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BIOGRAPHICAL DATA AND LIST OF PUBLICATIONS OF LEO SZILARD FROM 1922 to 1945

I obtained my doctor's degree in Physics at the University of Berlin in 1922.

My dissertation showed that the second law of thermodynamics permits to draw conclusions on the laws that control thermodynamics fluctuations. This dissertation was published in 1925.

(1) Zeitschrift fur Physik, 1925, p. 753, 32.

Subsequently I worked for about two years with H. Mark at the Kaiser Wilhelm Institute fur Faserstoffchemie in Berlin Dahlem. Our work on the anomalous scattering of X-rays in crystals and on the polarization of X-rays by reflection on crystals, resulted in two papers:

(2) Zeitschrift fur Physik, 1925, p. 688, 33.

(3) Zeitschrift fur Physik, 1926, p. 743, 35.

Subsequently I was, for three years, Assistant at the Institute fur Theoretische Physik at the University of Berlin.

During this period I investigated the decrease in entropy which appears to occur in a system in which variables that are subject to thermodynamic fluctuations are "observed," and used to control operations to which the system is subjected. This led me to the recognition of the connection between "entropy" and "information" and a theorem which now forms part of modern "information theory."

The resulting paper was accepted as habilitationschrift at the University of Berlin and I was appointed Privatdozent fur Physik. This paper was published in 1929:

(4) Zeitschrift fur Physik, 1929, p. 840, 35.

After working for three years as Assistant at the Institute fur Theoretische Physik I held a Forshhungsstipendum for one year and worked on problems of quantum theory. This work did not result in any published paper.

In the meantime jointly with Professor Albert Einstein I thought of a method of pumping liquid metals through tubes through the action of a moving magnetic field on electric currents induced by this field in the liquid metal. The German General Electric Co. (A.E.G.) wanted to develop a pump based on this principle and for abâut three years I acted as a consultant to them for this development. In 1932 the A.E.G. discontinued this development and not until the introduction of atomic reactors in 1942 did this system find application.

In 1932 my interest shifted to nuclear physics and I moved to the Harnack House in Berlin Dahlem with the thought of taking up some experimental work in one of the Kaiser Wilhelm Institutes there. I discussed the possibility of doing experiments in nuclear physics with Miss Lisa Meitner in the Kaiser Wilhelm Institute fur Chemie, but before this led to a final donclusion one way or the other, the political situation in Germany became tense and it seemed advisable to delay a final decision.

I was still at the Harnack House at the time of the Reichstagsbrand in March 1933 but soon thereafter went to England. The laws promulgated by the National Socialist Government barred me from holding a University position in Germany by virtue of my "non-Aryan" descent and I remained in England until 1938.

I began my work in nuclear physics in the summer of 1934 as a guest of St. Bartholomew's Hospital in London. Thereo, together with Chalmers, we developed a method (Szilard Chalmers separation) for the concentration of radioactive elements produced by neutrons. This method is used if a radioactive element has to be separated from the bulk of the stable element from which it is produced and with which it is chemically isotopic. This work was published in 1934:

(5) "Chemical Separation of the Radioactive Element from its Bombarded Isotope in the Fermi Effect" -- Szilard and Chalmers. Nature, p. 462, 134, 1934.

Together with Chalmers I found that Gamma Rays from radium eject neutrons from beryllium. These photoneutrons of beryllium are of low energy. (In 1939 when I investigated with Zinn whether uranium emits neutrons in the fission process, the use of these slow photo neutrons as a primary neutron source made it possible for us to distinguish the fact neutrons emitted in the fission process from the primary neutrons.)

The discovery of the photoneutrons from beryllium was published in 1934.

(6) "Detecting Neutrons Liberated from Beryllium by Gamma Rays," Szilard and Chalmers, Nature, p. 494, 134, 1934.

In 1934 it was generally believed that the mass of beryllium atoms was sufficient to permit its spontaneous disintegration into two alpha particles and a neutron. Since such spontaneous disintegration did not occur it seemed important investigate the energy threshold for a photo disintegration of beryllium X-rays. This was done jointly with six other authors. The threshold for photo-neutrons from beryllium we determined by varying the voltage of an X-ray tube and found it to be somewhat about 1.5 and well below m.e.v. This information was partially responsible for inducing Bethe to revise the accepted mass of He and thereby to resolve the paradox of the stability of beryllium. Our paper was published in 1934:

(7) "Liberation of Neutrons from Beryllium by X-rays -- jointly with a group of six others, Nature, p. 880, 134, 1934.

Working with Chalmers I found that indium which has only two isotopes shows three radioactive periods when bombarded by neutrons. This was the first case of isomerism found among the artificial radioactive elements and we recognized its importance. Subsequently, it was possible for me to show [see (11)] that one period is due to an excited indium nucleus which is isomeric with the stable indium nucleus 115. Our r sults obtained in 1934 were published in 1935:

(8) "Radioactivity Induced by Neutrons" -- Szilard and Chalmers, Nature, p. 96; 135; 1935.

In June 1935 I accepted a position at the Clarendon Laboratory, Oxford.

Working at the Clarendon Laboratory, I discovered that if a slow neutron bean is filtered by cadmium, in order to remove the thermal neutrons, the residual neutrons show strong resonance absorptions at low energies in various elements. I was able to give the estimate of the energy region at which these resonances occurred and to state that the resonance energy can be determined by observing the absorption of their neutrons in boron or lithium. This work was published in 1935:

(9) "Absorption of Residual Neutrons," Nature, p. 136, 1935.

Working at the Clarendon Laboratory jointly with Griffith, I investigated the Gamma Ray emission that occurs when slow neutrons are absorbed by odd elements. This work was published in 1937:

(10) "Gamma Rays Excited by Capture of Neutrons," Nature, p. 323, 139, 1937 — Szilard and Griffiths.

In January 1938 I went to the U.S. on a visiting basis under a new arrangement with the Clarendon Laboratory. While I was there, I received an offer of a lectureship in nuclear physics at Oxford University. But in September 1938 I declined this offer, resigned from the Clarendon Laboratory, and shifted my residence to New York.

In 1938, using the Rochester cyclotron I found that one of the radioactive periods of indium which Chalmers and I had previously discovered was due to the nuclear excitation of the stable isotope 115. This conclusion reached jointly with Goldhaber and Hill, we published in 1939:

(11) "Radioactivity Induced by Nuclear Excitation" — Szilard, Goldhaber and Hill, Physical Revue, p. 47, 55, 1939.

After the discovery of fission by Han and Strassman, working as a guest at Columbia University, I found on March 3, 1939, jointly with Walter Zinn, that fast neutrons are emitted in the fission process and that their number is about two per fission. The neutron emission in the fission of uranium was discovered independently, and about the same time, by Halban, Joliot and Kovarsky, and by Anderson and Fermi. This discovery revealed that the lement uranium might sustain a chain reaction.

In our experiment, photo neutrons from beryllium were used as a primary neutron source and the fast neutrons emitted in the fission were made visible by using a hydrogen filled ionisation chamber, and by recording the recoil protons. Our results were published in 1939:

(12) "Instantaneous Emission of Fast Neutrons in the Intersection of Slow Neutrons with Uranium" -- Szilard and Zinn, Physical Revie, p. 799, 55, 1939.

(13) "Emission of Neutrons by Uranium" -- Szilard and Zinn, Physical Revue, p. 619, 56, 1939.

The discovery of the neutron emission in the fission of uranium raised the question whether a chain reaction can in fact be sustained in a system containing uranium. A system composed of water and uranium-oxide was investigated by me jointly with Anderson and Fermi in the spring of 1939. We reached the conclusion that such a system came close to beibg chain-reacting but could not be used for a self-sustaining chain reaction. Our results were publised in 1939:

(14) "Neutron Production and Absorption in Uranium" -- Physical Revue, p. 284, 56, 1939.

In July 1939 I reached the conclusion that a graphite uranium system is likely to support a self-sustaining chain reaction, and I derived an approximate formula for a lattice of uranium spheres embedded in graphite. My manuscript, entitled "Divergent Chain Reactions in a System Composed of Uranium and Carbon" was submitted to the Physical Revue on February 16, 1940, and was accepted for publication. Publication was indefinitely deferred at the request of the U.S. Government:

(15) "Divergent Chain Reaction in a System Composed of Uranium and Carbon" received by the Physical Revue, February 16, 1940 - unpublished.

The chain reaction based on this system was first demonstrated on December 2, 1942 at Stagg Field on the campus of the University of Chicago.

(See official U.S. reports "Atmonic Energy for Military Purposes," Henry D. Smythe, 1945, Princeton University Press, pages 34, 47, etc.) A patent describing the system for which Fermi and I applied in 1944, was granted in 1955 to the U.S. Atomic Energy Commission, and named Fermi and me as inventors.

(16) Fermi and Szilard, U.S. Patent No. 2,708,656

In November 1940 I became a member of the staff of the University under contract with the U.S. Government and Columbia University, given to the University "for the purpose of developing the system proposed by Fermi and Szilard." Early in 1942 the group was transferred to the University of Chicago where I was a member of the staff of the Metallurgical Laboratory of the University of Chicago which was the code name for the uranium project. I held the position of "Chief Physicist" in that Laboratory.

In October 1946, I was appointed as a full professor on the regular staff of the University of Chicago.