REMINISCENCES*

by LEO SZILARD

edited by Gertrud Weiss Szilard and Kathleen R. Winsor

[EDITORS' NOTE: Leo Szilard at various times considered writing his own biography, but he never did. He had a sense of history, however, and carefully preserved, in folders marked "History," all correspondence and other documents which he thought to be of historical significance. In 1951, when he seriously contemplated writing the history of the Manhattan Project, he organized the pertinent documents into ten folders, by different topics and time periods. The documents which are appended here come largely from this collection which Szilard selected himself. He also drafted an outline for his memoirs.

During a period of serious illness in 1960, which kept him in the hospital for a year, he used a tape recorder—which had been put into his sick room for the purpose of an oral history project—to dictate instead the first draft of *The Voice of the Dolphins and Other Stories* (New York, 1961), a whimsical history of the future twenty-five years, which seemed vastly more important to him than the history of the past quarter century.

However, at times he enjoyed giving interviews to interested visitors. On a few such occasions his wife switched on his tape recorder. What follows is an exact transcription of parts of these tapes, with editing limited to the minimum necessary to change spoken to written English.

These highly personal, pungent, and incisive comments by a leading participant in three great episodes in recent American history—the migration of intellectuals from Hitler's Europe to America; the development of a nuclear chain reaction; and the effort to prevent the use of atomic bombs and to establish civilian control of atomic energy—are published here by courtesy of Mrs. Szilard and with the cooperation of the M.I.T. Press, which will include them in a forthcoming edition of Szilard's scientific and other writings.

The selection and editing has been a collaborative effort of Gertrud Weiss Szilard and Kathleen R. Winsor, with the help of Ruth Grodzins for part of the manuscript. The annotations were prepared by Kathleen R. Winsor. Unpublished papers referred to in the notes are in the possession of Mrs. Szilard. Although many others helped and advised in the project, Mrs. Szilard wishes particularly to thank Mr. Melvin Voigt, University Librarian, and his staff, who made available space and other facilities of the Library of the University of California, San Diego, to gather, store, and process the Szilard papers.]

I REACHED the conclusion something would go wrong in Germany very early. I reached this conclusion in 1930, and the occasion was a meeting in Paris. It was a meeting of economists who were called together to decide whether Germany could pay reparations, and just how much she could pay. One of the participants of that meeting was Dr. Hjalmar Schacht, who was at that time, I think, president of the German Reichsbank. To the surprise of the world, including myself, he took the position that Germany could not pay any reparations unless she got back her former colonies. This was such a frightening statement to make that it caught my attention, and I concluded that if Hjalmar Schacht believed that he could get away with it, things must look rather bad. I was so impressed by this that I wrote a letter to my bank and transferred every single penny I had out of Germany into Switzerland. I was not the only one, as I later learned. Within a few months after this speech of Schacht's, a very large sum of money, mainly by depositors from abroad, was drawn out of Germany. Apparently there are many people who are sensitive to this kind of signal.

I visited America in 1931. I came here on Christmas Day 1931, on the *Leviathan*, and stayed here for about three months [until May 4, 1932]. In the course of 1932 I returned to Berlin where I was privat-dozent at the University. Hitler came into office in January '33, and I had no doubt what would happen. I lived in the faculty club of the Kaiser Wilhelm Institute in Berlin-Dahlem and I had my suitcases packed. By this I mean that I literally had two suitcases which were packed standing in my room; the key was in them, and all I had to do was turn the key and leave when things got too bad. I was there when the *Reichstagsbrand* occurred, and I remember how difficult it was for people there to understand what was going on. A friend of mine, Michael Polanyi, who was director of a division of the Kaiser Wilhelm Institute for Physical Chemistry, like many other people, took a very optimistic view of the situation. They all thought that civilized Germans would not stand for anything really rough happening. The reason that I took the opposite position was based on observations of rather small and insignificant things. I noticed that the Germans always took a utilitarian point of view. They asked, "Well, suppose I would oppose this, what good would I do? I wouldn't do very much good, I would just lose my influence. Then why should I oppose it?" You see, the moral point of view was completely absent, or very weak, and every consideration was simply, what would be the predictable consequence of my action. And on that basis did I reach the conclusion in 1931 that Hitler would get into power, not because the forces of the Nazi revolution were so strong, but rather because I thought that there would be no resistance whatsoever.

After the Reichstag fire [February 27, 1933], I went to see my friend Michael Polanyi and told him what had happened, and he looked at me and said, "Do you really mean to say that you think that the secretary of the interior had anything to do with this?" and I said, "Yes, that is precisely what I mean," and he just looked at me with incredulous eyes. At that time he had an offer to go to England and to accept a professorship in Manchester. I very strongly urged him to take this, but he said that if he now went to Manchester, he could not be productive for at least another year, because it takes that much time to install a laboratory, and I said to him, "Well, how long do you think you will remain productive if you *stay* in Berlin?" We couldn't get together on this so I finally told him that if he must refuse this offer he should do so on the ground that his wife was opposed to it, because his wife always could change her mind, so that if he wanted to have the thing reconsidered, he would have an out. Later on when I was in England, in the middle of '33, I was active in a committee, this one was a Jewish committee incidentally, where they were concerned about finding positions for refugees from Germany. Professor Namier[1] came from Manchester and reported that Polanyi was now again interested in accepting a professorship in Manchester. He said that previously he had refused the offer extended to him on the grounds that he was suffering from rheumatism, but it appears that Hitler cured his rheumatism.

I left Germany a few days after the Reichstag fire. How quickly things move you can see from this: I took a train from Berlin to Vienna on a certain date, close to the first of April, 1933. The train was empty. The same train, on the next day, was overcrowded, was stopped at the frontier, the people had to get out and everybody was interrogated by the Nazis. This just goes to show that if you want to suceed in this

1. Sir Lewis Bernstein Namier, professor of modern history at the University of Manchester from 1931 to 1953.

world you don't have to be much cleverer than other people, you just have to be one day earlier than most people. This is all that it takes.

While I was in Vienna the first people were dismissed from German universities, just two or three; it was however quite clear what would happen. I met, by pure chance, walking in the street a colleague of mine, Dr. Jacob Marschak, who was an economist at Heidelberg and who is now [1960] a professor at Yale. He also was rather sensitive; not being a German, but coming from Russia he had seen revolutions and upheavals, and he went to Vienna where he had relatives because he wanted to see what was going to happen in Germany. I told him that I thought since we were out here we may as well make up our minds what needed to be done and take up this lot of scholars and scientists who will have to leave Germany and the German universities. He said that he knew a rather wealthy economist in Vienna who might have some advice to give. His name was Schlesinger and he had a very beautiful apartment in the Liechtensteinpalais. We went to see him and he said, "Yes, it is quite possible that there will be wholesale dismissals from German universities; why don't we go and discuss this with Professor Jastrow." Professor Jastrow[2] was an economist mainly interested in the history of prices, and we went to see him—the three of us now—and Jastrow said, "Yes, yes, this is something one should seriously consider," and then he said, "You know, Sir William Beveridge is at present in Vienna. He came here to work with me on the history of prices, and perhaps we ought to talk to him." So I said, "Where is he staying?" and he said, "He's staying at the Hotel Regina." It so happened that I was staying at the Hotel Regina, so I volunteered to look up Sir William Beveridge and try to get him interested in this.

I saw Beveridge and he immediately said that at the London School of Economics he had already heard about dismissals, and he was already taking steps to take on one of those dismissed, that he was all in favor of doing something in England to receive those who have to leave German universities. So I phoned Schlesinger and suggested that he invite Beveridge to dinner. Schlesinger said no, he wouldn't invite him to dinner because Englishmen, if you invite them to dinner, get very con-

2. Ignaz Jastrow, German economist, historian and sociologist, professor of political science at the University of Berlin.

ceited. However, he would invite him to tea. So we had tea, and in this brief get-together, Schlesinger and Marschak and Beveridge, it was agreed that Beveridge, when he got back to England, and when he got the most important things he had on the docket out of the way, would try to form a committee which would set itself the task of finding places for those who have to leave German universities. He suggested that I come to London and that I occasionally prod him on this, and that if I were to prod him long enough and frequently enough, he thought he would do it. Soon thereafter he left, and soon after he left, I left and went to London.

When I came to London I phoned Beveridge. Beveridge said that his schedule had changed and that he found that he was free and that he could take up this job at once, and this is the history of the birth of the so-called Academic Assistance Council in England. The English adopted a policy of mainly helping the younger people, but did not demand that somebody should have an established name or position in order to find a position in England, quite in contrast to American organizations. In addition to the Academic Assistance Council, there was a Jewish committee functioning. They raised funds privately and they found positions for people and provided them with fellowships for one or two years. The two committees worked very closely together, and in a comparatively short time practically everybody who came to England had a position, except me.

When I was in England, and after I no longer had to function in connection with placing the scholars and scientists who left the German universities—when this was more or less organized and there was no need for me to do anything further about that—I was thinking about what I should do, and I was strongly tempted to go into biology. I went to see A. V. Hill and told him about this. Now A. V. Hill himself had been a physicist and became a very successful biologist, and he thought it was quite a good idea. He said, "Why don't we do it this way? I'll get you a position as a demonstrator in physiology, and then twenty-four hours before you demonstrate you read up these things, and then you should have no difficulty in demonstrating them the next day. In this way, by teaching physiology, you would learn physiology and it's a good place to begin."

Now I must tell you why I did not make this switch at the time. In fact, I made the switch to biology in 1946. In 1932 while I was still in Berlin, I read a book by H. G. Wells. It was called *The World Set Free.*[3] This book was written in 1913, one year before the World War, and in it H. G. Wells describes the discovery of artificial radioactivity and puts it in the year of 1933, the year in which it actually occurred. He then proceeds to describe the liberation of atomic energy on a large scale for industrial purposes, the development of atomic bombs, and a world war which was apparently fought by allies of England, France, and perhaps including America, against Germany and Austria, the powers located in the central part of Europe. He places this war in the year 1956, and in this war the major cities of the world are all destroyed by atomic bombs. Up to this point the book is exceedingly vivid and realistic. From then on the book gets to be a little, shall I say, utopian. With the world in shambles, a conference is called in Brissago in Italy, in which a world government is set up.

This book made a very great impression on me, but I didn't regard it as anything *but* fiction. It didn't start me thinking whether or not such things could in fact happen. I had not been working in nuclear physics up to that time.

Now, this really doesn't belong here, but I will nevertheless tell you of a curious conversation which I had, also in 1932, in Berlin. The conversation was with a very interesting man named Otto Mandl, who was an Austrian, and who became a wealthy timber merchant in England, and whose main claim to fame was that he had discovered H. G. Wells at a time when none of his works had been translated into German. He went to H. G. Wells and acquired the exclusive right to publish his works in German, and this is how H. G. Wells became known on the Continent. In 1932 something went wrong with his timber business in London, and he found himself again in Berlin. I had met him previously in London and I met him again in Berlin and there ensued a memorable conversation.[4] Otto Mandl said that he not only thought,

3. *The World Set Free: A Story of Mankind* (London, 1914).

4. Otto Mandl (d. 1956) was the husband of the pianist Lili Kraus, to whom he was married in 1930. In a recent conversation, Miss Kraus told me that she remembered discussions of this kind between Szilard and her husband very well. When I showed her this portion of the tape she said, "Every word is true." [G.W.S.]

he *knew* what it would take to save mankind from a series of ever-recurring wars that could destroy it. He said that man has a heroic streak in himself. Man is not satisfied with a happy idyllic life. He has a need to fight and to encounter danger. And he concluded that what mankind must do to save itself is to launch an enterprise aimed at leaving the earth. On this start he thought the energies of mankind could be concentrated and the need for heroism could be satisfied. I remember my own reaction very well. I told him that this was somewhat new to me, and that I really didn't know whether I would agree with him. The only thing I could say was this: that if I came to the conclusion that this was what mankind needed, and if I wanted to contribute something to save mankind, then I would probably go into nuclear physics, because only through the liberation of atomic energy could we obtain the means which would enable man not only to leave the earth but to leave the solar system.

I was not thinking any more about this conversation or about H. G. Wells's book either, until I found myself in London about the time of the British Association meeting in September 1933. I read in the newspapers a speech by Lord Rutherford, who was quoted as saying that he who talks about the liberation of atomic energy on an industrial scale is talking moonshine.[5] This set me pondering as I was walking the streets of London, and I remember that I stopped for a red light at the intersection of Southampton Row. As the light changed to green and I crossed the street, it suddenly occurred to me that if we could find an element which is split by neutrons and which would emit *two* neutrons when it absorbed *one* neutron, such an element, if assembled in sufficiently large mass, could sustain a nuclear chain reaction. I didn't see at

5. A summary of the speech by Rutherford, delivered at the meeting of the British Association for the Advancement of Science, Leicester, September 11, 1933, and published in *Nature*, 132 (September 16, 1933), 432–433, contains the sentence: "One timely word of warning was issued to those who look for sources of power in atomic transmutations—such expectations are the merest moonshine." See also, A. S. Eve, *Rutherford, Being the Life & Letters of the Rt. Hon. Lord Rutherford, O.M.* (Cambridge, 1939), p. 374: "These transformations of the atom are of extraordinary interest to scientists but we cannot control atomic energy to an extent which would be of any value commercially, and I believe we are not likely ever to be able to do so. A lot of nonsense has been talked about transmutation. Our interest in the matter is purely scientific, and the experiments which are being carried out will help us to a better understanding of the structure of matter."

the moment just how one would go about finding such an element, or what experiments would be needed, but the idea never left me. Soon thereafter, when the discovery of artificial radioactivity by Joliot and Mme. Joliot was announced, I suddenly saw that tools were at hand to explore the possibility of such a chain reaction. I talked to a number of people about this. I remember that I mentioned it to G. P. Thomson[6] and to Blackett,[7] but I couldn't evoke any enthusiasm.

I had one candidate for an element which might be instable in the sense of splitting off neutrons when it disintegrates, and that was beryllium. The reason I suspected beryllium of being a potential candidate for sustaining a chain reaction was that the mass of beryllium was such that it could disintegrate into two other particles and a neutron. It was not clear why it didn't disintegrate spontaneously, since the mass was large enough to do that; but it was conceivable that it had to be tickled by a neutron which would shake the beryllium nucleus in order to trigger such a disintegration. I remember I told Blackett that we really ought to get a large mass of beryllium, large enough to be able to notice whether it could sustain a chain reaction. Beryllium was very expensive at the time, almost unobtainable, and I remember Blackett's reaction was, "Look, you will have no luck with such fantastic ideas in England. Yes, perhaps in Russia. If a Russian physicist went to the government and said, 'We must make a chain reaction,' they would give him all the money and facilities which he would need. But you won't get it in England." As it turned out later beryllium cannot sustain a chain reaction and is, in fact, stable. What was wrong was that a published mass of helium was wrong. This was later discovered by Bethe, and it was a very important discovery for all of us, because we did not know where to begin to do nuclear physics if there were an element which could disintegrate but didn't.

When I gave up the beryllium I did not give up the thought that there might be another element which could sustain a chain reaction. And in the spring of 1934 I had applied for a patent which described the laws governing such a chain reaction. It was the first time, I think, that

6. George Paget Thomson (son of J. J. Thomson), in 1933, professor of physics at University of London.

7. P. M. S. Blackett; in 1933 professor of physics at University of London.

the concept of critical mass was developed and that a chain reaction was seriously discussed. Knowing what this would mean—and I knew it because I had read H. G. Wells—I did not want this patent to become public. The only way to keep it from becoming public was to assign it to the government. So I assigned this patent to the British Admiralty.[8]

At some point I decided that the reasonable thing to do was to investigate systematically all the elements. There were ninety-two of them. But of course this is a rather boring task, so I thought that I would get some money, have some apparatus built, and then hire somebody who would just sit down and go through one element after the other. The trouble was that none of the physicists had any enthusiasm for this idea of a chain reaction. I thought, there is after all something called "chain

8. Beginning March 12, 1934, Szilard filed several British patent applications, which led to two British patents:

1) *No. 440,023*: "Improvements in or relating to the Transmutation of Chemical Elements" issued on December 12, 1935, covers the generation of radioactive elements by neutrons and the chemical separation of radioactive elements from non-radioactive isotopes.
2) *No. 630,726*: "Improvements in or relating to the Transmutation of Chemical Elements" was assigned to the British Admiralty and sealed secret in 1936; it was not published until September 28, 1949. This patent has as its subject the idea of the nuclear chain reaction, in which more than one neutron is emitted per neutron absorbed.

In a reply, dated January 15, 1957, to an inquiry from Samuel Glasstone, Szilard said:

> In the Spring of 1934 I applied for a provisional British application on a chain reacting system which was based on the concept that beryllium may give off two neutrons when it reacts with one slow neutron. The general concepts of a chain reaction including the critical size of the chain reacting system, were derived in this application. This application contained also the following passage:
>
> (a) Pure neutron chains, in which the links of the chain are formed by neutrons of the mass number 1 alone. Such chains are only possible in the presence of a metastable element. A metastable element is an element the mass of which (packing fraction) is sufficiently high to allow its disintegration into parts under liberation of energy. Elements like uranium and thorium are such metastable elements; these two elements reveal their metastable nature by emitting alpha particles. Other elements may be metastable without revealing their nature in this way.
>
> About one year later a patent application was filed by me in England based in part on this provisional application. This patent application was subsequently divided into two parts, one part was issued as a patent and the other part was assigned without financial compensation to the British Admiralty and was sealed secret. I assigned this patent to the British Admiralty because in England a patent could at that time be kept secret only if it was assigned to the Government. The reason for secrecy was my conviction that if a nuclear chain reaction can be made to work it can be used to set up violent explosions.

reaction" in chemistry. It doesn't resemble a nuclear chain reaction, but still it's a chain reaction. So I thought I would talk to a chemist, and I went to see Professor Chaim Weizmann, the Zionist leader, who was a renowned chemist. I had met him on one occasion or another. And Weizmann listened and Weizmann understood what I told him. He said, "How much money do you need?" I said that I thought £2,000 would be enough, which would have been at that time about $10,000. So Weizmann said that he would try to get this money. I didn't hear from him for several weeks, but then I ran into Michael Polanyi, who by that time had arrived in Manchester and was head of the chemistry department there.[9] Polanyi told me that Weizmann had talked to him about my ideas for the possibility of a chain reaction, and wanted Polanyi's advice about whether he should get me this money. And Polanyi thought that this experiment ought to be done, but then he didn't hear anything further. As a matter of fact, I did not see Weizmann again until the late fall of '45, after Hiroshima. I was at that time in Washington and I ran into him in the Wardman-Park Hotel. He seemed to be terribly happy to see me, and he said, "Do you remember when you came to see me in London?" I said, "Yes." He said, "And do you remember what you wanted me to do?" I said, "Yes." And he said, "Well, maybe you won't believe me, but I tried to get those £2,000 and found that I couldn't."

Because of these thoughts about the possibility of the chain reaction, and because of the discovery of artificial radioactivity, physics became too exciting for me to leave it. So I decided not to go into biology as yet, but to play around a little bit with physics, and I spent some months in the spring at the Strand Palace Hotel, doing nothing but dreaming about experiments which one could do, utilizing this marvelous tool of artificial radioactivity which Joliot had discovered. I didn't do anything; I just thought about these things. I remember that I went into my bath —I didn't have a private bath, but there was a bath in the corridor in the Strand Palace Hotel—around nine o'clock in the morning. There is no place as good to think as the bathtub. I would just soak there and think,

9. Michael Polanyi, the Hungarian-born physicist and chemist mentioned at the beginning of these Reminiscences, had become professor of physical chemistry at the University of Manchester.

and around twelve o'clock the maid would knock and say, "Are you all right, sir?" Then I usually got out and made a few notes, dictated a few memoranda; I played around this way, doing nothing, and the summer came around. At that time, I thought that one ought to try to learn something about beryllium; I thought that if beryllium is really so easy to split, the gamma rays of radium should split it and it should split off neutrons.

I had casually met the director of the physics department of St. Bartholomew's Hospital, so I dropped in for a visit and asked him whether in the summer, when everybody is away, I could use the radium, which was not much in use in summer, for experiments of this sort. And he said, yes, I could do this; but since I was not on the staff of the hospital, I should team up with somebody on his staff. There was a very nice young Englishman, Mr. Chalmers,[10] who was game, and so we teamed up and for the next two months we did experiments. It turned out that in fact beryllium splits off neutrons when exposed to the gamma rays of radium. This later on became really very important, because these neutrons are slow neutrons, and therefore if they disintegrate elements like uranium—of course we didn't know that until after Hahn's discovery—and if in that process fast neutrons come off,[11] you can distinguish them from neutrons of the source, which are slow.

We did essentially two experiments. We demonstrated that beryllium emits neutrons if exposed to the gamma rays of radium, and we demonstrated something else, which is called the Szilard-Chalmers effect. These experiments established me as a nuclear physicist, not in the eyes of Cambridge, but in the eyes of Oxford.[12]

There was an International Conference on Nuclear Physics in London in September, where these two discoveries were discussed by the par-

10. T. A. Chalmers, then a member of the physics department, Medical College, St. Bartholomew's Hospital, London.

11. O. Hahn and F. Strassman, "Über den Nachweis und das Verhalten der bei der Bestrahlung des Urans mittels Neutronen entstehenden Erdalkalimetalle," *Naturwissenschaften*, 27 (January 6, 1939), 11–15.

12. L. Szilard and T. A. Chalmers, "Detection of Neutrons Liberated from Beryllium by Gamma Rays: A New Technique for Inducing Radioactivity," *Nature*, 134 (September 29, 1934), 494–495; L. Szilard and T. A. Chalmers, "Chemical Separation of the Radioactive Element from its Bombarded Isotope in the Fermi Effect," *Nature*, 134 (September 22, 1934), 462–463.

ticipants[13] and so I got very favorable notice; and this led within six months to an offer of a fellowship at Oxford. However, I didn't get this offer until I had left England and come to America, where I didn't have a position but had some sort of fellowship. When I received the offer from Oxford, I had the choice of either keeping on this fellowship in America or returning to Oxford. I then wrote to Michael Polanyi, describing my choice between these two alternatives, and saying that I would accept the fellowship at Oxford and would stay in England until one year before the war, at which time I would shift my residence to New York City. That was very funny, because how can anyone say what he will do one year *before* the war? So the letter was passed around and a few people commented on it when I finally turned up in England.

And this is precisely what I did. In 1937 I decided that the time had come for me to change my full-time fellowship at Oxford to one which permitted me to spend six months out of the year in America. And on the basis of that arrangement (I had to take a cut of salary, of course; I had to go on half pay, so my total income amounted to $1,000 a year) I came over to America.

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.

One of the last books which I read before I left Berlin was "The World Set Free" by H. G. Wells. This book, which was published in 1933, predicted the liberation of atomic energy on an industrial scale and the development of the atomic bomb. It did not occur to me when I read it that I was reading a prophesy that might come true. But while strolling through the streets of London in the Fall of 1933, I was pondering upon the statement made by Lord Rutherford who said that "he who talks about the large scale liberation of atomic energy is talking moonshine"; it suddenly occurred to me how in certain circumstances it might become possible to set up a nuclear chain reaction, liberate energy on an industrial scale and construct atomic bombs. The thought that this might be, in fact, possible became a sort of obsession with me. It lead me to go into nuclear physics, a field in which I have not worked before and the thought stayed with me even though my first hunches in this regard turned out to be wrong.

When the German troops moved into the Rhineland and England advised France against invoking the Locarno Pact, I knew that there would be war in Europe and I came to America at the end of 1937 under an arrangement which permitted me to divide my time between America and Europe.

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May, 1969

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Additional material for page 98

re "everybody...had a position, except me."

cc. Letter, L.S. to ? 11 August, 1933
see page 2, paragraph 5

" Letter, Einstein to Donnan. (Translation) 16 August, 1933
See third paragraph.

L. Szilard.

Imperial Hotel,
Russell Square,
London, W.C.1.

R-1

11th August, 1933.

I hope this letter will catch the mail. I have been working all day at the Academic Assistance Council (the English organisation which Sir William Beveridge, whom I met in Vienna, built up to place German scientists). They have appointed a young secretary who is a very nice fellow and who will be efficient I hope, but who has gone away for four weeks, leaving the office in my care. Fortunately one of the lady secretaries is excellent and I hope we shall manage to get useful things done in August. She is my invention in so far as I got her to come to London to this office from Geneva where I spent a few days and did some work in which she helped me. I was impressed by her ability and devotion and got the London office to take her on their staff. Now I get the benefit of my good deed, as I would be buried by the work without her being in the office. The real problems have not yet been attacked at all, and the office exhausted its energy in bureaucratic activity.

I am going to Cambridge tomorrow to arrange with Kapitza, a Russian and a Fellow of the Royal Society, who is leaving for Russia next week, to take up the problem with the government there, and I hope that many of the scientists whom we cannot place in England will be able to work as experts in Russia, as there would be no point in crowding too many German scientists into England. Unfortunately I must be back in London on Sunday afternoon, so that my Cambridge visit will not be much of a rest.

In spite of being rather tired I feel very happy in England. This is partly due to the phenomenon that I always feel very happy for the first few months in a foreign country, but probably also due to the deeper sympathy I feel with the country and the people. I am not yet sure about the sympathy being mutual, but this is only a matter of practical importance.

The outlook in Europe is rather gloomy. It is quite probable that Germany will rearm and I do not believe that this will be stopped by intervention of other powers within the next years. Therefore it is likely to have in a few years two heavily armed antagonistic groups in Europe, and the consequence will be that we shall get war automatically, probably against the wish of either of the parties. Suppose if you have a large German and a large French air force, the false alarm is spread in Paris that the German air force has left the German air ports, no French government, even the most pacifist one, could take the responsibility for holding back their air force to wait for confirmation of that rumour. The utmost they will do will be to make arrangement to call back their air force if the rumour

"everybody...had a position, except me," subsequently turns out to be false. If they learn thereupon in Germany that the French aeroplanes have started, no hesitation whatever is conceivable in despatching their air force in their turn.

I am afraid this is the most optimistic history of the next war and I will be astonished if it does not hhppen within the next five or ten years.

England and America are certainly the most hopeful two countries and they may or may not be out of the next war, but even if they keep out of it I do notthink they can be proud of their aloofness.

I think most of my friends feel the burden of the situation and react by plunging deeper into their work and sealing hermetically their ears. I feel rather reluctant to follow their example, but I may have no choice left.

By now practically all phycisists who are any good have been placed and they have found out in Berlin at last that I have done nothing for myself, so they tried to get a fellowship for me from an industrial group, but the resources of that group are exhausted for the time being. Of course it is impossible to apply for a fellowship for myself with those English committees on whose work I have a direct influence.

I am not against going to America, but I would very much prefer to live in England. I have not dismissed the idea of going to India, neither has this idea grown stronger. The fact that I am in close touch with Sir Philip Hartog should possibly be of some use if an opportunity in India arises. I do not know if it would be wise to go to India for two years with a small English fellowship unless I were determined to stay there whatever happens.

I am spending much money at present for travelling about and earn of course nothing and ~~will~~ can not possibly go on with this for very long. At the moment, however, I canno be so useful that I cannot afford to retire into private life.

It is almost ten o'clock at night and I cannot ~~go on~~ have to stop.

You could send this letter on to Professor Bose, Dacca University, whom your friends will certainly know. Give him my best regards and tell him both Bitter (who happens to be in London) and I deplore his being absent.

R-1

TRANSLATION of letter from PROF. EINSTEIN to Prof. Donnan.

Le Coq sur Mer.

August 16th, 1933.

Dear Colleague,

I remember you very well, and the problems through which I got to know you.

Dr. Szilard is a many-sided and capable physicist. He is rich in ideas in both the experimental and the technical fields; at the same time he also has a flair for the essential in the theoretical field. He is one of those men, rich in ideas, who create intellectual and spiritual life wherever they are. I have grown to esteem greatly his capabilities in the course of several years' co-operation in the technical field.

In the upheaval of this last year his efforts on behalf of his younger colleagues have testified to his personal qualities, and it seems to me only right that he himself should not now be forgotten.

I feel I must tell you how profoundly I appreciate the readiness to help of our English colleagues and the English authorities. I also know very well all the good you yourself have achieved.

(Signed) A. EINSTEIN.

2nd set
May, 1969 R1

Additional Material for p. 101 (1)

re: "I talked to a number of people about this."

Letter, L.S. to Hugo Hirst. Mar. 17, 1934 *

~~Letter, L.S. to Walter Adams July 23, 1934~~

(Hirst-H) 6 65 42

6, Halliwick Road,
London, N.10.

17th March, 1934.

Dear Sir Hugo,

As you are on holiday you might find pleasure in reading a few pages out of a book by H.G. Wells which I am sending you. I am certain you will find the first three paragraphs of Chapter The First (The New Source of Energy, page 42) interesting and amusing, whereas the other parts of the book are rather boring. It is remarkable that Wells should have written those pages in 1914.

Of course, all this is moonshine, but I have reason to believe that in so far as the industrial applications of the present discoveries in physics are concerned, the forecast of the writers may prove to be more accurate than the forecast of the scientists. The physicists have conclusive arguments as to why we cannot create at present new sources of energy for industrial purposes; I am not so sure whether they do not miss the point.

It is perhaps possible to be more definite some time after your return, and in the meantime I hope you will in any case enjoy glancing through those few pages.

With best wishes for a pleasant stay,

Yours very truly,

Sir Hugo Hirst,
Carlton Hotel,
Cannes.

2nd set
May, 1969

Additional Material for page 102-103 (2)

re: STRUGGLE TO KEEP SECRET THE CHAIN REACTION PART OF BRITISH PATENT.

Letter, J. Coombes, Director of Artillery, War Office — Oct. 8, 1935
To Claremont Haynes & Co., Szilard's patent attorneys.
See especially paragraph 2, "...there appears to be no reason to keep the specification secret so far as the War Department is concerned."

After this, Szilard turned from the Army to the Navy.

Letter, to Wright — Feb. 26, 1936
We do not know who wrote this letter of introduction, ~~but it may well have been Lindemann. Szilard's attorney had written him on October 9th, 1935, enclosing the War Office letter (above), and saying, "I think your only chance now is to take the matter up with Lindemann."~~

Letter, L.S. to C.S. Wright of the Admiralty — Feb. 26, 1936
See especially the second paragraph on page 2, "... and my only concern is that the processes should be developed in this country a few years ahead of certain other countries."

Letter, Director of Navy Contracts, to L.S. — Mar. 20, 1936
Accepts Szilard's offer.

Letter, Treasury Solicitor to L.S. — Mar. 20, 1936

Letter, Director of Navy Contracts, to L.S. — Mar. 26, 1936
"Arrangements to lodge this certificate of secrecy have now been made."

4B D

Copy.

The War Office.
London.S.W. 1.

8th October 1935.

84/S/8473 (M.G.O.4 b).

Gentlemen.

With reference to your letter of the 16th September, 1935 (C/G) I am directed to inform you that in accordance with your suggestion arrangements were made for Dr Szilard to visit the Research Department at Woolwich on 27th September 1935, on which date he was afforded an opportunity fully to explain his ideas to the Director of Radiological Research.

The information given by Dr Szilard and the views expressed by him have been carefully considered with the result that the Department has confirmed the decision previously communicated to the effect that there appears to be no reason to keep the specification secret so far as the War Department is concerned.

I am to ask if you would be good enough to communicate this decision to Dr Szilard and at the same time thank him for the trouble he has taken in the matter.

In conclusoon I am to say that the Department appreciates the action you have taken to facilitate consideration and review of the proposal.

I am, Gentlemen.
Your obedient Servant.

J. Coombes.

Director of Artillery.

Messrs Claremont Haynes & Co.
Vernon House.
Bloomsbury Square.
W.C 1

F-53

February 26th, 1936.

Dear Wright,

I daresay you remember my ringing you up about a man working here who had a patent which he thought ought to be kept secret. I enclose a letter from him on the subject as you suggested. I am naturally somewhat less optimistic about the prospects than the inventor, but he is a very good physicist and even if the chances were a hundred to one against it seems to me it might be worth keeping the thing secret as it is not going to cost the Government anything.

With apologies for this hasty note, believe me,

Yours sincerely,

Dr. C.S. Wright.

Department of Scientific Research & Experiment,

Admiralty,

S.W. 1.

F-53

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

26th February, 1936.

Dr. C. S. Wright,
Department of Scientific
Research & Experiment,
Admiralty,
LONDON. S.W.1.

Sir,

I wish to draw your attention to the fact that I have applied for a British Patent, Application No. [19157/34] and you might find it advisable to prevent its publication.

The object of this Patent has nothing to do with instruments of war, but it contains information which could be used in the construction of explosive bodies based on processes described in the Specification. Such explosive bodies would be very many thousand times more powerful than ordinary bombs, and in view of the disasters which could be caused by their use on the part of certain Powers which might attack this country, it appears very undesirable that such information should be published through the medium of this Patent.

I understand that you would be able to intervene and prevent the publication of this Patent only if I assign the Patent to the Admiralty. If a form can be found which will enable me to retain my freedom of action concerning the applications which have nothing to do with instruments of war, I shall be pleased to assign the Patent to the Admiralty. No financial obligation of the part of the Admiralty would arise out of such a transaction.

I am fully aware of the fact that if a successful private manufacture is set up on the basis of this Patent, information will leak out sooner or later. It is in the very nature of this invention that it cannot be kept secret for a very long time, and my only convern is that the processes should be developed in this country a few years ahead of certain other countries. This purpose would be served by keeping the Patent secret; and we cannot aim at anything more.

I have to add that we have no certainty for the time being whether the processes described can be put into operation. I have, however, observed and investigated certain anomalies which indicate that this may very well be the case. At present there are still two alternative explanations for these anomalies, and it may take a year before we can decide in favour of one or the other.

I have so far delayed the publication of this Patent as far as possible by post-dating the original application, and asking for extensions of time. The last date for the acceptance of the Patent is now March 28th, and this cannot be postponed further.

I shall be glad to be at your disposal in London should you wish to see me regarding the details of the Patented Processes. My Patent Solicitor - Mr. Champneys, of Claremont Haynes & Co., Vernon House, Sicilian Avenue, Bloomsbury Square, London, Telephone No. Holborn 8811 would also be pleased to give you any information on this matter.

Yours very truly,

(Leo Szilard)

66s 51

Please address reply—
THE DIRECTOR OF NAVY CONTRACTS,
ADMIRALTY,
LONDON, S.W.1,

and quote—
C.P. Branch 10.

Patents/S.R.E.246/36.

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

ADMIRALTY,
LONDON, S.W.1.

20 March, 1936.

Sir,

With reference to your letter of 26th February to the Director of Scientific Research concerning your British Patent Application No.19157/34 and cognate applications, I have to inform you that the complete specification has been examined and the Department is pleased to accept your offer to assign the patent to the Admiralty, without financial charge for assignment, in order that it may be kept secret at least until such time as your further investigations have shewn whether the invention is of value for defence purposes.

The Treasury Solicitor has accordingly been instructed to communicate with you regarding the assignment of the invention and in order that the requisite certificate of secrecy may be filed at the Patent Office before the last date for acceptance of the complete specification viz: 28th March it is essential that the formal deed of assignment shall be executed within the next few days.

While the terms of the formal assignment will enjoin secrecy upon you it is noted that you wish to retain freedom of action concerning the applications of the invention to commercial purposes. In the circumstances you will be free at any time to request Admiralty permission to proceed with commercial exploitation of the invention with or without re-assignment of the patent and the question whether secrecy of the whole invention and of the patent (on terms to be agreed) will continue to be necessary, would be considered if and when such a request is received.

The Department accordingly desire that you will consult them before communicating any part of the invention to other parties.

L. Szilard, Esq.,
C/o The Clarendon Laboratory,
Parks Road,
OXFORD.

It

5-78

It is desired to make clear to you that the sealing
deferring of the British patent as secret will of course be contingent
publication upon your abandoning the corresponding patent application
of or in America and any other foreign patent applications you
may have made, without the Admiralty becoming liable for
any compensation, and you are requested to confirm that this
will in fact be done. In the event of secrecy being waived,
however, the act of waiver could be withheld, at your request,
in order not to prejudice any foreign patent applications you
might then wish to make.

I am, Sir,
Your obedient Servant,

[signature]

DIRECTOR OF NAVY CONTRACTS.

5-79

36518

Letters should be addressed to—
THE TREASURY SOLICITOR,
and the following reference quoted on the cover and in the letter:
A. 11223.

Telephone No.: WHITEHALL 1124.
EXTENSION: 17
Telegraphic Address: "PROCTOREX, LONDON."
Code used: A.B.C. 6TH EDN., 5 letter.

NATIONAL SCHEME FOR DISABLED MEN

STOREY'S GATE,
ST. JAMES'S PARK,
LONDON, S.W.1.

20th March 1936.

Dear Sir,

Patent Application No. 19157/34

On the instructions of the Admiralty I am forwarding herewith the engrossment of the Assignment of the above Invention for approval by you. If you approve the engrossment, I shall be glad if you will kindly execute it, in the presence of a witness, and return it to me at your early convenience.

SGG/EW.

Yours faithfully,

S. G. Gaine

for the Treasury Solicitor.

Dr. L. Szilard,
c/o The ~~County~~ Clarendon Laboratory,
Parks Road,
Oxford.

5-80

s.b.s. bk fldr #1

Please address reply—
THE DIRECTOR OF NAVY CONTRACTS,
ADMIRALTY,
LONDON, S.W.1,

and quote—
C.P. Branch 10.
Patents 8142/36.

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

NATIONAL SCHEME FOR DISABLED MEN

ADMIRALTY,
LONDON, S.W.1.

26 March, 1936.

Sir,

With reference to your letter dated 25th March I note that you are in agreement with the terms of paragraph 2 of Admiralty letter dated 24th March, C.P.Patents 8142/36 and that you are agreeable to the certificate of secrecy being lodged. Arrangements to lodge this certificate have now been made.

It is confirmed that the patent will be reassigned to you if and when secrecy of the patent is waived.

I am, Sir,

Your obedient Servant,

[signature]

for DIRECTOR OF NAVY CONTRACTS.

L.Szilard, Esq.,
C/o Clarendon Laboratory,
Parks Road,
OXFORD.

2nd set
May, 1969

Additional Material for page 101 (2)

re: "...WE REALLY OUGHT TO GET A LARGE MASS OF BERYLLIUM"

Szilard writes to his friends in Germany to get a block made.

Letter, L.S. to Lange — Nov. 6, 1934

Letter, L.S. to Brasch and Lange — Dec. 12, 1934
See point 4 on page 2

(from R-1)

(Beryllium block)

R-1

Strand Palace Hotel,
Strand, London W.C.2.

den 6. November, 1934.

Lieber Lange,

Diesesist eine Privatangelegenheit und als solches vertraulich. Ich möchte jetzt allmählich zu dem nächsten Kapitel übergehen und Versuche beginnen von denen ich Ihnen in London erzählt habe. Dazu ist eine grössere Menge von Beryllium nötig und die Firma Heraeus-Vacuumschmelze, Hanau a/M, Postfach 91, hat mir angeboten 2 Kgm Beryllium mir zu leihen doch will sie den Gegenwert Rm. 1,000.- zur Sicherheit hinterlegt haben. Da ich mich mit all den komplizierten Devisenbestimmungen nicht auskenne und Scherereien mit den Hin- und Herüberweisungen wenn möglich vermeiden möchte, wollte ich Sie bitten zu überlegen ob wir etwa den folgenden Weg einschlagen sollten. Wir könnten Heraeus, unter Hinweis auf die z.Zt. im Gange befindlichen gemeinsamen Beryllium Versuche von Ihnen und mir, bitten, dass das Beryllium nicht mir sondern Ihnen <u>persönlich</u> zur Verfügung gestellt wird, dass Sie den Betrag hinterlegen und wieder zurück erhalten wenn Sie das Beryllium unbeschädigt zurück geben. Es muss uns natürlich überlassen werden die Versuche in London oder sonst an einer geeigneten Stelle durchzuführen. Diese Lösung hat natürlich zur

Voraussetzung, dass Sie diesen Betrag ohne Schwierigkeit ein Jahr lang entbehren können und dass es Ihnen nichts ausmacht wenn es auf diese Weise festgelegt wird.

Bitte teilen Sie mir postwendend mit ob Ihnen dies als eine praktische Lösung erscheint. Ich werde dann bei Heraeus anfragen wie sie sich dazu stellen. Ist die Heraeus Vacuumschmelze der A.E.G. unterstellt?

Ich glaube, dass ich in den nächsten Monaten jedenfalls in England bleiben werde weil ich hier die Möglichkeit haben werde mit einer sehr grossen Menge Radium zu arbeiten. Im übrigen sollten wir uns, glaube ich, bald wieder sprechen, sobald Sie wissen bei welcher Spannung der Iodeffekt losgeht; es wäre dann an der Zeit zu verabreden wie die grossen Linien unserer weiteren Aktionen aussehen sollen. Ob man mit Bosch sprechen soll etc. etc. Spätestens müssen wir uns aussprechen sobald wir wissen wie die Energie der aus dem Beryllium ausgelösten Neutronen ist. Darüber sind im Augenblick direkte Versuche in Cambridge im Gange und ich habe etwas Beryllium dazu dorthin geschickt.

Mit herzlichem Grusse

Ihr

(Beryllium block)

R-1

Strand Palace Hotel,
Strand, London W.C.2.

den 12. Dezember, 1934.

Lieber Brasch und Lange,

Ich hoffe bald von Ihnen über das Ergebnis Ihrer Danziger Reise zu hören und zu erfahren ob Amsterdam, Brüssel oder Paris auserwählt wurde. Wie ich Ihnen schon schrieb bitte ich dringende Nachrichten zu-gleich an Simon, 10 Belbroughton Road, Oxford und an Strand Palace Hotel zu schicken, weil nichts nachgeschickt wird. Nicht dringende Sachen bitte weiter nach London wo ich am Sonntag gewöhnlich die Briefe abhole. Es wäre gut wenn die Verabredung bald zustande käme, weil ich gegebenenfalls plötzlich nach New York fahren muss.

Ich möchte folgendes im Telegrammstil berichten:

1. Fellner informiert mich über denn Mann mit dem mich Donnan zusammengebracht hat wenig begeistert; ich bremse dementsprechend.

2. Ich soll hier eventuell am 19. mit einem Brüsseler Bankier zusammentreffen, der zufällig Physiker ist und Beziehungen zur Union Minière (Radium Belge) hat. Er

heisst Philipson und ist allgemein bekannt. Ich will sehen in Brüssel Polonium aufzutreiben; dieses für praktische Zwecke wertlose Produkt könnte für uns die Lage für uns sehr vereinfachen; es ist gut denkbar, dass man für Geld (£1000) und gute Worte (oder vielleicht sogar gute Worte allein) das Äquivalent von 1 Gramm Radium erwerben kann wenn man Glück hat. Als Treffpunkt wäre mir im Augenblick in diesem Zusammenhang Brüssel am liebsten.

3. Guter Kontakt mit Rom über Indium. Es scheint, dass wir da auf der Spur eines Rätsels sind.

4.- Wäre es Ihnen möglich mir sogleich ein Stück Beryllium zu schicken? Ich brauche kompaktes Material (nicht gepulvert) in Form eines Zylinders (wenn möglich) von 4 bis 5 cm. Durchmesser, 3 bis 5 cm. hoch, in der Achse eine Bohrung von ½ cm. Durchmesser; Verwendung: es wird eine dünne Gammastrahlquelle in die Bohrquelle eingeführt und ein Stück Indiumblech ebenfalls in die Bohrung gelegt; es kommt auf eine möglichst günstige Geometrie an. Es genügen die kleinen Dimensionen also 4 cm. Durchmesser und 3 cm. hoch.

2nd set
May, 1969

Additional Material for page 102-103(1)

re: 1934 to 1936; GETTING FUNDS AND SUPPLIES, PATENTS

"Memorandum of Possible Industrial Applications arising out of a New Branch of Physics." attached to a letter entitled:		July 28, 1934
"On Nuclear Chain Reactions and their Bearing on the Question of Power Production." (Addressee unknown.)		Undated.
Letter,	Michael Polany (in Manchester) to L.S.	Nov. 11, 1934
Letter,	I.I. Rabi to L.S.	Dec. 21, 1934
Letter,	L.L. Whyte to L.S.	Jan. 29, 1935
Letter,	L.S. to Lindemann (from New York)	Mar. 4, 1935
Letter,	L.S. to Lindemann (from London) * (see especially pp. 2-3)	June 3, 1935
Letter,	L.S. to C.K. Ogden	June 4, 1935
Letter,	L.S. to L.L. Whyte	June 12, 1935
Letters (two),	L.S. to Prof. Singer. Also, draft of June 9th for these letters, with stress on "imminent disaster."	June 16, 1935
Letter,	Niels Bohr to L.S.	Feb. 4, ~~Mar. 8~~, 1936
Letter,	Giannini to L.S., mentioned in letter to Fermi, below.	Mar. 8, 1936
Letter,	L.S. to Fermi	Mar. 13, 1936
Letter,	L.S. to Bohr	Mar. 26, 1936
Letter,	L.S. to Rutherford	May 21, 1936
Letter,	L.S. to Cockcroft	May 21, 1936
Letter,	L.S. to Rutherford **	May 27, 1936
Letter,	L.S. to Cockcroft	May 27, 1936

* For note to last paragraph on page 4 of this letter, see note to letter to Rabi, Aug. 19, 1935, (with page 105 material).

** A letter to NATURE, "Anomalies in Radioactivity induced by Neutrons," was submitted on November 5, 1936, but was subsequently withdrawn and did not appear in print.

Page 102-103

re: SZILARD'S EFFORTS TO RAISE FUNDS, 1934-35, see especially:

Memorandum	July 28, 1934
Polany to L.S.	Nov. 11, 1934
Whyte to L.S.	Jan. 29, 1935
L.S. to Lindemann	March 4, 1935
" " "	June 3, 1935
L.S. to Ogden	June 4, 1935
L.S. to Whyte	June 12, 1935
L.S. to Singer	June 16, 1935

re: SZILARD'S ATTITUDE TOWARDS PATENTS, 1935-36, see especially:

L.S. to Lindemann	June 13, 1935
L.S. to Singer	June 16, 1935
Giannini to L.S.	March 8, 1936
L.S. to Fermi	March 13, 1936
L.S. to Rutherford	May 21, 1936
L.S. to Cockcroft	May 21, 1936

(Hist-J) (R-46

28th July, 1934.

Memorandum of Possible Industrial Applications arising out of a New Branch of Physics.

It is possible to indicate methods which might be successfully applied for the purpose of liberating atomic energy. It is not possible to foretell with certainty that these methods will be successful, but the experiments necessary for ascertaining this are fairly simple and could be carried out on a small scale in the university laboratories. Should such experiments give favourable results, the production of energy and its use for power production would be possible on such a large scale and probably with so little cost that a sort of industrial revolution could be expected; it appears doubtful, for instance, whether coal mining or oil production could survive after a couple of years.

I have applied for a group of patents in order to obtain patent protection for those methods which seemed to me promising, and it appears that these patents were successful in foreshadowing the latest developments in physics.

They include, for instance, methods for the artificial production of radio-active bodies based on a process which recently has been discovered by Fermi. The production of artificial "radium" for medical purposes based on these processes seems to be a sound commercial proposition, but it would be sidetracking the issue to concentrate on this point.

Facilities are required for two different purposes:-

1.) In order to develop and maintain a group of valid patents £500 are required for the next year, which would also take care of administrative expenses connected with the maintenance of the patents.

2.) If we wish to start the necessary experiments one ought to secure the continuity of work for two to three years. It is not possible to state exactly what facilities will be required as this will depend to a large extent on what facilities will be provided by the university laboratory which would be used as a frame for this work. It would, however, be advisable to have £2,000 available for expenditure that may be incurred.

From the point of view of a financier who could consider contributing to the required facilities the position is this:- the chances that the envisaged experiments will yield a favourable result may be estimated to anything between 1 to 20 and 1 to 5. The value of the return in case of success is, of course, enormous and could hardly be estimated in terms of money, so that from the purely financial point of view it is a sort of lottery with a fairly good chance to win a prize and enormous prizes. Yet it would be highly preferable to get financial support from quarters that would consider the experiments as a research work in the field of science which has a good chance of highly significant industrial applications, and realise that the exploitation of discoveries of this scope must not be organised on a purely commercial basis.

Difficulty will undoubtedly arise from the fact that it is not easy for anybody to form an independant opinion of his own on the merits of the case. A possible way out would be, to get the opinion of some of the professors of the University of London who are working themselves in this field, and with whom I can easily keep in touch on the matter.

On Nuclear Chain Reactions and their Bearing on the Question of Power Production.

Sir,

I wish to draw attention to theoretically possible transmutation processes of a special type and indicate simple experiments which could lead to their detection. The energy liberated in a process of this type may very well be large as compared to the energy input required for the maintenance of the process; if such a process can be realised and is used for the generation of power we may therefore have an active power balance. For instance by radiating a metastable element with neutrons it may prove to be possible to maintain a process in which neutrons cause a metastable element to transmute without being stopped from further remaining active in the process and increasing their average energy and number i.e. we may have a nuclear chain reaction. It is believed

Hist-J R-46

ABSCHRIFT.

Von:ä M. Polany, 11. Nov. 1934.
Kenmore,
Didsbury Park,
Manchester. Tel. 3838 Didsbury.

Dear Szilard,

I have spoken to Donnan about you when he was staying with us during the past two days. I have mentioned no details, it was he who did the speaking mostly. He would like to help you. His idea is a financier, a very rich man, who would let you do just as you please, asking only for a dividend at the end. Do you want him to follow up this direction ? Personally I do not feel too confident of the success, but the plan is not unintelligent. Furthermore: will you let me report to Aschner that you are in the course of making great inventions and that I would see a favourable opportunity for an investment as stiller Gesellschafter. He could ask me and Donnan to be his trustees. Especially if you come to work in Manchester. These overcautious people are often the first to invest their money under such extraordinary conditions.

Donnan told me that there is an opposition to you on account of taking patents. A physicist (not Rutherford) told him so.

Please answer question about Aschner soon, since I am about to write him about something else.

Yours

M.P.

Rabi/Szilard A/ JWS

Columbia University
in the City of New York

DEPARTMENT OF PHYSICS

December 21, 1934

Dr. Leo Szilard
Strand Palace Hotel
Strand, London W.C.2.

Dear Szilard:

I was very glad to hear that you are coming to New York and we are looking forward to seeing you. I turned your letter over to Professor Pegram to answer the questions with regard to the radon supply since I have no direct connection with that work. From what I hear, the situation does not look very favorable for obtaining considerable supplies of radon or of radium.

We have been concentrating on measurements of nuclear spins and moments with molecular beams and I hope we shall have some interesting things to show you when you arrive.

I want to congratulate you on the very ingenious and important experiments which you have made on gamma ray disintegration.

Please let me know when you expect to come and if I can be of help in any way.

Best regards from Mrs. Rabi and myself,

Sincerely yours,

I. I. Rabi

I.I. Rabi

IIR:BM

History A JWS

10, OLD JEWRY
LONDON, E.C.2.
METROPOLITAN 8601.

29.1.35.

Dear Dr Szilard.

I have thought about your plan - which - as I understand it - is an endeavour to combine in one organisation financial social and scientific interests. You are therefore bound to have severe inner clashes regarding policy - if it is ever near success - quite apart from the outside criticism which any 'mixed' plan of this kind must meet. As a financial proposition it can therefore be little interest to any ordinary investor & I don't know any 'freak' to whom I could put it up.

You will appreciate this letter is in answer to your financial enquiry: I don't know enough about your scientific or social ideas regarding the plan to estimate its suitability to the ends you may have in view. As far as I understand your ideas, I should think the whole thing could only be handled by some financially interested group or trust, of adequate standing to guarantee a clean policy and to stifle criticism.

Yours sincerely.

L. L. Whyte

March 4, 1935
c/o. B. Liebowitz
420 Riverside Drive
New York City, N.Y.

Dear Professor Lindemann:

A few days ago I saw Professor Einstein in Princeton. When he heard that we planned that I should work in the Clarendon and that the financial question is not yet settled, he suggested, - before I could mention our conversation on this subject - that the sum which was reserved for him by Christ Church might be used for this purpose. Subsequently I told Prof. Einstein of our conversation and asked him, if it were convenient to him that he should write to Christ Church, if you and Prof. Schrödinger come to the conclusion that this is the best course to take, and that in this case you should let him know in what way to write to Christ Church. I think, this is in perfect order and Prof. Einstein will write you a few lines direct.

I have informed the chairman of the Department at New York University of the position in Oxford and was relieved to see that he took a very friendly attitude in the matter. They seem to think that I ought to accept their offer and wait until I hear from you that something definite has been settled at Oxford. They emphasize that I could leave here at twenty-four hours' notice if required. For the moment I refrained from discussing this point beyond thanking for this offer.

I wonder if you could kindly let me know by cable (night-letter) the result of the I.C.I. meeting together with such comment of yours, as you think necessary.

Bethe has developped a simple theory which can explain the large cross-section of certain elements for the capture of slow neutrons and following up this line a number of simple experiments present themselves. I enclose a further page on this point. Could you perhaps also pass it on together with my kind regards to Collie and Griffiths ?

Yours sincerely

[Hist A]
also [Hist J]

PERSONAL.

74, Gower Street,
London, W.C.1.

3rd June, 1935.

Professor Lindemann,
Clarendon Laboratory,
Oxford.

Dear Professor Lindemann,

I hopevvery muchtto see you on Wednesday and talk to you about a matter which appears to me to be of great seriousness. For some time back I have suspected that the three radio-active periods which Chalmers and I found in the case of indium involved a new type of process in which a neutron

a) either knocks out another neutron from indium 113 in a non-capture process, or

b) liberates a neutron of the mass number 2 from indium 113 and gets captured in the process.

I have gradually come to the conviction that either a process of the type a) or, alternatively, a process of the type b) does occur and is possibly responsible for a number of other known radio-active periods, which I believe I can single out. I believe you will share this conviction after you have heard my arguments on Wednesday.

The question whether a neutron of the mass number 2 exists and can be liberated by fast neutrons cannot be answered offhand, but it is perhaps fair to say that since one of the two processes a) or b) certainly occurs, we have something like a fifty to fifty chance that such " double neutrons " are involved.

Professor Lindemann, Oxford. 3.6.35.

It seems to me that the question whether or not the liberation of nuclear energy and the production of radio-active material on a large scale can be achieved in the immediate future, hinges on the question whether or not " double neutrons " can be produced. If " double neutrons " can be produced, then it is certainly less bold to expect this achievement in the immediate future than to believe the opposite.

Even if I am grossly exaggerating the chances that these processes will work out as I envisage it at present, there is still enough left to be deeply concerned about what will happen if certain features of the matter become universally known. In the circumstances, I believe an attempt, whatever small chance of success it may have, ought to be made to control this development as long as possible.

There are two ways in which this can be attempted. The more important one is secrecy, if necessary, obtained by agreement among all those concerned that another form of publication should be used as far as the dangerous zone is concerned, which would make experimental results available to all those who work in the nuclear field in England, America and perhaps in one or two other countries, but otherwise keeping the result quiet, until those who are concerned ar satisfied that no " double neutron " is involved.

The other way, the less important one, is to take out patents. Early in March last year it seemed advisable to envisage the possibility that, contrary to current popular opinion, the release of large amounts of energy and the production of large amounts of radio-active material might be imminent. Realising to what extent this hinges on the " double neutron ", I have applied for a patent along these lines, including also the production of radio-active material by neutron bombardment. This was filed before Fermi started his fundamental experiments and was followed by a number of further patent applications along the same lines. Obviously it would be misplaced to consider patents in this field private property and pursue them with a view to commercial exploitation for private purposes. When the time is ripe some suitable body will have to be created to ensure their proper use. Also one has to avoid applying for patents wherever secrecy is endangered or in countries which are likely to misuse them; so far I have carefully observed this point.

Though I do not know for the present what will be the proper steps in this matter, I am very anxious to keep my full freedom of action in everything connected with it.

As far as experiments in this special field go, I should like to keep them, as far as possible, in my own hands and not merely act as a " catalyst ". As long as Collie, Griffithsaand I work alone in this field at Oxford, it is not quite easy for me to run these experiments in my own way and, without appearing pretentious, publish or not publish, according to what I think I should. I hesitate also to suggest that the whole Nuclear Department at Oxford should work in a field which may yield very little of purely scientific interest, if it turns out that we have to deal with a non-capture process after all.

If I knew that it would be convenient to you, I should make an attempt to get a budget of £1,000 for next year from private persons in order to be able to take on one or two helpers with whom I could work in this special field in the Clarendon Laboratory, while I would still, if it appears useful, work with Collie and Griffiths in the general field as envisaged hitherto.

~~Whether-~~ Whether an attempt to get financial assistance will be successful or not, I cannot tell, but I feel justified in approaching ~~a~~ man of vision in this matter. I should be very happy if you, too, thought that Oxford is in many ways well suited for this type of work and that, conversely, this type of work could greatly accelerate the building up of nuclear physics in Oxford.

There is another purely personal and therefore minor matter which I have to mention. I saw to-day in " Nature " a letter to the editor signed by Collie, Griffiths and myself. This is the first thing I knew of the experiments having been actually started, not to speak about the conclusions which Collie and Griffiths draw. I fully appreciate the good intentions which obviously actuated Collie and Griffiths and am anxious to avoid hurting their feelings. I am at a loss what to say. I am sorry that I have to speak about this at a time when there are so many more important things to worry about.

Yours sincerely,

Ordinary A fvs

74, Gower Street,
W. C. 1.

4th June, 1935.

C.K. Ogden, Esq.,

Dear Mr. Ogden,

I should very much appreciate it if I could have your advice on a matter of general interest. Mr. Poillon and Mr. Cottrell of the Research Corporation, New York, gave me your name and address, but it was in the last minute before I left New York and there may have been some misunderstanding in the matter.

Would Friday be convenient to you ?

My permanent address is c/o The Clarendon Laboratory, Oxford, but this time a message won't reach me there, so I am giving above a temporary London address.

Yours very truly,

LEO SZILARD

PERSONAL.

c/o Clarendon Laboratory,
Oxford.

12th June, 1935.

L.L. Whyte, Esq.,
10, Old Jury,
E. C. 2.

Dear Whyte,

I ought to have thanked you for your letter of the end of January long since, but I left for America early in February and did not come to England until June.

The discussion which I had with you and your letter were very helpful in getting my mind clear on the subject and I feel I ought to tell you that I have made good progress through a chance observation, the interpretation of which is no longer doubtful. This does not mean that all the crucial points have been cleared up, but there is something like a fifty to fifty chance for an exciting development in the immediate future. Even if I over-estimate these chances to some extent, there is enough left to make it necessary to take some action.

I am spending most of my time in Oxford, where I have been offered a three years appointment which I should accept, if I can fit it in with these other things and I hope, if I stay in England, I shall see you now and again.

With best wishes,

Yours sincerely,

(R-46) Hist-J

Clarendon Labarotory
Parks Road
Oxford
Telephon:3545

16th June, 1935.

Dear Professor Singer,

I believe I have not seen you since I had lunch with you and your niece sometimes in June last year. Many things have happened since then.

I arrived from America a fortnight ago and I may settle in Oxford where I have been offered a three years appointment in the Clarendon Laboratory.

I shuold be very pleased if you and your niece would care to have lunch with me if you happen to be in town. Please let me know which day would suit you best. I am in London once or twice a week and if so, I am staying at the Harewood Hotel, 74/8 Gower Street, Telephone:Museum 3941.

To=day I should like to write to you about another matter and I do this by a seprate letter. I wish it would be as easy as that to keep private and public life in watertight compartments.

With kindest regards,

Yours sincerely,

Hist-J

c/o Clarendon Laboratory
Park Road,
Oxford.

16th June, 1935.

Dear Professor Singer,

When I last saw you I told you about a plan to start research in a direction which may or may not be the starting point of a new industrial revolution. I thought that it might be possible to get financial support for such work, but abandoned any attempt in that direction after a few tentative discussions with you and a few others. I plunged into experimental work instead in which I was very lucky, and you see the results from the enclosed reprints. In the course of this work, I observed certain anomalies, the implications of which I did not realise until about four weeks ago.

There is no doubt left that these observations settle one of the two crucial points which determine whether or not the development will lead to a sort of industrial revolution in the near future, and settle it in a affirmative sense. The second crucial point is still unsrttled but leaves the way open for only two alternatives. So ~~that~~ we may fairly say of having something like a 50/50 chance for getting an answer in an affirmative sense. This? I believe, has to be considered a very high chance in view of the issue which is involved, and even if this chance were less we would have every reason to take action on it.

Let me tell you which steps I have taken unti now. [1] In March last year, it appeared advisable to envisage the possibility that the recent discoveries in the field of nucleus physics might enable us to liberate enrgy on a large scale and to store energy through the whole-sale production of radio-active bodies. At that time, I didnot see my way for a direct experimental attack but, seeing the outlines and the importance of the crucial points which I mentioned above, I applied for a number of patents dealing with methods and apparatus involved in the prospective industrial application. These patents were very lucky in foreshadowing the subsequent development. The first of these patents, for instance, protects the production of radio-active bodies through neutron bombardement and can be considered as a basic patent. A month or so, after this patent had been filed Fermi discoverd that radio-active bodies can be produced through neutron bombardement and a rapid development started thenthese radio-active bodies were used as a tool for further research. I myself got in August last year permission to use the radium in St.Bartholomew's Hospital during the holidays, and jumped into experimental work. This work went so well that I extended it beyond the holidays until the end of the year and went then, after a six weeks visit to Oxford, to New York. Not until I was in America did I realise the full significance of my London observations. About the same time, I received a definite offer of an appointment for three years from the Clarendon Laboratory and so I came to England a fortnight ago.

Here I informed Prof. Lindemann, who is in charge of the Clarendon Laboratory of the situation which has arisen and raised the question of fitting in my plans with the other work. I told him that I wish to keep the experimens in my own hands andtoo have all the freedom of movement which is necessary at this juncture. Prof.Lindemann promised me his support and all the facilities which were previously granted for my work in Oxford, the biggest item of which is the use of £ 15 000 worth radium.

In addition ot the facilities which are available right now in Oxford? I should require for the work a budget between £ 600 and £ 1 200 a year and should attempt to raise in the first instance £ 600 per year for three years, before actually starting the work. I thought it better not to ask Lindemann to take the initiative in the matter and so, once more, I am looking for a Maecenas.

While I am fully aware of the difficulties to raise such a sum, I am inclined to think that ultimately it will be possible to find somebody who has sufficient imagination to grasp the situatio and who is, at the same time, in a financial position to help in this matter.

Should you be able to think of somebody whom you know personally and of whom you think that he could be approached? Please, do let me know. I am very anxious though to keep this matter quie and this letter is only intended for your information.

I may add that it is not intended to approach industrial companies and that, though every effort will be made, to get pate protection and that, though these patents may be exploited, on a profit basis it wouldbe misplaced to apply in this matter primari commercial considerations. The patents might be handed over toaa body which would administer them along commercial lines, but woul use the profit for constructive purposes. A precedent for this, t though on a small scale, is the Research Corporation in New York, which was created in 1911 for the administration of the patent rights handed over to them by F.G. Cottrell and which has since grown into an active business organisation. While it is premature to discuss these things at present it is ~~better~~ necessary to mention them i order to explain that it is not possible to offer the patent righ to private persons or to draw up any commercial agreements about them, though an adequate for invested funds need not be excluded.

I have also to add, in order to give you a complete picture, that the disaster to which this development can lead may be more imminent than the industrial revolution which it may bring about, and that, from this point of view, an attempt will have to be made to keep the patents secret and gradually to bring about something like a conspiracy of those scientists who work in this field.

Yours sincerely,

[1451-J] (R-16)

c/o Clarendon Laboratory,
Park Road,-
Oxford.

9th June 1935.

Dear Professor Singer,

I should be very pleased indeed to see you again, and perhaps you and your niece would care to have lunch with me some time in the second half of June. Please let me know after you have settled in London which day would suit you best. I have just arrived from America and may settle in Oxford where I have been offered a three years' appointment in the Clarendon Laboratory. I can tell you more about these personal things which have worked out fairly well when I see you, but I had better write to you now about a matter of great earnestness in which you may or may not be able to help.

When I last saw you I told you about a plan to start research in a direction which ~~might possibly~~ may or may not be the starting point of a new industrial revolution. I thought that it might be possible to get financial support for such work, but abandoned any attempt in that direction after a few tentative discussions with you and a few others. I plunged into experimental work instead in which I was very lucky, and you see the results from the enclosed reprints. In the course of this work I observed certain anomalies, the implications of which I did not realise until about four weeks ago. To-day the position is this: I can demonstrate ~~the~~ crucial points on which I based my expectation that an industrial revolution may be brought about in the immediate future. Other points remain to be settled before we can say anything with certainty, but it may be fair to say that there is a fifty-to-fifty chance that these other points will work out alright. Even if these chances are smaller than I anticipate, they are certainly large enough to get excited at this juncture.

The disaster to which all this may lead is more imminent than the pleasant changes it may bring about, since applications for purposes of war are closer at hand than anything else and go beyond anything one is likely to conceive. An attempt to control this development will have to be made, however small the chances of success may be, and the most essential question in this respect is whether it will be possible to get the physicists in America and England to take precautions about publishing observations which fall into this dangerous zone, at a time

that one of the two

when such precautions necessarily seem to be premature. Unfortunately it will appear to many people premature to take some action until it will be too late to take any action. It will take [illegible] some time until one can see what practical steps have a chance of being put across.

In the meantime I shall make an attempt to make further experiments in order to settle as quickly as possible those questions which are still open. I informed yesterday Professor Lindemann who is in charge of the Clarendon Laboratory in Oxford of all this, and suggested that I should devote my time and the facilities which I have been previously promised in Oxford, to this task, that the limitations which would interfere with my freedom of action in this matter, should be removed, and told him that I should make an attempt to get further facilities from outside-sources. I know from my friends that the budget of the Clarendon Laboratory has been strained, and it would not be wise to embark on this venture without having the necessary equipment. I should rather not ask professor Lindemann to approach government institutions or private individuals, and that I should rather see what I can do in this respect myself. As far as Oxford is concerned the path is clear. I should think that facilities of about £1000.-.-. per year will be required for one or two years. This money would be used in two ways. For salaries of two men of the order of £200.- to £ 300.- a year each, who would be required to assist this work, and the rest would be used for buying equipment. There is already some equipment for this work in Oxford, the biggest item being £15.000.- worth of radium.

I am fully aware of the difficulty of getting hold of a private person who has some vision and who is able to provide part or the whole of the required sum. On the other hand I cannot believe that it should be impossible to find somebody in Great Britain. Should you be able to think of anybody whom you think one could approach and whom you happen to know personally, I should appreciate very much if you could let me know.

If it is not possible to get the facilities here, I shall have to make an attempt in America, but I can hardly do so before september, since everybody is leaving New York at this time of the year.

Yours sincerely

Leo Szilard

Leo Szilard

BK-f-1

UNIVERSITETETS INSTITUT
FOR
TEORETISK FYSIK

BLEGDAMSVEJ 15, KØBENHAVN Ø.

DEN February 4th 1936.

Dear Dr.Szilard,

I look forward to see you when I come to London, where my wife and I will be staying at Professor Donnan,23 Woburn Square, W.C.1, from Sundag evening Febsuary 9. till Wednesday morning February 12., when we are going up to Cambridge.

We have of course all here been very interested in your beautiful recent researches on the nuclear problems, with which we are also mueh occupied here. In the last months I have tried to develop some simple views about the constitution of the nucleus, which seem to account in a comprehensive way for the typical features of nuclear reactions including the disintegrations by γ-radiation observed by you. In my lecture in the university college, February 11. I will speak about these problems inatead of the more general one on space and time in atomic physics, originally proposed.

With kind regards,
yours sincerely,

N. Bohr

X

F-53

G. M. GIANNINI & CO., INC.
30 ROCKEFELLER PLAZA
NEW YORK

Circle 7.5895-96
Cable AMITARAD

St.Anton,AustrMarch the 8 th,1936.

Mr.Leo Szillard,
%Claremont Haynes and Co.,
Vernon House,Bloomsbury Square,
London,W.C.I.

Dear Mr.Szillard,

Last summer I wrote you asking for some information obout you artificial radioactivity patents,and received no answer:probably the letter was unproperly addressed.I must therefore introduce myself:I am a pupil pf Prof.Fermi of Rome,and studied with Amaldi,Segre' and Rasetti whom you well know either personally or by name.I am now a consultant phisicist and engineer in New York.

There I am handling some patents on artificial radioactivity taken by Fermi and his collaborators.A few days ago I was in Rome and understood by Amaldi and Segre' that you have met several times.

Now their applications and patents,fortunately enough, do not interfere with yours,namely the 440,023,but rather complete each other. I think therefore that it would be useful for us to meet. It may interest you that we are organizing in the U.S.a company for the manufacture of substances covered by some of our applications.

I am traveling Europe and am quite pressed. I expect to be in London the I6,I7,I8,I9:Segre' tells me that you are at the Clarendon Laboratory in Oxford,but he was not positive.We could meet in London if this is convenient to you,or otherwise I may come to Oxford or wherever you may be. Would you be kind enough to write or telegraph me an appointment for any of the above days,to Hotel Royal, Eindhoven,Holland,by the I3 th?

Looking forward to the pleasure of meeting you,I remain,

Yours very truly,

G.M.Giannini Pr.

R-1

c/o The Clarendon Laboratory,
Parks Road,
<u>O x f o r d</u>.

13th March 1936.

Dear Professor Fermi,

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

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

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

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

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

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

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

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

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

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

With best wishes,

Yours sincerely,

[Hist-H] 6 6 5 42

c/o Clarendon Laboratory,
Parks Road,
O x f o r d.

26th March, 1936.

Dear Professor Bohr,

Might I add to our last conversation that I have talked to Kalckar and Placzek before they left London about the same things about which I talked to you, so should you happen to wish to discuss any points which may occur to you in connection with this matter with either of them, you need not have any hesitation about it.

I wish to draw your attention to the paper of Hahn and Meitner in the last " Naturwissenschaften ". They overlook the fact that there exists a stable isotope of uranium (mass number 235) which has been discovered by Dempster in the course of this year. The case of uranium seems to me somewhat analogous to the case of indium, though certainly not similar. I find it difficult to assume so many isotopic isobars as we would have to assume in the case of uranium if we wanted to put all the blame on isotopic isobars.

Should you come to any conclusion on the subject of isotopic isobars and be able to give an upper limit for the half life period, I should appreciate your letting me know about it.

With best wishes,

Yours sincerely,

Written to Lord Rutherford (K.W.)

(R-46) (Hist-J)

c/o Clarendon Laboratory,
Parks Road,
Oxford.

21st May, 1936.

Sir,

You might perhaps remember that I mentioned to you some two years ago patents for which I had applied in March 1934. One such patent has now been granted on the principle of production of artificial radio-active elements by neutrons and the question arises to what use such patents ought to be put.

I cannot consider patents relating to nuclear physics as my property in any sense whatever. It would seem that if such patents are important, they ought to be administered in a disinterested way by some disinterested persons. It is, however, hardly for me to take any decisions about the patents which I have taken out in the capacity of a sort of self-appointed trustee, and apart from yourself, one could perhaps think of Chadwick, Cockroft, Joliot and Fermi as being the proper persons to say whether these patents should be withdrawn or maintained and in what way and by whom they should be administered, if they are maintained.

About two years ago I attempted to point out to Oliphant in greater detail why I thought that the existence of such patents might be useful. I am referring to this in the enclosed copy of a letter addressed to Dr. Cockroft, which I am sending you in case you should care to have more detailed information.

If I were convinced that industrial applications of great importance were imminent, I should not hesitate to ask you to give your attention to the matter at this juncture. This not being so, I shall merely ask Cockroft to inform you about all this if he thinks it necessary or, alternatively, I shall of course be at your disposal at any time you should think the matter sufficiently important to give some of your attention to it. I hope to be able to get Cockrofts advice next week and to discuss with him and Dee some unpublished observations which may or may not have a direct bearing on these matters.

Yours very truly,

(Leo Szilard)

b3c (Hist 5)
R-46

c/o Clarendon Laboratory,
Parks Road,
Oxford.

21st May, 1936.

Dear Dr. Cockroft,

Please forgive me for still being unable to talk about slow neutrons or any other similar subject in public (Kapitza Club). There are, however, some unpublished observations which I made in January and which I should very much like to discuss with you, Dee and Oliphant. They may or may not have a direct bearing on another matter on which I should like to have your advice. This concerns patents for the production of artificial radio-active elements for which I applied during a period of enforced leisure between March and September 1934.

I did not ever consider these patents as my property and the question now arises what to do with them. It is hardly for me to decide whether the patents should be withdrawn or maintained and in what form and by whom they should be administered. Though I should be glad to emphasise a definite point of view in this connection, it would seem that apart from Rutherford, it is for men like you, Chadwick, Joliot and Fermi to decide these questions, if the matter appears to be sufficiently important to deserve some attention.

Perhaps it is possible to envisage a proper form for a disinterested control of such patents. It would not seem right to me that physicists who take out such patents should derive financial or other privileges from them and if no disinterested form of control can be found, I personally would rather withdraw those patents which I have taken out. I am enclosing a booklet on the American Research Corporation which might interest you, though I do not believe that their example should be imitated.

Some two years ago I had a detailed conversation about these patents with Oliphant. I went to see him about them soon after Fermi's first dimoveryand I attempted tompointbout to him why I thought the existence of such patents might be useful. It seemed at the time that the possibility of an important industrial development hinges on the possibility of setting up enormously efficient sources of neutrons and that this possibility in its turn hinges on the question of the existens of a heavy isotope of the heutron.

If such multiple neutrons exist, we may envisage the remote possibility of an industrial revolution in the not too distant future. In that case patents might be used by scientists in a disinterested attempt to exercise some measure of influence over a socially dangerous development.

On the other hand, if no heavy neutron isotope exists, it would seem that an industrial development based on the application of nuclear physics must necessarily be very limited and there is no real need for any physicist to concern himself with such patents. The only use to which such patents could then be put, is towards obtaining funds for research purposes from persons interested in the promotion of industrial development. I personally felt inclined to think that good use could be made of such funds if they were forthcoming, especially in universities which are less well off than Cambridge or Oxford.

In these circumstances it appeared useful to apply for patents along two lines, i.e. the production of radio-active elements by neutrons and the construction of abundant sources of neutrons which is based on the hypothetical existence of multiple neutrons. Since we do not know at present whether such multiple neutrons exist and can be used for setting up abundant sources of neutrons, references to multiple neutrons in patents are either misleading or dangerous and accordingly care has been taken that patents which contain such references should not be published.

When I applied for the first patents in March 1934, I had an unclear idea that all these patents might be assigned to the Cavendish or some similar institution. After Fermi's discoveries and my conversations with Oliphant, I realised that this would not be feasible and made an attempt to assign all the patents to a Government Department. In this I was not entirely successful; while one patent has been assigned in this way and remains unpublished, another patent, relating to the Fermi effect, could not be assigned and has been published.

Some observations which I have so far not published may have a bearing on the question of the multiple neutrons. These observations may allow of more than one interpretation. However, the only interpretation which satisfies my desire for simplicity involves a heavy isotope of the neutron of mass number 4. You will perhaps take an altogether different view of these experiments, but I feel that I have no choice but to find out the more direct methods of observation, whether ~~xxxx~~ or not such a particle is involved in my experiments. A number of more direct experiments suggest themselves and about these I should very much like to have your advice.

It is fairly obvious that if such a particle exists, its mass exceeds 4.014. This again has rather obvious implications. One of them is rather frightening; and I felt it might be better not to publish anything on such a dangerous subject. It is quite likely that I am taking a rather exaggerated view of these things.

I may be in Cambridge in the beginning of next week and I shall then try to get hold of you and Dee. If I should not turn up, could you possibly let me know if you will be about during the second half of the week ?

Would you be kind enough to pass on this letter to Dee ?

With best wishes,

Yours sincerely,

P.S. I am sending a copy of this letter to Oliphant. Another copy is being sent to Rutherford, to whom so far I made only a passing remark about having taken out certain patents, when I had a short interview with him two years ago.

R-1

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x

c/o Clarendon Laboratory,
Parks Road,
Oxford.

27th May, 1936.

Dear Lord Rutherford,

Enclosed you will find a draft for a letter to " Nature ", which I am sending to you in the hope that it might interest you. Although it was written some time ago, I still hesitate to send it in for publication and I should like to mention here the reasons for this hesitation:

I have so far not been able to exclude the possibility that a heavy isotope of the neutron of mass number 4 exists and is involved in the anamalous Fermi effect of indium. It can easily be shown that such a particle would have a mass larger than 4.014. In the circumstances - if a heavy neutron isotope really exists - we would for the first time have to envisage the theoretical possibility of nuclear chain reactions. The prospect of bringing about nuclear transmutations on a large scale by means of such chain reactions is somewhat disconcerting. It is very unlikely that the misuse of chain reactions could be prevented if they could be brought about and became widely known in the next few years. I am quite aware that the view which I am taking on the subject may be very exaggerated. Nevertheless, the feeling that I must not publish anything which might spread information of this kind - however limited - indiscriminately, has so far prevented me from publishing anything on this subject. Since I am not quite clear about the proper course to take, I thought of discussing the matter with Dr. Cockroft, to whom I am going to write to-day.

If I were convinced that the matter is really important, I should perhaps not hesitate to ask for your advice at this juncture. Being at the moment unable to estimate the real importance of the matter, I shall ask Dr. Cockroft, if I see him, to inform you about our conversation if he thinks all this to be sufficiently important. I shall of course be glad to come to Cambridge at any time, in case you are disposed to express an opinion or desire to have further information on this subject. All this applies equally to a second matter which I equally hope to discuss with Dr. Cockroft:

You might perhaps remember that I mentioned to you in passing some two years ago patents for which I had applied in March 1934. One such patent has now been granted on the principle of producing artificial radio-active elements by neutrons and the question arises, to what use, if any, such patents ought to be put.

I cannot consider patents relating to nuclear physics as my property in any sense whatever. It would seem that if such patents are important, they ought to be administered in a disinterested way by disinterested persons. It is, however, hardly for me to take any irreversible steps about these patents which I have taken out in the capacity of a sort of self-appointed trustee. Apart from yourself, one could perhaps think of Chadwick, Cockroft, Joliot and Fermi as being the persons the most entitled to say whether these patents should be withdrawn or maintained and in what way and by whom they should be administered, if they are maintained.

Soon after Fermi's first discovery I discussedwwith Dr. Oliphant my reasons for thinking that the existence of such patents might be useful. I am referring to this in greater detail in the enclosed copy of a letter addressed to Dr. Cockroft, which I am sending you in case you should care to have more detailed information.

Yours very truly,

(Leo Szilard)

R-1

c/o Clarendon Laboratory,
Parks Road,
O x f o r d .

27th May, 1936.

Dear Dr. Cockroft,

Please forgive me for being unable to talk about slow neutrond or any other similar subject in public (Kapitza Club) this term. There are, however, some unpublished observations which I made in January and which I should very much like to discuss with you. They may or may not have a direct bearing on another matter on which I should like to have your advice. This concerns patents for the production of artificial radio-active elements for which I applied during a period of enforced leisure between March and September 1934.

I am enclosing a copy of a letter which I have sent to Lord Rutherford for your information and I should like to add the following:-

It would not seem right to me that physicists who take out such patents should derive financial or other privileges from them and some form of disinterested control ought to be found, if industrial applications are considered to be of some importance. I am enclosing a booklet on the American Research Corporation which might interest you in this connection, although I do not believe that their example ought to be too closely imitated.

When I applied for the first patent in March 1934, I somewhat foolishly thought such patents might simply be assigned to the Cavendish. After Fermi's first discovery, I went to see Dr. Oliphant and explained to him my reasons for thinking that the existence of such patents might be useful, if they are controlled by the proper persons. (Subsequently the patent relating to Fermi's discovery has, along with others, been offered to a Government Department; although it was made clear that the question of a financial compensation does not arise in any way, this particular patent has not been accepted and has been published under my name). I also told Dr. Oliphant that in my personal opinion the possibility of an important industrial development depends on the possibility of setting up enormoudlybefficient sources of neutrons and that this possibility in its turn depends on the question of the existence of a heavy isotope of the neutron.

If such multiple neutrons exist, we may envisage, if we wish to do so, the theoretical possibility of an industrial revolution in a not too distant future. In that case patents might be used by scientists in a disinterested attempt to exercise some measure of influence over a politically dangerous development.

On the other hand, if no heavy neutron isotope exists, it would seem that an industrial development based on the application of nuclear physics must necessarily be very limited and there is no real need for any physicist to concern himself with such patents. The only use to which such patents could then be put, is towards obtaining funds for research purposes from persons interested in the promotion of industrial development. I personally felt inclined to think that good use could be made of such funds if they were forthcoming.

I should very much like to have your opinion whether you think that some form of disinterested control of such patents can be found, since if this does not appear to be feasible, I personally should like to withdraw those patents which I have taken out.

In spite of the uncertainty of the assumption that a heavy neutron isotope is involved in my experiments, I have no choice but to try to investigate the matter by more direct methods of observation. I am not certain that the Wilson Cloud Chamber is the most suitable instrument in this case and I should very much like to have Dee's opinion on this question. Would you be kind enough to pass this letter with the enclosure on to him ? I may be in Cambridge on Saturday and Monday morning. Could you let me know if you will be about on one of these days ? I should then also attempt to get hold of Dee, if he happens to be free.

With best wishes,

Yours sincerely,

(Leo Szilard)

Encl.

2nd set ✓
May, 1969

Additional Material for page 104.

re: Planning for the experiment on beryllium splitting off neutrons when exposed to gamma rays of radium.

Letter, L.S. to Wigner — Aug. 7, 1934
Talks of plans for St. Bartholomew's Hospital.

re: Szilard-Chalmers effect.

Letter, L.S. to Professor Hopwood — Aug. 28, 1934

R-1

c/o Miss Simpson,
13, Brunswick Square,
London W.C.1.

7th August, 1934.

Dear Wigner,

I have made up my mind to jump into an experiment which is extremely simple, and which I shall attempt to carry out in August and September.

If one surrounds a Gamma ray source, Radium or Radon, with a substance, for instance Beryllium, in a thickness of about one centimeter and surrounds this substance with a thin sheet of silver, iodine or iridium etc. one can determin whether the Gamma rays liberate neutrons in the substance (Beryllium) by exposing a Geiger counter to the radiation (induced activity) of the silver, iodine or iridium.

Even very slow neutrons (and in our case they will be slow) must be expected to be active for the following reasion: the neutron is captured by silver, iodine or iridium and we must expect that its binding energy is sufficient to supply the energy necessary for the subsequent radio-active transformation. The view that we have to deal with a capture process is further strengthened by a note in "Nature" which shows that neutrons from the disintegration of diplogen by diplogen produce a fairly strong Fermi effect in silver.

The oddrr of magnitude of the expected effect cannot be foretold for Beryllium, but can be foretold for heavy water. One centimeter depth of heavy water yields about 1/5th of the neutron numbers which a Beryllium radium source would yield, provided that the number of Gamma quants in the case is equal to the number of Alpha particles in the other case.

Please don't discuss this experiment with anybody,for the present.

I have arranged to do the experiments at St. Bartholemews Hospital, and a young pjysicist will give his full time to it; however I do not yet know for certain if I can borrow a Geiger counter set from Frisch.

Another difficulty is to get heavy water in such a hurry. Could you please discuss this point with Polanyi. Perhaps he can let me have some of his supply or can write me a letter of introduction to Appleboy so that he should do his best about it. I am enclosing a letter which will show that I need, in the worst case water with 30% heavy water will do.

I am not quite clear whether in your formula relating to the Lee-Chadwick effect the ratio of the cross-section is given by a/a direct. I get for this ratio for 2.6 million volt Gamma rays and Chadwick's mass of the neutron and not 115 as you seem to get. Can you clear up this point? It really seems to be a crutial question.

Chadwick seems to get, by the way, from his experiment a neutron mass very close to the proton mass but let us not introduce that new value into your formula for the present in order not to confuse our discussion.

With kindest regards

Yours

R-1

Wigner zur Information mit der Bitte um Kommentar (wenn nötig) und mit der Bitte um Rücksendung.

28th August, 1934.

Was meint Polanyi zu dem chemischen Teil? Im übrigen: Vertraulich.

Dear Professor Hopwood,

Just a few lines to keep you informed. The counter tube with the large window is under construction. In the meantime I am concentrating on the question whether it is possible to increase the sensitivity by separating the radio-active isotope from the non-radio-active isotope by chemical methods. Most of the elements follow the example of Iodine which transmutes when bombarded by neutrons into a radio-active Iodine, and the question I am raising is whether we can separate the radio-active Iodine from the bulk of the bombarded Iodine. This sounds 'blasphemous' to the chemist, but I hope it can be achieved by proceeding like this: take an organic compoud of Iodine like Iodopropionicacid dissolved in water and bombard it with neutrons. One must expect that those Iodine atoms which swallow a neutron are thrown out of the organic compound and will be present in the solution as ions or bound as Iodides or Iodates or free Iodine according to the circumstances under which we choose to work. It should then be possible to separate the radio-active Iodine together with a small amount of

Iodine set free during the bombardment, ~~for instance~~ by precipitating with silver nitrate (after reducing the free
from the bulk of the irradiated Iodine.
Iodine and the Iodates)/. I am not yet quite certain as to the best conditions to be chosen from the point of view of preventing ~~an exchange of the~~ [illegible] [illegible].

In order not to keep the [illegible] I thought it would be wise to investigate Indium, Niobium and Scandium. Johnson Matthey had none but advised me how to get it. Mr. Chalmers fetched some Indium and Niobium and at midnight yesterday we were satisfied that Indium shows a fairly strong effect of a halfperiod of one or two hours; as far as I know these elements have not yet been investigated either in Rome or in Cambridge. Well, that is not too exciting.

I have arranged with Paneth to let Dr. Glueckauf see our experiments so that he should learn from them, and he is helping us now to build counter tubes for the benefit of all those concerned. Paneth himself is unfortunately away on holiday but I am getting Freundlich's advice if required.

As I hope to see you in about 10 days time I can tell you the rest and confine myself to wishing you a pleasant stay.

With kind regards,

Yours very sincerely,

2nd set
May, 1969

Additional Material for page 105 (section 5)

re: "I didn't get this offer until I had left England and come to America."

Letter, L.S. to I.I. Rabi Aug. 19, 1935

The letter in "Nature" mentioned on page 2 was: "Collisions between neutrons and diplons," by C.H. Collie, J.H.E. Griffiths, and L. Szilard. NATURE: 135, 903-904, June 1, 1935. Szilard never included this paper in his own bibliographies. *

Rabi must have read C.P. Snow's THE SEARCH, for he called it "the one novel which I knew which was really about scientists living as scientists." (Quoted in C.P. SNOW by Robert Gorham Davis, New York, Columbia University Press, 1965, page 11.)

see also:

Letters of Szilard to Lindemann of March 4th, 1935, and to Singer of June 16th, 1935 (both with page 102 material) which tell of his American trip, giving details not available elsewhere.

* See also, Letter, L.S. to Lindemann, June 3, 1935, (with page 102 material). Last paragraph.

Szilary A 8/S

c/o Clarendon Laboratory,
Parks Road,
Oxford.

19th August, 1935.

Dear Rabi,

I have sent you " The Search " by C.P. Snow. Snow is a physical chemist in Cambridge. Read it if you feelminclined to and pass it on to Urey with my kind regards if you think he will like it.

You will perhaps remember that we decided that Casimir in Leyden would be the best man to suggest to the university (or college) of Maine to be guest professor for a year. I have since met Casimir and found that he and his wife would very much like to go to America for a year. They are both very charming young people, Mrs. Casimir is an experimental physicist. They are adventurous and would like to see America. I do not suppose that anything can be done for this year, since it is rather late from the point of view of Maine, but perhaps you can get them to open the matter now for next year with Casimir, I think he is the best man we can find for this particular opening.

When I last saw you, I was considering taking up an entirely new line of experiments, but was not yet sure of my tentative conclusions. Since then I have changed my views on the subject, but since I made no statements to you, I need not withdraw them. The question of my appointment in Oxford has been postponed to the second half of September and I shall write you about this again. Meanwhile I have started to work in the laboratory, in order to make the best of the situation.

You may have seen a letter in " Nature " signed by Collie, Griffiths and myself which was sent in in May and printed on the 1st of Junes. If you read it, you will probably have guessed that I knew nothing about it until I saw it in " Nature ". A cable which was to have been sent to me about it to New York in order to get my consent to the publication, had not been despatched.

For the moment I am following your advice " die Wissenschaft überhaupt " and therefore I am not considering whether from the personal point of view it is wise or foolish to stay in Oxford, but shall work there, if a working arrangement can be found, without interruption until about March.

With kind regards to all,

Yours very sincerely,