I came to America [on January 2, 1938] and did nothing but loaf. I didn't look for a position; I just thought I would wait and see. Then came the Munich crisis. I was at that time visiting Goldhaber[14] in Urbana, Illinois. I spent a week listening to the radio giving news about Munich, and when it was all over I wrote a letter to Lindemann, later Lord Cherwell, who was director of the Clarendon Laboratory [at Oxford] where I was employed. The letter said that I was now quite convinced that there would be war, and therefore there would be little point in my returning to England unless they would want to use me for war work. If, as a foreigner, I would not be used for war work, I would not want to return to England but rather stay in America. And so I resigned at Oxford and stayed here.

I was still intrigued with the possibility of a chain reaction, and for that reason I was interested in elements which became radioactive when

13. A discussion of these experiments at the conference is quoted on pages 88 and 89 of *International Conference on Physics, London, 1934, Papers and Discussions in Two Volumes* (Cambridge, 1935), I (Nuclear Physics).

14. Maurice Goldhaber, in 1938 assistant professor of physics, University of Illinois.

they were bombarded by neutrons and where there were more radioactive isotopes than there should have been. In particular, I was interested in indium. I went up to Rochester [New York] and stayed there for two weeks and did some experiments on indium, which finally cleared up this mystery. It turned out that indium is not instable and that the phenomenon observed could be explained without assuming that indium is split by neutrons.

At that point I abandoned the idea of a chain reaction and of looking for elements which could sustain a chain reaction, and I wrote a letter to the British Admiralty suggesting that the patent which has been applied for should be withdrawn because I couldn't make the process work.[15] Before that letter reached them, I learned of the discovery of fission. This was early in January when I visited Mr. [Eugene] Wigner in Princeton. Wigner told me of Hahn's discovery: Hahn found that uranium breaks into two parts when it absorbs the neutron and this is the process which we call fission. When I heard this I saw immediately that these fragments, being heavier than corresponds to their charge, must emit neutrons; and if enough neutrons are emitted in this fission process, then it should be, of course, possible to sustain a chain reaction; all the things which H. G. Wells had predicted appeared suddenly real to me.

At that time it was already clear, not only to me but to many other people, and certainly it was clear to Wigner, that we were at the threshold of another world war. And so it became, it seemed to us, urgent to set up experiments which would show whether, in fact, neutrons are emitted in the fission process of uranium. I thought that if neutrons are in fact emitted in fission, this should be kept secret from the Germans; so I was very eager to contact Joliot and Fermi, the two men who were most likely to think of this possibility. I was still in Princeton and staying at Wigner's apartment (Wigner was in the hospital with jaundice).

I got up in the morning and wanted to go out. It was raining cats and dogs, and I said, "My God, I am going to catch a cold!" because at that time, the first years I was in America, each time I got wet I invariably

15. Szilard's letter to the British Admiralty withdrawing the patent was dated December 21, 1938. On January 26, 1939, he sent a telegram, followed by a letter on February 2nd, cancelling the December letter and reinstating the patent, which later issued as British patent 630,726.

caught a bad cold. However, I had no rubbers with me, so I had no choice, I just had to go out. I got wet and came home with a very high fever, so I was not able to contact Fermi. As I got ready to go back to New York, I opened the drawer to take my things out and saw there were Wigner's rubbers standing. I could have taken Wigner's rubbers and avoided the cold. But as it was I was laid up with fever for about a week or ten days. In the meantime, Fermi had also thought of the possibility of a neutron emission and the possibility of a chain reaction and he went to a private meeting in Washington and talked about these things. Since it was a private meeting, the cat was not entirely out of the bag, but its tail was sticking out. When I recovered I went to see Rabi,[16] and Rabi told me that Fermi had similar ideas and that he had talked about them in Washington. Fermi was not in, so I told Rabi to please talk to Fermi and say that these things ought to be kept secret because it was very likely that neutrons are emitted, that this might lead to a chain reaction, and this might lead to the construction of bombs. So Rabi said he would, and I went back home to bed at the Kings Crown Hotel.

A few days later I got up to see Rabi and asked, "Did you talk to Fermi?" Rabi said, "Yes, I did." I said, "What did Fermi say?" and he said Fermi said, "Nuts!" So I said, "Why did he say, 'Nuts!'?" and Rabi said, "Well, I don't know, but he is in and we can ask him." So we went over to Fermi's office, and Rabi said to Fermi, "Look, Fermi, I told you what Szilard thought and you said, 'Nuts!' and Szilard wants to know why you said, 'Nuts!' " So Fermi said, "Well, there is the *remote* possibility that neutrons may be emitted in the fission of uranium and then of course that a chain reaction can be made." Rabi said, "What do you mean by 'remote possibility'? " and Fermi said, "Well, 10 per cent." And Rabi said, "Ten per cent is not a remote possibility if it means that we may die of it. If I have pneumonia and the doctor tells me that there is a remote possibility that I might die, and that it's 10 per cent, I get excited about it."

From the very beginning the line was drawn; the difference between Fermi's position throughout this and mine was marked on the first day we talked about it. We both wanted to be conservative, but Fermi thought that the conservative thing was to play down the possibility

16. Isidor Isaac Rabi, professor of physics, Columbia University.

that this might happen, and I thought the conservative thing was to assume that it would happen and take all the necessary precautions. I then wrote a letter to Joliot in which I told Joliot that we were discussing here the possibility of neutron emission of uranium in the fission process and the possibility of a chain reaction, and that I personally felt that these things should be discussed privately among the physicists of England, France, and America; and that there should be no publication on this topic if it should turn out that neutrons are, in fact, emitted, and that a chain reaction might be possible. This letter was dated February 2, 1939. I sent a telegram to England to Professor F. A. Lindemann, at Oxford, asking them to send a block of beryllium which I had had made in Europe with the kind of experiments in mind which I now was actually going to perform.

Such a block of beryllium can be used to produce slow neutrons because if you put radium in the middle of it, under the influence of the gamma rays of radium, the beryllium splits and gives off slow neutrons. If uranium, in the process of fission, which can be caused by slow neutrons, emits fast neutrons, these fast neutrons can be distinguished from the neutrons of the source by virtue of their higher energy.

There was at Columbia University some equipment which was very suitable for these experiments. This equipment was built by Dr. Walter Zinn who was doing experiments with it. And all we needed to do was to get a gram of radium, a block of beryllium, expose a piece of uranium to the neutrons which come from beryllium, and then see by means of the ionization chamber which Zinn had built whether fast neutrons are emitted in the process. Such an experiment need not take more than an hour or two to perform, once the equipment has been built and if you have the neutron source. But of course we had no radium.

So I first tried to talk to some of my wealthy friends; but they wanted to know just how sure I was that this would work, so finally I talked to one of my not-so-wealthy friends. He was an inventor and he had some income from royalties.[17] I told him what this was all about, and he said, "How much money do you need?" and I said, "Well, I'd like to borrow $2,000." He took out his checkbook, he wrote out a check, I cashed

17. While this friend's name is mentioned in the tape, he has since informed me that he wishes to remain anonymous. [G.W.S.]

the check, I rented the gram of radium, and in the meantime the beryllium block arrived from England. And with this radium and beryllium I turned up at Columbia and, having talked previously to Zinn, said to the head of the department, "I would like to have permission to do some experiments." I was given permission to do experiments for three months. I don't know what caused this caution, because they knew me quite well; but perhaps the idea was a little too fantastic to be entirely respectable. And once we had the radium and the beryllium it took us just one afternoon to see those neutrons. Mr. Zinn and I performed this experiment.[18]

In the meantime Fermi, who had independently thought of this possibility, had set up an experiment. His did not at first work so well, because he used a neutron source which emitted fast neutrons, but then he borrowed our neutron source and his experiment, which was of completely different design, also showed the neutrons.

And now there came the question: Shall we publish this? There were intensive discussions about this, and so Zinn and I, and Fermi and Anderson, each sent a paper to the *Physical Review*, a "Letter to the Editor."[19] But we requested that publication be delayed for a little while until we could decide whether we wanted to keep this thing secret or whether we would permit them to be published. Throughout this time I kept in close touch with Wigner and with Edward Teller, who was in Washington. At this time I went to Washington. Fermi also went to Washington on some other business, I forget what it was, and Teller and Fermi and I got together to discuss whether or not this thing should be published. Both Teller and I thought that it should not. Fermi thought that it should. But after a long discussion, Fermi took the position that after all this is a democracy; if the majority was against publication he would abide by the wish of the majority, and he said that he would go back to New York and advise the head of the department, Dean Pegram,[20] to ask that publication of these papers be indefinitely delayed.

18. The experiment with Zinn was performed on March 3, 1939.

19. Leo Szilard and Walter H. Zinn, "Instantaneous Emission of Fast Neutrons in the Interaction of Slow Neutrons with Uranium," *Physical Review*, 55 (April 15, 1939), 799–800; H. L. Anderson, E. Fermi, and H. B. Hanstein, "Production of Neutrons in Uranium Bombarded by Neutrons," *Physical Review*, 55 (April 15, 1939), 797–798.

20. George B. Pegram, chairman of the physics department and dean of the Graduate Faculties, Columbia University.

While we were still in Washington, we learned that Joliot and his coworkers had sent a note to *Nature*, reporting the discovery that neutrons are emitted in the fission of uranium, and indicating that this might lead to a chain reaction.[21] At this point Fermi said that in this case we would now publish everything. I was not willing to do that, and I said that even though Joliot had published this, this was just the first step, and that if we persisted in not publishing, Joliot would have to come around; otherwise, he would be at a disadvantage, because we would know his results and he would not know our results. But from that moment on, Fermi was adamant that withholding publication made no sense. I still did not want to yield and so we agreed to put this matter up for a decision by the head of the physics department, Professor Pegram. Pegram hesitated for a while to make this decision, but after a few weeks he finally said that he had decided that we should now publish everything. He later told me why he decided this, and so many decisions were based on the wrong premises: Rabi was concerned about my stand because he said that everybody else was opposed to withholding publication, and I alone in the Columbia group wanted it. This would make my position difficult, in the end impossible, and he thought that I ought to yield on this. According to Pegram, Rabi had visited Urbana and found that Maurice Goldhaber in Urbana knew of our research at Columbia; and from this Rabi concluded that these results were already known as far as Urbana, Illinois, and there was no point in keeping them secret. The fact was that I was in constant communication with Goldhaber; I wrote him of these results, and he was pledged to secrecy. He had talked to Rabi, because of course Rabi was part of the Columbia operation. So on this false premise, the decision was made that we should publish.

In the following months Fermi and I teamed up in order to explore whether a uranium-water system would be capable of sustaining a chain reaction. The experiment was actually done by Anderson, Fermi, and myself. We worked very hard at this experiment and saw that under the conditions of this experiment more neutrons are emitted by uranium than absorbed by uranium. We were therefore inclined to con-

21. H. von Halban, Jr., F. Joliot, and L. Kowarski, "Liberation of Neutrons in the Nuclear Explosion of Uranium," *Nature*, 143 (March 18, 1939), 470–472.

clude that this meant that the water-uranium system would sustain a chain reaction. Whether finally we should have said that in print I do not know. However, the fact is that we believed it until George Placzek dropped in for a visit.[22] Placzek said that our conclusion was wrong because in order to make a chain reaction go, we would have to reduce the absorption of water; that is, we would have to reduce the amount of water in the system, and if we reduced the water in the system we would increase the parasitic absorption of uranium, and he recommended that we abandon the water-uranium system and use helium for slowing down the neutrons. To Fermi this sounded impractical, and therefore funny, and Fermi referred to helium thereafter as Placzek's helium.

I took Placzek more seriously, and while I had, for purely practical reasons, no enthusiasm for helium, I dropped then and there my pursuit of the water-uranium system. Thus, while Fermi went on examining this system in detail and trying to see whether by changing the arrangements he could not improve it to the point where it would sustain a chain reaction, I started to think about the possibility of perhaps using graphite instead of water. This brought us to the end of June. We wrote up our paper,[23] Fermi left for the summer to go to Ann Arbor, and I was left alone in New York. I still had no position at Columbia; my three months [March 1–June 1, 1939] as a guest were up, but there were no experiments going on anyway and all I had to do was to think. Some very simple calculations which I made early in July showed that the graphite uranium system was indeed very promising, and when Wigner came to New York, I showed him what I had done. At this point, both Wigner and I began to worry about what would happen if the Germans got hold of some of the vast quantities of the uranium which the Belgians had in the Congo. So we began to think, through what channels we could approach the Belgian government and warn them against selling any uranium to Germany.

It occurred to me then that Einstein knew the Queen of the Belgians, and I suggested to Wigner that we visit Einstein, tell him about the situation, and ask him whether he might not write to the Queen. We

22. George Placzek, in 1939 a physicist at Cornell University.

23. H. L. Anderson, E. Fermi, and Leo Szilard, "Neutron Production and Absorption in Uranium," *Physica Review*, 56 (August 1, 1939), 284–286.

knew that Einstein was somewhere on Long Island but we didn't know precisely where, so I phoned his Princeton office and I was told he was staying at Dr. Moore's cabin at Peconic, Long Island. Wigner had a car and we drove out to Peconic and tried to find Dr. Moore's cabin. We drove around for about half an hour. We asked a number of people, but no one knew where Dr. Moore's cabin was. We were on the point of giving up and about to return to New York when I saw a boy of about seven or eight years of age standing at the curb. I leaned out of the window and I asked, "Say, do you by any chance know where Professor Einstein lives?" The boy knew and he offered to take us there, though he had never heard of Dr. Moore's cabin.

This was the first Einstein heard about the possibility of a chain reaction. He was very quick to see the implications and perfectly willing to do anything that needed to be done. He was reluctant to write to the Queen of the Belgians, but he thought he would write to one of the cabinet members of the Belgian government whom he knew. He was about to do just that when Wigner said that we should not approach a foreign government without giving the State Department an opportunity to object. So Wigner proposed that Einstein write the letter and send a copy to the State Department with a covering letter. Einstein would say in that covering letter that if we did not hear from the State Department within two weeks, he would send the letter to Belgium.

Having decided on this course, in principle, we returned to New York and Wigner left for California. (This goes to show how "green" we were. We did not know our way around in America, we did not know how to do business, and we certainly did not know how to deal with the government.) I had, however, an uneasy feeling about the approach we had decided upon and I felt that I would need to talk to somebody who knew a little bit better how things are done. I then thought of Gustav Stolper. He used to live in Berlin, where he had published a leading German economic journal and had been a member of the German parliament; now he was living as a refugee in New York. I went to see him and talked the situation over with him. He said that he thought that Dr. Alexander Sachs, who was economic adviser to the Lehman Corporation and who had previously worked for the New Deal, might be able to give us advice on how to approach the American govern-

ment, and whether we should approach the State Department or some other agency of the government. He telephoned Dr. Sachs and I went to see him and I told him my story. Sachs said that if Einstein were to write a letter to President Roosevelt, he would personally deliver it to the President, and that there was no use going to any of the agencies or departments of the government; this issue should go to the White House. This sounded like good advice, and I decided to follow it.

In the meantime, Teller arrived in New York and I asked Teller whether he would drive me out to Peconic. Teller and I went to see Einstein and on this occasion we discussed with Einstein the possibility that he might write a letter to the President. Einstein was perfectly willing to do this. We discussed what should be in this letter and I said I would draft it. Subsequently, I sent Einstein two drafts to choose from, a longer one and a shorter one.

We did not know just how many words we could expect the President to read. How many words does the fission of uranium rate? So I sent Einstein a short version and the longer version; Einstein thought the longer one was better, and that was the version which he signed. The letter was dated August 2, 1939. I handed it to Dr. Sachs for delivery to the White House.[24]

I should perhaps say that this was not the first approach to the government. Soon after we had discovered the neutron emission of uranium, Wigner came to New York and we met—Fermi and I and Wigner —in the office of Dr. Pegram. Wigner said that this was such a serious business that we could not assume the responsibility for handling it, we must contact and inform the government. Wigner said that he would call Charles Edison, who was the new secretary of the navy.[25] He told Edison that Fermi would be in Washington the next day and would be glad to meet with a committee and explain certain matters which might be of interest to the Navy.

So Fermi went there. He was received by a committee. He told in his

24. Accompanying the Einstein letter of August 2nd was a letter of transmittal, Szilard to Sachs, dated August 15, 1939, and a four-page Memorandum for the President by Leo Szilard, also dated August 15th. Both of these documents are reprinted in their entirety below as Appendix I to these Reminiscences.

25. Charles Edison, son of Thomas Alva Edison, assistant secretary of the Navy 1937–1939; secretary of the Navy 1939–1940.

cautious way the story of uranium and what possibilities were involved. But there the matter ended. Nothing came of this first approach. I got an echo of this through Merle Tuve.[26] Ross Gunn, who was an adviser to the Navy and who attended this conference, telephoned Tuve and asked him, "Who is this man Fermi? What kind of a man is he? Is he a Fascist or what? What is he?"

In July, after I took a rather optimistic view of the possibility of setting up a chain reaction in graphite and uranium, I approached Ross Gunn and told him that the situation did not look too bad; that the situation, as a matter of fact, looked so good that we ought to experiment at a faster rate than we had done before; that we had no money for this purpose, and I wondered if the Navy could make any funds available. Afterward I had a letter in reply, in which Ross Gunn explained that there was almost no way in which the Navy could support this type of research, but that if we got any results which might be of interest to the Navy, they would appreciate it if we would keep them informed. This was the second approach to the government.

Einstein's letter was dated August 2nd. August passed and nothing happened. September passed and nothing happened. Finally I got together with Teller and Wigner and we decided we'd give Sachs two more weeks, and if nothing happened we would use some other channel to the White House. However, suddenly Sachs began to bestir himself, and we received a phone call from him in October saying that he had seen the President and transmitted Einstein's letter to him, and that the President had appointed a committee under the chairmanship of Lyman J. Briggs, director of the National Bureau of Standards. Other members of the committee were Colonel Adamson of the Army[27] and Commander Hoover from the Navy.[28] The committee was to meet on October 21st, and Briggs wanted to know who else he should include. I told Sachs that, apart from Wigner and me, I thought that Edward Teller ought to be invited because he lived in Washington and he could act as liaison between us and the committee. This was done. In addition,

26. Merle A. Tuve, physicist at the Carnegie Institution of Washington, Department of Terrestrial Magnetism, which was working closely with the Navy.

27. Colonel K. R. Adamson, Army Ordnance Department.

28. Commander G. C. Hoover, Navy Bureau of Ordnance.

Briggs invited Dr. Tuve. Dr. Tuve had to go to New York and so he suggested that Dr. Roberts[29] sit in for him.

It was our general intention not to ask the government for money, but to ask only for the blessing of the government, so that then, with that blessing, we would go to foundations, raise the funds, and get some coordinated effort going.[30] However, these things never go the way you have planned them.

After I presented the case, and Wigner had spoken, Teller spoke; and Teller spoke in two capacities. In his own name he strongly supported what I had said and what Wigner had said. Then he said, having spoken for himself, he would speak for Dr. Tuve. Dr. Tuve could not attend the meeting, but he had visited New York and had had a discussion with Fermi; it was Dr. Tuve's opinion that at this time it would not be advisable—in fact, it would not be possible—to spend more money on this research than $15,000.

We had not intended to ask for any money from the government at this point, but since the issue of money was injected, the representative of the Army asked, "How much money do you need?" And I said that all we need money for at this time is to buy some graphite; and the amount of graphite which we would have to buy would cost about $2,000. Maybe a few experiments which would follow would raise the sum to $6,000—something in this order of magnitude.

At this point the representative of the Army started a rather long tirade. He told us that it was naïve to believe that we could make a significant contribution to defense by creating a new explosive. He said that if a new weapon was created, it usually took two wars before one knew whether the weapon was any good or not. And then he explained rather laboriously that in the end, it is not weapons which win the wars, but the morale of the troops. He went on in this vein for a long time, until suddenly Wigner, the most polite of us, interrupted him. He said in his high-pitched voice that it was very interesting to hear this. He had always thought that weapons were very important and that this was

29. Richard B. Roberts, Carnegie Institution.

30. Letter and seven-page memorandum, Szilard to Briggs, dated October 26, 1939, but probably prepared earlier, according to the *Smyth Report* (cited in note 41) were "more or less the basis of the discussion at this meeting"; letter, Szilard to Pegram, dated October 21, 1939, reports on the meeting.

what costs money, that this is why the Army needed such a large appropriation. But he was very interested to hear that he was wrong: it's not weapons but morale which wins the wars. If this was correct, perhaps one should take a second look at the budget of the Army, maybe the budget could be cut. Colonel Adamson wheeled around to look at Mr. Wigner and said, "Well, as far as those $2,000 are concerned, you can have it." This is how the first money promise was made by the government.

I should mention that, until the government showed interest (and the first interest it showed was the appointment of this committee) I was undecided whether this development ought to be carried on by industry, or whether it ought to be carried on by the government. And so, just a week or two before the meeting in Washington, I had met with the director of research of the Union Carbon and Carbide Company, W. F. Barrett.[31] The appointment was made by Strauss, and there was some mix-up about it, because they expected Fermi, but it was I who turned up.

There were five people sitting around the table, and I told them that the possibility of a chain reaction between uranium and graphite must be taken seriously; that at this point we could not say very much about this possibility; and that we could talk about it with much greater assurance if we first measured the absorption of neutrons in graphite. It was for this purpose that we would need about two thousand dollars' worth of graphite, and I wondered whether they might give us this amount of graphite on loan; the experiment would not damage the graphite and we could return it to them.

W. F. Barrett said, "You know, I'm a gambling man myself, but you are now asking me to gamble with the stockholders' money, and I'm not sure that I can do that. What would be the practical applications of such chain reaction?" And I said that I really could not say what the practical applications would be at this point, that there was very little doubt in my mind that such a revolution was phenomenal and would find its practical applications ultimately, but it was too early to say that. We

31. The meeting with Barrett's group took place on Monday, October 16, just five days before the Uranium Committee meeting in Washington, according to Szilard's letter to Barrett of October 18, 1939.

had first to see whether we could get it going, and under what conditions it could be set up.

After I left the meeting I had an uneasy feeling that I did not convince anybody there. After all, I was a foreigner and my name was not so well known. I was not well known as a physicist, certainly not to these people. So I wrote a letter to Mr. Barrett in which I invited him to lunch the following week at Columbia with Dr. Pegram, who was head of the physics department and dean of the graduate school, and Dr. Fermi, who after all was a Nobel Prize winner and quite well known.[32] He replied that he would not be in town that week; he did not suggest an alternate date, and he wrote that they had decided that they would not be in a position to let us have any graphite except on a straight purchase basis. I remember that I was quite depressed by that letter, and showed it to Pegram, who thought that I was too easily discouraged. And maybe I was.

The Washington meeting was followed by the most curious period in my life. We heard nothing from Washington at all. By the first of February [1940] there was still no word from Washington—at least none that reached me. I had assumed that once we had demonstrated that in the fission of uranium neutrons are emitted, there would be no difficulty in getting people interested, but I was wrong. Fermi didn't see any reason to do anything right away, since we had asked for money to buy graphite but hadn't yet gotten it; at that point he was interested in working on cosmic rays. I myself waited for developments in Washington, and amused myself by making some more detailed calculations on the chain reaction of the graphite-uranium system.

It is an incredible fact, in retrospect, that between the end of June 1939 and the spring of 1940, not a single experiment was under way in the United States which was aimed at exploring the possibilities of a chain reaction in natural uranium.

Late in January or early in February of 1940, I received a reprint of a paper by Joliot in which Joliot investigated the possibilities of a chain reaction in a uranium-water system.[33] In a sense this was a similar ex-

32. Letter, with memorandum, Szilard to Barrett, October 18, 1939.

33. H. von Halban, Jr., F. Joliot, L. Kowarski, and F. Perrin, "Mise en évidence d'une réaction nucléaire en chaine au sein d'une masse uranifère," *Journal de Physique et le Radium*, série VII, tome x, no. 10 (October, 1939), 428–429.

periment to the one which Anderson, Fermi, and I had carried out and published in June 1939. However, Joliot's experiment was done in a different set-up, and I was able to conclude from it what I was not able to conclude from our own experiment: namely, that the water-uranium system came very close to being chain-reacting, even though it did not quite reach this point. However, it seemed to come so close to being chain-reacting, that if we had improved the system somewhat by replacing water with graphite, in my opinion we should have gotten over the hump.

I read Joliot's paper very carefully and made a number of small computations on it, and then I went to see Fermi, with whom I was no longer in daily contact because my work at Columbia had ceased. We had lunch together and Fermi told me that he was on the point of going to California. I asked him, "Did you read Joliot's paper?" He said he had, and I then asked him, "What did you think of it?" and Fermi said, "Not much." At this point I saw no reason to continue the conversation and went home.

I then went to see Einstein again in Princeton, and told him that things were not moving at all. And I said to Einstein that I thought the best thing I could do was to go definitely on record that a graphite-uranium system would be chain-reacting by writing a paper on the subject and submitting it for publication to the *Physical Review*. I suggested that we reopen the matter with the government, and that we propose to take the position that I would publish my results unless the government asked me not to do so and unless the government were willing to take some action in this matter.

Accordingly, I wrote a paper for publication and sent it to *Physical Review* on February 16th [1940].[34] I brought the paper to Pegram, who was somewhat embarrassed because Fermi was out of town and Pegram did not know what action he should take. However, he said that he

34. "Divergent Chain Reactions in Systems Composed of Uranium and Carbon." This paper was sent to the *Physical Review* twice, first as a shorter Letter to the Editor on February 6th, then in full on February 14 (received February 16), 1940. With each version Szilard sent a covering letter to John Tate, editor, asking that publication be delayed; it was delayed indefinitely. The paper became Report A–55 of the Uranium Committee. After the war it was given the Manhattan District declassified report number MDDC–446.

must take some action, so he went to see Admiral Bowen[35] in Washington, who, Pegram thought, might take some interest because, after all, atomic energy might be used for driving submarines.

On the basis of the conversation I had with him, Einstein wrote to Alexander Sachs, and Sachs wrote again to the President,[36] and the President replied that he thought that the best way to continue research would be to have another meeting of the Uranium Committee. And now something most tragic and comic happened. Having received a letter from the White House, Sachs called up Lyman J. Briggs, chairman of the Uranium Committee, and suggested a meeting be called. And Briggs said he was on the point of calling a meeting and wanted to invite Sachs and Dr. Pegram to attend. Sachs said, "Well, what about Szilard and Fermi?" and Briggs said, "Well, you know, these matters are secret and we do not think that they should be included."

At this point, Sachs blew up. This was, after all, his meeting, and why should the people who were doing the job and who produced the figures not be included? This, however, was a misunderstanding: Briggs did not want to call the meeting because he had heard from the White House; he wanted to call the meeting at the initiative of Admiral Bowen, whom Pegram had contacted, so that Sachs and Briggs talked to each other at cross purposes. They were in effect talking about different meetings. However, somehow things got straightened out and the meeting was called which Fermi and I did in fact attend.[37]

I now have to go back to the summer of 1939, when in July I made the first steps in computing the uranium-graphite system. As soon as I saw that the uranium-graphite system might work, I wrote a number of letters to Fermi telling him that I felt this was a matter of some urgency,

35. Admiral Harold G. Bowen, director of the Naval Research Laboratory.

36. Letter, Sachs to Roosevelt, March 15, 1940, forwarded the letter from Einstein to Sachs, March 7, 1940, which contains the following paragraph: "Dr. Szilard has shown me the manuscript which he is sending to the *Physics Review* in which he describes in detail a method for setting up a chain reaction in uranium. The papers will appear in print unless they are held up, and the question arises whether something ought to be done to withhold publication." Otto Nathan and Heinz Norden, eds., *Einstein on Peace* (New York, 1960), p. 299.

37. The Advisory Committee on Uranium met at the National Bureau of Standards on Saturday, April 27th. Present were Chairman Briggs, Colonel Adamson, Commander Hoover, Admiral Bowen, Dean Pegram, Fermi, Szilard, Wigner, and Sachs.

and that we should not waste our time by making detailed physical measurements of the individual constants involved, but rather try to get a sufficient amount of graphite and uranium to approach the critical mass and build up a chain-reacting system.[38] Fermi's response to this crash program was very cool.[39] He said that he had thought of the possibilities of using carbon instead of water, that he had computed how a homogeneous mixture of carbon and uranium would behave, and that he had found that the absorption of carbon would have to be indeed exceedingly low in order to make such a system chain-reacting. I knew very well that Fermi must have been aware of the fact that a homogeneous mixture of uranium and carbon was not as good as a heterogeneous uranium-carbon system; he computed the homogeneous mixture only because it was the easiest to compute. And this showed me that Fermi did not take this matter really seriously. It was one of the factors which induced me to approach the government quite independently of Fermi or Columbia University.

In July 1939 when I had reported to Pegram my optimistic views about graphite, and told him why I thought the matter was urgent, he took the position that even though the matter appeared to be rather urgent, it being summer and Fermi away, there was really nothing that usefully could be done until fall—September, or perhaps October. This was the second factor which induced me to disregard everything else and go to the government directly.

Now, in the spring of 1940, we were advised that the money, the $6,000 which the committee had promised us, was available. We bought some graphite, and Fermi started an experiment to measure the absorption of that graphite. When he finished his measurement, the question of secrecy again came up. I went to his office and said, "Now that we have this value, perhaps the value ought not to be made public." At this point Fermi really lost his temper; he really thought that this was absurd. There was nothing much more I could say, but next time I dropped in at his office he told me that Pegram had come to see him, and Pegram thought that this value should not be published. From that point on, secrecy was on.

38. Letters, Szilard to Fermi, July 3, July 5, July 8, and July 11, 1939.
39. Letter, Fermi to Szilard, July 9, 1939; letter, Fermi to Pegram, July 11, 1939.

[EDITORS' NOTE: This portion of the taped interviews ends here. However, in the fragmentary outline of his memoirs mentioned in the headnote above, Szilard described some of the subsequent events in 1940 and 1941 as follows:]

In May 1940 I received a letter from Turner[40] in Princeton, who pointed out that in the chain reaction which I hoped to be able to set up there would be formed a new element which might be capable of undergoing fission. As we now know, this is in fact the case, and the element formed in the chain reaction is now called plutonium. Neither Fermi nor I had thought of this possibility, which was obviously of the utmost importance, and this realization increased my sense of urgency.

On Rabi's advice, I enlisted the help of H. C. Urey, who prevailed on the chairman of the Uranium Committee to appoint those of us who were actively interested in this problem to serve as a technical subcommittee of the Uranium Committee. We thought this would put us in a position to approach various laboratories in the U. S. and to enlist their cooperation in pursuing the various aspects of the problem, including the possibility raised by Turner's suggestion.

The Committee,[41] having been duly appointed, met in Washington, and when the meeting was opened by the chairman, he told us that the committee would be dissolved upon termination of the current meeting, because if the government were to spend a substantial amount of money—we were discussing sums of the order of a half million dollars —and subsequently it would turn out that it is not possible to set up a chain reaction based on uranium, there might be a congressional investigation. If this were the case, in such a situation it would be awkward if the government had made available funds on the recommendation of a committee whose membership comprised men other than American citizens of long standing. Fermi and I were not American citizens. Though Wigner was an American citizen, he was not one of long standing. Thus the work on uranium in the United States was brought to a

40. Louis A. Turner, in 1940 associate professor of physics at Princeton. His letter to Szilard is dated May 27, 1940.

41. A special advisory group called together by Briggs met at the National Bureau of Standards on June 15, 1940. Attending were Briggs, Urey, Tuve, Wigner, Breit, Fermi, Szilard, and Pegram. Henry De Wolf Smyth, *Atomic Energy for Peaceful Purposes . . .* (Princeton, 1946), p. 48. (Hereafter referred to as *Smyth Report.*)

standstill for the next six months. Mr. Wigner wrote a very polite letter to the chairman of the Uranium Committee saying that he would hold himself in readiness to work for the government on all matters related to defense, with the exception of uranium.

After reorganization in Washington, which put the Uranium Committee under Dr. Vannevar Bush's committee, Columbia University was given a contract in the amount of $40,000 to develop the Fermi-Szilard system. On November 1, 1940, I was put on the payroll of Columbia University under this contract. Since I was instrumental in inducing the government to assume expenditures for exploring the possibility of setting up a chain reaction, and with a view to the possibility that our efforts might come to nothing, it was deemed advisable to set my salary at a low figure, *i.e.*, $4,000 a year.

6. *America 1938 through November 1940 (1)* *Book 1960*

During the Munich Crisis, I happened to be in Urbana, Illinois, and I spent one week listening to the news over the radio. When it was all over, I was convinced that within one year there would be war. I resigned my position at Oxford.

In January, 1939, I learned of the discovery of the fission of uranium by Otto Hahn. I saw at once that if neutrons are emitted in the fission process it might be possible to set up a chain reaction. "H.G. Wells, here we come!" I said to myself. Immediately I was obsessed with two thoughts: To do, as quickly as possible, an experiment to discover whether or not neutrons are in fact emitted in the fission process, and to contact those laboratories in America, England and France where such an experiment conceivably could be thought of, and performed. This I thought had to be done with a view to reaching an agreement that if neutrons were in fact emitted, this fact should remain a secret of the three countries involved, lest the Germans developed the atomic bomb first and used it in the impending war. I was not affiliated at the time with any university, but after scouting around I borrowed $2,000.00, rented ~~a radio~~ *radium*, and teamed up with Dr. Walter Zinn, at that time instructor at City College. The experiment was actually set up at the Physics Department of Columbia University, and was performed on March 3, 1939. When I saw the neutrons emitted in the fission of uranium, I knew that the world was headed for trouble.

At that point I thought ~~[illegible]~~ that from this point on there should be no difficulty about obtaining financial support for this work. But in this I was quite mistaken.

(cont'd)

7/12/67

6. America 1938 through November 1940 (2)

My attempt to keep the neutron emission of uranium secret ran into difficulties, and it collapsed when Joliot in Paris published his results and declined to cooperate. The circumstances surrounding this collapse are not without human interest.

Fermi and I teamed up and performed an experiment which we thought would show that a chain reaction could be maintained in a system composed of water and uranium. We actually thought that we had shown this when the experiment was completed, but then George Placzek dropped in for a visit and showed us that we were in error.

In July, when I was left alone in New York, I recognized that a graphite uranium system would have a much better chance to sustain a chain reaction, and that accordingly the liberation of nuclear energy on an industrial scale was at hand. My first concern was to warn the Belgian Government of this possibility, lest being unaware of it, they make available uranium to Germany from the Belgian Congo. It was this consideration which brought me into contact with Einstein, a contact which resulted in Einstein's historic letter to President Roosevelt.

(cont'd)

In response to Einstein's letter, the President appointed a committee which met for the first time on October 21, 1939. We did not look to the Federal Government for funds but rather for official recognition that would have enabled us to obtain private funds. During the meeting, through what might be described as a comedy of errors, the issue of funds came up and the committee promised to provide us with $6,000.00 in order to enable us to buy a few tons of graphite. By February, 1940, we had not heard anything further from the Government, and in the period from June 1939 to April 1940, not a single experiment was under way in the U.S. that was concerned with the possibility of setting up a chain reaction.

In February, 1940, I decided to take some drastic action. I sent a paper to the Physical Review, describing how a chain reaction may be set up in a graphite-uranium system. And I took a copy of this paper to Einstein in Princeton. Einstein wrote a letter saying that if the Government was not interested in pursuing this matter, my paper would be published in due course of time.

This provoked another meeting of the Uranium Committee at which I was asked to defer publication of my paper. In the mean time, the $6,000.00 promised to us were received by Columbia University, and some of the most urgent expenditures got under way, but nothing remotely resembling the scale that was needed.

At this point I received a letter from Turner in Princeton, who pointed out that in the chain reaction which I hoped to be able to set up there would be formed a new element which might be capable of undergoing fission. As we now know, this is in fact the case, and the element formed in the chain reaction is now called plutonium. Neither Fermi nor I had thought of this possibility, which was obviously of the utmost importance, and this realization increased my sense of urgency.

(cont'd)

On Rabi's advice, I enlisted the help of H. C. Urey, who prevailed on the Chairman of the Uranium Committee to appoint those of us who were actively interested in this problem to serve as a technical subcommittee of the Uranium Committee. We thought this would put us in a position to approach various laboratories in the U.S. and to enlist their cooperation in pursuing the various aspects of the problem, including the possibility raised by Turner's suggestion.

The Committee, having been duly appointed, met in Washington and when the meeting was opened by the chairman, he told us that the committee would be dissolved upon termination of the current meeting, because if the government were to spend a substantial amount of money--we were discussing sums of the order of a half million dollars--and subsequently it would turn out that it is not possible to set up a chain reaction based on uranium, there might be a Congressional Investigation. If this were the case in such a situation, it would be awkward if the Government had made available funds on the recommendation of a committee whose membership comprised men other than American citizens of long standing. Fermi and I were not American citizens. Though Wigner was an American citizen, he was not one of long standing. Thus the work on uranium in the United States was brought to a standstill for the next six months. Mr. Wigner wrote a very polite letter to the Chairman of the Uranium Committee saying that he would hold himself in readiness to work for the Government on all matters related to defense, with the exception of uranium.

After reorganization in Washington, which put the Uranium Committee under Dr. Vannevar Bush's committee, Columbia University was given a contract in the amount of $40,000.00 to develop the Fermi-Szilard system. On November 1, 1940, I was put on the payroll of Columbia University under this contract. Since I was instrumental in inducing the Government to assume expenditures for exploring the possibility of setting up a chain reaction, and with a view to the possibility that our efforts might come to nothing, it was deemed advisable to set my salary at a low figure, i.e., $4,000.00 a year.

7/12/67

(cont'd)
folder 7.

2nd set
May 69

Additional Material for page 105 (Section 6)

re: DECISION TO REMAIN IN U.S., 1938 to Jan. 1939.

Letter, G.M. Chapman, Secretary to Lindemann May 17, 1938
See last paragraph: "It is hoped that Dr. Szilard will be able to go to America for several months in the year..."

Telegram, L.S. to Miss Chapman. Undated, but soon after May 17, 1938.

Letter, F.A. Lindemann to L.S. June 6, 1938

Telegram, Lindemann to L.S. June 7, 1938

Letter, L.S. to Lindemann June 15, 1938
Letter, L.S. to Simon July 22, 1938
Letter, Lindemann to L.S. July 30, 1938
Mentions "...your application for an unconditional permit to remain in England"
Letter, Simon to L.S. Aug 23, 1938
Telegram, L.S. to Lindemann. Undated, probably Oct. 1938
Both typed and handwritten versions.
"Have on account of international situation with great regret postponed my sailing for an indefinite period..."

Letter, L.S. to Tuck. 4 pp. Oct. 21, 1938
See especially the first page.

Letter, L.S. to Lindemann 3 pp. Jan. 13, 1939
~~Very interesting~~ Szilard expresses his views on pure science versus social responsibility.

TELEPHONE 354-

(History A. GWS)

THE CLARENDON LABORATORY,
UNIVERSITY MUSEUM,
OXFORD.

England.

17th May, 1938.

Dear Dr. Szilard,

Professor Lindemann has asked me to let you know that the question about your proposed Lectureship came before the last Board of Faculty meeting (May 10th), but it has, of course, also to go before the General Board.

The terms of the proposal are as follows, and the Professor would be glad if you could let me know by cable whether they meet with your approval.

" 1. It is proposed that Dr. Szilard should lecture eight times a term for two terms in the year on High Tension Physics and Nuclear Stability.

2. Dr. Szilard was Privatdozent at Berlin University and is a man of great originality. He invented the apparatus for accelerating electrons, on which we are working and for which a special grant over a period of five years has been made by the Hebdomadal Council.

3. It is hoped that Dr. Szilard will be able to go to America for several months in the year in order to keep in touch with developments there and so to speak act as liaison officer between the two countries. The subject is progressing so rapidly especially in the U.S.A., that an enormous amount of effort can be saved if some such arrangement is made. As he is receiving no salary from the University it would seem that there should be no objection to such a scheme of work which would be most beneficial to us and might even by an advantage to American Laboratories which he visits.

6.5.1938. F.A.L. "

Yours sincerely,

E. M. Chapman
Secretary.

Dr. Leo Szilard.

c/o B. Liebowitz, Esq.

120 Riverside Drive

Not sent.

(Hist A)

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To MISS CHAPMAN 193

CLARENDON LABORATORY OXFORD

APPRECIATE VERY MUCH PROFESSOR LINDEMANNS PROPOSAL STOP NUCLEAR PHYSICS SUFFICIENTLY GENERAL TITLE SEEMS STOP BETTER NOT FIX NUMBER OF LECTURES ~~xxxxxxxxxx~~ RIGIDLY FOR THE PRESENT ESPECIALLY AS SIX MONTHS STAY UNDER PRESENT ARRANGEMENT MAY NOT ALWAYS COINCIDE WITH TWO ~~TERMS SZILARD~~ FULL TERMS THANKS SZILARD

JUST RECEIVED LETTER MAY 17

TELEPHONE 3545.

THE CLARENDON LABORATORY,
UNIVERSITY MUSEUM,
OXFORD.

Hist. A

England.

June 6th, 1938.

Dear Szilard,

Just a line, which I hope will catch the boat, to thank you for your letter and cable. Unfortunately the cable is not clear. The Board of Faculty insist on these conditions, and it is no use trying to vary them. All that I can now do is to try and cancel the whole thing if you are not prepared to accept their conditions. The University does not wish to make a lectureship if the lecturer is unable to give this number of lectures a year.

I am sorry you wish to postpone your return, but I think on the other hand it will be best to spend the six months from October to March here than to be away the whole of the winter; so this arrangement is probably the best. Please cable immediately and definitely whether or not you accept the lectureship.

With apologies for this hasty note, believe me,

Yours sincerely,

F. A. Lindemann.

Dr. Leo Szilard.

c/o Liebowitz.

420, Riverside Drive,

New York.

P.S. We do not know yet what money will be available. The thing is to find out what we can do for different sums so that we can get the right amount of money for the apparatus when required.

Hist-A

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Form 112 Ta-1141

History A. phys

c/o Liebowitz
420 Riverside Drive
New York City

June 15, 1938

Dear Professor Lindemann,

I answered your cable from Rochester, but it was to late then for a letter to catch a fast boat. I am therefore making use of the first fast boat to-day to write you a more detaile explanation than it was possible to give you by wire.

As I gathered from your cable and see even more clearly from your letter of June 6, which I received this morning, the board of faculty has created a fait accompli. I realise that it would be awkward trying to vary or even cancel it. One reason why I wished to avoid for the present a rigid fixation of the terms is the following. In order to be able to cover my living expenses, it seems advisable to complement the £ 200 per year, which I would receive from the I.C.I. by an income from an American source. A rigid fixation of the Oxford terms reduces my flexibility with regard to an American arrang ment. Since I have so far made no attempts in this direction, I am naturally unable to foresee, if my presence in America during the summer is a sufficient basis for this.As however obviously no choice, however, it seems best to try and see whether we can make the proposed Oxford arrangement work and f

in something on this side.

I appreciate very much the trouble which you have taken in this matter, and shall write you more within the next few days.

Yours very sincerely,

(Leo Szilard)

(Hist-A)

D

c/o Liebowitz
420 Riverside Drive
New York City

July 22nd, 1938

Dr. F. Simon
10 Bebroughton Road
Oxford, England.

Lieber Herr Simon!

Es gibt hier immer so viele Sachen zu tun, dass man schwer von New York fortkommt. Immerhin ist jetzt vielleicht das Meiste doch abgewickelt, und ich kann dann Anfang August nach Californien fahren. Fuer meinen Bruder und seine Familie ist es mir anscheinend gelungen, ein Immigrationvisum zu bekommen. Es war eine schwere Geburt, und es musste mit grossen Kanonen geschossen werden. Die ungarische Quote ist nämlich nur 860 im Jahr, und wie Sie sich vorstellen können, für längere Zeit vergeben.

Von Professor Lindemann hatte ich vor einiger Zeit ein Telegramm, aus dem hervorging, dass die geplante lectureship zur Bedingung hat, dass ich während ~~des~~ zweier terms je acht Vorlesungen halte. Ich habe Professor Lindemann ebenfalls telegraphisch mitgeteilt, dass ich das akzeptiere, nachdem ich ihm vorher schon brieflich auseinandergesetzt hatte, dass eine starre Festlegung nach dieser Richtung hin für mich die Lage erschwert.

Ich kann natürlich unmöglich von den £1000 leben, die ich von Oxford bekomme, und ich brauche eine gewisse Ellbogenfreiheit, wenn ich durch ein amerikanisches Einkommen das englische Einkommen zu ergänzen habe. Da aber auf Grund von Pro-

fessor Lindemann's Telegramm das Oxforder Arrangement unabänderlich schien, schrieb ich Professor Lindemann, dass ich seine Mitteilung zur Kenntnis nehme und gern versuchen will, wie sich das Oxforder Arrangement auswirkt, und abwarten, ob es gelingt, etwas in Amerika zu finden, was sich dem Oxforder Arrangement anpasst.

Da ich seither nicht gehört habe und eigentlich gern wüsste, ob ich mich auf Vorlesungen im nächsten Semester vorzubereiten habe, habe ich heute an Professor Lindemann geschrieben und ihn gebeten, mir mitzuteilen, ob die lectureship in diesem term perfekt geworden ist oder nicht. Falls diese Zeilen Sie in Oxford treffen, könnten Sie sich vielleicht erkundigen und mir das Resultat kabeln. (Ich wollte Professor Lindemann nicht bitten, dass er mir ein Kabel schickt.)

Hier ist eine riesige Einwanderung aus Oesterreich und Deutschland. Alle Schiffe sind voll mit Emigranten. Im Uebrigen finde ich mit Erstaunen, dass die gefürchtete New Yorker Sommerhitze von mir als angenehm empfunden wird. Oder glauben Sie, dass der Einfluss meiner augenblicklichen politischen Sympathien so weitreichend ist, dass er auch noch die Hitze als angenehm erscheinen lässt?

Was sind Ihre Pläne?

Mit freundlichen Grüssen

Ihr

(Leo Szilard)

TELEPHONE 3545.

THE CLARENDON LABORATORY,
UNIVERSITY MUSEUM,
OXFORD.

England.

July 30th, 1938.

Dear Szilard,

I thank you for your letter of the 22nd July. Everything is settled about your proposed lectureship, with the exception that it has to go before the General Board; but this is only a formality. The matter could have been settled straight off, but we did not hear from you definitely until after the last meeting of the Board ~~of Faculty~~. Your name has been put down in the Lecture List for Michaelmas term, but the time of the lecture will have to be arranged later.

With regard to your application for an ~~unconditional permit~~ to remain in England: I enclose a copy of a letter which I received from the Home Office last March although I understand that a copy has already been sent to you.

Please excuse this hasty note, and believe me,

Yours sincerely,

F. A. Lindemann.

Dr. Leo Szilard.

c/o B. Liebowitz, Esq.

420, Riverside Drive,

New York.

Hist A

23.August 38.

Lieber Herr Szilard,

Ich habe Lindemann nach Erhalt Ihres Briefes nicht mehr gesehen; er ist jetzt in Frankreich, kommt Anfang September wieder und fährt vielleicht Ende Sept. nach Amerika, aber das ist fraglich. - Soviel ich weiss, ist Ihre Lecturership sicher, ob alle Formalitäten erfüllt sind, weiss ich nicht und kann ich jetzt in den Ferien auch nicht in Erfahrung bringen.

Man muss schon sagen, dass Sie es den Leuten hier nicht leicht machen, etwas für Sie zu tun. Hören tut man kaum etwas von Ihnen, wenn ,dann ein paar Zeilen, in denen nichts wesentliches drin steht, und niemand weiss ,wann Sie eigentlich zurückkommen. Dann hört man plötzlich von einem Besucher, der Sie drüben gesehen hat, dass Sie jetzt seit einiger Zeit Psychologie treiben - das wäre viel wichtiger als Neutronen. Das ermuntert natürlich die Leute hier auch nicht, sich für Sie ins Zeug zu legen. Jetzt schreiben Sie, dass die Sache mit der Lecturership Ihnen nicht passt, wenn Sie 2 terms hier lesen sollen und Sie könnten doch nicht von 1000 doll. leben. a. können Sie doch nicht erwarten, dass man jemandem eine lecturership gibt, der nur den 3. Teil des Jahres liest. b. es hat Ihnen ja hier niemand gesagt, dass Sie nur das halbe Gehalt nehmen sollen und den Rest in Amerika dazuverdienen sollen!

Ich muss Ihnen mal wieder ins Gewissen reden. Jeder, den man hier spricht, ist wütend auf Sie, ob das Miss Simpson oder Tuck ist oder irgend jemand und das sind die Leute, die Ihnen wohlwollen! Mit Ihrer unsteten Handlungsweise , dem plötzlich auf lange Zeiten Verschollensein etc. kommen Sie nicht weiter und es wäre sehr schade, wenn Sie daran scheitern würden. Wenn Sie sich so weiter verhalten, können Sie nicht erwarten, dass Sie jemals eine vernünftige Stelle bekommen werden und man muss sogar sagen, man kann es den Leuten nicht verdenken. Mir hat mal ein Amerikaner gesagt, es gäbe einen Haufen Leute, die sich beennend gern mit Ihnen ein paar Tage unterhalten wollen, aber kaum einen, der Ihnen eine Stelle anbieten möchte. Ich glaube, das ist ganz richtig, leider.

Sie könen natürlich sagen, Sie ziehen es vor Ihr bisheriges Leben weiterzuführen und auf andere Leute und deren Sitten und Gebräuche keine Rücksicht zu nehmen. Dann müssten Sie aber auch die Consequenzen ziehen und von diesen anderen Leuten nichts verlangen.

Ich sage Ihnen dies nicht, um Sie zu ärgern, aber ich glaube es ist nötig und viele andere glauben es auch, aber scheinbar sagt es Ihnen niemnad. Vielleicht ist es am besten, Sie würden heiraten und zwar eine Frau, die etwas mehr als Sie die Realitäten beachtet. Ja, die Verantwortung für diesen Rat ist natürlich schwer, aber sie werden ihn ja doch nicht befolgen.

Hier geht es sonst gut, bloss die Sachen in Deutschland so sind fürchterlich deprimierend. Jeden Tag Hilferufe, man versucht zu machen, was man kann, fast nie mit Erfolg. Einzelaktionen nützen ja jetzt nichts mehr.

Mit herzlichen Grüssen, auch von meiner Frau,
Ihr
Tm L

Book folder No. 1. (5)

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Send the following message, subject to the terms on back hereof, which are hereby agreed to

To Lindemann 19

Street and No. Christ Church

Place Oxford

Have on account of international situation with great regret postponed my sailing for an indefinite period Stop would be very grateful if you could consider absence as leave without pay Stop writing Stop Please communicate my sincerely felt good wishes to all in these days of grave decisions Szilard 6.

[Oct. 1938 - K+W]

LINDEMANN
CHRIST CHURCH
OXFORD, ENGLAND

HAVE ON ACCOUNT OF INTERNATIONAL SITUATION WITH GREAT REGRET POSTPONED MY SAILING FOR AN INDEFINITE PERIOD STOP WOULD BE VERY GRATEFUL IF YOU COULD CONSIDER ABSENCE AS LEAVE WITHOUT PAY STOP WRITING STOP PLEASE COMMUNICATE MY SINCERELY FELT GOOD WISHES TO ALL IN THESE DAYS OF GRAVE DECISIONS

SZILARD

(3)

(5)

c/o Liebowitz
420 Riverside Drive
New York City

October 21st, 1938

Dear Tuck,

I wonder whether you see the European situation in the same way as I do. I believe that this last breach of faith which lead to the Munich agreement has definitely settled the fate of Europe for a long time to come. It will not be possible for either England or France to make international agreements and get anybody to believe that they will keep these agreements, if keeping them means risking war. It follows that international anarchy will rule in Europe and perhaps elsewhere. This may mean war or merely continuous unrest accompanied by "peaceful" changes.

I do not think that in the circumstances I would wish to live in England if I am limited to do work in physics in the Clarendon. But I would return to England if I find that it will be possible for me to cooperate with others in working towards changing this disastrous situation. I am taking steps to find out if anything of such nature is possible, in the meantime I shall ask Professor Lindemann for a further leave of absence without pay, or, if that should prove impossible, I shall resign from the Clarendon.

This for your personal information only. As to yourself, knowing that you are not very much interested in the fate of nations and believe that nothing can be done about it anyway, I think that you are very wise to think of America. Maybe you could try for a Common Wealth Fellowship for next year

Whether I can find something for you here right away I do not know, but I would try it at once and with great energy if, having received this letter, you advise me by cabbe that you want me to do so. I feel that it is doubtful whether you should accept any position which is not safe for at least two years and carries a salary of at least $2500. I might try to find something for you on such terms on the basis of our cystein work, as it seems that I can make people in this country enthusiastic about it. Of course, I do not know in the least whether I can get them to go at it seriously. I believe though that another year or two spent with radioactive indicator work with cystein, carried out in collaboration with physiological chemists, would give you a very good start here. There is some danger though that, if I talk to people about you, mentioning your name - and of course I would have to mention it - rumours might arise which could reach Oxford and perhaps prejudice your position. You must let me know whether you want to take that risk. I shall then take all precautions I can. As to the merits of the question whether you should or should not leave England, I have little to say. Unless you take a Common Wealth Fellowship and come here as a temporary visitor, leaving England means a very grave decision. If you decide at all that you want to leave England, you ought to take that decision either on the ground that you believe that you would rather live here for the next ten years than in England, or on the ground of some broad principle. Moreover, it can easily be that, in spite of your decision, I am not able to find anything suitable for you. Of course, the possiblity

to apply for a Common Wealth Fellowship would still remain. If you have something definite to say about (wanting me to do something) this point, please send me a cable, because I have some free time now and intend anyway to discuss cystein with a number of people in the near future. In a few weeks time I might be away from the East and unable to do much about it.

As to the cystein publication I am not certain whether it is wise to say something about "moonshine", but if you prefer a joint publication I shall reconsider the matter. There is one condition, however, on which I have to insist, that is that I may make a footnote pointing out that you carried out all observations (yourself) during my absence from Oxford. This might be unconventional, but I feel that I cannot take credit for what I have not done. If you think we can make such a footnote, then I would be very glad to publish jointly but would have still to think a little bit about the advisability of a joint publication from the point of view that it might be premature to say those things which I wanted to say. I quite agree that you have to publish in any case very soon in order to satisfy the Salter people. By the way, what is your status now? Have they not extended the fellowshipt for another year? Or did Lindemann make some other arrangement? What do you think are your chances of a real career at Oxford?

As to the electron transformer, the instability of the electron path in a radially decreasing field of cylindrical symmetry which you think you have proved, does not exist. The mistake you make is the following: you consider the force acting on the electron instead of considering, as you ought to, the product of time

and force. If the electron spends less time in the stronger field than in the weaker field, the time factor may compensate for the force factor, and that is exactly what in fact happens. You can see this by considering a special case in which you can easily construct the path of the electron. I have done that on the enclosed sheet of paper for the following special case: the magnetic field has a uniform value, both inside the circle F and outside the circle F, but the value jumps at the circle and the value outside is smaller than the value inside. The path of the electron which I have drawn, starts at B; it is a segment of a circle, the center of which is at O and the radius of which is ∞. This segment ends at point C, from then on, between point C and D, the path is a segment of another circle, the center of which is at A and the radius of which is R. At D the electron again enters into the stronger field and the path is again a segment of a circle with a radius r, The center of this segment is however now O . It is obvious that O is at the same distance from the cross which marks the center of the circle F as O. Therefore, there is no displacement of the electron path, either towards the weaker field or towards the stronger field.

Though there is no instability of the electron path, there is an instability of the political situation, and if you also feel that we should not build an electron transformer at Oxford, please let me know quickly so that I can officially tell Lindemann that we are abandoning the project.

Please write.

Yours,

Oxford

Bookfolder No. 1 (11)

c/o Liebowitz
420 Riverside Drive
New York City

January 13th, 1939

6.

Professor F.A. Lindeman
Christ Church
Oxford, England

Dear Professor Lindeman:

Three months have now passed since, acting on an impulse, I cabled you that I am postponing my sailing for an indefinite period on account of the international situation, and that I should be grateful if my further absence could be considered as leave without pay. I sent you this cable after Czechoslovakia was forced to accept the Berchtesgaden demands, and it must have reached you at a moment when many people believed that there was an immediate danger of general war. You may have therefore thought that this assumed danger prompted me to postpone my sailing, and you may then have wondered why I did not return to Oxford after the Munich agreement, that is if you gave any thought to my continued absence at a time when urgent political and defense questions must have been claiming most of your thoughts.

It seems to me that the Munich agreement created, or at the very least demonstrated, a state of international relations which now threatens Europe and in the long run will threaten the whole civilized world. This cannot fail to claim the attention of all of us, and, if the situation is to be improved, the active cooperation of many of us. I greatly envy those of my colleagues at Oxford who in these circumstances are able to give their full

(Not sent to Harvard)

attention to the work which has been carried on at the Clarendon Laboratory and who are able to do so without offending their sense of proportion. To my great sorrow I am apparently quite incapable of following their example.

Since my collaboration in the work, for which you were good enough to win the support of Imperial Chemical Industries, would be of little value unless Itgave the work my full attention, it seems best in the circumstances that I should not embark upon it. This being so, I do not feel that I am entitled to keep any payments which Imperial Chemical Industries may have made to me under the new agreement, i.e. after January 1st of last year. I should be grateful if you could perhaps communicate on this subject with Dr. Slade and tell him how very thankful I am for the help I had from Imperial Chemical Industries in the past, and how very much I regret that the deterioration of the international situation which occured while I was abroad, makes it impossible for me to collaborate in the work which Dr. Slade kindly consented to support. If Dr. Slade wishes me to refund payments made to me after January 1st of last year, I shall be very glad to do so. In this case Dr. Slade will have to let me know the amount which actually has been paid to my account, and also to what account and under what heading he wishes me to transfer this amount.

It seems to me that those who wish to continue to dedicate their work to the advancement of science would be well advised to move to America where they may hope for another ten or 15 years of undisturbed work. I myself find it very difficult, though, to elect such "individual salvation", and I may therefore return to

England if I can see my way of being of use, not only in science, but also in connection with the general situation. It is hardly necessary to state that, if I shall be in England and if you want me to do so, I shall be most happy again to cooperate with those who work in the Clarendon Laboratory. It may be best, however, that I should not receive financial support from the Laboratory, as such financial support is bound to be linked with fixed obligations which I would rather avoid.

For the time being, I do not yet see my way of being of use in England in connection with the general situation, though I see certain potential possiblities in this respect. In view of these I am at present not looking for a "job" on this side of the Atlantic. Perhaps I shall have an opportunity to talk to you about all this if I shall visit England in a not too distant future.

Naturally I regret that it will not be possible for me to collaborate in building up apparatus for the new Clarendon Laboratory. I trust that the spirit of inflation, which must necessarily accompany any armament race such as is on at present, will at least make it possible for you to obtain the funds necessary for carrying on research in the new laboratory.

Please excuse the three months' delay of this letter. Immediately after the Munich agreement it did not seem possible for me to have a sufficiently balanced view, and I had to allow some time to elapse before I was able to write without bitterness of this event.

With kind regards to all, I am,

yours very sincerely,

(Leo Szilard)

2nd set
May, 1969

Additional Material for page 106

re: "This was early in January when I visited Mr. Wigner in Princeton."

Wigner to L.S. Jan. 12, 1939

Backfolder No 1 (9)

Patent provisions January 12, 1939

Dear Szilard:

Many thanks for your telephone call and for your kind offer to come and see me. Fortunately, I am much better right now and hope to be up soon.

I would like to ask you to do two things for me in New York: Telephone to Mr or Mrs D. Parkinson: University 4-5790 that I have the jaundice and it is better if they do not come to Princeton on Sunday. I lost their address, otherwise, of course, I would write to them directly.

Could you get hold of affidavit blanks for quota-preference immigrants, Form 663

6.

not sent

(to my parents)? I would be very grateful to you.

Hoping to see you soon in Princeton again, I am, yours

Wigwam

I was in a hurry when writting this letter as somebody waits to mail it.

E. W.

Talán ~~[illegible]~~ inkább akkor ..... jöjjön ki, amikor megint egészséges ragyok.

2nd set
May 1969

Additional Material for page 106

re: NOTE No. 15

Letter,	L.S. to Director of Navy Contracts, Admiralty,	Dec. 21, 1938
Telegram,	" " " " " " "	Jan. 26, 1939
Letter,	" " " " " " "	Feb. 2, 1939
Letter,	Director of Navy Contracts to L.S.	Feb. 28, 1939

For preceding correspondence
see "Additional Material for pag 102-103 (2)"
in Section 5.

S.S. bk fld #1 G

c/o Clarendon Laboratory
Parks Road
Oxford,
~~November~~ December 21st, 1938

Director of Navy Contracts,
Admiralty,
London S.W.1.

Ref. No. C.P. Branch 10,
Patents 8142/36

Sir:

I refer to your letter of March 26th, 1936, which bears the above reference number, and beg to inform you that further experiments, carried out in collaboration with Professor M. Goldhaber, Cavendish Laboratory, Cambridge, and in cooperation with S.W. Barnes of the University of Rochester, have definitely cleared up the anomalies which I have observed in 1935. This work will be published in the Physical Review.

In view of this new work it does not now seem necessary to maintain the patent with which your letter deals, nor would the waiving of the secrecy of this patent serve any useful purpose. I beg therefore to suggest that the patent be withdrawn altogether.

I am, Sir,

Yours very truly,

(Leo Szilard)

Jan 26-39
Referring to CP 10 Patents 8142/36
KINDLY DISREGARD MY RECENT
LETTER STOP WRITING: Leo Szilard

B

Bk. S 1

DIRECTOR OF NAVY CONTRACTS
ADMIRALTY
LONDON, S.W. 1

JANUARY 26, 1939

REFERRING TO CP10 PATENTS 8142/36 KINDLY DISREGARD MY RECENT LETTER

STOP WRITING

LEO SZILARD

6-1-27 r

1)

c/o Liebowitz
420 Riverside Drive
New York City

February 2nd, 1939

Director of Navy Contracts
Admiralty
London S.W.1.

Ref. No. C.P. Branch 10,
Patents 8142/36

Sir:

I have written to you a letter dated of December 21st, 1938, and subsequently I cancelled this letter by cabling you on January 26th as follows: "Referring to C.P. 10 patents 8142/36 kindly disregard my recent letter stop writing Leo Szilard".

I wish to explain my reason for cancelling my letter by cable, and in order to do so I have to state the following.

When I wrote you the letter of December 21st, I was satisfied that the anomalies observed in indium have been definitely cleared up, and that indium cannot be used for the process described in the patent which has been assigned to you. This is still my opinion. Since I wrote you my letter of December 21st it has however turned out that anomalies observed in another element, which appeared to be similar to the case of indium, have an entirely different explanation, and that the process underlying the anomalies of this other element might very well turn out to be similar to the process described in the patent assigned to you. Intense work is now being done on this phen-

omenon, and I cabled you in order to ask you not to take any action until this new work has been completed.

I should be glad if you could perhaps let me know whether my cable reached you in time to stop any action which you may have started on the basis of my preceding letter, and accordingly whether or not the status of the patent remains unchanged.

Kindly note that I am at present in America and to be reached at the above address, but, of course, a letter addressed to the Clarendon Laboratory, Oxford, will also reach me with some delay.

I am, Sir,

Yours very truly,

(Leo Szilard)

663 51

Please address reply—
THE DIRECTOR OF NAVY CONTRACTS,
ADMIRALTY,
LONDON, S.W.1,
and quote—
C.P. Branch ...Patents 9512/39

Telegraphic Address:-
CONTRACTS, ADMIRALTY, LONDON.
Telephone No. WHITEHALL 9000, EXTENSION. 70

ADMIRALTY,
LONDON, S.W.1.

28 February, 1939.

Sir,

With reference to your letter 2nd February 1939, I have to inform you that your cablegram of 26th January reached this Department before any action had been taken on your letter of 21st December 1938 and the status of secret patent No.19157/34 therefore remains unchanged.

I would also state that unless secrecy is waived the patent will remain in force for the full term of sixteen years from the date of application. It is only in the event of secrecy being waived that the patent would be subject to renewal from year to year.

I am, Sir,

Your obedient Servant,

P. Lyles

for Director of Navy Contracts.

L.Szilard Esq.,
c/o Liebowitz,
420 Riverside Drive,
NEW YORK CITY.

6-1-30

2nd set
May, 1969

Additional Material for Page 106. -111

A running commentary on the events of early 1939 can be seen in Szilard's correspondence with Lewis Strauss during this time.

Letter,	L.S. to Strauss		Jan. 25, 1939
"	L.S. to Strauss		Feb. 13, 1939
Telegram,	L.S. to Strauss	(Handwritten)	Feb. 19, 1939
"	" " "	(Typed)	
Letter,	L.S. to Strauss		Feb. 22, 1939
"	L.S. to Strauss		Feb. 28, 1939
Telegram,	L.S. to Strauss	Undated, probably March 3rd or 4th, 1939 (Handwritten and typed versions)	(K.W.)
Letter,	L.S. to Strauss		April 11, 1939
"	L.S. to Strauss		April 14, 1939
"	Strauss to L.S.		April 14, 1939
"	Strauss to L.S.		April 17, 1939

Telegram, L.S. to Strauss — June 14, 1939

Letter, Wm. H. Mulqueen, Asst. to Strauss, to L.S. — June 16, 1939

Letter, L.S. to Strauss — July 3, 1939

K.W.

Bk.f. 1

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

January 25th, 1939.

Mr. Lewis L. Strauss
c/o Kuhn, Loeb & Co.
52 William Street
New York City

Dear Mr. Strauss:

I feel that I ought to let you know of a very sensational new development in nuclear physics. In a paper in the "Naturwissenschaften" Hahn reports that he finds when bombarding uranium with neutrons the uranium breaking up into two halves giving elements of about half the atomic weight of uranium. This is entirely unexpected and exciting news for the average physicist. The Department of Physics at Princeton, where I spent the last few days, was like a stirred-up ant heap.

Apart from the purely scientific interest there may be another aspect of this discovery, which so far does not seem to have caught the attention of these to whom I spoke. First of all it is obvious that the energy released in this new reaction must be very much higher than in all previously known cases. It may be 200 million volt instead of the usual 3-10 million volt. This in itself might make it possible to produce power by means of nuclear energy, but I do not think that this possibility is very exciting, for if the energy output is only two or three times the energy input, the cost of investment

would probably be too high to make the process worth while. Unfortunately, most of the energy is released in the form of heat and not in the form of radioactivity.

I see, however, in connection with this new discovery potential possibilities in another direction. These might lead to a large-scale production of energy and radioactive elements, unfortunately also perhaps to atomic bombs. This new discovery revives all the hopes and fears in this respect which I had in 1934 and 1935, and which I have as good as abandoned in the course of the last two years. At present I am running a high temperature and am therefore confined to my four walls, but perhaps I can tell you more about these new developments some other time. Meanwhile you may look out for a paper in "Nature" by Frisch and Meitner which will soon appear and which might give you some information about this new discovery.

With best wishes,

yours sincerely

(Leo Szilard)

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

February 13th, 1939

Mr. Lewis L. Strauss
Brandy Rock Farm
Brandy, Va.

Dear Mr. Strauss:

I hope you and Mrs. Strauss enjoyed staying at Palm Beach and that you are now having a nice time at your farm.

After I left your train in Washington I spent a day with Dr. Teller there and another day with Dr. Wigner at Princeton and told both of them of our tentative plan to make use of the form of an "association" and let such an association take action if it seems desirable that something should be done along the lines which we discussed. Dr. Teller, who is Professor for Theoretical Chemistry at George Washington University, will be at our disposal if it becomes necessary to keep some person close to the Administration informed of the developments, and he also can get the cooperation of his colleagues in Washington if this will be required. Dr. Wigner thought that some of the experiments which we discussed could be done at Princeton. As he is an old friend of mine and has much influence in the department there is very much in favor of following his suggestion, but I feel that it will be necessary to see what the position is from the point of view of equipment, and whether some younger members of the department could

cooperate without abandoning work in which they are at present engaged.

On my return to New York I went to see Fermi to tell him of all these conversations and also to discuss some of the small scale experiments which might be made in the near future.

Since my return almost every day some new information about uranium became available, and whenever I decided to do something one day it appeared foolish in the light of the new information on the next day. I found that the Radium Chemical Co. had in stock 200 mgm of radium mixed with beryllium, which is a nice constant source of fast neutrons. The rent for six months amounts to £ 500,00. As Fermi thought that he would like to use such a neutron source for his experiment I felt that I ought to get it for him. It did not seem fair to ask you to take any decisions from a distance, and so I thought it might be best that I should advance £ 500,00 for expenses of this type and to see later whether you could sanction the expenditures afterwards. A few days later it turned out that this neutron source was too bulky to be suitable for Fermi's experiment, and Fermi said that for the present he is quite satisfied with the radon sources which he is getting anyway once a week at Columbia. In these circumstances I arranged with the Radium Chemical Co. that they will let me have one gram of radium on loan instead. This radium used in conjunction with the beryllium block sent from Oxford represents an intense source of photo-neutrons which can be used for a

number of experiments. The rent is $ 125,00 per month, and we have to rent it for a minimum period of three months.

The outlook has changed in some important respects since I last saw you. It is now known that fast neutrons split both uranium and thorium, but slow neutrons do not split thorium, and they probably do not split the bulk of uranium either. If enough neutrons are emitted when fast neutrons split thorium or uranium it will still be necessary to see whether or not the emitted neutrons are slowed down to a velocity at which they are ineffective before they had a chance to split enough nuclei to make the maintenance of a chain reaction possible.

On the other hand, slow neutrons seem to split a uranium isotope which is present in an abundance of about 1% in uranium. If this isotope could be used for maintaining chain reactions, it would have to be separated from the bulk of uranium. This, no doubt, would be done if necessary, but it might take five or ten years before it can be done on a technical scale. Should small scale experiments show that the thorium and the bulk of uranium would not work, but the rare isotope of uranium would, we would have the task immediately to attack the question of concentrating the rare isotope of uranium.

As you see, the number of possibilities has increased since you left town. Some of the experiments which were devised, in particular the experiment which Fermi first planned, appear now to be much more difficult than before. Other experiments, such as those with photo-neutrons, are not affected,

but of course they have somewhat the character of preliminary experiments.

I am enclosing a clipping which might interest you, as it shows the state of mind of some physicists on February 4th. The man who inspired this article did his best to hide what he thought, but his dementi is somewhat clumsy, and he almost gives himself away in the last paragraph.

Anyway, things have calmed down to some extent in the last few days, and the newspapers at least might soon forget about uranium.

With best wishes,

Yours very sincerely

(Leo Szilard)

Patent

Strauss

publ. proposal

Bk. f. 2

(12)

1207-A

CLASS OF SERVICE DESIRED			
DOMESTIC		CABLE	
TELEGRAM		FULL RATE	
DAY LETTER		DEFERRED	
NIGHT MESSAGE		NIGHT LETTER	
NIGHT LETTER		SHIP RADIOGRAM	

Patrons should check class of service desired; otherwise message will be transmitted as a full-rate communication.

WESTERN UNION

R. B. WHITE, PRESIDENT — NEWCOMB CARLTON, CHAIRMAN OF THE BOARD — J. C. WILLEVER, FIRST VICE-PRESIDENT

CHECK
ACCT'G INFMN.
TIME FILED

Send the following message, subject to the terms on back hereof, which are hereby agreed to

19. Febr. 1939

To Strauss

Street and No.

Place

Private communication just received ~~that~~ shows that neutrons are emitted if uranium is split by very fast neutrons stop easy detection is due to time lag of 10 sec. in emission stop does not mean that our problem is solved in positive sense stop number ~~[illegible]~~ and velocity of emitted neutrons not known

Sender's address for reference

WESTERN UNION GIFT ORDERS ARE APPROPRIATE GIFTS FOR ALL OCCASIONS.

Sender's telephone number

Bk. f. 2 (13)

1207-A

CLASS OF SERVICE DESIRED	
DOMESTIC	CABLE
TELEGRAM	FULL RATE
DAY LETTER	DEFERRED
NIGHT MESSAGE	NIGHT LETTER
NIGHT LETTER	SHIP RADIOGRAM

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TIME FILED

Send the following message, subject to the terms on back hereof, which are hereby agreed to

_______________ 19___

To _______________

Street and No. _______________

Place _______________

stop yet result has bearing on our problem stop if we wished to prevent publication would have to act fast through administration and even so success doubtful stop Dr. Wigner in town Tuesday and Dr. Teller Wednesday. Szilard

Sender's address for reference

Sender's telephone number

Bkf. 2

(1)

LEWIS L. STRAUSS

February 19, 1939

PRIVATE COMMUNICATION JUST RECEIVED SHOWS THAT NEUTRONS ARE EMITTED IF URANIUM IS SPLIT BY VERY FAST NEUTRONS STOP EASY DETECTION IS DUE TO TIME LAG OF 10 SECONDS IN EMISSION STOP DOES NOT MEAN THAT OUR PROBLEM IS SOLVED IN POSITIVE SENSE STOP NUMBER AND VELOCITY OF EMITTED NEUTRONS NOT KNOWN STOP YET RESULT HAS BEARING ON OUR PROBLEM STOP IF WE WISH TO PREVENT PUBLICATION WOULD HAVE TO ACT FAST THROUGH ADMINISTRATION AND EVEN SO SUCCESS DOUBTFUL STOP DR. WIGNER IN TOWN TUESDAY AND DR. TELLER WEDNESDAY

6.

SZILARD

Bk.f.2 (35)

Sent Union Western Day an 23rd.

D

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

February 22nd, 1939 mistake of date correct 23rd

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

Dear Mr. Strauss:

This is just a short bulletin to complete the information contained in my wire from Sunday night.

There is a ~~very~~ fairly good chance now that, if we can concentrate the uranium isotope 235 from uranium, a chain reaction could be set up in the concentrate. I am therefore beginning to give attention to processes which could be used for concentrating this isotope, and this does not seem to be difficult to achieve with modern methods.

That the native uranium is not suitable for chain reactions is now believed for theoretical reasons by a number of physicists. Fermi, for instance, is convinced that the native uranium is no good. Wigner on the other hand, while admitting the validity of the theoretical arguments, feels that the foundations of this theory are too insecure to trust these conclusions. My own feeling is somewhere between Wigner's and Fermi's.

I am enclosing a clipping from yesterday's World Telegram. You will notice the great change of tone in comparison

6

with the clippings I sent you to Brandy Rock Farm. There will probably be a fresh outburst soon when the facts contained in my telegram become known.

It seems we will have to act very quickly if we want to act at all.

By the way, Dr. Wigner from Princeton will be here on Friday and Saturday for a meeting of the Physical Society, and it would be very nice if you could meet him. I assume that you are rather busy just now, and I fear that both Wigner and I will be pretty busy during the meeting, but perhaps something can be arranged nevertheless.

The situation has changed in many important respects since our talk in the train, and our plans would have to be changed accordingly, so perhaps we ought to talk about these things soon if your time permits.

With best wishes,

Yours very sincerely,

Bk. f. 2. (37)

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

February 28th, 1939

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

Dear Mr. Strauss:

I did not telephone you yesterday because I felt that you might want "to sleep it over" before formulating any plans for obtaining the necessary funds. I should, however, very much like to hear from you to-morrow, personally or over the telephone, about the present trend of your ideas.

One reason for this urgency is that I am going to see W. T. Richards to-morrow, Wednesday, night. He had offered on Sunday to co-operate in obtaining the necessary funds at short notice if his help were required. He is a brother-in-law of Conant's and has many connections in Boston. By the way, he is a personal friend of Loomis', and you met him at Tuxedo Park. It was understood, however, at our last conversation that Loomis would have to be approached through you, if he is to be approached at all. It seems advisable that I should inform Richards to-morrow of our present plans with respect of obtaining the funds.

Please excuse this undignified haste.

Yours very sincerely,

(Leo Szilard)

P.S. I am signing an agreement of lease to-morrow for one gram of rádium, and so you can reach me between 2 and 3 p.m. over the telephone at the office of the Radium Chemical Co., Vanderbilt 3-5177, where I shall be with Mr. Kearney. In the morning I shall probably be at my hotel, University 4-2700, up to 11.30 a.m.

Bk. f. 2

3

Deliver at 830 AM

SERVICE DESIRED			
BTIC		CABLE	
GRAM		FULL RATE	
Y LETTER		DEFERRED	
NIGHT MESSAGE		NIGHT LETTER	
NIGHT LETTER		SHIP RADIOGRAM	

Patrons should check class of service desired; otherwise message will be transmitted as a full-rate communication.

WESTERN UNION

1207-A

CHECK

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TIME FILED

R. B. WHITE, PRESIDENT — NEWCOMB CARLTON, CHAIRMAN OF THE BOARD — J. C. WILLEVER, FIRST VICE-PRESIDENT

Send the following message, subject to the terms on back hereof, which are hereby agreed to

To Lewis L. Strauss

Patent pending

Street and No. 25E 76 STR

Place New York City

PERFORMED TODAY PROPOSED EXPERIMENT WITH BERYLLIUM BLOCK WITH STRIKING RESULTS STOP VERY LARGE NEUTRON EMISSION ~~FROM HOW~~ FOUND STOP ESTIMATE PROBABILITY OF REACTION NOW ABOVE 50 PERCENT STOP RICHARDS SAYS YOU MIGHT WRITE LOOMIS CARE OF THORNE LOOMIS & CO N.Y.C. AND ~~support to~~ LOOMIS ~~to talk to R. over~~ ~~RIGHT~~ TELEPHONE ~~HERE FROM~~ BOSTON IN ORDER TO AVOID delay

Sender's address for reference Loomis

WESTERN UNION GIFT ORDERS SOLVE THE PERPLEXING QUESTION OF WHAT TO GIVE.

Sender's telephone number

6-1-116

(BK. 1.2)

(Early March? 1939,
Probably March 3 or 4, 1939 K.W. Dec. 1967)
(2)

LEWIS L. STRAUSS
25 EAST 76TH STREET
NEW YORK CITY

PERFORMED TODAY PROPOSED EXPERIMENT WITH BERYLLIUM BLOCK WITH STRIKING RESULT STOP VERY LARGE NEUTRON EMISSION FOUND STOP ESTIMATE PROBABILITY OF REACTION NOW ABOVE 50 PERCENT STOP RICHARDS SAYS YOU MIGHT WRITE LOOMIS CARE OF THORNE LOOMIS AND CO. NEW YORK CITY AND SUGGEST TO LOOMIS TO TALK TO RICHARDS OVER TELEPHONE IF LOOMIS IS IN BOSTON IN ORDER TO AVOID DELAY

SZILARD

6-1-117

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

April 11th, 1939

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

Dear Mr. Strauss:-

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

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

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

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

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

So far publication of the papers, which were sent to the Physical Review on March 16th (1939), is being delayed at our request, and efforts are made to get similar action in England and France. In the mean time a paper by Joliot appeared in Nature, which relates to our subject, but so far it did not attract much attention. Now we are trying to get Joliot to co-operate, but I do not know whether we will succeed.

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

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

With best wishes,

yours sincerely,

(Leo Szilard)

Bk. f. 3 (46)

April 14th, 1939

Dear Mr. Strauss:-

After an exchange of cables with Joliot in Paris and Blackett in England the Physics Department at Columbia University decided to publish our papers which were sent to Physical Review some time ago. This decision which runs contrary to my personal wishes was largely based on Joliot's unwillingness to delay his papers in connection with his view that the situation has already got out of hand.

I am enclosing a manuscript of the paper which will appear in the next issue of Physics Review.

With best wishes,

yours sincerely,

(Leo Szilard)

(Bk. f. 2)

LEWIS L. STRAUSS
52 WILLIAM STREET
NEW YORK

April
Fourteenth
1 9 3 9

Dear Dr. Szilard,

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

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

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

Faithfully yours,

Lewis Strauss

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

MG

Bk. f. 3

LEWIS L. STRAUSS
52 WILLIAM STREET
NEW YORK

April
Seventeenth
1 9 3 9

Dear Dr. Szilard,

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

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

Faithfully yours,

Lewis Strauss

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

MG

Bk f. 2 (16)

(3)

LEWIS L. STRAUSS
52 WILLIAMS STREET
NEW YORK CITY, NEW YORK

JUNE 14, 1939

YESTERDAY FERMI AND I HAVE COMPLETED SEMILARGE SCALE EXPERIMENT WITH POSITIVE RESULT STOP LARGE SCALE DEMONSTRATION IS NOW NEXT IMMEDIATE STEP WOULD LIKE TO SEE YOU SOON IF CONVENIENT AT BREAKFAST TIME OR AFTER DINNER

Patent priority

Bk f. 2 (15) 4

1207-A

CLASS OF SERVICE DESIRED

DOMESTIC		CABLE	
TELEGRAM		FULL RATE	
DAY LETTER		DEFERRED	
NIGHT MESSAGE		NIGHT LETTER	
NIGHT LETTER		SHIP RADIOGRAM	

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WESTERN UNION

R. B. WHITE, PRESIDENT
NEWCOMB CARLTON, CHAIRMAN OF THE BOARD
J. C. WILLEVER, FIRST VICE-PRESIDENT

CHECK

ACCT'G INFMN.

TIME FILED

Send the following message, subject to the terms on back hereof, which are hereby agreed to

June 14 K 1939

To LEWIS L. STRAUSS

Street and No. 52 WILLIAMSTREET

Yesterday Place N.Y.C.

Fermi and I have completed ~~semi~~ large scale experiment with positive result ~~large scale demonstration~~ is now next immediate step would like to see you soon if convenient at breakfast time or after dinner

imed

Sender's address for reference

WESTERN UNION MESSENGERS ARE AVAILABLE FOR THE DELIVERY OF NOTES AND PACKAGES.

Sender's telephone number

Bk f. 2. (8)

LEWIS L. STRAUSS
52 WILLIAM STREET
NEW YORK

June
Sixteenth
1 9 3 9

Dear Dr. Szilard,

Mr. Strauss has asked me to acknowledge your telegram and to say that he will be glad to see you at any time on Monday. He has moved to the country for the summer.

Sincerely yours,

William H. Mulgueen

Assistant to Mr. Strauss.

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

MG

6.

Bk.f.2 (42)

July 3rd, 1939

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

Dear Mr. Strauss:

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

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

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

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

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

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

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

Belge.

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

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

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

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

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

I am, yours sincerely,

(Leo Szilard)

2nd set
May, 1969

Additional Material for page 108 (1)

re: PROCURING BERYLLIUM BLOCK

The beryllium block was originally made for Szilard in Germany and sent to him in England.

See the 1934 letters to Lange and Brasch, with page 101 material.

Letter, S. Krewe to L.S. (re beryllium block?) Jan 19, 1939

Telegram, Lindemann to L.S. Undated, probably late January, 1939

Telegram, Lindemann to L.S. Undated, probably Feb. 3, 1939

Receipt to L.S. from Post Office, New York, Foreign Section. Feb. 18, 1939
15¢ Customs clearance and delivery charge, probably for beryllium block.

Bk. f. 1 (8)

19. I. 39. 87-38 168th St. Jamaica

Lieber Herr Dr. Spilard!

Anbei Slip des Postamts. Am Montag im Laufe des Tages wird sich Ihr Paket bestimmt in den U.S. Appraiserstores 201 Varick St. (7th Ave Subway; stop at West Houston Street) befinden (downtown). Normalerweise müssten Sie auf Nachricht von obiger Behörde warten. Sie können aber auf eigenes Risiko schon vorher Ihr Glück versuchen, was der Beamte im Postamt vorschlug, als ich sagte, es sei eilig.

Yours truly S. Krewer

[illegible]

6.

[illegible]

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WESTERN UNION

1201

SYMBOLS

DL = Day Letter
NL = Night Letter
LC = Deferred Cable
NLT = Cable Night Letter
Ship Radiogram

BK / No. 1. (6)

R. B. WHITE, PRESIDENT
NEWCOMB CARLTON, CHAIRMAN OF THE BOARD
J. C. WILLEVER, FIRST VICE-PRESIDENT

The filing time shown in the date line on telegrams and day letters is STANDARD TIME at point of origin. Time of receipt is STANDARD TIME at point of destination

Received at

B298CC 5L VIA RCA

Dec 20, 1939

CD OXFORD 20 1

NLT SZILARD CARE LIEBOWITZ

420 RIVERSIDE DRIVE NEWYORKCITY

FROM HOME WAS BERYLLIUM BORROWED DO THEY AUTHORISE IT LEAVING ENGLAND

LINDEMANN

publ. priority

6.

Backfile Vol. 1
(7)

CLASS OF SERVICE

This is a full-rate Telegram or Cablegram unless its deferred character is indicated by a suitable symbol above or preceding the address.

WESTERN UNION

1201

SYMBOLS

DL = Day Letter
NL = Night Letter
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Ship Radiogram

R. B. WHITE, PRESIDENT
NEWCOMB CARLTON, CHAIRMAN OF THE BOARD
J. C. WILLEVER, FIRST VICE-PRESIDENT

The filing time shown in the date line on telegrams and day letters is STANDARD TIME at point of origin. Time of receipt is STANDARD TIME at point of destination

Received at

Feb 3, 1939

C182CC 9M VIA RCA

CD OXFORD 15 3

NLT SZILARD, CARE LIEBOWITZ

420 RIVERSIDE DR NYK QR

BERYLLIUM SENT FEBRUARY 3RD REGISTERED POST

LINDEMANN

Duplicate returned

THE COMPANY WILL APPRECIATE SUGGESTIONS FROM ITS PATRONS CONCERNING ITS SERVICE

6.

(M 27)

U.S. Appraiser Stores
201 Varick Street
7th Sub. West Houston ST.

POST OFFICE, NEW YORK, N.Y.
DIVISION OF MAILS
FOREIGN SECTION, MORGAN ANNEX

NO. V1468   DEC 18 1939 193

Received from Dr. Leo Szilard c/o Liebowitz

—— Dollars 15 cents in payment of ~~storage charges~~ and Customs Clearance and delivery charges 1 parcels held at Customs Department, Foreign Section (5th Floor), Morgan Annex.

Due stamps affixed by

____
Clerk

ALBERT GOLDMAN,
Postmaster.

per______________________

D-575
10/20/35

2nd set
May, 1969

Additional Material for page 108 (2)

re: PROCURING RADIUM, and also ADDITIONAL BERYLLIUM

Letter, H.B. Kearney to L.S. Feb. 14, 1939
From the Sales Manager, Radium Chemical Co.

Letter, Arno Brasch to L.S. March 6, 1939
Szilard mentioned in his letter to Wigner of April 30, 1941 (with page 102 material) that he had given an exclusive license agreement for the chemical separation portion of his British patent. Brasch was the nephew of the licensee, and knew some but not all of the story.

Letter, L.S. to Merle Tuve March 22, 1939

Letter, L.S. to Merle Tuve March 24, 1939
Offers to get a second beryllium block from a friend in England (Polany).

Letter, Tuve to L.S. March 27, 1939
Declines beryllium block for then.
Bush, then President of Carnegie Institution, wanted to "stay clear of the Navy."

Letter, Wigner to L.S. April 17, 1939
In re, second beryllium block, see 2nd, ~~3rd and~~ to 4th paragraphs.
The first paragraph is the more important one, and Szilard had this ~~one~~ copied. It is included with the SECRECY, page 109-110, material; also with the page 113 material.

Letter, Teller to L.S.
Undated, and in Hungarian, but probably fits with this material (KW)

Invoice, Radium Chemical Co. to L.S. Feb. 21, 1940
Note that the invoice, for insurance on radium rented March 2nd, 1939, is made out personally to Szilard. (The decisive experiment with Zinn was done March 3rd, 1939).

Letter, L.S. to George H. Loftus of Radium Chemical Co. Sept. 29, 1941
Szilard encloses a check, in payment.

CHICAGO OFFICE
MARSHALL FIELD ANNEX BUILDING

RADIUM CHEMICAL COMPANY INC.

1 EAST 42ND STREET

NEW YORK

RADIUM SALES DEPARTMENT
RADIUM LEASING DEPARTMENT

February 14, 1939

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

Dear Dr. Szilard:

Referring to our negotiations thus far for rental of approximately one gram of radium for physical experiments with gamma rays:

Before consummating the lease, will you be kind enough to have Dr. Wigner, or some other member of the faculty at Princeton University, state that some of the experiments will actually be conducted under the direction of the Physics Department of the University.

Our Board feels that we should have some such assurance, particularly to satisfy the insurance underwriters, of your own reliability and that the radium will have proper safeguards while out of our possession.

Yours very truly,

H B Kearney

Sales Manager

HBK:TD

6.

Athenaeum
551 South Hill Avenue
Pasadena, California

March .6. 39.

Lieber Szilard, falls Sie metallisches Uran brauchen sollten,so weiss ich, dass die Westinghouse eine ganze Menge davon besitzt und es Ihnen eventuell zur Verfuegung stellen wuerde.Man sollte es doch wenigstens versuchen,und wahrscheinlich ist die Stelle,die darueber verfuegt, in Bloomfield.Wir kennen ja die Leute.

Die Westinghouse hat vor laengerer Zeit einmal versucht Uran an Stelle von Wolfram fuer Roentgen antikathoden zu benutzen,daher die Vorraete.Falls Ihnen also mit diesem Hinweis gedient sein sollte.

Glauben Sie nicht,dass bei der Uranzertruemmerung eventuell noch ein ganz anderer Vorgang mitspielen koennte und es sich dabei garnicht um die normalen Neutronen handelt,sondern vielleicht um eine spezielle Strahlung,die beim Aufprall von Neutronen auf Uran entstehen.Vielleicht von sehr kurzer oder sehr langer Reichweite ?Eine Art Doppelneutronen.oder sonst etwas.

Mit vielen Gruessen

Ethel Browne

6.

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

March 22nd, 1939

Dr. M. Tuve
Carnegie Institute of Terrestrial Magnetism
5241 Broad Branch Road
Washington, D.C.

Dear Tuve:-

I told Fermi of the conversation which we had in Washington. I also raised the question whether he would see much advantage in your using some private funds, which I believe we could find for you if necessary, instead of the Navy funds. Since the Navy funds have already been offered to you Fermi seems to think that you might as well use those for the present. He asked me to send you a copy of his paper which you will find enclosed for your information as well as for Hafstaadt's and Heydenburg's.

I have telephoned to the Radium Chemical Co. and asked them if we could have another gram of radium for a minimum period of one month to be used by your group at Washington. Their reaction was so far favorable, and they cabled at my request to Brussels in order to ask for a permission. I expect a telephone call from them in the next few days and will let you know as soon as I hear from them.

I have also telephoned to the Beryllium Corporation, but so far I did not succeed in getting hold of anybody to whom I could talk sense. I shall try again to-morrow.

I shall keep you informed as to how things develop. At present we are trying to find out the best conditions for the "chemical" experiment.

If you want uranium oxide you can get it in ten pound packages from $ 2.65 to $ 2.70; uranium of Belgian origin from B.F. Drakenfeld, 45 Park Place, New York City, and uranium of Canadian origin from Du Pont de Nemours, The R. and H. Chemical Department, Empire State Building, New York City.

Following your suggestion that rare earth contamination might be important, we are going to test different samples for such contaminations and shall write you the results.

With kind regards to all,

Yours sincerely,

(Leo Szilard)

P.S. Perhaps before you definitely commit yourself to rent one gram of radium you might wish to reconsider whether you cannot do the same experiments equally well with deuteron-carbon neutrons. Maybe we can have another talk on this subject in a fortnight or so. By then it will be possible to see clearly what experiments are most urgently needed to complete the picture.

UNDER
KNOTT MANAGEMENT

TELEPHONE
UNIVERSITY 4-2700

King's Crown Hotel

420 WEST 116TH STREET
NEW YORK
OPPOSITE COLUMBIA UNIVERSITY

March 24th, 1939

Dear Tuve:-

The Beryllium Corporation, whith whom I established contacts at last, can only supply beryllium flakes. This is very unsatisfactory. They said they could get from Germany beryllium plates about 1" by 1" in about four to six weeks. These plates are 1/8th of an inch high, and they charge $ 13.50 for each. This seems to me hardly worth while.

I could perhaps get a beryllium block which is similar to mine and which belongs to a friend of mine in England. I could try and cable for it if you wish me to do so.

With kind regards to all,

Yours sincerely,

(Leo Szilard)

6.

565 7

CARNEGIE INSTITUTION OF WASHINGTON

DEPARTMENT OF TERRESTRIAL MAGNETISM

5241 BROAD BRANCH ROAD, N. W.
WASHINGTON, D. C.

Joliots' expt 6×10^{-25} hardly agrees with his interpretation $\nu > 1$ in for fission. 2.

Monday 3/27/39

Dear Szilard

Thanks for your two letts, & MS of Fermi's expts.

We are in the midst of an expt to demonstrate simultaneous neutrons > 500 kv similar to your photo-n expt, but using C-neutrons. This type of expt gives very little of quantitative value, but after 2 or 3 more days I will stop it & run up to NY to talk to you & Fermi.

Do not cable for Be block as yet. Thanks.

President Bush (C/W) insisted that we stay clear of the Navy; we can have what funds we need, after we decide what really is worth doing. At present I think the Fermi expt with large (200 lbs) amounts of UO_2 is the only good expt. We could use our water tank 8 ft diam by 8 ft deep, and with ample neutrons, 100 to 1000 curies (new machine) make a real measure of area under the "tail". Before making any moves or purchases we will confer with you & Fermi.

Please advise Fermi!

Regards — Tuve

BK, f. 2.

April 17, 1939

Leo Szilard:

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

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

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

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

Hoping to hear from you very soon,

Sincerely

Wigner

Teller letters in Red 6.I: Dating?

(1) in add'l material pr page 108(2)

Comes from Box folder 2: is preceded by July 1939 material, followed by March + April 1939 material

Translation

Dear Szilard!

To-day I was out at Tuve's. Just then a letter was being prepared to the Physical Review with the following contents: After bombardment of Uranium, the number of neutrons emerging (? reseeling ?) at periods of 10 minutes is approximately half of that of the fission processes (?). This, and other experiments, show that these neutrons are not brought forth by γ-rays. Those 10-minute neutrons result also from bombardment of Thorium with the same relative yield. Thus, one may take it for certain that the neutrons are derived from a "splitter" (splitting).

As soon as I began taking interest in Uranium sharp discussion started on the practical significance. Tuve, Hafstad and Roberts are entirely aware of what is involved. They know also of Fermi's experiments. Of course, I didn't say anything. The abovementioned letter cannot cause any harm. They do not plan any further experiments for lack of larger amounts of Uranium. They are particularly interested in the separation of U_{238} and U_{235} as, according to Bohr, only the latter produces fission.

I do not know their detailed plans, but I believe that urgent action is required. Very many people have discovered already what is involved. Those in Washington would like to persuade the Carnegie - Institute that it should provide more money for U-research in view of the practical significance of the matter. This is perhaps not a bad idea because the Carnegie people are of good will and are cautious (perhaps a little too cautious). But this has no immediate actuality unless the leadership (?) becomes more interested than until now (I believe this would be desirable), due to Fermi's visit or in connection with some other occurrence.

I repeat that there is a chain-reaction mood in Washington. I only had to say "uranium" and could then listen for two hours to their thoughts. I would be glad to hear further, and to help if possible

Teller

negatív

probe
mon

EDWARD TELLER
2610 GARFIELD STREET
WASHINGTON, D. C.

Kedves Szilárd!

Ma kinn voltam Tuve-éknál. Épen levél készült a Phys. Rev.-nek a következő tartalommal: Az Urán-bombázás után 10-perces periódussal föllépő neutronok száma körülbelül fél-akkora mint a fission-processzusok száma. Ez és más kísérletek mutatják hogy ezen neutronokat nem γ-sugárzás idézi elő. A 10-perces neutronok Thorium bombázásnál is föllépnek (kb. azonos relatív intenzitás-viszonyban). Tehát biztosra vehető hogy a neutronok egy "splitter"-ből erednek.

Mihelyt csak elkezdtem érdeklődni Urán iránt mindjárt heves diskusszió indult meg a gyakorlati jelentőségről. Tuve Hafstad és Roberts teljesen tisztában vannak hogy miről van szó. A Fermi kísérletekről is tudtak. Én persze nem mondtam semmit. A föntemlített levél bajt

Bkf.2(21)

nem okozhat. További kísérletek nagyobb mennyiségű Urán hiánya miatt nem folynak. Különösen érdeklődnek U_{238} és U_{235} elválasztása iránt mivel Bohr szerint csak az utóbbi ad fissiont.

Nem tudom a részletes terveiket de úgy látom, hogy sürgős akció szükséges. ~~A~~ Nagyon sok ember rájött már, hogy miről van szó. A Washingtoniak szeretnék Carnegie-intitutot ~~rá~~ rábeszélni hogy a dolog gyakorlati jelentőségére való tekintettel több pénzt adjanak U-research-re. Ez talán nem rossz idea mert a Carnegie-ék jóakaratúak és óvatosak (talán kicsit túl óvatosak) De ez egyelőre nem közvetlenül aktuális, hacsak a vezetőség Fermi sürgetése vagy más esemény hatására nem kap nagyobb kedvet mint eddig (Azt hiszem ez kívánatos volna)

Ismétlem hogy Washingtonban chain-reaction hangulat van. Csak azt kellett mondanom, hogy "Urán" és aztán két órát hallgathattam, hogy ők mit gondolnak. Örülnék továbbiakról hallani és ha lehet segíteni.

Teller

M-1.6

RADIUM CHEMICAL COMPANY, INC.

570 LEXINGTON AVENUE
~~1 EAST 42ND STREET~~

NEW YORK

SOLD TO

Dr. Leo Szilard
Kings Crown Hotel
420 E. 116th St.
New York.

DEBIT MEMORANDUM 119-40
~~INVOICE NO.~~

DATE Feb. 21, 1940

OUR ORDER No.

YOUR ORDER No.

TERMS

SHIPPED VIA

ON

DESCRIPTION	PRICE	AMOUNT
Insurance premium for one year from March 2nd, 1939 to March 2nd, 1940 on 906.24 milligrams of Radium element contained in platinum-iridium tubes Nos. 36, 37, 42, 43, 51, 55, 58, 60, 63, 65. 67, 71, 72, 73, 75, 76, 79 and 80, and including the platinum tubes -	$161.11	
Less payment made on Debit Memorandum 118-39, of March 6th, 1939 covering three months' period from March 2nd, 1939 to June 2, 1939 -	40.28	
		$ 120.83

Form M 6

M-16

September 29, 1941

Mr. George H. Loftus, Vice President
Radium Chemical Company, Inc.
570 Lexington Avenue
New York City

Dear Mr. Loftus:

I acknowledge the receipt of your letter of September 10th, and you will find enclosed a check for $120.83 in settlement of your bill covering insurance premium for radium which you had rented to me in 1939.

Yours sincerely,

(Leo Szilard)

LS:MEB

Enclosure

Copies
1 Pegram
1 Fermi
1 Szilard
2 Mitchell

Sect 6-I

Nov. 1970
2nd set

for pages 108-109.

SECRECY. Spring, 1939 (2)

Letter, L.S. to Joliot July 5, 1939

July 5th, 1939

Professor F. Joliot
Collège de France
Paris

Dear Professor Joliot:

I thank you for your letter of April 19th and the enclosed manuscript. Please excuse my delay in acknowledging its receipt. I personally regret very much that it was not possible to arrange for a universally accepted policy with regard to publications. As far as I can see, the really dangerous questions have so far not been raised, and maybe it will be possible later, if the necessity arises, to halt certain types of publications and so to establish a sort of second line of defense. I may write to you again about this at some later date.

Meanwhile I am, with kind regards to all,

yours very sincerely,

2nd set
May, 1969

Additional Material for pages 108-109

re: SECRECY, SPRING 1939 (1)

On November 4th and 12th, 1942, while already at the Metallurgical Laboratory in Chicago, Szilard wrote a memorandum to the director, A.H. Compton, emphasizing his attempts to keep the work secret. We include here the introduction, table of contents, and the 1939 portions of this memorandum.

Memorandum, L.S. to A.H. Compton	Nov. 4, 12, 1942
"First Approach to France, Febr. 1939"	COMPTON MEMO
Letter, L.S. to Joliot	Feb. 2, 1939

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Letters Joliot to L.S., L.S.	Nov. 26, 1937

These letters shows that Szilard's relationship with Joliot was already well-established and friendly, before 1939

Letter, L.S. to Joliot	Dec 24, 1937
"Agreement about Secrecy, March 1939"	COMPTON MEMO
"First Approach to the Navy through Fermi, March 1939	" "
Telegram, Wigner to L.S.	March 16, 1939
Telegram, Cockcroft to Pegram re Joliot's discovery.	March 23, 1939

X 665 49

November 12, 1942

A. H. Compton

L. Szilard

I am enclosing a collection of documents which you might find useful, if it is desired to make a more thorough investigation of my background in connection with the work of our project. This collection of documents does not give the full history and was selected rather from the point of view of describing the attempts which we made to keep our work secret.

Any investigation, if limited to this country, could go back for 10 years, since I first came to this country in 1931. Although I did not reside here without interruption for more than 5 or 6 years. The following is a list of persons who have had some knowledge of the background of our present work and who had known me for a long period of time:

E. P. Wigner, Metallurgical Laboratory, University of Chicago. (He has known me intimately for 20 years).

Dr. B. Leibowitz, president of the Trubanizing Process Corp., Empire State Bldg., N. Y. Dr. Leibowitz has known me intimately since 1932. I approached him in February 1939 and told him of the importance of the discoveries concerning uranium for national defense. It was through his generosity that Fermi and I had radium available for our experiments in 1939.

Professor Albert Einstein, Princeton. He has known me for 20 years, and I was in close contact with him between 1922 and 1932 in Germany.

Professor Edward Teller, Metallurgical Laboratory, Univ-

ersity of Chicago, has known me intimately since 1933, and I was in close touch with him during my stay in England from 1933 to 1938.

If it should be desirable to extend the investigation to England, I should be very glad to give a list of those institutions and persons with whom I was most closely associated during my stay in England from 1933 to 1938.

L. Szilard

(Hest_I) (bbs 49)

ATTEMPTS AT SECRECY FROM MARCH 1939 TO JUNE 1940

L. Szilard

November 4, 1942

CONTENTS

Attempts At Secrecy-March 1939-June 1940

November 4, 1942

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

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

First Approach to France / Febr. 1939

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

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

(Bk. f. 3)

c/o Liebowitz
420 Riverside Drive
New York City

February 2nd, 1939

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

Dear Professor Joliot:

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

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

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

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

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

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

~~In the hope that there will not be sufficient neutrons emitted by uranium, I am,~~

Yours sincerely,
(Leo Szilard)

COLLÈGE DE FRANCE

Laboratoire de Chimie Nucléaire

Place Marcellin-Berthelot
PARIS (V^e^)

Téléph. : ODÉON 81-60

Paris, le 26 novembre 1937

Cher Monsieur Szilard

L'installation du Laboratoire de Synthèse Atomique qui conviendrait pour l'expérience que vous envisagez est actuellement en train d'être remaniée : nous avons démonté l'ancien tube qui permettait d'atteindre 1.950.000 volts et nous installons un nouveau tube qui nous permettra, espérons-le, d'arriver à 2.500.000 volts. Dans quelques semaines la nouvelle installation sera utilisable.

Je serais heureux de vous donner à cette époque la possibilité d'exécuter cette expérience qui s'annonce très intéressante. Bien entendu si vous venez à Paris dès la fin de ce mois je serai très content d'en discuter et d'avoir avec vous une conversation à ce sujet.

Sincères salutations.

F.JOLIOT

Monsieur Léo SZILARD, c/o Clarendon Laboratory, Parks Road, Oxford.

neutrons from Radium-Be (1½ gm Ra, 250 gm Be) do not excite the 4½ period and it would be very valuable to have that combined with your very much stronger X ray - Be neutron source

Cunard White Star
"Franconia"

Dec 24th-37

Dear Professor Joliot,

I am going to sail earlier than expected and regret very much not to see you before going abroad. - Enclosed you will find an indium foil, so that lack of indium shall not prevent an experiment which you would otherwise like to try. Goldhaber will, I expect, give you all information. There is one experiment which, though carefully done at Oxford, ought to be repeated in Paris: Photo-

(1)

Agreement about Secrecy

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

First Approach to the United States Government through Fermi

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

Bk.f. 3 (2)

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COPY OF WESTERN UNION TELEGRAM

Received 3:20 P. M.
March 23, 1939

PEGRAM

Joliot measured neutron production by Amaldi Fermi method using ammonium nitrate and uranyl nitrate solutions Evidence of neutron production from increase of area under curve cross section greater than six ten minus twenty five Conclude more than one neutron provided for each neutron absorbed

Cockcroft