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February 6, 1940

about "Short Version"

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)

originally in Bk. f. 5 (57)

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. 5 (58)

Back of this card, with postmark.

S.b. S.

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(with 1946 revisions)

S6S1

SUMMARY

It is shown that a divergent chain reaction may be maintained in a system composed of uranium and carbon. Conditions particularly favorable for a chain reaction are obtained if instead of using a homogeneous mixture of uranium and carbon a large number of rather small spheres of uranium metal are used embedded in a mass of graphite. The small uranium spheres may form a close-packed hexagonal or cubic lattice embedded in a large sphere of graphite. The average number of fast neutrons emitted by uranium for one thermal neutron absorbed by uranium is calculated from known experimental data and is found to be about 2. In our system conditions for a chain reaction become more and more favorable as the temperature increases and it is shown that we could expect a chain reaction to be self- generating in such a system at about 900° C. even if the cross-section of carbon were as high as 0.01, its present experimental upper limit. As the intensity of the chain reaction increases with increasing temperature the system is thermally unstable. It can be controlled artificially. The time within which the control would have to respond is found to be longer than one second. As much as 100 tons of graphite and 30 tons of uranium might perhaps be required in order to reach the point of divergence at which nuclear transmutation will go on with an intensity limited only by the necessity of avoiding over heating. But in so far as the capture cross-section of carbon is likely to be below 0.01 the amount of material required will probably be smaller.

(With 1946 revisions) 9b 51

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K W thinks this is the old Summary of A55

It ain't

GW

5 b 51

CRITICAL DIMENSIONS

If a large sphere of graphite is used ~~and a neutron source placed in the center~~, the critical value for the radius of the graphite sphere for which the chain reaction becomes divergent may be calculated ~~approximately~~ under various assumptions. The optimum distribution of uranium is not uniform within the sphere and will either decrease or increase with r according to whether we want to have a minimum amount of uranium or a minimum value for l. ~~the critical radius of the graphite sphere~~. The treatment of this question is perhaps best postponed until the value of the carbon capture cross-section is known. It will then be possible to find the optimum distribution of uranium as a function of the distance from the center of the graphite sphere and give a value for l. In the meantime, a very rough approximation may be presented only for the purpose of giving some idea of the order of magnitudes which are involved. If $\sqrt{\overline{r^2}}$ denotes the average distance to which a fast neutron emitted by uranium diffuses away from its point of origin in graphite until it becomes a thermal neutron and reacts with uranium or carbon, then the critical radius l of the graphite sphere is of the order of magnitude of

$$l \sim 3\sqrt{\frac{\overline{r^2}}{\mu q - 1}}$$

Taking as a reasonable value in graphite of density 1.7, $\sqrt{\overline{r^2}} = 50$ cm and $(\mu q - 1) \sim \frac{1}{3}$ we would then have $l \cong 250$ cm corresponding to about 100 metric tons of graphite. The corresponding amount of uranium can be taken from equation No. 33. For $A \cong 76$ cm ; $q_m \cong 0.7$; $\varphi = 0.4$; and R = 5 cm we have

$$\frac{v}{\frac{4\pi}{3}R^3} \sim 100$$

With the densities of 15 for uranium metal and 1.7 for graphite we find for the ratio of the weights about 1 to 10 or about 10 tons of uranium for 100 tons of graphite. The amount of uranium required may be reduced by choosing a smaller value for R than the value corresponding to the maximum value of ε.

Original p. 21 of ~~A 55~~ paper → Phys. Rev.

February 26, 1940

Mr. Howard A. Poillon
President, Research Corporation
405 Lexington Avenue
New York City

Dear Mr. Poillon:

I wonder whether you will remember that I visited you in the spring of 1935. I believe I was introduced to you then by G. B. Pegram while on a visit to New York shortly before my return to Oxford, England. At that time I talked to you about the potential possibilities of producing power by liberating nuclear energy on a large scale and you told me that you did not propose at that time to support any experiments except those in Berkeley and kindly suggested that I get in touch with Ogden in Englandd. By now you have perhaps completely forgotten this incident.

As you can see from the enclosed reprints, I have been recently doing some work along the line which I proposed to follow in 1935. More can be said on this subject than would be wise to say in publications which are printed in periodicals and I should very much appreciate having your comments on a number of questions which arise out of the present situation. If you are free this week perhaps you would be kind enough to have your secretary telephone me at UNiversity 4-2700, Extension 302.

Yours very truly,

(Leo Szilard)

Bk.f. 6 (15)

C
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P
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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.

Lat 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 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 by K.W. Dec. 1967
Copied from white-on-black photostat

Bk. f-6

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112 Mercer Road
Princeton, N.J.

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Yours sincerely,

Albert Einstein (signed)

Typed by K.W.
Copied from white on black photostat in Bk. f.

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420 West 118th 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

Bk.f. 6 (3)

Not sent

March 7, 1940

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

Bk f. 6 (4)

Not sent

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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,

copy of similar letter actually sent (copy to Fold)

S.b.3. bk fldr. 6

Not sent

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Yours very truly,

Einstein (per Lyg. 1265)

S.b.s.
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Yours very truly,

For Einstein Signature
(Rw Aug. 1965)

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)

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

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Sincerely yours,

John T. Tate

John T. Tate,
Editor

JTT:B

N

S.S.S.
bk. folder C

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.

No PL

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Feld (?)

UNDER
KNOTT MANAGEMENT

TELEPHONE
UNIVERSITY 4-2700

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.

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.

Done in 1941.

s.b.s. 19

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

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

Yours sincerely,

(Leo Szilard)

Week of April 25, 1940

Bk f. 6 (24)

Statement by Dr. Sachs.

Early in March (March 7, 1940) 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 (April 22, 1940) 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 ~~xxxxxxxxxxxxx~~ and others. General Watson, on the same date, asked for suggestions from Dr. Einstein and myself as to the supplementary names for attendance at this conference, and so, through the kind offices and direction of Dr. Briggs, this conference has resulted.

This week, (April 25, 1940) 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.

sbs bk 8A

CORNELL UNIVERSITY
ITHACA, NEW YORK

DEPARTMENT OF PHYSICS
ROCKEFELLER HALL

9. Mai 1940.

Lieber Szilard,

Besten Dank für das Manuskript. Sie können dies Zusatzprodukt, sobald es getippt ist. Das Resultat ist sehr einfach: Die Anzahl der von einem schwarzen Detektor pro sec. & und Flächeneinheit absorbierten Neutronen*) ist:

$$I = \frac{1}{8\pi} \frac{Q\, l_f}{L\, \rho^3} \left\{ 1 - \varphi\left(\frac{\rho}{L}\right) \right\}$$

Die Funktion $\varphi\left(\frac{\rho}{L}\right)$ für die Sie eine kurze Bezeichnung, ist Null für $\frac{\rho}{L} = 0$ und z. B. 0,6 für $\rho = L$, und & geht dann schnell gegen 1. L ist etwa 1 km, l_f bekanntlich ca 140 m. Für I Ihre Genauigkeit reichen Sie aber einfach für mässige Abstände ρ mit $I \sim \frac{1}{8\pi} \frac{Q\, l_f}{\rho^3}$

*) erzeugt von Quelle über festem Boden.

Das ist Ihnen hoffentlich einfach gelungen.
Viel scheint es ja nicht zu sein. Ich weiss
nicht wie Ihre Nordpflanze sind, aber
Sie sehen dass der um einen Faktor $\frac{e^2}{s}$ zu
weniger ist als der Effekt einer Quelle im
Vakuum ohne jede Diskussion.

Beste Grüsse

Ihr

C. Planer

sbs 13

Feld ?

AMERICAN INSTITUTE OF PHYSICS

Incorporated

175 FIFTH AVENUE, NEW YORK

Member Societies:

American Physical Society
Optical Society of America
Acoustical Society of America
Society of Rheology
American Association of Physics Teachers

May 20, 1940

Copy

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

Dear Professor Tate:

On February 13th you sent me a short article by Dr. Leo Szilard on "Divergent Chain Reaction in Systems Composed of Uranium and Carbon" for publication in THE PHYSICAL REVIEW when Dr. Szilard released the article. Inasmuch as we have not received this release from Dr. Szilard, I am returning the manuscript to you.

letter to editor form.

Sincerely,

Madeline M. Mitchell

Madeline M. Mitchell
Publications Manager

mmm/pl
encl.
cc: Dr. Leo Szilard ✓

Bk. f. 6 (2)

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.f.8 (30)

Dr Tekin

[illegible]

May 30, 1940

See p 2

MEMORANDUM for Dr. Sachs

Please find enclosed memorandum for Dr. Urey of May 30, 1940. In addition to the items included in the above-mentioned memorandum the following points seem to require attention.

a. It is important that Dr. Urey and the non-governmental members of the Special Advisory Committee be authorized to investigate whether there is a possibility of mining uranium ore in the Belgain Congo and transporting it to this country under the present conditions. If it is considered premature for the Government to buy any uranium ore perhaps some arrangement could be made with Dr. Sengier, the managing director of the Union Miniere who is at present in New York, or through the Belgian Government in exile that uranium ore be brought to the United States with the assistance of the United States Government, the Belgian company retaining the title of this ore but committing itself not to re-export it without special permission. It is impossible to know whether such and other alternative solutions are feasible, unless a preliminary inquiry is made, and it is not advisable to make such an inquiry without proper authorization.

b. It appears necessary that some experimentation be started at once by industrial firms who are willing to supply 10 to 20 tons of uranium metal at about six months notice. It is necessary that the non-governmental members of the Special Advisory Committee and Dr. Urey should be in a position of approaching industrial firms on this subject and should feel authorized to do so.

c. It would be desirable that Dr. Urey and the non-governmental members of the Special Advisory Committee should form the nucleus for a board of trustees and work out the ~~standards~~ statutes for some

PL

Bk.f. 8 (31)

for some non-profit organization which would act as a link ~~as far as the physicists in the universities are concerned form the link~~ between the Government and the university laboratories. If such an organization were formed the physicists ought to be encouraged to take out patents for their inventions which would be assigned either to this non-profit organization or direct to the Government. In any case the Government would thus be safeguarded against having to pay royalties for the use of such inventions, which otherwise might be patented by industrial firms whose research employees begin to show increasing interest in this field of development.

In this connection the question has to be raised whether it is possible to keep such patents secret. In order to do so in an adequate way it might be advisable to modify if necessary the present law. Such a modification of the present law ought of course not to be made exclusively with a view to inventions concerning chain reactions but also with a view to all inventions which have important applications in national defense. The physicists and engineers ought not to be deprived of the stimulus arising out of the possibility of patenting their inventions and at the same time collaborating with the Government in the effort to keep certain of these inventions secret.

THE UNIVERSITY OF MANCHESTER.
DEPARTMENT OF CHEMISTRY

FROM
PROFESSOR M. POLANYI

TELEPHONE: ARDWICK 2681.

June 18th 1940.

Dear Szilard,

I am interested at the moment in devising a method for the purification of the light isotope of uranium. I know that this is a subject on which a large number of workers are engaged and in order not to waste my time I would like to ask you to help me. I would like to know how far the work has been carried out already in the United States and which is considered there to be the most promising line of attack. I believe that the centrifugal method in the form of a fractionation column, as suggested by Urey, is the most hopeful, but I have no experimental experience of anything of the kind and in consequence do not trust my estimate very much. The method of Clusius seems also capable of development to a practicable degree but again I feel uncertain about this possibility.

This letter written by anyone but myself to anyone but yourself would seem very silly but in view of our old friendship you will understand the sort of advice I need in this matter. My only trouble is your unwillingness to write letters but may I please assure you that this may be really important, both to me and to my collaborators in the Physics Department here.

We are not in high spirits here at the moment but are sufficiently composed to carry on with work even though it may not be of immediate utility for the war but, of course, all other interests are almost completely effaced. I hope that I may yet see you and Wigner again and would be grateful to receive a letter from either of you.

Yours ever,

Michael Polanyi

Dr.Leo Szilard,
Pupin Physics Laboratories,
Columbia University,
NEW YORK.

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. 8A (42)

June 24, 1940

MEMORANDUM

In the memorandum which was submitted in the course of a meeting held under the chairmanship of Dr. Briggs on April 27, 1940, I discussed the possibility of using uranium as a source of power for the purpose of driving naval vessels. In the case of a chain reaction maintained in a system composed of carbon and uranium a conservative estimate leads to the prediction that one ton of uranium will be equivalent to about 3,000 tons of oil. Certain recent developments make it appear conceivable that the conditions can be so chosen as to obtain from 1 ton of uranium as much power as from about a few million tons of oil. Professor Louis A. Turner of Princeton sent me a manuscript in which this possibility is discussed. In discussions which Dr. Turner had with Dr. Wigner and myself he expressed his willingness to have the publication of his paper delayed if required. Certain observations made by Macmillan and Abelson which were published in the June 15th issue of the Physical Review opened up the way for investigating the potential possibility discussed by Dr. Turner. By following up the work of Macmillan and Abelson and by carrying out the contemplated general survey of the nuclear constants it will be possible to decide whether 1 ton of uranium "burned" in a system composed of uranium and carbon is capable of supplying as much power as a few million tons of oil or whether it is only capable of supplying as much power as 3,000 tons of oil, the previously given conservative estimate.

L. Szilard

(Leo Szilard)

Bk. f. 8A

June 24, 1940

MEMORANDUM

In the memorandum which was submitted in the course of a meeting held under the chairmanship of Dr. Briggs on April 27, 1940, I discussed the possibility of using uranium as a source of power for the purpose of driving naval vessels. In the case of a chain reaction maintained in a system composed of carbon and uranium a conservative estimate leads to the prediction that one ton of uranium will be equivalent to about 3,000 tons of oil. Certain recent developments make it appear conceivable that the conditions can be so chosen as to obtain from 1 ton of uranium as much power as from about a few million tons of oil. Professor Louis A. Turner of Princeton sent me a manuscript in which this possibility is discussed. In discussions which Dr. Turner had with Dr. Wigner and myself he expressed his willingness to have the publication of his paper delayed if required. Certain observations made by Macmillan and Abelson which were published in the June 15th issue of the Physical Review opened up the way for investigating the potential possibility discussed by Dr. Turner. By following up the work of Macmillan and Abelson and by carrying out the contemplated general survey of the nuclear constants it will be possible to decide whether 1 ton of uranium "burned" in a system composed of uranium and carbon is capable of supplying as much power as a few million tons of oil or whether it is only capable of supplying as much power as 3,000 tons of oil, the previously given conservative estimate.

L. Szilard

(Leo Szilard)

565 20

STATION J

L. Szilard

420 W 116 St.

N.Y.C.

Beryllium

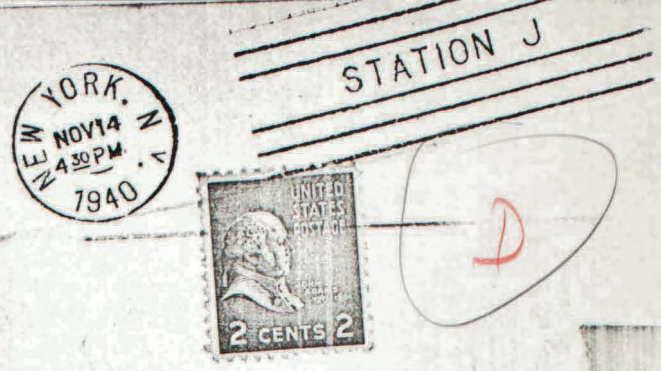
June 28, 1940

postmarked Nov 14, 1940

June 28, 1940

Memorandum.

It would appear that the chances of a chain reaction with slow neutrons in a system essentially composed of uranium and carbon could be considerably improved by having a lattice of spheres of uranium metal embedded in graphite and each sphere surrounded with a spherical shell of beryllium metal. Beryllium metal has a density of about 2, and if the shell had a thickness of about 3 cm this would correspond to about 6 times 10^{-23} atoms of beryllium per cm^3. The binding energy of the neutrons in beryllium being only about 1.8 MEV, the fast neutrons emitted from uranium will knock out a certain number of neutrons of the beryllium shell which will contribute towards a positive balance of neutron emission and absorption in the system. If the average cross-section for this knock out process in beryllium were of the order of magnitude of 10^{-24}cm for uranium fission neutrons then we would have a very considerable contribution from the beryllium shell. The velocity distribution of the uranium fission neutrons which was observed by Zinn and myself makes it appear rather promising to improve the condition for the chain reaction by the use of beryllium, and it would appear that testing this point by a small scale laboratory experiment ought to be considered one of the most urgent tasks within the framework of the proposed survey of nuclear constants. The cost of this experiment would be taken care of by item 6 of the estimate of cost drafted by me on June 19th.



D

L. Szilard

420 W 116 Str.

N.Y.C.

July 4, 1940

Dated Photocopy — postmarked Nov-14, 1940 Book folder 8A

If this small scale experiment gives an encouraging result then we ought to attempt to obtain plates of beryllium metal, for instance plates of sizes 5 x 15 x 15 cm and other plates 5 x 5 x 10 cm. Such plates used in conjunction with a cube of uranium metal can be so arranged as to have a cubic layer of beryllium 5 cm thick surrounding the uranium cube. Each uranium cube would require four of the smaller and two of the larger type beryllium plates. The use of beryllium might be of marked advantage even if the cross-section for the knock out process were as low as $10^{-25} cm^2$ for uranium fission neutrons.

(Leo Szilard)

July 4, 1940

MEMORANDUM

If we used in the chain reaction experiment uranium spheres of 5 cm. diameter surrounded by $2\frac{1}{2}$ cm. layer of beryllium metal we would have about six times as much volume of beryllium as uranium and taking into consideration the ratios of the densities about $\frac{1}{2}$ as much beryllium as uranium. Assuming that beryllium metal may be bought at a price of $10.00 per lb. and uranium metal at a price of $5.00 per lb., 10 tons of uranium metal will be about $100,000. and 5 tons of beryllium would be $100,000. making a total of $200,000.

Bk. f. 8

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

Professor E. Wigner
c/o Physics Department
University of Michigan
Ann Arbor, Michigan

Dear Wigner:

I am end osing a letter which I just had from Polanyi and which does not sound very cheerful. I am embarassed about answering his question since I believe that no information should be sent abroad. However, I shall perhaps try to write up something and give it to Urey with the request of passing it on if Urey is willing to take the responsibility.

I now believe that your resignation was exactly the right thing to do and that it will have some beneficial effect. Urey did not understand your letter but Briggs did. I hope that eventually some framework will be set up for the work on uranium and that I shall then be in the position of persuading you to collaborate, but for the time being there is an increasing amount of confusion and a constant change of the personnel of committees concerning uranium and a growing dissatisfaction on my part as well as on the part of Sachs. I almost reahhdd the point of following your example. I had a growing suspicion during the last fortnight that Pegram's conception concerning the role which I am going to play in the work is very far from my own conception. Finally I decided to explain to him the stand which I propose to take in connection with the principle which is involved and ask him to take any decision which may be required in this connection as soon as possible. Naturally he will consult Fermi before doing so. I am writing you on this in order to keep you informed but I do not think that you ought to intervene in any way. Sachs is very much aroused by the way in which matters are treated in Washington on the part of Briggs and for the present I have to restrain him from taking the matter up with the White House. As far as Wheeler is concerned I do not think that Breit or you need to worry about this aspect for the present. I am keeping him constantly in mind and I believe I can convince Pegram very soon that only part of the necessary experiments can be done at Columbia and that a collaboration with other universities is essential. I am enclosing a copy of a letter I wrote to Wheeler some time ago to show you the line which I am taking in the meantime.

I shall be very glad to see Dr. Torda if I hear from her.

Yours,

(Leo Szilard)

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Bk.f 7

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 convinced 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 this 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)

THE UNIVERSITY OF WISCONSIN
MADISON

DEPARTMENT OF PHYSICS

July 16, 1940.

Dr.Leo Szilard
420 W.116th St.
New York City

Dear Szilard:

I am sorry to be answering your letter with some delay. It is partly due to the fact that I have been waiting for a clarification of some official sides of the arrangements which have not materialized so far.

I wonder whether you would consider it satisfactory to have a request from the President of the National Academy to the Royal Society in which he would ask for a return of manuscripts from the United States having a possible military value. The request could mention several subjects so as not to stress any one of them unduly. I do not see what can be done about French work. In a way it is to our advantage if they publish it in England. If they do not the chances are large that they will publish in France or Germany.

I have written to Tuve, Lawrence, DuBridge and Condon asking their opinion regarding the procedure with papers and asking to be informed concerning the special fields that interest them. I have received no objections and some encouragement.

Personally I am in favor of having a fairly wide circulation of the papers. You will recall, however, that Briggs, Pegram and Urey are not and that their reasons are of an official character. It would help me very much if you were to let me know who, in your opinion, should be informed of the contents of papers and in which branch of the subject. I believe that in the consideration of special cases there will probably not be many objections.

I am very glad to learn that you are considering large scale experiments.

Sincerely,

Gregory Breit

G.Breit

sbs bh 8A

CORNELL UNIVERSITY
ITHACA, NEW YORK

DEPARTMENT OF PHYSICS
ROCKEFELLER HALL

July 15, 1950.

Dear Sziland,

Enclosed with my thanks your undamaged MS. I do not need it at present, but it may be practical if I could refer to it at several passages of one of my future papers. You know how bad my memory is. Therefore I would be grateful if I could dispose of your paper again when I come to the writing up of the passages in question. In order to avoid extralegal procedure, we may consider it as this time not as paper any longer, but as "aide-memoire".

Please let me know about Teller.

Best regards Yours G.P.

CORNELL UNIVERSITY
ITHACA, NEW YORK

DEPARTMENT OF PHYSICS
ROCKEFELLER HALL

July 15, 1950.

Dear Sti Carl,

Enclosed with my thanks your undamaged MS. I do not need it at present, but it may be practical if I could refer to it at several passages of one of my future papers. You know how bad my memory is. Therefore I would be grateful if I could dispose of your paper again when I come to the writing up of the passages in question. In order to avoid extra legal procedure, we may consider it as this time not as paper any longer, but as "aide-memoire."

Please let me know about Teller.

Best regards Yours G.P.

14 August, 1940 Memorandum Report on Proposed Experiments with Uranium

Bk f. 8A.

Excerpt from bottom of p.6 to bottom of p.8

The whole question of an uranium-carbon chain reaction in the light of the 1940 experiments of Fermi and Szilard was the subject for discussion by a special advisory group assembled by Dr. Briggs to advise the Uranium Committee. This group, composed of Messrs. Briggs, Urey, Tuve, Wigner, Breit, Fermi, Szilard and Pegram, met at the Bureau of Standards on June 13, 1940. After full discussion, the recommendation of the group to the Uranium Committee was that funds should be sought to support research on the uranium-carbon experiment along two lines: (A) further measurements of the nuclear constants involved in the proposed type of reaction; (B) experiments with amounts of uranium and carbon equal to about one-fifth to one-quarter of the amount that could be estimated as the minimum in which a chain reaction could sustain itself. It was estimated that about $40,000 would be necessary for the further measurements of the fundamental constants and that approximately $100,000's worth of metallic uranium and pure graphite would be needed for the "intermediate scale" experiment.

The desirability of the measurements of the nuclear constants is obvious. It should be remarked that the immediate value will be to enable the "intermediate experiment" recommended as "B" above, and, subsequently, a full-scale experiment, to be designed with more knowledge than would be possible without the measurements under recommendation "A" above.

As to recommendation "B", the "intermediate experiment", the argument in its favor is the following. As nearly as can be estimated at present the smallest amount of materials necessary to secure a chain reaction with uranium and carbon would be 25 tons of

uranium metal and 60 tons of graphite. This would represent an expenditure of perhaps $500,000. However, even if this rather large amount of material were in hand it would be advisable to proceed only by stages to set up the mass of material presumed necessary for the chain reaction. Measurements taken on the behavior of neutrons in intermediate amounts of the uranium-carbon mixture will not only be of the greatest value in predicting the total amount of material necessary, but will be absolutely essential, from the standpoint of safety, to the persons who are working on the experiment. Since the amount of material required for the chain reaction is certainly not in hand, and since it would cost a large sum of money, it is obvious that progress should be attempted by stages, and it is believed that the first stage should make use of not more than one-quarter of the amount which, so far as present knowledge goes, would be the minimum required for sustaining a chain reaction. It is not believed that there would be any danger in working with this intermediate amount of material, particularly since even this amount of material would not be put together all at once but would be assembled in stages and measurements taken at the several stages. Some question has been raised as to whether this intermediate experiment should be carried out in a university laboratory or in some more isolated spot. Prof. Fermi thinks there would not be the slightest hazard in carrying out the experiment in any laboratory.

Pegrams Memo dated Aug 19, 1940. Exhib. D.

Excerpt from bottom of p.6 to bottom of p.8

The whole question of an uranium-carbon chain reaction in the light of the 1940 experiments of Fermi and Szilard was the subject for discussion by a special advisory group assembled by Dr. Briggs to advise the Uranium Committee. This group, composed of Messrs. Briggs, Urey, Tuve, Wigner, Breit, Fermi, Szilard and Pegram, met at the Bureau of Standards on June 13, 1940. After full discussion, the recommendation of the group to the Uranium Committee was that funds should be sought to support research on the uranium-carbon experiment along two lines: (A) further measurements of the nuclear constants involved in the proposed type of reaction; (B) experiments with amounts of uranium and carbon equal to about one-fifth to one-quarter of the amount that could be estimated as the minimum in which a chain reaction could sustain itself. It was estimated that about $40,000 would be necessary for the further measurements of the fundamental constants and that approximately $100,000's worth of metallic uranium and pure graphite would be needed for the "intermediate scale" experiment.

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420 West 116th Street
New York City

September 29, 1940

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

Dear Wigner:

You have asked me if I had any innome, the source of which is in Germany.

Prior to March 1933 I had an income from the German General Electric Co. (A. E. G.), Berlin, consisting in royalties and consultant's fee amounting to about ℳ 3000.- per annum. The agreement from which this income was derived was terminated before March 1933, and I received no payments from the A. E. G. after that date.

After March 1933 the only source of income which could perhaps be considered as from German origin was an agreement concluded with the Berlin office of Phillips, a well-known Dutch industrial corporation, before March 1933. This agreement provided for the payment of royalties to me in the amount of about ℳ 250.- per annum. The agreement was terminated in or before 1938, and no income from this source was forthcoming after 1938.

Yours sincerely,

Signed
Leo Szilard

56 s 20

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L. Szilard

420 W 116 St.

N.Y.C.

Berlin

Nov. 3, 1940

November 5, 1940

Memorandum concerning the use of beryllium

It may be that conditions for a chain reaction are considerably more favorable by surrounding the uranium spheres with a spherical layer of a few centimeters thickness of beryllium and surrounding such beryllium coated uranium spheres [illegible] masses of graphite. The [illegible] from the breeding [illegible] beryllium according to [illegible] and a fraction of [illegible] above 1.8 million [illegible] of [illegible] the beryllium and thus leading to the liberation of an additional neutron. In order to estimate the [illegible] which can be achieved by the use of beryllium it is proposed to perform an experiment similar to the experiment set forth in the memorandum of October 14th. The spherical uranium shell can be sandwiched between two spherical beryllium shells of a few centimeters thickness each. Otherwise the arrangement is the same as described before. It will be best to use metallic beryllium for this purpose. A preliminary test shows that beryllium filings which are commercially obtainable can be compressed to form a uniform mass and that at the pressure of 100 tons per 2½ sq. centimeters one obtains a bulk density of 1.54 grams per cc. If the facilities for obtaining the rather expensive beryllium are unavailable one might perhaps perform the experiments with beryllium oxide. A preliminary test shows that at the above pressure beryllium oxide calcined at 1450° C are brought to a bulk density of 1.68 grams per cc. and beryllium oxide calcined are brought to a bulk density of [illegible] grams per cc but using higher pressures the bulk density can be

presumably materially increased.

The Beryllium Corporation of Pennsylvania advises me by letter dated July 5, 1940 that they are able to furnish beryllium flakes 98% pure at forty-five dollars per pound, and the Brush Beryllium Company advises me by letter dated August 8, 1940 that their price for small quantities of beryllium oxide is seven dollars per pound.

sbs 20

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L. Szilard

420 W 116 St.

N.Y.C.

Berylleum

Nov. 3, 1940

postmarked Nov 14, 1940

November 3, 1940

Memorandum concerning the use of beryllium

It may be that conditions for a chain reaction are considerably more favorable by surrounding the uranium spheres with a spherical layer of a few centimeters thickness of beryllium and thereby bringing such beryllium quoted uranium spheres [illegible] mass of graphite. The few neutrons emerging from the uranium spheres traversing the beryllium coating and a fraction of the [illegible] neutrons crossing above 1.8 million volts are capable of disintegrating the beryllium and thus releasing two neutrons for an additional neutron. In order to estimate the [illegible] of this advantage which can be achieved by the use of beryllium it is proposed to perform an experiment consisting in the [illegible] set forth in the memorandum of October [illegible]. The spherical uranium shell can be sandwiched between two spherical beryllium shells of a few centimeters thickness each. Otherwise the arrangement is the same as described before. It will be best to use metallic beryllium for this purpose. A preliminary test shows that beryllium chips which are commercially obtainable can be compressed to form a dense mass and that at the pressure of 100 tons per 2? sq. centimeters one obtains a bulk density of 1.54 grams per cc. If the necessity for obtaining the rather expensive beryllium are unavoidable one might perhaps perform the experiment with beryllium oxide. A preliminary test shows that at the above pressure beryllium oxide calcined at 1450° C are brought to a bulk density of 1.65 grams per sq. cent. and beryllium oxide calcined are brought to a bulk density of 2 grams per cc but using higher pressures the bulk density can be

presumably materially increased.

The Beryllium Corporation of Pennsylvania advises me by letter dated July 5, 1940 that they are able to furnish beryllium flakes 96% pure at forty-five dollars per pound, and the Brush Beryllium Company advises me by letter dated August 8, 1940 that their price for small quantities of beryllium oxide is seven dollars per pound.

Sbs 1

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November 22, 1940

Note to Physical Review

In a previous letter to the Physical Review dated and a more detailed paper dated which are both in press I have shown that we may expect to be able to maintain a divergent neuclear chain reaction in a system composed of uranium and carbon. The particular system discussed was so designed as to require a comparatively small amount of uranium and this end was achieved by having a lattice of comparatively small uranium spheres embedded in a large mass of graphite. The spheres of uranium of 4-8 cm. radius may thus be used and the total amount of uranium required may be expected to fall almost within the square of the radius in this interval.

The particular system which was discussed in detail in these papers consisted of a lattice of uranium spheres, the individual spheres having a high density and a radius of the order of magnitude of 5 cm. embedded in a large mass of graphite. It was shown that even if carbon had a noticeable absorption which could be measured with the method described in more detail in the paper, a divergent chain reaction could be maintained in such a system. The purpose of the present note is to communicate a method by which the number of fast neutrons generated in the system is one thermal neutron which is absorbed by uranium within the system can be increased thereby making conditions for maintaining a chain reaction more favorable. This method consists in introducing beryllium into the system in such a manner that the beryllium would be exposed to the fast neutrons emitted from the uranium before these neutrons have been appreciably slowed down by collisions with

carbon. Since the ~~above~~ it was found found "that a considerable fraction of neutrons emitted in fission which is caused by thermal neutrons have energies above the disassociation energy of beryllium, we may expect that a fraction of the fast neutrons emitted from uranium will liberate further neutrons by disintegrating the the beryllium. ~~It appears worth emphasizing that~~ (Fermi) if the over all cross section of such a disintegration process of beryllium were as low as ~~10 to 25 sq~~ 10^{-25} cm.^{2} we still ~~would~~ may obtain an increase in neutrons of ~~10 25%~~ 10 to 25% by surrounding each uranium sphere with a spherical layer of metallic beryllium having a thickness of 5 cm. The total amount of beryllium required would in this arrangement be about in weight of the amount of uranium used ~~and~~ ~~xxxxx~~ For this reason and also because the beryllium is concentrated in location where the thermal neutrons density is lowest the capture cross section of beryllium for thermal neutrons would be considerably higher than that of carbon without appreciably decreasing the efficiency of the arrangement for the purpose of maintaining a chain reaction,

C-1A

PATENT December 9, 1940

Use of Beryllium.

Beryllium may be used in the form of beryllium metal either cast in vacuum or pressed from beryllium flitters by applying a pressure of about in order to obtain the beryllium metal in the suitable shape and at a density between 1.5 gm per cc and 1.9 gm per cc.

Beryllium may be used for instance in the form of spherical shells which surround a sphere of uranium metal, the radius of the uranium sphere being for instance 3 cm and the thickness of the beryllium shell being about 5 cm.

Beryllium may also be used in the form of a sphere of about 2 cm radius surround by a spherical shell of uranium metal which made up in two hemispherical shells, the uranium metal having a density of more than 9 gm per cc, preferably a density of about 18 gm per cc, and the uranium shell having a thickness of about 3 cm or more. This arrangement is less efficient from the point of view of the chain reaction but has the advantage of requiring a smaller total amount of beryllium metal which at present is rather expensive.

Beryllium may be used in the form of a cylindrical arrangement, in which case we have song cylinders of uranium surrounded by a cylindrical spherical shell of beryllium metal or cylindrical bodies of beryllium surrounded by a cylindrical shell of uranium metal. In the case of the cylindrical arrangement this second alternative, i.e. having the uranium outside and the beryllium inside is almost equally good as the other arrangement, namely having the beryllium outside and the uranium inside, from the point of view of the efficiency of the chain reaction, and it has the advantage of requiring a smaller total amount of beryllium.

Plane Arrangement as shown in Figure.

Arrangement in which cylinders having the form of tubes form a cage-like construction as shown in figure.

C-4A

PATENT December 9, 1940

Method of Cooling.

1. No cooling liquid inside the graphite-uranium system.

2. Cooling by liquid bismuth, the bismuth surrounding the uranium spheres; the bismuth flowing in graphite channels and not in iron pipes.

3. Cooling by some cooling liquid, for instance a bismuth-lead compound containing 60% of bismuth, melting at 126°, flowing inside a uranium tube inside a uranium cylinder. This method can be used only if cylindrical bodies of uranium are embedded in graphite. In this arrangement liquid mercury could be used instead of liquid bismuth or bismuth alloys; also perhaps water. Note: melting point of bismuth 322°; melting point of lead 326°. A Pb-Sn alloy containing 70% Sn melts at 185°. There may be suitable Sn-Pb-Bi alloys. Boiling point of bismuth is at 1470°. Boiling point of lead is at 1613°.

Heating up.

If the cooling medium is used which becomes solid at room temperature it may be necessary to heat up the carbon-uranium system in order to start the machine. ~~xxxxxxxxx~~ Another reason for heating up may lie in the fact that the graphite used contains impurities which have an appreciable thermal absorption. In the case of an appreciable thermal absorption in the graphite, whether due to impurities of a certain kind or to the carbon itself, the efficiency of the arrangement for the chain reaction increases with ~~x~~ increasing temperatures within the range between room temperature and the highest temperature which is practicable in such machines. This in itself may be a reason for heating up the carbon-uranium system in order to start the chain reaction, and the temperature will then be maintained at a high level, perhaps at 800° during the operation of the machine by the heat which

is liberated in the chain reaction.

In order to heat up the graphite-uranium system heating elements may be pushed into cavities in the graphite and these elements must then be withdrawn in order to start the chain reaction, otherwise their thermal neutron absorption may interfere with the chain reaction.

PATENT December 9, 1940

Method of Cooling.

1. No cooling liquid inside the graphite-uranium system.

2. Cooling by liquid bismuth, the bismuth surrounding the uranium spheres; the bismuth flowing in graphite channels and not in iron pipes.

3. Cooling by some cooling liquid, for instance a bismuth-lead compound containing 60% of bismuth, melting at 126°, flowing inside a uranium tube inside a uranium cylinder. This method can be used only if cylindrical bodies of uranium are embedded in graphite. In this arrangement liquid mercury could be used instead of liquid bismuth or bismuth alloys; also perhaps water. Note: melting point of bismuth 322°; melting point of lead 326°. A Pb-Sn alloy containing 70% Sn melts at 185°. There may be suitable Sn-Pb-Bi alloys. Boiling point of bismuth is at 1470°. Boiling point of lead is at 1613°.

Heating up.

If the cooling medium is used which becomes solid at room temperature it may be necessary to heat up the carbon-uranium system in order to start the machine. ~~xxxxxxxx~~ Another reason for heating up may lie in the fact that the graphite used contains impurities which have an appreciable thermal absorption. In the case of an appreciable thermal absorption in the graphite, whether due to impurities of a certain kind or to the carbon itself, the efficiency of the arrangement for the chain reaction increases with ~~x~~ increasing temperatures within the range between room temperature and the highest temperature which is practicable in such machines. This in itself may be a reason for heating up the carbon-uranium system in order to start the chain reaction, and the temperature will then be maintained at a high level, perhaps at 800° during the operation of the machine by the heat which

is liberated in the chain reaction.

In order to heat up the graphite-uranium system heating elements may be pushed into cavities in the graphite and these elements must then be withdrawn in order to start the chain reaction, otherwise their thermal neutron absorption may interfere with the chain reaction.

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PATENT December 9, 1940

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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.

C
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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. 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)

Bkf.7

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, 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 [illegible] things in greater detail. Could you possibly let me have

your Woodsholo address?

Please consider all the information contained in this letter as ~~very~~ 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)

3k f 7

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 convented 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. 6 (11)

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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)

C
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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

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.

Bk. f.6

C
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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.

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Yours sincerely,

(Leo Szilard)

Bk f. 8

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 Bowen, 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, Saks 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 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,

Harold C. Urey

Harold C. Urey