

c/o Warendon Laboratory,
Parks Road,
Oxford.

2nd January, 1936.

Dear Professor Fermi,

I was very much cheered up by reading your manuscript. I had to struggle a little both with the Italian language and the wicked nature of the neutrons, but at last I made friends with both. It is astonishing that you could make such a large number of observations. I have lately been limiting my own activities to studying the absorption of various components of slow neutrons in boron and lithium. One can tentatively assume that the absorption in these two elements is proportionate to \sqrt{E} though this law obviously breaks down for other elements. In order to get some check on this assumption, I always determine, if I can, the absorption both in boron and lithium and so far the ratio seems to be always the same. That is somewhat reassuring. Assuming that boron follows the theory and that the bulk of the observed unfiltered neutrons in my arrangement has thermal energies, I have to conclude from the observed absorption in boron that the residual radiation of cadmium, which is strongly absorbable in indium, corresponds to energies of a few volts.

I was very glad to have your manuscript, both on account of the interesting results and because it enabled me to avoid unnecessary repetition.

With kind regards,

Yours sincerely,

Leo Szilard.

c/o The Clarendon Laboratory,
Parks Road,
Oxford.

13th March 1936.

Dear Professor Fermi,

I have been intending to write to you for some time in order to thank you for the last manuscript which you sent me, and also in order to tell you about ideas which have been put forward about selective absorption in the course of the last two months. They came simultaneously from Bohr, who was here for a visit, and from Wigner and Breit, with whom I have been in Correspondence. You certainly saw Bohr's paper in "Nature" and I am going to send you a manuscript from Breit and Wigner as soon as I get it back from Cambridge.

I have to write to you now about the question of practical applications of modern nuclear physics, though I am by no means certain that such practical applications of importance at present really exist. Since however I received a letter from Mr. Giannini (a copy of which I enclose), I wish to tell you why I have applied for certain patents in this field and what I propose to do with them.

I feel that I must not consider these patents as my private property and that if they are of any importance, they should be controlled with a view of public policy. I see no objection to a commercial exploitation of some such patents, but I believe that the income (if there is any substantial income) should not be used for private purposes, but rather for financing further research or, if the income is very large, for other constructive purposes.

I know of one precedent for such procedure which was fairly successful.. Some years ago, Cotrell, whom you perhaps know, took out in the U.S.A. certain patents and formed a research corporation to which he handed over his patents for commercial exploitation of the products. The profits are being used entirely for the promotion of further research.

In 1928 I formed the mistaken view that artificial

disintegration would be developed in the course of a few years and would soon lead to practical application of very great importance. At that time I filed three patents which described the methods for the production of fast protons which later on were developed and published by Lawrence, one of them being the cyclotron, and also described the production of radio-active elements by bombardment of fast protons and alpha-particles. All these patents have subsequently been abandoned.

In 1933 I again formed the view that practical applications of very great importance are impending. Whether this view is correct, I could not say. It seemed to me that the production of radio-active elements by neutrons might have some importance and I filed a patent on this subject, after Joliot's discovery, on March 12th, 1934. A number of other applications followed, but it remains to be seen whether any of them have real practical significance.

When I filed all these patents, my intention was to hand them over, as soon as they turn out to be important, to a research corporation which could be set up at any moment in one form or another and which could use its funds for promoting further research. I personally do not think very much of producing radio-active elements for medical purposes and I should not like to be responsible for inducing manufacturers to embark upon such an enterprise at present. On the other hand, it is conceivable that certain applications of very great importance might materialise in a not too distant future and I have lately been taking, rightly or wrongly, a rather optimistic view on this subject. I have been writing a memorandum on the subject, which I might send you in due course of time and am sufficiently optimistic to feel justified in proposing that a fund should be created of about £5,000 and used for further experiments. It seems to me that such a fund should be used up in the course of the next three years and used in three different ways. First of all, it should be used for salaries of young physicists who could carry on systematic investigations which would fit in well with our present work, but on which we cannot embark for lack of time. Secondly, it should be used for hiring radium to be used for some such experiments on the basis of a very favourable offer of radium which I have received in this connection. Thirdly, such funds should be used for enabling

any of us to move from one laboratory to another whenever such a movement is justified from the point of view of apparatus which is present in one laboratory and lacking in another.

Though I do not know whether my efforts to raise such a fund will be successful, I should be glad if you could let me know whether you would care to share the responsibility for controlling such a fund.

I understand from Giannini's letter that you have applied for certain patents. You might perhaps have similar ideas about the commercial exploitation of your patents. Should you, however, no longer control these patents, or should you have other intentions with them, that would in no way affect the present issue. One should not give at present too much significance to any single patent in this field. If important applications should materialise in the future, some importance might, however, be attached to the co-operation of those who work in this field and also to their willingness to take responsibility in this matter.

Forgive me please for writing to you such a long and somewhat boring letter. I should appreciate any comment which you care to make on this subject; Giannini will leave England on March 19th, and if I hear from you before this date, I could discuss the matter with him before he leaves.

With best wishes,

Yours sincerely,

c/o The Clarendon Laboratory,
Parks Road,
Oxford.

13th March, 1936.

Dear Professor Fermi,

I have been intending to write to you for some time in order to thank you for the last manuscript which you sent me, and also in order to tell you about ideas which have been put forward about selective absorption in the course of the last two months. They came simultaneously from Bohr, who was here for a visit, and from Wigner and Breit, with whom I have been in correspondence. You certainly saw Bohr's paper in "Nature" and I am going to send you a manuscript from Breit and Wigner as soon as I get it back from Cambridge.

I have to write to you now about the question of practical applications of modern nuclear physics, though I am by no means certain that such practical applications of importance at present really exist. Since however I received a letter from Mr. Giannini (a copy of which I enclose), I wish to tell you why I have applied for certain patents in this field and what I propose to do with them.

I feel that I must not consider these patents as my private property and that if they are of any importance, they should be controlled with a view of public policy. I see no objection to a commercial exploitation of some such patents, but I believe that the income (if there is any substantial income) should not be used for private purposes, but rather for financing further research or, if the income is very large, for other constructive purposes.

I know of one precedent for such procedure which was fairly successful. Some years ago Cotrell, whom you

perhaps know, took out in the U.S.A. certain patents and formed a research corporation to which he handed over his patents for commercial exploitation of the products. The profits are being used entirely for the promotion of further research.

In 1928 I formed the mistaken view that artificial disintegration would be developed in the course of a few years and would soon lead to practical application of very great importance. At that time I filed three patents which described the methods for the production of fast protons which later on were developed and published by Lawrence, one of them being the cyclotron, and also described the production of radio-active elements by bombardment of fast protons and alpha-particles. All these patents have subsequently been abandoned.

In 1933 I again formed the view that practical applications of very great importance are impending. Whether this view is correct, I could not say. It seemed to me that the production of radio-active elements by neutrons might have some importance and I filed a patent on this subject, after Joliot's discovery, on March 12th, 1934. A number of other applications followed, but it remains to be seen whether any of them have real practical significance.

When I filed all these patents, my intention was, as soon as they turned out to be important, to hand them over to a research corporation which could be set up at any moment in one form or another and which could use its funds for promoting further research. I personally do not think very much of producing radio-active elements for medical purposes and I should not like to be responsible for inducing manufacturers to embark upon ~~research~~ ^{such an} enterprise at present. On the other hand, it is conceivable that certain applications of very great importance might materialise in a not too distant future and I have lately been taking, rightly or wrongly,

a rather optimistic view on this subject. I have been writing a memorandum on the subject, which I might send you in due course of time and am sufficiently optimistic to feel justified in proposing that a fund should be created of about £5,000 and used for further experiments. It seems to me that such a fund should be used up in the course of the next three years and used in three different ways. First of all, it should be used for salaries of young physicists who could carry on systematic investigations which would fit well ⁱⁿ ~~in~~ ^{with} our present work, but on which we cannot embark for lack of time. Secondly, it should be used for hiring radium to be used for some such experiments on the basis of a very favourable offer of radium which I have received in this connection. Thirdly, such funds should be used for enabling any of us to move from one laboratory to another whenever such a movement is justified from the point of view of apparatus which is present in one laboratory and lacking in another.

Though I do not know whether ~~my~~ efforts to raise such a fund will be successful, I should be glad if you could let me know whether you would care to share the responsibility for controlling such a fund.

I understand from Giannini's letter that you have applied for certain patents. You might perhaps have similar ideas about the commercial exploitation of your patents. Should you, however, no longer control these patents, or should you have other intentions with them, that would in no way affect the present issue. ^{One} I should not give at present too much significance to any single patent in this field. If important applications should materialize in the future, some importance might, however, be attached to the co-operation of those who work in this field, and also to their willingness to take responsibility in this matter.

Forgive me please for writing to you such a long and somewhat boring letter. I should appreciate any comment which you care to make on this subject; Giannini will leave England on March the 19th, and if I hear from you before this date, I could discuss the matter with him before he leaves.

With best wishes,

Yours sincerely,

1-9 (1)

UNIVERSITY OF MICHIGAN
ANN ARBOR

DEPARTMENT OF PHYSICS

July 1 1939

Dear Szilard:

Thank you for sending me the copies of the last edition of the letter. I have no changes to suggest and it seems to me that everything is clearly explained.

So far I had no opportunity of performing any experiment because the cyclotron is at present out of order. But as soon as it starts working again I want to repeat first of all the last experiments that we did on the absorption of the resonance neutrons because the results that we got seem to me rather crazy.

I am thinking of several possibilities for reducing the absorption at resonance during the slowing down process, and I shall let you know if I reach any conclusion.

Yours

Enrico Fermi
Enrico Fermi

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 ~~the~~ 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)

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

July 5th, 1939

Dear Fermi:

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

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

with

$$D = \frac{W \lambda_{sc}}{3} ; A = \frac{W}{\frac{\sigma_c}{\sigma_s} N_s}$$

it is:

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

where

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

For small ar we have

$$\rho(r) = 2aC \left(1 + \frac{a^2 r^2}{6}\right)$$

and the ratio of the densities on the surface and in

the center is given by $\frac{\rho(r)}{\rho(0)} = \frac{e^{ar} - e^{-ar}}{2ar} \approx 1 + \frac{a^2 r^2}{6}$

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

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

As you see, the method is beginning to get somewhat awkward in the case of carbon for smaller capture cross-sections than 0.005. It seems that it will be possible to get sufficiently pure carbon at a reasonable price. Carbon would also have an advantage over

hydrogen insofar as there is no change in the scattering cross-section in the transition from the resonance region to the thermal region. Consequently, if layers of uranium oxide of finite thickness are used, the diffusion of the thermal neutrons produced in the carbon to the uranium layer is not adversely affected as in the case of hydrogen by such a change. Whether this point is of any importance depends of course on the absolute value of the carbon cross-section. Pending reliable information about carbon we ought perhaps to consider heavy water as the "favorite", and I shall let you know as soon as I can how many tons could be obtained within reasonable time.

With kind regards to all,

Yours,

(Leo Szilard)

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

July 5th, 1939

Dear Fermi:

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

The thermal neutron density within the sphere obeys the equation

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

with

$$D = \frac{W k_{eff}}{3} \quad ; \quad A = \frac{W}{\sigma_{sc} N_{sc}}$$

it is:

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

where

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

For small ar we have

$$\rho(r) = 2aC \left(1 + \frac{a^2 r^2}{6}\right)$$

and the ratio of the densities on the surface and in the center

is given by

$$\frac{\rho(r)}{\rho(0)} = \frac{e^{ar} - e^{-ar}}{2ar} \approx 1 + \frac{a^2 r^2}{6}$$

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

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

As you see, the method is beginning to get somewhat awkward in the case of carbon for smaller capture cross-sections than 0.005. It seems that it will be possible to get sufficiently pure carbon at a reasonable price. Carbon would also have an advantage over hydrogen insofar as there is no change in the scattering cross-



section in the transition from the resonance region to the thermal region. Consequently, if layers of uranium oxide of finite thickness are used, the diffusion of the thermal neutrons produced in the carbon to the uranium layer is not adversely affected as in the case of hydrogen by such a change. Whether this point is of any importance depends of course on the absolute value of the carbon cross-section. Pending reliable information about carbon we ought perhaps to consider heavy water as the "favorite", and I shall let you know as soon as I can how many tons could be obtained within reasonable time.

With kind regards to all,

Yours,

(Leo Szilard)



COPY

LETTERS OF LEO SZILARD TO E. FERMI

July 3 and July 8, 1939

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

July 3rd, 1939

Dear Fermi:

This is to keep you informed of the trend of my ideas concerning chain reactions. It seems to me now that there is a good chance that carbon might be an excellent element to use in place of hydrogen, and there is a strong temptation to gamble on this chance. The capture cross section of carbon is not known: the only experimental evidence available asserts an upper limit of 0.01 times 10^{-24} cm². If the cross section were 0.01 carbon would be no better than hydrogen, but the cross section is perhaps much smaller, and it might be for instance 0.001. If it were so carbon not only could be used in place of hydrogen, but would have great advantages, even if a chain reaction were possible with hydrogen also. The concentration of uranium oxide in carbon could be kept very low, so that one could have about 2 gm. of carbon per cc. This compares favorably with 1/2 gm. of water per cc. at the most and means that the mean square of the displacement of a neutron for slowing down to thermal velocities would be only 1.5 times as large in the carbon-uranium-oxide mixture than in the water-uranium-oxide mixture. If capture by carbon can be neglected, the concentration of uranium oxide is determined by the consideration that the average displacement of a thermal neutron for capture by uranium in the mixture must not become too large. With this as a limiting factor about 1/10 of the weight of the mixture would have to be uranium, and that means that one would need only a few tons of uranium oxide if our present data about uranium are correct.

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

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

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

Yours,

(Leo Szilard)

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

July 8th, 1939

Dear Fermi:

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

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

I thought that perhaps 50 tons of carbon and 5 tons of uranium should be used as a start. The value of the carbon would only be about \$ 10,000. Since the carbon and the uranium oxide would not be mixed but built up in layers, or in any case used in some canned form, there will be no waste of material or waste of labor involved in unmixing after the experiment is over. Since the uranium layers may be separated by carbon layers of 20 to 30 cm. thickness, or even more, we have to deal with a comparatively simple structure. Much simpler than would be the case for alternating water and uranium layers.

I told Professor Pegram yesterday how I felt about the situation, and he

-2-

seemed to be not unwilling to take the necessary action. I wonder whether you think it wise to proceed as outlined in this letter.

With kindest regards,

Yours,

(Leo Szilard)

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

July 8th, 1939

Dear Fermi:

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

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

I thought that perhaps 50 tons of carbon and 5 tons of uranium should be used as a start. The value of the carbon would only be about \$ 10,000. Since the carbon and the uranium oxide would not be mixed but built up in layers, or in any case used in some canned form, there will be no waste of material or waste of labor involved in unmixing after the experiment is over. Since the uranium layers may be separated by carbon layers of 20 to 30 cm. thickness, or even more, we have to deal with a comparatively simple structure. Much simpler than would be the case for alternating water and uranium layers.

I told Professor Pegram yesterday how I felt about the situation, and he

-2-

seemed to be not unwilling to take the necessary action. I wonder whether you think it wise to proceed as outlined in this letter.

With kindest regards,

Yours,

(Leo Szilard)

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

July 8th, 1939

Dear Fermi:

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

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

I thought that perhaps 50 tons of carbon and ~~500~~⁵ tons of uranium should be used as a start. The value of the carbon would only be about \$ 10,000.- Since the carbon and the uranium oxide

would not be mixed but built up in layers, or in any case used in some canned form, there will be no waste of material or waste of labor involved in unmixing after the experiment is over. Since the uranium layers may be separated by carbon layers of 20 to 30 cm thickness, or even more, we have to deal with a comparatively simple structure. Much simpler than would be the case for alternating water and uranium layers.

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

With kindest regards,

yours,

(Leo Szilard)

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 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

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

Yours

Enrico Fermi

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

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
Rochester and Carbon Chem Co~~

Warne Dr. P.O.

Eastman Kodak Corp
Rochester and Carbon Chem Co

COPY

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

July 11th, 1939

Dear Fermi:

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

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

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

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

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

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

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

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

will then be obeyed by $\rho = \rho_1 - \rho_2$, and ρ will satisfy the boundary condition

$$\rho(r) = b \quad \text{Therefore we have}$$
$$\frac{\rho(r)}{\rho(0)} = \frac{\rho_1(r) - \rho_2(r)}{\rho_1(0) - \rho_2(0)} = \frac{e^{ar} - e^{-ar}}{2ar}$$

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

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

Yours,

(Leo Szilard)

University of Michigan
Ann Arbor

Department of Physics

July 11 1939

Professor George B. Pegram

Pupin Physics Laboratories
Columbia University

New York, N.Y.

Dear Professor Pegram:

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

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

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

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

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

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

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

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

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

Sincerely yours
signed: Enrico Fermi m.p.
(Enrico Fermi

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

July 11th, 1939

Dear Fermi:

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

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

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

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

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

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

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

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

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

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

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

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

Yours,
Y. L.

(Leo Szilard)

University of Michigan
Ann Arbor

Department of Physics

July 18 1939

Dear Anderson:

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

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

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

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

Yours
signed: Enrico Fermi m.p.

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

W. J. ...
COLUMBIA UNIVERSITY
NEW YORK

April 7, 1940

Attention of:

Professor G.B. Pegram
Professor E. Fermi

Memo.

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

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

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

L. Brillouin

not sent

420 West 116th Street
New York City
June 19, 1940

Dear Fermi:

I saw Professor Pegram yesterday and discussed with him the situation. He had a letter from Admiral Bowen which he wanted to answer right away.

I told Professor Pegram that in my personal opinion the semi large scale experiment for which you have suggested using 5 tons of uranium metal ought to have the right of way before everything else and that we should not hesitate to place an order for this amount of metal; ^{and} perhaps as much as 50 tons of graphite. I have no doubt that this material will be needed in any case and will have to be ordered sooner or later. Clearly, it will be impossible for us to say with certainty even if we succeed in measuring all nuclear constants involved rather accurately within a year that a chain reaction with slow neutrons can not be made to work. Consequently, if we defer ordering this material we would only lose time but not save any money.

Since it is conceivable that 10 tons of uranium metal and perhaps 100 tons of graphite might be sufficient to make a chain reaction work, the ordering of such amounts should also be taken under consideration. Finally, 200 tons of graphite and perhaps 25 tons of uranium metal would give us all the scope for a large scale experiment which we might desire to have.

Assuming that an order will be placed for perhaps 5 tons of uranium metal and 50 tons of graphite we would consider the performance of

a semi large scale experiment (which can not be expected to give a divergent chain reaction) as our most important task; but while waiting for the arrival of this material and in our spare time we would gradually organize the measurement of all nuclear constants involved. While it is impossible to say how long such a survey of the nuclear constants would take it is possible to estimate the cost as amounting to about \$50,000. The men who would carry out this survey would be also available for the performance of the semi large scale experiment and the preparation of a large scale experiment so that these experiments would not require additional salaries and the expenditure involved would be mainly the cost of material and perhaps some expenditure for manual labor and apparatus. As to the survey of the nuclear constants for which an expenditure of \$50,000. has been envisaged, it seems to me that such a survey has to be carried out whether the semi large scale experiment shows a favorable result or not. Clearly, if the semi large scale experiment has a negative result we must know the value of the nuclear constants in order to be able to determine the optimum conditions for a chain reaction and the knowledge of these optimum conditions is even more important and an urgent necessity if the semi large scale experiment has a positive result.

Enclosed you will find an estimate for the survey of the nuclear constants; the experiments which will actually be carried out may be different from those which we are at present envisaging since in the meantime we might be able, perhaps, to think of improved methods, but I do not believe that such changes will greatly affect the total expenditure.

While discussing with Professor Pegram, it became evident that it will be necessary to define my own status with respect to the work

on the uranium chain reaction in such a way that all those who are immediately concerned with this work should have the same conception of it. Otherwise, difficulties might arise later. I told Professor Pegram that I explained my conception of this status to you early in March of this year and again a few days ago just before you left for Chicago and that I had the impression that you accepted this conception. However, our conversation referred to the work on making a chain reaction work in general and not any details of the work to be carried out within the Physics Department in particular, concerning which you would, of course, not want to make any commitments. Moreover, I realize that I may have misinterpreted your attitude and that it is preferable that this point should be rediscussed both with Pegram and Urey.

In the conversation which I had with Professor Pegram I defined my conception in general terms saying that you and I would, according to this conception, jointly be responsible for the task of taking all necessary steps to make a chain reaction with slow neutrons work if it can be made to work at all. This means that all experiments would be carried out under joint direction with such division of labor as appears expedient to us. In practice, this may lead to some overlapping of work, insofar as you may prefer one method for measuring some important nuclear value and I might prefer another method. I would not consider such overlapping a disadvantage, in particular since, if we are unable to agree on a single method to use, then, in all probability, none of the proposed methods are entire satisfactory and accordingly, a cross checking is desirable.

In practice, this would mean that, while we may carry out jointly certain experiments there will be other experiments for which you, and again other experiments for which I will have to bear the responsibility.

Correspondingly, probably some of those who collaborate with us will primarily work with you and others primarily with me, but I do not see any reason for any rigid coordination of collaborators to either of us. The question arose in the conversation with Professor Pegram what to do if we two should be unable to agree on some such thing as the method of carrying out a large scale experiment; i.e. an experiment which is so expensive that we can not afford to have an overlapping. In my opinion, in such a case, we would have to appeal to a small group of scientists composed of men who, in the opinion of both of us, are capable of balanced judgment and we would have to abide by the verdict to which they arrived, after having carefully studied the issue. I hardly think that such a disagreement between us is likely to arise but the question raised by Pegram may serve as an example to illustrate a certain spirit which I would be glad to see uniformly recognised by all those concerned. It seems to me that one may say the following in order to enter into the merits of the case. It is probably a fair statement to make that if we had separated in April last year and worked independently of each other on this problem both of us would have been quite capable, if given the necessary facilities, of making a chain reaction work by now, provided it can be made to work using an element like carbon. For us to work jointly in this matter has both its advantages and disadvantages and we may at this juncture leave the question open whether the advantages outweigh the disadvantages from the point of view of obtaining speedy results. We may leave this question open because I feel that we are not as free to decide this issue as we have been, in April of last year or even in March of this year. I should certainly feel at a loss to know what to do if it proved impossible now

to find a satisfactory arrangement. However, I feel that if I allowed myself to be influenced by this fact into accepting an arrangement which I would inwardly, rightly or wrongly, not consider as fair and just in the circumstances, this would put a strain on our collaboration. I think it would be useful if you defined your attitude in this matter in a letter addressed to Professor Pegram; and if you would be kind enough to send me a copy I would show it to Urey and perhaps to others who have a legitimate interest.

A program of work of the scope which is at present envisaged would, (if all the work is carried out in the Physics Department at Columbia) no doubt, strain the department to some extent and represent a not negligible encroachment upon the available space and other facilities of the department. The strain is perhaps somewhat lessened by the fact that the number of our collaborators would increase only very gradually since both you and I realize that it will be a slow process to find the right collaborators and neither of us has the desire to rush into a large number of new experiments simultaneously. Nevertheless, if it becomes certain that in addition to these limitations there are other limitations within the Physics Department which make it impossible to carry out the proposed survey within a year, then it seems to me it is our duty to see if some of the experiments can not be started in some other laboratory either under the supervision of one of us or under the supervision of somebody else whose judgment can be trusted. This is a point which I think ought to be carefully considered upon your return in connection with a list of experiments which may be set forth in detail.

Yours sincerely,

(Leo Szilard)

420 West 116th Street
New York City
June 19, 1940

Dear Fermi:

I saw Professor Pegram yesterday and discussed with him the situation. He had a **letter** from Admiral Bowen which he wanted to answer right away.

I told Professor Pegram that in my personal opinion the semi large scale experiment for which you have suggested using 5 tons of uranium metal ought to have the right of way before everything else and that we should not hesitate to place an order for this amount of metal ^{and,} perhaps as much as 50 tons of graphite. I have no doubt that this material will be needed in any case and will have to be ordered sooner or later. Clearly, it will be impossible for us to say with certainty even if we succeed in measuring all nuclear constants involved rather accurately within a year that a chain reaction with slow neutrons can not be made to work. Consequently, if we defer ordering this material we would only lose time but not save any money.

Since it is conceivable that 10 tons of uranium metal and perhaps 100 tons of graphite might be sufficient to make a chain reaction work, the ordering of such amounts should also be taken under consideration. Finally, 200 tons of graphite and perhaps 25 tons of uranium metal would give us all the scope for a large scale experiment which we might desire to have.

Assuming that an order will be placed for perhaps 5 tons of uranium metal and 50 tons of graphite we would consider the performance of

a semi large scale experiment(which can not be expected to give a divergent chain reaction)as our most important task; but while waiting for the arrival of this material and in our spare time we would gradually organize the measurement of all nuclear constants involved. While it is impossible to say how long such a survey of the nuclear constants would take it is possible to estimate the cost as amounting to about \$50,000. The men who would carry out this survey would be also available for the performance of the semi large scale experiment and the preparation of a large scale experiment so that these experiments would not require additional salaries and the expenditure involved would be mainly the cost of material and perhaps some expenditure for manual labor and apparatus. As to the survey of the nuclear constants for which an expenditure of \$50,000. has been envisaged, it seems to me that such a survey has to be carried out whether the semi large scale experiment shows a favorable result or not. Clearly, if the semi large scale experiment has a negative result we must know the value of the nuclear constants in order to be able to determine the optimum conditions for a chain reactions and the knowledge of these optimum conditions is even more important and an urgent necessity if the semi large scale experiment has a positive result.

Enclosed you will find an estimate for the survey of the nuclear constants; the experiments which will actually be carried out may be different from those which we are at present envisaging since in the meantime we might be able, perhaps, to think of improved methods, but I do not believe that such changes will greatly affect the total expenditure.

(all this)
While discussing with Professor Pegram, it became evident that it will be necessary to define my own status with respect to the work

on the uranium chain reaction in such a way that all those who are immediately concerned with this work should have the same conception of it. Otherwise, difficulties might arise later. I told Professor Pegram that I explained my conception of this status to you early in March of this year and again a few days ago just before you left for Chicago and that I had the impression that you accepted this conception. However, our conversation referred to the work on making a chain reaction work in general and not any details of the work to be carried out within the Physics Department in particular, concerning which you would, of course, not want to make any commitments. Moreover, I realize that I may have misinterpreted your attitude and that it is preferable that this point should be rediscussed both with Pegram and Urey.

In the conversation which I had with Professor Pegram I defined my conception in general terms saying that you and I would, according to this conception, jointly be responsible for the task of taking all necessary steps to make a chain reaction with slow neutrons work if it can be made to work at all. This means that all experiments would be carried out under joint direction with such division of labor as appears expedient to us. ~~In practice,~~ This may lead to some overlapping of work, insofar as you may prefer one method for measuring some important nuclear value and I might prefer another method. I would not consider such overlapping a disadvantage, in particular since, if we are unable to agree on a single method to use, then, in all probability, none of the proposed methods are entire satisfactory and accordingly, a cross checking is desirable.

In practice, this would mean that, while we may carry out jointly certain experiments there will be other experiments for which you, and again other experiments for which I will have to bear the responsibility.

Correspondingly, probably some of those who collaborate with us ^{may} will primarily work with you and others primarily with me, [but I do not see any reason for any rigid coordination of collaborators to either of us.] The question arose in the conversation with Professor Pegram what to do if we two should be unable to agree on some such thing as the method of carrying out a large scale experiment; i.e. an experiment which is so expensive that we can not afford to have an overlapping. In my opinion, in such a case, we would have to appeal to a small group of scientists composed of men who, in the opinion of both of us, are capable of balanced judgment and we would have to abide by the verdict to which they arrived, after having carefully studied the issue. I hardly think that such a disagreement between us is likely to arise but the question raised by Pegram may serve as an example to illustrate a certain spirit which I would be glad to see uniformly recognised by all those concerned. // It seems to me that one may say the following in order to enter into the merits of the case. It is probably a fair statement to make that if we had separated in April last year and worked independently of each other on this problem both of us would have been quite capable, if given the necessary facilities, of making a chain reaction work by now, provided it can be made to work using an element like carbon. For us to work jointly in this matter has both its advantages and disadvantages and we may at this juncture leave the question open whether the advantages outweigh the disadvantages from the point of view of obtaining speedy results. We may leave this question open because I feel that we are not as free to decide this issue as we have been, in April of last year or even in March of this year. I should certainly feel at a loss to know what to do if it proved impossible now

to find a satisfactory arrangement. However, I feel that if I allowed myself to be influenced by this fact into accepting an arrangement which I would inwardly, rightly or **wrongly**, not consider as fair and just in the circumstances, this would put a strain on our collaboration. I think it would be useful if you defined your attitude in this matter in a letter addressed to Professor Pegram; and if you would be kind enough to send me a copy I would show it to Urey and perhaps to others who have a legitimate interest.

A program of work of the scope which is at present envisaged would, (if all the work is carried out in the Physics Department at Columbia) no doubt, strain the department to some extent and represent a not negligible encroachment upon the available space and other facilities of the department. The strain is perhaps somewhat lessened by the fact that the number of our collaborators would increase only very **gradually** since both you and I realize that it will be a slow process to find the right collaborators and neither of us has the desire to rush into a large number of **new** experiments simultaneously. Nevertheless, if it becomes certain that in addition to these limitations there are other limitations within the Physics Department which make it impossible to carry out the proposed survey within a year, then it seems to me it is our duty to see if some of the experiments can not be started in some other laboratory either under the supervision of ~~one of us~~ or under the supervision of somebody else whose judgment can be trusted. This is a point which I think ought to be carefully considered upon your return in connection with a list of experiments which may be set forth in detail.

Yours sincerely,

(Leo Szilard)

420 West 116th Street
New York City
July 1, 1940

Dear Fermi:

Since I wrote you the enclosed letter dated June 19th, Professor Pegram has been down to Washington and I understand that the situation is changed in the following respects:

The Naval Research Laboratory will probably contribute towards the expenses of the isotope separations experiments of Urey but not towards the expenses of our program of work. Our program would probably be supported through Bush's committee and a sum of \$140,000. might be appropriated in the near future. This would be sufficient to organize the survey of the nuclear values and also to perform the intermediate experiment provided that we succeed in getting the materials required at a reasonable price. I am quite confident that this can be done if we obtain a free hand in finding the best possible sources for the materials which we require and some Government support in impressing upon these firms the necessity for their friendly collaboration. I must make however one reservation: Dr. Sachs who together with Urey has at long last contacted the Belgian Uranium Company has the impression that we can not count on their friendly cooperation and that they will try to make as much money as they possibly can. Thus the Canadians are our last hope for obtaining uranium oxide for our experiment at a low rate. Professor Pegram is trying to arrange something with them.

There is another change in which I was particularly interested. The Naval Research Laboratory now says that they prefer to give a

Chicago July 9 1940

Dear Trilard:

Thank you for your letter of July 4.

1: Lawson - I am sorry that he cannot stay with us, but I don't think that we can do anything about it.

2: Wigner - I can perfectly well see the reasons of his withdrawing from the collaboration. And I am sure that if and when we shall need his help he will be willing to help.

3: I am waiting to know more about this point. But perhaps the proposed arrangement might work better.

4: The experiments that you suggest will certainly cost a sum of the order of magnitude that you suggest; my impression is that your estimate is somewhat too large (not very much); but

it depends of course to a large extent on what program is carried out. I share your view that we shall have plenty of time for this part of the program, no matter whether we wish it or not.

5: I read your letter to Gunn and I agree with it entirely.

6: I had for a long time the impression that we could not expect much cooperation from the Belgians. I hope that the Canadians will appreciate the situation somewhat better.

7: I think that a frank explanation between Pegram and yourself would be very desirable. This would be helped, however, if you could express in a clear way what you consider as a satisfactory arrangement. This, however, might be difficult. I expect to hear

from Pegram about this matter.

I had a very extensive discussion of all the situation with Teller, who stopped in Chicago on his way to the mountains.

Yours sincerely

E. Fermi

420 West 116th Street
New York City
July 4, 1940

Dear Fermi:

I think I ought to write you about the following events which took place after you left:

1. Lawson, after carefully consideration, decided that he would prefer to go to the University of Pennsylvania next fall as previously arranged. Since I told him that I would try to get a salary of \$3000. per year for him, his decision was not due to a lack of financial inducement.

2. Wigner told me during the last meeting which we jointly attended in Washington that he intends to withdraw from further cooperation with the Government representatives on the subject of uranium. At my request he refrained from saying anything about this during the meeting but I understand that he wrote a letter to this effect a few days later to Urey and Briggs. I do not think that we ought to worry unduly about this since I am sure that if a proper frame-work is eventually created for carrying out work on uranium it will be possible to get Wigner's cooperation. For the present though his resignation is one of several disturbing elements.

3. Bowen has shifted his grounds insofar as he now prefers to give the university a lump sum rather than pay salaries and everything else as much as possible directly to the recipient. Moreover, according to the present plans, Bowen will support isotopic separation rather than the work we contemplated and our project is supposed to receive support through another Government committee which is headed by Bush. I assume that Professor Pegram will write you about this in greater detail so I need not go into this for the present.

4. I compiled an estimate of cost for a complete survey of the nuclear constants involved which you will find enclosed. Though the experiments eventually carried out may be very different from those which I have listed the changes will hardly affect the conclusion that about \$50,000. will have to be spent by the time the survey is carried out. I believe that you share my opinion that such a survey has to be carried out whether or not the intermediate experiment shows a positive result. According to present plans, \$90,000. would be requested for buying materials for the intermediate experiment and I believe our policy should be to give the intermediate experiment the right of way before the general survey of nuclear constants. I feel that this will be a somewhat academic statement since we may have to wait for quite a long time before we get materials for the intermediate experiment.

5. Enclosed you will find a copy of a letter which I sent to Gunn which speaks for itself.

6. Sachs and Urey saw the Belgians. From what Sachs tells me we have probably "missed the bus" as far as Belgian ore is concerned. As far as uranium oxide is concerned he has the impression that the Belgians will treat it merely as a business matter and if they are not handled skillfully they will charge an exaggerated price for the amounts which we need for the large scale experiment. In these circumstances, it appears essential to find out from the Canadians just how much they are able to do for us and Professor Pegram is looking after this end of the matter.

7. During the last fortnight I was considerably worried by doubts concerning the possibility of realizing my conception of our collaboration within the frame-work of the Physics Department. Having discussed with you my conception of our proposed collaboration quite extensively in March and also shortly before you left for Chicago I considered this point settled and it was not my intention to raise in this connection any questions of principle. However, it so happened that the question came up more or less accidentally in a conversation with Professor Pegram and was raised by him rather than by me.

Having explained to Professor Pegram what I had previously explained to you I believe he has now a clear picture of the stand which I propose to take with regard to the question of principle which is involved. At first I thought that we might leave the matter open until your return though this did not appear necessary since there are no questions of detail which will require being discussed. On further consideration I feel that it may be better to ask Professor Pegram to take any decision which may be required in this matter as soon as possible, and no doubt he will want to consult you before doing so. You may therefore expect to hear from him in the course of the next few days in this connection.

With kind regards.

Yours,



(Leo Szilard)

page 2 of a letter to

Fermi, noted "not sent"

*Letter to
not sent*

5. Enclosed you will find a copy of a letter which I sent to Gunn which speaks for itself.

6. Sachs and Urey saw the Belgians. From what Sachs tells me we have probably "missed the bus" as far as Belgian ore is concerned. As far as uranium oxide is concerned he has the impression that the Belgians will treat it merely as a business matter and if they are not handled skillfully they will charge an exaggerated price for the amounts which we need for the large scale experiment. In these circumstances, it appears essential to find out from the Canadians just how much they are able to do for us and Professor Pegram is looking after this end of the matter.

7. During the last fortnight I was considerably worried by doubts concerning the possibility of realizing my conception of our collaboration within the frame-work of the Physics Department. Having discussed with you my conception of our proposed collaboration quite extensively in March and also shortly before you left for Chicago I considered this point settled and it was not my intention to raise in this connection any questions of principle. However, it so happened that the question came up more or less accidentally in a conversation with Professor Pegram and was raised by him rather than by me.

In answer to Professor Pegram's question I defined my conception of our collaboration in general terms saying that you and I would, according to this conception, be jointly responsible for the task of taking all necessary steps to make a chain reaction with slow neutrons work if it can be made to work at all. This means that all experiments would be carried out under joint direction with such division of labor as appears expedient to us. This may lead to some overlapping of work, insofar as you may prefer one method for measuring some important nuclear value and I might prefer another method. I would not consider such overlapping a disadvantage, in particular since, if we are unable to agree on a single method to use, then, in all probability, none of the proposed methods are entirely satisfactory and accordingly, a cross checking is desirable. In practice, this would mean that, while we may carry out jointly certain experiments there will be other experiments for which you, and again other experiments for which I will have to bear the responsibility. Correspondingly, probably some of those who collaborate with us may primarily work with you and others primarily with me. The question arose in the conversation with Professor Pegram what to do if we two should be unable to agree on some such thing as the method of carrying out a large scale experiment; i.e. an experiment which is so expensive that we can not afford to have an overlapping. In my opinion, in such a case, we would have to appeal to a small group of scientists composed of men who, in the opinion of both of us, are capable of balanced judgment, and we would have to abide by the verdict to which they arrived, after having carefully studied the issue. I hardly think that such a disagreement between us is likely to arise but the question raised by Pegram may serve as an example to illustrate a certain spirit which I would be glad to see uniformly recognized by all those concerned.

Having talked things over with Professor Pegram I think that he has now a clear picture of the stand which I propose to take with regard to the question of principle which is involved. At first, I thought I

~~Copy~~

Copy

December 31, 1941

Professor E. Fermi
Department of Physics
University of Chicago
Chicago, Illinois

Dear Fermi:

Wigner just showed me the letter which he wrote you and I would like to make the following comments:

I believe that in the long run concentration in one place is unavoidable, and that we should therefore take a positive attitude towards it and support it provided that a location is proposed which is not unacceptable.

Princeton seems to me to be the second best choice. The best choice would be New York. It seems that New York would be quite acceptable to everybody if one could be sure that full authority would be delegated to Compton. On this point people seem to have doubts, and since I am not sure about it myself, I have the feeling that Princeton is probably the most likely choice if concentration takes place in the near future.

I see the following points in favor of a concentration in the near future in New York or at commuting distance from New York:

1. Compton would be really free to devote his full attention to this work.
2. Equipment could be moved gradually and for a long period to come we would still have the facilities of Columbia available for ~~the particularly scientific~~ experiments.
3. Negotiations with firms could be directly under Compton's competence.
4. There would be no rivalry, no possible row about the large scale experiments and the total amounts of materials would be available for such large scale experiments.

My feeling is that if you are in favor of this or some similar plan it could be put through. The essential point is to make it clear to Compton that most people consider him as the only hope to bring order into the present mess. Compton seems to be too modest to realize that he could carry this matter by the sheer weight of his personality, provided of course that he devotes his full attention to it.

decision

I first heard of yesterday's discussion from Loomis who thought that the matter was practically settled. Later Smyth told me more details and my feeling now is that the issue is still open. Consequently, it may very well be that the discussion will turn on your attitude, i.e. whether or not you are able to persuade Compton that his moving to the East Coast would be greatly appreciated.

If I have a chance to talk to Compton, I may mention to him the particular aspect of manufacturing our materials, but if I don't see him, I hope you will find an opportunity to discuss this point with him.

Sincerely yours,

L. Szilard

L. Szilard

LS:LS

July 16, 1941

Professor Henry D. Smyth
Department of Physics
Princeton University
Princeton, New Jersey

Dear Professor Smyth:

I am sending to you today a sphere of compressed uranium oxide having a density of about 6. The sphere is contained in a copper shell weighing 500 grams. There is a hole in the copper shell and you may use it for boring a channel through the compressed oxide in which to put your probes. We did not bore the hole and prepare the channel completely because we did not know exactly what size probes you wanted to use.

I am enclosing in this letter two of our standards, one of U X and one of Ra D + E + F. The activities of both standards have been determined by alpha particle counts before covering them with cellophane. The number of disintegrations in the U X standard is *1005 dis/min*. The Ra D + E + F standard is conditioned in such a way that only the Ra E beta particles are recorded by the counter and the number of disintegrations per minute is indicated on the back.

I would appreciate it if you could return to me this last standard since it would take a considerable time to prepare a new one.

Sincerely yours,

EF:H

Enrico Fermi

cc: 1 - Dr. Briggs

1 - Dr. Szilard

August 16 1941

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

Dear Dr. Briggs:

Some preliminary measurements on the 8'x8'x8' uranium oxide - graphite cube have now been carried out. Although I am not yet ready to submit a final report on the experiment I should like to advise you of the main indications which appear thus far.

The result is (altogether) less favorable than I had anticipated. The indications are that a lattice such as was employed by us would not give a chain reaction even if the dimensions were made infinite. Indeed even in this case the average number of neutrons reproduced when a neutron is absorbed would be about 10 percent less than 1.

As you know the present experiment was designed in order to obtain information with the least possible delay and no attempt was made to attain directly the best conditions. Consequently we cannot interpret the present result as indicating that a chain reaction with uranium and graphite cannot go. There is still in my opinion a fairly good chance to attain the result by exercising considerable care as to the purity of the materials used and the design of the lattice.

Undoubtedly the present low value of the multiplication factor can be quite considerably improved in several ways. Even without any change in the present set up we know that a gain of about 4 percent can be expected by removing the cans in which the U_3O_8 is at present contained. This factor ~~only~~ represents more than one third of the improvement that is needed to have the multiplication factor attain the critical value 1. Further gains could be obtained by improving the purity of the materials used, especially that of the uranium oxide. We are at present looking into this matter in order to make a precise estimate of its importance. The use of uranium metal and perhaps, in a first stage, of compressed oxide and a better adjustment of the relative amounts of uranium and graphite will certainly further improve the situation.

I hope to have the opportunity to discuss these matters when I shall be in Washington next week.

Sincerely yours


Enrico Fermi

Fermi

Metallurgical Laboratory

September 24, 1942

C. N. Cooper
R. F. Christy

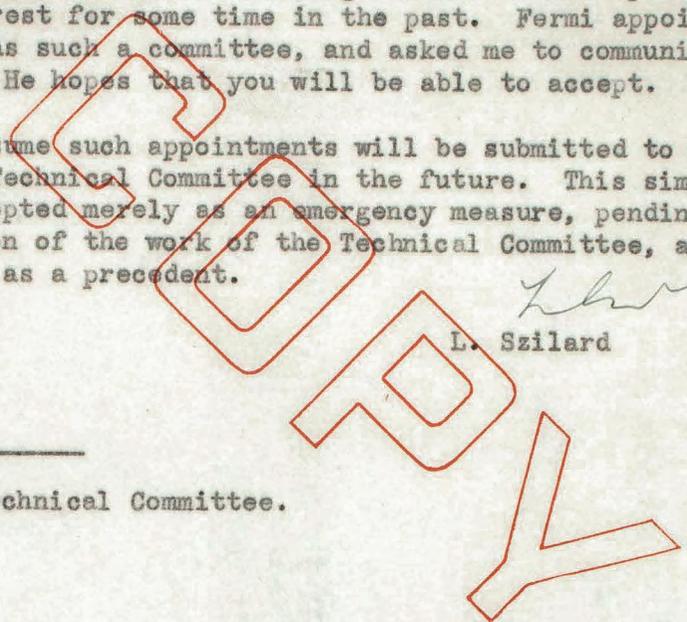
L. Szilard

I have requested Fermi who is chairman of the Technical Committee to appoint a two man committee for the purpose of giving to the Technical Committee a preliminary report on the advantages and disadvantages of bismuth cooling in which I have personally taken an interest for some time in the past. Fermi appointed you two to serve as such a committee, and asked me to communicate this fact to you. He hopes that you will be able to accept.

I assume such appointments will be submitted to the scrutiny of the whole Technical Committee in the future. This simplified procedure was adopted merely as an emergency measure, pending future intensification of the work of the Technical Committee, and must not be considered as a precedent.


L. Szilard

E. Fermi,
Chairman of Technical Committee.



October 16, 1942

THIS DOCUMENT IS TAKEN FROM A FILE OF THE ARGONNE NATIONAL LABORATORY AND WAS TURNED OVER TO DR. LEO SZILARD ON

E. Fermi

L. Szilard

Committee on Helium Cooled Power Plant.

Dear Fermi:

Handwritten signature: J. Johnson

There is a committee under your chairmanship which has the task of looking to Moore's and Leverett's report on the helium cooled power plant. While this committee has not yet reported back to Compton, according to a note from Compton to Allison, dated about October 6, he has authorized the placing of an order for the pressure tank and compressors.

That the pressure tank is too small, as designed, has in the meantime been amply discussed among all those concerned. I have in the past expressed misgivings concerning the use of reciprocating compressors and advocated the use of turbo compressors instead, and had talked about this to Moore, to Compton and to you. The purpose of the present note is to put on record this view. I am not satisfied that the supposedly greater difficulty in obtaining turbo compressors is sufficient to offset the advantages of turbo compressors over reciprocating compressors under the operating conditions of the pile. In the circumstances, placing an order for reciprocating compressors might very well be a mistake.

As I repeatedly pointed out, a parallel flow offers considerable advantages over a series flow, inasmuch as the pressure drop in the pile becomes much lower. Mr. Feld and I have calculated a number of examples, and I put these calculations at the disposal of Mr. Cooper, who has in the meantime checked them.

I do not believe that, from a constructional point of view, the parallel flow has any appreciable disadvantages, particularly if graphite bricks of 8" x 8" can be used. Mr. Hammister is at my request carrying out at present experiments in order to determine whether graphite bricks of this cross section can be deter-

obtained with the required degree of purity. Perhaps somebody might look into the question whether the circulation outside the pile could be redesigned in such a manner as to reduce the pressure loss in the outside part of the circulation also. Only if that is done can we really benefit from shifting over to parallel flow inside the pile.

THIS DOCUMENT IS NOT TO BE TAKEN FROM A FILE OF THE ARGONNE NATIONAL LABORATORY AND WAS TURNED OVER TO DR. LEONARD O. SLOAN

1956
W. S. ...

As I told you before, I find it very difficult to form an opinion concerning the advisability of helium cooling in general, and the chance of success of the Moore, Leverett design, in particular. I could possibly form such an opinion in close collaboration with an engineer, but I have no engineer in my division with whom I could collaborate in such a manner. Undoubtedly it would take very intensive work and quite an effort for me to form such an opinion, since I did not follow the step by step development of the design. I hesitate to make that effort, particularly in view of the fact that I do not know just what weight my opinion would carry, with the "administration" of our project.

In the circumstances I would suggest that you consider the remarks made concerning the compressors and the parallel flow as the only contribution to the work of your committee which I am at present able to make, and that you relieve me from further service on your committee on the helium cooled power plant.

It seems to me that if in the future we want to make sure to have a sound opinion available on future designs for power units, it would be best to have a small committee composed of 3 men; for instance, a nuclear physicist, either you or Wigner, an engineer, either McAdams or Lewis from M. I. T., and a chemical engineer, for instance, Mr. Cooper, who could work in close collaboration with each other, and could look at various stages of the new design as it is being developed. After the design is ready, such a committee should have no difficulty to advise our project whether or not a power unit built along the lines of the design would have a fair chance to succeed, if it were built.

L. Szilard

June 30, 1943

Mr. E. Fermi
University of Chicago
Chicago, Illinois

Dear Fermi:

I saw Captain Lavender in Washington and told him that you and I discussed the possibility that a certain sum be paid annually by the government to some university or research institution. I told him that if no direct arrangement is made with you your part ought to be included in my contract.

Captain Lavender did not seem to like the idea of anything being paid to a university which is not one of the contracted parties, but even more serious however seems to be the following point: If I stipulated that a certain sum has to be paid to a university it might be required that I pay income tax not only for the amount which I receive but for the total amount which I plus the university receives.

I am told that Captain Lavender will be here tomorrow and that he will probably see you in the west and I am writing you this letter so that you shall be aware of this income tax difficulty which we did not foresee. I shall try to avoid any definite commitments so that we can discuss the situation on your return.

With best wishes,

Yours,

L. Szilard

November 16, 1944

Dear Fermi:

Tuesday of this week I received a memo from Col. Metcalf which said "I talked to Dr. Fermi on the phone this morning and he told me he was willing to sign this case with you. He has carefully checked the case and will sign the papers as soon as forwarded to him with your signature. . . ." I thereupon signed the patent application which was submitted to me as a joint inventor.

It seems to me advisable that I should send you copies of the correspondence that I had with Captain Lavender in connection with this case. You will see from these letters that the long delay in this matter arose because I was unable to get either a positive or a negative answer to my request to be permitted to consult a patent attorney. You will also see from the last paragraph of the first page of the letter dated October 30, 1944 that I proposed at that time that the case be submitted to me for signature after your and Wigner's approval had been obtained. I am now told that this letter was misinterpreted.

Sincerely yours,

Leo Szilard

Dr. E. Fermi
P. O. Box 1663
Santa Fe, New Mexico