in Chicago at the end of September. He took the stand that all three cooling systems which were at that time under consideration (helium cooling worked on by our engineering staff, water cooling which was considered by Wigner who had begun by that time to make some designs with the help of a single engineer, and liquid metal cooling for which I tried to get interest and support) should be developed into the process design stage, but added the restriction that only those systems should be given full support

which could be in operation for production soon enough to yield usable quantities of the product in the spring of 1944. In my opinion this restriction would have ruled out all three systems, or if you wish would have favored the one whose supporters are most willing to tell the biggest lies. <sup>Mr</sup> For a short while there was an intention to place Moore of Standard Oil in charge of the developing all the three cooling systems, ~~a procedure which was strongly opposed by all physicists in the laboratory,~~ and shortly afterwards General Groves decided to try to place a contract with the DuPont Company for building a power unit. Between November 2 and 6th a large group of Du Pont engineers visited the laboratory. The helium cooled system was explained to them in great detail and half an hour was devoted to discussing with them the water cooled and the metal cooled systems. This group of engineers came to the conclusion that the helium system is the best, next they placed the ~~liquid~~ ~~xxxxxxxxxxxx~~ homogeneous heavy water system, third they placed the liquid metal system, and fourth they placed the water cooled system which was sponsored by Dr. Wigner. Somewhat later we heard that DuPont felt there was only one possible probability of successfully building a power unit and that they wanted this to be understood in case they should be willing to accept a contract. Thereupon General Groves appointed another committee headed by Lewis of M. I. T., having as a member Murphree, I believe, of Standard Oil (I am not quite sure where my memory is correct on this point). All the other members of the committee were employees of DuPont. We were told on November 19th, 1942 of the appointment of this committee and were asked to have a report ready by November 23rd. The report

was to include three pages about the water cooled system and three pages about the liquid metal cooling. Mr. Wigner and I were not heard by the committee and so these three pages were all the information to which the committee had access. Shortly afterwards we heard that DuPont was now satisfied that they could build a helium cooled power unit and on that basis was favorably considering the acceptance of a contract from the government.

I myself was privately convinced that DuPont would not be able to construct the helium cooled power unit in time to be of use in this war, but felt completely powerless to do anything about it except seriously considering to return to the peaceful pursuit of physics. Mr. Wigner on the other hand continued his work on the water cooled power unit and submitted a detailed report to DuPont. During the second half of January I heard that Wigner's system was being seriously considered by DuPont. On February 8th Wigner urged very strongly that both types of cooling, helium and water, should be built. Soon afterwards we learned that DuPont had abandoned the idea of building the helium cooled power unit and had now decided to build a water cooled power unit along the lines of Wigner's design. The whole laboratory was anxious to cooperate in this work and an offer to send Mr. Fermi to Wilmington to help in this work was politely refused. An offer of Wigner and his group to move to Wilmington and collaborate in developing further their original design was declined.

Numerous changes which were thought to be improvements were introduced by DuPont into Wigner's design, but by the middle of June of this year, after four months of designing work, with few exceptions these changes have again been withdrawn so that

the design at present is almost exactly identical with the design submitted by Wigner and his group.

I should perhaps add that this is, at least in peace time, the usual way things go and that we must not hold it against DuPont if they go through the routine to which they are accustomed since this may be the only way in which they are able to operate.

A very serious mistake was however made as far as I can ascertain without the approval of any of the key men of the Chicago laboratory when DuPont was authorized to build an air cooled unit producing 1,000 kw in conjunction with the separation plant at X. This air cooled unit cannot be considered as a pilot plant for the production plant at W, which is water cooled and which is scheduled to produce power 250 times larger than the air cooled pile at X. A jump of 250 is far too big. It is too big for the chemical separation plant and it is too big for the power unit. The correct procedure as far as I can see advocated by everybody in the laboratory would have been to ~~design~~ decide in January to build a 10,000 to 20,000 water cooled power unit at X in conjunction with the separation unit. Why this was not done I am unable to say. DuPont decided to build a production plant at an exceedingly remote site at W, the remoteness partly being due to the necessity of having cold and pure water and partly on an irrational fear based on insufficient familiarity with the dangers involved. They are afraid the plant might blow up and loss of life might result unless there is a great distance between the plant and the nearest

inhabited locality. Clearly the selection of such a remote site will lead to great delays ~~xxxx~~ since it will be necessary practically to build a complete town with laundries and other accessories and to organize a supply of goods which are readily available in less remote localities. Clearly these delays may mean a much bigger loss of life if considered in its bearing on the duration and possibly on the outcome of the war than the possible loss of life in neighboring villages might mean even assuming that the danger is real and not imaginary.

The independence of a cold and pure water made it appear to me very attractive to consider the metal cooled system. I therefore asked Dr. Compton after the supposed adoption of the helium cooled system at the end of last year for the reasons which induced the committee which visited ~~xxxx~~ us in November of last year for refraining from scrutinizing the system more closely. In the absence of any permanent board of experts to whom such questions could be turned over, all Dr. Compton could do was to ask Dr. Greenwald from DuPont to see me and to give me those reasons. Having heard them I still went on believing that the metal cooled system ought to be scrutinized though of course I am quite unwilling to say anything about the relative merits of the various systems at this junction. The point which I wish to make is that there is no permanent board of experts representing the government to whom the scientists could turn for a fair hearing on any technical issue which may be of a controversial nature.