D

(3 bis - 6

420 West 116th Street
New York City

April 19, 1940

Dear Dr. Bourse:

Since I know that you are a graduate of the Naval Academy, I hope that you might be able to answer a question, to which I am anxious to find the answer. It involves some elementary knowledge of naval strategy which I do not possess. As you will see in the following, I have put the question as a hypothetical question, and you need not have or express any opinion on the subject whether the hypothesis on which the question is based is moonshine or reality.

For your information I wish, however, to add that it has become necessary for me to raise this question in connection with some recent developments in physics, of which I am keeping the US Government fully informed. These developments might lead to results which have a bearing on questions of national defense. There are in this respect quite a variety of possibilities, and in presenting the case to the Government I am anxious to give each of them its proper weight so as to convey a well balanced picture. Just how much emphasis I shall put on the point of naval applications will depend on the answer to the hypothetical question which I am submitting to you in the following:

Let us assume that we had a new fuel of which one ton could supply as much power as 3000 tons of oil, and let us assume that it would be possible to produce about 300 tons of this new fuel per year. In this case one could increase the cruising radius of larger naval units by giving each a reserve of about 50 tons of the new fuel, corresponding to an oil reserve of 150.000 tons. In view of the limited supply, the ships would continue to burn oil in peace time, and the new fuel would have to be considered as a reserve to be used only at manoeuvres and in case of war. The equipment for "burning" the new fuel would represent an additional weight of perhaps 1000 tons. This increase of weight would however be more than compensated by reducing the oil load from a maximum of perhaps 4000 tons to a normal oil load of about 1000 tons. I wish to illustrate this by quoting an example based on data which I found in Jane's "Fighting Ships" for larger units of the British navy.

A 30.000 ton battleship of the fastest type has nowadays a maximum oil load of about 4000 tons and uses somewhat more than ½ ton of oil per mile if cruising at an economical speed. This corresponds to a cruising radius of about 8000 miles. Let us now consider such a battleship equipped with an installment weighing 1000 tons, adapted to "burn" the new fuel, and carrying 50 tons of it as a reserve in addition to a normal oil load of 1000 tons. The 50 tons of the new fuel represent the equivalent of an oil reserve of about 150.000 tons. The limitations which arise out of the present finite cruising radius would thus be removed. There would also be a saving in weight of about 2000 tons due to the reduction of the oil load, and this ought to lead to an increase in the top speed of the vessel.

I should imagine that the combination of high speed and a greatly increased cruising radius might be of decisive importance in case of a war with Japan. Am I right on this point? Could you let me have your detailed comment on this subject?

If you are unable to answer this question yourself, please feel free to put it in confidence to some of your friends. In this case, if you ask the advice of somebody who is in active service, you ought to mention that this is a purely private inquiry for our personal information, and consequently, in answering the question your friend ought to confine himself to draw on such knowledge as can be considered public property. He ought to make sure not to disclose anything which might be considered as a secret by the naval authorities. Naturally, I shall mention no names in passing on your information unless I am authorized to do so.

Thanking you for the attention which you are giving this matter, I am,

Yours sincerely,

(Leo Szilard)

Sect - II

Nov. 1970
2nd set

Added material for pages 118-119

re MEETING OF BRIGGS COMMITTEE, APRIL 27, 1940.

Schedule of speakers.

3b.s.6

Dr. Briggs: Opening remarks,

Dr. Sachs: Background of this meeting

Dr. Fermi: State of experimental work

Dr. Szilard: Possible implications concerning national defense

Dr. Pegram: Outline of possible organization of future work

Dr. Wigner: Remarks on the above

April, 1940 (K.W.)

For Sect 6-II

Nov, 1970
2nd set

Additional Material for pages 118-119 + notes 36-37

Letter, L.S. to Einstein. Mar. 7, 1940

3 b.s. 6

420 West 116th Street
New York City

den 7. Maerz 1940

Lieber Herr Professor!

Den Briefentwurf, den wir zusammen durchgesprochen haben, habe ich Dr. Sachs zugesandt. Er schlaegt, wie Sie aus seinem anliegenden Brief sehen, gewisse Aenderungen vor. Diese und andere Aenderungen sind mit Bleistift in dem letzten Entwurf eingetragen, und Sie koennen jeweils an dem Gekritzel sehen, welche Aenderungsvorschlaege von Sachs und welche von mir stammen.

Im beiliegenden Umschlag finden Sie die neue Fassung, in der ich versucht habe, den Wuenschen von Dr. Sachs, so weit es mir moeglich schien, nachzukommen. Eine Kopie fuer Ihre Akten liegt ebenfalls im Umschlag.

Falls Sie einige der Aenderungen wieder rueckgaengig machen oder sonst etwas aendern wollen, so koennten Sie Ihre Korrekturen in das saubere Exemplar eintragen und mir dieses zur nochmaligen Abschrift zurueckschicken.

Mit freundlichem Gruss

Ihr sehr ergebener

For Sect 6-II

Nov. 1970
2nd set

Additional Material for page 120 (last paragraph) (3)

"the question of secrecy ~~xxx~~ again came up."

Letter, L.S. to Joliot April 12, 1940

36.5.6

UNDER
KNOTT MANAGEMENT

TELEPHONE
UNIVERSITY 4-2700

King's Crown Hotel
420 WEST 116TH STREET
NEW YORK
OPPOSITE COLUMBIA UNIVERSITY

April 12,1940

Professor F. Joliot
College de France
Paris, France

Dear Professor Joliot:

Many things have considerably changed since March last year, and therefore I should like to raise once more the question whether or not results concerning chain reactions in uranium ought to be published.

It is reported that such publications are prevented in Germany and that work on uranium there is carried out in secrecy.

As you know, my own inclination would be to delay publications on this subject, but I have not discussed this matter with anyone else here in America since April last year, and I do not know what view others would take if the question were to be raised again. If, however, I should hear from you that in the meantime you have adopted some new policy of delaying publications, I could then perhaps talk to others here and find out what the general feeling is on this subject.

Everybody here hopes that you and your colleagues will not suffer too many inconveniences on account of the war and that your work will go on uninterrupted.

With best wishes to all,

yours sincerely,

(Leo Szilard)

For Sect 6-II

Nov. 1970
2nd set

Added material for page 122

re: "Group working at Columbia University was transferred to Chicago"

"Memorandum on my Vistit to Chicago, January 23-24" Jan.26, 1942

Conversation with Compton and others about choice of location.

Note: we still have an original typing of this memo. It may not have been sent. (KW.)

MEMORANDUM ON MY VISIT TO CHICAGO, JANUARY 23-24.

COPIES TO:
H. Anderson
E. Fermi
B. Feld
John Marshall, Jr.
E. Teller
W. H. Zinn
Alvarez
A. H. Compton
E. O. Lawrence

January 26, 1942

I arrived in Chicago on the evening of Friday, January 23, and had a conversation with Compton, Lawrence, and Alvarez. Compton told us that he would try to bring the Princeton group, which includes Wigner, to Chicago and build up the work in Chicago while the work in New York would also be expanded. Compton intended to have an office in New York, and he proposed spending a considerable amount of his time in New York. Lawrence, Alvarez and I felt that this plan was very bad since our main task is to create a working organization as quickly as possible and this would not be achieved. Lawrence and Alvarez were dead against it. I told Compton that if we were faced with a choice between this plan and Berkeley and had no third alternative, I personally would much prefer to go to Berkeley in spite of the fact that I fully shared Fermi's objections to the West Coast. Therefore, if we had no third alternative, I would like to return to New York and make an attempt to win over Fermi to the Berkeley solution rather than split the project in this way. I emphasized that I believed that if Compton decided to move the Chicago group to Berkeley or to any other place which he considered suitable, we would all do our best to get Fermi's approval, and that I was rather confident that in such circumstances Fermi would overcome his strong preference for Columbia in the interest of concentration. I asked for a few days to accomplish this since it would obviously be wrong to decide in favor of a place as long as Fermi had a strong objection.

Compton gave us his reasons for thinking that Columbia University was not a suitable place for concentration. I told

Professor Compton that on Thursday, January 22nd, Anderson, Feld, Marshall, Zinn and I talked over this matter at great length. We came to the conclusion that immediate concentration under his exclusive direction is imperative. We realized that it is impossible to get anywhere with this question as long as each group exerts pressure for its own university. Therefore, we decided that each of us would let Compton know by telegram that our first loyalty is to the project, and therefore that if he were not satisfied that Columbia is the right place for this work, and if he wanted to move both the Columbia and Chicago groups to a third place of his choice, we felt that he ought to have the whole-hearted support of all of us. The telegrams were supposed to go out Thursday night. Unfortunately, we were unable to locate Fermi and it was felt that Fermi should have an opportunity to take a stand and send a wire to Compton if he so desired. The following morning we contacted Fermi, but instead of sending the telegrams I decided to fly to Chicago, acquaint Compton with the views expressed in those telegrams, and give him such additional information as appeared necessary.

I raised tentatively the question with Compton whether he would consider Harvard a possible place for the project, and also whether he would consider moving the Chicago group to Princeton and leaving the Columbia group for the time being at Columbia. Princeton being of commuting distance from New York, frequent meetings which might be arranged between the two groups could serve as a substitute for a complete concentration. I asked Compton for an opportunity to make a complete statement on the whole subject under discussion, and an interview was fixed for the next day, Saturday, at 10 a. m.

At this point, Lawrence, Alvarez and I left Compton and the three of us continued the conversation. Lawrence told me that in the circumstances he would no longer press for a concentration at Berkeley and asked Alvarez and me to tell Compton that, in his view, it would be best to concentrate at Columbia, Princeton, or Harvard, or any other convenient location which appeared acceptable. Alvarez stressed that, in his opinion, Harvard would be a very good place for the project.

I saw Compton Saturday morning as arranged and learned that he had decided in favor of concentration at Chicago and had already telephoned Conant and considered the matter as definitely settled. The text of the telegrams of January 22nd written by Anderson, Feld, Marshall, and Zinn, was then submitted to Compton. I expressed concern about the decision he had reached, and also about the manner in which the decision was arrived at. A full statement of my immediate reaction to it was given, and I said that I would communicate my final reaction after I had had an opportunity to consult with Fermi and Wigner.

L. Szilard

2nd set
May, 1969

Additional Material for pages 118-119

re: "I wrote a paper for publication and sent it to PHYSICAL REVIEW on February 16th ⌊1940⌋."

also re: Note No. 34.

Letter, L.S. to Tate — Feb. 6, 1940
Encloses short version, requests delay in publication.

Post Card, receipt for paper from PHYSICAL REVIEW — Feb. 8, 1940

Letter, Madeline M. Mitchell to L.S. — Feb. 14, 1940
Publications Manager confirms delay (short version).

Letter, L.S. to Tate — Feb. 14, 1940
Encloses full version, requests delay.

Post Card, receipt from PHYSICAL REVIEW — Feb. 19, 1940

Telegram, Tate to L.S. — Feb. 20, 1940
Full version received Feb. 16th.

L.S. to Tate — Apr. 5, 1940
Stresses delay. Inquires whether paper accepted.

Tate to L.S. — Apr. 8, 1940
Paper is accepted.

L.S. to Tate — May 23, 1940
Asks for still further delay, finally on request of the government, after the April 27th meeting of the Advisory Committee on Uranium.

~~L.S. to Lyman J. Briggs — May 23, 1940~~

5b5 20

February 6, 1940

John T. Tate, Editor
Physical Review
University of Minnesota
Minneapolis, Minn.

Dear Dr. Tate:

Enclosed you will find a manuscript which I am sending you with the request that you have it printed in the Physical Review as a "letter".

Since this manuscript deals with a matter in which the government has shown a certain amount of interest from the point of view of national defense it is felt that inquiries should be made in Washington as a matter of courtesy before the letter is actually printed. Would you, therefore, perhaps be kind enough to ask the Lancaster Press not to print this manuscript until they have a telegram from me releasing the matter for publication. I trust this way of proceeding will not cause any undue inconvenience.

Yours very truly,

(Leo Szilard)

S.b.s. 13

(2)

February 8, 1940

The Editors of THE PHYSICAL REVIEW acknowledge receipt of the following manuscript: Divergent Chain Reaction in Systems Composed of Uranium and Carbon. by Leo Szilard

Information concerning the publication of this article will be sent as soon as possible.

JOHN T. TATE, Editor,
THE PHYSICAL REVIEW,
University of Minnesota,
Minneapolis, Minnesota.

AMERICAN INSTITUTE OF PHYSICS
Incorporated
175 FIFTH AVENUE, NEW YORK



February 14, 1940

Dr. Leo Szilard
Pupin Physics Laboratories
Columbia University
New York, New York

Dear Dr. Szilard:

My attempt to reach you by telephone today was unsuccessful. This is to confirm, however, the message which I left for you, to the effect that we will hold the manuscript of your article "Divergent Chain Reaction in Systems Composed of Uranium and Carbon" in this office awaiting your release on it, before sending it to the printer to be set for THE PHYSICAL REVIEW.

Because of the fact that we publish eight different magazines and there are always several issues for each of them in the process of publication we do not like to complicate the printers work by requesting special treatment in the plant for one manuscript. Since you are in New York this method of handling your paper will not cause any special delay.

Very truly yours,

Madeline M. Mitchell

Madeline M. Mitchell
Publications Manager

m/g/pl

sbs 20

February 14, 1940

John T. Tate
The Physical Review
University of Minnesota
Minneapolis, Minnesota

Dear Dr. Tate:

Enclosed you will find a manuscript which I am submitting for publication to the Physical Review. There is just a chance that I might be requested by certain departments of the Administration to delay a publication of this paper though I personally do not think that this is very likely to happen. Still if you do not prefer to read the paper yourself, perhaps you might think it advisable to let the referee know about this possibility.

Pages 22, 23, and 24 of the manuscript deal with two experiments which I described in some detail because it is hoped that it will be possible to give the result of at least one of these experiments within a short time and the values found may be added in proof. Both experiments are in a state of preparation and might be completed by the time the paper appears in print.

Yours very truly,

(Leo Szilard)

Bk. f. 5

February 19, 1940

The Editors of THE PHYSICAL REVIEW acknowledge receipt of the following manuscript: Divergent Chain Reaction in a System Composed of Uranium and Carbon. by Leo Szilard

Information concerning the publication of this article will be sent as soon as possible.

JOHN T. TATE, Editor,
THE PHYSICAL REVIEW,
University of Minnesota,
Minneapolis, Minnesota.

Bk. f. 6 (28)

Class of Service

This is a full-rate Telegram or Cablegram unless its deferred character is indicated by a suitable symbol above or preceding the address.

# WESTERN UNION

1201

SYMBOLS

DL = Day Letter

NL = Night Letter

LC = Deferred Cable

NLT = Cable Night Letter

Ship Radiogram

R. B. WHITE PRESIDENT

NEWCOMB CARLTON CHAIRMAN OF THE BOARD

J. C. WILLEVER FIRST VICE-PRESIDENT

(53) 1940 FEB 20 PM 3 00

The filing time shown in the date line on telegrams and day letters is STANDARD TIME at point of origin. Time of receipt is STANDARD TIME at point of destination

NS103 10 COLLECT=MINNEAPOLIS MINN 20 133P

LEO SZILARD=

:420 WEST 116 ST=

96

YOUR MANUSCRIPT RECEIVED FEBRUARY 16TH. ACKNOWLEDGEMENT SENT FEBRUARY 19TH=

JOHN T TATE.

16 19.

Bk.f. 6 (10)

April 5, 1940

Dear Dr. Tate:

I am writing to you concerning the manuscript of a paper which was sent to you enclosed in my letter of February 14, 1940. I am anxious that this manuscript should not be sent to print until I have definitely heard from the Administration that there is no objection to its publication. In the meantime, however, I should be glad to know whether the manuscript has been accepted for publication in the Physical Review and perhaps you would be kind enough to inform me with regard to this point.

Yours very truly,

(Leo Szilard)

Bk. f. 6

THE PHYSICAL REVIEW
REVIEWS OF MODERN PHYSICS

*Conducted by*

THE AMERICAN PHYSICAL SOCIETY
JOHN T. TATE, *Managing Editor*

*University of Minnesota, Minneapolis, Minn., U. S. A.*

April 8, 1940

Dr. Leo Szilard
King's Crown Hotel
420 West 116 Street
New York, New York

Dear Dr. Szilard:

Your paper on "Divergent Chain Reaction in a System Composed of Uranium and Carbon" will be published in THE PHYSICAL REVIEW. On the other hand, I feel that it should be condensed somewhat, particularly in the introduction. I should think too that you might wish to modify parts of it in the light of more recent experiments, particularly those of Dunning and Nier.

Sincerely yours,

John T. Tate

John T. Tate,
Editor

JTT:B

N

bk. folder 6

C
O
P
Y

May 23, 1940

Dr. John T. Tate, Editor
The Physical Review
University of Minnesota
Minneapolis, Minnesota

Dear Dr. Tate:

I was asked by Dr. Briggs acting as chairman of a committee at which various Government departments are represented to delay the publication of those two manuscripts which I sent to the Physical Review dealing with the subject of chain reactions in systems composed of uranium and carbon. I gave the assurance that I would write you asking for a further delay concerning the publication of these papers which I am doing herewith.

In the circumstances it appears to me now likely that considerable time may elapse before these papers will be released. I shall, however, send you the revised manuscripts for which you asked and would be grateful if you would hold both manuscripts until such time as there will no longer be any objection to their publication.

Since work on this and related subjects is being intensified it appears likely that you will receive more papers with or without the request for a delay in publication in the near future. This may raise questions of principle and I propose therefore to discuss the matter with various colleagues and having obtained their reaction to take it up with Dr. Briggs so that he may inform you of his attitude as well as theirs.

Yours sincerely,

(Leo Szilard)

2nd set
May 1969

Additional Material for pages 118-119 and Notes 36-37

re: MEETING of BRIGGS COMMITTEE, APRIL 27, 1940

This material comes mostly from one of the ~~xxxxx~~ folders referred to in the headnote, labelled by Szilard: "Second Attempt to Reach White House, Febr to May, 1940."

re: "...we propose to take the position that I would publish my results unless the government asked me not to do so..." (page 118) see paragraph 4 of Einstein's letter to Sachs of March 7, 1940, already quoted on page 119. The letter in our file varies very slightly from the text printed in EINSTEIN ON PEACE.

Szilard, in the REMINISCENCES, specifically mentions the Einstein letter, Sachs' letter of transmittal to the President, and Roosevelt's reply. Other documents concern the ~~preparation~~ resulting meeting with Briggs on April 27, 1940, and the ~~problem of uranium ore supply~~ recommendations ~~coming from~~ following it.

"Second Approach to the President of the United States. March-April 1940" by L.S. (from the memo to Compton, 1942)

Letter, Einstein to Sachs, March 7, 1940. Photostatic copy.
" " " " " " " Retyped, for easier reading.

" " " " " " " Draft, marked "Not sent"

Letter, Sachs to the President, March 15, 1940, transmitting above.

Letter, Roosevelt to Sachs, April 5, 1940, suggesting a second meeting on uranium research.

Letter, Edwin M. Watson, Roosevelt's Secretary, to Sachs, April 5, 1940.

Letter, Sachs to Einstein, April 15, 1940. Urges Einstein to attend.

Memorandum by L.S. Included in letter, L.S. to Sachs, April 22, 1940. On "the present work on nuclear chain reactions...its possible bearing on questions of national defense."

Letter, Einstein to Briggs, April 25, 1940. Declines to attend meeting. Suggests setting up non-profit organization.

K.W.

(continued on next page)

Additional Material for pages 118-119 (continued)

"Statement by Dr. Sachs." Undated. Probably April 27, 1940 (kw)
Probably presented at the April 27th conference, (see end of first paragraph).
This statement of Sachs tells exactly the same story as Szilard tells in the REMINISCENCES, about events in the spring of 1940, ~~and~~ the paper sent to PHYSICAL REVIEW and its being held up.

Memorandum by George B. Pegram May 8, 1940
"Memorandum on the experiments with uranium and carbon that are bein done at Columbia University under the direction of Professor Fermi and Dr. Szilard." 4 pp. Marked "Copy".
Recommends survey of nuclear constants, at the same time planning for full-scale experiment. Cost estimates on page 4, for survey only.

Letter, L.S. to Sachs May 10, 1940
Recommendations similar to Pegram's above, but urges organization of large scale experiment.

Letter, L.S. to Briggs May 23, 1940
enclosing copy of
Letter, L.S. to John T. Tate May 23, 1940
Delay of publication of paper on chain reactions in a uranium-carbon system is finally official.

From Memo, L.S. to Compton, Nov. 12, 1942 K.W. bbs 49



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.

Bkf. 6 (15)

A. Einstein
112 Mercer Road
Princeton, N.J.

March 7, 1940

Dr. A. Sachs
c/o Lehman Corp.
1 South William St.
New York, N. Y.

Dear Dr. Sachs:

In view of our common concern in the bearings of certain experimental work on problems connected with national defense, I wish to draw your attention to the development which has taken place since the conference which was arranged through your good offices in October last year between scientists engaged in this work and government representatives.

Last year when it was realized that results of national importance might arise out of the research on uranium, I thought it my duty to inform the administration of this possibility. You will perhaps remember that in the letter which I addressed to the President I also mentioned the fact that C. F. von Weizsaecker, son of the German Undersecretary of State, von Weizsaecker, was collaborating with a group of chemists working upon uranium at one of the Kaiser Wilhelm Institutes, namely, the Institute of Chemistry. Since the outbreak of the war, interest in uranium has intensified in Germany. I have now learned that research there is carried out in great secrecy and that it has been extended to another of the Kaiser Wilhelm Institutes, the Institute of Physics. The latter has been taken over by the Government and a group of physicists, under the leadership of C. F. von Weizsaecker, who is now working there on uranium in collaboration with the Institute of Chemistry. The former director was sent away on a leave of absence apparently for the duration of the war.

Bk.f.6. (16)

Should you think it advisable to relay this information to the President, please consider yourself free to do so. Will you be kind enough to let me know if you are taking any action in this direction.

Dr. Szilard has shown me the manuscript which he is sending to the Physical Review in which he describes in detail a method for setting up a chain reaction in uranium. The papers will appear in print unless they are held up, and the question arises whether something ought to be done to withhold publication. The answer to this question will depend on the general policy adopted by the [illegible] with respect to uranium.

I have discussed with Professor Wigner of Princeton University and Dr. Szilard the situation in the light of the information available. Dr. Szilard will let you have a memorandum informing you of the progress made since October last year so that you will be able to take such action as you think in the circumstances advisable. You will see that the line he has pursued is different and apparently more promising than the line pursued by M. Joliot in France about whose work you may have seen reports in the papers.

Yours sincerely,

Albert Einstein
(Signed)

Bk. f. 6

C

O

P

Y

See especially
the fourth paragraph

A. Einstein
112 Mercer Road
Princeton, N.J.

March 7, 1940

Dr. A. Sachs
c/o Lehman Corp.
1 South William St.
New York, N.Y.

Dear Dr. Sachs:

In view of our common concern in the bearings of certain experimental work in problems connected with national defense, I wish to draw your attention to the development which has taken place since the conference that was arranged through your good offices in October last year between scientists engaged in this work and governmental representatives.

Last year when I realized that results of national importance might arise out of the research on uranium, I thought it my duty to inform the Administration of this possibility. You will perhaps remember that in the letter which I addressed to the President I also mentioned the fact that C.F. von Weizsaecker, son of the German Secretary of State, von Weizsaecker, was collaborating with a group of chemists working upon uranium at one of the Kaiser Wilhelm Institutes, namely, the Institute of Chemistry. Since the outbreak of the war, interest in uranium has intensified in Germany. I have now learned that research there is being carried out in great secrecy and that it has been extended to another of the Kaiser Wilhelm institutes, the Institute of Physics. The latter has been taken over by the Government and a group of physicists, under the leadership of C.F. von Weizsaecker, who is now working there on uranium in collaboration with the Institute of Chemistry. The former director was sent away on a leave of absence apparently for the duration of the war.

Should you think it advisable to relay this information to the President, please consider yourself free to do so. Will you be kind enough to let me know if you are taking any action in this direction.

Dr. Szilard has shown me the manuscript which he is sending to the Physics [sic] Review in which he describes in detail a method for setting up a chain reaction in uranium. The papers will appear in print unless they are held up, and the question arises whether something ought to be done to withhold publication. The answer to this question will depend on the general policy which is being adopted by the Administration with respect to uranium.

I have discussed with Professor Wigner of Princeton University and Dr. Szilard the situation in the light of the information that is available. Dr. Szilard will let you have a memorandum informing you of the progress made since October last year so that you will be able to take such action as you think in the circumstances advisable. You will see that the line he has pursued is different and apparently more promising than the line pursued by Monsieur Joliot in France about whose work you may have seen reports in the papers.

Yours sincerely,

Albert Einstein (signed)

Typed from photostat by R.W.
Dec 1967

Not sent

Sb. 9
bk. folder 6

March 7, 1940

? Earlier draft of letter on preceding page
BWS

Dr. Alexander Sachs
c/o Lehman Corporation
One South William Street
New York City

Dear Dr. Sachs:

I understand that you are familiar with the situation which has arisen in connection with the study of uranium, and that thanks to your disinterested intervention in October some support will now be forthcoming for certain experiments on uranium.

Last year, when I realized the danger which might arise out of this situation, I thought it my duty to draw the attention of the administration to this point. You will perhaps remember that in the letter which I addressed to the President I also mentioned the fact that C.F. von Weizsaecker, son of the German Secretary of State von Weizsaecker, was collaborating with a group of chemists working on uranium at one of the Kaiser Wilhelm Institutes, namely the Institute of Chemistry. I have now learned that this research is being carried out in great secrecy, and that it has been extended to another of the Kaiser Wilhelm Institutes, the Institute of Physics. This Institute has been taken over by the government, and a group of physicists is now working there on

Not sent

uranium under the leadership of Weizsaecker in collaboration with the group of chemists at the Institute of Chemistry.

Should you think it advisable to relay this information to the administration in Washington, please consider yourself free to do so. Would you perhaps be kind enough to let me know whether you intend to forward this information?

Dr. Szilard has shown me the manuscripts of two papers which he has sent to the Physical Review. A method for setting up a chain reaction is described in detail in these papers which will appear in print in the near future unless something is done to prevent their publication.

I have discussed this and other aspects of the situation with Dr. Wigner of Princeton University and Dr. Szilard in the light of the information which is at present available. They will let you have a short memorandum informing you of the progress made since October last year, so that you may be able to take such action as you think necessary in the circumstances. You will see that the work of Dr. Szilard has proceeded along a line entirely different from that pursued by Joliot in France, about whose work you may have seen reports in the papers.

Yours very truly,

From Einstein (K.W.)

program under the leadership of Weizsacker in collaboration with the group of chemists at the Institute of Chemistry.

Should you think it advisable to relay this information to the administration in Washington, please consider yourself free to do so. Would you perhaps be kind enough to let me know whether you intend to forward this information?

publ. prior

Dr. Szilard has shown me the manuscripts of two papers which he has sent to the Physical Review. A method for setting up a chain reaction is described in detail in these papers which will appear in print in the near future unless something is done to prevent their publication.

I have discussed this and other aspects of the situation with Dr. Wigner of Princeton University and Dr. Szilard in the light of the information which is at present available. They will let you have a short memorandum informing you of the progress made since October last year, so that you may be able to take such action as you think necessary in the circumstances. You will see that the work of Dr. Szilard has proceeded along a line entirely different from that pursued by Joliot in France, about whose work you may have seen reports in the papers.

Yours very truly,

Bk.f. 6 (17)

March
~~October~~ 15, 1940

Dear Mr. President:

As a sequel to the communication which I had the honor to submit to you on October 12, Professor Albert Einstein sent me another regarding the latest developments touching on the significance of research on uranium for problems of national defense. In that letter he suggests that I convey to you the information that has reached him that since the outbreak of the war, research with uranium is being carried out in great secrecy at the Berlin Institute of Physics, which has been taken over by the Government and placed under the leadership of C. F. von Weissaecker, son of the German Secretary of State.

In the realization that these further views of Dr. Einstein have a definite bearing on the favorable report submitted to you by Dr. Briggs as Chairman of the Committee which conferred with experimental scientists concerned and myself, I am enclosing his communication for your kind perusal. May I also ask whether and when it would be convenient for you to confer on certain practical issues brought to a focus by the very progress of the experimental work, as indicated in the concluding paragraph of Dr. Einstein's letter.

In view of your original designation of General Watson in this matter, I am transmitting it through his good offices.

Yours sincerely,

[Sachs
gprs]

The President
The White House
Washington, D. C.

Bk.f. 6 (13)

C
O
P
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THE WHITE HOUSE
WASHINGTON

April 5, 1940

My dear Dr. Sachs:

I am grateful for your letter of March fifteenth enclosing the information from Dr. Einstein regarding the recent development in Uranium research. I have asked my Secretary, General Watson, to arrange another meeting in Washington at a time convenient for you and Dr. Einstein. I think Dr. Briggs should be included, and special representatives from the Army and Navy.

I am of the opinion that this is the most practical method of continuing this research, and I shall always be interested to hear the results.

Very sincerely yours,

FRANKLIN D. ROOSEVELT

Dr. Alexander Sachs,
One South William Street,
New York, N. Y.

COPY

Bk.f. 6. (14)

C
O
P
Y

THE WHITE HOUSE

WASHINGTON

APRIL 5, 1940

Dear Dr. Sachs:

In order to carry out the suggestions of the President's letter to you today, will you please let me know who you think ought to be at the conference, any professors, and when exactly would be most convenient to all concerned. It strikes me perhaps Dr. Einstein would have some suggestions to offer as to the attendance of the other professors. I believe it would be quite appropriate to hold this meeting at the Bureau of Standards.

If you will give me fully your reactions on all this, I will proceed to get into action.

With best wishes, I am,

Very sincerely yours,

EDWIN M. WATSON
Secretary to the President

Dr. Alexander Sachs,
One William Street,
New York, N. Y.

Bk. f. 6 (29)

File

April 15, 1940

Dear Dr. Einstein:

In connection with your important communication of March 7th in regard to the research in uranium and its bearing on national defense, I wrote to the President on March 15th, as per enclosed copy, and have at first received an acknowledgment from his secretary, General Watson. It would appear that upon his return to Washington after his trip to the Canal Zone, he decided to adopt the procedure suggested in my original communication. Accordingly, I received on Saturday, April 13th, a letter of his dated April 5th which was postmarked from Washington on April 12th, 5:30 P.M., - a delay which is understandable in view of the tragic international occurrence of the intervening week. In the wake of that letter I also received on the 13th a note from General Watson dated the 5th, and, in furtherance of a telephone call on Saturday, Dr. Briggs's letter of the 13th.

Naturally, having been brought into the orbit of this problem by Dr. Szilard, I have been in continuous touch with him at every stage of the developments and over this weekend and particularly today we have discussed aspects of the appropriate procedure for the forthcoming conference which the President has instructed General Watson and Dr. Briggs to arrange in conformity with the ideas implicit in your original letter. May I add that in the interest of assuring an adequate scale for the experimentation and a right tempo for the work it will be most helpful if you could see your way to attending, along with Drs. Wigner and Szilard, as I am sure that the President would feel all the more confident and would be delighted to know that any program that is worked out will have had your sagacious cooperation and your approval.

I am looking forward to seeing you and conferring with you before the meeting which, owing to the exigencies of conference and the development of a coordinate policy, might require postponement.

Yours sincerely,

Dr. Albert Einstein,
112 Mercer Road,
Princeton, N. J.

from Sachs.
(K.W.)

No Ph

Bk. f. 6 (6)

420 West 116th Street
New York City

April 22, 1940

Dr. Alexander Sachs
c/o Lehman Corporation
One South William Street
New York City

Dear Dr. Sachs:

In accordance with the letter written to you by Professor Einstein on March 7, I am submitting to you in the following a memorandum dealing with the present work on nuclear chain reactions. Only one aspect of the subject is discussed in this memorandum, namely its possible bearing on questions of national defense.

Memorandum.

We have to discuss separately two different types of chain reactions, i. e.

a) chain reactions in which the neutrons are slowed down, and in which only a small fraction of the uranium can be utilized, corresponding to the content of uranium 235 in ordinary uranium; (if ordinary uranium is used for the purposes of such a chain reaction, a ton of uranium will be exhausted after having supplied as much energy as corresponds to the burning of about 3000 tons of oil)

b) chain reactions in which the neutrons are not slowed down and in which the bulk of the ordinary uranium could be utilized; (if it were possible to maintain a chain reaction of this type in uranium, one ton of uranium could supply more energy than 300.000 tons of oil.

There is reason to expect that a chain reaction of the type described under a) can be maintained in a system composed of uranium and carbon.

BK.f. 6 (7)



Whether or not a chain reaction of the second type, as discussed under b), can be maintained in uranium is not known and has for the present to be considered an open question which, in view of its far reaching consequences, urgently requires further study.

Part I.

Chain Reactions maintained in Systems composed of Carbon and Uranium.

A chain reaction of this type is capable of applications which may have a bearing on questions of national defense.

1. A system composed of carbon and uranium might be used for purposes of power production. Questions relating to the transformation into power of the energy liberated in the chain reaction as well as questions relating to the regulation of the chain reaction have been studied, and methods for solving these problems have been devised.

Personnel has to be protected from being exposed to the radiations emanating from the chain reaction by means of water tanks, and such an atomic engine equipped in this way could be used as a power reserve in larger naval units. The weight of the water tanks rules out the possibility of using an atomic engine for the purpose of driving aeroplanes.

One ton of uranium would be capable of supplying about as much power as 3000 tons of oil. For instance, a 30.000 ton battleship, which would ordinarily have a maximum oil load of 4000 tons could in the future be equipped for the use of both oil fuel and atomic power and would carry perhaps 1000 tons of oil and 50 tons of uranium, the latter representing the equivalent of an oil reserve of about 150.000 tons. Accordingly, such a boat would have a practically unlimited cruising radius.

Since a battleship equipped with an atomic engine need not carry in war-time more than a normal oil load of perhaps 1000 tons, there would result a saving in weight, even if allowance is made for the weight of the atomic engine. This saving in weight would lead to an increase in the top speed of the vessel.

The limited supply of uranium would make it inadvisable to use up any considerable amounts for naval purposes in peace time, and the atomic engines with which battleships may be equipped must not be used except occasionally in maneuvers and in case of actual warfare. Since a large battleship or battle-cruiser will use more than ½ ton of oil per mile if cruising at an economical speed, it would exhaust its full oil load of about 4000 tons during a cruise covering about 10.000 miles. This means that a fast ship can not operate for any length of time at a distance of about 4-5000 miles from its nearest base. The advantage of a battleship having an equivalent of an oil reserve of 150.000 tons would in these circumstances be decisive, since apart from the increased speed it could stay for a long period near its objective at any distance from its base.

2. A system composed of carbon and uranium may be used as a weapon in the following manner: A chain reaction may be maintained in this system and the neutrons emanating from the chain reaction may be allowed to escape. The intensity of the neutron radiation could be made so high that this radiation would fatally injure by its physiological action human beings who are exposed to it within a radius of one kilometer. By mentioning this fact it is not desired to imply that such a system represents a desirable or particularly efficient military weapon. The reason for emphasizing this point lies rather in the belief that such a system could be used as a weapon by some other country during the present war, possibly in the near future, and that it could be used with considerable effect on a country which is not prepared to meet this new type of attack.

Bk.f.6 (9)



Part II.

Chain Reactions in which the Neutrons Are Not Slowed Down.

It is not known at present whether or not chain reactions of this type can be brought into existence. If, however, this could be done they would have a bearing on questions of national defense, going in their scope of applications far beyond the applications discussed in Part I.

1. In a chain reaction of this second type one ton of uranium used as driving power in a warship could supply more power than 300,000 tons of oil. Consequently, it would probably be possible for the larger types of naval vessels to dispense entirely with the use of oil.

2. A chain reaction of this second type would make it possible to bring about explosions of extraordnary intensity. If for purposes of aggression, a bomb baded on such a chain reaction were set off at sea near the coast, the tidal waves brought about by the explosion might lead to the destruction of coastal cities. Such a a bomb would not be too heavy to be carried by small boats, but could hardly be carried by existing airplanes.

Yours sincerely

Bk.f. 6 (12)

April 25, 1940

Dr. Lyman J. Briggs, Director,
National Bureau of Standards,
U. S. Department of Commerce,
Washington, D. C.

Dear Dr. Briggs:

I thank you for your recent communication concerning a meeting of the Special Advisory Committee appointed by President Roosevelt.

As, to my regret, I shall not be able to attend this meeting, I have discussed with Dr. Wigner and Dr. Sachs particularly the questions arising out of the work of Dr. Fermi and Dr. Szilard. I am convinced as to the wisdom and the urgency of creating the conditions under which that and related work can be carried out with greater speed and on a larger scale than hitherto. I was interested in a suggestion made by Dr. Sachs that the Special Advisory Committee submit names of persons to serve as a board of trustees for a non-profit organization which, with the approval of the Government committee, should secure from governmental or private sources, or both, the necessary funds for carrying out the work. It seems to me that such an organization would provide a framework which could give Drs. Fermi and Szilard and co-workers the necessary scope. The preparation of the large scale experiment and the exploration of the various possibilities with regard to practical applications is a task of considerable complexity, I think that given such a framework and the necessary funds, it could be carried out much faster than through a loose cooperation of University laboratories and Government departments.

Yours sincerely,

Copy to Dr. Sachs

[from Einstein - K.W.]

Probably presented at Briggs' conference, April 27th, 1940, (KW)

bk. folder 6

Statement by Dr. Sachs.

Early in March I ~~xxx~~ received a letter by Dr. Einstein, in which he informed me that he had learned from reliable sources that work on uranium in Germany is being carried out in great secrecy and on a very large scale. I understand that this information is confirmed by Prof. Debye who recently came from Germany. Dr. Einstein wrote me that Dr. Szilard has written a detailed paper on the possibility of chain reactions in a system composed of uranium and graphite, and that this paper has been sent to the Physical Review, and Dr. Einstein raised the question of secrecy in connection with all this work. At the same time, Dr. Einstein asked Dr. Szilard to submit a memorandum on the possible bearing of Dr. Fermi's and Dr. Szilard's work on questions of national defense, which memorandum I have in the meantime received. Accordingly, I submitted his communication to the President, and upon the President's return from his trip in the Canal Zone I was advised by him that he had asked his Secretary, General Watson, to arrange another meeting in Washington, with Dr. Briggs and the representatives of the Army and Navy ~~anxthexanaxhand~~ and others. General Watson, on the same date, asked for suggestions from Dr. Einstein and myself as to the supplemantary names for attendance at this conference, and so, through the kind offices and direction of Dr. Briggs, this conference has resulted.

This week, having heard from Dr. Einstein that he could not attend, I had the pleasure of calling on him and hearing his views. He told me that he had discussed the scientific aspects with Dr. Wigner and emphasized his conviction as to the importance of creating conditions under which the work can be pursued on an adequate scale. He also discussed some aspects of organization and sought my views on that, but this can be deferred for a later stage of this conference.

Congo 100000 lbs

Canada 27000

Checkosl 27000

Portugal 88000

1938 U. S. 51000

1937 Canada 200000 lbs

Ra Canada 70 gr

$\frac{235,3}{705}$

$\frac{16,8}{128}$

100 tons graphite
6 " U.

[on verso of Statement by Dr. Sachs] K. W.

COPY

Bk.f. 6 (18)

May 8, 1940

Memorandum on the experiments with uranium and carbon that are being done at Columbia University under the direction of Professor Fermi and Dr. Szilard.

I. General

The project of attempting to bring to practical use the release of energy from uranium "by a chain reaction" involving the use of carbon to slow down the neutrons released in the reaction so that they may be picked up by the Uranium-235 involves three rather distinct stages.

A. The first stage is an investigation of the fundamental physics involved in the scheme by which it is hoped to produce and utilize the chain reaction. The experiments in this stage can be done as effectively in a university laboratory as anywhere. Since the whole field is rather new, it is especially necessary at this stage to have the work in charge of physicists of imagination and ingenuity. It would not seem necessary to surround the experiments at this stage with precautions to secure great secrecy. In general the results of the experiments are not immediate but come out only from computations by rather laborious methods and the results of these computations need be known to very few and need not be disclosed to others. Some of the experiments that are needed are such as various physicists may be undertaking and of which they may publish the results. However, since the results are necessary to this project it can hardly wait until some physicist happens to do the particular experiments necessary for a better knowledge of the facts concerned. As these fundamental measurements are made they may at any stage do one of three things, namely, either indicate that the project is hopeless, lie within a range which indicates neither the feasibility nor the unfeasibility of the project, or lie within a range that indicates the project to be feasible and encourages further steps. If the work in this stage is pushed hard it will still take a number of months to get the results that should be well in hand before the final stage is reached.

B. The second stage is that of planning for the third or final stage. Work in this second stage need by no means await the completion of work in the first stage but may well begin at once. This work will start with the assumption that within certain limits of quantities of uranium and carbon the chain reaction with slow neutrons can be made to proceed at a rapid but controllable rate. If this assumption is made then the second stage is really one of engineering planning anddesigning. The constant advice of physicists will be needed but the designing of the setup of an actual experiment, preferably with the appropriate means for utilizing the energy liberated will involve good engineering judgment and designing. The second stage includes a study of the supply and the means of acquiring uranium, and incidentally carbon, in the proper form. A study of the limit to the supply of uranium is obviously of importance. The results of work in this second stage would presumably be kept confidential by whatever organization does the work. The work in this stage might be done at a university but the desirability of doing so is by no means as obvious as is the case with stage one.

C. The third stage is that of constructing the necessary equipment and setting up an experimental plant for trying out the final experiment if it appears to be justified by the findings in the first stage. If the second stage, that of planning the full scale experiment, goes on simultaneously with the first stage, the final experiment might be tried rather promptly if and after the results of the fundamental experiments in the first stage have indicated the conditions for success of the project. The third stage is an undertaking that should be carried out in some isolated location, certainly not on a university campus.

II. Experiments of the First Stage

In the first or physics stage a more accurate knowledge of certain properties of the materials used is the objective. The chief of these properties are the following:

(a) The magnitude of the "capture cross-section" of a carbon atom with slow neutrons. If this capture cross-section is too great, the carbon would swallow up so many of the slow neutrons in the mixture of uranium and carbon that there would be no hope of a chain reaction.

Since this capture cross-section was known with less accuracy than any of the other quantities involved it was the subject of the first experiment. Preliminary results have been secured. Further results will be ready by the end of this week. The indications are that carbon does not capture slow neutrons at so rapid a rate that the feasibility of a chain reaction is excluded. It is not certain yet whether another more elaborate experiment using a sphere of carbon should be carried out with the hope of somewhat greater accuracy in the measurement of the capture cross-section of carbon.

(b) The average number of neutrons that are given off when a fission of a Uranium-235 atom takes place after its capture of a slow neutron. Additional experiments to determine more accurately this fundamentally important number need to be done. The previous experiments, particularly those of Anderson and Fermi, and those in France of Joliot, are not in very good agreement.

(c) Improved measurements of the fission cross-section of Uranium-235. Measurements of this quantity have been made but it would be advantageous to have the results more accurately worked through.

(d) The resonance capture cross-section of Uranium-238. The capture of neutrons at a certain velocity (semi-slow) by Uranium-238 is quite the worst obstacle to getting a chain reaction between the slow neutrons and Uranium-235 to proceed. Anderson, who by the way is Professor Fermi's research assistant, in this laboratory, and others, have made some measurements on this resonance cross-section. Experiments to check the results would be desirable.

(e) Other capture cross-sections. Professor Fermi is inclined to suspect the existence of one or more other kinds of capture of neutrons by uranium than the fission capture and the resonance capture.

(f) A good many questions relating to the chemistry and metallurgy of uranium will need to be investigated. This investigation may partly belong to stage one but will also be prominent in stage two in connection with planning for obtaining uranium in the necessary form for use in the final experiment.

III. Resources needed to speed the work on the project.

Stage 1. The first or physics stage of the project needs in order to expedite the work at least the following: For research assistance, four young physicists who are acquainted with this type of work, men who would have salaries of approximately $3,000 each for at least a year. If it were felt necessary to proceed very cautiously, considerable progress might be made through the summer months from June 15 to September 15 by the employment of physicists from university staffs who might be willing to work through the vacation period. In addition, there would need to be funds available for supplies for the expense of shop work and construction of various pieces of equipment. It is difficult to estimate what the cost of these items would be, probably of the order of a $1,000 a month for the better part of a year. Very likely too it would be necessary to rent a gram of radium which surrounded by beryllium would furnish a steady concentrated source of neutrons. I believe that a gram of radium can be rented for about $300 a month.

Stage 2. I am not prepared to suggest any estimate as to the cost of stage two. Engineers to do most of the work with physicists to advise would be the chief requirements.

Stage 3. No estimate is submitted at this time as to the cost of stage three.

George B. Pegram

Bk. f. 8 (5)

420 West 116th Street
New York City
May 10, 1940

Dr. Alexander Sachs
c/o Lehman Corporation
One South William Street
New York City

Dear Dr. Sachs:

Our work concerning systems composed of carbon and uranium has now reached a stage at which it seems necessary to organize a large scale experiment. Only through actually carrying out such an experiment can it be demonstrated beyond doubt that a nuclear chain reaction can in fact be maintained in a system composed of carbon and uranium.

Since it appears necessary and urgent to obtain certainty in this matter we desire to start organizing a large scale experiment. This experiment would require about 100 tons of graphite and perhaps 10 to 20 tons of uranium metal. It would also require elaborate mechanisms designed to stabilize the chain reaction and to safeguard against over-heating and the possibility of an explosion. Realizing that this is an enterprise which may require to its conclusion an expenditure of $200,00 to $500,000, we propose to carry out this project in successive stages. If the results obtained during the first stage are satisfactory, then the expenditure necessary for the second stage would appear to be justified, and the second stage could be started according to schedule, etc. If this procedure were adopted, then the expenditure would gradually rise parallel to the increase in our assurance of the smooth functioning and the final success of the large scale experiment.

In the first stage we would propose to carry out a general survey of all nuclear constants involved with a view to confirming the values previously obtained and to narrowing down the limits of experimental error of the observed values of these constants. A successful conclusion of this survey would strengthen our assurance of the ultimate success of the experiment and would enable us to find the optimum condition for its performance. Concurrently, with this survey, certain other work would have to be done in order to prepare the ground for the experiment. Such work would include the designing of constructional details, the carrying out of technological tests on samples of materials which have to be used in large quantities in the ultimate experiment, and obtaining bids for the manufacturing of such material in the required quality and quantity. An expenditure of $50,000 would probably be sufficient to bring this first stage in the organization of the large scale experiment to its conclusion, so that we would be in the position of entering into the second stage of the work, provided that the result of the proposed survey of the nuclear constants is favorable. In this second stage the expenditure would gradually rise and might reach a total of $500,000 by the time the large scale demonstration experiment will be completed.

If a fund were set up under the direction of a board of trustees who had the confidence of the Government, as set forth in the letter by Dr. Einstein that was written following his conversations with you and read by Dr. Briggs at the meeting of April 27, Dr. Fermi and I would be glad to accept the responsibility of carrying out this work under the direction of such a board, and would be pleased to have our work supervised by a small committee of scientists who might be entrusted with the task of advising the board.

In my personal opinion, it would be advisable that the proposed small committee of scientists be left some latitude in devoting, as was suggested at the last conference, up to 25% of the total expenditure for investigating the possibility of a fast neutron reaction. It is further my personal opinion that, if the study of the separation of the uranium isotope were to be included in the program of the work, then Dr. Urey of Columbia and Dr. Beans of the University of Virginia ought to be asked to accept the responsibility for the direction or coordination of this line of work in the same way in which Dr. Fermi and I are prepared to take upon ourselves the responsibility in connection with the work on commercial, unseparated uranium.

Yours sincerely,

Leo Szilard
(Signed)

s.b.s.
bk. folder 6

May 23, 1940

Dr. Lyman J. Briggs, Director
National Bureau of Standards
Washington, D. C.

Dear Dr. Briggs

Enclosed you will find a copy of a letter to Dr. Tate which I wrote in pursuance of the course upon which we decided during our last discussion on April 27.

Yours sincerely,

(Leo Szilard)

LS/jbc
ENC.

bk. folder 6

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May 23, 1940

Dr. John T. Tate, Editor
The Physical Review
University of Minnesota
Minneapolis, Minnesota

Dear Dr. Tate:

I was asked by Dr. Briggs acting as chairman of a committee at which various Government departments are represented to delay the publication of those two manuscripts which I sent to the Physical Review dealing with the subject of chain reactions in systems composed of uranium and carbon. I gave the assurance that I would write you asking for a further delay concerning the publication of these papers which I am doing herewith.

In the circumstances it appears to me now likely that considerable time may elapse before these papers will be released. I shall, however, send you the revised manuscripts for which you asked and would be grateful if you would hold both manuscripts until such time as there will no longer be any objection to their publication.

Since work on this and related subjects is being intensified it appears likely that you will receive more papers with or without the request for a delay in publication in the near future. This may raise questions of principle and I propose therefore to discuss the matter with various colleagues and having obtained their reaction to take it up with Dr. Briggs so that he may inform you of his attitude as well as theirs.

Yours sincerely,

(Leo Szilard)

2nd set
May 1969

Additional Material for page 120 (1)

re: FERMI CORRESPONDENCE, JULY, 1939

Letters:

| | |
|---|---|
| L.S. to Fermi | July 3, 1939 |
| L.S. to Fermi | July 5, 1939 |
| L.S. to Fermi | July 8, 1939 |
| Fermi to L.S. | July 9, 1939 |
| L.S. to Fermi | July 11, 1939 |
| Fermi to George Pegram (marked: Copy) | July 11, 1939 |
| Fermi to Herbert Anderson ( " " ) | July 18, 1939 |

Two additional letters from Fermi to Szilard, dated June 26th and July 1st, 1939, are with the page 111 material.

Bk. f4 4

Hotel King's Crown
420 West 116th Street
New York City

July 3rd, 1939

Dear Fermi:

This is to keep you informed of the trend of my ideas concerning chain reactions. It seems to me now that there is a good chance that carbon might be an excellent element to use in place of hydrogen, and there is a strong temptation to gamble on this chance. The capture cross-section of carbon is not known: the only experimental evidence available asserts an upper limit of 0.01 times $10^{-24}$ cm$^2$. If the cross-section were 0.01 carbon would be no better than hydrogen, but the cross-section is perhaps much smaller, and it might be for instance 0.001. If it were so carbon not only could be used in place of hydrogen, but would have great advantages, even if a chain reaction were possible with hydrogen also. The concentration of uranium oxide in carbon could be kept very low, so that one could have about 2 gm of carbon per cc. This compares favorably with 1/2 gm of water per cc at the most and means that the mean square of the displacement of a neutron for slowing down to thermal velocities would be only 1.5 times as large in the carbon-uranium-oxide mixture than in the water-uranium-oxide mixture. If capture by carbon can be neglected, the concentration of uranium oxide is determined by the consideration that the average displacement of a thermal neutron for capture by uranium in the mixture must not become too large. With this as a limiting factor about 1/10 of the weight of the mixture would have to be uranium, and that means that one would need only a few tons of uranium oxide if our present data about uranium are correct.

I personally would be in favor of trying a large scale experiment with a carbon-uranium-oxide mixture if we can get hold of the material.

I intend to plunge in the meantime into an experiment designed for measuring small capture cross-sections for thermal neutrons. This is the proposed experiment: A sphere of carbon of 20 cm radius or larger is surrounded by water and a neutron source is placed in the center of the sphere. The slow neutron density is measured inside the carbon sphere by an indium or rhodium indicator at two points, one close to the surface, and one close to the center. The slow neutron density at these two points is measured once with, and once without, an absorbing layer of boron (or cadmium), covering the surface of the sphere. It is easy to calculate from the observed ratio of the differences (of the observed neutron density with and without absorber at the surface of the sphere) obtained for the two points and the scattering cross-section the ratio of the capture cross-section to the scattering cross-section for thermal neutrons. I calculate that a ratio of the neutron densities of the order of magnitude of 75 to 100 would for instance be obtained for two points in a sphere of carbon of about 20 cm radius if the capture cross-section of carbon were 0.005. It seems that very

Bk, f, 4, ⑥



small capture cross-sections can conveniently be measured by this method.

If carbon should fail, our next best guess might be heavy water, and I have therefore taken steps to find out if it is physically possible to obtain a few tons of heavy water. Heavy hydrogen is supposed to have a capture cross-section below 0.003, and the scattering cross-section ought to be 3 or 4 times $10^{-24}$ for neutrons above the 1 volt region. (It is 6 to 7 times $10^{-24}$ for the thermal region). Since heavy hydrogen slows down about as efficiently per collision as ordinary hydrogen, and since hydrogen has a capture cross-section of 0.27 and a scattering cross-section of 20, heavy hydrogen is more favorable.

Yours,

(Leo Szilard)

BKf. 1 7

Hotel King's Crown
420 West 116th Street
New York City

July 5th, 1939

Dear Fermi:

I think the letter I wrote you on July 3rd contains a mistake insofar as the ratio of the thermal neutron density at the center of the sphere and at the surface of the sphere is not 75 to 100, but 95 to 100 for the values given in that letter. The thermal neutron density within the sphere obeys the equation

$$D \frac{d^2(r\rho)}{dr^2} - A(r\rho) = 0$$

with

$$D = \frac{w\lambda_{sc}}{3} \; ; \; A = \frac{w}{\frac{\sigma_{sc}}{\sigma_c}\lambda_s}$$

it is:

$$\rho(r) = C\,\frac{e^{ar} - e^{-ar}}{r}$$

where

$$a = \frac{\sqrt{3}}{\lambda_{sc}}\sqrt{\frac{\sigma_c}{\sigma_{sc}}}$$

For small $ar$ we have $\rho(r) = 2aC\left(1 + \frac{a^2r^2}{6}\right)$ and the ratio of the densities on the surface and in the center is given by $\frac{\rho(r)}{\rho(0)} = \frac{e^{ar} - e^{-ar}}{2ar} \cong 1 + \frac{a^2r^2}{6}$

For $r$ = 20 cm, $\lambda_{sc}$ = 2 cm, and $\frac{\sigma_c}{\sigma_{sc}} = \frac{1}{1000}$ we have

$$\frac{\rho(20)}{\rho(0)} \cong 1.05$$

As you see, the method is beginning to get somewhat awkward in the case of carbon for smaller capture cross-sections than 0.005. It seems that it will be possible to get sufficiently pure carbon at a reasonable price. Carbon would also have an advantage over hydrogen insofar as there is no change in the scattering cross-section in the transition from the resonance region to the thermal region. Consequently, if layers of uranium oxide of finite thickness are used, the diffusion of the thermal neutrons produced in the carbon to the uranium layer is not adversely affected as in the case of hydrogen by such a change. Whether this point is of any importance depends of course on the absolute value of the carbon cross-section. Pending reliable information about carbon we ought perhaps to consider heavy water as the "favorite", and I shall let you know as soon as I can how many tons could be obtained within reasonable time.

With kind regards to all,

Yours,

(Leo Szilard)

Bk.f. 4 (9) (2)

Hotel King's Crown
420 West 116th Street
New York City

July 8th, 1939

Dear Fermi:

Sorry to bombard you with so many letters about carbon. This is just to tell you that I have reached the conclusion that it would be the wisest policy to start a large scale experiment with carbon right away without waiting for the outcome of the absorption measurement which was discussed in my last two letters. The two experiments might be done simultaneously. The following can be said in favor of this procedure:

A chain reaction with carbon is so much more convenient and so much more important from the point of view of applications than a chain reaction with heavy water or helium that we must know in the shortest possible time whether we can make it go. This can be decided with certainty in a relatively short time by a large scale experiment, and therefore this experiment ought to be performed. If we waited for the absorption measurement we would lose three months, and in case the result is positive we would still not know with a 100% certainty the answer with respect to the question of the chain reaction.

I thought that perhaps 50 tons of carbon and ~~500~~ 5 tons of uranium should be used as a start. The value of the carbon would only be about $ 10.000.- Since the carbon and the uranium oxide would not be mixed but built up in layers, or in any case used in some canned form, there will be no waste of material or waste of labor involved in unmixing after the experiment is over. Since the uranium layers may be separated by carbon layers of 20 to 30 cm thickness, or even more, we have to deal with a comparatively simple stmucture. Much simpler than would be the case for alternating water and uranium layers.

I told Professor Pegram yesterday how I felt about the situation, and he seemed to be not unwilling to take the necessary action. I wonder whether you think it wise to proceed as outlined in this letter.

With kindest regards,

yours,

(Leo Szilard)

Bk f. 4

**UNIVERSITY OF MICHIGAN**
**ANN ARBOR**

DEPARTMENT OF PHYSICS

July 9 1939

Dear Szilard,

Thank you for your letter. I was also considering the possibility of using carbon fo slowing down the neutrons; in the obviously optimistuc hypothesis that carbon should have no absorption at all for neutrons, and assuming for the resonance absorption band of uranium the usual data (which also I rather suspect to be optimistic) one finds from an elementary calculation that the ratio of the concentrations (ratio of the numbers of atoms) of uranium and carbon should be about one thousandth in order to avoid too much resonance absorption. According to my estimates a possible recipe might be about 30000 Kg. of carbon mixed with 600 Kg of uranium. If it were really so the amounts of materials would certainly not be too large.

Since however the amount of uranium that can be used, especially in a homogeneous mixture is exceedingly small, even a very small absorption by carbon either at thermal energy or even before might be sufficient for preventing the chain reaction; perhaps the use of thick layers of carbon separated by layers of utanium might allow to use a somewhat larger percentage of uranium.

I have been thinking about the experiment that you propose for measuring the small absorption cross section in carbon. It seems to me that you have probably over estimated the difference between rand and center activity in the carbon sphere; moreover I dont see how you can take into account the contribution of those neutrons that become thermal due to impacts against carbon. Their number should probably not be very large, but might disturb very considerably the measurement of a small difference.

I had discarded heavy water as too expensive; but if you can easily get several tons of it it might work very nicely.

The cyclotron here will start working again next week and I hope to be able to get reliable information on the so called resonance absorption of uranium. I shall inform you of the results.

Yours sincerely

Enrico Fermi

Enrico Fermi

P.S. I have received your second letter. If heavy water is too expensive, as I believe, it would be important to find some way of knowing some thing of the carbon absorption. It seems to me that the use of very thick layers of C might do the trick

Yours

Enrico Fermi

bk.f-4 (4)

Hotel King's Crown
420 West 116th Street
New York City

July 11th, 1939

Dear Fermi:

Many thanks for your letter of July 9th. It obviously crossed with my third letter about carbon which probably reached you on Monday. Today, being in a hurry, I confine myself to discuss one point which you mentioned. You write with reference to the carbon sphere experiment that it might be difficult to take into account the distribution of those neutrons which become thermal due to impacts against carbon, and I wish to say the following in this connection.

The number of such neutrons which become thermal within the carbon is quite large, but their number is taken fully into account by the proposed method.

The density of the thermal neutrons within the carbon obeys the equation

$$D \frac{d^2(\rho r)}{dr^2} - A(r\rho) + f(r) = 0$$

where $f(r)$ stands for the number of thermal neutrons ~~existing~~ produced in unit time and unit volume at any point within the carbon sphere of the radius.

Let $\rho_1(r)$ be a solution of this equation for the boundary condition $\rho_1(R) = b$ where $b$ is ~~a~~ the thermal neutron density at the boundary surface of the carbon sphere in water under the conditions of the experiment.

Let further be $\rho_2(r)$ a solution of the same equation for the boundary condition $\rho_2(R)=0$ which is realized by covering the surface of the carbon sphere with a thermal neutron abosrber. The equation

$$D\frac{d^2(r\rho)}{dr^2} - A(r\rho) = 0$$

will then be obeyed by $\rho = \rho_1 - \rho_2$, and $\rho$ will satisfy the boundary condition $\rho(R) = b$. Therefore we have

$$\frac{\rho(r)}{\rho(0)} = \frac{\rho_1(r) - \rho_2(r)}{\rho_1(0) - \rho_2(0)} = \frac{e^{ar} - e^{-ar}}{2ar}$$

So much for the "theory". Practical difficulties are of course present.

I may write you again in the next few days and wish today only to add this: Since Anderson did not get an acknowledgment from Physical Review about our note I asked Pegram today to enquire about it. It turned out that the note was too long for a Letter to the Editor and that it will appear as a short paper in the issue of August 1st.

Yours,

L. S.

(Leo Szilard)

Bkf. 4 (15)

(6)

. C o p y

University of Michigan
Ann Arbor

Department of Physics

July 11 1939

Professor George B. Pegram

Pupin Physics Laboratories
Columbia University

New York, N.Y.

Dear Professor Pegram:

As I have already written to Szilard I was myself considering the slowing down of neutrons with carbon as one of the possibilities to avoid a large absorption of the neutrons at resonance during the slowing down process. There are at present no data on the absorption cross section of carbon for slow neutrons, since it is apparently too small for being detected by the usual methods. Since however even a small absorption, at thermal energies or otherwise, might be sufficient for preventing a chain reaction it would be at present, as Szilard correctly puts it, a gamble to attempt a large scale experiment on the chance that the absorption by carbon is considerably lower than the upper limits that can be given at present.

From what Szilard writes to me I understand that he considers the advantage of saving time by attempting, without a preliminary investigation, a large scale experiment worth the risk that the absorption might be too large. I agree with him that the loss of time for a semi large scale experiment would presumably be considerable. Nonthe-less I would feel much better at ease if it were possible to try the large scale experiment after having convinced ourselves that the chances of success are greater than we can estimate now.

One might perhaps think of a preliminary experiment on the following lines:

A tank which can be either empty or filled with carbon and through which are scattered some cans that can be empty or filled with uranium oxide is placed inside a larger tank containing a manganese solution. The activity of this solution is measured under three different conditions:

| | | |
|---|---|---|
| A | without carbon | without uranium |
| B | with carbon | without uranium |
| C | With carbon | with uranium |

The differences between A and B should give an estimate of the neutron absorption in carbon. The large scale experiment should be attempted only if C is larger than A. Indeed this would mean that the number of neutrons produced by uranium is larger than the number of those that are absorbed by carbon + uranium.

I dont know as yet whether the intensity will be sufficient for such an experiment and I shall think meanwhile whether it is possible to find a better arrangement. In any case it seems to me that it will be essential not to use a homogeneus mixture of carbon and uranium but to keep them in separated layers. Otherwise the absorption at resonance becomes important even when the ratio of the number of uranium atoms to that of carbon is as low as 1/1000.

I was very much interested in the fact that the fission has been observed also in protoactinium. I immagine that Dunning will now try also Ionium.

We had a very pleasant journey and are now settled here quite comfortably.

Sincerely yours

signed: Enrico Fermi m.p.

(Enrico Fermi

Copy

Bk.f.A (17) (8)

University of Michigan
Ann Arbor

Department of Physics

July 18 1939

Dear Anderson:

Thank you for your two last letters. I am convinced now that the absorption cross section in uranium at exact resonance is considerably larger than 1200; probably two or three times. It seems to me however that the absorption law is considerably more complicated than would correspond to a Breit Wigner formula. It is therefore worth while to look closer into the matter.

From the experiments that we performed before my leaving, it would appear that the absorption for large thicknesses does not decreae as fast as it should; there seems to be some contradiction between these results and the latger cross section at resonance that corresponds to your measurements. I entirely agree with you that it is rather uncertain how to interpret absorption data without a parallel geometry. It seems to me however that if the intensity of the cyclotron does not increase by a very large factor you might get into trouble with the intensity with the arrangement that you propose to use.

I wanted to perform several experiments but I have been considerably hanicapped by a very bad cut in one of my fingers. I cut about one centimeter off the index of my right hand and I am afraid that it will take about a month before I shall again be able to use freely my hands.

Would you please tell to Szilard that the experiment that he proposes for measuring the absorption in carbon seems to be all right and that my former criticism was due to my not having understood what he proposed to do ? I think that the experiment is very important and should be performed.

Yours
signed: Enrico Fermi m.p.

Bk.f. 1 (18) (9)

P.S. I have a favor to ask of you. For a long time I have received no mail addressed to Columbia. Could you please find out whether any mail has arrived for me and see that it is forwarded to me regularly ? Thank you.

2nd set
May, 1969 ✓

Additional Material for page 120 (2)

re: "In July 1939 ... I had reported to Pegram my optimistic views about graphite, and told him why I thought the matter was urgent..."

Letter, L.S. to Richards July 9, 1939

Letter, Bill R. [Richards? K.W.] to L.S. July 11, 1939

BKf.2 (17)

July 9th, 1939

Dear Richards:

I tried to reach you at your home over the telephone, but you seemed to be away, and so I am sending this letter in the hope that it might be forwarded to you. You can best see the present state of affairs concerning our problems from a letter which I wrote to Mr. Strauss on July 3rd, a copy of which I am enclosing for your information and the information of your friends. Not until three days ago did I reach the conclusion that a large scale experiment ought to be started immediately and would have a good chance of success if we used about $ 35.000 worth of material, about half of this sum representing uranium and the rest other ingredients. All of this material would remain unharmed and would be returned if the experiment failed. The possibility that the experiment will fail cannot be entirely excluded, but the experiment will decide once for all if a chain reaction can be made to work with the ingredients used in the proposed experiment.

I told Pegram about the situation and have also written to Fermi who is teaching at the summer course at Ann Arbor. He will have my letter on Monday 10th, and I hope to hear from him and that he will share my opinion.

6.

I am rather anxious to push this experiment as fast as possible. At present I do not know just how quickly the Physics Department can move in this matter, and whether outside funds will be needed or not. Such outside funds, if required, could perhaps be used with a minimum of formality by making the experiment as a joint venture of the Physics Department and the Association for Scientific Collaboration, which is a taxfree non-profit association created for such purposes, and which has so far paid the rent for the radium we used etc. I would, of course, like to know whether there is a chance of getting outside funds if this is necessary to speed up the experiment, and if you have any opinion on the subject, please let me know.

If you think that a discussion of the matter would be of interest I shall of course be very pleased to take part in it. You could probably reach me at the King's Crown Hotel, with the exception of Wednesday, July 12th. Please let me know in any case where I can get hold of you over the telephone and your postal address.

Yours,

(Leo Szilard)

Bk.f.2 (19)

Loomis Laboratory
Tuxedo Park, N. Y.

Tel. Tuxedo 307

July 11, 1939.

Dear Leo:

Your telegram was forwarded to me here by mail, and I've supplied the necessary information on the letter head.

I shall be here most of July, but in August I'm planning to take some sort of motor trip, so I probably won't have any fixed address. Does your inquiry mean that you've got something new on your mind?

Best wishes for the summer.

Cordially,

Bill R.

2nd set
May, 1969

Additional Material for page 120 (last paragraph) (3)

re: "...the question of secrecy again came up."

| | |
|---|---|
| Memo. L.S. to Pegram and Fermi. | April 7, 1940 |
| Letter, J. Cockcroft to Wigner | April 15, 1940 |

Bk f. 8A I (32)

April 7, 1940

Attention of:

Professor G.B. Pegram
Professor E. Fermi

Memo.

Since the experiments on graphite are conducted with government support, and since the result may have a direct bearing on questions of national defense, I should like to raise the following question: Should the value for the absorption cross section of graphite obtained in these experiments come out to be smaller than $10^{-26}cm^2$, the upper limit given by Frisch, Halban and Koch, ought we then

a) within the laboratory freely discuss such a result before its publication, or

b) during the next three months evade questions concerning this value and restrict a free discussion of this value to a limited number of workers in the laboratory?

L. Szilard

Bk.f.3 (50)

THE ROYAL SOCIETY MOND LABORATORY
UNIVERSITY OF CAMBRIDGE

Tel. 4655

Free School Lane
Cambridge

15th April, 1940.

Prof. E. P. Wigner,
University of Princeton,
New Jersey,
U.S.A.

Dear Wigner,

Dirac has given me your message about uranium. Up to the present I have felt that it is very unlikely that anything useable can come out of this in the next few years. However, under present circumstances we cannot afford to take any chances and I should be very grateful to receive privately any information as to any work going on in the United States.

Yours sincerely,

J.D.Cockcroft

2nd set
May, 1969

CORRECTION to page 121

RE: Note No. 41

The correct date of this meeting was June 13th, not June 15th as given in the SMYTH REPORT. June 13th is the date mentioned in all our correspondence, and also in THE NEW WORLD, 1939/1946.

2nd set
May, 1969

Additional Material for page 121 (second paragraph)

At the time of Turner's letter there was still no generally accepted policy on secrecy, so Szilard personally undertook to try to prevent news leaks on the vital subject of this new element (plutonium) and its possible fission. This is chronicled in correspondence among Szilard, Louis A Turner, Gregory Breit, and Ernest O. Lawrence.

Letters:

| | |
|---|---|
| Turner to L.S.<br>(enclosed m.s., mentioned in letter, [redacted]d. - kw) is in folder s.b.s.7 in our files) | May 27, 1940 |
| L.S. to Turner | May 30, 1940 |
| Turner to L.S. | June 1, 1940 |
| G. Breit to L.S. | June 5, 1940 |
| L.S. to Breit | June 7, 1940 |
| Breit to L.S. | June 20, 1940 |
| L.S. to Turner | June 24, 1940 |
| L.S. to Breit | July 6, 1940 |
| Turner to L.S. | July 11, 1940 |
| Turner to Lawrence | July 11, 1940 |
| L.S. to Turner | July 12, 1940 |
| L.S. to Lawrence | July 12, 1940 |
| L.S. to Lawrence | Aug. 6, 1940 |

See also "Third Approach to the Navy, May 1940" from Szilard's 1942 Memorandum to A.H. Compton, attached to the documents on Urey's Advisory Committee on Nuclear Research, page 121.

Bk. f. 7 (9)

PALMER PHYSICAL LABORATORY
PRINCETON UNIVERSITY
PRINCETON NEW JERSEY

May 27, 1940

Dr. Leo Szilard
Pupin Physics Laboratories
Columbia University
New York, New York

Dear Szilard:

Enclosed is a copy of the manuscript of a Letter to the Editor on the subject of fission. I thought that you would be interested in it. Wigner tells me that some of the work on the subject is not being published at present because of its possible military value. I find it a little difficult to figure out the guiding principle in view of the recent ample publicity given to the separation of isotopes. Nevertheless, if that is the case, I should be pleased if you would turn this over to ~~whoever is~~ the authority on such matters and I shall be glad to hear from him and conform to his wishes. It seems as if it was wild enough speculation so that it could do no possible harm, but that is for someone else to say.

Wigner also spoke about some general plans which are developing for a large scale concerted attack on this problem of getting atomic energy out of uranium. He thought that perhaps we could do something about it here. I should be very glad to assist in that enterprise if there is any useful part that I could play in it. I do have a few ideas as to methods of attacking the problem. Naturally I don't just want to charge ahead and start some research which some of the rest of you have probably considered and rejected or considered and planned to begin. We'll just let the matter rest until we hear something from you further. I was sorry that I didn't have a chance to talk to you at some length the other day when you were down.

Sincerely yours,

Louis A. Turner

Louis A. Turner

LAT:MH
Enclosure

Bk.f.7 (11)

420 West 116 th Street
New York City
May 30, 1940

Professor Louis A. Turner
Palmer Physical Laboratory
Princeton University
Princeton, New Jersey

Dear Turner:

I am very grateful to you for letting me have a copy of your manuscript which might eventually turn out to be a very important contribution.

You are certainly justified in finding it difficult to figure out the guiding principle which regulates at present what is being kept secret and what is not. However, things are perhaps not as bad in this respect as they might seem and, at any rate, a sincere effort is being made to bring order out of chaos. The publicity given to the separation of isotopes is rather unpleasant and was regretted by all those with whom I collaborate, but at present there is a view that we may now make the best of it by using it as a smoke screen behind which other work might go on in comparative seclusion.

As you perhaps know, I have written a rather detailed paper on the subject of chain reactions which was sent to the Physical Review early in February but I have been asked to delay the publication of this paper and to refrain from discussing the subject matter for the time being. This was the reason why I did not feel free to show you more than those few pages in which you had "legitimate" interest.

Obviously, we are at present in an awkward situation which requires a better adjustment. It appears important that free discussion of all results and ideas among as many physicists as is practicable should not be inhibited and I believe that it is our right and duty to insist that such free discussion should not be hindered by undue secrecy. Perhaps the best solution would be to draw up a list of all trustworthy people who wish to do serious work on uranium and to have free discussion within this group. An uncontrolled diffusion of information would be prevented by pledging those included in this list to refrain from discussing the subject with those who are not included in the register. From time to time new names could be added as the need arises. Manuscripts, the publication of which is being delayed, would be communicated to everybody within the group. I have the impression that some solution of this type will be worked out in the near future and you will be approached as soon as such a solution is worked out.

At the last meeting at which this subject was discussed a representative of the Government suggested that the scientists might themselves form some sort of voluntary association and impose upon themselves the restrictions concerning publications which appear to be necessary in order to safeguard the required secrecy. Professor Urey has now taken upon himself the task of carrying out this suggestion and he will have a discussion on this subject with the Government authorities in the next few days.

In the circumstances I felt that the best course for me to take ~~was to hand~~ over your letter to Urey rather than send your manuscript to the Government departments concerned. By choosing this avenue it will take longer for you to hear officially anything about the fate of your paper, but on the other hand, ~~may~~ we take less risk in the long run that our work will be hampered by undue secrecy.

In the meantime, you could perhaps write to Tate advising him that your paper is being submitted to certain Government departments and ask him to delay the publication until he hears from you to the contrary.

From what I know there is little doubt that the publication of your paper will have to be delayed indefinitely in the same way as that of my own last paper.

If you wish me to do so I could transmit your paper direct to the Government departments interested and ask point-blank for a decision in this particular case. However, if it is agreeable to you, I would rather await the outcome of Urey's discussion with the authorities and then ~~transmit~~ have your paper ~~to~~ submitted by Urey.

Your paper is certainly very stimulating even if somewhat hypothetical and I was very glad to have an opportunity to read it. As I repeatedly explained to Wigner I personally would be very happy if you at Princeton could collaborate with the rest of us and I shall get in touch with you as soon as I am free to do so. If there is no other solution I might get in touch with you in Woodshole and perhaps run up for a day if there is anything important to settle before you return. We could then discuss things in greater detail. Could you possibly let me have your Woodshole address?

Please consider all the information contained in this letter as ~~[illegible]~~ confidential, and I should be very grateful if you did not discuss it with anyone except Wigner to whom I am sending a copy.

Could you possibly confirm whether you have asked Tate for a temporary delay until further notice by dropping me a line?

Yours sincerely,

L. Sz.

(Leo Szilard)

Bk.f. 7 (10)

PALMER PHYSICAL LABORATORY
PRINCETON UNIVERSITY
PRINCETON NEW JERSEY

June 1, 1940

Dr. Leo Szilard
420 West 116th Street
New York, New York

Dear Szilard:

Your letter of May thirtieth was received. It seems to me that the present situation is a very unsatisfactory one. If the matter is really important, it should not be on such a catch-as-catch-can basis. Assume for the sake of argument that my paper is a really important contribution. The question of its being published or held up should not have to depend on the accident of our being acquainted and my having sent it to you. There ought to be some general way in which the thing was being handled -- right now, not week after next or some later time.

I find it hard to understand how it can be that one important paper was held up in February and now, more than three months later, the matter is in the stage of being discussed informally sometime next week. I think that it is better that I decline to ask for delay in the publication of my paper. Please do not misunderstand me. I am not anxious to rush publication for any personal reasons, and I certainly do not want to fail to cooperate reasonably. I feel that there is a matter of principle involved; that it is high time that the matter was brought to a focus; that somebody, either in the government or outside, like Urey, should take the authority and request all editors to defer publication of papers on the subject until some plan has been worked out. I feel that if this minor paper can produce some action instead of talk it may be of some importance quite apart from any ultimate consequence of the ideas expressed in it.

I am sending a copy of this letter to Tate and also one to Urey.

Sincerely yours,

Louis A. Turner

LAT:MH

Louis A. Turner

P.S. Wigner and I will see you Monday. The above is my present reaction. I feel that something should be done right away; details can be ironed out later.

3bs7

THE UNIVERSITY OF WISCONSIN
MADISON

DEPARTMENT OF PHYSICS

June 5, 1940

Dear Szilard:

I have received from Tate Turner's Letter to the Editor and a copy of Turner's letter to you. I do not know what you have written to Turner but it appears that you do not think the letter should be published, or at least that it should be delayed. I should like to know your opinion and Fermi's very much indeed.

As chairman of the committee of the Division of Physical Sciences on uranium fission I have written Tate concerning the advisability of control over such publications. Tate is very willing to cooperate and the present understanding is that he will send me all such papers so that the committee may decide on whether they should be published. The committee consists of Pegram, Beams and myself. I have also asked for Wigner to be appointed. It may be best for you and Fermi not to be officially on the committee but I plan, of course, to have the benefit of your advice.

I should suggest that Fermi speak to Urey asking for control of publications in the Journal of Chemical Physics and in the publications of the American Chemical Society. I have asked for such control through official channels but there are unavoidable delays.

Sincerely yours,

G. Breit

Bk.f. 7 (7)

C
O
P
Y

420 West 116th Street
New York City
June 7, 1940

Dr. G. Breit
Department of Physics
The University of Wisconsin
Madison, Wisconsin

Dear Breit:

Many thanks for your letter. I am enclosing a copy of Turner's first letter to me to which I replied that if he would be willing to have his paper delayed I would be glad to forward his manuscript to the appropriate authorities. I also enclose a copy of Turner's second letter of which you have apparently received a copy. Subsequently, I saw Turner. He expressed his willingness to have his paper delayed and assuming that the paper has already passed out of the hands of Tate, he proposed to advise the New York office of the American Institute of Physics (Miss Mitchell) accordingly. Meanwhile, I was supposed to forward his paper to the Government departments interested and ask them to notify Turner officially concerning their wishes in this matter. I take it that since, in the meantime, you have arranged with Tate to receive all papers on uranium, this somewhat clumsy procedure upon which Turner and I agreed need not take place and that, accordingly, I need not take any further steps in the matter of Turner's paper except communicating with you about it.

Clearly, for you to be in a position to fulfill your function, it is necessary that you should be fully informed of the work of Fermi and myself as well as other related work. It would be unsatisfactory for you to have Fermi's and my personal opinions without being informed of our reasons. This makes it necessary that we should be free to give you information concerning our work.

This and other considerations make it advisable that a small group of scientists should receive full information on the work which is being carried out and that you should be a member of this group. I have been lately taking a strong stand in favor of such a solution, and I understand that the 13th of June may be fixed as the time and Washington, D. C. asthe place for a meeting. No doubt, you will receive official notice within the next few days from the proper authorities. It would be very useful if you could come to New York a day or two earlier so that we may have a number of informal discussions, in connection with the various complicated questions which will necessarily arise. If possible, thought should precede action.

I take it that as far as preventing publication goes you are already handling the situation efficiently, and I have communicated your suggestion, that the Journal of Chemical Physics and the American Chemical Society should fall in line, to Urey. I told him that you have already asked for such control through official channels.

Yours sincerely,

(Leo Szilard)

Dk f. 8 A I (33)

THE UNIVERSITY OF WISCONSIN
MADISON

DEPARTMENT OF PHYSICS

June 20, 1940.

Dr. Leo Szilard
Department of Physics
Columbia University
New York City

Dear Szilard:

I should like to thank you for the many discussions we have had in New York and for your hospitality. It seems to me that matters would be helped along very much if the intermediate experiment could be performed and if the set up could be kept flexible. My impression is that in work of this type practical success in a limited ~~time~~ may depend considerably on detailed planning regarding the ease of assembley and flexibility. I still think that more rapid progress will be achieved by arranging an intermediate or full scale experiment rather than by careful measurement.

Sincerely yours,

Gregory Breit

G. Breit

Bk.f.7 (15)

420 West 116th Street
New York City

June 24th, 1940

Professor Louis A. Turner
Palmer Physics Laboratory
Princeton University
Princeton, N.J.

Dear Turner:

I understand that you have sent to Tate a copy of your last letter which was addressed to me and that, in consequence of that, some official action has been taken about delaying your paper. I take it therefore that I need not do anything about the matter myself.

I wish to draw your attention to the last issue of Physical Review in which McMillan and Abelson show that element 94 is produced from uranium by thermal neutrons. My guess is that they will try to see whether this element shows fission with thermal neutrons, but I do not know this for certain. Since this is perhaps one of the most important questions to be decided by a single experiment, and since it is urgently necessary to know the answer to it, I feel that the matter ought to be taken up officially or unofficially with Lawrence. Before doing anything about it, however, I wanted to ask you if you perhaps would prefer to write to Lawrence yourself and perhaps offer to go out to Berkeley yourself during this summer and collaborate in such an experiment.

Would you be kind enough to let me know whether you intend to write to Lawrence yourself?

With best wishes, yours sincerely,

(Leo Szilard)

Bk. f. 7 (2)

420 West 116th Street
New York City
July 6, 1940

Dear Breit,

Many thanks for your letter. Following the conversation we had on our way from Washington to New York. I have given some thought to the issue mentioned in your letter and I am now entirely convânced to your point of view. Consequently, I am taking a strong stand in favor of an experiment on as large a scale as possible. This large scale experiment, or some intermediate experiment, operating with at least five tons of uranium ought to have the right-of-way before the general survey of the neuclei values involved. Nevertheless, this general survey will also have to be carried out.

There is another point about which I became converted to your opinion. I now think that steps should be taken to prevent certain publications in Nature and the Proceedings of the Royal Society of London. With the collapse of France there is an immediate danger that Joliot and his co-workers will start publishing something of their previous work in these periodicals.

On the other hand I feel even more strongly than before that your attempt to prevent publication will break down unless we create a satisfactory substitute in the form of some private publication. If that is not done there will be a growing tendency towards indulgence and finally practically everything will be published as it has been in the past. I wonder whether you have given the matter further thought since your return to Madison.

With kindest regards.

Yours,

(Leo Szilard)

BK. f. 7 (3)

Woods Hole, Mass.,
July 11, 1940

Dear Szilard;-

Your letter of June 24-July 3 was forwarded to me here. Segrè wrote me about the work of McMillan and Abelson after reading my paper in the June 1 Phys. Rev. He made no mention of any prospective work on the fission of 94-239 as it would have been natural for him to do in that connection if they were contemplating such work. White and I had discussed the possibility of having a go at the thing in Princeton but I think probably that it is better that it should be done in Berkeley since plenty of intensity will probably be required for getting the kind of second order effect that this will be. Accordingly I have written the enclosed letter to Lawrence.

Holding up Szilard's paper was of course his own initative. See: L.S. to Tate Feb. 6, 14 (page 118) (KW)

I'm sorry that you didn't see fit to send the copy of my paper on to whoever it was that was responsible for having yours held up, as I understood that you were going to. I felt that such action would contribute to a more rapid clarification of the whole problem, which was the end sought. Breit has my paper, but as far as I can make out he thinks that I am holding it up and I thought that he was, and there has been nothing official about it one way or the other. I shall write to him about it.

With best regards, sincerely yours,

Louis A. Turner.

Sorry - that I neglected to enclose this.
S.A.T.

Bk.f.7 (4)

Woods Hole, Mass.,
July 11, 1940.

Professor E.O. Lawrence,
The Radiation Laboratory,
The University of California,
Berkeley, California.

Dear Ernest;-

Enclosed is the copy of a manuscript of a Letter to the Editor which I sent to Tate over a month ago and a letter to me from Szilard which came a couple of days ago. As you will see, the Letter has to do with an aspect of the fission problem which may turn out to be of some importance. As sent in, the Letter would have appeared appropriately enough in the June 15 issue of the Physical Review along with the Letter of McMillan and Abelson which put the foundation for the whole argument on a much surer basis. Its publication was deferred, however, pending the working out of arrangements for a sort of censorship of papers on such matters which may be of military significance and for the circulation of them among those doing active work in the field. Knowing through Wigner that Szilard already had been asked to hold back a paper I sent it to him and asked him to transmit it to whomsoever it was that was willing to take the authority in the matter. I won't bore you with all the further details of conferences and correspondence all over the place. There have been three separate committees worrying over the general problem, and I have yet to get from anybody a direct, official request that the paper be held up or statement that it is being held up by him or them. I believe, however, that the thing will be soon worked out in some sensible way in connection with the Bush committee.

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Anyhow, the scientific question is, does EkaOs 94 undergo fission with thermal neutrons? I feel that that is reasonably sure, granting that the theory of Bohr and Wheeler is right in a general sort of way, as it seems to be (see my recent paper, Phys. Ref. 56, 426, 1940) Experimental proof, however, is what's needed. If some of your chemical experts can make a guess as to the chemistry of element 94 on the basis of what they have learned about no. 93 perhaps some of the stuff could be separated out of that two year bombarded sample of U that you have. Perhaps the known U, ~~//////////~~ $UX_1$ and $UX_2$ could be eliminated with some assortment of carriers in the solution that would be fairly sure to keep the 94 in, the residue then to be compared with a similar blank sample in ~~///~~ looking for fission to be blamed on the 94. In any case, it would be possible to give a good long bombardment to a sample, separate off the 93, and wait a few days for it to decay to 94. Not knowing just what intensities of neutrons you can produce I can't make a guess as to whether enough of the 94 could be thus produced in a reasonable time so that there might be some prospect of observing its fission. In either case it seems that your lab is the place where the work ought to be done, because of your having the ~~////~~ well-bombarded U and the greatest intensity of neutrons for a fresh start if that be indicated.

I'd like nothing better than to come out and help do some work on this problem, as Szilard suggests, but it does not ~~/////~~ seem feasible, even if you should feel that it is desirable to get the problem going out there. I do not think that I could bring any skill to the matter that would warrant my making the trip for the purpose. Your gang

Bk. f. 7 (6)



is entirely capable of doing the job without help from me or anyone else. All that would be lost would be the experince and fun that I could get out of coming out to partècipate. If you feel that the problem is a good one I hope that you will find someone amongst your colleagues to work on it. I suppose that it will be more important to withhold publication of any positive results of such experiments than it is to hold up guesses about them. I leave the matter to your discretion as to whom should be let in on it.

I do wish that I could join the mass migration of my Eastern colleagues to Berkeley this summer, problem or no problem. We gave serious thought to coming out but for various family reasons it seemed not to be the right thing for this summer.

Please give my regards to Mrs. Lawrence.

Sincerely yours,

P.S. I'm sending a copy of this to Szilard.

Bk. f. 7 (18)

420 West 116th Street
New York City

July 12, 1940

Dear Turner:

Many thanks for your letter of July 11. I am sorry to say that it did not contain the copy of your letter to Lawrence which was supposed to be enclosed. Could you possibly send me that copy, as I am rather anxious to keep in close touch with the developments concerning element 239?

A few days after our last meeting I was given to understand that the Physical Review is officially considering whether or not to publish your note, and I have thereupon advised Wigner that in view of this I do not propose to raise the same question with another authority. You probably realize that the action taken by Physical Review followed directly from your sending to Tate a copy of a letter which you had written me, and that it would create confusion if two different agencies were asked to decide about the fate of one and the same paper. If, however, I should have misunderstood the action taken by Physical Review concerning your note, and if no official action concerning your note is under way, then I would be glad to take up the matter again at the point where it was left, all the more as I personally think it very important that your note should not appear in print. Perhaps you will let me know if I can do anything further in the matter after you will have obtained a clear picture through your further correspondence with Breit.

With best wishes, yours sincerely,

L. Sz.

Bk. f. 7 (16)

420 West 116th Street
New York City

July 12, 1940

Dear Lawrence:

Some time ago Turner has sent me a manuscript for a note to the Editor, which he sent to Physical Review and the publication of which is being delayed because it is considered that it is not in the public interest to have it appear in print. After the publication of McMillan and Abelson's note on element 239 I wrote Turner according to the enclosed copy, and I understand that he has already taken up the matter with you.

The purpose of the present letter is to draw your attention to the fact that if element 239 shows fission for thermal neutrons it would be highly advisable to keep this a closely guarded secret. Looking at this question from the perspective of my own work I would say that a fission cross-section for thermal neutrons in excess of 10 to 20 x would have to be considered as one of the most important single facts which have a bearing on the possibility and scope of the chain reaction. Since I do not know whether you are at present investigating the fission of element 239 I do not propose to go into details in this letter. My main purpose in writing it is to prevent that if you should obtain a result concerning the fission of element 239 the result should be made public inadvertently. If you want to know reasons for secrecy I shall be glad either to write you about them myself or to have the information sent to you through official channels.

Yours sincerely,

(Leo Szilard)

Bk.f.7 (17)

TELEPHONE
UNIVERSITY 4-2700

**KING'S CROWN HOTEL**

OPPOSITE
COLUMBIA UNIVERSITY

UNDER KNOTT MANAGEMENT

420 WEST 116TH STREET, NEW YORK N.Y.

August 6, 1940

Professor E. O. Lawrence
The Radiation Laboratory
The University of California
Berkeley, California

Dear Professor Lawrence:

Turner sent me a copy of his letter which he wrote you on July 11 and I should like to make the following comment on his letter:

If it were shown that element 239 has a fission cross-section for thermal neutrons of the order of $25 \times 10^{-24}$ $cm^2$ or larger, then we would have to expect that the line of work which Fermi and I are following up at present may lead in the near future to practical applications which are rather important from the point of view of national defense. I should be glad to give you a more detailed account if necessary.

In the meantime, I am writing to you primarily because I am anxious that if fission of element 239 by thermal neutrons is discovered in your laboratory information about it should not leak out in the newspapers or otherwise. In case of a publication through regular channels such as the Physical Review you would be probably officially approached about the matter and I assume you would be requested to delay the publication of such a phenomenon.

This year I had hoped as I did in the past two years to be able to visit Berkeley in the summer time, and I have not yet given up hope altogether; but I don't know if I can get away from New York just now.

With best wishes and kind regards to all,

Yours sincerely,

Leo Szilard

(Leo Szilard)

PLAN TO VISIT WORLD'S FAIR IN NEW YORK