First rough draft

PROPOSED CONVERSATION WITH BUSH

February 28, 1944
L. Szilard

PART I

The Government sponsorship of the uranium work started over four years ago in October 1939 by the appointment of Dr. Briggs' committee by the President. At that time we were far ahead of other countries in knowledge, but this initial advantage we have presumably lost since that time. The work of the scientists was crippled from the start by a mistaken attitude on the part of the administrators toward the scientists upon whose discoveries and inventions all this work rests. Subsequently the same attitude was manifested toward most of the other competent scientists who joined this work later. A number of reorganizations took place and there were changes both in the agency and in the persons who were entrusted with the responsibility of administering this work but the attitude toward the scientists remained the same. There can be no substantial improvement unless there is a complete change of heart in this respect.

1. The worst consequence of this attitude and one from which many evils have derived is the fact that it is made impossible for the scientists (who are giving their full time and attention to various aspects of the uranium work) to have an adequate discussion with each other of the pertinent issues. It is therefore difficult for them to form a well-founded opinion and even if they individually arrive at definite opinions they are not able to put collective recommendations on record.

A direct consequence of this is that there can be no judgment on which the administration can base sound decisions on issues which often

involve the expenditure of hundreds of millions of dollars. Most of the decisions made are based on false premises. Decisions are often clearly recognized as mistakes by the competent scientists at the time when the decision was taken but they are not officially recognized as mistakes until four or eight months later, at which time the situation is in some cases beyond remedy. This point will be illustrated by representative examples taken from the past record.

Other unsatisfactory conditions which arise out of the same general attitude towards the scientists which can be stated in general terms here are as follows:

2. From October 1939 to December 1941 inadequate financial support and complete frustration of the scientists in their attempts to make arrangements with industrial firms for the technological development which was considered by them a prerequisite of the industrial stage of the development. This affected the work of Dr. Urey on the separation of isotopes as well as the work of Dr. Fermi and myself on the chain reaction in unseparated uranium.

3. From January 1942 on there was adequate financial support for this work but as far as the Chicago Laboratory is concerned, the Laboratory was not given the authority to make arrangements for obtaining materials which the scientists considered necessary for their work. This nearly wrecked the Chicago project. We got the required materials in 1942 only through a series of exceedingly lucky circumstances which we encountered when we finally tried to make arrangements for these materials ourselves.

4. The free interchange of views between different groups working on uranium along related lines was prohibited beginning the fall of 1940 and as a consequence of this interference with the work the contact between Fermi and myself on the one hand, working on the fission aspects at Columbia, and Dr. Urey on the other hand, working on the separation aspect at Columbia, became inadequate. Consequently it was not realized by us, though we had performed the pertinent nuclear measurements, that atomic bombs of small weight could be constructed from the separated isotope. This fact might not have come to the knowledge of the United States Government at all if it had not been brought to its attention in the middle of 1941 by the British Government. Fortunately a handful of British physicists who were not subject to compartmentalization of information ~~and~~ were free to discuss with each other the interrelationship of the isotope separation and the nuclear physics program and consequently saw that small size atomic bombs could be constructed from the separated isotope.

At present there are two methods for separating the uranium isotope in the industrial stage. One of these (K-25) is using the diffusion of uranium hexafluoride; the other (Y-12) is using a magnetic separation. Both of these methods are clumsy methods. The first one may not even succeed. The second may have a good chance of success, but it is very costly in time, materials, and skilled labor. It is more likely than not that if the competent men who are at present working for the Government on various aspects of the uranium problem had been given enough information to encourage them to think up better methods for separating the uranium isotope we would by now have much faster, simpler, and cheaper methods. This must not be interpreted as saying that the men who originated the

present methods lack originality or ingenuity, but simply to say that there is no way of telling beforehand what man is likely to discover and invent a new method which will make the old methods obsolete. The only thing we can do in order to play safe is to encourage sufficiently large groups of scientists to think along those lines and to give them all the basic facts which they need to be encouraged to such activity. This was not done in the past and it is not being done at present.

It is of course not possible to indicate what methods would have been discovered or invented if compartmentalization of information had not interfered with this work. Only when compartmentalization will be lifted will it be seen what rapid succession of new inventions and improvements will emerge or, alternatively, we shall see what we have missed should it turn out that the German physicists, though they are few in number and not superior in inventive ability, have produced large quantities of the uranium isotope by some method vastly superior to the present forms of K-25 or Y-12. It is, however, possible to give, on the basis of the past record, representative examples to show the loss of time.

5. Another consequence of this situation is that certain large firms, particularly du Pont, are acting in a double capacity, both as contractors and as advisors of the War Department.

So far the work intimately connected with the chain reaction in unseparated uranium has been exclusively concentrated in the hands of du Pont. The engineering staff of du Pont is of course limited both in number and in men with sufficient theoretical background. Consequently they are not adequate to handle <u>every</u> problem that the scientists consider necessary to follow up. In two <u>important</u> instances of this type the

scientists have attempted to obtain permission to cooperate with some other firm in order to push the development of certain alternative methods at least into the process design stage. In both cases this permission was refused.

There was an increasing tendency of regulating the work of scientists by means of directives, some of which are clearly based on false premises. While clearly a large fraction of the effort of the scientists has to be regulated by some sort of directives the success of the work is at stake if sweeping directives make it impossible to have 10 to 20% of the scientific staff pursue lines of development which the scientists themselves consider essential.

For instance, we were directed in September 1942 to refrain from developing such cooling systems which could not be put in the industrial stage at such an early date as to give usable quantities of the product by the spring of 1944. If this directive and the accompanying "executive order" had not been disobeyed, the water cooled system which is now being used would not have been available to du Pont. The same directive, however, succeeded in suppressing the development of the bismuth cooled system.

The situation is getting increasingly worse in this respect and there can be no improvement until the charter is adopted freeing a certain percentage of the effort from the effect of constantly shifting directives.

As a result of all this the present state of the uranium work may be described as follows:

A. The Field of Unseparated Uranium.--A water cooled production plant is being built at a cost of about $300 million by the du Pont Company.

This plant is supposed to go into production on June 1, 1944. The decision to build a water cooled production plant was made by the du Pont Company in January 1943 on the basis of a process design which was developed in the Chicago Laboratory under Dr. Wigner during the year 1942. This line of development was suppressed during the year 1942 in favor of the development of a helium cooled power unit (Dr. Wigner had the support of only one engineer for his work) and consequently there were many uncertain points that had not been previously investigated, Mr. Wigner and all competent scientists in the Chicago Laboratory were therefore of the opinion that a water-cooled pilot plant of 10,000 or 20,000 kw should be built immediately. This was not done and three production units scheduled to work at 250,000 kw are thus being built with no other basis than experience based on a 1,000 kw air cooled plant. As it is there is hardly more than a fifty-fifty chance that the 250,000 kw production units will stand up to operation for a reasonable period of time.

Even if they had been designed under more favorable conditions the water cooled system would have its hazards and could not have been made safe from the operational point of view. The choice of the water cooled system was nevertheless welcomed by the scientists because they believed that this system could be constructed and put in operation at an early date and at comparatively low cost and it seemed worth while to sacrifice operational safety for speed. After all, safety first did not seem a very sound principle in January 1943 with the outcome of the war hanging in the balance. However, what followed was a vain attempt at attaining operational safety (which is not attainable under this system) and

the adoption for the sake of that illusory safety, of a scheme which is exceedingly costly in men and materials. Since money can be spent only at a certain rate, this meant slow pace of development. The result is a combination of great loss of time and no appreciable gain in safety.

The situation would have been more satisfactory if at least a fraction of our efforts could have gone into developing an alternative scheme at least into the stage of process design. Two such schemes had been proposed: one was a graphite-uranium power unit, cooled by a liquid bismuth-lead alloy; the other was a design of a heavy water power unit. It was not possible to obtain authorization for following up either of these alternative lines.

B. Separation of Isotopes.--

(a) K-25, based on the diffusion of uranium hexafluoride, was transferred into the industrial stage and taken out from under Urey's supervision in 1942. This was done at a time when no pilot plant was in existence and it is said that at the time when the development was taken out of Urey's hands and placed in the hands of the Kellogg Company, there was not even an experimental unit in existence in which the diffusion method had actually been tested on the gas (uranium hexafluoride) to which it was to be applied. It is doubted that this system in its present industrial form has more than a fifty-fifty chance for success.

(b) In the fall of 1942 of the various methods for producing heavy water which were developed under Urey's supervision, one was chosen by du Pont and put into industrial production. The scientists in Urey's laboratory were of the opinion that the process chosen by du Pont was much

more costly and inefficient from the point of view of coal consumption than another method. Since it is, of course, the privilege of du Pont to decide for themselves which method they believe they could successfully put in the industrial stage, there could have been no objection on the part of the scientists to the placing of the contract with du Pont, but it was the desire of the scientists to develop the alternative method, which they estimated required three to four times smaller quantity of coal, at least into the process design stage. It was impossible, however, to obtain permission to collaborate with any firm other than du Pont in the development of this alternative method.

As a result of this our heavy water production does not exceed three tons a month and consumes 30,000 tons of coal per ton. ~~per month~~. The quantity of heavy water produced is not sufficient as a basis of adequate production of U^{233} or as a second line of defense for the production of plutonium. The production is expensive and we have no alternative method ready on which to fall back if larger quantity of heavy water is needed or if economic conditions should compel us to discontinue the inefficient process used by du Pont.

(c) The electromegnetic method in its present form may very well be successful but it will require enormous expenditures of materials and skilled labor in order to supply the required quantities of the separated isotope. It is believed that on the present industrial scale the method will supply only a fraction of what is needed to win the war by this weapon. Even at this late stage, new inventions in the field of the electromagnetic method might bring about favorable changes in the situation but

the competent physicists working on uranium in branches other than the special project of Lawrence are not supplied with information which is required in order to be encouraged to think up something new in this field.

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PART II

THE EXCLUSION PRINCIPLE

A general principle seems to have consistently been applied to the conduct of the uranium work. This principle consists in not giving authority to act or authority officially to advise to men who have a direct knowledge of the intricate problems involved. It is preferred to give such authority to men who have no such direct knowledge and who know only what they are told about our problems. If one knows only what one is told, one does not know enough to be able to arrive at a well-balanced decision.

While this general principle emanated from the Washington end of our organization it has from there also diffused into the organization of the Metallurgical Laboratory. Essential aspects of the history of the work at Chicago cannot be understood except in the light of this general principle and the following two sections describing the material procurement and the designing work at the Metallurgical Laboratory in 1942 serve as an illustration of this principle.

Material Procurement in 1942

At the end of 1941 a reorganization took place which was carried through by Dr. Bush and the Metallurgical Laboratory was set up in Chicago under the direction of Dr. Compton. When it became known that Dr. Compton was not given authority to make arrangements for procuring the materials which were needed for his project but that this authority was given to

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Dr. Murphree, it was clearly realized by the scientists that this division of authority might wreck the Chicago project. There was a unanimous opinion in this respect which was voiced at a meeting held under the chairmanship of Dr. Compton at Columbia University. However, this opinion was neither recorded nor communicated to Dr. Bush. Dr. Compton later on wrote and asked that such authority be given to him but it was not evident from his letter that his opinion was unanimously shared by all those who were competent to judge. The only way through which Dr. Bush gradually learned of this unsatisfactory situation was through individual conversations with Creutz, Wigner and Szilard, a rather irregular procedure which cannot as a rule be relied on in the conduct of such projects.

When it was finally recognized that this division of authority was harmful, attempts were made to remedy the situation. That these attempts were successful is due only to a series of very lucky circumstances which as a rule cannot be counted upon: One was personal friendship between Dr. Compton and the Mallinckrodt family through which Dr. Compton was able to arrange for a supply of chemically pure uranium oxide based on an ether purification of the uranium nitrate on an industrial scale (originally proposed by Dr. Creutz). When the metal situation became desperate owing to the unsuccessful attempts of producing pure metal on a large scale by means of calcium reduction, I found the Brush Beryllium Company willing to make a few simple experiments and try to reduce uranium tetrafluoride by magnesium. I was not encouraged to make these arrangements, but through great luck the first few experiments already showed that pure uranium metal could be thus obtained by using commercial magnesium as a reducing agent. Since this result was obtained before the

private arrangement with the Brush Beryllium Company was interfered with, it became possible for me to take a strong stand in favor of magnesium reduction. Finally, Dr. Spedding, a college professor, developed the magnesium reduction into a satisfactory industrial process and, by turning his laboratory into a factory, soon had the production up to [two tons] of metal per day and so saved the situation.

Designing work in 1942

By May 1942 Dr. Compton officially took the position that the chain reaction would work in the graphite uranium system. Within two days after the pure materials were available we had a chain reacting pile (December 2, 1942).

From May 1942 it was considered by many of the scientists at the Chicago Laboratory that our main task lies now in making a design for a chain reacting power unit which could dissipate several hundred thousand kw. Mainly due to outside influence, the official support of the laboratory went to an engineering group headed by Mr. Moore (recommended by Mr. Murphree) and this group decided to design a helium cooled power unit. The scientists were anxious that other methods of cooling should be developed at least into the process design stage. There were two such other cooling systems proposed--water cooling sponsored by Mr. Wigner, and cooling by bismuth or bismuth-lead alloy sponsored by me. Mr. Wigner found it difficult to get any support for developing the water cooled design but finally succeeded in having one engineer assigned to him. No such support was given to the development of the bismuth cooled system.

At first there was a committee called "engineering council"

officially entrusted with supervising the engineering developments in Chicago. This Council had weekly meetings, but this council did not invite Mr. Wigner to attend these meetings. I made repeated representations about this point but they were not successful, though I went to the length of going on record by writing a memo about it.

Shortly after Thanksgiving 1942 the War Department placed a contract with du Pont for building an industrial plant. At the time when this contract was placed it was assumed that a helium cooled plant would be built.

Many scientists had doubts as to the wisdom of this decision since it did not seem likely that the large turbo compressors needed for the helium cooled plant could be obtained in a reasonably short period of time and the reciprocating compressors which was supposed to be obtainable did not seem to be suitable.

In January 1943 Mr. Wigner submitted the design of his group for a water cooled plant and in February 1943 the du Pont decided in favor of the water cooled system. This decision was welcomed by the scientists because they felt that the water cooled system, though it is far from being a safe system, might be built in a very short time and could yield, with luck, large quantities of the product at an early date.

REVIEW OF PAST DECISIONS

Since the government was unable to utilize the best advice which the scientists could supply, it was compelled to use some kind of a substitute and various substitutes were tried in the course of time during which the mechanism for bringing about decisions was repeatedly changed. I shall describe in the following a few of these mechanisms in order to

show how they functioned, what decisions they have brought forth, and whether or not the decisions were in accord with the opinion prevailing among the scientists.

Contract with Stone and Webster

In July 1942 we were informed that the War Department was partly taking over the responsibility for our work. Soon afterwards we were told that we were supposed to build an experimental plant and a pilot plant for the production of plutonium in collaboration with Stone and Webster under the general supervision of the Army engineers. As far as we could see the choice of Stone and Webster was made over the head of Dr. Compton. There was a practically unanimous opinion among the scientists who knew what kind of engineering staff that could be made available by Stone and Webster for this work that the choice of Stone and Webster and the form of collaboration proposed was a mistake and could not possibly succeed. This conviction was freely and universally voiced in the Laboratory. It took four months until it was officially recognized that Stone and Webster was incapable of carrying out the task and thus four months were lost from the point of view of the industrial development.

Decisions by Executive Committee

This committee contained one man from each special project, i.e., the project leader, namely, Urey, Compton, and Lawrence. They were the only members of the committee who gave their full time and attention to the uranium work and the other members of the committee had no intimate contact with the scientific staff of the projects. This committee made recommendations on important issues.

It is difficult to say exactly what the recommendations of such

a committee represent. Clearly Urey, Compton and Lawrence were the only members of the committee who knew enough of the details to be able to weigh most of the pertinent factors but even they, since they were exceedingly busy with the administration of their own projects, had only a limited knowledge of each other projects. Moreover, none of them could push his own project in such a committee without putting himself into an awkward position and each of them had to lean backward if they had to give their opinion concerning one of the projects which competed with their own project.

This committee made, in the course of 1942, quite important decisions concerning which side-lines should be pushed and which side-lines should be dropped and the scientists had always an uneasy feeling concerning the wisdom of these decisions. Many believed that in this committee Dr. Conant's views always prevailed and so the committee's decisions were mostly regarded as Dr. Conant's decisions.

Dr. Conant at that time was far from being able to give his full time and attention to that matter and it ought to be generally recognized that no one who devotes only part of his time to these very complex questions can hope to have as good a chance to take the right decision as men who may be less capable but who devote all their attention to the uranium work.

According to the official schedule, construction was to be started in September 1942 on the experimental power unit of 1000 kw in the neighborhood of Chicago which was supposed to go into operation in May 1943 and which was to have the main purpose of supplying the chemists with 1 gram of plutonium per day. This schedule was suddenly upset by the executive committee in September 1942. We were told that the experimental plant would not be built near Chicago but in the Tennessee Valley and the reason given was that the experimental power unit ought to be built at the same place where the production plant would be built. Since a helium cooled

production plant would have to be built in the Tennessee Valley where there is sufficient power available, the experimental plant would be built there also. There was a unanimous opinion in the Chicago Laboratory that this decision was a mistake and it almost came to an open revolt.

The experimental power unit was nevertheless built in the Tennessee Valley and did not start operation until November 1943, which represents a loss of five months. The scientific staff had to be split between Chicago and the Tennessee Valley with Dr. Compton commuting between the two places. No production units are in fact being built in the Tennessee Valley, so that the decision was based on a false premise.

(Incidentally, the question of where the experimental plant ought to be built was reconsidered in December 1942. At that time it was the War Department's desire to have the plant built in the Tennessee Valley. It was doubted by many scientists that the reasons given were valid.)

Decisions by ad hoc appointed committees

Our first experience with an ad hoc appointed committee was as follows: In the fall of 1942 General Groves decided to try to place a contract with du Pont to build a power unit. Between November 2nd 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 metal bismuth cooled systems. This group of engineers came to the conclusion that the helium system was the best, next they placed the homogeneous heavy water system, third they placed the bismuth cooled system, and fourth they placed the water cooled system ~~which was sponsored by Dr. Wigner~~. Somewhat later we heard that du Pont felt there was only one chance in a hundred for successfully building a power unit and that they wanted this to be under-

stood 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 Mr. Murphree I believe, of Standard Oil of New Jersey (I am not quite sure whether my memory is correct on this point). All the other members of the committee were employees of du Pont. We were told on November 19, 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 bismuth cooling but no more. Shortly afterwards we heard that du Pont 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.

After a contract was placed with du Pont Mr. Wigner submitted a scheme for a water cooled power unit in the form of a detailed report. During the second half of January I heard that this system was seriously being considered by du Pont who by that time discovered the difficulties of the helium system, of which the competent men at Chicago had known and written long before. About the middle of February du Pont abandoned the idea of building a helium cooled power unit and had decided to build three water cooled power units of 250,000 kw each along the lines of Mr. Wigner's design. Mr. Wigner urged that in view of the uncertainties of the water cooled system an alternative design should be carried out at least into the process design stage but no such action was taken.

Decision as to Site

In the assumption that a helium cooled power unit would be built a remote site in the Pacific Northwest was selected because of the availability of large quantities of power that is required by the helium cooled

system. What is actually being built at this remote site now is a water cooled production unit which should not require great quantities of electric power. By a lucky coincidence, water of ~~fairly~~ good quality was available at that site so that the false selection of the site did not cause damage in this respect. However, the difficulty of obtaining labor in that area ~~and the expected loss of secrecy owing to a conspicuous construction within reach of Seattle~~ might have ruled out that site had not the false assumption of power requirement induced its acceptance.

Other Decisions by ad hoc Appointed Committees

1. The same or a very similar ad hoc appointed committee looked, about Thanksgiving 1942, into proposals presented by Dr. Urey and Dr. Lawrence and others. There is no reason to believe that the method of their investigation was different from that exhibited in Chicago. On the basis of their recommendation contracts were placed with the Kellog Company and the Union Carbon and Carbide Company for the industrial development of the uranium hexafluoride diffusion method. This development was taken at once out of the Laboratory stage and pushed into the industrial stage at a time when there had been not only no pilot plant in operation, but not even an experimental unit in existence which demonstrated the feasibility of the diffusion method with the gas (uranium hexafluoride), to which the diffusion method had actually to be applied.

Decision of unknown origin

2. In February 1943 we heard that an air cooled experimental plant of 1,000 kw would be built in the Tennessee Valley. The men whom I contacted in the laboratory did not know how this decision came about. It was everybody's opinion that since du Pont decided to build and in-

dustrial plant based on a water cooled system, a water cooled pilot plant of 10,000 or 20,000 kw ought to be built with the utmost speed in place of an air cooled plant of ten times lower capacity. Competent men in the Chicago Laboratory were quite willing to take the responsibility of building this pilot plant or collaborate with du Pont if du Pont cared to build a pilot plant. No pilot plant was built and a $300 million industrial plant is now supposed to go into operation in June 1944 without the benefit of ever having built and operated a pilot plant.

COLLABORATION CHICAGO - DU PONT

The question-answer game

I wish to describe now what is commonly called, in the Chicago Laboratory, the "question-answer" game. Take for instance the Northwest Pacific construction which will swallow 3/4 million cubic meters of concrete and require the construction of 180 miles of roads and 160 miles of railroad. In this sprawling power plant the first three power units are spaced at 10 miles apart and it will probably be claimed, and in a sense it is probably true, that these large distances were chosen at the recommendations of the Chicago Laboratory. Such recommendations come about through the working of the question-answer game. We may be asked what would be a safe distance to place neighboring power units and if the question is put this way we are inclined to go out of our way and make the most pessimistic conceivable assumptions and then come out with 10 miles as a safe distance to be recommended. If instead of being faced with such a question we had to deal with the problem as a whole and were asked to suggest a reasonable compromise between maximum

possible safety and minimum waste of men and materials, which incidentally also coincides with minimum waste of time, the answer would come very different.

DuPont not able to Utilize our Staff

It is difficult to avoid this question-answer game if two organizations collaborate without integrating their staff into one unified group. When we learned that du Pont decided to drop the "favorite" and build a water cooled system, the whole Laboratory was anxious to cooperate in this work. This, however, did not prove to be feasible. An offer to send Mr. Fermi to Wilmington to help in this work was politely refused. An offer of Mr. Wigner and his group to move to Wilmington to collaborate and help in furthering this design was declined. The last attempt in this direction was made by H. Anderson who went to Wilmington in the assumption that his help was both needed and acceptable. The first assumption was right but the second was not and he is now back in Chicago.

Numerous changes which were throught to be improvements were introduced by du Pont into Wigner's design, but by the middle of June of this year after four months of designing, most of these changes have again been withdrawn so that the design at present is, with few exceptions, ~~almost exactly~~ identical with the original design submitted by Mr. Wigner and his group. This design, which was drawn up in a hurry, would be capable of quite substantial improvements which would make the chances for good operational safety considerably larger than that of the present design, though in the case of a water cooled system we could in no case be entirely sure in advance of successful operation.

In the circumstances the collaboration of the Chicago Laboratory in designing work is quite unsatisfactory. It is true that the blueprints of the construction are made available to the Chicago Laboratory, but the competent men in this Laboratory say that they are unable to see from these blueprints the whole story and therefore are unable to see whether, from a point of view of the knowledge accumulated in this Laboratory, one ought to expect the construction shown in the blueprints to function properly.

Present Status of the du Pont Design

Since the responsibility for this construction was taken out of the hands of the Chicago Laboratory early in 1943, work on a number of technological questions was stopped because it was understood that these questions were being handled by du Pont. These technological questions, however, had to be taken up again by the Laboratory at the end of 1943 when it turned out that they had not been solved by du Pont. One of them concerns the question of how to can individually 50,000 pieces of uranium in such a manner that they can be exposed to the cooling water without corroding. It is planned to use 50,000 Al cans for this purpose, but the details of this canning have not been solved up to date--just four months before the production plant is scheduled to go into operation. If only a small fraction of these cans leak at the welded edges or if there are such leaks due to corrosion or erosion of the Al can, water will penetrate to the uranium, the uranium will swell, and the production unit will cease to be usable.

The design chosen by the du Pont Company requires very narrow tolerances. A linear contraction or expansion of the graphite by 1 per mil will burst the construction. It is quite possible that such a charge

in density will occur within a few weeks of operation at 250,000 kw since we know that corresponding changes in electric resistivity and Young's modulus occur during the bombardment. Perhaps if the power unit can be allowed to run at a few hundred degrees such changes in density can be avoided, but whether or not this is the case is, at the moment, anybody's guess. We were forewarned by Wigner that such changes in density might occur and another construction which does not require such very narrow tolerances could have gone a long way toward avoiding potential destruction of the power unit by this effect.

Potential corrosion and erosion of the Al cans is one of the major weaknesses of the water cooled system. The cooling water may deposit a thin film on the Al cans thereby causing overheating and corrosion. In the absence of a pilot plant no satisfactory tests of this point could be performed under operating conditions and such tests as might have been performed in the Tennessee Valley were not performed either.

In spite of all this, we must hope now that through a series of lucky breaks the production units will operate at 250,000 kw each for a reasonable period of time, say three months, and if that should happen one might say that the foolhardy risks which were taken were justified.

Influence of the du Pont Company

Since in the present setup the Government cannot be guided by the advice of the scientists, it has to turn for advice to its contractors, who thus play a dual role as both contractors and advisors of the Government. This dual role of the contractors is severely criticized by the scientists who believe that it leads to the suppression of certain promising branches of this work.

So far all those lines of development which utilize the chain reaction and can be expected to survive the war are concentrated in the hands of the du Pont Company. The scientists have come to believe that the du Pont Company picks out certain lines of development for which it happens to be particularly well adapted by its existing equipment by by its available personnel and subsequently they successfully exert an influence in the direction of preventing the scientists from establishing a collaboration with other firms for the development of alternative lines of development (for which the du Pont Company is not suited either because it lacks equipment or because it lacks suitable personnel or because the men in charge of the du Pont Company lack the theoretical background to enable them to appreciate those alternative lines of development).

The first instance of keeping out competitors of du Pont occurred shortly after Thanksgiving 1942. At that time a contract was placed with the du Pont Company for producing heavy water by one of several processes which were proposed by Dr. Urey. The du Pont Company found one of these processes more suitable than the others from the point of view of their previous experience and from the point of view of their equipment. It is certainly the privilege of every company to choose the method which suits them best. Since, however, in the process adopted by du Pont a very large quantity of coal is required for the production of heavy water and since the quantity of heavy water produced by this process is necessarily too small to be of real significance for the production of plutonium, it appeared desirable to develop a more economical process. In the opinion of Urey and his collaborators such an alternative process was available and could be expected to be about three or four times as efficient with respect to coal consumption. In addition they were of the opinion that the investment costs would be considerably

lower for this alternative process. Naturally they were anxious to discuss this alternative process with some chemical firm from the production point of view. Their request for permission to do so was refused. The view that this refusal is justified because, in the interest of secrecy, firms other than du Pont must be kept out of the field of heavy water production is not shared by the scientists.

That the scientists are unable to obtain permission to collaborate along a major line in the field of unseparated uranium with a company other than du Pont has become a general conviction by January 1943. This conviction was further borne out by the experience encountered in connection with the development of a heavy water power unit. The history of the efforts of the scientists to develop a design for the heavy water power unit at Chicago is told in the next paragraph. It acquires particular significance because the events connected with it have led to a loss of faith in the management of the Washington end of our organization on the part of a very large number of scientists in the Chicago Laboratory.

DESIGN OF THE HEAVY WATER POWER UNIT

In April 1943 it was decided that a heavy water production unit should be designed and built as speedily as possible. In view of Mr. Wigner's past achievements in connection with the engineering development of the water cooled power unit he was asked to take charge of this work at the Chicago Laboratory. Mr. Wigner felt that if the Laboratory put forward a process design and later a contract for building this power unit were given to an industrial firm, that firm would want to re-design the unit and that a period of something like six months would thereby be lost. For this reason, he asked that the du Pont Company delegate a number

of their men to participate in the designing work so that redesigning could be avoided. When it turned out that the du Pont Company had no suitable men available at this time, Mr. Wigner asked to be permitted to approach a company other than the du Pont Company who could collaborate in the designing work and could take over the construction afterwards.

Few scientists in the Laboratory believed that Mr. Wigner's group would succeed in obtaining permission to collaborate with a company other than du Pont. However, in the meantime preparations for this work continued. A large number of men were brought over from Mr. Urey's project to the Chicago Laboratory and a number of men in the Chicago Laboratory were assigned to work under Mr. Wigner on this problem.

In the middle of August 1943 an ad hoc appointed committee under the chairmanship of Lewis came to Chicago and we were informed that this committee would make a final recommendation concerning the organization of the heavy water power unit work.

As a rule ad hoc appointed committees were found by us to be very unsatisfactory. Sometimes these committees are composed of an overwhelming number of men who are employed by a contractor of the Government. In other cases the composition of the committees may not be objectionable on that ground, but rarely is the majority of such a committee acquainted with our work and I know of no other case where the ad hoc appointed committee remained sufficiently long in action to give its members an opportunity to get acquainted with our problems.

In this particular case, however, two of the four members of the committee had previous contact with our work at Chicago. The committee went to great trouble to listen to all points which were brought up by the

members of the Chicago Laboratory and also heard Dr. Urey. For the first time the scientists felt that they had been heard by an unbiased ~~ad hoc appointed~~ committee.

The report of the committee was appreciated by the scientists. It recommended continuation of the designing work on the heavy water power unit and also recommended the immediate establishment of contact with the industrial company that would later be in charge of the construction work. They specifically named a number of industrial companies from which one ought to be chosen for this purpose. The list given by them did not include du Pont. Few men believed, however, that the recommendation of the committee would be carried out and many considered it more likely that no contract for the construction of the heavy water power unit would be placed rather than that the contract would be placed with a firm other than du Pont.

Soon afterwards it was pointed out to us that the heavy water construction was not very important after all, since it was only an insurance against a possible failure of the water cooled graphite pile, which in turn was only an insurance of the possible failure of the hexafluoride diffusion process, which is most likely to give large quantities of the product at an early date.

Next we learned that no contracts would be authorized for the construction of a heavy water power unit. The Chicago Laboratory was permitted to make a design and carry it into the process design stage but no permission was given to contact any of the firms recommended by the committee.

In the meantime, Mr. Wigner's authority in guiding the heavy

water designing work was seriously diminished and important responsibilities relating to this designing work were delegated to Mr. Vernon and others (Mr. Vernon is an engineer employed by du Pont who is at present assistant director of the Metallurgical Laboratory). Mr. Vernon put in a request for 80 chemists and engineers and another man put in a request for 50, that is, a total of 130 highly trained men. These requests were forwarded by the Chicago Laboratory without the approval of Mr. Wigner. Shortly afterwards we were informed of a new directive to the effect that all work on the design of a heavy water power unit had to be stopped. This happened in August 1943.

From April to August a large number of men in the Chicago Laboratory were giving their attention to the heavy water power unit and all these men were suddenly thrown out of their track. This sequel of events had an intensely demoralizing effect on those men.

The picture however would not be complete and somewhat unfair to the Washington end of our organization if I did not record one more significant fact. By the time we were informed that the heavy water designing work must be discontinued, Mr. Fermi and Mr. Wigner had reached the conclusion that the defects of the organization of the heavy water designing work in the Chicago Laboratory (which manifested themselves for example in exaggerated requests for highly trained men) were so serious that they would have endangered the success of the designing work. Both Mr. Wigner and Mr. Fermi told me that for that reason they did not wish to express any regret over the total discontinuance of the work.

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PART III

RULE BY DIRECTIVES

One of the great weaknesses of the administration of the uranium work consists in suppressing lines of development completely or almost completely which could be carried out with comparatively little effort and expense and which may play an important part in the future. Some of such previously suppressed lines of development have risen to prominence and salvaged some of the projects which otherwise would have failed.

Thus the water cooling suddenly became prominent when it turned out that the favorite, i.e., the helium cooled system, had to be dropped. The number of scientists and engineers working on the water cooled system was then quite suddenly raised from one engineer and a handful of physicists to an enormous staff of physicists, engineers, and chemists. This is an unbalanced way of carrying out development work.

It is in the nature of this work that when a new line of development is started a small sum of money goes a long way toward carrying forward such development and in many cases it would not be possible materially to accelerate the speed of development in the early stages by spending huge sums of money and putting a large number of scientists on the problem. The modest facilities needed for the early stages of new lines should not be withheld.

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The rule by directives is based on the assumption that there are some men high above in our organization who possess infinite wisdom based on knowledge and foresight and who can therefore issue directives not only governing 90% of the effort of the uranium projects which would be acceptable, but governing 100% of this effort. If 10% of our effort would be removed from these constantly shifting directives it might easily turn out that this 10% of our efforts produced more results than the 90%.

One example for such directives illustrates this. In September 1942 we were informed that only such cooling systems should be given time and attention on an appreciable scale which could meet the basis of a production plant that would supply plutonium in quantities of military importance by the spring of 1944. This directive was based on the assumption that some of the isotopic separation projects could meet this requirement and on the assumption that the helium cooling could meet this requirement. The directive was clearly based on a false premise but it had a disturbing effect on our work. In line with this directive I abandoned certain plans for developing a bismuth cooled system, a system which I believed at that time could have been developed within a short time but not by spring 1944.

How such a rule by directives may work may be illustrated by considering a fortunately hypothetical situation. Had it been known, as it ought to have been known to the authorities, that the processes pursued in the Chicago Laboratory could not yield substantial quantities of plutonium before spring 1945 while some of the methods for separating the isotopes offered a good chance for the supply of their product at an early date, most probably a directive would have been issued shifting all the manpower of the Metallurgical Laboratory to the isotopic separation projects.

On the basis of purely formal thinking on which most of our directives are based, this decision would have been perfectly justified on the basis of what was known to those who had to make the decision.

This hypothetical situation is here described because it illustrates the dangers of running these projects by means of formal decisions on the part of men who know only what they are told about these projects and are not told everything because it is physically impossible to transmit knowledge in the form of a well-wrapped package.

In the past six months the chemists have been harrassed by directives perhaps even more than the physicists and it was the lack of understanding of the true nature of our problems which was more than anything else responsible for the resignation of Dr. Franck, the director of the chemistry division of the Metallurgical Laboratory.

Lack of Permanent Board of Experts

The scientists who feel the need of developing one or the other side line forseeing future needs and future difficulties and the possible or probable collapse of the favorite line, have nowhere to turn since there is no permanent board of experts before whom they can put their views. Ad hoc appointed bommittees would be a poor substitute for such a permanent board even if their recommendations were accepted by the authorities. Clearly only men who give their full time and attention to

these complex problems can have a balanced judgment and the confidence of the scientific workers in the various projects. I can give an example from personal experience of how the lack of such a board makes it impossible for the scientists to obtain a hearing in the following:

Since February 1940 I have been advocating building a graphite-uranium system power unit cooled with liquid bismuth or an alloy of liquid bismuth and lead. As more and more information became available this system appeared more and more satisfactory from a point of view of operational safety and from a point of view of the cost of the production plant in men and material. Though no valid objections were at any time raised against this system, the system never had any official support. When I was informed that the official committee which visited Chicago Thanksgiving 1942 decided against this system in favor of the helium cooled system, I asked Dr. Compton to be notified of the reasons which led to this decision. The best Dr. Compton could do to satisfy me was to arrange for an interview with Mr. Greenewalt of the du Pont Company who informed me that in the opinion of the committee the bismuth cooled system would require the use of uranium carbide and they felt that developing the production of uranium carbide would take quite a long time. It would have taken the work of perhaps three men for six weeks to demonstrate that uranium carbide is not needed in this system and that the objection is therefore based on false premises. However, by that time we already had the directive to concentrate the whole effort of the Metallurgical Laboratory on helping du Pont carry out the construction of the helium cooled power unit. It is this distinction between the whole effort and almost the whole effort which makes all the difference from the point of view of suppressing developments

which are needed for safeguarding these projects from possible failure.

Without a permanent board of experts to whom the scientists can go and who can raise their voice in favor of sanity, the course of this work remains hazardous and insecure. Whether some of the scientists ought to be used as members of such a board or whether it would be preferable to have such a board composed of others and gradually educate them to make them capable of fulfilling a useful function is a question which I wish to leave open at the present time.

THE SCIENTISTS WITHOUT REPRESENTATION

There is reason to believe that in the past in a surprisingly large number of cases of impending decisions there was in fact a practically unanimous opinion among the scientists who were competent to judge. This opinion found, however, no expression and has not become a matter of official record. In most other cases when there was a division of opinion the dissenting opinion represented a comparatively small minority.

If there had been a mechanism for putting this collective opinion on record it would have been difficult for the authorities who were responsible for taking far-reaching decisions to make the mistakes which were made because those in authority would have been faced with the choice of following the collective recommendation of the scientists or taking the full responsibility of going against a practically unanimous recommendation.

The importance of this question was recognized by us from the very beginning of this work and therefore as soon as we realized that the work on unseparated uranium might enable us to construct at an early date atomic bombs consisting of element 49 we pressed as hard as we could for

the official recognition of a group of competent scientists who could put their opinions on the record. Such a committee was at last appointed under the chairmanship of Dr. Urey in June 1940 and was to act in an advisory capacity to Dr. Briggs. The first meeting of this committee was, however, also its last meeting, since the group was immediately dissolved on the ground that it might lead to criticism if later at a Congressional investigation it should be found that Government funds were expended at the recommendation of this committee which included men who were not American citizens of long standing. It was said in order to avoid such criticism it was necessary to have a committee the membership of which was limited to American citizens of long standing.

(Incidentally, the committee recommended unanimously before it disbanded the allocation of the sum of about one-half million dollars for demonstrating by means of a large scale experiment the possibility of the chain reaction with unseparated uranium in a graphite uranium system. As a consequence of the dissolution of this committee Mr. Wigner wrote to Dr. Briggs and asked to be relieved of any further participation in the uranium work and he did not resume this work until about six months later. Others who did not go this far were nevertheless deeply affected in their attitude towards this work.)

As time went on it became gradually clear that the attitude that first manifested itself with respect to the foreign born scientists extended to practically all competent men and at no time since the dissolution of the first committee of scientists was it possible to have a representation of the competent men who are giving their full time and attention to the uranium work. Such a group of scientists would of course have to cut across the borderlines of the different uranium projects in order to be able to perform a useful function and no such group can therefore function as long as compartmentalization within the uranium work is maintained. It will therefore be necessary to discuss whether or not this

compartmentalization of information is justified from the point of view of maintaining secrecy so that we may see whether the gain in secrecy, if any, can compensate for the very great damage which this compartmentalization causes. This point will be discussed in another section.

At present there are numerous scientists, mostly physicists and chemists, engaged in this work and among them a number of men whose record in this work during the past four years clearly shows that they are capable of foresight and balanced judgment. Most of the scientists engaged in this work have established positions at some university from which they have taken leave of absence. Here is a body of men whose full time and attention is devoted to this work. If they were permitted to discuss with each other freely impending decisions and if they could go collectively on record it would become apparent that the men who are competent are in a surprisingly large number of cases united in their opinion on the more important issues and that there is more often than not a strong majority and a comparatively small dissenting minority on most other issues.

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PART IV

PSYCHOLOGICAL SITUATION OF THE SCIENTISTS

In order to understand this situation one has also to understand the psychological reaction of the scientists which is a very important part of the general picture. The trouble started when, in June 1940, it became evident that it would be impossible to have a body who could speak in the name of the scientists who know most about this field. At that time nobody knew more about the potential possibilities of the chain reaction in uranium than Fermi, Wigner, and myself. At first when we were excluded from the committee which Dr. Urey tried to organize in June 1940 (on the ground that if money were appropriated on the recommendation of men who were not American citizens of long standing it might cause trouble in a later Congressional investigation) some of our colleagues argued that it would be possible for us to make our voices heard through the mouths of some of our American colleagues.

The attitude taken toward the foreign born scientists in the early stages of this work had far reaching consequences affecting the attitude towards the American born scientists. Once the general principle that authority and responsibility should be given to those who had the best knowledge and judgment is abandoned by discriminating against the foreign born scientists, it is not possible to uphold this principle with respect to American born scientists either. If authority is not given to the best man in the field there does not seem to be any compelling

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reason to give it to the second-best man and one may give it to the third- or fourth- or fifth-best men, whichever of them appears to be most agreeable on purely subjective grounds.

Immediately after the dissolution of Urey's committee, Mr. Wigner wrote to Dr. Briggs asking to be relieved from collaborating in the uranium work. He did not take an active part in this work until much later. Since Mr. Wigner was a sort of symbol for those who were convinced of the necessity of collaborating in this field with the U. S. Government, the fact that he was discouraged was an incalculable loss at that time.

When in November 1940 contracts were placed with Columbia University, it was specified that Fermi and I not be given knowledge concerning the development of the centrifuge supported by the Navy. Mr. Fermi was visibly affected by this and he has from that time on shown a very marked attitude of being always ready to be of service rather than considering it his duty to take the initiative. It so happens that early in 1940 I had given some thought to methods of maintaining a counter current of uranium hexafluoride in a centrifuge which I discussed with Urey and after November 1940 Urey was not free to continue these discussions. I felt so discouraged by this that I failed even to work up and write down some earlier nuclear measurements which Zinn and I made in 1939. These measurements gave information on the fission cross section of U^{235}. Had our value been known to Urey and had we been aware that separation of the uranium isotopes would receive adequate support, we certainly would have gone through the simple calculations.

In the period from November 1940 to November 1941 information was withheld not only from us but pertinent information was not communicated for reasons unknown by Dr. Briggs to members of the uranium committee who served under his chairmanship. After the British Government drew the attention of the U. S. Government to the possibility of making small atomic bombs from U^{235}, an advisory subcommittee to the uranium committee was appointed of which both Fermi and I were members. However, instead of allowing us to look at the problems as a whole and put on record our balanced conclusions, we were asked specific questions. The chief question we were asked was somewhat like this: "Will the spontaneous fission or uranium that has to be attributed to U^{235} make it difficult to set off bombs made of 235?" I remember that I asked that a statement be included in the recommendation of the subcommittee to the effect that there is no reason to believe that the spontaneous fission of uranium has to be attributed to U^{235} rather than to U^{238}. The text of the recommendation of the subcommittee was never communicated to us so that we did not even know what we had "recommended." This is another example of the question-answer game and another example for how not to use the scientists. This subcommittee had, I believe, two meetings throughout the whole of its existence. Incidentally, Mr. Wigner, who at this time was again working on uranium, was excluded from the meetings.

After collaboration with England was established, a number of British colleagues came over to visit this country. They were asked not to discuss with Fermi and me the setting off of the atomic bomb , but they were free to talk to Mr. Wigner. Now Fermi and I had certain ideas on this subject, but since there was nobody in the U. S. working along

that line, there was nobody in the U. S. with whom to discuss our ideas and the only efficient way of discussing them and communicating them to the U. S. Government would have been through our British colleagues and this way was barred to us.

Subsequently I did discuss some of my ideas with Dr. Teller who joined us at Columbia University, but in January 1942 we were told to stop talking to Teller because Teller was not considered cleared. By that time I succeeded in getting Teller interested in the [redacted] method of setting off of the atomic bomb in which I firmly believed. I suggested to Teller that he go on with this work alone, but I found him so shaken by the attitude taken towards him that he was unable to work along this line and did not take it up until after the creation of Dr. Oppenheimer's project of which he is now a member.

Our British colleagues who visited this country in 1941 fully realized the harm done to the American work through this compartmentalization and freely expressed themselves to this effect. Their attitude was summed up by G. P. Thompson who said something like this: I believe that the Government may succeed in keeping most of the work on uranium secret from the men who do the work--but will it succeed in keeping any of it secret from the Germans?

It would, however, be a mistake to think that in keeping pertinent information from physicists is an isolated phenomenon limited to the foreign-born. That was the case only when the trouble started; later information was more and more kept from Dr. Urey who is American born, a Nobel prize winner, and from other Americans who have a key position in this work. Among the American Nobel prize winners Dr. Urey has been perhaps pushed

around more than any of the others and the only explanation of which the scientists could think for this phenomenon is the fact that he took a stand against the application of the neutrality act toward the Republican Spanish government during the Spanish Civil War. He is, people say, considered to have been prematurely anti-Fascist. Another American born scientist may be looked upon with some suspicion because he was considered a pacifist or non-interventionist and he is not encouraged to keep contact with Oppenheimer's project. Some excuse can always be found in every individual case, but the net result is that the scientists are annoyed, feel unhappy and incapable of living up to their responsibility which this unexpected turn in the development of physics has thrown into their lap. As a consequence of this, the morale has suffered to the point where it almost amounts to a loss of faith. The scientists shrug their shoulders and go through the motions of performing their duty. They no longer consider the (overall) success of this work as their responsibility. In the Chicago project the morale of the scientists could almost be plotted in a graph by counting the number of lights burning after dinner in the offices of Eckhart Hall. At present the lights are out.

SECRECY

I wish now to discuss what gain in secrecy is achieved by withholding information from those scientists who have shown initiative in the past and who would show initiative at present if they were not frustrated in their work. There is a unanimous opinion among the scientists that there is no appreciable gain in secrecy by withholding information from the key men. All these men have a long record which is known to many

of their colleagues. Many of them have known each other for over 20 years. It is inconceivable that any of these scientists should disclose technical information to the enemy. As a matter of fact, there is not a single case in the history of the world on record where a scientist has betrayed the trust of the Government for which he worked in wartime.

That secrecy is important was recognized by many of us in March 1939 and there is a long record of documents showing that we did our best to get the support of the Government for organizing secrecy and that it was the representatives of the Government who failed to realize the importance of secrecy at that time, just as now they failed to realize the harm due to secrecy in the wrong place.

In the circumstances the compartmentalization imposed on the scientists is considered by them as unjustified and the argument that it is necessary for reasons of security is rejected by them.

At present the responsibility for secrecy rests with the Army and the methods for safeguarding secrecy are essentially the routine methods which usually are applied by the Army to developments very different from the character of the uranium projects. Leaks at present are numerous and the nature of some of those leaks is well known to the scientists. Few of us doubt, for instance, that the Germans know the location of our sites, which is very regrettable. No information of a technical nature is, however, leaking out through the scientists who have key positions in this work, at least none of us believes that leaks of this nature occur.

OBJECTIVE LOSSES DUE TO COMPARTMENTALIZATION

The most important loss due to compartmentalization of information in the uranium work is probably represented by the methods which remain undiscovered in America in spite of the large number of potential inventors engaged in the uranium work because of the frustration of the scientists due to the compartmentalization. This loss cannot be objectively evaluated at present. In the following I will list, however, a number of instances of which I have intimate personal knowledge of the facts and am satisfied that compartmentalization of information led to a considerable delay of the work:

1. The failure to realize the possibility of small and efficient atomic bombs built of 25 mentioned before.

2. Our failure to introduce the magnesium reduction of uranium tetrafluoride in 1941. This was primarily due to the fact that the knowledge that uranium tetrafluoride was available in Urey's project was withheld from me and so I was not able to take any action along this line until the middle of 1942. This was both a great loss in time and a substantial loss in money since the magnesium reduction would have made it unnecessary to build installations for calcium distillation and for the whole Westinghouse metal production program.

3. Information being withheld from Teller is chiefly responsible for the late start on the theoretical work on [redacted] method for setting off atomic bombs.

4. The inhibition of the discussion between Teller and me is directly responsible for a loss of six months in realizing the importance

of purifying element 94. This point was recorded by me in a memo addressed to Dr. Compton.

6. Another instance where secrecy in the wrong place has caused considerable damage is connected with the development of the centrifuge by Westinghouse. The men at Westinghouse who knew the portent of that development were not free to communicate their knowledge to certain other key men in the Westinghouse organization upon whose good will the facilities made available for this development by Westinghouse depended. This slowed down the development of the centrifuge by Westinghouse. This and also that Murphree's men failed adequately to realize the importance of the counter current method sponsored by Urey, accounts probably for the fact that the centrifuge development fell behind so much that it practically was thrown overboard in favor of other developments. The scientists do not know enough of these happenings to be able to express any opinion with assurance, but they are far from being satisfied that the dentrifuge is not a potent method for separating isotopes. It would

not be too surprising to me if atomic bombs manufactured by the Germans by the centrifuge method were the first to go into action.