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LEO SZILARD BIOGRAPHICAL INFORMATION

Professor Leo Szilard of the University of Chicago helped unlock the forces of atomic energy, alert the nation to its military value and warn the world of its dangers.

Professor Szilard was a member of the original University of Chicago group led by the late Enrico Fermi which achieved mankind's first sustained nuclear chain reaction, December 2, 1942.

Szilard was instrumental in getting President Franklin D. Roosevelt to understand the military significance of atomic energy. He participated in drafting a letter for Albert Einstein's signature which urged the President to step up research on the development of an atomic bomb.

Leo Szilard, a professor of biophysics at the Enrico Fermi Institute for Nuclear Studies of the University of Chicago, was born in Budapest, Hungary, February 11, 1898.

He received his doctor of philosophy degree from the University of Berlin but left Germany in 1933 because of the Nazis to go to England. In 1934, he began work in the field of nuclear physics in London and later continued his research at the University of Oxford.

He settled in the United States in 1938 and became a citizen. He left England, he said, because of its "betrayal of Czechoslovakia" in the Munich Pact with Hitler.

Until November 1940, he worked at Columbia University.

During the next two years, Szilard worked with Fermi and other scientists to achieve the successful chain reaction in an atomic pile

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that was constructed under the now-demolished west stands of Stagg Field at the University of Chicago. Szilard was a key member of the metallurgical project at the University of Chicago which developed the atomic bomb.

In the fall of 1946, Szilard joined the faculty of the University of Chicago as a professor of biophysics. Szilard is married to the former Dr. Gertrud (cq) Weiss, a physician. He went on leave to work on a public health grant in New York City last year.

Szilard is a key figure in the history of atomic energy. Szilard described an experiment with Walter H. Zinn that took place on March 3, 1939 at Columbia University in these words:

> "After two days of preparation (Szilard wrote several years later), everything was ready and all we had to do was to turn a switch, lean back and watch the screen of a television tube. If flashes of light appeared on the screen, that would mean that neutrons were emitted in the fission process of uranium, and this, in turn, would mean that the large-scale liberation of atomic energy was just around the corner. We turned the switch and we saw the flashes. We watched them for a little while and then we switched everything off and went home. That night there was little doubt in my mind the world was headed for grief."

Szilard was one of a group of scientific leaders trying to warn the U.S. government of German progress in the late 1930's in solving the secrets of uranium fission. Enrico Fermi himself had tried to give the message to the military but had been rebuffed. Szilard, working with Fermi, Edward Teller, Victor Weisskop and Eugene Wigner, convinced Albert Einstein that only the magic of the Einstein name would catch the ear of President Franklin Delano Roosevelt. Szilard composed the letter which Einstein signed. The letter began:

> "Some recent work by E. Fermi and L. Szilard, which has been communicated to me in manuscript, leads me to expect that the element uranium may be turned into a new and important source of energy in the immediate future. Certain aspects of the situation which has arisen seem to call for watchfulness and, if necessary, quick action on the part of the Administration. I believe therefore that it is my duty to bring to your attention the following facts and recommendations."

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Then the letter warned:

> "This new phenomenon would also lead to the construction of bombs, and it is conceivable -- though much less certain -- that extremely powerful bombs of a new type may thus be constructed."

In a memorandum to President Roosevelt in March, 1945, which came to the attention of James F. Byrne following President Roosevelt's death, Szilard expressed the hope that a successful program of reciprocal control of atomic weapons could be established "and having kept it in operation for a few years, neither the United States nor Great Britain nor Russia would attempt to interfere with this system of control in such a manner that its acts would be considered by the other partners as a menace. We would then perhaps have a chance of living through this century without having our cities destroyed."

In November, 1947, Szilard wrote an "open letter" to Premier Stalin of the Soviet Union, urging him to "speak directly and personally to the American people" on Russia's proposals for general postwar settlement. The letter was printed in the Bulletin of the Atomic Scientists after the Justice Department refused Szilard permission to transmit it directly to Stalin.

Szilard on September 24, 1949 warned: "We should stop underestimating the Russians" in regard to atomic bomb production. His view was in direct opposition to that of Maj. Gen. Groves, wartime chief of atomic weapons production, who said the Russians "are too far behind."

In a letter to the New York Times on February 6, 1955, regarding the power conflict between the United States and the Soviet Union, Szilard said: "Unless we find the right answers soon war will come; and maybe in the final analysis it will come because there was too much patriotism in the United States and too few patriots."

In the February, 1960 issue of the Bulletin of the Atomic Scientists Szilard presented a proposal for a security system in which the United States and Soviet Union would maintain their bomb-carrying rockets and still avoid mutual annihilation. "It is my contention," Szilard states in the article, "that, as the world moves into this stage (a power stalemate between the United States and the Soviet Union), the vicious circle of the classical power conflict will cease to operate between America and Russia....they will have one interest in common which may override all of their other interests: to be able to live with the bomb without having to fear an all-out war that neither of them wants...."

Last year, Szilard advanced a new theory about how aging occurs. In the January 1959 issue of the Proceedings of the National Academy of Sciences, Szilard presented mathematical formulas for age, based on a hypothesis of "aging hits" on the human cell. The theory has yet to be tested.

Szilard in the February 1960 issue of the Bulletin of the Atomic Scientists, detailed his proposal on how "Russia and America could continue to operate with the threat of force and yet forego war, provided only that they impose upon themselves certain specific restraints."

In the 15,000-word article, Szilard states:

"At some point, either Russia or America could decide to respond to the threat of a 'limited' war, not by a counterthreat of the same kind, but by the threat of demolishing --if need be-- a specified number of cities, which have received adequate warning to permit their orderly evacuation. This would then represent a novel method for 'exacting a price' which might be quite appropriate -- if a price has to be exacted at all.

"In what circumstances would a threat of this type be believable and effective? Would it be possible actually to demolish evacuated cities without triggering a chain of events in which more and more cities would be destroyed, until in the end no major city of either nation might remain standing?

"I am assuming here that America and Russia are going to possess rockets capable of carrying a hydrogen bomb of the clean variety, which is large enough so that if the bombs were exploded at such a height that the fireball would not touch the earth, it would still destroy a good-sized city. Accordingly, no lives need be endangered by radioactive dust, if such a bomb were exploded over a city that has been evacuated.

"...The threat to demolish one or more evacuated cities need not trigger a chain of destructive events, provided the nation making the threat is willing to pay just as high a 'price' as it proposes to exact. This means that the nation making a threat of this type would have to be willing to tolerate -- without threatening reprisals -- as much destruction of cities in its territory as it proposes to cause in the territory of the 'enemy.'

"Russia and America could thus continue to operate with the threat of force and yet forego war, provided only that they impose upon themselves certain specific restraints...

"It is my contention that, if Russia and America want to maintain a 'metastable' state in the long-range rocket stage, so that an initial disturbance may not lead to a chain of events progressing to greater and greater destruction, they must accept the principle of 'one-for-one.' This principle must not be interpreted to mean that if Russia demolishes a city in America she must tolerate America's demolishing any one of her cities. Rather if Russia demolishes one or more evacuated cities in America, she must tolerate the destruction of cities with the same aggregate population.

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"For this principle to be operative, it is not necessary for Russia and America to conclude an agreement with each other; either Russia or America could establish this principle by unilateral declaration....

"The world would be in a more stable state than it is today, if Russia and America did not ever threaten to use bombs for anything worse than demolishing of cities which have evacuated. Moreover, if Russia and America were to go one step further and decide to forego war -- whether fought with small atomic bombs used against troops in combat or with conventional weapons -- this would represent an unprecedented advance from the moral point of view...."

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LEO SZILARD

Biographical Notes

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About the Author 1961

Dr. Leo Szilard is Professor of biophysics at the University of Chicago. He is a fellow of the American Academy of Arts and Sciences and a member of the National Academy of Sciences.

He was among the first to conceive of the possibility of an atomic chain reaction and to recognize what it would mean to the world. In 1939, Szilard took the initiative in inducing the U. S. Government to assume responsibility for the development of atomic energy; the letter which Albert Einstein wrote on August 2, 1939, to President Roosevelt was based on the work of Fermi and Szilard. In 1945, Szilard assumed the leadership of those of his colleagues who were opposed to dropping atomic bombs on the cities of Japan. In 1946, he led the successful fight of his colleagues against the May-Johnson Bill, which would have placed the development of atomic energy in the U.S. in the hands of an agency not under the direct "civilian" control of the President.

Dr. Szilard was born in 1898 in Budapest, Hungary. He received his degree in physics at the University of Berlin in 1922 and subsequently he worked in the field of physics in Germany, in England, and finally in the United States. While in Germany he discovered the relationship between entropy and information, which subsequently became one of the basic tenets of modern information theory. While in England, jointly with Chalmers, he discovered the so-called Szilard-Chalmers effect. In America he was among the first to find that neutrons are emitted in the fission process of uranium; the possibility of setting up an atomic chain reaction rests on this phenomenon.

The first patent issued in America on atomic reactors was issued jointly in his name and in the name of the late Enrico Fermi. With E. P. Wigner he shared the Atoms for Peace Award for 1959.

Since 1946, Szilard's main interest has shifted to molecular biology. His latest work is concerned with the aging process and the formation of antibodies.

He recently published "The Voice of the Dolphins" - five stories of social and political satire - which is now in its third printing.

Dr. Szilard has spent most of the current year in Washington, D. C.

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He recently published "The Voice of the Dolphins" - five stories of social and political satire - which is now in its ~~third~~ fifth printing.

Since Febr. 1961 stayed stayed

Dr. Szilard has ~~spent most of the current year~~ in Washington, D.C.

~~Since February~~

9-21

<u>BIOGRAPHICAL TABLE</u>

February 11, 1898
Born in Budapest, Hungary

June 27, 1916
Graduated from the "Königliche Staatliche Oberrealschule des VI Bezirkes," ~~xx~~ Budapest.

Christmas 1919
Left Budapest for Berlin, to enter the Institute of Technology, Berlin-Charlottenburg.

October 30, 1920
Registered ~~Matriculated~~ at the Friedrich Wilhelms-Universität, Berlin. Stayed until the winter semester, 1922/23.

August 14, 1922.
Granted Ph. D. degree Cum Laude, from the Friedrich Wilhelms-Universität, Berlin. Dissertation entitled: "Über die thermodynamischen Schwankungserscheinungen."

1922-1924
At the Kaiser Wilhelm Institut für Faserstoffchemie in Berlin Dahlem. Worked with Hermann Mark on the anomalous scattering of X-rays in crystals and on the polarization of X-rays by reflection on crystals.

1925-~~1928~~ 1933
Assistant at the Institut für Theoretische Physik, ~~at the~~ University of Berlin, under the direction of Max von Laue. ~~Later~~, Privatdozent für Physik at the University of Berlin. 192 -1933

Habilitationsschrift

Also in this period, jointly with Albert Einstein he developed and patented a method for pumping liquid metals by a moving magnetic field. This was designed for domestic refrigerators, but ~~xxxx~~ found its practical application for cooling atomic reactors after 1942.

1928-1932
Consulting Physicist for the Research Institute of the German General Electric Co. (A.E.G.), ~~developing refrigerator systems.~~

Dec. 1931 - May 1932
In the United States.

~~Fellowship at NYU~~ ?

Feb. 21, 1935 to May 23, 1935
In the United States

Summer 1934 to Jan. 1935

St. Bartholomew's Hospital, London. Guest of the Physics Department. ~~Developed the Szilard-Chalmers reaction fro chemically separating radioactive elements from their stable isotopes.~~

Jan. 9, 1935 to late 1937 (except for three months spent in the U.S. in early 1935).

Clarendon Laboratory, Oxford University. Research physicist under Prof. F.A. Lindemann; starting June 1, 1935, on a fellowship from Imperial Chemical Industries.

Jan. 2, 1938

Arrived in New York, under an arrangement in which he was to spend half a year in Oxford on a lectureship and half in the U.S. However, due to the political situation he resigned from the Clarendon Laboratory in September 1938.

February 2, 1939

Wrote letter to Joliot, suggesting that scientista agree to secrecy concerning chain reaction experiments.

March 1 to June 1, 1939

At Columbia University, as guest of the Physics Department.

March 3, 1939. Performed the experiment (with Walter Zinn) showing that fast neutrons are emitted in uranium fission, about two per fission, and that uranium might sustain a chain reaction.

June to July 1939

Meetings with Einstein to draft the letter to President Roosevelt about the possibility of atomic energy from uranium fission.

Oct. 21, 1939

Member of the "Uranium Committee" appointed by President Roosevelt. First meeting at the National Bureau of Standards, Washington, D.C.

June 7, 1940

Appointed a Member of the Advisory Committee on Nuclear Research of the President's Committee on Uranium.

Nov. 1, 1940
Joined the staff of Columbia University, as a member of the National Defense Research Division, under a contract given by the U.S. government.

Aug. 19, 1941
Appointed a member of two subsections of the "Uranium Section" (called S-1) of the National Defense Research Committee:
Consultants on Power Production
Consultants on Theoretical Aspects

February, 1942
The Columbia group was transferred to the University of Chicago, where it formed the nucleus of the Metallurgical Laboratory, code name for the uranium project.

February 1942 to ~~summer~~ July 1946.
Chief Physicist, Metallurgical Laboratory

March 29, 1943
Naturalized ~~as~~ a U.S. citizen.

June 1, 1946
Resigned from Metallurgical Laboratory

late
~~October,~~ Autumn 1946
Professor at the University of Chicago, in various divisions, until his retirement as Professor Emeritus in 1963

October 1946
Professor of Biophysics, Institute of Radiobiology and Biophysics, University of Chicago. Part-time

see 3b57

October 1, 1949
(Half-time) Professor of Biophysics, Institute of Radiobiology and Biophysics.
(Half-time) One-year appointment in the Division of Social Sciences as Adviser, Office of Inquiry into the Social Aspects of Atomic Energy.

October 1, 1950 to June 30, 1954
Returned as full time Professor of Biophysics, Institute of Radiobiology and Biophysics.

Winter quarter, 1951
University of Colorado. Visiting Professor of Biophysics in the School of Medicine.

September 1, 1953 to August 31, 1954
Brandeis University, Waltham, Mass. Visiting Professor of Physics.
(on leave from the University of Chicago)

July 1, 1954
Institute of Radiobiology and Biophysics at the University of Chicago abolished.

Szilard appointed Professor of Social Sciences in the Division of the Social Sciences, University of Chicago. (1954-1956)

July 1, 1956
Professor of Biophysics, (Enrico Fermi) Institute for Nuclear Studies, University of Chicago.

July 7-10, 1957
Delegate to the first Pugwash Conference on Science and World Affairs; also to the Conferences in subsequent years.

May 18, 1960
Given the Atoms for Peace Award.

April 25, 1961
Elected to membership, National Academy of Sciences.

June 1962
Founded the Council for a Livable World, Washington, D.C.
(Original title: Council for Abolishing War).

July 1, 1963
Appointed Non-Resident Fellow, Salk Institute for Biological Studies, La Jolla, Calif.

September 30, 1963
Retires from the University of Chicago as Professor Emeritus.

April 1, 1964
Becomes Resident Fellow at the Salk Institute.

May 30, 1964
Died of a heart attack in La Jolla,

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More to come soon.

27. Juni 1916
Königliche Staatliche Oberrealschule des VI. Bezirkes zu Budapest
Reifezeugnis

Christmas 1919
Left Budapest for Berlin Institute of Technology, Berlin-Charlottenburg

30. Oktober 1920

Immatrikuliert
Friedrich Wilhelms-Universität zu Berlin (bis Wintersemester 1922/23)
Nr. 1521.111.

Fr. W. Univ. Berlin 14. August 1922
Doctor Philosophiae
Dissertation: Über die thermodynamischen Schwankungserscheinungen

1922-1924
Kaiser Wilhelm Institut für Faserstoffchemie in Berlin Dahlem.
Worked with H. Mark on anomolous scattering of x-rays in crystals

Jan & Feb 1935
Six-week visit to Clarendon Laboratory, Oxford.

Autumn 1928 - Autumn 1932
Research Institute of the German General Electric Co. (A.E.G.)
Consulting Physicist, developing refrigerator systems.

Spring 1937
Accepted fellowship at Clarendon Lab, Oxford. Stayed until 1937, full-time

Tape. Bk 2. P. 28

Aug. 1934 to Early 1935
At St Bartholomew's Hosp. London.
(letter LS → Goldhaber 1/6/35
LS → Hopwood 2/14/35 in $\boxed{R\text{-}1}$
L.S. → Singer. 16 June, 1936 in $\boxed{R\text{-}46}$

June 1, 1935
Fellowships at Oxford from I.C.I. to run from June 1, 1935 to May 31, 1938

Jan. 1935
Left St. Bartholomew's, London.
Planned to go to Clarendon, Oxford
1/14/35 L.S. → Goldhaber $\boxed{R\text{-}1}$

March 1, 1939
Started work as guest, for three months, of Physics Dept at Columbia

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Curriculum Vitae of Leo Szilard

I was born in Budapest, Hungary, in 1898. I went through officers' school there during the first World War and studied engineering there.

In 1920 I left Hungary to continue my engineering studies in Berlin. However, the attraction of physics proved to be too great. Einstein, Planck, Von Laue, Schroedinger, Nernst, Haber, and Franck were at that time all assembled in Berlin and attended a journal club in physics which was also open to students. I switched to physics and obtained a Doctor's degree in physics at the University of Berlin under Von Laue in 1922. My thesis (1 — see attached list of publications) showed that the Second Law of Thermodynamics covers not only the mean values, as was up to then believed, but also determines the general form of the law that governs the fluctuations of the values.

Subsequently, I was a research worker in one of the Kaiser Wilhelm Institutes in Berlin and later joined the teaching staff of the University of Berlin (as Privatdozent) where I remained until 1933. Of the papers (1 - 4) published during this period, some are experimental, and some are theoretical. The last one (4) established the connection between entropy and information which forms part of present day information theory.

In 1933 I went to England. I considered at that time becoming a biologist, and A. V. Hill said that he would find a position for me as a demonstrator in physiology. It occurred to me, however, just then that a nuclear chain reaction might be possible if we could find an element that would emit neutrons when bombarded by neutrons. Artificial radioactivity was discovered a few months later by Joliot and seemed to provide an important new research tool in nuclear physics. This decided me to move into nuclear physics.

In the summer of 1934 I started work as a guest in St. Bartholomew's Hospital in London and this work resulted in the establishment of the Szilard-Chalmers Reaction (5) and the discovery that slow neutrons are emitted by beryllium if the beryllium is exposed to gamma rays of radium (6). In 1939, after the discovery of the fission of uranium, the use of these slow neutrons from beryllium made it possible to see that uranium emits neutrons when bombarded by neutrons; the fast neutrons emitted by uranium could be easily distinguished from the bombarding slow neutrons.

In 1935, after a visit to New York, where I spent a few months as research associate at New York University, I accepted a position at the Clarendon Laboratory, Oxford University. During this period I worked in the field of nuclear physics (8-11). In 1938 I came to America under arrangement with Oxford University, which permitted me to spend half my time in the United States. I was in the United States during the time the Munich Agreement was negotiated. After Munich I decided to stay in the United States on a full-time basis, and I resigned at Oxford.

In January 1939 I learned of the discovery of fission. It seemed important to find out at once if neutrons are emitted in that process, for in that case a chain reaction in uranium had to be regarded as a serious possibility. I, therefore, asked the permission of Columbia University to work there as a guest and perform an experiment in order to settle this question. This experiment (jointly performed with Walter Zinn) led to the discovery of the neutron emission of uranium, upon which the chain reaction is based (12, 13). The same discovery was made independently at about the same time by Fermi and his co-workers and by Joliot and his group.

In July, 1939, I recognized that a chain reaction might be set up in a system composed of graphite and uranium. Because of the serious consequences of this possibility, it seemed that this was a matter in which the government ought to take an interest. I, therefore, went to see Professor Einstein to enlist his help in approaching the government. After several consultations, in which E. P. Wigner and Edward Teller participated, Einstein wrote a letter to President Roosevelt; and in response to this letter, the President appointed a committee under the chairmanship of the Director of the National Bureau of Standards.

In February 1940 I described the chain-reacting uranium-graphite system in a paper I sent to the Physical Review (February, 1940). For reasons of secrecy, this paper was not published.

In November of 1940 a government contract was given to Columbia University for the development of the graphite-uranium system, and I became a member of Columbia University's National Defense Research Staff. Early in 1942 our group was moved to the University of Chicago; and on December 2, 1942, the chain reaction system was put into action.

Recently a patent was granted to the Atomic Energy Commission on the chain-reacting graphite-uranium system, jointly in the names of Enrico Fermi and myself.

In 1943 I became a naturalized citizen of the United States.

In October, 1946, I joined the staff of the University of Chicago as Professor of Biophysics in the Institute of Radiobiology and Biophysics. This institute never grew as originally intended, it had a succession of directors, and it was recently dissolved. I remained on the staff of the University of Chicago as Professor of Biophysics and was transferred to the Enrico Fermi Institute for Nuclear Studies.

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When in 1946 I was faced with the task of converting myself into a biologist, I teamed up with Dr. Aaron Novick, a physical chemist. I had known him from his work in the uranium project. We both got our training in biology through summer courses, such as Dr. Delbrück's course in Cold Spring Harbor in bacterial viruses, and Dr. Van Niel's course in bacterial biochemistry at Pacific Grove. Dr. Novick and I worked as a team until ~~xxxxxxkkyxxxxxx~~ the Institute of Radiobiology and Biophysics was dissolved.

A list of publications is attached, containing a short description of each paper. When we started out, we tried to understand a striking phenomenon just then discovered by A. Kelner, who showed that bacteria killed by ultraviolet light can be reactivated by shining visible light at them (17). A detailed analysis of the phenomenon enabled us to interpret it in terms of a "poison" that is produced by ultraviolet light and is decomposed by visible light. This interpretation was at first controversial due to Dulbecco's work on light reactivation of ultraviolet killed bacterial viruses, but has in the meantime become widely accepted. My own interest in the subject waned when I could not convince myself that we were dealing with a phenomenon that serves a useful biological purpose in the life of the bacteria.

Next, we turned our attention to the study of bacterial viruses in the assumption that viruses may prove to be much simpler than bacteria. We obtained some very interesting results (18) but decided to shift after a while to the study of bacteria.

The two phenomena in which we were particularly interested were a) mutations and b) the formation of adaptive enzymes which promised to provide a tool for the study of protein synthesis.

We were dissatisfied, however, with the methods that were available for the study of these phenomena. It seemed to us necessary to study bacterial populations in the growing condition in a stationary state, i.e. we thought we ought to use a continuous flow device. We developed such a device, which we called a "Chemostat." In this particular device the rate of growth of the bacteria can be changed by changing the concentration of one of the growth factors of our choosing which we make the controlling growth factor.

We started out by using the "Chemostat" for the study of mutations and obtained quite unexpected results at the very outset. It turned out, for instance, that the rate at which certain mutations occur does not change when we change the rate at which the bacteria divide; we could vary the rate of growth within a wide range without changing the rate at which these mutations occurred. We found one family of compounds - purines - which may cause an about tenfold increase in the mutation rate of bacteria without any appreciable killing. And we also found antimutagens, which in very small ~~contractions~~ concentration will fully counteract the effect of purine-type mutagens.

In a bacterial population maintained in the "Chemostat" there occur evolutionary changes (19) and one strain of bacteria is replaced by a mutant strain, which can grow faster in the conditions prevailing in the growth tube of the "Chemostat". We observed successive evolutionary steps of this sort in each experiment of sufficiently long duration and were able to analyze the phenomenon.

After the dissolution of the Institute of Radiobiology and Biophysics I did not maintain a laboratory. In the last few years my interests centered mainly on quantitative studies of general biological phenomena, with strong emphasis on molecular biology. The paper I published most recently (#25) attempts to give a quantitative theory of ~~the process~~ of aging which should be applicable to mammals.

PARTIAL BIBLIOGRAPHY OF LEO SZILARD
(with annotations)

A. Physics

(1) Zeitschrift für Physik, 1925, p. 753, 32. This paper extends the application of thermodynamics to the derivation of the laws of thermodynamical fluctuations. It was accepted as dissertation by the University of Berlin.

(2) Zeitschrift für Physik, 1925, p. 688, 33. - jointly with H. Mark. This paper reports experiments which revealed anomalous scattering of X-rays.

(3) Zeitschrift für Physik, 1926, p. 743, 35. - jointly with H. Mark. This paper reports experiments on polarizing X-rays by reflection on crystals.

(4) Zeitschrift für Physik, 1929, p. 840, 35. This paper evaluates the increase of entropy which is connected with operations of an intelligent being on a thermodynamical system if these operations are controlled by measurements of variables which are subject to thermodynamical fluctuations. This paper was accepted as Habilitationsschrift by the University of Berlin.

(5) "Chemical Separation of the Radioactive Element from its Bombarded Isotope in the Fermi Effect" -- jointly with Chalmers. Nature, p. 462, 134, 1934. This paper demonstrates a generally applicable process (Szilard-Chalmers reaction) for the concentration of a radioactive element produced by neutrons if the element has to be separated from a mass of a stable element with which it is chemically isotopic.

(6) "Detecting Neutrons Liberated from Beryllium by Gamma Rays," p. 494, 134, 1934. Nature.
This paper describes the discovery of radium-beryllium photo neutrons which, being of low energy, represent a useful tool in nuclear research. They were universally used later in the discovery and investigation of neutron emission of uranium on which a chain reaction is based.

(7) "Liberation of Neutrons from Beryllium by X-Rays" -- jointly with a group of six others, p. 880, 134, 1934. Nature. Using x-rays in place of gamma rays the threshold for the emission of photo neutrons from beryllium is determined by varying the voltage of an X-ray tube and is found to be somewhat above 1.5, and well below 2 m.e.v.

(8) "Radioactivity Induced by Neutrons" -- jointly with Chalmers, p. 98, 135, 1935. Nature. In this paper a neutron induced radioactive period of about 3-1/2 hours is reported in Indium which does not fit in with the explanations found for other radioactive periods. In a later paper it is shown that it is due to an excited Indium nucleus which is isomeric with stable indium nucleus 115.

(9) "Absorption of Residual Neutrons," p. , 136, 1935. Nature.
This paper reports the discovery of neutron resonances at low energies, gives an estimate of their energies, and states that the energies can be measured by observing the absorption of the residual neutrons in boron or lithium. 20

(10) "Gamma Rays Excited by Capture of Neutrons," p. 323, 139, 1937 -- jointly with Griffiths. Nature. This paper reports on the observation of gamma rays emitted by a number of odd elements which are strong neutron absorbers. The counts observed per absorbed neutron were found to be 15 per cent identical for all these elements.

(11) "Radioactivity Induced by Nuclear Excitation" -- jointly with Goldhaber and Hill, p. 47, 55, 1939. Phys. Rev. In this paper the previously reported period in indium is investigated and the conclusion is reached that it is due to nuclear excitation of the stable indium isotope 115.

(12) "Instantaneous Emission of Fast Neutrons in the Interaction of slow Neutrons with Uranium" -- jointly with Zinn, p. 799, 55, 1939. Phys. Rev. In this paper the discovery of the neutron emission of uranium is reported. It is estimated that two neutrons are emitted per fission. The neutrons from uranium are made visible on an oscillograph screen. As primary neutrons, radium-beryllium photo neutrons were used which, because they are slow, can be easily distinguished from the fast neutrons emitted by uranium. This discovery which was made independently by Fermi in the same year indicated the feasibility of a sustaining nuclear chain reaction.

(13) "Emission of Neutrons by Uranium" -- jointly with Zinn. p. 619, 56, 1939. Phys. Rev. Detailed report of above mentioned experiments, number of neutrons per fission measured as 2.3.

(14) "Neutron Production and Absorption in Uranium" -- jointly with Anderson and Fermi. p. 284, 56, 1939. Phys. Rev. This paper reports an investigation on the chain reaction qualities of a uranium-water system. It is estimated that 1.5 neutrons are emitted for every thermal neutron which is absorbed by uranium.

Dr. Szilard's part in bringing about of the first nuclear chain reaction; in the design of the first nuclear reactor (atomic pile) are described, insofar as these matters can be made public, in the Official Report: Atomic Energy for Military Purposes, Henry D. Smythe, 1945, Princeton University Press, pages 34, 47, etc.

B. BIOLOGY

(17) A. Novick and Leo Szilard - EXPERIMENTS ON LIGHT-REACTIVATION OF ULTRA-VIOLET INACTIVATED BACTERIA. Proceedings of the NATIONAL ACADEMY OF SCIENCES. Vol. 35, No. 10, pp. 591-600.

(18) Aaron Novick and Leo Szilard - VIRUS STRAINS OF IDENTICAL PHENOTYPE BUT DIFFERENT GENOTYPE. Science, January 12, 1951, Vol. 113, No. 2924, pp. 34-35.

(19) Aaron Novick and Leo Szilard - EXPERIMENTS WITH THE CHEMOSTAT ON SPONTANEOUS MUTATIONS OF BACTERIA. Proceedings of the NATIONAL ACADEMY OF SCIENCES. Vol. 36, No. 12, pp. 706-719, December, 1950.

(20) Aaron Novick and Leo Szilard - DESCRIPTION OF THE CHEMOSTAT. Science, December 15, 1950. Vol. 112, No. 2920, pp. 715-716.

(21) Aaron Novick and Leo Szilard - EXPERIMENTS ON SPONTANEOUS AND CHEMICALLY INDUCED MUTATIONS OF BACTERIA GROWING IN THE CHEMOSTAT. Cold Spring Harbor Symposia on Quantitative Biology. Vol. XVI, 1951.

(22) Aaron Novick and Leo Szilard - ANTI-MUTAGENS. Nature, Vol. 170, p. 926, November 29, 1952.

(23) Aaron Novick and Leo Szilard - EXPERIMENTS WITH THE CHEMOSTAT ON THE RATES OF AMINO ACID SYNTHESIS IN BACTERIA. Dynamics of Growth Processes. Princeton University Press, pp. 21-32, 1954.

(24) Maurice S. Fox and Leo Szilard - A DEVICE FOR GROWING BACTERIAL POPULATIONS UNDER STEADY STATE CONDITIONS. Journal of General Physiology 39, p. 261-6, 1955.

(25) Leo Szilard - ON THE NATURE OF THE AGING PROCESS. Proc. Nat. Academy of Sciences. Vol. 45 pp. 30-45, 1959.

The first of these papers (#17) investigates a phenomenon discovered by A. Kelner after the war, who showed that bacteria "killed" by ultraviolet light can be revived by shining visible light on them. Experiments designed to analyze the phenomenon are described in this paper; they lead to the conclusion that the ultraviolet light produces a "poison" which can be inactivated by light and that this "poison," if present when, subsequent to irradiation, the bacteria divide, will cause both death and mutations.

The second paper (#18) describes the discovery that, when a bacterium is infected simultaneously with two related viruses which differ from each other both in genotype and phenotype, the virus population emerging from the bacterium contains a class of viruses which have the genotype of one and the phenotype of the other.

The papers #19 to #23 describe a new way of studying bacteria by maintaining a bacterial population in a stationary (exponentially growing) state indefinitely and controlling the growth rate by controlling the rate of supply of an essential growth factor. An apparatus is described in these papers which will conveniently accomplish this and which is designated as the Chemostat.

In studying mutations in bacteria or the formation of adaptive enzymes in bacteria inaccurate, and, therefore, misleading results are frequently obtained by studying bacterial cultures in flasks in which the number of bacteria increases exponentially and today the use of the Chemostat appears to be indispensable.

In the papers #19 to #22, the Chemostat is used in the study of mutations. It turns out that the rate at which mutations occur in a growing bacterial population under the conditions studied is not proportional to the rate at which cell division occurs, rather the mutation rate is constant per unit time independent of the rate at which the culture is growing. There is found one group of compounds, all purine derivatives, of which caffein is one, which greatly increases the mutation rate without having an appreciable killing effect on the bacteria.

There is another group of compounds described in these papers, all of them ribosides of purines which in small quantities will completely counteract the action of the above mentioned purine type mutagens and also reduce the rate of spontaneous mutations.

In paper #23, the Chemostat is used to study the biosynthesis of amino acids in bacteria and the regulatory mechanisms which are involved in it. The bio-synthetic apparatus of the bacteria respond to amino acid concentrations in the medium, which are exceedingly low. For instance, a bacterium which can make arginine and will do so if there is no arginine in the medium, will stop making arginine if an arginine concentration of 10^{-9} ga/ce is maintained in the medium in the Chemostat. (Novick and Szilard - unpublished.)

One way of studying such regulatory mechanisms is based on the use of a mutant which is blocked in the synthesis of an amino acid--in our case Tryptophane--and which pours out into the medium a "precursor" of that amino acid. Paper #23 utilizes such a mutant. In the absence of Tryptophane in the medium, a precursor of Tryptophane is poured out by the mutant into the medium at a rate which is independent of the growth rate of the bacteria. In the presence of Tryptophane this "precursor" is <u>not</u> poured out by the bacteria. It is conceivable that this indicates a general phenomenon of regulation through a negative feed-back of the final product at one of the <u>early</u> steps of the metabolic pathway leading to Tryptophane.

In paper #24, there is described a device called a breeder. In this device bacteria may be grown in a continuous flow of nutrient. The flow of the nutrient is controlled by the turbidity of the bacterial culture and the growth is not limited by a growth factor, as is the case in the "Chemostat."

This device was developed in order to study mutations in bacteria under conditions of growth at the maximal rate, and such a study was carried out by Maurice S. Fox.

Paper #25 develops a theory of the basic process of aging. According to the theory, the elementary step in the process of aging consists in the random inactivation of whole chromosomes. The differences of longevity of individuals are attributed to the difference of the number of defective "vegetative genes" they have inherited.

The preceding pages were written by Szilard as part of an "Application for a Research Grant" which he filed with the General Medical Sciences Division of the National Institutes of Health in June 1959. His Curriculum Vitae written for this purpose, therefore contains only information pertinent to his scientific career and omits other activities which ~~focussed~~ of ~~on~~ great importance ~~parts of~~ in his life.

In 1933, after Szilard left Germany for England, he ~~teamed up~~ established jointly with Sir William Beveridge, ~~Miss~~ Esther Simpson and others ~~to establish~~ the "Academic Assistance Council", an organization to relocate scholars displaced from Nazi Germany.

While Szilard was instrumental in ~~bringing about~~ launching the atomic bomb project under the assumption--later proved to be erroneous--that there was a race against Nazi Germany, it became clear to him in the Spring of 1945 that Germany had been defeated and he ~~concentrated his~~ made desperate efforts to prevent the bomb from being used. ~~From there on~~ Thereafter a major part of his life was devoted to the prevention of nuclear warfare. In July 1945 he circulated a petition in the Uranium project, addressed to the President of the United States, asking that the bomb not be dropped. Within the year following the war he fought successfully for civilian control of atomic energy.

In the late 1940's ~~with~~ through the Emergency Committee of Atomic Scientists and ~~through~~ numerous publications in the Bulletin of the Atomic Scientists and elsewhere, he launched a perpetual campaign to make people aware of the dangers of nuclear warfare and the ~~dangers of an~~ arms race. He was a dedicated participant of the international Pugwash Movement, attending all the conferences during his lifetime.

In 1962 he founded the Council for Abolishing War now called the Council for a Livable World, an organization aimed at influencing the decisions and actions of the U.S. Senate in foreign policy issues.

His writings on these topics will be ~~the subject of~~ included in succeeding volumes.

In the Grant Application, mentioned ~~above~~ earlier, the title of the project was "Quantitative Studies of General Biological Phenomena" (RG 6876). Because the proposed research plan is unusual indeed, parts of it are excerpted here:

RG 6876 (9)

Research Plan and Supporting Data

The purpose of the proposed study is to gain insight into certain general biological phenomena rather than to try to understand the functioning of specialized biological structures (such as, for instance, of the nerve fiber, of the muscle fiber, or of the specialized sense organs). I am particularly interested in those general biological phenomena where it may now be possible to gain insight into quantitative relationships which can be checked against data obtained from available observations or experiments as yet to be made. The proposed work would take as its starting point preliminary theoretical studies which I carried out in the past three years.

At The University of Chicago, I am holding a Research Professorship. I have neither any teaching duties nor any fixed obligations to be in Chicago at certain fixed periods of time. This freedom has enabled me in the past three years to spend considerable time at various laboratories away from Chicago. It is my understanding that, under the grant here requested, I would have full freedom to move about wherever my research interests may take me. It is anticipated that I may spend nine months of the year away from my home, at various laboratories where work may be pursued in fields in which I am interested.

I anticipate that my work will have a strong theoretical orientation. But in order to be able to function as a theoretical biologist, it is necessary to have intimate knowledge of experiments relating to a variety of biological materials and involving diverse techniques.

Biology has not quite reached the stage which was attained by physics half a century ago when enough relevant facts were established to permit a theoretical physicist to come up with significant insights. Yet in biology we might be very well on the verge of a similar situation, and a few scientists who are so inclined may now perhaps attempt to function as theoretical biologists. Accordingly, these days it might be well for a few scientists to put less emphasis on their own experiments and spend more time trying to keep in touch with the experiments of others in the hope of being able to recognize new patterns and to try to gain insight into some general biological laws. It may be that the main difference between the theoretical physicists of the past and the would-be theoretical biologist of the present is quantitative rather than qualitative. The would-be theoretical biologist would probably not be able to keep on studying the results of others and thinking about them for a very long stretch of time. Much sooner than a theoretical physicist, he will feel impelled to do further experiments (or to induce someone else to do them) because he will need to cut down the number of possible avenues along which his further thinking may be tempted to wander.

How successfully a man may be able to function as a theoretical biologist is likely to depend (apart from his inclinations and abilities) on whether he is put in the position where he can maintain close cooperations with a number of laboratories, initiate some new experiments or observations, and perhaps participate actively in experimental work carried out in laboratories where he holds no position of administrative responsibility.

On a rather limited scale I have tried, in the last few years, to function somewhat as such a theoretical biologist. ~~Below I list a number of problems with which I dealt during the past three years in lectures or in extended conversations with interested colleagues at institutions other than The University of Chicago.~~

Appendix to Biographical Notes

Rx fine print

LEO SZILARD

Honors

1954	Fellow, American Academy of Arts and Sciences
1960	Humanist of the Year, American Humanist Association
1960	Einstein Gold Medal of the Lewis & Rosa Strauss Memorial Fund
1960	Atoms for Peace Award
1961	Elected Membership, National Academy of Sciences
1961	Honorary Doctor of Humane Letters, Brandeis University
1970	Crater on far side of the moon named "Szilard" by International Astronomical Union

At that time Szilard contacted a number of scientists to explore whether they might be interested in joining the kind of institute he and Doering had visualized. He was fortunate in finding a sympathetic response among scientists with similar inclinations and "was one of the moving spirits who helped to conceive the idea of the Salk Institute and to bring it into being."*

He joined the Institute as non-Resident Fellow in July 1963, and became a Resident Fellow on April 1, 1964. After a few short, but happy and productive months in La Jolla, he died there suddenly on May 30, 1964.

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*From: Topics in the Biology of Aging, A Symposium held at The Salk Institute for Biological Studies, San Diego, California, 1965. Ed. Peter L. Krohn, Interscience Publishers, John Wiley & Sons, N. Y., 1966.

After the Institute of Radiobiology and Biophysics at the University of Chicago was dissolved in 1954 Szilard found himself without a laboratory. "I would rather have roots than wings," he said at that time, "but if I cannot have roots I shall use wings." During the last ten years of his life Szilard became a roving sponsor of the newly emerging science of molecular biology. He was Visiting Professor at the newly created Department of Biophysics at the University of Colorado Medical Center, Visiting Professor at Brandeis University and served as consultant to the Basic Research Program of the National Institute of Mental Health in Washington.

In 1963, after reaching the age of 65 Szilard became Professor Emeritus at the University of Chicago but remained in active service until March 31, 1964.

On several visits to Europe between 1957 and 1963 Szilard served as advisor to the research program of the World Health Organization; was consulted by the German Government on the organization of postwar scientific research in West Germany, and rounded up the European biologists to explore the possibility of creating a European Laboratory for Molecular Biology. These efforts led to the establishment of EMBO, the European Molecular Biology Organization.*

In 1957, while working on problems of population control, he drafted with his friend Professor William Doering "A Proposal to Create Two Interdependent Research Institutes Operating in the General Area of Public Health, Designated as: Research Institute for Fundamental Biology and Public Health and Institute for Problem Studies." This proposal contains many far reaching thoughts and is therefore partially reproduced as an Appendix to ~~this section~~. Part IV.

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*References:

J.C. Kendrew, EMBO and the Idea of a European Laboratory, Nature, 218: 840-842, (June 1) 1968.

Marie Luise Zarnitz, MOLEKULARE UND PHYSIKALISCHE BIOLOGIE, Bericht zur Situation eines interdisziplinaren Forchungsgebietes in der Bundesrepublik Deutschland, Erstattet im Auftrag der Stiftung Volkswagenwerk, (Vandenhoeck & Ruprecht in Gottingen, 1968).

(3) It has been shown that the observed frequent occurrence of a striking resemblance between a child and one of its parents might be explained in one of two ways:

(a) The perceptible phenotype might be determined by a number of different genetic loci, all of which are located on one pair of homologous autosomes. Such an autosome might possess a certain 'strength' and a 'strong' autosome might suppress the homologous autosome if it is substantially less 'strong.' (For details see the enclosed paper on 'On the occasional dominance of the 'perceptible phenotype' in man,' dated July 12, 1963). Revised version dated May 18, 1964.

(b) The genes which determine the perceptible phenotype might all be located within the same operon and different operons might be more strongly or less strongly repressed. This could then account for the observed resemblances, if one assumes that the perceptible phenotype is determined by the ratio of the quantities of the products of these genes in the diploid cell.

(4) An experimental method has been devised for determining whether the competition which exists between spermatozoa for fertilizing an ovum might serve the purpose of protecting the ova against being fertilized by a spermatozoan which might contain genetic material that has deteriorated as a result of the aging process. The method devised consists in inseminating females with a mixture of spermatozoa, derived from two donors, and in determining how the fraction of the offspring which is derived from the older donor, decreases with increasing age differences of the two donors. (For details see the enclosed paper 'The aging process and the 'competitive strength' of spermatozoa,' dated July 25, 1963.) Arrangements for carrying out experiments of this type are at present under discussion."

In addition to the four published papers Szilard during this period outlined several theories in the following reports:

(1) INDUCTION OF MUTATIONS IN MAMMALS BY IONIZING RADIATION (1961)

(2) THE SEX CHROMATIN IN MAMMALIAN CELLS, 'DOSAGE COMPENSATION' IN THE FRUIT FLY, AND THE ENZYME REPRESSION IN BACTERIA (1962)

(3) ON THE OCCASIONAL DOMINANCE OF THE 'PERCEPTIBLE PHENOTYPE' IN MAN (1963, rev. 1964)

(4) THE AGING PROCESS AND THE 'COMPETITIVE STRENGTH' OF SPERMATOZOA (1963)

He summarized them in his progress reports as follows:

"(1) An experimental method has been devised and a theory of the experiment developed which should make it possible to determine the dose of radiation which would raise the mutation rate to twice the value of the spontaneous mutation rate. The method consists in exposing a population of mice to ionizing radiation and subsequently determining, among the first generation off-spring, the proportion of females whose off-spring shows an abnormal sex ratio. The method is described in a paper dated March 10, 1961: 'Induction of Mutations in Mammals by Ionizing Radiation' which is being privately circulated to those interested in this type of problem.

(2) In mammals and also in the fruit fly the somatic cells of the female contain two X chromosomes while the somatic cells of the male contain only one. Accordingly, the cells of the female carry two homologous copies of each sex linked gene, whereas the cells of the male carry only one copy of each. This difference in 'dosage' does <u>not</u> usually manifest itself in a phenotypic difference between the male and the female. Recent observations indicate that in the case of mammals at some point of the embryonal development of the female, one of the two X chromosomes ceases to be functional in the somatic cells. This, on the face of it, could account for the fact that the double dosage of the sex linked genes in the female, as compared to the single dosage of the same genes in the male, does not lead to a difference in the phenotype. However, no such difference in phenotype exists in the fruit fly either, and yet I find that the phenomenon of 'dosage compensation', which has been studied in the fruit fly by H. J. Muller cannot be explained on the assumption that only one of the two X chromosomes is functional in the somatic cells of the female. In those circumstances it is necessary to look for another explanation for 'dosage compensation' in the fruit fly. I propose to explain this phenomenon in the fruit fly by assuming that the relevant gene products in the fruit fly are under the control of repressors, in much the same way in which many enzymes are under the control of repressors in bacteria, and by further assuming that in the fruit fly the genes corresponding to the repressors (of those gene products which show 'dosage compensation') are located on the X chromosome. These considerations are described in a paper, 'The sex chromatin in mammalian cells, 'dosage compensation' in the fruit fly and enzyme repression in bacteria,' which is being circulated in preprint among those interested in this kind of problem.

presented by the author in paper #25. The theory accounts for the decrease in the ratio of boys to girls, with increasing age of the father, on the ground that a spermatogonium in which the X-chromosome suffers an "aging hit" may not continue to give rise to sperm, whereas a spermatogonium in which the Y-chromosome suffers an "aging hit" may continue to give rise to the sperm."

Paper #29, Szilard's last effort, was published posthumously. He had prepared a much longer manuscript and sent preprints of the shortened version to his friends accompanied by this memorandum:

May 5, 1964

"Enclosed is a preprint of a paper which will appear in the June issue of the Proceedings of the National Academy of Sciences. Because authors are limited to eight pages in any one issue of the Proceedings, this preprint is but the first of three instalments.

Had I merely postulated -- as others seem to have done -- that if two neurons fire simultaneously, thereafter the synapse bridging these two neurons has a higher efficacy, then I would not be able to account even for Pavlov's experiments on the conditioned salivary reflex of the dog. As it is, it seems conceivable that the two fundamental postulates of my model might be able to account not only for the peculiarities of all of Pavlov's basic experiments but -- in conjunction with neuron-networks, as yet to be invented -- also for the higher mental functions. This could be true even if the details of the biochemical underpinnings of these two postulates should turn out to be incorrect.

(Signed) Leo Szilard"

The Research Grant became effective on January 1, 1960 and continued through March 31, 1964. During this time the following papers by Szilard were published:*

(26) Leo Szilard - THE CONTROL OF THE FORMATION OF SPECIFIC PROTEINS IN BACTERIA AND IN ANIMAL CELLS. Proceedings of the National Academy of Sciences, 46: 277-292 (March) 1960.

(27) Leo Szilard - THE MOLECULAR BASIS OF ANTIBODY FORMATION. Proceedings of the National Academy of Sciences, 46: 293-302 (March) 1960.

(28) Leo Szilard - DEPENDENCE OF THE SEX RATIO AT BIRTH ON THE AGE OF THE FATHER. Nature, 186: 649-650 (May 21) 1960. Letter to the Editor.

(29) Leo Szilard - ON MEMORY AND RECALL. Proceedings of the National Academy of Sciences, 51: 1092-1099 (June) 1964.

In his progress reports to NIH Szilard described the first three papers as follows:

"A model for the control of the rate of production of repressible enzymes has been developed and this model is described in detail in paper #26. This model assumes that in bacteria the repressor controls the rate of formation of the enzyme by the enzyme forming site, rather than the rate of formation of the enzyme site itself. Experiments which are at present being conducted in a number of different laboratories, with which the author maintains contact, might elucidate, within a year, whether this "premise" is correct.

The above-quoted paper also assumes that the repressor can attach itself to the enzyme and it is shown that accordingly the cell might have two stable states, a state in which the enzyme level is high and a state in which the enzyme level is low. The validity of this assumption does not depend on the above-mentioned "premise" and the assumption might provide the key to the understanding of a certain type of differentiation, discussed in the paper.

A second paper #27 discusses the possibility that antibody formation--in the primary response--is based on this type of differentiation, triggered by the injection of an antigen into the rabbit. This theory can account for a number of phenomena listed in the paper, including the phenomenon of immune tolerance of the new-born rabbit. The explanation of immune tolerance is, however, again based on the "premise" that the repressor controls the rate at which the protein--in this case the antibody--is formed by the specific protein forming site. If future experiments should show that this "premise" is wrong, then the theory of immune tolerance would have to be modified and it is not as yet clear whether a satisfactory modification of the theory would be possible, in that contingency.

A theory for the dependence of the sex ratio at birth on the age of the father has been presented in paper #28 which is based on a theory of aging previously

* We continue the numbering system used by Szilard in the preceding pages

This part of the memorandum was labeled "Confidential" because it mentioned specific persons by name. Paragraphs mentioning names were deleted from the following pages: 3, 6, 7, 8, 10, 11, 14, and 15.

LEO SZILARD

BIOGRAPHICAL TABLE

1898	Feb. 11. Born in Budapest.
1908 - 1916	Reáliskola, Budapest VI. (Budapester Staatsoberrealschule des VI Bezirks). Reifezeugnis, June 27, 1916.
1916 - 1919	Kir. József-müegyetem, Budapest. Gépészmernöki. (Kön. Josef Technische Hochschule. Maschineningenieur Abteilung). Registered, Sept. 9, 1916. Completed three years (six semesters). Passed examination (I. Maschineningenieur Rigorosum) July 16, 1919.
1917 - 1918	K.u.K. Oesterreichisch-Ungarische Armee. Sept. 27, 1917. Entered as Einjährig-Freiwilliger, 4 Gebirgs-artillerie-Regiment, no.18. Reserveoffizierschule, Budapest. Kramsach, Tirol. Feuerwerkenkadetsaspirant, May 1918 for ca. 3 months. Kufstein, Tirol. Saebelchargenkurs, end of August to end of September. Reservespital, no.4, Budapest. (Attending lectures at Technische Hochschule, Oct. 1918) Nov. 17, 1918. Discharged from "Vereinigte Ersatzbatterien der Gebirgsartillerie Abteilungen 31 & 32, as Kadetsaspirant" at the end of World War I.
1919	Christmas, left Budapest for Berlin.
1920	Technische Hochschule, Berlin. Feb.9 - Nov.4, 1920. (2 semesters Maschinen Ingenieurwesen, Elektrotechnik) During that time he also took courses at K. Friedrich Wilhelms-Universität zu Berlin (two during Winter semester 1919/20 and four during Sommer semester 1920, including v. Laue, Franck, and Planck).
1920 - 1922	Friedrich Wilhelms-Universität zu Berlin (Philosophische Fakultät). Oct. 30, 1920, ~~registered~~ entered. (Studied ~~five~~ four semesters: Winter 1920/21 to ~~Winter~~ Summer 1922/~~23.~~) Aug. 14, 1922, Dr. Phil. (cum laude). Dissertation: "Über die thermodynamischen Schwankungserscheinungen" (Eximia) under Max von Laue.

1922 - 1924 Kaiser Wilhelm Institut für Physik, Berlin-Dahlem (Act. Dir. M. v. Laue).

Research fellow ~~Worked~~ until ~~the~~ end of 1924 under a "Forschungsstipendium der Kaiser-Wilhelm-Gesellschaft zür Förderung der Wissenschaften."

1924 - 1927 Assistent, Institut für Theoretische Physik, Friedrich Wilhelms-Universität, Berlin (Director: M. v. Laue).

1927 - 1933 Privatdozent, Friedrich Wilhelms-Universität, Berlin.

May 17, 1927. Habilitation.

Nov. 23, 1933. Position terminated for political reasons.

Cold Spr. H
Van Niels

BIOGRAPHICAL SKETCH - DR. LEO SZILARD

Born 1898, Budapest, Hungary

Ph. D. Physics 1922 University of Berlin, working with Von Laue

Scientific papers dealing with statistics and thermodynamics, 1922-1925

Privatdozent, University of Berlin, 1925-1933

Teaching of physics; scientific papers in experimental X-ray physics.

Research appointments in nuclear physics at Clarendon Laboratory, Oxford

University, Oxford, England, and at St. Bartholomew's Hospital 1934-1938

Theoretical analysis leading to identification of Information with negative entropy, 1925

Discovery of Szilard-Chalmers reaction, 1938

Discovery of photo-disintegration of Beryllium, 1939

Discovery with Zinn of neutron emission in Uranium fission, 1939

Memorandum to U.S. Government first proposing a nuclear chain-reaction utilising uranium with a graphite moderator, Oct. 1939

In 1939 Dr. Szilard organized the Uranium Committee, to interest the U.S. Government in atomic energy as a source of power, and in an atomic bomb. This committee, whose activities are described in detail in the Smyth report, persuaded the government to establish what eventually became the Manhattan Project. (Official Report: Atomic Energy for Military Purposes, Henry D. Smyth, 1945, Princeton University Press, Pages 34, 47, etc.)

Design of first nuclear pile for harnessing atomic energy - with Fermi, 1939-1940 (Smyth report, page 34)

Member of staff of Columbia University's National Defense Division, 1940-1942. Szilard and Fermi were in charge of all work on the Chain Reaction. (Smyth report, page 55)

Chief Physicist of University of Chicago Metallurgical Laboratory, Manhattan Project, which developed the first nuclear chain reaction, and built the first pile for production of radioisotopes, 1942-1946.

Professor of Biophysics, University of Chicago, 1946 to present. Dr. Szilard also holds a joint professorship in Social Science at the University of Chicago, in order to assist in studies of the social and political effects of atomic energy.

Investigations on mechanism by which cells killed by ultraviolet radiation can be revived by visible light; the physics of cellular mutation processes; and the mode of action of viruses, 1946-1949.

The second paper (#2) describes the discovery that, when a bacterium is infected simultaneously with two related viruses which differ from each other both in genotype and phenotype, the virus population emerging from the bacterium contains a class of viruses which have the genotype of one and the phenotype of the other.

The papers #3 to #7 describe a new way of studying bacteria by maintaining a bacterial population in a stationary (exponentially growing) state indefinitely and controlling the ~~growth~~ rate by controlling the rate of supply of an essential growth factor. An apparatus is described in these papers which will conveniently accomplish this and which is designated as the Chemostat.

In studying mutations in bacteria or the formation of adaptive enzymes in bacteria inaccurate, and therefore misleading, results are frequently obtained by studying bacterial cultures in flasks in which the number of bacteria increases exponentailly and today the use of the Chemostat appears to be indispensable.

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III b57A

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L. Szilard

M

STATEMENT ATTACHED TO FORM N-400, Pages 1 and 2

re (2)

I first arrived in the United States on an immigration visa in New York, N. Y. on board of S.S.Leviathan on December 25, 1931.

I left the United States from New York, N. Y. on board of S. S. Bremen on May 4, 1932 and returned on permit to re-enter on board of S. S. Olympic to New York, N. Y. on February 21, 1935.

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I have not been absent at any other time except for a trip to Canada, leaving July 3, 1938 and returning July 5, 1938.

re (9)

My last foreign residence preceding my immigration in 1931 was Berlin, Germany.

Preceding my entry in 1935 I lived in London and Oxford, England.

re (20)

Previous to April 5, 1937 I have resided, from December 25, 1931 to January 31, 1932, in Princeton, New Jersey. From February 1, 1932 to April 5, 1937 in New York State, during all my stays in the United States. These stays are as follows:

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The University of Chicago
CHICAGO 37, ILLINOIS
Institute of Radiobiology and Biophysics

April 1, 1952

BIOGRAPHICAL DATA ON LEO SZILARD

Leo Szilard was born in Hungary and between 1925 and 1933 was on the teaching staff of the University of Berlin. He began to work in the field of nuclear physics in 1934 in London and later continued his work at the University of Oxford. His early experiments at Columbia University, where he, jointly with Dr. Walter H. Zinn, showed in March, 1939, that neutrons are emitted in the fission process from uranium, became fundamental for the later work on atomic energy.

Dr. Szilard was instrumental in getting President Franklin Roosevelt to take an interest in the atomic energy field. Early phases of the work on the chain reaction were carried out at Columbia University under the direction of Enrico Fermi and Dr. Szilard. Early in 1942 this group moved to the University of Chicago, where the chain reaction was demonstrated on December 2, 1942.

In the fall of 1946 Dr. Szilard was appointed Professor of Biophysics at the University of Chicago in the Institute of Radiobiology and Biophysics, and he is also a member of the Social Science Division. His latest work is concerned with the study of mutations in microorganisms.

/sds

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Theoretical analysis leading to identification of Information with negative entropy, 1925

Discovery of Szilard-Chalmers reaction, 1938

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In 1939 Dr. Szilard organized the Uranium Committee, to interest the U.S. Government in atomic energy as a source of power, and in an atomic bomb. This committee, whose activities are described in detail in the Smyth report, persuaded the government to establish what eventually became the Manhattan Project. (Official Report: Atomic Energy for Military Purposes, Henry D. Smyth, 1945, Princeton University Press, Pages 34, 47, etc.)

Design of first nuclear pile for harnessing atomic energy - with Fermi, 1939-1940 (Smyth report, page 34)

Member of staff of Columbia University's National Defense Division, 1940-1942. Szilard and Fermi were in charge of all work on the Chain Reaction. (Smyth report, page 55)

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Professor of Biophysics, University of Chicago, 1946 to present. Dr. Szilard also holds a joint professorship in Social Science at the University of Chicago, in order to assist in studies of the social and political effects of atomic energy.

Investigations on mechanism by which cells killed by ultraviolet radiation can be revived by visible light; the physics of cellular mutation processes; and the mode of action of viruses, 1946-1949.

The second paper (#2) describes the discovery that, when a bacterium is infected simultaneously with two related viruses which differ from each other both in genotype and phenotype, the virus population emerging from the bacterium contains a class of viruses which have the genotype of one and the phenotype of the other.

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Leo Szilard - Biographical Notes

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What does Leo think might account for his idealism - moral and constructive efforts? Does he know what is the origin of these attitudes and attributes?

- Yes. My mother. She told me children's tales. Not any kind you can find in a book, but little tales she made up from time to time. These were usually designed to instruct in an interesting way some higher principle. For instance, to seek the truth was important. I remember one of the stories along this vein. My grandfather was a man of great integrity and rectitude. When he was a boy, the schools had somebody to monitor the behavior of the children before the teacher came in. My grandfather, in one of my mother's stories, was monitor. This was about the time of the 1848 revolution. Some soldiers went by and the children rushed to the window to watch them - something that was against the rules of behavior. When the teacher came in he asked for a list of those who had misbehaved. My grandfather had made a list of names which, to tell the truth, included his own name. That was the kind of story she used to tell me and they made a great impression. These were told me when I was about 2½ or 3 year old, for that is when such character is being formed, I believe.

- When my mother was about 60 years old she wrote her biography. She made a point about the significance of the exact meaning of words and how important that can be in affecting people's lives. When she was a little girl she had a beautiful sister who died suddenly. Now the critical word meaning revolves around the fact that in Hungarian the word for "naughty" and for "useless" is exactly the same. Shortly after her sister died, my mother overheard a couple of nannies talking. One of them remarked that it was too bad, wasn't it, that that beautiful little girl should have died and only the "naughty" little one was left. My mother interpreted that the meaning intended was that she was "useless" and she tried very hard from that day on to be useful - to do something useful.

[I pointed out that in many ways this had characterized his own life but he protested that he had never tried to be useful - that in any formal sense he never cared for being useful. Trude and I insisted that in a higher sense he had gone after caring for the world.]

Leo remembers the time when he was $2\frac{1}{2}$ and was sent to stay at his grandparents while his little brother was being born. He said they could never get him to say "please". "It was beneath my dignity" - and they had many wars over this.

It was his father who interested him in science. His father had engineering training but was really an <u>entrepreneur</u> getting jobs for building bridges, etc. Budapest was really an elegant place for the relatively well-to-do. It was beneath anyone to speak of money, or to ask how much something would cost.

- My father explained photography to me. I was really interested. I knew about what was the apparatus, the bulb, the box, the ground glass screen, etc. My father explained that the image was brought into focus on the ground glass screen - then the photographer squeezed the bulb and glue ran down on the image to hold it there.

In 1931 Leo had a post in Berlin but was already concerned about Hitler's rise. He had made a few inventions which later helped him support himself. He went to the U.S. on an immigrant visa. It was easy to get one. The quota was only 800 but nobody wanted to leave Hungary. He established residence and went back to Germany on a re-entry permit. In 1933 just about the time of the Reichstag fire he realized it was time to leave. He had had his bags packed (two suitcases) ever since coming back to Germany. One day he just picked up the bags and took the train to Vienna. What had made him especially suspicious was that the Nazis had proclaimed that <u>no Jewish professors</u> would be disturbed or would lose their passports. He realized that this was the last possible moment to go. The next train to Vienna was boarded by Nazi soldiers who precluded anybody of Jewish ancestry from leaving Germany.

In Vienna Leo worried about doing something to help take care of all the academic people who he was certain would be leaving Germany by the droves. He talked with several people who suggested he talk to Beveridge who was staying in the same hotel (Regina). Beveridge said there might be a problem and urged Leo to come back to England to keep reminding Beveridge to do something - that after a while he would. Leo did this and worked for a year helping to organize and allocate privately raised monies for fellowships. They decided to provide help principally to younger persons - the well-known academicians would be able to find positions on the basis of their reputations.

Leo worked at this until the main flood of refugees was finished. Then, having gotten a job for everyone else, he himself was out of a job and rather hard put. He did two experiments in the nuclear physics field at St. Bartholomew's during the summer of 1934 when he could use the radiation equipment. Even before this - while in Germany - Leo had thought and talked about doing nuclear physics. In 1931 he had discussed this with Lisa Meintner. She doubted he could do anything useful since his entire prior experience had been with probability theory and statistics.

The two experiments led to the Szilard-Chalmers effect and to the production of slow neutrons from bombarding beryllium. These, he said, are "useful neutrons" since this is about the only way you can label a neutron - by changing its energy.

He was given a 6 month fellowship at N.Y.U. by friends. On the strength of the St. Bartholomew's work he was invited to the Cavendish Lab at Oxford. Cambridge wouldn't have him because the work had not yet been confirmed at Cambridge. He felt lonely at Oxford for the first time in his life - "being <u>at</u> Oxford but not <u>of</u>

Oxford leaves you out of things." He had received the offer from Oxford while at N.Y.U. They couldn't match the British offer - but he got Oxford to agree to his spending 6 months at Oxford and 6 months in the U.S. In the U.S. he lived off savings (from the German inventions) and the Oxford salary, which wasn't much.

At the time of the Munich crisis Leo wrote a letter (to Oxford?) predicting with some certainty that there would be a general war within 2 years.

He indicated that if he could work on war work he would stay in England. If not, he would immigrate to the U.S. The British wouldn't let any foreigner work on war work. But since uranium was not useful and since non-useful things were not secret, refugees could work on uranium studies. He emphasized that there could not have been a bomb if it hadn't been for British contributions. A memorandum on the subject of a bomb from uranium originating from German refugees in England was brought to the U.S. as part of the British contribution to the Allied scientific efforts. This is mentioned in the Smythe Report. The U.S. has not given due credit to those responsible for this British contribution in its official histories.

Leo came to the U.S., lived off savings, worked as a guest worker at Columbia. He had suggested to Fermi that they do the experiment Leo's way, i.e., using slow neutrons from beryllium. Fermi wanted to do it his way. He tried and failed. When Leo got a radium source by borrowing $2000 he had the answer within

32 hours - by watching the surges above background. Then Fermi came over to his way of doing it and within a week the problem was completed. Leo wanted not to publish the results. He had written to Joliot to this effect. They had a meeting with Teller, Condon(?), Fermi and others and decided against publishing. Fermi wanted to publish but abided by the "majority rule." Then Joliot published comparable results and Fermi insisted.

Conant visited him at this time. Szilard was certain they could make a bomb. Conant was doubtful. Szilard said "we just don't understand each other."

[I asked Leo whether the hypothesis that the Germans <u>avoided</u> working on a bomb for <u>moral</u> reasons were true or not.] He said, "only at a subconscious level." They had no sense of being able to do the job and hence didn't have the motivation to do it. After the war he asked Heisenberg why they hadn't done it. Heisenberg said that he had known that one critical experiment had been done by some responsible German scientist using carbon and uranium, but this had failed and the idea had been given up.

May 25, 1961

Dear Leo,

Here is something that Dr. Livingston thinks will interest you.

Would you like to make corrections, amendments, etc. and return to us? You can keep the copy if you like.

Regards,

Andy

For Dr. Livingston

V-62

Leo Szilard - Biographical Notes

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It was his father who interested him in science. His father had engineering training but was really an entrepreneur getting jobs for building bridges, etc. Budapest was really an elegant place for the relatively well-to-do. It was beneath anyone to speak of money, or to ask how much something would cost.

- My father explained photography to me. I was really interested. I knew about what was the apparatus, the bulb, the box, the ground glass screen, etc. My father explained that the image was brought into focus on the ground glass screen - then the photographer squeezed the bulb and glue ran down on the image to hold it there.

In 1931 Leo had a post in Berlin but was already concerned about Hitler's rise. He had made a few inventions which later helped him support himself. He went to the U.S. on an immigrant visa. It was easy to get one. The quota was only 800 but nobody wanted to leave Hungary. He established residence and went back to Germany on a re-entry permit. In 1933 just about the time of the Reichstag fire he realized it was time to leave. He had had his bags packed (two suitcases) ever since coming back to Germany. One day he just picked up the bags and took the train to Vienna. What had made him especially suspicious was that the Nazis had proclaimed that <u>no Jewish professors</u> would be disturbed or would lose their passports. He realized that this was the last possible moment to go. The next train to Vienna was boarded by Nazi soldiers who precluded anybody of Jewish ancestry from leaving Germany.

In Vienna Leo worried about doing something to help take care of all the academic people who he was certain would be leaving Germany by the droves. He talked with several people who suggested he talk to Beveridge who was staying in the same hotel (Regina). Beveridge said there might be a problem and urged Leo to come back to England to keep reminding Beveridge to do something - that after a while he would. Leo did this and worked for a year helping to organize and allocate privately raised monies for fellowships. They decided to provide help principally to younger persons - the well-known academicians would be able to find positions on the basis of their reputations.

Leo worked at this until the main flood of refugees was finished. Then, having gotten a job for everyone else, he himself was out of a job and rather hard put. He did two experiments in the nuclear physics field at St. Bartholomew's during the summer of 1934 when he could use the radiation equipment. Even before this - while in Germany - Leo had thought and talked about doing nuclear physics. In 1931 he had discussed this with Lisa Meintner. She doubted he could do anything useful since his entire prior experience had been with probability theory and statistics.

The two experiments led to the Szilard-Chalmers effect and to the production of slow neutrons from bombarding beryllium. These, he said, are "useful neutrons" since this is about the only way you can label a neutron - by changing its energy.

He was given a 6 month fellowship at N.Y.U. by friends. On the strength of the St. Bartholomew's work he was invited to the Cavendish Lab at Oxford. Cambridge wouldn't have him because the work had not yet been confirmed at Cambridge. He felt lonely at Oxford for the first time in his life - "being _at_ Oxford but not _of_

Oxford leaves you out of things." He had received the offer from Oxford while at N.Y.U. They couldn't match the British offer - but he got Oxford to agree to his spending 6 months at Oxford and 6 months in the U.S. In the U.S. he lived off savings (from the German inventions) and the Oxford salary, which wasn't much.

At the time of the Munich crisis Leo wrote a letter (to Oxford?) predicting with some certainty that there would be a general war within 2 years.

He indicated that if he could work on war work he would stay in England. If not, he would immigrate to the U.S. The British wouldn't let any foreigner work on war work. But since uranium was not useful and since non-useful things were not secret, refugees could work on uranium studies. He emphasized that there could not have been a bomb if it hadn't been for British contributions. A memorandum on the subject of a bomb from uranium originating from German refugees in England was brought to the U.S. as part of the British contribution to the Allied scientific efforts. This is mentioned in the <u>Smythe Report</u>. The U.S. has not given due credit to those responsible for this British contribution in its official histories.

Leo came to the U.S., lived off savings, worked as a guest worker at Columbia. He had suggested to Fermi that they do the experiment Leo's way, i.e., using slow neutrons from beryllium. Fermi wanted to do it his way. He tried and failed. When Leo got a radium source by borrowing $2000 he had the answer within

32 hours - by watching the surges above background. Then Fermi came over to his way of doing it and within a week the problem was completed. Leo wanted not to publish the results. He had written to Joliot to this effect. They had a meeting with Teller, Condon(?), Fermi and others and decided against publishing. Fermi wanted to publish but abided by the "majority rule." Then Joliot published comparable results and Fermi insisted.

Conant visited him at this time. Szilard was certain they could make a bomb. Conant was doubtful. Szilard said "we just don't understand each other."

[I asked Leo whether the hypothesis that the Germans avoided working on a bomb for moral reasons were true or not.] He said, "only at a subconscious level." They had no sense of being able to do the job and hence didn't have the motivation to do it. After the war he asked Heisenberg why they hadn't done it. Heisenberg said that he had known that one critical experiment had been done by some responsible German scientist using carbon and uranium, but this had failed and the idea had been given up.

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12 copies

MATERIAL CONCERNING LEO SZILARD'S CONTRIBUTIONS TO THE DEVELOPMENT OF ATOMIC ENERGY

Enclosed is

(quote)

1) A letter to the editor of the Physical Review ~~by Szilard and Zinn~~, which describing their ~~the~~ discovery of the neutron emmission and in the fission of uranium and states ~~stating~~ that about two neutrons are emitted per fission.

[illegible]

2) Three letters written by Szilard to Fermi in July, 1939, proposing the use of the ~~a~~ carbon-uranium system.

3) A letter by Einstein to President Roosevelt, which was written after Szilard presented to Einstein his reasons for believing that a ~~the~~ graphite-uranium system is likely to sustain a chain-reaction. the enclosed Attached to that letter was a memorandum written by [illegible].

4) A memorandum, submitted by Szilard to Dr. Briggs in October, 1939. This memorandum refers to Szilard having estimated the chances of maintaining a chain-reaction in the lattice of uranium-spheres embedded in graphite.

5) ~~A copy of~~ excerpt from a paper submitted by Szilard, which was accepted for publication by the Physical Review in 1940. This paper ~~which~~ presents a rough theory of the ~~a~~ chain-reacting uranium-graphite system. On the basis of ~~[illegible]~~ ~~constants, Szilard derived from~~ the measurements of which Joliot and his co-workers ~~he concludes~~ on a water-uranium system have that one can maintain a chain-reaction in a sufficiently large mass of graphite and uranium ~~and~~ Szilard ~~[illegible]~~ He states that it should be possible to control the chain-reaction in such a system by slowly moving ~~absorbing bodies~~ neutron absorber to position of higher neutron density. For reason of secrecy this paper was not published. The text of the paper is identical with the report A-55 of the old Uranium Committee.

stet

[illegible]

LIST OF SCIENTIFIC PAPERS BY DR. LEO SZILARD BETWEEN 1947 and 1954.

1) A. Novick and Leo Szilard - EXPERIMENTS ON LIGHT-REACTIVATION OF ULTRA-VIOLET INACTIVATED BACTERIA. Proceedings of the NATIONAL ACADEMY OF SCIENCES. Vol 35, No.10, pp.591-600.

2) Aaron Novick and Leo Szilard - VIRUS STRAINS OF IDENTICAL PHENOTYPE BUT DIFFERENT GENOTYPE. Science, January 12, 1951, Vol 113, No. 2924, pp. 34-35.

3) Aaron Novick and Leo Szilard - EXPERIMENTS WITH THE CHEMOSTAT ON SPONTANEOUS MUTATIONS OF BACTERIA. Proceedings of the NATIONAL ACADEMY OF SCIENCES. Vol 36, No.12, pp. 708-719, December, 1950.

4) Aaron Novick and Leo Szilard - DESCRIPTION OF THE CHEMOSTAT. Science, December 15, 1950. Vol 112, No. 2920, pp. 715-716.

5) Aaron Novick and Leo Szilard - EXPERIMENTS ON SPONTANEOUS AND CHEMICALLY INDUCED MUTATIONS OF BACTERIA GROWING IN THE CHEMOSTAT. Cold Spring Harbor Symposia on Quantitative Biology. Vol XVI, 1951.

6) Aaron Novick and Leo Szilard - ANTI-MUTAGENS. Nature, Vol 170, p.926. November 29, 1952.

7) A. Novick and Leo Szilard - EXPERIMENTS WITH THE CHEMOSTAT ON THE RATES OF AMINO ACID SYNTHESIS IN BACTERIA. Dynamics of Growth Processes. Princeton University Press, pp.21-32, 1954.

The first of these papers(#1) investigates a phenomenon discovered by A. Kelner after the war, who showed that bacteria "killed" by ultra-violet light can be revived by shining visible light on them. Experiments designed to analyse the phenomenon are described in this paper; they lead to the conclusion that the ultra-violet light produces a "poison" which can be inactivated by light and that this "poison", if present when, subsequent to irradiation, the bacteria divide, will cause both death and mutations.

The second paper (#2) describes the discovery that, when a bacterium is infected simultaneously with two related viruses which differ from each other both in genotype and phenotype, the virus population emerging from the bacterium contains a class of viruses which have the genotype of one and the phenotype of the other.

The papers #3 to #7 describe a new way of studying bacteria by maintaining a bacterial population in a stationary (exponentially growing) state indefinitely and controlling the ~~growth~~ rate by controlling the rate of supply of an essential growth factor. An apparatus is described in these papers which will conveniently accomplish this and which is designated as the Chemostat.

In studying mutations in bacteria or the formation of adaptive enzymes in bacteria inaccurate, and therefore misleading, results are frequently obtained by studying bacterial cultures in flasks in which the number of bacteria increases exponentailly and today the use of the Chemostat appears to be indispensable.

In the papers #3 to #6 , the Chemostat is used in the study of mutations. It turns out that the rate at which mutations occur in a growing bacterial population under the conditions studied is not proportional to the rate at which cell division occurs, rather the mutation rate is constant per unit time independent of the rate at which the culture is growing. There is found one group of compounds, all purine derivitives, of which caffein is one, which greatly increases the mutation rate without having an appreciable killing effect on the bacteria.

There is another group of compounds described in these papers, all of them ribosides of purines which in small quantities will completely counteract the action of the above mentioned purine type mutagens and also reduce the rate of spontaneous mutations.

In paper #7, the Chemostat is used to study the bio-synthesis of amino acids in bacteria and the reglatory mechanisms which are involved in it. The bio-synthetic apparatus of the bacteria respond to amino acid concentrations in the medium, which

are exceedingly low. For instance, a bacterium which can make arginine and will do so if there is no arginine in the medium, will stop making arginine if an arginine concentration of 10^{-9} gm/cc is maintained in the medium in the Chemostat. (Novick and Szilard - unpublished.)

One way of studying such regulatory mechanisms is based on the use of a mutant which is blocked in the synthesis of an amino acid - in our case Tryptophane - and which pours out into the medium a "precursor" of that amino acid. Paper #7 utilizes such a mutant. In the absence of Tryptophane in the medium, a precursor of Tryptophane is poured out by the mutant into the medium at a rate which is independant of the growth rate of the bacteria. In the presence of Tryptophane this "precursor" is <u>not</u> poured out by the bacteria. It is conceivable that this indicates a general phenomonen of regulation through a negative feed-back of the final product at one of the <u>early</u> steps of the metabolic pathway leading to Tryptophane.

This type of phenomena will now be investigated by Dr. Bernard Davis in the Department of Pharmacology at New York University. The kinetics of adaptive enzyme formation will be investigated by Dr. Novick in the Department of Microbiology at the University of Chicago by means of the Chemostat.

Compiled by Univ. of Col. 1949

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Leo Szilard 1955

Curriculum Vitae

I was born in Budapest, Hungary in 1898. I went through officers' school there during the first World War and studied engineering there.

In 1920 I left Hungary to continue my engineering studies in Berlin. However, the attraction of physics proved to be too great. Einstein, Planck, Von Laue, Schroedinger, Nernst, Haber, and Frank were at that time all assembled in Berlin and attended a journal club in physics which was also open to students. I switched to physics and obtained a Doctor's degree in physics at the University of Berlin under Von Laue in 1922. My thesis (1 - see attached list of publications) showed that the Second Law of Thermodynamics covers not only the mean values, as was up to then believed, but also determines the general form of the law that governs the fluctuations of the values.

Subsequently, I was a research worker in one of the Kaiser Wilhelm institutes in Berlin and later joined the teaching staff of the University of Berlin (as Privatdozent) where I remained until 1933. Of the papers (1 - 4) published during this period, some are experimental, and some are theoretical. The last one (4) established the connection between entropy and information which forms part of present day information theory.

In 1933 I went to England. I considered at that time becoming a biologist, and A. V. Hill said that he would find a position for me as a demonstrator in physiology. It occurred to me, however, just then that a nuclear chain reaction might be possible if we could find an element that would emit neutrons when bombarded by neutrons. Artificial radioactivity was discovered a few months later by Joliot and seemed to provide an important new research tool in nuclear physics. This decided me to move into nuclear physics.

In the summer of 1934 I started work as a guest in St. Bartholomew's Hospital in London and this work resulted in the establishment of the Szilard-Chalmers Reaction (5) and the discovery that slow neutrons are emitted by beryllium if the beryllium is exposed to gamma rays of radium (6). In 1939, after the discovery of the fission of uranium, the use of these slow neutrons from beryllium made it possible to see that uranium emits neutrons when bombarded by neutrons; the fast neutrons emitted by uranium could be easily distinguished from the bombarding slow neutrons.

In 1935, after a visit to New York, where I spent a few months as research associate at New York University, I accepted a position at the Clarendon Laboratory, Oxford University. During this period I worked in the field of nuclear physics (8-11). In 1938 I came to America under arrangement with Oxford University, which permitted me to spend half my time in the United States. I was in the United States during the time the Munich Agreement was negotiated. After Munich I decided to stay in the United States on a full-time basis, and I resigned at Oxford.

In January 1939 I learned of the discovery of fission. It seemed important to find out at once if neutrons are emitted in that process, for in that case a chain reaction in uranium had to be regarded as a serious possibility. I therefore asked the permission of Columbia University to work there as a guest and perform an experiment in order to settle this question. This experiment (jointly performed with Walter Zinn) led to the discovery of the neutron emission of uranium, upon which the chain reaction is based (12, 13). The same discovery was made independently at about the same time by Fermi and his co-workers and by Joliot and his group.

In July, 1939, I recognized that a chain reaction might be set up in a system composed of graphite and uranium. Because of the serious consequences of this possibility, it seemed that this was a matter in which the government ought to take an interest. I therefore went to see Professor Einstein to enlist his help in approaching the government. After several consultations, in which E. P. Wigner and Edward Teller participated, Einstein wrote a letter to President Roosevelt; and in response to this letter, the President appointed a committee under the chairmanship of the Director of the National Bureau of Standards.

In November of ~~February~~ 1940 I described the chain-reacting uranium-graphite system in a paper I sent to the Physical Review (February, 1940). For reasons of secrecy, this paper was not published.

In November of 1940 a government contract was given to Columbia University for the development of the graphite-uranium system, and I became a member of Columbia University's National Defense Research Staff. Early in 1942 our group was moved to the University of Chicago; and on December 2, 1942, the chain reaction system was put into action.

Recently a patent was granted to the Atomic Energy Commission on the chain-reacting graphite-uranium system, jointly in the names of Enrico Fermi and myself.

After the war, in 1945, I took a leave of absence and spent six months in Washington, working on atomic energy legislation. Initially an atomic energy bill, the May-Johnson Bill, was introduced and it was necessary for the physicists to explain to the legislature the weaknesses of this bill. The May-Johnson Bill, though favorably reported by the Military Affairs Committee of the House, never got a rule, and another bill, introduced by Senator MacMahon, was passed.

In October, 1946 I joined the staff of the University of Chicago as Professor of Biophysics in the Institute of Radiobiology and Biophysics.

This institute never grew as originally intended, it had a succession of directors, and it was recently dissolved. I remained on the staff of the University of Chicago but have so far not joined any department in the biology division.

I should perhaps mention here that I have been for a number of years also Visiting Professor in the Department of Biophysics of the Medical School at the University of Colorado.

When in 1946 I was faced with the task of convertin myself into a biologist, I teamed up with Dr. Aaron Novick, a physical chemist. I have known him from his work in the uranium project. We both got our training in biology through summer courses, such as Dr. Delbruck's course in Cold Spring Harbor in bacterial viruses, and Dr. VanNiel's course in bacterial bio-chemistry at Pacific Grove. Dr. Novick and I worked as a team until recently when the Institute of Radiobiology and Bio-physics was dissolved.

A list of publications is attached, containing a short description of each paper. When we started out, we tried to understand a striking phenomenon just then discovered by A. Kelner, who showed that bacteria killed by ultra-violet light can be reactivated by shining visible light at them. (1) A detailed analysis of the phenomenon enabled us to interpret it in terms of a "poison" that is produced by ultra-violet light and is decomposed by visible light. This interpretation was at first controversial due to Dulbecco's work on light reactivation of ultra-violet killed bacterial viruses, but has in the meantime, become universally accepted. My own interest in the subject waned when I could not convince myself that we were dealing with a phenomenon that serves a useful biological purpose in the life of the bacteria.

Next, we turned our attention to the study of bacterial viruses in the assumption that viruses may prove to be much simpler than bacteria. We obtained some very interesting results (2) but decided to shift after a while to the study of the bacteria themselves.

The two phenomena in which we were particularly interested were a) mutations and b) the formation of adaptive enzymes which promised to provide a tool for the study of protein synthesis.

We were dissatisfied, however, with the methods that were available for the study of these phenomena. It seemed to us necessary to study bacterial populations in the growing condition in a stationary state, i.e. we thought we ought to use a continuous flow device. We developed such a device, which we called a "Chemostat." In this particular device the rate of growth of the bacteria can be changed by changing the concentration of one of the growth factors of our choosing which we make the controlling growth factor.

We started out by using the "Chemostat" for the study of mutations and obtained quite unexpected results at the very outset. It turned out, for instance, that the rate at which certain mutations occur does not change when we change the rate at which the bacteria divide; we could vary the rate of growth within a wide range without changing the rate at which these mutations occurred. We found one family of compounds - purines - which may cause an about tenfold increase in the mutation rate of bacteria without any appreciable killing. And we also found antimutagens, which in very small contractions will fully counteract the effect of purine-type mutagens.

In a bacterial population maintained in the "Chemostat" there occurred evolutionary changes (3) and that one strain of bacteria is replaced by a mutant strain, which can grow faster in the conditions prevailing in the growth tube of the "Chemostat." We observed successive evolutionary steps of this sort in each experiment of sufficiently long duration and were able to analyse the phenomenon.

Experiments on adaptive enzyme formation performed by means of the "Chemostat" are still in their infancy but it seems that the "Chemostat" will prove to be a necessary tool in that field also.