p. 129

OFFPRINT FROM

PERSPECTIVES IN AMERICAN HISTORY

VOLUME II · 1968

Reminiscences

by Leo Szilard

edited by Gertrud Weiss Szilard and Kathleen R. Windsor

pp. 122 – 125
copied for Mr.
Yasumo

Jap. Transl. p. 113
p. 108 – 109
p. 117 – 118
p. 120
p. 99 – 100
p. 101 – 102
p. 122 – 124
p. 130 – 131
p. 150 – 151

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 Unpublished papers referred to in the notes are in the possession of Mrs.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.

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

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reaction, and I wrote a letter to the British Admiralty suggesting that the patent which has been applied for should be withdrawn because I couldn't make the process work.[15] Before that letter reached them, I learned of the discovery of fission.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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Having decided on this course, in principle, we returned to New York and Wigner left for California. (This goes to show how "green" we were. We did not know our way around in America, we did not know how to do business, and we certainly did not know how to deal with the government.) I had, however, an uneasy feeling about the approach we had decided upon and I felt that I would need to talk to somebody who knew a little bit better how things are done. I then thought of Gustav Stolper. He used to live in Berlin, where he had published a leading German economic journal and had been a member of the German parliament; now he was living as a refugee in New York. I went to see him and talked the situation over with him. He said that he thought that Dr. Alexander Sachs, who was economic adviser to the Lehman Corporation and who had previously worked for the New Deal, might be able to give us advice on how to approach the American government, and whether we should approach the State Department or some other agency of the government. He telephoned Dr. Sachs and I went to see him and I told him my story. Sachs said that if Einstein were to write a letter to President Roosevelt, he would personally deliver it to the President, and that there was no use going to any of the agencies or departments of the government; this issue should go to the White House. This sounded like good advice, and I decided to follow it.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

29. Richard B. Roberts, Carnegie Institution.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

38. Letters, Szilard to Fermi, July 3, July 5, July 8, and July 11, 1939.

39. Letter, Fermi to Szilard, July 9, 1939; letter, Fermi to Pegram, July 11, 1939.

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

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

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

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

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

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

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

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

While up to this point we had suffered from the lack of official recognition, during this period we were suffering from having official recognition. H. C. Urey was under orders not to discuss with Fermi and myself the possibility of preparing substantial amounts of Uranium 235. Because of this compartmentalization, we failed to put two and two together, and at no time were we or any other physicist able to say to the American government that atomic bombs could be made with amounts of Uranium 235 which it was practicable to obtain. Thus our project and Urey's remained projects of low priority until the British colleagues, who were not so compartmentalized (hamstrung?), pointed out that making atomic bombs of Uranium 235 must be regarded as a practical proposition.

This led to a reorganization of the project and the group working at Columbia University was transferred to Chicago [in February 1942].

[EDITORS' NOTE: In these oral reminiscences Szilard does not cover his activities at the "Metallurgical Laboratory" in Chicago from February 1942 to the spring of 1945. During that time his title was Chief Physicist. The scientific aspects of this period, in the form of some thirty reports written by Szilard, will be included in the forthcoming collected works. Szilard picks up the story again in 1945.]

In the spring of '45 it was clear that the war against Germany would soon end, and so I began to ask myself, "What is the purpose of continuing the development of the bomb, and how would the bomb be used if the war with Japan has not ended by the time we have the first bomb?"

Initially we were strongly motivated to produce the bomb because we feared the Germans would get ahead of us, and the only way to prevent them from dropping bombs on us was to have bombs in readiness ourselves. But now, with the war won, it was not clear what we were working for.

I had many discussions with many people about this point in the Metallurgical Laboratory of the University of Chicago, which was the code name for the uranium project which produced the chain reaction. There was no indication that these problems were seriously discussed at a high government level. I had repeated conversations with Compton[42] about the future of the project, and he too was concerned about its future, but he had no word of what intentions there were, if there were any intentions at all.

There was no point in discussing these things with General Groves[43] or Dr. Conant[44] or Dr. Bush,[45] and because of secrecy there was no intermediate level in the government to which we could have gone for a careful consideration of these issues.[46] The only man with whom we were sure we were entitled to communicate was the President. In these circumstances I wrote a memorandum addressed to the President, and was looking around for some ways and means to communicate the memorandum to him. Since I didn't suppose that he would know who I was, I needed a letter of introduction.

I went to see Einstein and I asked him to write me such a letter of introduction, even though I could tell him only that there was trouble ahead, but I couldn't tell him what the nature of the trouble was. Einstein wrote a letter and I decided to transmit the memorandum and the letter to the President through Mrs. Roosevelt, who once before had

42. Arthur Holley Compton, then director of the "Metallurgical Laboratory" at the University of Chicago.

43. Major General Leslie R. Groves, Manhattan Engineer District, director of all army activities of the Project at that time.

44. James B. Conant, President of Harvard University and chairman of the National Defense Research Committee at that time.

45. Vannevar Bush, director of the Office of Scientific Research and Development at that time.

46. The "Metallurgical Laboratory" was transferred from the civilian OSRD to the War Department Manhattan District in April 1943.

channelled communications from the project to the President. I have forgotten now precisely what I wrote to Mrs. Roosevelt; I suppose that I sent her a copy of Einstein's letter—but not the memorandum. This I could not do. The memorandum I couldn't send her, because the memorandum would have been considered secret.[47]

Mrs. Roosevelt gave me an appointment for May 8th. When I had this appointment I called on Dr. Compton, who was in charge of the project, and told him that I intended to get a memorandum to the President, and I asked him to read the memorandum. I was fully prepared to be scolded by Compton, to be told that I should go through channels rather than go to the President directly. To my astonishment, this is not what happened.

Compton read the memorandum very carefully, and then he said, "I hope that you will get the President to read this." Elated by finding no resistance where I expected resistance, I went back to my office. I hadn't been in my office for five minutes when there was a knock on the door and Compton's assistant came in, telling me that he had just heard over the radio that President Roosevelt had died [April 12, 1945].

There I was now with my memorandum, and no way to get it anywhere. At this point I knew that I was in need of advice. I went to see the associate director of the project, Dr. [Walter] Bartky, and told him of my plight. He suggested that we go and see Dr. [Robert M.] Hutchins, president of the University of Chicago. This was the first time that

47. Letter, Einstein to Roosevelt, March 25, 1945, introducing Szilard. Einstein recalls his letter of 1939 on the importance of uranium and Szilard's work, says he has "much confidence in his judgment," and explains that secrecy prevents his knowing about Szilard's current work:

> However, I understand that he now is greatly concerned about the lack of adequate contact between scientists who are doing this work and those members of your Cabinet who are responsible for formulating policy. In the circumstances I consider it my duty to give Dr. Szilard this introduction and I wish to express the hope that you will be able to give his presentation of the case your personal attention.

This letter has been published in *Einstein on Peace*, cited in note 36 above, pp. 304–305.

The memorandum by Szilard to the President, entitled "Enclosure to Mr. Albert Einstein's Letter of March 25, 1945 to the President of the United States," warns of precipitating an atomic arms race between the United States and Russia, suggests delay in our use of the atomic bomb, calls for setting up a system of international controls, and asks for formation of a cabinet-level committee through which scientists could express their views to the government. The document is printed in its entirety below as Appendix II to these Reminiscences.

I had met Hutchins. I told him briefly what the situation was, and this was the first time he knew that we were close to having an atomic bomb, even though the Metallurgical project had been on his campus for several years. Hutchins grasped the situation in an instant. He used to be an isolationist before the war, but he was a very peculiar isolationist, because where most isolationists held that the Americans should keep out of war because those foreigners do not deserve to have American blood shed for them, Hutchins' position was that the Americans should keep out of war because they would only mess it up. After he heard my story he asked me what this all would mean in the end, and I said that in the end this would mean that the world would have to live under one government. Then he said, "Yes, I believe you are right." I thought this was pretty good for an isolationist. As a matter of fact, a few days after the bomb was dropped on Hiroshima, Hutchins went on the radio; he gave a speech about the necessity of world government.

In spite of the good understanding which I had with Hutchins, he was not able to help with the task immediately at hand. "I do not know Mr. Truman," Hutchins said. I knew any number of people who could have reached Roosevelt, but I knew nobody offhand who could reach Truman. Truman just did not move in the same circles, so for a number of days I was at a complete loss as to what to do. Then I had an idea. Our project was very large by then, and there ought to be somebody from Kansas City. And three days later we had an appointment at the White House.

I asked the associate director of the project, Dr. Bartky, to come to Washington; and armed with Einstein's letter and my memorandum we went to the White House and were received by Matt Connelly, Truman's appointment secretary. I handed him Einstein's letter and the memorandum to read. He read the memorandum carefully from beginning to end, and then he said, "I see now this is a serious matter. At first I was a little suspicious, because this appointment came through Kansas City." Then he said, "The President thought that your concern would be about this matter, and he has asked me to make an appointment with you with James Byrnes, if you are willing to go down to see him in Spartanburg, South Carolina." We said that we would be happy to go anywhere that the President directed us, and he picked up the phone and made an appointment with Byrnes for us. I asked whether I might bring Dr. H. C. Urey[48] along, and Connelly said I could bring along anyone whom I wanted. So I phoned Chicago and asked Urey to join us in Washington, and together we went down the next day to Spartanburg, taking an overnight train from Washington.

We were concerned about two things: we were concerned first about the role which the bomb would play in the world after the war, and how America's position would be affected if the bomb were actually used in the war; we were also concerned about the future of atomic energy, and about the lack of planning as to how this research might be continued after the war. It was clear that the project set up during the war would not be continued but would have to be reorganized. But the valuable thing was not the big projects; the valuable things were the numerous teams, which somehow crystallized during the war, of men who had different abilities and who liked to work with each other. We thought that these teams ought to be preserved even though the projects might be dissolved.

We did not quite understand why we had been sent by the President to see James Byrnes. He had previously occupied a high position in the government, but was now out of the government and was living as a private citizen in Spartanburg. Clearly the President must have had in mind appointing him to a govermnent position, but what position? Was he to be the man in charge of the uranium work after the war, or what? We did not know.

Finally we arrived in Spartanburg, and I gave Byrnes Einstein's letter 5/28
to read and the memorandum which I had written. Byrnes read the memorandum, and then we started to discuss the problem. When I spoke of my concern that Russia might become an atomic power—and might become an atomic power soon, if we were to demonstrate the power of the bomb and use it against Japan—his reply was, "General Groves tells me there is no uranium in Russia."

I told Byrnes that there was certainly a limited amount of rich uranium ore in Czechoslovakia to which Russia had access; but apart from this, it was very unlikely that in the vast territory of Russia there should be no low-grade uranium ores. High-grade uranium ore is, of course,

48. Harold C. Urey, then professor of chemistry at Columbia University.

Byrnes had heard Groves' 20 yr + sci's 2-5 yr estimates re USSR bomb at 5/19 Interim cttee mtg New World: 554

another matter: high-grade deposits are rare, and it is not at all sure whether new high-grade deposits can be found. In the past, only the high-grade deposits were of interest because the main purpose of mining uranium ores was to produce radium, and the price of radium was such that working low-grade uranium ores would not have been profitable. But when you are dealing with atomic energy you are not limited to high-grade ores; you can use low-grade ones, and I doubted very much that anyone in America would be able to say, in a responsible way, that there were no major low-grade uranium deposits in Russia.

I thought it would be a mistake to disclose the existence of the bomb to the world before the government had made up its mind how to handle the situation after the war. Using the bomb certainly would disclose that the bomb exists. As a matter of fact, even testing the bomb would disclose that the bomb exists. Once the bomb has been tested and shown to go off, it would not be possible to keep it a secret.

Byrnes agreed that if we refrained from testing the bomb, people would conclude that its development did not succeed. However, he said that we had spent two billion dollars on developing the bomb, and Congress would want to know what we got for the money spent. "How would you get Congress to appropriate money for atomic energy research if you do not show results for the money which has been spent already?"

I saw his point at that time, and in retrospect I see even more clearly that it would not have served any useful purpose to keep the bomb secret, waiting for the government to understand the problem and to formulate a policy; for the government will not formulate a policy unless it is under pressure to do so, and if the bomb had been kept secret there would have been no pressure for the government to do anything in this direction.

Byrnes thought that the war would be over in about six months, and this proved to be a fairly accurate estimate. He was concerned about Russia's postwar behavior. Russian troops had moved into Hungary and Rumania; Byrnes thought it would be very difficult to persuade Russia to withdraw her troops from these countries, and that Russia might be more manageable if impressed by American military might. I shared Byrnes's concern about Russia's throwing around her weight in the

Byrnes' version of the meeting: All In One Lifetime (NY, 1958), 285.

postwar period, but I was completely flabbergasted by the assumption that rattling the bomb might make Russia more manageable.

I began to doubt that there was any way for me to communicate with Byrnes in this matter, and my doubt became certainty when he turned to me and said, "Well, you come from Hungary—you would not want Russia to stay in Hungary indefinitely." I certainly didn't want Russia to stay in Hungary indefinitely, but what Byrnes said offended my sense of proportion. I was concerned at this point that by demonstrating the bomb and using it in the war against Japan, we might start an atomic arms race between America and Russia which might end with the destruction of both countries. I was *not* disposed at this point to worry about what would happen to Hungary.

After all was said that could be said on this topic, the conversation turned to the future of the uranium project. To our astonishment, Byrnes showed complete indifference. This is easy to understand in retrospect because, contrary to what we had suspected, he was not slated to be director of the uranium project but he was slated to be secretary of state.

I was rarely as depressed as when we left Byrnes's house and walked toward the station. I thought to myself how much better off the world might be had I been born in America and become influential in American politics, and had Byrnes been born in Hungary and studied physics. In all probability there would have been no atomic bomb, and no danger of an arms race between America and Russia.

When I returned to Chicago, I found the project in an uproar. The Army had violently objected to our visit to the White House, and to Byrnes. Dr. Bartky was summoned to see General Groves; General Groves told him that I committed a grave breach of security by handing a secret document to Byrnes, who did not know how to handle secret documents. To calm the uproar, Dr. Compton, the leader of the project, decided to regularize the discussions by appointing a committee under the chairmanship of James Franck[49] to examine the issue of whether or not the bomb should be used, and if so, how.[50] The report 7/11

49. James Franck, physicist at the Chicago Laboratory. Other members of the Franck Committee were Hogness, Hughes, Nickson, Rabinowitch, Seaborg, Stearns, and Szilard.

50. Szilard wrote an unpublished article called "The Story of a Petition," dated July 28, 1946, which essentially covers the same ground as the oral tape. In this article, he says the Franck report,

of the committee has been published, and it was meant to be presented to the secretary of war, Mr. Stimson. Whether it ever reached his desk I do not know.

On my way from Spartanburg to Chicago I stopped in Washington to see Oppenheimer, who had arrived there to attend a meeting of the Interim Committee.[51] I told Oppenheimer that I thought it would be a very serious mistake to use the bomb against the cities of Japan. Oppenheimer didn't share my view. He surprised me by starting the conversation by saying that the atomic bomb is no good.[52] "What do you mean by that?" I asked him. He said, "Well, this is a weapon which has no military significance. It will make a big bang—a very big bang—but it is not a weapon which is useful in war." He thought it would be important, however, to inform the Russians that we had an atomic bomb and that we intended to use it against the cities of Japan, rather than taking them by surprise. This seemed reasonable to me, and I know that Stimson also shared this view. However, while this was necessary, it was certainly not sufficient. "Well," Oppenheimer said, "don't you think if we tell the Russians what we intend to do and then use the bomb in Japan, the Russians will understand it?" And I remember that I said, "They'll understand it only too well."

The time approached when the bomb would be tested. The date was never communicated to us in Chicago, nor did we ever receive any of-

---

was rushed to Stimson and advised against the outright military use of atomic bombs in the war against Japan. It took a stand in favor of demonstrating the power of the atomic bomb in a manner which will avoid mass slaughter but yet convince the Japanese of the destructive power of the bomb. By the beginning of July it became evident, at least to me, personally, that the use of the bomb will be examined by the Interim Committee purely on the basis of expediency, and that great weight will be given by them to the immediate effect, rather than to the long range effects.

51. The Interim Committee was organized in early May of 1945 by Secretary of War Henry L. Stimson to consider uses of the bomb and possible international control. He was chairman; members were Bush, Conant, Karl T. Compton, Under Secretary of the Navy Ralph Bard, Assistant Secretary of State William Clayton, and as the personal representative of President Truman, James Byrnes, who at that point held no official position. Robert Oppenheimer, director of the Los Alamos laboratory, was on the scientific advisory panel to the Interim Committee, whose other members were Arthur Compton, Fermi, and Lawrence. Richard G. Hewlett and Oscar E. Anderson, Jr., *The New World, 1939/1946: A History of the United States Atomic Energy Commission* (University Park, Pa., 1962–), I, 344–346.

52. A stronger word was used in the tape.

ficial indication of what was afoot. However, I concluded that the bomb was about to be tested when I was told that we were no longer permitted to call Los Alamos over the telephone. This could mean only one thing: Los Alamos must get ready to test the bomb, and the Army tried by this ingenious method to keep the news from the Chicago project.

I knew by this time that it would not be possible to dissuade the government from using the bomb against the cities of Japan. The cards in the Interim Committee were stacked against such an approach to the problem. Therefore all that remained was for the scientists to go unmistakably on record that they were opposed to such action. While the Franck Report argued the case on grounds of expediency, I thought the time had come for the scientists to go on record against the use of the bomb against the cities of Japan on moral grounds. Therefore I drafted a petition which was circulated in the project.[53] 7/3

This was again violently opposed by the Army. They accused me of having violated secrecy by disclosing in the petition that such a thing as a bomb existed. What the Army thought that we thought we were doing all this time, I cannot say. However, we did not yield to the Army's demand. The right to petition is anchored in the Constitution, and when you are a naturalized citizen you are supposed to learn the Constitution prior to obtaining your citizenship.

The first version of the petition which was circulated drew about fifty-three signatures in the Chicago project. What *is* significant is that these fifty-three people included *all* the leading physicists in the project and many of the leading biologists. The signatures of the chemists were conspicuously absent. This was so striking that I went over to the chemistry department to discover what the trouble was. What I discovered

53. In "The Story of a Petition" Szilard wrote, "A petition to the President was thus drafted in the first days of July and sent to every group leader in the 'Metallurgical Laboratory,' with the request to circulate it within his group." Szilard's covering letter to the group leaders is especially intense on the moral position, raising the analogy of individual Germans' guilt for Germany's acts. The text of this letter, dated July 4, 1945, appears below as Appendix III. The first version of the petition was dated July 3, 1945, and was signed by fifty-nine scientists. The final paragraph states: "In view of the foregoing, we, the undersigned, respectfully petition that you exercise your power as Commander-in-Chief to rule that the United States shall not, in the present phase of the war, resort to the use of atomic bombs." The text of the petition is printed in full below, as Appendix IV.

Byrnes becomes Sec of State 7/3

was rather disturbing: the chemists argued that what we must determine is solely whether more lives would be saved by using the bomb or by continuing the war without using the bomb. This is a utilitarian argument with which I was very familiar through my previous experiences in Germany. That some other issue may be involved in dropping the bomb on an inhabited city and killing men, women, and children did not occur to any of the chemists with whom I spoke.

Some of the members of the project said that they would sign the petition if it were worded somewhat more mildly, and I therefore 7/17 drafted a second version of the petition which drew a somewhat larger number of signatures—but not a significantly larger number.[54] The second petition was dated one day before the bomb was actually tested at 7/16 Alamogordo, New Mexico.[55]

After the petition had been circulated we were faced with the decision of what channels to use to communicate it to the White House. Several

54. The second version was dated July 17, 1945, and drew ~~seventy~~ 69 signatures. The final three paragraphs, concluding in a significant modification of the final paragraph of the original petition, are as follows:

> If after this war a situation is allowed to develop in the world which permits rival powers to be in uncontrolled possession of these new means of destruction, the cities of the United States as well as the cities of other nations will be in continuous danger of sudden annihilation. All the resources of the United States, moral and material, may have to be mobilized to prevent the advent of such a world situation. Its prevention is at present the solemn responsibility of the United States—singled out by virtue of her lead in the field of atomic power.
>
> The added material strength which this lead gives to the United States brings with it the obligation of restraint and if we were to violate this obligation our moral position would be weakened in the eyes of the world and in our own eyes. It would then be more difficult for us to live up to our responsibility of bringing the unloosened forces of destruction under control.
>
> In view of the foregoing, we, the undersigned, respectfully petition: first, that you exercise your power as Commander-in-Chief, to rule that the United States shall not resort to the use of atomic bombs in this war unless the terms which will be imposed upon Japan have been made public in detail and Japan knowing these terms has refused to surrender; second, that in such an event the question whether or not to use atomic bombs be decided by you in the light of the consideration presented in this petition as well as all the other moral responsibilities which are involved.

Both petitions were declassified finally on July 23, 1957.

55. While Szilard mentions that the petition was dated one day before the Alamogordo test, which was July 16, 1945, we have not found in the files any version dated July 15th. There is one dated the 16th, the day of the test, which is almost identical to the July 17th version, but without any signatures. All the copies with signatures are dated either the 3rd or the 17th.

new World is 1962

people, and above all James Franck, took the position that they would sign the petition because they agreed with it, but they could do this only if the petition were to be forwarded to the President through the regular channels rather than outside of these channels. I did not like this idea because I was just not sure whether the regular channels would forward the petition or whether they would sabotage it by filing it until the war was over. However, to my regret, I finally yielded and handed the petition to Compton, who transmitted it to Colonel Nichols,[56] who promised that he would transmit it to General Groves for immediate transmittal to Potsdam. I have no evidence that this petition ever reached the President.[57]

56. Letter of transmittal, Szilard to A. H. Compton, July 19, 1945, requesting that he "forward this petition to the President via the War Department." The final paragraph of this letter, significant for its anticipation of an arms race with Russia, reads:

> It would be appreciated if in transmitting these copies you would draw attention in your covering letter to the fact that the text of the petition deals with the moral aspect of the issue only. Some of those who signed the petition undoubtedly fear that the use of atomic bombs at this time would precipitate an armament race with Russia and believe that atomic bombs ought not to be demonstrated until the government had more time to reach a final decision as to which course it intends to follow in the years following the first demonstration of atomic bombs. Others are more inclined to think that if we withhold such a demonstration we will cause distrust on the part of other nations and are, therefore, in favor of an early demonstration. The text of the petition does not touch upon these and other important issues involved but deals with the moral issue only.

In his memorandum to Colonel K. D. Nichols, July 24, 1945, entitled "In re: Transmittal of Petitions addressed to the President," A. H. Compton urged speed in transmitting the documents, and enclosed the result of an opinion poll of 150 scientists, conducted by Farrington Daniels, new director of the Chicago laboratory. Compton commented that "the strongly favored procedure . . . to give a military demonstration in Japan, to be followed by a renewed opportunity for surrender before full use of the weapons is employed . . . coincides with my own preference . . ." Fletcher Knebel and Charles W. Bailey, "The Fight over the A-Bomb; Secret Revealed after 18 Years," *Look*, 27 (August 13, 1963), 22–23.

results to Compton 7/13, Compton to Nichols/Groves 7/24

57. The petition never reached the President, according to Knebel and Bailey. Nichols delivered the petition on July 25 to Groves, they write,* who kept it until August 1, when it was delivered to Secretary of War Stimson's office by messenger. But President Truman was then at the Potsdam conference, about to embark for home aboard the *U.S.S. Augusta*. On August 6, the day of Hiroshima, Truman was still on the Atlantic. Knebel and Bailey quote a memorandum written almost a year later, May 24, 1946, by Army Lieutenant R. Gordon Arneson, secretary of the Interim Committee. ". . . since the question of the bomb's use 'had already been fully considered and settled by the proper authorities,' . . . it was decided that 'no useful purpose would be served by transmitting either the petition or any of the attached documents to the White House, particularly since the President was not then in the country.' " "The Fight over the A-Bomb," p. 23.

New World: 397 says Groves took it himself

7/26 Potsdam decl'n warning Japan to surrender

* Stimson + adviser Harrison not interested, put in Harrison's files.

After the bomb was dropped on Hiroshima, I called the responsible officer of the Manhattan District in Chicago and told him that I was going to declassify the petition and asked him if there were any objection. There could not have been any objection, and there wasn't, and so I declassified the petition. A short time thereafter I sent a telegram to Matt Connelly, the President's secretary, to advise him that it was my intention to make the contents of the petition public, and that I wanted to advise him of this as a matter of courtesy.[58] When the telegram was not acknowledged I phoned the White House, upon which I received a telegram saying that the matter had been presented to the President for his decision, and that I would be advised accordingly.[59] Shortly thereafter I received a call from the Manhattan District saying that General Groves wanted the petition reclassified "Secret." I said that I would not do this on the basis of a telephone conversation, but that I wanted to have a letter explaining why the petition, which contained nothing secret, should be reclassified. Soon after, I received a three-page letter, stamped "Secret," in which I was advised that while the officer writing the letter could not possibly know what was in General Groves's mind when he asked that the petition be reclassified "Secret," he assumed that the reason for this request was that people reading the petition might conclude that there must have been some dissension in the project prior to the termination of the war; this might have slowed down the work of the project which was conducted under the Army.[60]

Immediately after Hiroshima, I went to see Hutchins and told him that something needed to be done to get thoughtful and influential people to think about what the bomb may mean to the world, and how the world and America can adjust to its existence. I proposed that the Uni-

58. We have so far not found this telegram to Connelly, but have found a corresponding letter, Szilard to Connelly, August 17, 1945.

59. Telegram, Connelly to Szilard, August 25, 1945.

60. This letter, which is in the Szilard files, is from Captain James S. Murray, Intelligence Officer, Manhattan Engineer District, dated August 27, 1945. Page three contains a paragraph giving exactly the explanation here summarized by Szilard. This letter was eventually declassified on May 13, 1960, and returned to Dr. Szilard. A few days after receiving it in 1945, Szilard commented in a letter to Robert M. Hutchins, dated August 29, 1945: "The Manhattan District's definition of 'Secret' includes 'information that might be injurious to the prestige of any governmental activity,' which is, of course, very different from the definition adopted by Congress in passing the Espionage Act."

versity of Chicago call a three-day meeting and assemble about twenty-five of the best men to discuss the subject. Hutchins immediately acted on this proposal and he invited a broad spectrum of Americans ranging from Henry Wallace to Charles Lindbergh. Lilienthal attended this meeting; so did Chester Barnard, Beardsley Ruml, Jake Weiner.[61]

This was one of the best meetings that I ever attended. In a short period of time we discussed a variety of subjects. We discussed the possibility of preventive war; we discussed the possibility of setting up international control of atomic energy, involving inspection. The wisest remarks that were made at this meeting were made by Jake Weiner, and what he said was this: "None of these things will happen. There will be no preventive war, and there'll be no international agreement involving inspection. America will be in sole possession for a number of years, and the bomb will exert a certain subtle influence; it will be present at every diplomatic conference, in the consciousness of the participants, and will exert its effect. Then, sooner or later, Russia also will have the bomb, and then a new equilibrium will establish itself." He had certainly more foresight than the rest of us, though it is not clear whether what we have now is an equilibrium or whether it is something else.

One of those who attended the Chicago meeting was Edward Condon. Henry Wallace was at that time looking around for a director for the Bureau of Standards, because Lyman J. Briggs had reached the retirement age. I asked that Condon be invited, with the possibility in mind that he might be a suitable candidate. Wallace liked him at first sight, and Condon was interested in the position. What I did not know when I thought of Condon as a suitable candidate was the fact that Condon had admired Henry Wallace for a number of years. After the conference I had a discussion with Hutchins and Condon, and I proposed that Condon and I go to Washington for a few days and try to find out what thinking in Washington about the bomb might be.

William Benton, vice president of the University of Chicago, had just accepted an appointment as assistant secretary of state under Byrnes.

61. Chester I. Barnard, Bell Telephone Company executive, foundation officer, author, and government consultant; Beardsley Ruml, treasurer of R. H. Macy and Son and chairman of the Federal Reserve Bank of New York; Joseph Lee Weiner, deputy director of the Division of Civilian Supply, Office of Production Management.

When he heard that we were going down to Washington he offered to invite the top desk men of the State Department to dinner, and he asked whether Condon and I might give a short discourse on the bomb for the benefit of the Department of State. This we actually did, and I think that this was the first intimation that these people in Washington had, that the advent of the atomic bomb did not necessarily mean that American military power would be enhanced for an indefinite period of time.

While we were in Washington, we somehow picked up a copy of a proposed bill on the control of atomic energy which the War Department had prepared, and which went under the name of the May-Johnson Bill. I took this bill back home to Chicago and gave it to Edward Levi of the Chicago Law School to read, who promptly informed me that this was a terrible bill and we had better do something to stop its passage.

While I was in Chicago I read in the newspapers that the House Military Affairs Committee had held a hearing on the bill which lasted for a day, and then they closed the hearing and prepared to report out the bill. At that one-day hearing the proponents of the bill testified for the bill, but no opponent of the bill was heard. This was disquieting news, but I doubt very much that I would have swung into action had it not been for a more or less accidental circumstance.

When the war ended, we were asked not to discuss the bomb publicly. We were under the impression that this request was made because there were some important international negotiations on the control of atomic energy under way, and any public discussion at this point could have disturbed these negotiations. We were not actually told this, but we were permitted to infer it, and having inferred it, we all decided to comply. Therefore all of us refused the numerous requests to speak over the radio or before groups, on what the atomic bomb was and what it might mean to the world. We kept silent. S. K. Allison[62] was the only one who gave a speech, and he said that he hoped very much that the secrecy which was imposed upon this type of work during the war

62. Samuel K. Allison, senior physicist from Los Alamos and newly appointed director of the Institute for Nuclear Studies. He gave "Sam's butterfly speech" at a luncheon at Chicago's Shoreland Hotel, September 1, 1945, at which the University of Chicago announced formation of its new research institute. Alice K. Smith, *A Peril and a Hope: The Scientists' Movement in America, 1945–1947* (Chicago, 1965), p. 88.

would be lifted after the war; otherwise, he said, he personally would cease to work on atomic energy and would start to work on the color of butterflies.

When his speech became known, Colonel Nichols flew from Oak Ridge to Chicago, and gathered a number of physicists and asked them just for a little while to be quiet and not to stir things up. "There is a bill being prepared," he said, "on the control of atomic energy, and when that bill is introduced in Congress that will be the right time to discuss these matters. Hearings will be held, and everyone will have an opportunity to appear as a witness and to have his say."

On the day when the one-day hearing was held before the House Military Affairs Committee and the hearings were closed, A. H. Compton arrived in Chicago and he met with the members of the project. He 10/9 told us on that occasion that the War Department had prepared a bill for passage through Congress, and that the request which was addressed to us to refrain from publicly speaking on the subject of the atomic bomb was due to the War Department's desire to pass this law without unnecessary discussions in Congress. I remember that I got mad at this point, and got up and said that no bill on the control of atomic energy would be passed in Congress without discussion if I could possibly help it.

Through pure chance I received a telephone call the next morning from Hutchins, who had lunched the previous day with Marshall Field, asking whether I would be willing to talk to somebody from the *Chicago Sun*. 10/10 I said that I was eager to talk to the *Sun*, but I would not want to talk to the *Sun* without also talking to the *Chicago Tribune*, and would Hutchins call up Colonel McCormick and have somebody from the *Chicago Tribune* come and see me?

In two separate interviews I told the reporters who came to see me that there was an attempt on the part of the Army to pass a bill through Congress without "unnecessary discussions," and the physicists would see to it that this would not happen. Because the information came from Compton and I regarded it as confidential, I did not feel free to identify either myself or Compton in this context; and the *Chicago Tribune* told me that under these circumstances they could not use the story. The *Chicago Sun*, being a less well-run newspaper, did not care, and printed

10/11

the story on its front page. In retrospect, I know that I made a mistake, and should have permitted the papers to use my identity and have the story printed both in the *Tribune* and the *Chicago Sun*.

But in any case, the fight was on.

I went back to Hutchins and called up Condon, who was at that time associate director of research of Westinghouse, and Condon and I once more went down to Washington to see what we could do. We could probably have done very little, had it not been for the excellent advice which we received from Bob Lamb, who was at that time legislative advisor of the C.I.O.[63] He was recommended to us very highly by a number of people, and even though we did not like the idea of working with somebody who was legislative advisor of the C. I. O., because we did not want to involve the C.I.O., we decided to overlook this for the sake of getting really first-class advice.

I don't think that anyone knew the Congress as well at that time as did Bob Lamb. When he read the bill, he agreed with us that this bill must not pass. He arranged for us to see Chet Holifield and George Outland. Chet Holifield was on the House Military Affairs Committee, and was picked by Bob Lamb for this reason; George Outland was a friend of Chet Holifield, and a highly intelligent and competent Congressman. Both Condon and I went to see these two gentlemen and explained the situation to them. In the evening Bob Lamb reported to us that they were convinced that we had a good case, and that Chet Holifield would fight for us. Chet Holifield then arranged for Condon and me to see the chairman of the House Military Affairs Committee, May, and Sparkman. He himself joined us at this conversation, and we presented the case to them. May was not impressed, and he shortly thereafter made it public that he was not going to reopen the hearing even though Dr. Condon and Dr. Szilard had asked him to do so.

By this time, however, the scientists in the project got organized in Chicago, in Oak Ridge, and in Los Alamos. Both Chicago and Oak Ridge came to the conclusion that the May-Johnson bill was a bad bill which must not pass, and they were so vocal about it that a larger and

63. Robert K. Lamb counseled Szilard and Condon, also Lyle B. Borst and Harrison Davies, two younger scientists from Clinton Laboratories, helping in the campaign to defeat the May-Johnson bill.

See p. 29, GWS #3

larger portion of the press got interested in the fight. Los Alamos, under the influence of Oppenheimer, took the opposite position, and was in favor of the passage of the bill.

Condon and I found that everybody in Washington was greatly interested in the issue. We set ourselves a schedule: everybody wanted to see us, and we decided that we would keep Cabinet members waiting one day, Senators for two days, and Congressmen for three days before we'd give them an appointment.

Henry Wallace was very much interested, and he arranged for us to meet Senator Lister Hill.

We went to see Ickes and Ickes grumbled that he had not read this bill at all. The War Department brought it over, left it there for half a day, and then took it away again. "This is not the first time," he said, "that Royall[64] has been giving me the bum's rush."

We went to see Lewis Strauss who was at that time in the Department of the Navy, and discovered that the Navy did not have any particular views about this bill. The bill was prepared in the War Department, and even though the President made some friendly remarks about the bill, it was not really in any sense an Administration bill. It was a War Department bill.

We then went to see James Newman, in Snyder's office,[65] which was supposed to steer the bill through Congress. James Newman had read the bill, and he said to us, "I don't believe that you really understand this bill." "Well," we said, "we didn't really claim to understand it, but we just didn't think it was a good bill."

"Well, I don't think it is a good bill either," said Newman, "but I doubt that you understand what it says. Look," he said, "here the bill says: 'there will be a Managing Director and an Assistant Managing Director, and the Managing Director has to keep the Assistant Managing Director informed at all times.' Now," said Newman, "have you ever seen a provision of this type in a bill? What does this mean? Clearly,

64. Brigadier General Kenneth C. Royall, who was co-author with William L. Marbury of the May-Johnson bill, later became secretary of war.

65. James Newman, head of the science section of OWMR, became *de facto* science adviser to the President. John Snyder was director, Office of War Mobilization and Reconversion (OWMR). On October 18, President Truman authorized OWMR to take charge of atomic energy legislation.

it means that the managing director will be someone from the Army and the assistant managing director will be someone from the Navy, and since the Navy and the Army don't talk to each other, you have to write into the bill that they must talk to each other on this occasion." For all I know it may well be that he was right.

Under public pressure, May, the chairman of the House Military Affairs Committee, in the end was forced to reopen the hearings. He reopened the hearings just for one more day. Towards six one evening I received a telephone call from the office of the Military Affairs Committee, asking me whether I could testify before the committee the next morning. I said that I would testify. Who else could testify? There was no one in town whom I knew had anything to do with atomic energy except Herbert Anderson, who had worked on the project mainly as Fermi's assistant. He was a spirited young man at that time. He is now director of the Enrico Fermi Institute of Nuclear Studies at the University of Chicago. I asked Anderson whether he was willing to testify and he said he would, so I gave his name to the committee. The War Department asked Oppenheimer and A. H. Compton to testify *for* the bill, and so there were four witnesses.

I worked through the night and ended up with some sort of a prepared testimony, which I delivered, and I was then questioned by members of the committee.[66] Herbert Anderson testified after me and then came Compton and Oppenheimer. Neither Compton nor Oppenheimer were really, at heart, in favor of the bill. Oppenheimer managed to give the most brilliant performance on this occasion, for he gave members of the committee the impression that he was in favor of the bill, and the audience, mostly composed of physicists, his colleagues, the impression that he was against the bill. He did that by the simple expedient of answering a question put to him by a member of the committee. He was asked, "Dr. Oppenheimer, are you in favor of this bill?" And he answered, "Dr. Bush is in favor of this bill, and Dr. Conant is in favor of the bill, and I have a very high regard for both of these gentlemen." To the members of the committee this meant that he favored the

66. Szilard's testimony is recorded in "Hearings before the Committee on Military Affairs," *House Report*, 79 Cong., 1 Sess., no. 4280 (October 9 and 18, 1945), 71–96. See also the text of Szilard's speech in *Cong. Record*, 79 Cong., 1 Sess. (1945), A4877–A4878.

bill; to the audience composed of physicists this meant that he did not favor the bill.

H. C. Urey was ready to testify and this was communicated to the chairman, but he was not called. After my testimony, the chairman dryly remarked that I had consumed two and a half hours of the committee's time. It was obvious that the chairman played ball with the War Department and that the committee was stacked against us. There was no hope of inducing the committee into amending the bill; but even if there had been some hope, it is not possible to get a good bill by writing a bad bill and amending it. The only hope was to have the bill bottled up in the Rules Committee, and in this we succeeded. The bill never reached the floor of the House.

One of the men whom I saw rather late in the game was Judge Samuel Rosenman, in the White House. There was no need to convince Rosenman. "I told the President," Judge Rosenman told me, "that it looks as though the Army wants to pass this bill by number only."

The Senate set up a Committee on Atomic Energy under the chairmanship of McMahon, and this committee started hearings on atomic energy legislation early in 1946. They heard a number of witnesses, and when I testified before this committee, delivering a carefully prepared testimony, I found a much friendlier reception than I had found before the House Military Affairs Committee.[67]

In retrospect it seems to me that at this point I could have left Washington because there was not very much more that I needed to do. There were plenty of other people interested who were more influential than I was, yet I stayed throughout most of the hearings and listened to the testimony of several distinguished witnesses. One of the most impressive of these testimonies was that of Langmuir.[68]

One of the things which we tried to get across, and tried to get across very hard, was the notion that it would not take Russia more than five years to develop an atomic bomb also. Even though all younger men and everybody who had a creative part in the development of atomic energy were of that opinion, this is a case of "youth did not prevail."

67. See U. S. Senate, *Hearings before the Special Committee on Atomic Energy*, 79 Cong., 1 Sess. (1945), 267–300.

68. Irving Langmuir, physicist at the General Electric laboratories.

In his book, *Speaking Frankly*, James Byrnes relates that when he became secretary of state he tried to find out how long it would take Russia to develop a bomb. He needed this information in order to evaluate proposals for the control of atomic energy. He reports in his book that, from the best information which he could gather, he concluded that it would take Russia seven to fifteen years to make the bomb. He adds that this estimate was based on the assumption that postwar recovery would be faster than it actually was, and therefore he thinks that this estimate ought to be revised upward rather than downward. Dr. Conant, Dr. Bush, and Dr. Compton all estimated that it would take Russia perhaps fifteen years to make the bomb. Why this should be so is not clear, though it is of course possible to contrive a psychological explanation for these overestimates. If you are an expert, you believe that you are in possession of the truth, and since you know so much, you are unwilling to make allowances for unforeseen developments. This is, I think, what happened in this case.

APPENDIX I

A. LETTER OF TRANSMITTAL, SZILARD TO DR. ALEXANDER SACHS, AUGUST 15, 1939

Dear Dr. Sachs:

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

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

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

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

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

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

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

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

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

Yours sincerely,

B. MEMORANDUM, SZILARD TO THE PRESIDENT, AUGUST 15, 1939

Much experimentation on atomic disintegration was done during the past five years, but up to this year the problem of liberating nuclear energy could not be attacked with any reasonable hope for success. Early this year it became known that the element uranium can be split by neutrons. It appeared conceivable that in this nuclear process uranium itself may emit neutrons, and a few of us envisaged the possibility of liberating nuclear energy by means of a chain reaction of neutrons in uranium.

Experiments were thereupon performed, which led to striking results. One has to conclude that a nuclear chain reaction could be maintained under certain well defined conditions in a large mass of uranium. It still remains to prove this conclusion by actually setting up such a chain reaction in a large-scale experiment.

This new development in physics means that a new source of power is now being created. Large amounts of energy would be liberated, and large quantities of new radioactive elements would be produced in such a chain reaction.

In medical applications of radium we have to deal with quantities of grams; the new radioactive elements could be produced in the chain reaction in quantities corresponding to tons of radium equivalents. While the practical application would include the medical field, it would not be limited to it.

A radioactive element gives a continuous release of energy for a certain period of time. The amount of energy which is released per unit weight of material may be very large, and therefore such elements might be used—if available in large quantities—as fuel for driving boats or airplanes. It should be pointed out, however, that the physiological action of the radiations emitted by these new radioactive elements makes it necessary to protect those who have to stay close to a large quantity of such an element, for instance the driver of the airplane. It may therefore be necessary to carry large quantities of lead, and this necessity might impede a development along this line, or at least limit the field of application.

Large quantities of energy would be liberated in a chain reaction, which might be utilized for purposes of power production in the form of a stationary power plant.

In view of this development it may be a question of national importance to secure an adequate supply of uranium. The United States has only very poor ores of uranium in moderate quantities; there is a good ore of uranium in Canada where the total deposit is estimated to be about 3000 tons; there may be about 1500 tons of uranium in Czechoslovakia, which is now controlled by Germany; there is an unknown amount of uranium in Russia, but the most important source of uranium, consisting of an unknown but probably very large amount of good ore, is Belgian Congo.

It is suggested therefore to explore the possibility of bringing over from Belgium or Belgian Congo a large stock of pitchblend, which is the ore of both radium and uranium, and to keep this stock here for possible future use. Perhaps a large quantity of this ore might be obtained as a token reparation payment from the Belgian Government. In taking action along this line it would not be necessary officially to disclose that the uranium content of the ore is the point of interest; action might be taken on the ground that it is of value to secure a stock of the ore on account of its radium content for possible future extraction of the radium for medical purposes.

Since it is unlikely that an earnest attempt to secure a supply of uranium will be made before the possibility of a chain reaction has been visibly demonstrated, it appears necessary to do this as quickly as possible by performing a large-scale experiment. The previous experiments have prepared the ground to the extent that it is now possible clearly to define the conditions under which such a large-scale experiment would have to be carried out. Still two or three different setups may have to be tried out, or alternatively preliminary experiments have to be carried out with several tons of material if we want to decide in advance in favor of one setup or another. These experiments cannot be carried out within the limited budget which was provided for laboratory experiments in the past, and it has now become necessary either to strengthen —financially and otherwise—the organizations which concerned themselves with this work up to now, or to create some new organization for the purpose. Public-spirited private persons who are likely to be interested in supporting this enterprise should be approached without delay, or alternatively the collaboration of the chemical or the electrical industry should be sought.

The investigations were hitherto limited to chain reactions based on the action of *slow* neutrons. The neutrons emitted from the splitting uranium are fast, but they are slowed down in a mixture of uranium and a light element. Fast neutrons lose their energy in colliding with atoms of a light element in much the same way as a billiard ball loses velocity in a collision with another ball. At present it is an open question whether such a chain reaction can also be made to work with *fast* neutrons which are not slowed down.

There is reason to believe that, if fast neutrons could be used, it would be easy to construct extremely dangerous bombs. The destructive power of these bombs can only be roughly estimated, but there is no doubt that it would go far beyond all military conceptions. It appears likely that such bombs would be too heavy to be transported by airplane, but still they could be transported by boat and exploded in port with disastrous results.

Although at present it is uncertain whether a fast neutron reaction can be made to work, from now on this possibility will have to be constantly kept in mind in view of its far-reaching military consequences. Experiments have been devised for settling this important point, and it is solely a question of organization to ensure that such experiments shall be actually carried out.

Should the experiments show that a chain reaction will work with *fast* neutrons, it would then be highly advisable to arrange among scientists for withholding publications on this subject. An attempt to arrange for withholding publications on this subject has already been made early in March but was abandoned in spite of favorable response in this country and in England on account of the negative attitude of certain French laboratories. The experience gained in March would make it possible to revive this attempt whenever it should be necessary.

APPENDIX II

ENCLOSURE TO MR. ALBERT EINSTEIN'S LETTER OF MARCH 25, 1945, TO THE PRESIDENT OF THE UNITED STATES, BY L. SZILARD

The work on uranium has now reached a stage which will make it possible for the Army to detonate atomic bombs in the immediate future. The "demonstration" of such bombs may be expected rather soon and naturally the War Department is considering the use of such bombs in the war against Japan.

From a purely military point of view this may be a favorable development. However, many of those scientists who are in a position to make allowances for the future development of this field believe that we are at present moving along a road leading to the destruction of the strong position that the United States hitherto occupied in the world. It appears probable that it will take just a few years before this will become manifest.

Perhaps the greatest immediate danger which faces us is the probability that our "demonstration" of atomic bombs will precipitate a race in the production of these devices between the United States and Russia and that if we continue to pursue the present course, our initial advantage may be lost very quickly in such a race.

If a nation were to start now to develop atomic bombs, so to speak from scratch, it could do so without reproducing many of the expensive installations which were built by the War Department during the War. *For over a year now we have known that we could develop methods by means of which atomic bombs can be produced from the main component of uranium which is more than one hundred times as abundant than the rare component* from which we are manufacturing atomic bombs at present. We must expect that a cost of about $500 million some nations may accumulate, within six years, a quantity of atomic bombs that will correspond to ten million tons of TNT. A single bomb of this type weighing about one ton and containing less than 200 pounds of active material may be expected to destroy an area of ten square miles. Under the conditions expected to prevail six years from now, most of our major cities might be completely destroyed in one single sudden attack and their populations might perish.

In the United States, thirty million people live in cities with a population of over 250,000 and a consideration of this and other factors involved indicates that the United States will be much more vulnerable than most other countries.

Thus the Government of the United States is at present faced with the necessity of arriving at decisions which will control the course that is to be followed from here on. These decisions ought to be based not on the *present* evidence relating to atomic bombs, but rather on the situation which can be expected to confront us in this respect a few years from now. This situation can be evaluated only by men who have first-hand knowledge of the facts involved, that is, by the small group of scientists who are actively engaged in this work. This group includes a number of eminent scientists who are willing to present their views; there is, however, no mechanism through which direct contact could be maintained between them and those men who are, by virtue of their position, responsible for formulating the policy which the United States might pursue.

The points on which decisions appear to be most urgently needed are as follows:

1. Shall we aim at trying to avoid a race in the production of atomic bombs between the United States and certain other nations?

2. Can a system of controls relating to this field be devised which is sufficiently tight to be relied on by the United States and which has some chance of being accepted under otherwise favorable conditions by Russia and Great Britain?

3. Can we materially improve our chances to obtain the cooperation of Russia in setting up such a system of controls by developing in the next two years modern methods of production which would give us an overwhelming superiority in this field at the time when Russia might be approached?

4. What framework could immediately be set up within which the scientific development of such "modern" methods could vigorously be pursued both under present and postwar conditions? Should, for instance, this framework be set up under the Secretary of Commerce or under the Secretary of the Interior, or should the scientific development be under a Government-owned corporation jointly controlled by the Secretary of Commerce, the Secretary of the Interior, and the Secretary of War?

5. Should the scientific development work be based on the assumption that a race in the production of atomic bombs is unavoidable and accordingly be aimed at maximum potential of war, say in six years from now, or should the scientific development be rather aimed at putting us into a favorable position with respect to negotiations with our Allies two or three years from now?

6. Should, in the light of the decisions concerning the above points, our "demonstration" of atomic bombs and their use against Japan be delayed until a certain further stage in the political and technical development has been reached so that the United States shall be in a more favorable position in negotiations aimed at setting up a system of controls?

Other decisions, which are needed but which are perhaps less urgent, would come within the competence of the Department of the Interior.

If there were in existence a small subcommittee of the Cabinet (having as its members, the Secretary of War, either the Secretary of Commerce or the Secretary of the Interior, a representative of the State Department, and a representative of the President, acting as the secretary of the Committee), the scientists could submit to such a committee their recommendations either by appearing from time to time before the committee or through the secretary of the committee.

The latter, if so authorized, by the President, could also act as a liaison to the scientists prior to the designation of such a subcommittee. At his disposal could then be placed a memorandum which has been prepared in an attempt to analyze the consequences of the scientific and technical development which we have to anticipate. The memorandum was prepared on the basis of consultations with ten scientists from six different institutions in the United States. These and other eminent scientists who were not consulted would undoubtedly avail themselves of the opportunity of presenting their views to a man authorized by the President, assuming that such a man would have the time at his disposal which a study of this kind would require.

APPENDIX III

SZILARD TO GROUP LEADERS OF "METALLURGICAL LABORATORY," JULY 4, 1945

Dear ——:

Inclosed is the text of a petition which will be submitted to the President of the United States. As you will see, this petition is based on purely moral considerations.

It may very well be that the decision of the President whether or not to use atomic bombs in the war against Japan will largely be based on considerations of expediency. On the basis of expediency, many arguments could be put forward both for and against our use of atomic bombs against Japan. Such arguments could be considered only within the framework of a thorough analysis of the situation which will face the United States after this war and it was felt that no useful purpose would be served by considering arguments of expediency in a short petition.

However small the chance might be that our petition may influence the course of events, I personally feel that it would be a matter of importance if a large number of scientists who have worked in this field went clearly and unmistakably on record as to their opposition on moral grounds to the use of these bombs in the present phase of the war.

Many of us are inclined to say that individual Germans share the guilt for the acts which Germany committed during this war because they did not raise their voices in protest against those acts. Their defense that their protest would have been of no avail hardly seems acceptable even though these Germans could not have protested without running risks to life and liberty. We are in a position to raise our voices without incurring any such risks even though we might incur the displeasure of some of those who are at present in charge of controlling the work on "atomic power."

The fact that the people of the United States are unaware of the choice which faces us increases our responsibility in this matter since those who have worked on "atomic power" represent a sample of the population and they alone are in a position to form an opinion and declare their stand.

Anyone who might wish to go on record by signing the petition ought to have an opportunity to do so and, therefore, it would be appreciated if you could give every member of your group an opportunity for signing.

APPENDIX IV

A PETITION TO THE PRESIDENT OF THE UNITED STATES, JULY 3, 1945

Discoveries of which the people of the United States are not aware may affect the welfare of this nation in the near future. The liberation of atomic power which has been achieved places atomic bombs in the hands of the Army. It places in your hands, as Commander-in-Chief, the fateful decision whether or not to sanction the use of such bombs in the present phase of the war against Japan.

We, the undersigned scientists, have been working in the field of atomic power for a number of years. Until recently we have had to reckon with the possibility that the United States might be attacked by atomic bombs during this war and that her only defense might lie in a counterattack by the same means. Today with this danger averted we feel impelled to say what follows:

The war has to be brought speedily to a successful conclusion and the destruction of Japanese cities by means of atomic bombs may very well be an effective method of warfare. We feel, however, that such an attack on Japan could not be justified in the present circumstances. We believe that the United States ought not to resort to the use of atomic bombs in the present phase of the war, at least not unless the terms which will be imposed upon Japan after the war are publicly announced and subsequently Japan is given an opportunity to surrender.

If such public announcement gave assurance to the Japanese that they could look forward to a life devoted to peaceful pursuits in their homeland and if Japan still refused to surrender, our nation would then be faced with a situation which might require a re-examination of her position with respect to the use of atomic bombs in the war.

Atomic bombs are primarily a means for the ruthless annihilation of cities. Once they were introduced as an instrument of war it would be difficult to resist for long the temptation of putting them to such use.

The last few years show a marked tendency toward increasing ruthlessness. At present our Air Forces, striking at the Japanese cities, are using the same methods of warfare which were condemned by American public opinion only a few years ago when applied by the Germans to the cities of England. Our use of atomic bombs in this war would carry the world a long way further on this path of ruthlessness.

Atomic power will provide the nations with new means of destruction. The atomic bombs at our disposal represent only the first step in this direction and there is almost no limit to the destructive power which will become available in the course of this development. Thus a nation which sets the precedent of using these newly liberated forces of nature for purposes of destruction may have to bear the responsibility of opening the door to an era of devastation on an unimaginable scale.

In view of the foregoing, we, the undersigned, respectfully petition that you exercise your power as Commander-in-Chief to rule that the United States shall not, in the present phase of the war, resort to the use of atomic bombs.

in the course of this development. Thus a nation which sets the precedent of using these newly liberated forces of nature for purposes of destruction may have to bear the responsibility of opening the door to an era of devastation on an unimaginable scale.

In view of the foregoing, we, the undersigned, respectfully petition that you exercise your power as Commander-in-Chief to rule that the United States shall not, in the present phase of the war, resort to the use of atomic bombs.

APPENDIX III

SZILARD TO GROUP LEADERS OF "METALLURGICAL LABORATORY," JULY 4, 1945

Dear ——:

Inclosed is the text of a petition which will be submitted to the President of the United States. As you will see, this petition is based on purely moral considerations.

It may very well be that the decision of the President whether or not to use atomic bombs in the war against Japan will largely be based on considerations of expediency. On the basis of expediency, many arguments could be put forward both for and against our use of atomic bombs against Japan. Such arguments could be considered only within the framework of a thorough analysis of the situation which will face the United States after this war and it was felt that no useful purpose would be served by considering arguments of expediency in a short petition.

However small the chance might be that our petition may influence the course of events, I personally feel that it would be a matter of importance if a large number of scientists who have worked in this field went clearly and unmistakably on record as to their opposition on moral grounds to the use of these bombs in the present phase of the war.

Many of us are inclined to say that individual Germans share the guilt for the acts which Germany committed during this war because they did not raise their voices in protest against those acts. Their defense that their protest would have been of no avail hardly seems acceptable even though these Germans could not have protested without running risks to life and liberty. We are in a position to raise our voices without incurring any such risks even though we might incur the displeasure of some of those who are at present in charge of controlling the work on "atomic power."

The fact that the people of the United States are unaware of the choice which faces us increases our responsibility in this matter since those who have worked on "atomic power" represent a sample of the population and they alone are in a position to form an opinion and declare their stand.

Anyone who might wish to go on record by signing the petition ought to have an opportunity to do so and, therefore, it would be appreciated if you could give every member of your group an opportunity for signing.

APPENDIX IV

A PETITION TO THE PRESIDENT OF THE UNITED STATES, JULY 3, 1945

Discoveries of which the people of the United States are not aware may affect the welfare of this nation in the near future. The liberation of atomic power which has been achieved places atomic bombs in the hands of the Army. It places in your hands, as Commander-in-Chief, the fateful decision whether or not to sanction the use of such bombs in the present phase of the war against Japan.

We, the undersigned scientists, have been working in the field of atomic power for a number of years. Until recently we have had to reckon with the possibility that the United States might be attacked by atomic bombs during this war and that her only defense might lie in a counterattack by the same means. Today with this danger averted we feel impelled to say what follows:

The war has to be brought speedily to a successful conclusion and the destruction of Japanese cities by means of atomic bombs may very well be an effective method of warfare. We feel, however, that such an attack on Japan could not be justified in the present circumstances. We believe that the United States ought not to resort to the use of atomic bombs in the present phase of the war, at least not unless the terms which will be imposed upon Japan after the war are publicly announced and subsequently Japan is given an opportunity to surrender.

If such public announcement gave assurance to the Japanese that they could look forward to a life devoted to peaceful pursuits in their homeland and if Japan still refused to surrender, our nation would then be faced with a situation which might require a re-examination of her position with respect to the use of atomic bombs in the war.

Atomic bombs are primarily a means for the ruthless annihilation of cities. Once they were introduced as an instrument of war it would be difficult to resist for long the temptation of putting them to such use.

The last few years show a marked tendency toward increasing ruthlessness. At present our Air Forces, striking at the Japanese cities, are using the same methods of warfare which were condemned by American public opinion only a few years ago when applied by the Germans to the cities of England. Our use of atomic bombs in this war would carry the world a long way further on this path of ruthlessness.

Atomic power will provide the nations with new means of destruction. The atomic bombs at our disposal represent only the first step in this direction and there is almost no limit to the destructive power which will become available proposed excerpts outlined in black

OFFPRINT FROM

PERSPECTIVES IN AMERICAN HISTORY

VOLUME II · 1968

Reminiscences
by Leo Szilard

edited by Gertrud Weiss Szilard
and Kathleen R. Windsor

p. 102 - [illegible]

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 succeed 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.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Having decided on this course, in principle, we returned to New York and Wigner left for California. (This goes to show how "green" we were. We did not know our way around in America, we did not know how to do business, and we certainly did not know how to deal with the government.) I had, however, an uneasy feeling about the approach we had decided upon and I felt that I would need to talk to somebody who knew a little bit better how things are done. I then thought of Gustav Stolper. He used to live in Berlin, where he had published a leading German economic journal and had been a member of the German parliament; now he was living as a refugee in New York. I went to see him and talked the situation over with him. He said that he thought that Dr. Alexander Sachs, who was economic adviser to the Lehman Corporation and who had previously worked for the New Deal, might be able to give us advice on how to approach the American government, and whether we should approach the State Department or some other agency of the government. He telephoned Dr. Sachs and I went to see him and I told him my story. Sachs said that if Einstein were to write a letter to President Roosevelt, he would personally deliver it to the President, and that there was no use going to any of the agencies or departments of the government; this issue should go to the White House. This sounded like good advice, and I decided to follow it.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

While up to this point we had suffered from the lack of official recognition, during this period we were suffering from having official recognition. H. C. Urey was under orders not to discuss with Fermi and myself the possibility of preparing substantial amounts of Uranium 235. Because of this compartmentalization, we failed to put two and two together, and at no time were we or any other physicist able to say to the American government that atomic bombs could be made with amounts of Uranium 235 which it was practicable to obtain. Thus our project and Urey's remained projects of low priority until the British colleagues, who were not so compartmentalized (hamstrung?), pointed out that making atomic bombs of Uranium 235 must be regarded as a practical proposition.

This led to a reorganization of the project and the group working at Columbia University was transferred to Chicago [in February 1942].

[EDITORS' NOTE: In these oral reminiscences Szilard does not cover his activities at the "Metallurgical Laboratory" in Chicago from February 1942 to the spring of 1945. During that time his title was Chief Physicist. The scientific aspects of this period, in the form of some thirty reports written by Szilard, will be included in the forthcoming collected works. Szilard picks up the story again in 1945.]

In the spring of '45 it was clear that the war against Germany would soon end, and so I began to ask myself, "What is the purpose of continuing the development of the bomb, and how would the bomb be used if the war with Japan has not ended by the time we have the first bomb?"

Initially we were strongly motivated to produce the bomb because we feared the Germans would get ahead of us, and the only way to prevent them from dropping bombs on us was to have bombs in readiness ourselves. But now, with the war won, it was not clear what we were working for.

I had many discussions with many people about this point in the Metallurgical Laboratory of the University of Chicago, which was the code name for the uranium project which produced the chain reaction. There was no indication that these problems were seriously discussed at a high government level. I had repeated conversations with Compton[42] about the future of the project, and he too was concerned about its future, but he had no word of what intentions there were, if there were any intentions at all.

There was no point in discussing these things with General Groves[43] or Dr. Conant[44] or Dr. Bush,[45] and because of secrecy there was no intermediate level in the government to which we could have gone for a careful consideration of these issues.[46] The only man with whom we were sure we were entitled to communicate was the President. In these circumstances I wrote a memorandum addressed to the President, and was looking around for some ways and means to communicate the memorandum to him. Since I didn't suppose that he would know who I was, I needed a letter of introduction.

I went to see Einstein and I asked him to write me such a letter of introduction, even though I could tell him only that there was trouble ahead, but I couldn't tell him what the nature of the trouble was. Einstein wrote a letter and I decided to transmit the memorandum and the letter to the President through Mrs. Roosevelt, who once before had

42. Arthur Holley Compton, then director of the "Metallurgical Laboratory" at the University of Chicago.

43. Major General Leslie R. Groves, Manhattan Engineer District, director of all army activities of the Project at that time.

44. James B. Conant, President of Harvard University and chairman of the National Defense Research Committee at that time.

45. Vannevar Bush, director of the Office of Scientific Research and Development at that time.

46. The "Metallurgical Laboratory" was transferred from the civilian OSRD to the War Department Manhattan District in April 1943.

channelled communications from the project to the President. I have forgotten now precisely what I wrote to Mrs. Roosevelt; I suppose that I sent her a copy of Einstein's letter—but not the memorandum. This I could not do. The memorandum I couldn't send her, because the memorandum would have been considered secret.[47]

Mrs. Roosevelt gave me an appointment for May 8th. When I had this appointment I called on Dr. Compton, who was in charge of the project, and told him that I intended to get a memorandum to the President, and I asked him to read the memorandum. I was fully prepared to be scolded by Compton, to be told that I should go through channels rather than go to the President directly. To my astonishment, this is not what happened.

Compton read the memorandum very carefully, and then he said, "I hope that you will get the President to read this." Elated by finding no resistance where I expected resistance, I went back to my office. I hadn't been in my office for five minutes when there was a knock on the door and Compton's assistant came in, telling me that he had just heard over the radio that President Roosevelt had died [April 12, 1945].

There I was now with my memorandum, and no way to get it anywhere. At this point I knew that I was in need of advice. I went to see the associate director of the project, Dr. [Walter] Bartky, and told him of my plight. He suggested that we go and see Dr. [Robert M.] Hutchins, president of the University of Chicago. This was the first time that

47. Letter, Einstein to Roosevelt, March 25, 1945, introducing Szilard. Einstein recalls his letter of 1939 on the importance of uranium and Szilard's work, says he has "much confidence in his judgment," and explains that secrecy prevents his knowing about Szilard's current work:

> However, I understand that he now is greatly concerned about the lack of adequate contact between scientists who are doing this work and those members of your Cabinet who are responsible for formulating policy. In the circumstances I consider it my duty to give Dr. Szilard this introduction and I wish to express the hope that you will be able to give his presentation of the case your personal attention.

This letter has been published in *Einstein on Peace*, cited in note 36 above, pp. 304–305.

The memorandum by Szilard to the President, entitled "Enclosure to Mr. Albert Einstein's Letter of March 25, 1945 to the President of the United States," warns of precipitating an atomic arms race between the United States and Russia, suggests delay in our use of the atomic bomb, calls for setting up a system of international controls, and asks for formation of a cabinet-level committee through which scientists could express their views to the government. The document is printed in its entirety below as Appendix II to these Reminiscences.

I had met Hutchins. I told him briefly what the situation was, and this was the first time he knew that we were close to having an atomic bomb, even though the Metallurgical project had been on his campus for several years. Hutchins grasped the situation in an instant. He used to be an isolationist before the war, but he was a very peculiar isolationist, because where most isolationists held that the Americans should keep out of war because those foreigners do not deserve to have American blood shed for them, Hutchins' position was that the Americans should keep out of war because they would only mess it up. After he heard my story he asked me what this all would mean in the end, and I said that in the end this would mean that the world would have to live under one government. Then he said, "Yes, I believe you are right." I thought this was pretty good for an isolationist. As a matter of fact, a few days after the bomb was dropped on Hiroshima, Hutchins went on the radio; he gave a speech about the necessity of world government.

In spite of the good understanding which I had with Hutchins, he was not able to help with the task immediately at hand. "I do not know Mr. Truman," Hutchins said. I knew any number of people who could have reached Roosevelt, but I knew nobody offhand who could reach Truman. Truman just did not move in the same circles, so for a number of days I was at a complete loss as to what to do. Then I had an idea. Our project was very large by then, and there ought to be somebody from Kansas City. And three days later we had an appointment at the White House.

I asked the associate director of the project, Dr. Bartky, to come to Washington; and armed with Einstein's letter and my memorandum we went to the White House and were received by Matt Connelly, Truman's appointment secretary. I handed him Einstein's letter and the memorandum to read. He read the memorandum carefully from beginning to end, and then he said, "I see now this is a serious matter. At first I was a little suspicious, because this appointment came through Kansas City." Then he said, "The President thought that your concern would be about this matter, and he has asked me to make an appointment with you with James Byrnes, if you are willing to go down to see him in Spartanburg, South Carolina." We said that we would be happy to go anywhere that the President directed us, and he picked up the phone and made an appointment with Byrnes for us. I asked whether I might bring Dr. H. C. Urey[48] along, and Connelly said I could bring along anyone whom I wanted. So I phoned Chicago and asked Urey to join us in Washington, and together we went down the next day to Spartanburg, taking an overnight train from Washington.

We were concerned about two things: we were concerned first about the role which the bomb would play in the world after the war, and how America's position would be affected if the bomb were actually used in the war; we were also concerned about the future of atomic energy, and about the lack of planning as to how this research might be continued after the war. It was clear that the project set up during the war would not be continued but would have to be reorganized. But the valuable thing was not the big projects; the valuable things were the numerous teams, which somehow crystallized during the war, of men who had different abilities and who liked to work with each other. We thought that these teams ought to be preserved even though the projects might be dissolved.

We did not quite understand why we had been sent by the President to see James Byrnes. He had previously occupied a high position in the government, but was now out of the government and was living as a private citizen in Spartanburg. Clearly the President must have had in mind appointing him to a government position, but what position? Was he to be the man in charge of the uranium work after the war, or what? We did not know.

Finally we arrived in Spartanburg, and I gave Byrnes Einstein's letter to read and the memorandum which I had written. Byrnes read the memorandum, and then we started to discuss the problem. When I spoke of my concern that Russia might become an atomic power—and might become an atomic power soon, if we were to demonstrate the power of the bomb and use it against Japan—his reply was, "General Groves tells me there is no uranium in Russia."

I told Byrnes that there was certainly a limited amount of rich uranium ore in Czechoslovakia to which Russia had access; but apart from this, it was very unlikely that in the vast territory of Russia there should be no low-grade uranium ores. High-grade uranium ore is, of course,

48. Harold C. Urey, then professor of chemistry at Columbia University.

of the committee has been published, and it was meant to be presented to the secretary of war, Mr. Stimson. Whether it ever reached his desk I do not know.

On my way from Spartanburg to Chicago I stopped in Washington to see Oppenheimer, who had arrived there to attend a meeting of the Interim Committee.[51] I told Oppenheimer that I thought it would be a very serious mistake to use the bomb against the cities of Japan. Oppenheimer didn't share my view. He surprised me by starting the conversation by saying that the atomic bomb is no good.[52] "What do you mean by that?" I asked him. He said, "Well, this is a weapon which has no military significance. It will make a big bang—a very big bang—but it is not a weapon which is useful in war." He thought it would be important, however, to inform the Russians that we had an atomic bomb and that we intended to use it against the cities of Japan, rather than taking them by surprise. This seemed reasonable to me, and I know that Stimson also shared this view. However, while this was necessary, it was certainly not sufficient. "Well," Oppenheimer said, "don't you think if we tell the Russians what we intend to do and then use the bomb in Japan, the Russians will understand it?" And I remember that I said, "They'll understand it only too well."

The time approached when the bomb would be tested. The date was never communicated to us in Chicago, nor did we ever receive any of-

---

was rushed to Stimson and advised against the outright military use of atomic bombs in the war against Japan. It took a stand in favor of demonstrating the power of the atomic bomb in a manner which will avoid mass slaughter but yet convince the Japanese of the destructive power of the bomb. By the beginning of July it became evident, at least to me, personally, that the use of the bomb will be examined by the Interim Committee purely on the basis of expediency, and that great weight will be given by them to the immediate effect, rather than to the long range effects.

51. The Interim Committee was organized in early May of 1945 by Secretary of War Henry L. Stimson to consider uses of the bomb and possible international control. He was chairman; members were Bush, Conant, Karl T. Compton, Under Secretary of the Navy Ralph Bard, Assistant Secretary of State William Clayton, and as the personal representative of President Truman, James Byrnes, who at that point held no official position. Robert Oppenheimer, director of the Los Alamos laboratory, was on the scientific advisory panel to the Interim Committee, whose other members were Arthur Compton, Fermi, and Lawrence. Richard G. Hewlett and Oscar E. Anderson, Jr., *The New World, 1939/1946: A History of the United States Atomic Energy Commission* (University Park, Pa., 1962–), I, 344–346.

52. A stronger word was used in the tape.

ficial indication of what was afoot. However, I concluded that the bomb was about to be tested when I was told that we were no longer permitted to call Los Alamos over the telephone. This could mean only one thing: Los Alamos must get ready to test the bomb, and the Army tried by this ingenious method to keep the news from the Chicago project.

I knew by this time that it would not be possible to dissuade the government from using the bomb against the cities of Japan. The cards in the Interim Committee were stacked against such an approach to the problem. Therefore all that remained was for the scientists to go unmistakably on record that they were opposed to such action. While the Franck Report argued the case on grounds of expediency, I thought the time had come for the scientists to go on record against the use of the bomb against the cities of Japan on moral grounds. Therefore I drafted a petition which was circulated in the project.[53]

This was again violently opposed by the Army. They accused me of having violated secrecy by disclosing in the petition that such a thing as a bomb existed. What the Army thought that we thought we were doing all this time, I cannot say. However, we did not yield to the Army's demand. The right to petition is anchored in the Constitution, and when you are a naturalized citizen you are supposed to learn the Constitution prior to obtaining your citizenship.

The first version of the petition which was circulated drew about fifty-three signatures in the Chicago project. What *is* significant is that these fifty-three people included *all* the leading physicists in the project and many of the leading biologists. The signatures of the chemists were conspicuously absent. This was so striking that I went over to the chemistry department to discover what the trouble was. What I discovered

53. In "The Story of a Petition" Szilard wrote, "A petition to the President was thus drafted in the first days of July and sent to every group leader in the 'Metallurgical Laboratory,' with the request to circulate it within his group." Szilard's covering letter to the group leaders is especially intense on the moral position, raising the analogy of individual Germans' guilt for Germany's acts. The text of this letter, dated July 4, 1945, appears below as Appendix III. The first version of the petition was dated July 3, 1945, and was signed by fifty-nine scientists. The final paragraph states: "In view of the foregoing, we, the undersigned, respectfully petition that you exercise your power as Commander-in-Chief to rule that the United States shall not, in the present phase of the war, resort to the use of atomic bombs." The text of the petition is printed in full below, as Appendix IV.

In his book, *Speaking Frankly*, James Byrnes relates that when he became secretary of state he tried to find out how long it would take Russia to develop a bomb. He needed this information in order to evaluate proposals for the control of atomic energy. He reports in his book that, from the best information which he could gather, he concluded that it would take Russia seven to fifteen years to make the bomb. He adds that this estimate was based on the assumption that postwar recovery would be faster than it actually was, and therefore he thinks that this estimate ought to be revised upward rather than downward. Dr. Conant, Dr. Bush, and Dr. Compton all estimated that it would take Russia perhaps fifteen years to make the bomb. Why this should be so is not clear, though it is of course possible to contrive a psychological explanation for these overestimates. If you are an expert, you believe that you are in possession of the truth, and since you know so much, you are unwilling to make allowances for unforeseen developments. This is, I think, what happened in this case.

APPENDIX I

A. LETTER OF TRANSMITTAL, SZILARD TO DR. ALEXANDER SACHS, AUGUST 15, 1939

Dear Dr. Sachs:

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

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

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

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

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

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

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

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

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

Yours sincerely,

B. MEMORANDUM, SZILARD TO THE PRESIDENT, AUGUST 15, 1939

Much experimentation on atomic disintegration was done during the past five years, but up to this year the problem of liberating nuclear energy could not be attacked with any reasonable hope for success. Early this year it became known that the element uranium can be split by neutrons. It appeared conceivable that in this nuclear process uranium itself may emit neutrons, and a few of us envisaged the possibility of liberating nuclear energy by means of a chain reaction of neutrons in uranium.

Experiments were thereupon performed, which led to striking results. One has to conclude that a nuclear chain reaction could be maintained under certain well defined conditions in a large mass of uranium. It still remains to prove this conclusion by actually setting up such a chain reaction in a large-scale experiment.

This new development in physics means that a new source of power is now being created. Large amounts of energy would be liberated, and large quantities of new radioactive elements would be produced in such a chain reaction.

In medical applications of radium we have to deal with quantities of grams; the new radioactive elements could be produced in the chain reaction in quantities corresponding to tons of radium equivalents. While the practical application would include the medical field, it would not be limited to it.

A radioactive element gives a continuous release of energy for a certain period of time. The amount of energy which is released per unit weight of material may be very large, and therefore such elements might be used—if available in large quantities—as fuel for driving boats or airplanes. It should be pointed out, however, that the physiological action of the radiations emitted by these new radioactive elements makes it necessary to protect those who have to stay close to a large quantity of such an element, for instance the driver of the airplane. It may therefore be necessary to carry large quantities of lead, and this necessity might impede a development along this line, or at least limit the field of application.

Large quantities of energy would be liberated in a chain reaction, which might be utilized for purposes of power production in the form of a stationary power plant.

In view of this development it may be a question of national importance to secure an adequate supply of uranium. The United States has only very poor ores of uranium in moderate quantities; there is a good ore of uranium in Canada where the total deposit is estimated to be about 3000 tons; there may be about 1500 tons of uranium in Czechoslovakia, which is now controlled by Germany; there is an unknown amount of uranium in Russia, but the most important source of uranium, consisting of an unknown but probably very large amount of good ore, is Belgian Congo.

It is suggested therefore to explore the possibility of bringing over from Belgium or Belgian Congo a large stock of pitchblend, which is the ore of both radium and uranium, and to keep this stock here for possible future use. Perhaps a large quantity of this ore might be obtained as a token reparation payment from the Belgian Government. In taking action along this line it would not be necessary officially to disclose that the uranium content of the ore is the point of interest; action might be taken on the ground that it is of value to secure a stock of the ore on account of its radium content for possible future extraction of the radium for medical purposes.

Since it is unlikely that an earnest attempt to secure a supply of uranium will be made before the possibility of a chain reaction has been visibly demonstrated, it appears necessary to do this as quickly as possible by performing a large-scale experiment. The previous experiments have prepared the ground to the extent that it is now possible clearly to define the conditions under which such a large-scale experiment would have to be carried out. Still two or three different setups may have to be tried out, or alternatively preliminary experiments have to be carried out with several tons of material if we want to decide in advance in favor of one setup or another. These experiments cannot be carried out within the limited budget which was provided for laboratory experiments in the past, and it has now become necessary either to strengthen —financially and otherwise—the organizations which concerned themselves with this work up to now, or to create some new organization for the purpose. Public-spirited private persons who are likely to be interested in supporting this enterprise should be approached without delay, or alternatively the collaboration of the chemical or the electrical industry should be sought.

The investigations were hitherto limited to chain reactions based on the action of *slow* neutrons. The neutrons emitted from the splitting uranium are fast, but they are slowed down in a mixture of uranium and a light element. Fast neutrons lose their energy in colliding with atoms of a light element in much the same way as a billiard ball loses velocity in a collision with another ball. At present it is an open question whether such a chain reaction can also be made to work with *fast* neutrons which are not slowed down.

There is reason to believe that, if fast neutrons could be used, it would be easy to construct extremely dangerous bombs. The destructive power of these bombs can only be roughly estimated, but there is no doubt that it would go far beyond all military conceptions. It appears likely that such bombs would be too heavy to be transported by airplane, but still they could be transported by boat and exploded in port with disastrous results.

Although at present it is uncertain whether a fast neutron reaction can be made to work, from now on this possibility will have to be constantly kept in mind in view of its far-reaching military consequences. Experiments have been devised for settling this important point, and it is solely a question of organization to ensure that such experiments shall be actually carried out.

Should the experiments show that a chain reaction will work with *fast* neutrons, it would then be highly advisable to arrange among scientists for withholding publications on this subject. An attempt to arrange for withholding publications on this subject has already been made early in March but was abandoned in spite of favorable response in this country and in England on account of the negative attitude of certain French laboratories. The experience gained in March would make it possible to revive this attempt whenever it should be necessary.

APPENDIX II

ENCLOSURE TO MR. ALBERT EINSTEIN'S LETTER OF MARCH 25, 1945, TO THE PRESIDENT OF THE UNITED STATES, BY L. SZILARD

The work on uranium has now reached a stage which will make it possible for the Army to detonate atomic bombs in the immediate future. The "demonstration" of such bombs may be expected rather soon and naturally the War Department is considering the use of such bombs in the war against Japan.

From a purely military point of view this may be a favorable development. However, many of those scientists who are in a position to make allowances for the future development of this field believe that we are at present moving along a road leading to the destruction of the strong position that the United States hitherto occupied in the world. It appears probable that it will take just a few years before this will become manifest.

Perhaps the greatest immediate danger which faces us is the probability that our "demonstration" of atomic bombs will precipitate a race in the production of these devices between the United States and Russia and that if we continue to pursue the present course, our initial advantage may be lost very quickly in such a race.

If a nation were to start now to develop atomic bombs, so to speak from scratch, it could do so without reproducing many of the expensive installations which were built by the War Department during the War. *For over a year now we have known that we could develop methods by means of which atomic bombs can be produced from the main component of uranium which is more than one hundred times as abundant than the rare component* from which we are manufacturing atomic bombs at present. We must expect that a cost of about $500 million some nations may accumulate, within six years, a quantity of atomic bombs that will correspond to ten million tons of TNT. A single bomb of this type weighing about one ton and containing less than 200 pounds of active material may be expected to destroy an area of ten square miles. Under the conditions expected to prevail six years from now, most of our major cities might be completely destroyed in one single sudden attack and their populations might perish.

In the United States, thirty million people live in cities with a population of over 250,000 and a consideration of this and other factors involved indicates that the United States will be much more vulnerable than most other countries.

Thus the Government of the United States is at present faced with the