PALMER PHYSICAL LABORATORY
PRINCETON UNIVERSITY
PRINCETON NEW JERSEY

May 27, 1940

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

Dear Szilard:

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

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

Sincerely yours,

Louis A. Turner

Louis A. Turner

LAT:MH
Enclosure

COPY

Princeton University
Princeton, New Jersey
May 27, 1940

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

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Sincerely yours,

Louis A. Turner

LAT:MH
Enclosure

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

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

Dear Turner:

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

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

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

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

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

In the circumstances I felt that the best course for me to

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

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

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

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

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

your Woodshole address?

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

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

Yours sincerely,

L. Sz.

(Leo Szilard)

PALMER PHYSICAL LABORATORY
PRINCETON UNIVERSITY
PRINCETON NEW JERSEY

June 1, 1940

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

Dear Szilard:

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

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

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

Sincerely yours,

Louis A. Turner

LAT:MH

Louis A. Turner

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

420 West 116th Street
New York City

June 24th, 1940

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

Dear Turner:

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

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

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

With best wishes, yours sincerely,

(Leo Szilard)

C
O
P
Y

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

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

Dear Turner:

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

With best wishes, yours sincerely,

(Leo Szilard)

Sorry that I neglected to enclose this.
J.A.T.

Woods Hole, Mass.,
July 11, 1940.

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

Dear Ernest:-

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

239

Anyhow, the scientific question is, does Eka_0s

94

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

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

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

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

Please give my regards to Mrs. Lawrence.

Sincerely yours,

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

Woods Hole, Mass.,
July 11, 1940

Dear Szilard:-

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

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

With best regards, sincerely yours,

Louis A. Turner.

420 West 116th Street
New York City

July 12, 1940

Dear Turner:

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

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

With best wishes, yours sincerely,

L. S.

Memorandum [illegible] Turner January 5, 1941

It is practically certain that if a chain reaction can be made to work with unseparated uranium trans-uranic elements can be produced from U^{238} which can be chemically separated from uranium and which can then be used to maintain a chain reaction in system composed of such a trans-uranic element and another element which serves to slow down the neutrons to thermal energies. If uranium 238 ~~kept as~~ captures a neutron a trans-uranic element is produced which ~~is~~ has a mass number of 239 and which after going through ~~to better~~ beta- transformations does not show any appreciable spontaneous disintegration. At present it is not known whether this element itself shows fission by thermal neutrons but if it does not then it is practically certain to go over if exposed to slow thermal neutrons into an element having a mass number of 240. Again this element will either show fission with thermal neutrons or if exposed to thermal neutrons go over into an element which has a mass number 241 etc, etc. It follows that if uranium 238 is exposed to slow neutrons for a sufficiently long time trans-uranic elements which show fission with thermal neutrons will accumulate in the uranium.

Figure 10 shows an arrangement for the production of such trans-uranic elements which can be used for maintaining a chain reaction. In figure 10 (1) is a sphere of four-meter diameter which is composed of carbon, natural uranium or carbon, natural uranium and beryllum; (2) is a sphereical layer about 50 cm thick which is either composed of a mixture of natural uranium or sub-normal uranium and graphite containing these two elements in a ratio of about one uranium atom to twenty carbon atoms or it is composed of ~~ordinate~~ alternate layers of graphite and uranium. The uranium may be present in the form of U_3O_8 having about a density of 6 gm per cc and layers of graphite having a density of about 1.7 gms per cc. Somewhat less than 1/3 of the volume may then be occupied by the uranium oxide in somewhat more than 2/3 of the volume by graphite. The graphite layers may be about 1cm to $2\frac{1}{2}$ cm thick. A third layer (3) is a spherical layer of graphite also about 50 cm thick serving the purpose of reducing the number of neutrons which escape from the system.

See also Breit memorandum

The amount of the trans-uranic element which shows fission with thermal neutrons and which is produced in the layer (2) increases gradually and will reach an equilibrium which is determined by equilibrium amount of the element from which it is produced and the ratio of the capture cross section of this element to the fission cross section of the trans-uranic element in which we are interested. Since this fission cross section may be expected to be large compared to the capture cross section of the preceding element the equilibrium concentration of the trans-uranic element in which we are interested will be a small fraction of the concentration of the element from which it is produced. In order to obtain an economical production it is advisable to remove chemically the trans-uranic element which we wish to produce as soon as concentration reaches about 1/4 of the equilibrium concentration.

uranium oxide is used the radius of the sphere or cylinder should be considerably larger in order to have sufficient thermal absorption. A thermal neutron which reaches the surface of the sphere or cylinder and which reaches the uranium body without being absorbed has then a considerable chance of returning to the same uranium body again and be eventually absorbed by it. In this case a reduction of the graphite density would have the effect of decreasing the thermal absorption and the disadvantages would thus over compensate the advantages.

Atomic Energy from U^{238}.

The recent experiments of Nier, Booth, Dunning and Grosse[1] and of Kingdon, Pollock, Booth and Dunning[2] have confirmed Bohr's prediction[3] that the nuclear fission produced in U by thermal neutrons is to be attributed practically entirely to the rarer isotope of mass 235. / This is present in an amount only 1/139 as great as that of the 238 isotope so it is natural to conclude that only 1/140 of any quantity of U can be considered as a possible source of atomic energy if slow neutrons are to be used. It seems likely, however, that this is wrong, that the energy of a large part of the U^{238} nuclei can also possibly be made to undergo fission by slow neutrons in an indirect way. This is of obvious importance for practical utilization of the atomic energy of U.

The indirect use of U^{238} can come about through the fission either of the U^{239} nuclei formed by the capture of neutrons, or of the nuclei descending from U^{239}. In a forthcoming paper[4] it is shown that $_{93}$ EkaRe^{239} and $_{94}$ EkaOs^{239}, the probable long-lived descendants from U^{239}, would be expected to undergo fission upon capture of thermal neutrons. A similar argument leads to the conclusion that U^{239} itself would probably also give fission with thermal neutrons. For such fission to be of consequence it is necessary that nearly one U^{239} atom be produced for every U^{235} atom lost. That this condition can be satisfied may be seen as follows. Consider N secondary neutrons. A number Np will suffer resonance capture by U^{238} to give U^{239}, p being the probability for such capture. The remaining N(1 - p) neutrons will become thermal. Of these, N(1 - p)w will produce fission of U^{235} nuclei and N(1 - p)(1 - w) will be captured by U^{238} nuclei, w being the probability that a thermal neutron

will produce fission. It is assumed that no other capture processes are important for the thermal neutrons, that a non-capturing method of slowing the neutrons needed for developing the divergent chain reaction has been found. The total number of U^{239} nuclei formed is thus Np N(1 - p)(1 - w) and the number of U^{235} nuclei lost is N(1 - p)w. The condition that every U^{235} nucleus be replaced by a U^{239} or descendant nucleus becomes Np N(1 - p)(1 - w) N(1 - p)w which reduces to 1(1 - p) 2w. Since w 1 this condition can be met if p 0.5 as it was in the experiments of Halban, Joliot, Kovarski, and Perrin.[5] Presumably it can be made so with other methods of slowing down the neutrons.

It seems likely, however, that of the three nuclei $_{92}U^{239}$, $_{93}$EkaRe^{239}, and $_{94}$Eka0s^{239} only the last might be expected to have a large cross-section for fission. The excess energy in the U^{240} and $_{93}$EkaRe^{240} nuclei after capture of neutrons by U^{239} and $_{93}$EkaRe^{239}, respectively, would be small compared to what it is in the U^{236} nuclei coming from U^{235}. The cross-sections for fission would thus be correspondingly small. In $_{94}$Eka0s^{240}, on the other hand, the excess energy would be even larger than in U^{236} and a large cross-section for fission would be expected. It might thus be necessary to allow U from which the U^{235} had been used up to lie idle for several times the as yet unknown half-life of $_{93}$EkaRe^{239} until a sufficient quantity of $_{94}$Eka0s^{239} had developed in it, after which it could again be used. This could be repeated until all of the U^{238} had been used up.

Louis A. Turner
Palmer Physical Laboratory
Princeton, New Jersey
May 27, 1940

1. Nier, Booth, Dunning, and Grosse, Phys. Rev. 57, 748, 1940.
2. Kingdon, Pollock, Booth and Dunning, Phys. Rev. 57, 749, 1940
3. Bohr, and Wheeler, Phys. Rev. 56, 426, 1940

4. Turner, Phys. Rev. 57, xxx, 1940. (In press, page to be filled in.)
5. Halban, Joliot, Kowarski, and Perrin, J. de phys. et rad. 10, 428, 1939. See also Turner, Phys. Rev. 57, 334, 1940