

Approach to England; Last Appeal to France

On March 20, 1939, we learned that Joliot had also observed the neutron emission from uranium and had published his observations in England on March 18. He actually started on these experiments some time in January.

Great efforts were made thereupon to persuade the French and English physicists to stop publications on this subject. The negotiations were carried out by Wigner, Weisskopf, and myself. Teller tried to persuade the physicists at Columbia University to wait before publishing the outcome of these negotiations.

A telegram was sent by Weisskopf to Halban in Joliot's laboratory reminding Joliot of my letter and advising him that we were approaching the British physicists. Another telegram was sent by Weisskopf to Blackett in England suggesting that the British withhold all publications on this subject. A letter was sent by Wigner to Dirac in Cambridge, England, to the same effect. Blackett cabled to Weisskopf that the collaboration of the Royal Society could be expected, but Joliot's reply was not satisfactory. Joliot's cable pointed out that articles had appeared in the American press in February which were based on statements by Roberts in Tuve's laboratory and let the cat out of the bag. To this cable of Joliot I replied that we had in the meantime secured the collaboration of Tuve's laboratory and I urged Joliot to agree to a collaboration in this matter. The answer of Joliot to my telegram was negative.

The text of all these telegrams as well as Joliot's final letter is inclosed.

Third Approach to the Navy, May 1940

The second meeting held under the chairmanship of Dr. Briggs on April 27, <sup>1940 (K.W.)</sup> 1942, represented some progress, insofar as I was now requested to delay the publication of my papers, whereas, up until then, my request to the Physical Review to hold up the publication of my papers was an arbitrary action on my part, and was open to criticism on the part of some of my colleagues. No general recommendation to hold up the publication of dangerous papers was however made by the Uranium Committee. Enclosed is a copy of my letter to Physical Review.

Soon afterwards, Professor Turner in Princeton wrote a paper, which, if it were allowed to publish, would have drawn attention to the importance of element 94. Fortunately Turner showed his manuscript to Wigner, and, on his advice, sent me a copy, asking me whether I saw any objection to its publication. I wrote Turner that I have, in the meantime, approached Urey with the request of bringing about the general policy of withholding publication and asked Turner to delay the publication of his paper.

I suggested to Urey that some committee should be formed under his chairmanship to deal with the requirement of secrecy, and that this committee should include G. Breit, in order to secure the adherence of the Physical Review to the policy of secrecy which may be worked out.

In order to have government sanction for Urey's committee, I introduced Urey to Dr. Sachs, and asked Dr. Sachs to introduce Urey to Admiral Bowen, who, in the meantime, <sup>had taken</sup> took over the Naval Research Laboratory. Urey and Sachs visited Admiral Bowen, and Urey's appointment as the chairman of a committee followed.

The committee met under Urey's chairmanship in Washington on June 13, 1940. A general policy of withholding publication was formulated at this meeting in which G. Breit participated. Breit arranged with the Physical Review a practical method for establishing a sort of censorship in execution of the policy formulated at the meeting. After June 13, 1940 papers dealing with uranium were subject to "censorship".

I enclose copies of my letters to Turner and Breit, and a copy of Urey's letter to me, in which he reports of the result of his contact with Admiral Bowen. Urey's letter is a form letter sent with identical texts to some seven men, the members of one of the project committees. <sup>ed</sup>

November 12, 1942

A. H. Compton

L. Szilard

I am enclosing a collection of documents which you might find useful, if it is desired to make a more thorough investigation of my background in connection with the work of our project. This collection of documents does not give the full history and was selected rather from the point of view of describing the attempts which we made to keep our work secret.

Any investigation, if limited to this country, could go back for 10 years, since I first came to this country in 1931. Although I did not reside here without interruption for more than 5 or 6 years. The following is a list of persons who have had some knowledge of the background of our present work and who had known me for a long period of time:

E. P. Wigner, Metallurgical Laboratory, University of Chicago. (He has known me intimately for 20 years).

Dr. B. <sup>le</sup>Leibowitz, president of the Trubanizing Process Corp., Empire State Bldg., N. Y. Dr. <sup>le</sup>Leibowitz has known me intimately since 1932. I approached him in February 1939 and told him of the importance of the discoveries concerning uranium for national defense. It was through his generosity that Fermi and I had radium available for our experiments in 1939.

Professor Albert Einstein, Princeton. He has known me for 20 years, and I was in close contact with him between 1922 and 1932 in Germany.

Professor Edward Teller, Metallurgical Laboratory, Univ-

ersity of Chicago, has known me intimately since 1933, and I was in close touch with him during my stay in England from 1933 to 1938.

If it should be desirable to extend the investigation to England, I should be very glad to give a list of those institutions and persons with whom I was most closely associated during my stay in England from 1933 to 1938.

L. Szilard

- A. First Approach to the Navy through Royal Naval Research Laboratory, 1933
- B. Approach to England; Earl Amhurst; Frank Bigney; Sir Robert Robinson; Sir John Cockcroft; Sir James Chadwick; Sir George Thomson; Sir John Randall; Sir John Cockcroft; Sir James Chadwick; Sir George Thomson; Sir John Randall
- C. Collapse of Secrecy, April 1934; Szilard and Teller to Tamm (not sent); Teller to Szilard
- D. Second Approach to the Navy, June 1934; Naval Research Laboratory to Szilard
- E. First Approach to the President of the United States, October 1934; Dr. Teller to Tamm; Szilard's note to the President; Einstein's letter to the President
- F. Renewal of Policy of Withholding Publications; Letters of Szilard to Tamm, Physical Review
- G. Second Approach to the President of the United States, March - April 1935; Letter of the President to Dr. Teller
- H. Third Approach to the Navy, June 1935; Letters of Szilard to Physical Review, Dr. Teller, Dr. Teller; Teller's letter of Teller to Szilard
- I. Early Exposure on the Graphite Moderator System, July to October 1935; Letters to Tamm, July 1935; Memorandum to Dr. Tamm, October 1935

ATTEMPTS AT SECRECY FROM MARCH 1939 TO JUNE 1940

L. Szilard

November 4, 1942

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Attempts At Secrecy-March 1939-June 1940

November 4, 1942

It was realized by some of my colleagues and myself in January, 1939, that a chain reaction may be possible in uranium and that this would have important military applications. In the eighteen months that followed a few of us were engaged in a struggle to persuade our colleagues and the United States government of the necessity of keeping this subject secret. The following is a short account of that struggle illustrated by letters and telegrams exchanged between various physicists and various persons and the White House.

The approach to the U. S. government was broader in its scope than the question of secrecy alone and we asked first for moral, and later on also for financial, support of research work.

First Approach to the United States Government  
through ...

Informed of our discoveries, Dr. S. D. Dyer came to New York and strongly appealed to us immediately to inform the United States government of these discoveries. At his insistence G. N. Papanicolaou, then Under Secretary of the Navy, was called to reach his but he arranged with someone else in the Navy Department that a conference would be called at which Papanicolaou would inform the government of these discoveries. This conference took place at the Naval Research Laboratory on 15th or 16th of ... and did not lead to definite conclusions. One of the participants in this conference was ... of the Naval Research Laboratory.

### Agreement about Secrecy

Immediately after Fermi and I observed the neutron emission of uranium early in March 1939 I made a request to G. B. Pegram to withhold the publication. This was opposed by a number of our colleagues. Some opposed it on the ground that we had not actually proved that a chain reaction can take place and that they did not believe that this would be the case. Others opposed it on the ground that even if a chain reaction did take place, it was doubtful if explosions could be brought about. Still others opposed it simply on the ground that it was not customary to withhold publication of scientific discoveries. At a meeting between Fermi, Teller and myself held in Washington on March 19, it was, however, decided that we would ask that the publication of our papers be withheld. Fermi was entrusted with the execution of this decision and he returned to New York and arranged with G. B. Pegram to hold up our papers in the Physical Review.

### First Approach to the United States Government

through Fermi

Informed of our discoveries, E. P. Wigner came to New York and strongly appealed to us immediately to inform the United States government of these discoveries. At his insistence G. B. Pegram tried to telephone Edison, then Under Secretary of the Navy. He failed to reach him but he arranged with someone else in the Navy Department that a conference would be called at which Fermi could inform the government of these discoveries. This conference took place about March 17th or 18th in Washington but it did not lead to definite conclusions. One of the participants at this conference was Ross Gunn of the Naval Research Laboratory.



First Approach to the President of the United States

August - October 1939

In July 1939 I reached the conclusion that the chain reaction might be set up in a uranium/graphite system, and that this possibility had to be considered as an imminent danger. In a conference with Wigner and Teller, we examined the situation and came to the conclusion that we ought to approach the United States Government through some new channel. For this purpose we enlisted the assistance of Professor Einstein, to whom we explained the situation in great detail. I also approached Dr. Alexander Sachs, at that time economic advisor and vice-president of the Lehman Corporation.

Professor Einstein wrote a letter addressed to the President in which I enclosed a memorandum. These documents were handed to Dr. Alexander Sachs who submitted them to the President in a personal interview together with a memorandum of his own. In response the President appointed Dr. Briggs as chairman of a committee subsequently called the Uranium Committee.

The enclosed copy of a letter written by Dr. Sachs to Dr. Wigner relates this phase of the development. Copies of my memorandum and Professor Einstein's letter to the President are also enclosed.

The last paragraph of my memorandum raised again the question of secrecy. This question was further stressed at the meeting held under the chairmanship of Dr. Briggs, October 21, 1939, but as far as I know, Dr. Briggs report to the President contained no recommendation concerning this point.

Second Approach to the President of the  
United States. March-April 1940

At my request, Professor Einstein sent a letter to Dr. Sachs, and Dr. Sachs forwarded Professor Einstein's letter to the President stressing the necessity of deciding upon a government policy towards this matter, and in particular, stressing the necessity of a general policy of withholding publications.

In response to Professor Einstein's letter, the President instructed General Watson to arrange another meeting.

A copy of the President's letter to Dr. Sachs is enclosed.

## RENEWAL OF POLICY OF WITHHOLDING PUBLICATIONS

February 1940

When by February 1940 no word reached me from the Government indicating their interest in uranium research, I sent two manuscripts on the subject of the chain reaction in an uranium/carbon system to the Physical Review. I wrote to the editor of the Physical Review asking him to withhold the publication of these papers until further notice, and simultaneously I advised Professor Einstein of the urgent need of some general policy concerning withholding publications of this nature.

Enclosed are copies of my letters to Dr. Tate, editor of the Physical Review.

## EARLY EMPHASIS ON THE GRAPHITE-URANIUM SYSTEM

July to October 1939

Enclosed are copies of letters sent to Fermi in July 1939 and of a memorandum submitted to Dr. Briggs in October 1939. This memorandum puts on record the recommendations which I made orally at the first meeting of the Uranium Committee, under the chairmanship of Dr. Briggs, on October 21, 1939.

To keep our work secret.

Any investigation, if limited to this country, could not last for 10 years, since I could not stay in this country for more than 1, though I did not reside here since my departure for more than 1, or 2 years. The following few names of people who have had some knowledge of the background of our previous work and who had known me for a long period of time:

Dr. P. Nigam, Metallurgical Laboratory, University of Chicago. He has known me intimately for 20 years.

Dr. J. L. Lohr, president of the Radioactive Research Corp., Empire State Bldg., N. Y. Dr. Lohr has known me intimately since 1932. I approached him in February 1939 and told him of the importance of the discovery of a moderator for nuclear science. It was through his generosity that Fermi and I had access to his laboratory for our experiments in 1939.

Professor Albert Einstein, Princeton. He has known me for 20 years, and I was in close contact with him between 1932 and 1933 in Germany.

Professor Enrico Fermi, Metallurgical Laboratory, Univ.

June 24, 1942

Columbia University  
Attention: Professor G. B. Pegram  
Pupin Physics Laboratory  
Columbia University  
New York, New York

Gentlemen:

While I was employed by Columbia University I signed a statement concerning inventions made by me during my employment at Columbia University. Since I am now asked to sign a similar statement in connection with my employment at the University of Chicago, and since I do not remember the exact text of the statement which I signed at Columbia University, I would very much appreciate receiving an explicit confirmation from Columbia University to the effect that the statement which I signed did not intend to cover in any way inventions made by me after leaving the employment of Columbia University, that is, February 1, 1942.

I would also appreciate your letting me have a copy of the text of the statement which I actually signed.

Very truly yours,

L. Szilard

LS:g

June 29, 1942

G. B. Pogram  
Pupin Physical Laboratory  
Columbia University  
New York City

Dear Professor Pogram:

Many thanks for your kind letter of June 27th.  
The matter seems to be perfectly clear now and I shall  
advise Mr. Stearns that the Columbia group is free to  
sign a patent agreement with the University of Chicago.

Sincerely yours,

L. Sillard

LS:NA

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COLUMBIA UNIVERSITY  
DIVISION OF WAR RESEARCH

COMMITTEE

GEORGE B. PEGRAM, CHAIRMAN  
FRANK D. FACKENTHAL, PROVOST  
W. EMERSON GENTZLER, BURSAR

REPLY TO THE UNDERSIGNED

COLUMBIA UNIVERSITY  
BROADWAY AT 116TH STREET  
NEW YORK, N. Y.

Dean George B. Pegram, Chairman  
Committee on War Research  
Columbia University

Dear Dean Pegram:

As necessitated by the patent agreement made with Columbia University by the undersigned as a condition of his employment of 1940, 1941, or 1942 by Columbia University to work on research and development under a contract or contracts between Columbia University and the government National Defense Research Committee or the government Office of Scientific Research and Development, I, the undersigned,

declare that to my present knowledge and belief the only new and useful developments that were made by me in the course of, or arising from, the aforesaid work which may in my opinion possibly constitute inventions or discoveries of a patentable nature are the following listed by brief titles:

1. \_\_\_\_\_  
\_\_\_\_\_
2. \_\_\_\_\_  
\_\_\_\_\_
3. \_\_\_\_\_  
\_\_\_\_\_
4. \_\_\_\_\_  
\_\_\_\_\_

(over)

COLUMBIA UNIVERSITY  
DIVISION OF RESEARCH

CONFIDENTIAL

5.

that a fuller description of each of these possibly patentable inventions is given on an attached sheet with indication of date of the invention and of notes or reports or other records to substantiate the fact of invention and the date claimed;

and that the undersigned further states that in case he later discovers that he has omitted any invention that should appear on the foregoing list, he will promptly report such invention to Columbia University.

(Signed) \_\_\_\_\_

Date \_\_\_\_\_

Place \_\_\_\_\_



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F-51  
"The designation "LIMITED" indicates a report dealing with information which is more restricted than the information in the usual C reports. Persons receiving these reports are not to show them to other members of the project, even of the same laboratory, without specific authorization."

WHAT IS WRONG WITH THIS

L. Szilard

September 21, 1942

~~LIMITED~~

No RP

Introduction

These lines are primarily addressed to those with whom I have shared for years the knowledge that it is within our power to construct atomic bombs. That the existence of these bombs will mean we all know. It will bring disaster upon the world if the Germans are ready before we are. It may bring disaster upon the world even if we anticipate Germany to win the war, but lose the peace that will follow.

CLASSIFICATION CANCELLED  
Date 9/25/1952  
For The Atomic Energy Commission  
C. T. Marchall, M.R.  
Director, Division of Classification

We cannot have peace in a world in which various sovereign nations have atomic bombs in the possession of their armies and any of these armies could win a war within twenty-four hours after it starts one. One has to visualize a world in which a lone airplane could appear over a big city like Chicago, drop his bomb, and thereby destroy the city in a single breath. Not one house may be left standing and the radioactive substances scattered by the bomb may make the area uninhabitable for some time to come.

It will be for those whom the constitution has entrusted with determining the policy of this country to take determined action near the end of the war in order to safeguard us from such a "peace". They will have to be prepared for this task in order to be able to fulfill it and some way will have to be found to do this.

Perhaps it would be well if we devoted more thought to the ultimate political necessities which will arise out of a world in which you may feel, however, that it is of more immediate concern to us that the work which is pursued at Chicago is not progressing as rapidly as we would like.

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at the Washington End?

Roughly speaking, there are two kinds of troubles which frustrate our work. Troubles which originate at Washington end of our organization, and troubles which originate at the Chicago end of our organization.

The unsatisfactory state of our metal supply, nine months after the reorganization which led to a merger of the Chicago and the Columbia projects, is illustrative of the effect of the trouble which originates in Washington. But although this trouble originates outside of Chicago, I do not think that we ought to blame anybody but ourselves for this calamity. We knew from the outset that the division of authority between Murphree and Compton, with respect to processing our materials, would lead precisely to this sort of catastrophe. This was not only known privately to many members of our group, but it was openly stated by a number of us at an almost public joint meeting of Columbia and Chicago groups at Columbia in January. Even "outsiders" like D. P. Mitchell and Smyth saw the point and joined in the chorus of those who condemned the proposed arrangement as unworkable. There was not a single voice raised in favor of the proposal of dividing the authority for processing our materials between Compton and Murphree.

We may have to answer before history the question why we tolerated an arrangement which we knew could not work. It is not possible for us to shift the blame to Dr. Bush or Dr. Conant, who originally decided in favor of that arrangement without consulting us. They cannot devote their full time and attention to our problems, and this matter is of such complexity that nothing less than that will do. However intelligent a man may be, if he is not in direct contact with our problems, he is not able to foresee the consequences of decisions which affect the outcome of our work.

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Under the circumstances, it would have been our duty to tell Dr. Bush, in January, that the arrangement which he devised would not work. We might not have been able to persuade him that the arrangement was, objectively speaking, unworkable, but surely he would have known that no arrangement can be successful if the men who are supposed to make it work lack the faith that they can make it work.

We may be tempted, of course, to shift the blame on to Compton and say that it would have been for him to tell Dr. Bush that the scheme was unacceptable. This, I believe, would be unfair to Compton, and on this point I have to elaborate because it has an important bearing on the future, as well as on the past.

Our project is exceptionally rich in men who belong to the creative type and represent what may be called the artistic temperament. Compton is one of them, and there are quite a few others. To be sure, each of these men has his shortcomings and limitations. However, it would be a grave mistake to believe that because each of us has certain obvious weaknesses, the group as a whole is not fit to carry the full responsibility for its task. If we were properly organized, those shortcomings and weaknesses would not add up, but rather cancel out, within the group. If we were properly organized, there would be no task in physics, engineering or production, which we could not tackle and master, as long as each of us realizes the limitations of the others and sincerely tries to find out something about his own limitations.

Compton was put in charge of our project last fall by Dr. Bush, but if the question of leadership had been put up to our group, he would have been elected by unanimous vote. We may complain about his not taking a strong stand in Washington, but let us be clear on this point above

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all: We cannot eat our cake and have it; if we want a man who can run our project at Chicago and create here an atmosphere of friendship and loyalty, we cannot expect the same to go to Dr. Bush and threaten to resign unless he is given all the authority required for the success of our project, or to go over the head of Dr. Bush to the President, and ask the President for this authority.

This is amply born out by past experience. Only rarely is it possible to make a forecast with a high degree of assurance and such a clear case as we had in January with respect to the division of responsibility between Compton and Murphree may not occur again. If Compton did not take action then, he cannot be expected to take action in any other case which is less clear cut as long as he has to ask for things for himself.

The situation might be different if Compton considered himself as our representative in Washington and asked in our name for whatever was necessary to make our project successful. He could then refuse to make a decision on any of the issues which affect our work until he had an opportunity fully to discuss the matter with us.

Viewed in this light, it ought to be clear to us that we, and we alone, are to be blamed for the frustration of our work which originates from the Washington end of our organization. We should have asked Compton to make our views clear, or if he should have preferred this, we should have made our stand clear in Washington ourselves. It is my personal conviction, however, that we shall not be able to make progress in this direction until we have put our own house in order.

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### What is Wrong at the Chicago End?

While the metal situation illustrates the trouble which originates in Washington, the fact that we have not developed a satisfactory cooling system and are not mentally prepared to do so in the near future, illustrates the trouble which originates at the Chicago end of our organization.

In one respect at least, the troubles which originate at Chicago are less serious since it is entirely within the authority of Compton to remedy the situation.

Stated in abstract form, the trouble at Chicago arises out of the fact that the work is organized along somewhat authoritative rather than democratic lines. There is a sprinkling of democratic spots here and there, but they do not form a coherent network which could be functional. This is partly due to a compartmentalization of information which is imposed on Compton from Washington. Since this is again trouble which does not originate at Chicago, we may pass it over here until such time as we have solved the "local" Chicago problems.

In order that we should be able to do so, it is necessary to realize that there are certain inherent difficulties which cannot be removed and must be met by skillful adjustment of our organization.

I believe that we ought to say at the outset that the breakdown of the Chicago organization is of our own making. Though certain trends followed by Compton which will have to be mentioned later do represent obstacles standing in the way of a well functioning organization, it was entirely within our power to compensate for these trends by making full use of the existing machinery. This we did not do and therefore we may say that the blame is ours, and ours only. At the most we can say in our defense that there were mitigating circumstances.

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Compton likes to avoid violent clashes of opinions which are unavoidable within a committee small or large if vital issues are debated, on which strong feeling prevails. In accordance with this, he has a tendency to steer a meeting towards greatest harmony rather than toward vital decisions. He prefers to deal with the more important issues in private conversations, a method which is either time consuming, or leaves many members of the planning board uninformed. But even in private conversation with Compton I personally find it difficult to have an issue settled. Perhaps this is my fault. I am, as a rule, rather outspoken, and if I do not call a spade a spade I find it rather difficult to find a suitable name for it. It may be that in talking to Compton I am overplaying a delicate instrument. This is, by the way, an opportunity to apologize to all members of our group for my outspokenness and to ask them to consider it as one of the inevitable hardships of the war.

Just as the question of the metal supply illustrates the trouble at the Washington end, the question of the choice of a cooling system for the power unit illustrates the difficulties at the Chicago end.

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We knew for a long time that there are three possible ways in which the power plant could be cooled: by helium, by water and by bismuth. The question whether these three systems should be designed simultaneously and the design be carried out possibly up to the blueprint stage on all three of them, or whether one of these systems should be worked out in preference to others, and which should be given preference, was never put up for decision to any of our committee. Even so these committees should have considered as one of their most important tasks to decide this question at an early date. No such decision was taken.

By May, it became apparent to everybody that it will go over 1. An engineering group turned up somehow in our project and started to work, under the direction of Mr. Moore, on plans for the helium cooled system. Nobody knew why the helium cooled system was given preference by this group, but it was the general impression that somehow, somewhere, somebody decided that we shall build a helium cooled system first and place orders for the heavy accessory equipment which the system required at an early date.

In the meantime, Wigner and his division became more and more convinced that a water cooled system could be built in a much shorter time, although they were not willing to say that they could foresee with certainty all questions of operational safety. The question whether Wigner would be willing to assume the responsibility for developing this system, if necessary, into the blueprint stage, was never put to him, and remains undecided up until the present day. Clearly, if Wigner were willing to assume this responsibility, an engineering staff would have to be added to his division in order to help them to produce usable plans for a water cooled unit. A proper balance between physicists and engineers in Wigner's division

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require the addition of quite a number of engineers to this division.

It has been my personal opinion that it is not possible to judge the relative advantages and disadvantages of any of the three systems until they have been designed in detail, although it may not be necessary to carry the design into the blueprint stage. I further held the view that, particularly in the case of the water and bismuth cooled power units, certain simple technological tests will have to be made before the detailed designing of the power unit can be carried through with faith in its ultimate success. A thorough designing job can hardly be accomplished in the absence of such faith. I have therefore pressed for the establishment of a technological division which after a while came into existence.

I was put in charge of this division which had the task of looking after the technological problems involved in the three different cooling systems. For a time, it looked as if we might have three almost different groups, each comprising physicists and engineers to work simultaneously on designs for these different cooling systems. It is still my opinion that that would have been the right course of action. While I personally felt that I can contribute most towards developing the bismuth cooled system because I have more faith in this system than in the others, I am quite aware of the fact that this may be a purely personal preference, and I do not have any well founded opinion as to which of these three systems shall prove to be the most successful, or can be made to work fastest.

I, myself, was quite prepared to assume the responsibility for developing a design for the bismuth cooled plant. I felt, however, that this work could not be pursued with confidence unless the metallurgical problems which are involved were settled. As far as I could tell, Compton was in full agreement with this approach, and thanks to his vigorous help we

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succeeded in having Foote, a metallurgist, released from Cooper Union. Simultaneously, I contacted various organizations with a view of having a pump designed for liquid bismuth. Four tons of bismuth were ordered at a cost of \$10,000.

Thus I made arrangement to divide my time between looking after the technological division and the designing of a bismuth cooled power unit. These two tasks are almost too much for one man and the only reason why I thought that I would be able to manage them was the fact that Creutz, Foote and Marshall can very well carry most of the burden of the technological division. Foote was supposed to look after the bismuth, Marshall after the recasting of uranium metal, and Creutz after the problems of the water cooled plant, and many other things as well. What became of all these plans?

About six weeks ago I was informed that "we are sending through Marshall and Creutz to Boston to fuse Alexander's metal at M.I.T." If Creutz had left, the whole technological division would have collapsed and the work of the theoretical division working on the water cooled power unit would have been frustrated, since Creutz was looking after the technology of the water cooled unit. I succeeded in substituting Foote for Creutz. I agreed to his departure because I assumed that it would be for two weeks at the most and besides I had reached the conclusion that it is not longer worth while to fight about individual issues.

It was not possible to have a date set for Marshall's and Foote's return in six weeks. All work on the bismuth cooled plant is stopped. While I may be wrong in laying such emphasis on the bismuth cooled plant, it is a fact that nobody else has looked into this matter, with the possible exception of the theoretical division, and consequently there is nobody in a position to express an opinion whether the damage brought about by Foote's and Marshall's continued absence is counterbalanced by the good they can do at M.I.T. So

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body has made an effort to find men who could replace Foote and Marshall at M.I.T.

In the meantime, Dr. Moore's division carried the design of the helium plant into considerable detail. We were all under the impression that it has been decided to build a helium cooled plant and to place an order for heavy parts of the equipment at an early date. For this reason, while I had been studying this type of cooling at the earlier stages of our project, I did not continue to pay any attention to the questions connected with the helium cooling.

Suddenly, about a week ago, a few of us were informed that we are expected to express an opinion whether a helium cooled plant should or should not be built. I possibly could have formed such an opinion if I had followed the development of the helium cooled power plant for the past two or three months and I should certainly have been very glad to do so. In the circumstances, I do not believe that any opinion that I personally might now express on the subject would be worth anything, and I see no way how I could form a valid opinion within a short time.

#### Diagnosis

If I have to give a diagnosis in an abstract and therefore necessarily misleading form, I should say this: In the past, the men who were on the technical committee and the planning board did not have the feeling that they were responsible for the success of the project. There was no mechanism for reaching decisions and consequently decisions were reached in a haphazard way. Essential decisions were being omitted or were taken many months after they were due. As time went on, more and more of us began to emphasize that we do not want to be held responsible for what was or was not happening. There was more and more shrugging of shoulders among the group leaders and an increasing tendency of narrowing down their responsibility to this or that detail of the work with which they had been

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explicitly entrusted.

To all this came an increasing tendency of compartmentalization of information imposed upon Compton by the Washington end of our organization and inevitably our best men are losing the zest for their work. Recently, a major decision was taken by the Executive Committee which usually meets once a month in Washington. This decision will vitally affect our project. It was the third major decision within a year which was taken without consulting our group, our planning board, or our technological committee. Finally, a stage has been reached where it becomes clear that we have to choose between two alternatives:

We may take the stand that the responsibility for the success of this work has been delegated by the President to Dr. Bush. It has been delegated by Dr. Bush to Dr. Conant. Dr. Conant delegates this responsibility (accompanied by only part of the necessary authority) to Compton. Compton delegates to each of us some particular task and we can lead a very pleasant life while we do our duty. We live in a pleasant part of a pleasant city, in the pleasant company of each other, and have in Dr. Compton the most pleasant "boss" we could wish to have. There is every reason why we should be happy and since there is a war on, we are even willing to work overtime.

Alternatively, we may take the stand that those who have originated the work on this terrible weapon and those who have materially contributed to its development, have, before God and the World, the duty to see to it that it should be ready to be used at the proper time and in the proper way.

I believe that each of us has now to decide how he feels that his responsibility lies.

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*Compton*

*NRD*  
*Compton*

THE UNIVERSITY OF CHICAGO

DATE October 7, 1942

To Mr. Leo Szilard

DEPARTMENT

FROM A. H. Compton

DEPARTMENT

~~SECRET~~

IN RE: Engineering of Bismuth Cooled Plant

In accord with instructions from General Groves, I have requested Mr. Moore to proceed at once with the engineering of a bismuth cooled power plant.

May I ask you to act as consultant to Mr. Moore on the design of this plant. One of the most useful things you could do now would be to recommend to him by Friday of this week an engineer with whom you could work to advantage and who under Mr. Moore's direction would have the immediate responsibility for designing the plant.

In accord with the directive which I have been given by General Groves, the immediate objective is to design a plant that can be ready for operation by June 15, 1943. We have in mind the dissipation of roughly 100,000 kw. This figure is, however, flexible if there should arise important advantages in a plant of larger or smaller capacity.

In case it should appear impracticable at this time to design a bismuth cooled plant to be ready by June 15, work on such a plant will cease to be of the greatest urgency. Interest in it will nevertheless continue both as a possible plant for large capacity and as a possible means of utilizing the power that is developed. Studies aimed toward such developments would be continued.

*A. H. Compton*

KT

cc: Mr. Moore  
Mr. Allison  
Mr. Doan

~~SECRET~~

CLASSIFICATION CANCELLED  
Date 10/3/56  
For The Atomic Energy Commission  
*E. L. Marshall/nck*  
Director, Division of Classification

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