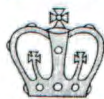


Materials given to Henry
Barton in 1942 for safekeeping.
In Barton Papers, American
Institute of Physics' Niels
Bohr Library.

UNDER
NOTT MANAGEMENT

TELEPHONE
UNIVERSITY 4-2700

King's Crown Hotel



420 WEST 116TH STREET
NEW YORK

OPPOSITE COLUMBIA UNIVERSITY

RECEIVED
FEB 16 1940
Auto 2/19/40

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)

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
- NT=Overnight Telegram
- 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

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

NA70 22=QR NEWYORK NY 20 1003A

DUPLICATE OF TELEPHONED TELEGRAM FEB 20 AM 9 45

PROFESSOR TATE, EDITOR=

PHYSICAL REVIEW UNIVERSITY OF MINNESOTA MPLS=

HAVE RECEIVED NO ACKNOWLEDGEMENT OF MANUSCRIPT MAILED TO YOU ON FEBRUARY 14 STOP KINDLY WIRE WHETHER RECEIVED

SZILARD 420 W. 116 STR.=

UNSigned.

MAR 17 7 P.M. (LW)
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14 SZILARD 420 W 116 STR.

THE COMPANY WILL APPRECIATE SUGGESTIONS FROM ITS PATRONS CONCERNING ITS SERVICE

Pages 22, 23, ...
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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

(Leo Szilard)

UNDER
IT MANAGEMENT

TELEPHONE
UNIVERSITY 4-2700

King's Crown Hotel



420 WEST 116TH STREET
NEW YORK

OPPOSITE COLUMBIA UNIVERSITY

RECEIVED
APR 8 1940
RECEIVED

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)

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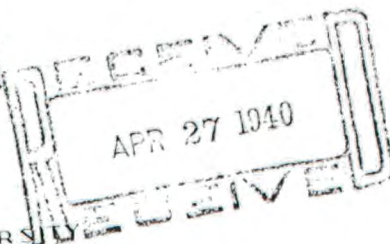
King's Crown Hotel



420 WEST 116TH STREET
NEW YORK

OPPOSITE COLUMBIA UNIVERSITY

April 16, 1940



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

18, 1940

Dear Dr. Tate:

Many thanks for your letter of April 8. Following your suggestion I shall send you a new manuscript in which I shall attempt to shorten the paper. Apart from rewriting the introduction, the paper could perhaps be shortened by shifting some of the purely arithmetical parts of the paper into an appendix, which could be set in small type.

I feel, however, that perhaps I ought to refrain from modifying the paper in the light of more recent experiments. If there is considerable delay in the printing of this paper, and if much additional information becomes available, it will probably be necessary to add

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PLAN TO VISIT NEW YORK WORLD'S FAIR

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a short note in proof. This note could then
take into account all additional information.

Yours sincerely,

Leo Szilard

(Leo Szilard)

UNDER
KNOTT MANAGEMENT

King's Crown Hotel

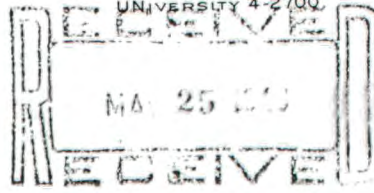


420 WEST 116TH STREET
NEW YORK

OPPOSITE COLUMBIA UNIVERSITY

TELEPHONE

UNIVERSITY 4-2700



May 23, 1940

May 8, 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 to 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 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

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UNDER
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TELEPHONE
UNIVERSITY 4-2700

King's Crown Hotel



420 WEST 116TH STREET
NEW YORK

OPPOSITE COLUMBIA UNIVERSITY

Dr. John T. Tate

May 23, 1940

May 8, 1940

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)

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UNDER
KNOTT MANAGEMENT

Kings Crown Hotel



420 WEST 116TH STREET
NEW YORK.

OPPOSITE COLUMBIA UNIVERSITY

February 6, 1940

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

Dear Dr. Tate:

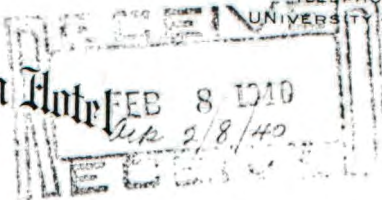
Enclosed you will find a manuscript which I am sending you with the request that you have it printed in the Physical Review as a "letter".

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)

TELEPHONE
UNIVERSITY 4-2700



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From Lane Richards

Nov 17, 1974 JMS

ATTEMPTS AT SECRECY FROM MARCH 1939 TO JUNE 1940

L. Szilard

November 4, 1942

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Attempts At Secrecy-March 1939-June 1940

November 4, 1942

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

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

SECRET

First Approach to France

About one month before Fermi and I actually observed the neutron emission of uranium I wrote to Joliot advising him of the projected experiments and suggesting that he collaborate with us in keeping any positive results secret.

The text of the letter which speaks for itself is inclosed.

c/o Liebowitz
420 Riverside Drive
New York City

February 2nd, 1939

Professor M. Joliot
Laboratoire de Chemie Nucléaire
Collège de France
Paris

Dear Professor Joliot:

The only reason for my writing to you this letter to-day is the remote possibility that I shall have to send you a cable in some weeks, and if that happens this letter will help you to understand what the cable is about. This letter is therefore merely a precaution, and we hope an unnecessary precaution.

When Hahn's paper reached this country about a fortnight ago, a few of us got at once interested in the question whether neutrons are liberated in the disintegration of uranium. Obviously, if more than one neutron were liberated, a sort of chain reaction would be possible. In certain circumstances this might then lead to the construction of bombs which would be extremely dangerous in general and particularly in the hands of certain governments.

It is of course not possible to prevent physicists from discussing these things among themselves, and, as a matter of fact, the subject is fairly widely discussed here. However, so far, every individual exercised sufficient discretion to prevent a leakage of these ideas into the newspapers.

In the last few days there was some discussion here among

physicists whether or not we should take action to prevent anything along this line from being published in scientific periodicals in this country, and also ask colleagues in England and France to consider taking similar action. No definite conclusions have so far been reached in these discussions, but if and when definite steps are being taken I shall send you a cable to tell you what is being done.

We all hope that there will be no, or at least not sufficient, neutron emission and therefore nothing to worry about. Still, in order to be on the safe side, efforts are made to clear up this point as quickly as possible. Experiments at Columbia University are in charge of Fermi and ^{he} will perhaps be the first to give reliable results.

Perhaps you have also thought of the same things and have contemplated or started such experiments. May be you are able to get definite results at an earlier date, which, of course, would be very valuable towards ending the present disquieting uncertainty. Whatever information on the subject you might care to transmit by letter or cable at some later date will, I am sure, be greatly appreciated. Also, should you come to the conclusion that publication of certain matters should be prevented, your opinion will certainly be given very serious consideration in this country.

Yours sincerely,
signed: (Leo Szilard)

Agreement about Secrecy

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

First Approach to the United States Government through Fermi

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

SECRET

Approach to England; Last Appeal to France

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

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

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

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

COPY OF LETTER FROM WIGNER TO DIRAC

PALMER PHYSICAL LABORATORY
Princeton University
Princeton, New Jersey

March 30, 1939

Dear Paul:

I am writing to you in a rather serious matter this time. The enclosed letter, sent by Szilard to Joliot on February 2nd is self explanatory. Experiments undertaken since that time by Fermi and by Szilard did not help to dispell the fear which prompted Szilard's letter. In realisation of the danger mentioned in this letter, all efforts are made here to delay publications relating to this subject as there could possibly enhance the danger of a grave misuse by certain powers. The papers of Szilard and of Fermi, although received by the Physical Review some time ago, are withheld from publication and it is intended that they be printed only in the form of reprints to be distributed among the most interested laboratories in England, the U. S., France and Denmark. Similar arrangements are intended for all papers on this subject by other workers in the United States.

Halban-Joliot-Kovarski's letter to Nature prompted the physicists who loyally cooperated here to inquire today by cable concerning Joliot's attitude in this matter. Bohr undertakes to communicate with Copenhagen and a cable is sent simultaneously to Blackett. The proposition made in there communications is to use for the publication of all papers, relating to this subject, the method foreseen for this purpose for workers in the U. S. and described above.

What we would like to ask you at this time is to get in touch with Blackett and to actively support him in his endeavours if you find our position to be the reasonable one.

It is my impression that there is some urgency in the matter. Although there exists apparently a great willingness for cooperation here, it is realised that the interests of the scientific workers in the U. S. may be prejudiced to some extent if America abeyed alone by the proposed procedure.

Hoping to hear from you soon and with best regards to all,

Sincerely,

(signed) Jenö

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April 6, 1939

JOLIOU
COLLEGE DE FRANCE PARIS

REPLYING YOUR CABLE WEISSKOPF STOP ROBERTS PAPERS CONCERNING DELAYED
NEUTRON EMISSION WHICH IS MUCH WEAKER THAN HE THINKS AND HARMLESS
STOP HOWEVER TUVES GROUP WAS RECENTLY APPROACHED AND PROMISED
COOPERATION STOP WE HAVE SO FAR DELAYED PAPERS IN VIEW OF POSSIBLE
MISUSE IN EUROPE STOP KINDLY CABLE AS SOON AS POSSIBLE WHETHER
INCLINED SIMILARLY TO DELAY YOUR PAPERS OR WHETHER YOU THINK THAT
WE SHOULD NOW PUBLISH EVERYTHING STOP
KINGS CROWN HOTEL SZILARD

April 8, 1939

NLT WEISSKOPF
FINE HALL PRINCETON (NJ) USA

YOUR SUGGESTION PASSED TO NATURE AND ROYAL WHO WILL SURELY COOPERATE
STOP AWAITING LETTER WITH DETAILS
BLACKETT

April 7, 1939

LC SZILARD
KINGSCROWN HOTEL NY

QUESTION ETUDIEE SUIS D AVIS MAINTENANT PUBLIER AMITIES
JOLIOU

COPY.

March 31st, 1939

HANS VON HALBAN

11 RUE GUYENNE
SCEAUX SEINE

KINDLY INFORM JOLIOU THAT PAPERS RELATING TO SUBJECT OF YOUR JOINT NOTE TO NATURE HAVE BEEN SENT BY VARIOUS PHYSICISTS TO PHYSICAL REVIEW BEFORE PUBLICATION OF YOUR NOTE STOP AUTHORS AGREED HOWEVER TO DELAY PUBLICATION FOR REASONS INDICATED IN SZILARD'S LETTER TO JOLIOU FEBRUARY SECOND AND THESE PAPERS ARE STILL HELD UP STOP NEWS FROM JOLIOU WHETHER HE IS WILLING SIMILARLY TO DELAY PUBLICATION OF RESULTS UNTIL FURTHER NOTICE WOULD BE WELCOME STOP IT IS SUGGESTED THAT PAPERS BE SENT TO PERIODICALS AS USUAL BUT PRINTING BE DELAYED UNTIL IT IS CERTAIN THAT NO HARMFUL CONSEQUENCES TO BE FEARED STOP RESULTS WOULD BE COMMUNICATED IN MANUSCRIPTS TO COOPERATING LABORATORIES IN AMERICA ENGLAND FRANCE AND DENMARK STOP COMMUNICATING BLACKETT AND DIRAC IN ATTEMPT TO GET COOPERATION OF NATURE AND PROCEEDINGS ROYAL SOCIETY STOP PLEASE CABLE WEISSKOPF FINE HALL PRINCETON NJ

March 31st, 1939

BLACKETT PHYSICS DEPARTMENT VICTORIA UNIVERSITY MANCHESTER

PHYSICISTS HERE HAVE SENT PAPERS TO PHYSICAL REVIEW ON SUBJECT RELATED TO HALBAN JOLIOU LETTER TO NATURE STOP AUTHORS AGREED TO DELAY PUBLICATION IN VIEW OF REMOTE BUT NOT NEGLIGIBLE CHANCE OF GRAVE MISUSE IN EUROPE STOP IT IS SUGGESTED THAT PAPERS BE SENT TO PERIODICALS AS USUAL BUT PRINTING BE DELAYED UNTIL IT IS CERTAIN THAT NO HARMFUL CONSEQUENCES TO BE FEARED STOP RESULTS WOULD BE COMMUNICATED IN MANUSCRIPTS TO COOPERATING LABORATORIES IN AMERICA ENGLAND FRANCE AND DENMARK STOP IS IT POSSIBLE FOR YOU TO OBTAIN COOPERATION OF NATURE AND PROCEEDINGS ? WIGNER WRITING DIRAC STOP WEISSKOPF FINE HALL PRINCETON NJ

April 5 1939

~~DR LEO SZILARD~~

~~DEPT OF PHYSICS COLUMBIA UNIV NYC~~

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JOLIOU HALBAN KOWARSKY

COPY OF LETTER FROM WEISSKOPF TO BLACKETT, ENGLAND, MARCH 1939

Victor Weisskopf
University of Rochester

Dear Blackett,

I hope you were not too much upset about my telegram but I believe that you realize the great danger which would arise, if one really could construct a bomb with uranium. The probability that this is possible might be small, but the product of the probability with the graveness of the consequences is high.

I enclose here first a letter which Szilard has written to Joliot Febr. 2. Joliot has not answered this letter and we do not know Joliot's attitude to the whole situation after his recent publication. I have sent to Halban a similar telegram as to you urging him to cooperate.

Further I enclose a note which Szilard has sent to Physical Review but the publication of which is being delayed. There are other papers from Columbia sent in and kept back, which could be sent to you if the cooperation begins to work. I am also enclosing a letter from Szilard to myself which gives you further details about his experiments.

I would like tell you how far the cooperation here for delaying "dangerous" manuscripts has developed so far. We know that the group around Tuve is now willing to cooperate. Lawrence is coming here on April 3rd. and we shall discuss the matter with him then. Tate (editor of Phys. Rev.) is being approached and it is suggested that authors who may send in manuscripts concerning "dangerous" neutron emissions be advised to communicate with us. We shall send you a cable when a definite procedure has been decided upon in connection with Phys. Rev.

Much love to your family.

Very truly yours

SECRET

COLLÈGE DE FRANCE

Laboratoire de Chimie Nucléaire

Paris, le

19 avril

1939

Place Marcellin-Berthelot

PARIS (V*)

Téléph. : ODEON 81-60

Monsieur L. SZILARD

Kings Crown Hotel

420 West 116th street

New - York

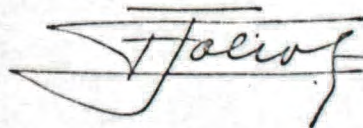
Mon cher Szilard,

J'ai bien reçu votre lettre du 7 avril et votre intéressante note sur la libération des neutrons. Nous avons continué les recherches sur cette question et vous trouverez ci-joint le texte manuscrit d'une note que nous avons envoyé à Nature. Il est malheureusement trop tard pour que nous puissions ajouter en référence votre communication, cependant nous ne manquerons pas de le faire dans un article général qui sera publié prochainement.

J'étais très embarrassé en ce qui concerne l'ajournement des publications sur ce sujet, étant certainement l'un des premiers à comprendre vos raisons. Cependant vous pouvez comprendre que nous ne sommes pas, ainsi que ceux que vous avez pu prévenir, les seuls à nous occuper de cette question, et rapidement nous avons pu lire dans des publications scientifiques et dans la presse d'information, en France et à l'étranger, des articles où étaient clairement expliquées les conséquences énergétiques du phénomène en question. Ce sont les seules raisons qui ont motivé les termes de mon dernier câble. Je suis certainement d'accord avec le principe d'une entente, mais pour qu'elle soit efficace il faut qu'elle soit étendue à tous les laboratoires susceptibles de s'occuper de la question.

Je vous serais reconnaissant de bien vouloir faire part de ces considérations aux collègues américains que vous avez pu touchés.

Avec mes sincères salutations,



COLLAPSE OF SECRECY - APRIL 1939

After Joliot's final refusal to collaborate, all my colleagues at Columbia University expressed themselves in favor of publishing our papers. I continued to take the stand that irrespective of Joliot's policy, we ought not to publish our own work. Pegram, the head of the department, was undecided. It seemed impossible to reconcile the two opposing views, and Professor I. Rabi at Columbia, who was not himself involved in this work, gave me a friendly warning that if I continued to take such an irreconcilable stand, I would probably be left without facilities for further work at Columbia. At the suggestion of Fermi, we finally agreed to leave the decision up to Pegram, the head of the department.

Having expressed our views orally to Pegram, Dr. Zinn and I put down our ^{opposing} views also in writing in the form of letters addressed to Pegram. A copy of Dr. Zinn's letter and my own is enclosed. Before we could hand over these letters, Dr. Pegram decided, after one week of deliberation, to release our papers for publication.

I informed Blackett in England, and Wigner, that the policy of secrecy has been abandoned. Wigner's reply to my communication was to urge me to advise the United States Government of the situation. A photostatic copy of his letter is enclosed.

These letters by Szilard and Zinn did not actually reach Professor Pegram since in the meantime Professor Pegram decided to release the papers for publication. The hand-written originals are in my files.

L. Szilard

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March 27, '39

Dear Professor Pegram:

It seems we shall have to decide today about delaying the letter which Zinn and I sent to the Physical Review. I feel that if we delay this letter now, and if you write to Tate along the lines which Fermi suggested on his return from Washington, we may have a chance, although not a very great one, to get others to cooperate. If we publish now, we cannot ask others to withhold future, perhaps more important, papers. Zinn, I believe, is of somewhat different opinion. It seems that in the circumstances you, as head of the department, will have to take the responsibility for deciding this difficult question, one way or another. I am very sorry to have to worry you with this awkward decision, but it happens only once in a lifetime.

Yours sincerely,

Leo Szilard

since in the meantime Professor Pegram decided to release the papers for publication. The hand-written originals are in my files.

L. Szilard

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Dear Professor Pegram:

Dr. Szilard has shown me the note he has written concerning the publication of our letter. I dislike very much imposing on you to the extent of asking you to decide this question. However, Szilard and I do take opposite views. My opinion may be summed up as follows:

Withholding publication cannot now keep the matter from becoming generally known among physicists here and abroad. A small rumor to certain people would start them off on experiments just as quickly as a full publication.

Joliot's paper already provides them for an excuse to begin work.

Our fears apparently are not shared by the French workers and I can hardly believe that they are ignorant of the possibilities. Withholding publication can, at most, delay the discoveries we fear for some months, in which case secrecy would be impossible. On the other hand, publication would accelerate research work in several laboratories and I feel that this country will not be put on the "spot" by its research workers failing to do their job.

Finally, withholding publication sets a new and undesirable precedent among physicists.

Despite the above arguments I would be influenced a great deal by Professor Fermi's opinion. My reason for this is that he inevitably will be forced to accept the major part of blame or honor which might result from these publications. Columbia University also has a vital interest in the matter from this viewpoint and therefore I am inclined to give your opinion great weight.

W. H. Zinn

*These documents are at
the records of
the area which the
L. Szilard*

THE STRUGGLE FOR SECRECY

L. Szilard

October 31, 1942

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April 17, 1959

Dear Szilard !

Thank you for letting me have the news concerning the abandonment of any policy in the publication matter. I cannot help feeling, on the one hand, that this was, under the conditions, a wise decision as nothing really could be achieved in this matter. On the other hand I do feel, and I do feel it very strongly, that the U.S. Government should be advised of the situation. This is indicated, among many other reasons, by the necessity of preparing it to a possible sudden threat. Let me know, please, whether you have already taken steps in this direction and whether you intend to take some in the near future.

REPLY IN DUPLICATE
AND REFERENCE TO

WILL BE APPRECIATED

NAVAL RESEARCH LABORATORY

ANACOSTIA STATION

RG/ejh

WASHINGTON, D. C.

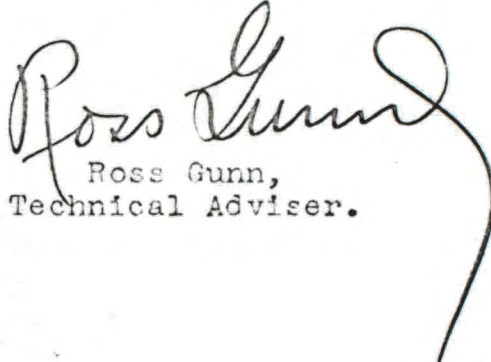
10 July 1939

Dr. Leo Szilard,
Department of Physics,
Columbia University,
New York, N.Y.

Dear Dr. Szilard:

The matter which we discussed at the Princeton meeting of the Physical Society has been carefully considered. As I indicated to you at that time, it seems almost impossible, in light of the restrictions which are imposed on Government contracts for services, to carry through any sort of an agreement that would be really helpful to you. I regret this situation but see no escape. We are anxious, however, to cooperate with you in every respect and appreciate your assistance on this important problem.

Very truly yours,


Ross Gunn,
Technical Adviser.

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Second Approach to the Navy

June 1939

In June 1939 I approached the Naval Research Laboratory through Ross Gunn with a view of establishing some sort of cooperation between the Columbia group and the United States government. The reply was negative.

A photostatic copy of the reply is inclosed.

First Approach to the President of the United States

August - October 1939

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

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

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

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

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October 17, 1939

Dear Professor Wigner:

In keeping with our conversation on your recent visit to my office and in furtherance of later developments reported to you by Dr. Szilard, I had a conference in Washington on October 11th with a committee appointed by the President, headed by General Watson, his executive secretary and military aide. After that conference I had the honor to present the matter to the President and to leave with him a dossier consisting of Dr. Einstein's letter, Dr. Szilard's memorandum, and my own original letter-memorandum on the subject addressed to him.

On the following day the President appointed a small committee representing the Army, the Navy and the Bureau of Standards, in the persons of Colonel Adamson, Commander Hoover and Dr. Lyman Briggs. Dr. Briggs then, in consultation with me, arranged and formally issued an invitation on the following day for a conference to be held this week at Washington with your good self and Dr. Szilard, as the scientific complement, and myself as the intermediary, and the informal committee above mentioned. To suit your joint preferences, as conveyed to me by Dr. Szilard, the date was shifted from Wednesday to Saturday morning, October 21st, at 9:30 at the office of the Bureau of Standards in the U. S. Department of Commerce. This afternoon Dr. Briggs warmly approved the suggestion of Dr. Szilard regarding the inclusion of Professor E. Teller of George Washington University and indicated that he would add two scientists conversant with this subject. Such, then, is the diary of the events since our last talk.

Will you be good enough to confirm to me your acceptance and will you also indicate whether you would wish to have a conference prior to our departure, or, alternatively, that we meet Friday night on the 12:50 train from Pennsylvania Station to Washington. In either event, I should like to have you and Dr. Szilard as my guests at breakfast at the Carlton Hotel Saturday morning, and we would thereafter proceed to the Department of Commerce building for our appointment.

Yours sincerely,

Alexander Sachs

Professor E. P. Wigner
Fine Hall
Princeton University,
Princeton, N. J.

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COPY

MEMORANDUM OF LEO SZILARD

August 15, 1949

Submitted to the President

October, 1939

MEMORANDUM

August 15, 1939

Much experimentation on atomic disintegration was done during the past five years, but up to this year the problem of liberating nuclear energy could not be attacked with any reasonable hope for success. Early this year it became known that the element uranium can be split by neutrons. It appeared conceivable that in this nuclear process uranium itself may emit neutrons, and a few of us envisaged the possibility of liberating nuclear energy by means of a chain reaction of neutrons in uranium.

Experiments were thereupon performed, which led to striking results. One has to conclude that a nuclear chain reaction could be maintained under certain well defined conditions in a large mass of uranium. It still remains to prove this conclusion by actually setting up such a chain reaction in a large-scale experiment.

This new development in physics means that a new source of power is now being created. Large amounts of energy would be liberated, and large quantities of new radioactive elements would be produced in such a chain reaction.

In medical applications of radium we have to deal with quantities of grams; the new radioactive elements could be produced in the chain reaction in quantities corresponding to tons of radium equivalents. While the practical application would include the medical field, it would not be limited to it.

A radioactive element gives a continuous release of energy for a certain period of time. The amount of energy which is released per unit weight of material may be very large, and therefore such elements might be used -- if available in large quantities -- as fuel for driving boats or airplanes. It should be pointed out, however, that the physiological action of the radiations

emitted by these new radioactive elements makes it necessary to protect those who have to stay close to a large quantity of such an element, for instance the driver of the airplane. It may therefore be necessary to carry large quantities of lead, and this necessity might impede a development along this line, or at least limit the field of application.

Large quantities of energy would be liberated in a chain reaction, which might be utilized for purposes of power production in the form of a stationary power plant.

In view of this development it may be a question of national importance to secure an adequate supply of uranium. The United States has only very poor ores of uranium in moderate quantities; there is a good ore of uranium in Canada where the total deposit is estimated to be about 3000 tons; there may be about 1500 tons of uranium in Czechoslovakia, which is now controlled by Germany; there is an unknown amount of uranium in Russia, but the most important source of uranium, consisting of an unknown but probably very large amount of good ore, is Belgian Congo.

It is suggested therefore to explore the possibility of bringing over from Belgium or Belgian Congo a large stock of pitchblend, which is the ore of both radium and uranium, and to keep this stock here for possible future use. Perhaps a large quantity of this ore might be obtained as a token reparation payment from the Belgian Government. In taking action along this line it would not be necessary officially to disclose that the uranium content of the ore is the point of interest; action might be taken on the ground that it is of value to secure a stock of the ore on account of its radium content for possible future extraction of the radium for medical purposes.

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Since it is unlikely that an earnest attempt to secure a supply of uranium will be made before the possibility of a chain reaction has been visibly

demonstrated, it appears necessary to do this as quickly as possible by performing a large-scale experiment. The previous experiments have prepared the ground to the extent that it is now possible clearly to define the conditions under which such a large-scale experiment would have to be carried out. Still two or three different setups may have to be tried out, or alternatively preliminary experiments have to be carried out with several tons of material if we want to decide in advance in favor of one setup or another. These experiments cannot be carried out within the limited budget which was provided for laboratory experiments in the past, and it has now become necessary either to strengthen-- financially and otherwise--the organizations which concerned themselves with this work up to now, or to create some new organization for the purpose. Public-spirited private persons who are likely to be interested in supporting this enterprise should be approached without delay, or alternatively the collaboration of the chemical or the electrical industry should be sought.

The investigations were hitherto limited to chain reactions based on the action of slow neutrons. The neutrons emitted from the splitting uranium are fast, but they are slowed down in a mixture of uranium and a light element. Fast neutrons lose their energy in colliding with atoms of a light element in much the same way as a billiard ball loses velocity in a collision with another ball. At present it is an open question whether such a chain reaction can also be made to work with fast neutrons which are not slowed down.

There is reason to believe that, if fast neutrons could be used, it would be easy to construct extremely dangerous bombs. The destructive power of these bombs can only be roughly estimated, but there is no doubt that it would go far beyond all military conceptions. It appears likely that such bombs would be too heavy to be transported by airplane, but still they could be transported by boat and exploded in port with disastrous results.

Although at present it is uncertain whether a fast neutron reaction can be made to work, from now on this possibility will have to be constantly kept in mind in view of its far-reaching military consequences. Experiments have been devised for settling this important point, and it is solely a question of organization to ensure that such experiments shall be actually carried out.

Should the experiments show that a chain reaction will work with fast neutrons, it would then be highly advisable to arrange among scientists for withholding publications on this subject. An attempt to arrange ~~for~~ withholding publications on this subject has already been made early in March but was abandoned in spite of favorable response in this country and in England on account of the negative attitude of certain French laboratories. The experience gained in March would make it possible to revive this attempt whenever it should be necessary.

SECRET

Leo Szilard

copy

Albert Einstein
Old Grove Rd.
Nassau Point
Peconic, Long Island

August 2nd, 1939

F.D. Roosevelt,
President of the United States,
White House
Washington, D.C.

Sir:

Some recent work by E. Fermi and L. Szilard, which has been communicated to me in manuscript, leads me to expect that the element uranium may be turned into a new and important source of energy in the immediate future. Certain aspects of the situation which has arisen seem to call for watchfulness and, if necessary, quick action on the part of the Administration. I believe therefore that it is my duty to bring to your attention the following facts and recommendations:

In the course of the last four months it has been made probable - through the work of Joliot in France as well as Fermi and Szilard in America - that it may become possible to set up a nuclear chain reaction in a large mass of uranium by which vast amounts of power and large quantities of new radium-like elements would be generated. Now it appears almost certain that this could be achieved in the immediate future.

This new phenomenon would also lead to the construction of bombs, and it is conceivable - though much less certain - that extremely powerful bombs of a new type may thus be constructed. A single bomb of this type, carried by boat and exploded in a port, might very well destroy the whole port together with some of the surrounding territory. However, such bombs might very well prove to be too heavy for transportation by air.

The United States has only very poor ores of uranium in moderate quantities. There is some good ore in Canada and the former Czechoslovakia, while the most important source of uranium is Belgian Congo.

In view of this situation you may think it desirable to have some permanent contact maintained between the Administration and the group of physicists working on chain reactions in America. One possible way of achieving this might be for you to entrust with this task a person who has your confidence and who could perhaps serve in an unofficial capacity. His task might comprise the following:

a) to approach Government Departments, keep them informed of the further development, and put forward recommendations for Government action, giving particular attention to the problem of securing a supply of uranium ore for the United States;

b) to speed up the experimental work, which is at present being carried on within the limits of the budgets of University laboratories, by providing funds, if such funds be required, through his contacts with private persons who are willing to make contributions for this cause, and perhaps also by obtaining the co-operation of industrial laboratories which have the necessary equipment.

I understand that Germany has actually stopped the sale of uranium from the Czechoslovakian mines which she has taken over. That she should have taken such early action might perhaps be understood on the ground that the son of the German Under-Secretary of State, von Weizsäcker, is attached to the Kaiser-Wilhelm-Institut in Berlin where some of the American work on uranium is now being repeated.

Yours very truly,

(Albert Einstein)

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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)

RENEWAL OF POLICY OF WITHHOLDING PUBLICATIONS

February 1940

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

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

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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)

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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)

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.

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THE WHITE HOUSE
WASHINGTON

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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.

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Third Approach to the Navy, May 1940

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

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

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

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

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

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

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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)

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420 West 116th Street
New York City
May 30, 1948

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

SECRET

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.

SECRET

May 30, 1940

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

May 30, 1940

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, 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 have your paper 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?

May 30, 1940

Please consider all the information contained in this letter as 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,

(Leo Szilard)

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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. as the 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)

420 West 116th Street
New York City
June 24th, 1940

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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,

SECRET

(Leo Szilard)

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COLUMBIA UNIVERSITY
IN THE CITY OF NEW YORK

Department of Chemistry

June 7, 1940

Dr. Leo Szilard
Pupin Laboratories

Dear Dr. Szilard:

At the suggestion of Admiral Bowman, and with the approval and suggestions of Dr. Briggs, I have been organizing a committee to be called the "Advisory Committee on Nuclear Research." This is to be an advisory committee to the President's Committee on Uranium, which consists of Drs. Briggs, Pegram, Sachs and Einstein. The committee as suggested at present has been chosen from among easterners in order to decrease the expense of meetings and to permit more frequent conferences. It is proposed that the committee shall consist of the following:

H. C. Urey, Chairman
M. A. Tuve
G. Breit
G. B. Pegram
E. Fermi
L. Szilard
E. P. Wigner
E. Teller

In the second place, another advisory committee on the separation of uranium isotopes has been proposed, to consist of the following men:

H. C. Urey, Chairman
J. W. Beams
R. Gunn
E. Fermi
G. B. Kistiakowsky

My colleagues here have been responsible for working me into the position of chairman of both committees. I do not know that I am the best man, but at least I am near to the center of work in this field and have the virtue of being an American citizen, which is probably advisable in this case.

We should like to have you serve on the first committee, for we believe that your advice on problems dealing with uranium fission would be valuable. It is proposed that the first committee shall have its first

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Dr. Leo Szilard

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June 7, 1940

meeting next Thursday, the 13th, in Washington, at the Bureau of Standards at 9 A. M., and I hope very much that you will be there and be prepared to discuss these problems.

We should like to keep the existence of these committees a relatively little publicized matter for one of our objectives is to prevent the dissemination of too much discussion of points which might have military value and if the committees are not known to exist there will be less inquiry about them.

Hoping to see you in Washington.

Sincerely yours,

(signed) Harold C. Urey

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EARLY EMPHASIS ON THE GRAPHITE-URANIUM SYSTEM

July to October 1939

SECRET

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

COPY

SECRET

LETTERS OF LEO SZILARD TO E. FERMI

July 3 and July 8, 1939

SECRET

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} cm². 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 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, SECRET

(Leo Szilard)

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

July 5th, 1939

Dear Fermi:

SECRET

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 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 structure. 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

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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)

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COPY

MEMORANDUM OF LEO SZILARD

submitted to Dr. Briggs

October 26, 1939

SECRET

THE POSSIBILITY OF A LARGE-SCALE EXPERIMENT
IN THE IMMEDIATE FUTURE

SECRET

At present it appears quite possible that a nuclear chain reaction could be set up in a system composed of uranium oxide (or uranium metal) and graphite. The graphite would have to be piled up in a space of perhaps 4 x 4 x 4 metres and might weigh about 100 metric tons. Perhaps 10 to 20 tons of uranium oxide would have to be used, embedded in some such pile of graphite.

The probable success or failure of such a large-scale experiment cannot be forecast at present with any degree of assurance. The properties of a system composed of uranium and graphite have been calculated independently, for a homogeneous mixture, by Fermi, and, for a lattice of spheres of uranium oxide, or uranium metal, embedded in graphite, by myself. The results of these two independent calculations are in reasonable agreement and show that the two arrangements have different properties. For instance, in the case of using a lattice of spheres a great advantage could be obtained by using uranium metal instead of uranium oxide, whereas in the case of the homogeneous mixture the use of uranium metal would be of no great advantage. In spite of these calculations, we cannot foretell with certainty whether or not a nuclear chain reaction can be maintained in such a system because the absorption cross section of carbon for slow neutrons is not sufficiently known.

In order to remove this uncertainty Fermi and I have devised two different experiments by means of which the absorption cross section of carbon, which is very small, could be measured. It is assumed that one of these experiments, or both of them, will be started at Columbia University as soon as the facilities required can be obtained.

If the absorption of carbon should turn out to be comparatively large we could conclude that the large-scale experiment is bound to fail, and in this

case it need not be started. If the absorption of carbon should prove to be exceedingly small the large-scale experiment would appear to very promising, and it can be assumed that everybody will then be in favor of starting it without delay.

Unfortunately, we must be also prepared to find an intermediate value for the carbon absorption. In this case a large-scale experiment will have to be performed in order to find out whether or not a nuclear chain reaction can be achieved with a combination of uranium and graphite. So we may have to make the experiment and risk its possible failure.

It should be borne in mind that a negative result of the large-scale experiment could also be of value by showing with certainty that a chain reaction cannot be achieved with simple means in the near future. Otherwise there remains an ever-present potential threat arising out of experiments on uranium, which are carried out in certain other countries. Therefore, in my personal opinion, a large-scale experiment ought to be performed unless the possibility of its success can be excluded with reasonable assurance on the basis of experiments which are designed to determine the absorption of carbon, or other similar experiments which can be carried out on a moderately small scale.

RECOMMENDATIONS CONCERNING LARGE SCALE EXPERIMENTS.

No expenses need be incurred in connection with large-scale experiments until the absorption of carbon has been measured. On the other hand, steps ought to be taken now in order to prepare the ground for a large-scale experiment, so that this can be started without delay at the proper time. For instance, the possibility of converting uranium oxide into uranium metal ought to be explored. An attempt ought to be made to obtain a promise on the part of certain industrial corporations to supply at the proper time the quantities of the materials, which are required. If possible, these materials ought to be loaned without any financial consideration. Barring an accident in the case of a successful large-scale

experiment, most of the materials used would remain unaffected and could be returned after the experiment is completed.

100 metric tons of graphite represent a value of about \$33.00--at the rate of 15¢ per pound. If a purer brand of graphite has to be used, which rates at 24¢ per lb. the value involved would be \$53,000.

20 metric tons of uranium oxide represent a value of \$100,000.--at the rate of \$2.50 per lb. If it need not be converted into uranium metal but can be used in the form of oxide in the large-scale experiment, this material could be kept pure and could be returned unamaged. It would be desirable to have up to 50 tons of uranium oxide readily available for experiments in the United States.

STATEMENT CONCERNING THE POTENTIAL ASSISTANCE OF THE
UNION MINIERE DU HAUT KATANGA

It would be of particular value to enlist the assistance of this Belgian corporation which is to some extent controlled by the Belgian Government. It appears to be the only corporation which could supply at short notice 20 metric tons of uranium oxide, and probably even 50 tons. I understand that the Managing Director, Mr. E. Sengier, is on a short visit in America.

From conversations which Professor G. B. Pegram of Columbia University had with a representative of the Eldorado Gold Mines, Ltd. it appears that this Canadian corporation might be able to supply uranium oxide for our purposes at the rate of 1 ton per week. If the uranium oxide were to be bought rather than obtained as a gift or a loan, it might be secured from Canada probably just as easily as from Belgium. On the other hand, the Canadian corporation is rather small and can hardly be asked to give away large quantities of material without financial compensation.

So far, radium up to about 2.5 gms. was used in our experiments, and we had to pay a high rent to a subsidiary of the Union Miniere, the only corporation

from which large quantities of radium can be readily rented in this country. An attempt ought to be made to obtain radium for the purposes of such experiments rent-free from the Union Miniere in the future.

Carnotites containing uranium are mined in the U.S.A by the U. S. Vanadium Corporation which is owned by the Union Carbon and Carbide Corporation. A conversation which I recently had with William F. Barrett, Vice-President of this corporation, did not encourage the hope of obtaining large quantities of uranium oxide from this firm, but the issue could perhaps be reopened.

STATEMENT ABOUT URANIUM ORE

As far as I was able to find out, pitchblend, which is an ore rich in uranium, is mined in Czechoslovakia, Canada and Belgian Congo. The total content of uranium in the deposit in Czechoslovakia is estimated to be between 1000 and 1500 tons. The Canadian deposit visibly contains a total of 3000 tons. The amount of pitchblend in the Belgian Congo is not known, but it is believed to be very much larger. In the United States uranium occurs chiefly in the form of carnotites, which is an ore poor in uranium, and is mined for the sake of its vanadium content. The total deposit is estimated to contain 3000 tons of uranium oxide. (Perhaps there are in the United States larger quantities of ore containing a very small amount of uranium which are not included in the above estimate.)

RECOMMENDATION CONCERNING URANIUM ORE

Steps to secure a stock of uranium ores for the government can hardly be recommended at the present time if such steps would involve financial commitments on the part of the government. It might, however, be advisable to begin to study the question in what manner the government could secure such a stock at a later date if required.

For instance, the question has been raised whether it might not be

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possible to obtain for the government a large quantity of pitchblend from Belgium as a token reparation payment. Such a transaction would not cause alarm abroad if it were arranged before the world learns of the results of some successful large-scale experiment. The transaction could be justified without reference to the uranium content of the ore. Pitchblend is also the ore of radium, and action could be taken on the ground of securing the ore for the sake of its radium content, with a view of extracting the radium at some future date for medical purposes. Action taken on this ground alone might in fact be entirely justified.

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