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Interview with L. Szilard on History of

February 12, 1945

Present: L. L. Szilard, K. K. Darrow, R. S. Mulliken

Szilard: I thought of the possibility of chain reaction in 1933-34. In 1933 Rutherford made a statement about releasing nuclear energy. At that time a mass effect of Be was measured by Bainbridge. He mass was already known. I thought, why does not Be disintegrate, and perhaps if one would "tickle" Be with a neutron it would disintegrate. I calculated the critical size of a chain reacting system; calculated the critical mass for Be.

In March ~~1944~~ 1934 (K.W.) I started to write these findings down. It turned out that the mass of He was wrong and the mass of Be was right. Actually it wasn't possible to make a chain reaction using Be. I thought of disintegration of Be with gamma rays. Found definite threshold, began to suspect that something was wrong. Later Bethe and Oliphant came to conclusion that Be mass was wrong. I did not give up the idea. Photo-disintegration was method of testing mass. It seemed that there should be a liberation with gamma rays. There was. I thought U, Th, or even In might be used. 3½ hours in indium. We now know it is an excited state, did not know it then. Did not know that it was a stable atom. This was first case of ~~a long-lived~~ nuclear isomerism. Later on a similar situation was found in bromine.

Darrow restates Szilard's remarks:

In 1934, Dr. Szilard calculated from the available data on the mass of the Be nucleus that this mass was greater than the sum of the masses of two alpha particles and one neutron, and that therefore it should be possible

to detach the neutron by tickling the nucleus. At this time he first conceived the idea of a chain reaction, and worked with the size of a sphere of Be sufficient to bring about such a chain reaction. The experiment was tried with γ rays rather than neutrons, and it was found that there was a sharp threshold at 1.6, which indicated that the mass was after all smaller than the sum of the masses of the particles composing it as was later to be confirmed by better measurements of the mass of the α particle.

Szilard: Designed an experiment at Oxford which I never carried out there. I thought of detecting such a neutron emission by using an unknown element to emit neutrons which would be fast, then detecting by an ionization chamber.

Darrow restates:

It then occurred to Dr. Szilard to use Ra-γ-Be neutrons to bombard various elements from which he might expect to expel fast neutrons and detect these with a detector sensitive only to fast neutrons. Preparations were made for this experiment at Oxford, but were interrupted by departure of Dr. Szilard for America.

Szilard: In 1935 I sent a patent to the British Navy. It is first document pertaining to concepts of chain reaction. I thought this knowledge should be kept secret as I felt it was of military importance and so to do this in Britain one had to send such a paper to the government. (A corresponding American patent application was filed in March 1935, but British claimed the idea was Secret-Secret and parts relating to chain reaction had to be withdrawn.) By the winter of 1938 I had found that Be, Indium did not work. I worte a letter to the British Navy suggesting that the paper be dropped as by then I thought it useless. Before the letter reached them, I sent a telegram asking them to disregard the letter when it arrived because meantime fission had been discovered.

Szilard: I came to see Wigner at Princeton about two weeks or ten days before the meeting in Washington, D. C. (one of Gamov's meetings). I sent to Britain for a Be block to use as a gamma ray source. During the Washington meeting I was ill. Later I went to see Rabi and told him of the importance of the discovery of fission. Suggested to Rabi that he talk with Fermi and emphasize the necessary secrecy of any experiments to be carried out. After their discussion, I asked Rabi what was Fermi's reaction, and Rabi said that Fermi had said "Nuts!" But both Rabi and I then talked with Fermi and we finally got him to say that there was a remote possibility. I told Fermi about the experiment I had planned. Fermi discussed his own experiment.

Darrow restating:

Szilard then planned to perform the experiment which had been abandoned by his leaving Oxford. He tried to sell Fermi the idea of using photo neutrons. [(Fermi, however, preferred to use the available Ra-Be source and slow down the Be neutrons in water(--Szilard breaks in)]. However, Fermi's interest was extremely mild and accordingly Szilard and Zinn nevertheless went ahead with Szilard's method involving the use of photo-neutrons. Between March 1 and March 3, 1939, they got positive results. This was later written up and published in the Physical Review.

On the morning of March 4, 1939, Szilard visited Fermi who in the meantime had been working with his own method and had got indications of the positive results. Fermi's experiment was designed in hope of showing that U produces neutrons in excess of the number of neutrons absorbed. Szilard's experiment did not show absorption of neutrons. It was then realized that Fermi's experiment did not exclude the possibility of (n, 2n) reactions which are not chain reacting. Fermi then conceded that Szilard was right, used photo-neutrons himself and found fast neutrons appearing at distances greater than the photo-neutrons could reach.

Szilard: From March to April 1939, I was agitating for non-publication of these various experiments. On February 2, 1939, I wrote to Joliot to dissuade him from publishing any possible results which he might have found. I knew that Joliot was a clever man and that he was probably carrying on some experiments of his own after the news of fission became known. Fermi was willing not to publish if there was sufficient backing in this country. Szilard met Fermi and Teller in Washington. Later in New York, Fermi had told Wigner we should not publish anything. On March 20, 1939, I learned that Joliot had published. I still wanted to continue not publishing even after Joliot had published. I thought we could finally bring him around to our side before much harm was done. Wigner and Teller supported this view. (Mentions much correspondence between scientists of several countries about question of publishing). Joliot answered finally, saying "The question has been studied."

Darrow restates:

Many efforts were made to induce people to suspend publication.

Szilard: Joliot based his refusal to publish on the appearnce of new stories in America about fission. Neutron emission/and possibility of delayed neutron emission had been discussed at Washington conference. We finally agreed to leave the whole decision up to Wigner; he decided in favor of publication. After that, the question was, how can we decide whether a chain reaction can be maintained in a water system? Fermi, Anderson and I made a joint experiment which was published in Physical Review. The experiment consisted of a lattice of Uranium oxide in water. Some manganese sulphate was dissolved in the water. We measured the number of neutrons in the water with and without uranium. We found more neutrons were produced than were absorbed by using uranium. Placzek visited us and asked what we were doing. Suggested we use helium and not water. His idea was that if we used a greater proportion of uranium we might find resonance- absorption was so great as to

destroy the evidence of a chain reaction. The original experiment proved that more neutrons are produced than are absorbed by using uranium, not more neutrons are produced than are absorbed by using uraning plus water. At that time we were working against a deadline because Fermi had to leave in June for Ann Arbor. We modified our aims and agreed that Fermi should calculate what is absorbed by resonance in the experiment. We needed that value, which he calculated to be 1.5. We wanted to at least get the number of neutrons emitted by uranium to the ratio of neutrons absorbed. Fermi noticed that resonance absorption was smaller than in a homogeneous mixture. Thought he could change ~~the~~ dimensions. By that time I lost all interest in water. I began to dream of the graphite system.

Darrow restates:

Up to and including part of June 1939, Fermi was working with lumps of uranium in water and was varying the sizes of the lumps in the hope of minimizing the resonance absorption. Meanwhile Szilard had become totally discouraged with the use of water and was beginning to contemplate the use of carbon. In the first ten days of July, Szilard became convinced that the chain reaction could be accomplished with graphite. This is attested by letters from Szilard to Fermi dated July 3 and July 8, 1939, in which at first a method of measuring the cross section of carbon was proposed to find out whether this cross section was so small as to be really promising. Then it was suggested that an attempt be made to produce a chain reaction in a mixture of carbon and uranium without waiting for the outcome of the foregoing experiment.

(.3 is now the cross section of hydrogen.)

Fermi received Szilard's proposal with very tempered enthusiasm ~~on the basis that he~~ because he had made calculations on the homogeneous mixture -- which was the wrong thing to do.

Szilard: Does not want following remarks included in which he tells of necessity for haste and slowness of negotiations between committee which the

President appointed (Briggs chairman) and Dean Pegram at Columbia. Mentions that from June 1939 until March 1940 Fermi has been working on cosmic rays, and all that time nothing had been done on fission. By middle of February 1940 they were still not able to get a statement as to whether or not the graphite would be ordered for them. To push things along Szilard wrote up his results and opinions at the time and sent them to the Physical Review with instructions not to publish until notified to do so. This unpublished article is essentially the report A-55. Meanwhile Fermi calculated the lattice experiment, and confirmed Szilard's view that one gains a lot by using lattice in a homogeneous system.

Darrow restates:

In March 1940, graphite arrived and experiments were started in a cube of pure graphite such as is now called a Σ pile. The experiment as was carried out was not an accurate one. Got .03 for cross section. The value found, although inaccurate, was low enough to give optimism. The theory at the time was still in an incipient state but in June 1940 Wigner proposed the method of computing thermal utilization, which perhaps with slight modifications is still employed. Wigner suggested boundary conditions but Fermi worked out the actual computation and he and Teller worked it out in the summer of 1940. The assumption about resonance absorption was that it takes place an surface of uranium. This assumption was made in Szilard's paper. Improvement was made over thermal absorption mentioned in Szilard's paper, no improvement was made over resonance absorption. The net result was worse than if it had not been attempted.

Interview adjourned

until Feb. 13, at 2:00 P.M.

Szilard: Late in 1941 the next advance was made by Mr. Wigner. Mr. Wigner proved his point that the resonance absorption takes place not only in the thin layer of a surface of the sphere but takes place throughout the whole mass of the sphere. That is that the mass absorption term is important and in certain circumstances it is the dominating term.

Darrow: In other words this question of resonance bears on the evaluation of p?

Szilard: Yes.

This conclusion reached by Mr. Wigner was supported by measurements of Szilard and Marshall who found a large capture cross section for photo-neutrons in U^{238}. Marshall and Szilard had also shown in the fall of 1941 that fission is produced in U^{238} by fission neutrons which indicated that fast neutron fission in U^{238} may be an important correction in the neutron balance of the chain reaction.

Darrow: Was this the first observation of fast fission?

Szilard: Was first observation of fission in U^{238} induced by fission neutrons. Slow neutrons cannot produce fission in U^{238}.

Thus by end of 1941 all factors which enter into calculating the neutron balance of the chain reaction in the uranium graphite system were known in principle although the accuracy of some of these factors was still not very great.

Darrow: By the factors do you mean specifically η, ϵ, p, and f?

Szilard: Yes.

In the spring of 1941 enough uranium oxide and graphite was accumulated to make an empirical determination of the multiplication factor of a uranium oxide graphite lattice. This was done by Mr. Fermi who designed a method called the exponential experiment which applies

to a uranium lattice the same principles as were used for determining the graphite absorption in the Σ pile experiments.

Darrow: Asks if by empirical Mr. Szilard means empirical method signifies getting k by use of the formula involving the Laplacian and the migration area. Szilard agrees to this statement.

Szilard: Throughout 1941 my main concern was to make arrangements for obtaining materials in the required purity and in making provisions for the future supply of pure graphite and uranium metal. It did not seem probable that any important results could be based on the use of uranium oxide.

Another point of concern was the possibility that fast neutron chain reaction might be possible in uranium metal. That possibility, if it had been real, would have constituted a very important danger to the security in this country. In order to increase our knowledge in this respect, Mr. Zinn and I measured the inelastic scattering cross section of uranium. The value which we found was so high that it effectively reassured us that fast neutron chain reaction in all uranium metal appeared to be a rather unlikely possibility.

A very important step in clarifying the over-all picture was a manuscript which was sent to us by Mr. L. A. Turner in the spring of 1940. This drew attention to the importance of element 94 and contained particular significance after the work of Abelson and McMahon made it appear almost certain that 94 can be chemically produced and precipitated.

Darrow: This is very interesting. If Turner had not found these results, you feel your work might now have progressed as it did?

Szilard: Yes. Turner's activity encouraged me to hope that U^{238} would be ultimately utilized in place of U^{235}. It also became clear that one could produce element 94 and use it for any purposes for which

U^{235} might be used. In spite of this, to my knowledge, no one in this country had ever made any estimate up to the middle of 1941 which would show that amounts of U^{235} or 94 which we could hope to manufacture would be sufficient for the construction of bombs that would be detonated on the basis of fast neutron chain reaction. In order to estimate the amount needed for such a bomb one had to know the fission cross section of U^{235} for photo-neutrons. An experiment determining this cross section had in fact been performed by Mr. Zinn and myself as early as the summer of 1939. However, our measurements were never properly evaluated since up to the middle of 1941 we were not aware of the fact that there was a good prospect of separating the U^{235} isotope. This was due to the compartmentalization of information which prevented Mr. Fermi and I from talking about this work with Mr. Urey. Thus the fact that atomic bombs are a practical possibility was not brought to the attention of this (U.S.) government by anyone in this country, but was communicated by the British government and, so far as I know, was due to the collaboration of Frisch and Peierls, one of whom carried out the neutron measurements and the other of whom was concerned with the practicability of the separation of U^{235}.

Darrow: Asks if British made any publications about this.

Szilard: No. This brings the information up to the beginning of the Chicago Project in 1941.

Hist D 1a

Szilard

The thought that such a thing as a nuclear chain might be possible first occurred to me in 1933~~1934~~. At that time the mass of beryllium as measured by Bainbridge indicated that beryllium is a metastable element which could potentially disintegrate with a release of energy into two alpha-particles and a neutron. It appeared conceivable that a slow neutron when interacting with a beryllium nucleus might induce such a disintegration and that in a sufficiently large structure of beryllium a nuclear chain reaction could be maintained. It appeared also conceivable that other elements such as thorium and uranium might be metastable in the same sense as beryllium, ~~and~~ In 1934 I worked out the differential equation which controls the diffusion of neutrons and which determines the critical dimensions in such a chain reacting ~~mass~~ structure. These considerations were written down in the course of 1934 in the form of British patent applications and the resulting patent was assigned to the British Admiralty and was sealed secret.

It appeared likely that beryllium, if it was metastable, could be split by gamma rays of radium so that beryllium exposed to gamma rays of radium would emit neutrons which could be detected by virtue of the radioactivity which they would induce. Mr. Chalmers and I found in fact such a neutron emission from beryllium under the action of gamma rays of radium (at present called photo neutrons) but subsequent experiments ~~carried~~ conducted out in collaboration with Bretscher and Lange showed that the photodisintegration of beryllium had an energy threshhold of about 1.6 million volts ~~which was~~ this was difficult to reconcile with the condeption that beryllium was a metastable element which could sustain a chain reaction. ~~XXX~~ Subsequently Bethe and Oliphant showed that Astons mass of the alpha particle was in error and there was no longer any ~~further~~ reason to suspect beryllium of being metastable.

I did not, however, give up the idea that other elements might be metastable and might release fast neutrons when interacting with neutrons so that the chain reaction might be maintained. I had no conception of the possibility of fission, however, and was therefore unable to think of a mechanism which would permit such a neutron emission except by assuming that there might be neutrons of mass number 2 or 4 having a sufficient binding energy to make it possible for them to be emitted in an exothermic process from certain nuclei when they capture a neutron.

I had now the plan of making a systematic search for some such neutron emission by using the photoneutrons from beryllium as a tool. These photoneutrons have a sharp upper limit to their energy and if fast secondary neutrons were emitted by any element which is exposed to these photoneutrons the secondary neutrons could be distinguished from the primary neutrons by virtue of their higher energies. In preparation for such experiments I had cast a cylindrical beryllium block of 6 cm diameter and 6 cm high with a cylindrical hole in the center so that one gram of radium could be inserted in the center giving a strong source of photoneutrons. I also had built a linear amplifier in order to detect such fast secondary neutron emission by amplifying the pulses induced by means of the hydrogen recoils in an ionization chamber. All this equipment was at Oxford but the amplifier was never actually made to work and the plan for such a systematic search was virtually abandoned before I came to the United States.

If first heard of the discovery of fission from Wigner at Princeton during the first half of January, about ten days before the Washington meeting, and told him I expected this phenomenon to be accompanied by a neutron

emission which should be easily observable if the fission were induced by photoneutrons slowed down by paraffin. I cabled to Oxford requesting them to send the beryllium block which had been procured for the purpose and after a short illness contacted Rabi, Pegram, and Fermi at Columbia with proposals which were based on the assumption that a chain reaction might be set up in uranium. Fermi, who had independently thought of a neutron emission accompanying fission ~~intended to see whether this phenomenon existed,~~ planned an experiment using radium-beryllium neutrons from which he ~~xxx~~ hoped he might be able to conclude whether there is an excess of neutron emission over neutron absorption from uranium by measuring the total number of neutrons emitted from a sphere of uranium oxide when a radium-beryllium neutron source is placed at the center.

Fermi had independently thought of the possibility of a neutron emission accompanying fission and discussed this possibility at the Washington meeting in January. Soon afterwards ×× → U absorption

The beryllium block having arrived from Oxford, I obtained a gram of radium and with this photoneutron source Zinn and I irradiated uranium oxide and found that fast neutrons (about 2 per fission) were emitted from the uranium. This experiment was performed on March 3 and 4, 1939.

About the same time Fermi was engaged in an experiment that was also designed to obtain information concerning a possible neutron emission from fission. Fermi and Anderson used a fast neutron radium-beryllium source, slowed down the neutrons in a water tank, and measured the thermal neutron density in the tank as a function of the distance from the source. He then surrounded his neutron source with a sphere of uranium oxide and by measuring the thermal neutron and again measured the thermal density. These two measurements were supposed to show whether or not the uranium brought about an increase in the total number of neutrons slowed down and absorbed in the water. Such an increase would have meant that uranium emits neutrons in fission and also showed that more neutrons are emitted than absorbed in uranium under certain conditions. This would have been a favorable omen for the possibility of a chain reaction in a uranium-water system.

Fermi and Anderson indeed observed an increase in the number of thermal neutrons but they were unable to interpret the result because they could not exclude the possibility of an n, 2n reaction caused by the fast neutrons of their neutron source. The photoneutron source in used in my

experiments with Zinn was then turned over to Anderson and Fermi and by using this source they were able to demonstrate that for large distances from the source there was a very pronounced increase in the neutron density if they introduced uranium oxide into their system which they considered conclusive proof of the fact that neutrons are emitted in fission by uranium which are more energetic and therefore have a longer range than the photo-neutrons of the source.

3. In May and June 1939 Anderson and Fermi and I performed an experiment in which we observed the number of neutrons emitted from a cylindrical lattice of uranium oxide filled tubes. This lattice was immersed in water and was exposed to a photoneutron source in the center of the arrangement. We found that the number of neutrons which were slowed down and absorbed in water was about 20% greater in the presence of the uranium lattice than in its absence. While we were doing this experiment we had hoped that such a result might enable us to conclude that the chain reaction could be maintained in a water-uranium system since the result meant that more neutrons are emitted by uranium under the conditions of this experiment than are absorbed by uranium. Of course, a very large fraction of the neutrons were absorbed by the water, but we tacitly assumed that by increasing the amount of uranium and reducing the amount of water the fraction of neutrons absorbed by the water could be reduced as much as desired. While we were engaged in the measurement Placzek came to visit us and drew our attention to the fact that no such conclusion would be possible on the basis of our experiment. He pointed out that if we increase the amount of uranium and decrease the amount of water we would increase the fraction of neutrons absorbed by uranium at

resonance and thereby decrease the number of neutrons emitted by uranium per neutron absorbed by uranium. Placzek was skeptical about the possibility of having a chain reaction in a uranium-water system and advocated the use of helium for slowing down the neutrons in the place of hydrogen.

Realizing that we could not decide by means of our experiment whether or not a chain reaction could be maintained in uranium and water experiments, we decided to make use of the experiment for calculating the number of neutrons emitted from uranium per thermal neutron absorbed by uranium, a number which is a constant of uranium and which is independent of the water-uranium ratio. In order to do this we had to calculate the fraction of the neutrons which was absorbed at resonance by uranium under the conditions of our experiment and this calculation was actually carried out by Fermi. The calculation showed that the fraction of neutrons absorbed by uranium at resonance in our system was smaller than the value that would hold for the corresponding homogeneous mixture of uranium and water and this observation led Anderson and Fermi to hope that perhaps by skillfully choosing the characteristics of such a heterogeneous water system one could obtain conditions in which a chain reaction could be expected to go in a uranium water system. The advantage which they were able to obtain over a homogeneous water system was fairly small and not sufficient to enable them to conclude that a chain reaction could be maintained in any such system. While Anderson and Fermi were engaged in these calculations I became interested in using graphite as a slowing down agent in place of water. Between July 1 and July 15 I reached the conclusion that there was a very good chance of maintaining a chain reaction in graphite, that the use of a heterogeneous

system, particularly a lattice of uranium metal spheres in graphite gave an enormous advantage in the multiplication factor in graphite and that graphite in the required purity was available at a moderate price. I devised an experiment for measuring the diffusion length in graphite (rather than measuring the absorption cross section of carbon. While I wanted to perform this experiment I pressed for making arrangements for a large scale experiment with a lattice of uranium and graphite without waiting for the outcome of the measurement of the graphite absorption. During this period I bombarded Fermi with letters on this subject and had conversations with Pegram, Wigner, Teller and Einstein. I was so optimistic about the possibility of using graphite that based upon this technique an approach was made to the U. S. Government which led to the appointment of the Uranium Committee under the chairmanship of Lyman J. Briggs, which on October 21, 1939 promised to supply four tons of graphite for the proposed absorption measurement.

My optimism with respect to graphite was based on the view that a water-uranium system, even though not chain reacting, may come fairly close to the multiplication factor of 1. From the slowing down and scattering properties of carbon which were known it was possible to deduce that without taking full advantage of the properties of a heterogeneous uranium-graphite system the multiplication factor of a carbon-uranium system would be about the same as of the best uranium-water system if the carbon absorption cross section were .001. This value, however, was the measured upper limit of the carbon absorption cross section and one could hope that the actual carbon absorption would be below this upper limit.

Fermi wrote in July that he had also considered carbon as one of the

possibilities. It seems that Fermi considered a system containing very little uranium and very large amounts of carbon and therefore came to the conclusion that the system would be only chain reacting if the carbon absorption were very low. Fermi told me in September that he had actually calculated a homogeneous system which he of course knew to be less good than a heterogeneous system. He did not think at that time that the advantage of the heterogeneous system would be sufficient to give a material change in the overall picture.