

c/o Department of Physics
Columbia University
New York, N.Y.

October 26th, 1939

Dr. Lyman J. Briggs
U.S. Bureau of Standards
Connecticut Avenue
Washington, D.C.

Dear Dr. Briggs:

Enclosed you will find a memorandum in which the statements and recommendations made by me at the meeting of October 21st are repeated and somewhat amplified.

Both at the meeting and in the memorandum I have refrained from putting forward a detailed plan for promoting further research on uranium. Having recently started conversations on this subject with Dr. Pegras, Dr. Fermi, Dr. Wigner and others, I feel that it is best to limit myself to general recommendations until a consensus of opinion on details has been reached.

I personally believe that if sufficient interest in the subject could be aroused, intensive research on uranium might be carried on at four or five different laboratories. Columbia, the Carnegie Institute for Terrestrial Magnetism, the University of Virginia, M.I.T and Princeton were so far tentatively mentioned in this connection. If a committee, foundation, or some other non-profit organization considered it his task to encourage research on uranium, and had the approval of the government, it could approach the presidents of certain universities in order to obtain the release of some younger physicists from their teaching duties. These men could then devote their

Boston

entire time to experiments on uranium, which they might want to undertake. They could work either at their own universities, or could work as guests of one of the four or five universities at which larger groups are active on the same subject. In a year or two these men could return to their regular work, and we would thus avoid creating the problem of how to place them later. Such a problem might arise if some of the alternative schemes that have tentatively been put forward were adopted. Also, by proceeding in this way we could avoid interfering with existing research projects in various physics departments, which would inevitably suffer if a large number of men in any single department were persuaded to work on uranium.

One point which might have to be considered in this connection is the following: some of the work which has to be done may be of such nature that the publication of the results had better be avoided. For a young physicist, who has not yet made a name for himself, refraining from publication means a sacrifice which he should not be asked to make without being offered some compensation. Some addition to the salary which he is normally drawing from his university might therefore be desirable and might require the creation of some special fund. This observation is based on experiences gained early in March, when Fermi and I agreed to delay the publication of our experiments on the neutron emission of uranium and attempted to obtain the cooperation of French and English physicists with regard to withholding all publications on this particular subject. I am enclosing for your information copies of the letters and cables exchanged on this issue between February 2nd and April 19th of this year.

Copies of the enclosed memorandum will be sent by me to Dr. Wigner and Dr. Teller, who are old personal friends of mine and with whom I have been in almost constant consultation on this subject since January of this year. I shall also send copies to Dr. Alexander Sachs, Professor G.D. Fogarty and Professor E. Fermi. Three additional copies will be sent to you, to be used at your convenience.

Yours sincerely,

(Leo Szilard)

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MEMORANDUM OF LEO SZILARD

submitted to Dr. Briggs

October 26, 1939

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

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 $4 \times 4 \times 4$ 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 be 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 \$ 33,000—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 undamaged. 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

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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WARDMAN PARK HOTEL

Washington, D. C.

CONNECTICUT AVENUE & WOODLEY ROAD
1800 ROOMS

October 21, 1939.

Prof. G. B. Fogran,
Physics Department,
Columbia University,
Broadway at 120th Street,
New York City.

Dear Professor Fogran:

Oct 20,
1939

I wish to give you a short account of yesterday's meeting, at which Briggs acted as chairman. I will give you a longer account in the form of a memorandum, which I am now writing and which I will leave with Briggs before returning to New York. This memorandum is essentially a repetition of the statements and recommendations which I made at the meeting, and it serves the purpose of making things easier for Briggs, when he writes his own report.

On the whole everything came off as could be expected. Teller, who returned from New York, where he spoke with Tuve and Ferni, acted in a double capacity - speaking once in his own name and once in the name of Tuve, who was asked by Briggs to attend the meeting but was unable to come. Tuve put forward certain recommendations which he has discussed and on which he has agreed with Ferni. He said that government funds ~~are~~ to be made available for our graphite absorption experiment at Columbia, and named a specific sum, which I do not remember. He also named a sum which he thought ought to be given for purposes of isotope-separation to the University of Virginia, and so on. These recommendations, though they were beside the point, had nevertheless a beneficial effect. The diversion of government funds for such purposes as ours appears to be hardly possible, and I have therefore myself avoided to make any such recommendation, but Tuve's suggestion provoked detailed discussion of the proposed experiments, and the representative of the Army and the Navy almost committed themselves to the extent of providing some four metric tons of graphite for ~~the~~ experiments, if we so desire.

I was astonished how active and enthusiastic Mr. Lavee was during the meeting, and was most favorably impressed. After the meeting he asked me jokingly to confess that I suspected that he was no good, that he would really never get anything done, and that I was surprised, when the time came, that he really became active and started to do things. It seems to me now that he is performing his task efficiently and in the right spirit, and now I am in favor of giving him a fairly free hand, and see what he can achieve.

I expect to be in New York by Tuesday night at the latest.

Yours sincerely,

(Leo Ballard).



WARDMAN PARK HOTEL

Washington, D. C.

CONNECTICUT AVENUE & WOODLEY ROAD
1800 ROOMS

October 21, 1939.

Prof. G. B. Pogram,
Physics Department,
Columbia University,
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Boston

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Yours sincerely,

(Leo Szilard).

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Washington, D. C.

CONNECTICUT AVENUE & WOODLEY ROAD
1800 ROOMS

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Prof. G. E. Pogram,
Physics Department,
Columbia University,
Broadway at 120th Street,
New York City.

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I expect to be in New York by Tuesday night at the latest.

Yours sincerely,

(Leo Szilard).

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KING'S CROWN HOTEL
420 West 116th St.
New York, N.Y.

October 18, 1939

Mr. William F. Barrett, Vice President,
Union Carbide and Carbon Corporation,
30 East 42nd Street
New York City

Dear Mr. Barrett:

Oct 16
Enclosed I am sending you a short memorandum referring to the conversation which we had on Monday this week. We have calculated how much graphite we would need for the preliminary experiment, which we propose to start immediately, and find that we could probably manage with 4 metric tons. As you will see from the enclosed copy of a letter of the National Carbon Company the price quoted for this amount of graphite is about \$3500.00. As soon as you let us know whether we can have this amount of graphite we would take all the necessary steps for preparing this experiment.

I have telephoned to your office today and left a message with Mr. Mills in order to ask you whether you would care to meet Professor Pegram, who is in charge of the Physics Department at Columbia University, and Professor Enrico Fermi one of these days for lunch at the Faculty Club. If you let me know what days would be convenient to you I would find out when the others are free and communicate with you.

On Friday I have to leave for Washington where a meeting has now been fixed for Saturday, but I hope to be back by Tuesday at the latest.

Yours very sincerely,

(signed)

(Leo Szilard)

(typewritten)

Boston

October 18, 1939

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MEMORANDUM

Recent results concerning the possibility of setting up a nuclear chain reaction in uranium make it appear desirable that we should establish some sort of cooperation with the Union Miniere. Just what form of cooperation would be most appropriate has not been decided as yet. I have seen Mr. Jean E. V. Cattier, whose father is President of the Union Miniere, and arranged with him to meet in the near future the Managing Director, Mr. Sengier, who is now here on a visit.

It appeared desirable that the firms who use American uranium ores should be contacted before any definite arrangements are made with the Union Miniere, especially since, in an emergency, the United States might be cut off from Canadian and Belgian supplies. I was advised that most of the carnotites containing uranium are mined by the Vanadium Corporation of America, which is a subsidiary of the Union Carbide and Carbon Corporation. This was primarily the reason for my contacting the Carbide and Carbon Corporation.

A second reason for my contacting this corporation was the fact that an experiment is being considered for which about 50 tons of graphite might be required. This experiment also requires a large quantity of uranium oxide. It is assumed that it will be possible to obtain the uranium oxide required for this experiment as a loan from the Union Miniere. While it is impossible to say with certainty that this experiment will lead to a large scale liberation of nuclear energy, there is a good chance that this will be the case. Obviously, the success of the experiment would lead to a great increase in the value of uranium and would thereby directly benefit those who control the supply of uranium ores. The cooperation of the Union Miniere and of the Union Carbide and Carbon Corporation would appear to be justified on this ground alone, even without emphasizing the possible importance of these experiments from the point of view of national defense.

50 Metric tons of graphite, which might possibly be required for the experiment which is envisaged, represent a value of about \$16,000.00 if ordinary graphite is used, which rates at a price of 15 cents per pound. However, it seems that a special brand of graphite has

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Oct. 18, 1939

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to be used, for which the National Carbon Company quotes a price of 35 cents per pound; the value involved would then be about \$40,000.00. A letter of the National Carbon Company containing a price quotation is enclosed.

It seems impossible to foresee with certainty the outcome of the proposed large scale experiment, and it appears that we simply will have to have the courage to embark on it on a fifty-fifty chance for success and failure.

The estimate of the chances of this experiment might be slightly improved by investigating the properties of graphite in a separate experiment which we propose to start as soon as possible. This requires 4 metric tons of graphite of the grade specified in the enclosed letter of the National Carbon Company, and on the basis of the quotation contained in this letter this amount would represent a value of about \$3500.00. The Physics Department at Columbia University has at present no funds available for the purpose of this experiment, but it is believed that, if the National Carbon Company would consent to supply the graphite material, the other facilities required could be obtained from the Rockefeller Foundation or some other Foundation. An early decision on this point would be appreciated.

(signed) LEO SZILARD

Bk. f. 5. (5) X

August 15th, 1939

Dr. Alexander Sachs
One William Street
New York City

Dear Dr. Sachs:

Enclosed I am sending you a letter from Prof. Albert Einstein, which is addressed to President Roosevelt and which he sent to me with the request of forwarding it through such channels as might appear appropriate. If you see your way to bring this letter to the attention of the President, I am certain Prof. Einstein would appreciate your doing so; otherwise would you be good enough to return the letter to me?

If a man, having courage and imagination, could be found and if such a man were put - in accordance with Dr. Einstein's suggestion - in the position to act with some measure of authority in this matter, this would certainly be an important step forward. In order that you may be able to see of what assistance such a man could be in our work, allow me please to give you a short account of the past history of the case.

In January this year, when I realized that there was a remote possibility of setting up a chain reaction in a large mass of uranium, I communicated with Prof. E.P. Wigner of Princeton University and Prof. E. Teller of George Washington University, Washington, D.C., and the three of us remained in constant consultation ever since. First of all it appeared necessary to perform certain fundamental experiments for which the use of about one gram of radium was required. Since at that time we had no certainty and had to act on a remote possibility, we could hardly hope to succeed in persuading a university laboratory to take charge of these experiments, or even to acquire the radium needed. Attempts to obtain the necessary funds from other sources appeared to be equally hopeless. In these circumstances a few of us physicists formed an association, called "Association for Scientific Collaboration", collected some funds among ourselves, rented about one gram of radium, and I arranged with the Physics Department of Columbia University for their permission to carry out the proposed experiments at Columbia. These experiments led early in March to rather striking results.

At about the same time Prof. E. Fermi, also at Columbia, made experiments of his own, independently of ours, and came to identical

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conclusions.

A close collaboration arose out of this coincidence, and recently Dr. Fermi and I jointly performed experiments which make it appear probable that a chain reaction in uranium can be achieved in the immediate future.

The path along which we have to move is now clearly defined, but it takes some courage to embark on the journey. The experiments will be costly since we will now have to work with tons of material rather than - as hitherto - with kilograms. Two or possibly three different alternatives will have to be tried; failures, set-backs and some unavoidable danger to human life will have to be faced. We have so far made use of the Association for Scientific Collaboration to overcome the difficulty of persuading other organisations to take financial risks, and also to overcome the general reluctance to take action on the basis of probabilities in the absence of certainty. Now, in the face of greater certainty, but also greater risks, it will become necessary either to strengthen this association both morally and financially, or to find new ways which would serve the same purpose. We have to approach as quickly as possible public-spirited private persons and try to enlist their financial co-operation, or, failing in this, we would have to try to enlist the collaboration of the leading firms of the electrical or chemical industry.

Other aspects of the situation have to be kept in mind. Dr. Wigner is taking the stand that it is our duty to enlist the co-operation of the Administration. A few weeks ago he came to New York in order to discuss this point with Dr. Teller and me, and on his initiative conversations took place between Dr. Einstein and the three of us. This led to Dr. Einstein's decision to write to the President.

I am enclosing memorandum which will give you some of the views and opinions which were expressed in these conversations.

I wish to make it clear that, in approaching you, I am acting in the capacity of a trustee of the Association for Scientific Collaboration, and that I have no authority to speak in the name of the Physics Department of Columbia University, of which I am a guest.

Yours sincerely,

(Leo Szilard)

UNIVERSITY OF MICHIGAN
ANN ARBOR

DEPARTMENT OF PHYSICS

July 9 1939

Dear Szilard,

Thank you for your letter. I was also considering the possibility of using carbon for slowing down the neutrons; in the obviously optimistic hypothesis that carbon should have no absorption at all for neutrons, and assuming for the resonance absorption band of uranium the usual data (which also I rather suspect to be optimistic) one finds from an elementary calculation that the ratio of the concentrations (ratio of the numbers of atoms) of uranium and carbon should be about ~~10~~ one thousandth in order to avoid too much resonance absorption. According to my estimates a possible recipe might be about 39000 Kg. of carbon mixed with 600 Kg of uranium. If it were really so the amounts of materials would certainly not be too large.

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

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

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

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

Yours sincerely

Enrico Fermi
Enrico Fermi

P.S. I have received your second letter. If heavy water is too expensive, as I believe, it would be important to find some way of knowing some

thing of the carbon absorption. It seems to me that the use of very thick layers of C might do the trick

Yours
Lewis Fermig

~~Eastman Kodak Corp~~
Charleston and Carbon Co
New York

Eastman Carbon Co
New York
Dr. P.O.
Came Dr. P.O.

July 3rd, 1939

Mr. Lewis L. Strauss
52 William Street
New York City

Dear Mr. Strauss:

These are just a few lines to refresh your memory in case you find time to contact the Société Générale or the Union Minière.

As you know Fermi and I made a number of experiments on uranium, some of these independently of each other, others jointly. All these experiments were carried out at the Physics Department of Columbia University with uranium rented from the American agents of the Union Minière. In order to meet these and other expenses which would have strained the budget of the Department, other physicists and I formed an association called "Association for Scientific Collaboration" and collected some funds among ourselves. I am writing to you in my capacity as one of the trustees of the Association rather than on behalf of the Physics Department, as I have not yet discussed the matter with the Head of the Department and have no authority to speak in the name of the Department.

A joint paper by Anderson, Fermi and myself, which has

been recently completed and is not yet published, states that a nuclear chain reaction could be maintained under certain conditions in uranium, but expresses serious doubt whether such a chain reaction can be maintained in uranium oxide, or in uranium oxide mixed with water. It is my personal opinion that a chain reaction leading to the formation of practically unlimited amounts of radioactive material is an immediate possibility, though it requires careful control of the conditions under which the experiment is performed.

There is also a 50 to 50 chance that the matter may be of great importance from the point of view of national defense.

In the circumstances it seems to me that this work should be supported financially in a more efficient way than hitherto, and I wondered whether the Union Minière, as the company who would have the chief financial benefit of any use to which uranium is put, would be willing to support this work.

This support could take various forms: First of all, we would appreciate assurance that we can obtain at short notice 30 tons of uranium oxide on loan, of which we would probably need 5 tons in the next six weeks. Secondly it would be of great help if we need not pay rent for radium up to 2.5 gm, as long as we use it in the form in which the company has it in stock rather than in some other form. Thirdly, we would appreciate it if we could obtain refund of the rent which we paid between February and to date for radium loaned out of the stocks of the Radium Chemical Co., the American agent of the Radium

Belge.

I am mentioning these details though I feel that a general agreement for co-operation along some broad lines would do much towards obtaining results quickly.

I personally have no doubt that ~~it~~ the Union Minière would prefer to loan the uranium to Columbia University rather than to a comparatively young "association" such formal difficulties that might exist could be overcome by ~~the~~ general good will.

I would much appreciate if you could let me know by cable from England whether you were able to see any of the directors of the Union Minière by the end of July, because if you see for instance that you will not find time to deal with this matter I would like to attempt to contact the Union Minière at once through Professor Biot who is at present in Belgium.

Forgive me please for troubling you with all this on the last day, but I assume that you might find time to read this letter on the boat.

Wishing you and Mrs. Strauss a pleasant stay in Europe,

I am, yours sincerely,

(Leo Szilard)

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 ~~section~~ 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,

(Leo Szilard)

History A 100

D 7

April 23, 1939

Memorandum

By comparing the values for the fission cross section, the absorbing cross section and the balance of neutron production and absorption, as manifested in experiments in which neutrons emitted from uranium are slowed down and the total number of the slow neutrons present is measured, it appears at present almost certain that the number of neutrons emitted per fission from uranium is about two. I believe that experiments which are now being carried out will confirm this in the near future. It seems to me that it should then be possible to decide the question whether or not a chain reaction with fast neutrons is possible. The following simple experiments will decide this question, perhaps not with certainty, but with a high degree of probability.

An uranium compound is to be exposed to thermal neutrons, the radioactive uranium having a 25 minute period which is produced by radiative capture, and is separated from the other activities which are produced by fission. Both activities are measured by a beta ray counter or by a ionisation chamber, and the ratio: $(R = \frac{\text{activity due to capture}}{\text{activity due to fission}})$ of the two - in arbitrary units - is determined. The same uranium compound is then exposed to photo neutrons from a radon-beryllium source, and the ratio $(\beta = \frac{\text{activity due to capture}}{\text{activity due to fission}})$ of the two activities, observed in the same way as before and expressed in the same arbitrary units, is

again determined.

The quotient $\frac{B}{F} = C$ is a measure of the shift in balance between neutron absorption and neutron production by changing over from thermal neutrons to photo neutrons. If C is small, for instance if it is smaller than one could be almost certain that a chain reaction can be maintained in uranium in the absence of hydrogen containing substances.

It seems therefore reasonable to perform the above mentioned experiments, and, if the outcome is positive, to take at once steps in the direction of a large scale experiment in the absence of hydrogen containing substances. It is to be expected that neutrons emitted from uranium will be slowed down by inelastic collisions in uranium to velocities between 10.000 to 100.000 volt, and therefore behave in a way very similar to photo neutrons from a radon-beryllium source. It may be added that the number of neutrons emitted by fission with energies above 200.000 volt seems to be only about one per fission, and that we therefore suspect a large number of neutrons to be emitted with energies below 200.000 volt. We suspect therefore that the slow neutron density at the surface of a sample of uranium will be appreciably increased by these comparatively slow fission neutrons, and this point is now being checked by comparing the apparent absorption of a sample of uranium metal with a sample of boron, both in a narrow and in a wide paraffin cavity.

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

April 18th, 1939

Dear Goldhaber:-

Congratulations!

I assume that you are going to the Washington meeting. If so, wouldn't you like to spend a few days in New York? I am sure you could help us with our experiments if you were here. So try to arrange matters so that you pass through New York either on your way to Washington or on your way back to Urbana.

Though I have written to Joliot early in February, as you will see from the enclosed letter, it was not possible to arrange a co-operation with Paris. Essentially for this reason Pegrum has finally decided to let the papers appear, and you will see them in the next issue of Physical Review.

Let me know your plans and also if you succeeded in getting the visitors visa for T. S.

Yours,

(Leo Szilard)

P.S. Please could you send me as soon as possible 50 to 100 reprints of our joint indium paper?

THE UNIVERSITY OF ROCHESTER
COLLEGE OF ARTS AND SCIENCE
ROCHESTER, NEW YORK

Bk. f. 3 (38)

[1939-KW]

DEPARTMENT OF PHYSICS

April 17.

Lieber Szilard,

Danke für Ihren Brief. Sie haben hoffentlich
Blackett vom Zusammenbruch der Kooperation
telegraphisch mitgeteilt.

Ich bin sehr neugierig auf die Resultate
Ihrer weiteren Versuche. Ich bin Donnerstag u.
Freitag in Washington, Samstag u. Sonntag in
New York. Die langsame Bombe dürfte nicht
fehlen auf Grund von Diskussionen zwischen Teller, Rorer,
Bethel u. mir.

Regl. Gross

Herr Wissenschaftl

This half of the *Bkfolster* No. 2 (2)
Wigner moved to sbs 9, to be
with the other half

April 17, 1939

Dear Szilárd !

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.

I received a communication from Polanyi's secretary in reply to the cable which I sent to him concerning the Be block. She says that Polanyi had a motor accident about a month ago and is abving now a month's rest in southern England. The Secretary writes that she communicated with Tuck and Griffiths and that the block will be with her, no doubt, in a few days from the date (April 6) of her letter. She is going to forward it then to my address by a fast boat.

Unfortunately, the Secretary wrote to my Madison address and I suspect that she will send the block to the same place. This may entail difficulties with the custom examination which would, under these conditions, naturally take place in Milwaukee. I am writing, therefore, to the Secretary of the Physics Department in Madison to have the block forwarded to this address if possible but at any rate to let me know immediately if she has any news in connection with it.

I just wrote to Polanyi and told him that you will write to him explaining the situation with the block and why you needed it. I am unfamiliar with the circumstances myself.

Hoping to hear from you very soon,

Sincerely

Wigwam

April 17, 1939

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Not sent to Howard

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Hoping to hear from you very soon,

Sincerely

Wigwam

LEWIS L. STRAUSS
52 WILLIAM STREET
NEW YORK

publicizing
Bkf.3 (20)

April
Seventeenth
1 9 3 9

Dear Dr. Szilard,

It was good of you to send me the very interesting manuscript of your paper which I have read with the greatest interest. I share your regret that it was not possible to defer publication of these developments at this more or less critical juncture.

Hoping to see you within the very near future, I am, as ever,

Faithfully yours,

Lewis Strauss

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

MG

... to the ward

LEWIS L. STRAUSS
52 WILLIAM STREET
NEW YORK

~~Handwritten~~
Handwritten notes

April
Fourteenth
1 9 3 9

Dear Dr. Szilard,

I was very much obliged indeed to receive your letter of April eleventh as I had begun to feel rather out of touch with what was going on.

I presume you know that Dr. Brasch has returned here for a visit and that you have been in touch with one another. If you have some time next week I hope I shall see you.

Nils Bohr came in to see me a few days ago. It was the first time I had ever met him and I was tremendously impressed with his simplicity and obvious greatness. Have you considered discussing your ideas with him?

Faithfully yours,

Lewis Strauss

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

MG

sent Oct 27, 1967

UNIVERSITY OF ILLINOIS
DEPARTMENT OF PHYSICS
URBANA

April 12, 1939

Dear Szilard,

Thank you for the paper. It reached me just before I was leaving for a trip to Madison, Wisconsin, together with Rabi.

I suppose you would have detected delayed neutrons with the He chamber if these neutrons had an energy of ~ 5 M.e.v. as Roberts et al. estimate. This makes either Roberts' estimate of the cross section for production of the 'delayed neutron' emitter much too large or your multiplication value too small. Roberts' estimate seems on the face of it unlikely high. - Have you any idea how fast your ^{multiplication} neutrons are?

Of course, it is a good idea to keep results secret until it is settled whether the U-bomb is possible, in reasonable dimensions, or not. Let us hope not. But if yes, it is important to be a step ahead of the dictator countries, though I fear it means only a step ahead. Have you thought of definite steps of buying off all U. ^(etc.!!) supplies? A map of their distribution on the earth looks a

little discouraging, though it is only a qualitative
one, which I looked up. — I fear you will
not be too successful with the attempt to keep
results secret. The first indications that
people in Berlin are doing similar work appear
in Naturwiss. ^(arrived to-day) in an article of Droste who with
Reidemann is looking for fourth ventricles,
when D+D ventricles are used on U. He mentions
this at the end of a letter. As soon as the
first papers from Germany appear, I am
sure, many of those who have agreed to secrecy
will see no further good in it, ^{and} ~~but~~ though
they are right as far as the ultimate results
are concerned, the immediate effect of secrecy
is very important. Have you any results on Tu?

Talking of 'secrets': Please keep this a secret for
the time being. I have become engaged to Trude
Schroff, and working on the hypothesis that there
will be no war within the next few weeks (a very
weak hypothesis, I fear) I have sent her an invitation
(from Louis) to come here on a visitor's visa.
When she is here we can get married and then she
can immigrate non-quota via Canada. This seems
the only workable plan in a hurry.

I have written to Fowler.

Kind regards

Yours A. Goldhaber

Paul Arthur

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

April 11th, 1939

Mr. Lewis L. Strauss
25 East 76th Street
New York City

Dear Mr. Strauss:-

These are just a few lines to keep you informed of how things have developed since I last saw you. Fermi and I were sorry that we could not see you in Virginia when we were in Washington.

The following is for your private information only. Co-operation was established in Washington with the Carnegie Institute for Terrestrial Magnetism, and also contacts were made through the official channels via the Physics Department of Columbia University with the Navy. These contacts are perhaps too loose, but for the present this is of no importance.

(M. Tuve)
[K.W.]

Since my return from Washington I cut down all extra-laboratory activities and tried to get more information about the number of neutrons emitted, which is the most important point at present. Though this number seems to be above one, I am still not certain about it. Fermi bases his plans on the belief that the number is larger than one.

Accordingly we are preparing an experiment on a semi-large scale, using 500 pounds of uranium oxide. I am glad to

FOR INFO ONLY
FOR REPLY FROM 22300
HOTEL KING'S CROWN

say that we could borrow this amount, otherwise I might have approached you for financial assistance.

So far publication of the papers, which were sent to the Physical Review on March 16th, is being delayed at our request, and efforts are made to get similar action in England and France. In the mean time a paper by Joliot appeared in Nature, which relates to our subject, but so far it did not attract much attention. Now we are trying to get Joliot to co-operate, but I do not know whether we will succeed.

Some time ago Loomis asked Fermi out to Tuxedo Park, and I understand from Richards that Loomis talked to you over the telephone after Fermi's visit. Since then Loomis one inquired over the telephone, asking Fermi about the present state of the experiments.

I hope to see you some time when you are not very busy, and in any case I shall let you know of the further developments.

With best wishes,

yours sincerely,

(Leo Szilard)

S. B. S. 64 folder 2

Paul Porter

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

April 11th, 1939

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25 East 76th Street
New York City

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NEW YORK CITY
APR 11 1939
HOTEL KING'S CROWN

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With best wishes,

yours sincerely,

(Leo Szilard)

These were papers previously sent to A.S.S. and were published.