NATIONAL SCIENCE FOUNDATION
Washington 25, D. C.

April, 1953

International Travel Grants

The National Science Foundation has established a program to defray expenses of attendance of American scientists at international scientific meetings and congresses abroad. This program fosters the exchange of scientific information for the mutual benefit of all participating nations and provides United States scientists with face-to-face contact with foreign research activities and personnel. The benefits from attendance at these meetings accrue to this country not only in terms of the increased competence of our scientists but also in terms of the international good-will, both scientific and cultural, which is created.

Applications for travel grants to support attendance at international scientific meetings may be obtained by writing the National Science Foundation, Washington 25, D. C. Completed applications should be returned to the same address.

Applications are evaluated and travel grants awarded on the basis of the scientific competence of the applicant, the nature of the meeting to be attended, and the potential benefits which would accrue to the scientist, his sponsoring institution, and the meeting as a result of his attendance. In considering requests for travel grants, the Foundation may call upon outside individuals or organizations for advice and assistance in making the evaluation.

Travel grants normally are based upon cost of domestic travel to and from the point of embarkation and round-trip air passage to the meeting plus a limited allowance toward subsistence. Appropriate adjustments may be made to take into account other sources of support to the individual and availability of Foundation funds.

Upon return from international meetings recipients of National Science Foundation travel grants are requested to submit a brief report of significant aspects of the meeting. They are also invited to visit the Foundation if in Washington. The U. S. Department of State maintains Science Attaches in London, Paris, Stockholm, and Bonn, and scientists are cordially invited to call upon them.

GPO 83-51462

F-24,

N.S.F. application 1956
in Biography Box

N.S.F

QUESTIONNAIRE

(1) Name _____

(2) Have you been abroad before? _____

(a) If so, when? _____

(b) What were the circumstances? _____
_____
_____

(3) Do you have a research grant or contract with another Federal agency? _____

(a) If so, what agency? _____

(b) Have you explored this source of support for your travel needs? _____

(c) If so, what has been the agency's response? _____
_____
_____

(4) Are you an employee of the Federal Government? _____

(a) If so, name the agency. _____

(b) Have you explored that source of support for your travel needs? _____

I am sending a copy of my revision of your visit to Chicago. The original is to have not yet

April 20, 1956

Dr. Leo Szilard
Quadrangle Club
University of Chicago
Chicago 37, Illinois

Dear Szilard:

Consolazio in NSF would like to try setting up a roving professorship for you along the lines we suggested. He suggests the following:

That a proposal come from one institution. He suggests here.

That research be emphasized.

That the proposal be for support for five years.

The five-year period, of course, isn't so good from the standpoint of your security. Do you think this problem could be solved by your arranging with the University of Chicago for a leave of absence? This would protect you and save them your salary for as long as the scheme worked. Maybe leave could be on a part-time basis.

If you want to go ahead with the plan, could you write the proposed activities, including emphasis on research, in the three or four institutions?

I believe that if Caltech were to administer the grant, we could designate Chicago as your base. This would have to be checked.

Do you want to suggest a stipend and an amount you think you would need for travel?

We'll be glad to have any other suggestions you have plus instructions as to whether to go ahead or not.

We hope very much a satisfactory plan can be evolved, not only because of the good you could do science, but also to prove NSF can do something new and different.

Best personal regards,

G. W. Beadle
Chairman

GWB gc

cc: T. Puck
B. Davis
M. Delbrück

Ass: Dir:
John T:
Wilson

Dir Chief
Bur of Prod

COPY COPY

May 16, 1956

Dr. George W. Beadle
Division of Biology
California Institute of Technology
Pasadena, California

Dear Beadle:

Thank you for your letter of last week concerning the proposal for Szilard's Senior Investigatorship under the National Science Foundation. I think this plan will make maximum use of his unique talents, and I was enthusiastic about it when Ted Puck first told me about it and asked me if The Rockefeller Institute might possibly be interested to be a co-sponsor. I am sure from my conversations with people here that there are many individuals at this Institute who would profit from discussing their problems from time to time with Dr. Szilard. Furthermore, it should be one more desirable step toward inter-institutional cooperation in general, I believe.

Accordingly, I have talked the matter over with Dr. Bronk and with Dr. Whitaker, found them both aware of Szilard's potentialities, and am assured that they will be in favor of the plan for an Institute-wide basis. I have also found Dr. Frank Horsfall in favor of the proposal.

In consideration of the official connections our top administrative officers have with the National Science Foundation, it is proposed that I shall act for our Institute. It is to be clearly understood, however, that the whole institution would be within Szilard's field of action.

Please let me know whatever might be needed of me in furtherance of the application.

Sincerely yours,

Rollin D. Hotchkiss

COPY COPY

June 22
Aug 16

WA 3-2520

David Ben Asriberg

THE Windsor HOTEL
100 W. 58TH ST. AT 6TH AVE.
NEW YORK 19, N.Y.

Memo on proposed letter to be written by Dr. Warren C. Johnson, prepared by Leo Szilard.

D R A F T

Mr. William Consolazio
The National Science Foundation
Washington 25, D. C.

Dear Mr. Consolazio:

I am writing to you concerning the application for a Grant of Research Support that is being submitted to the National Research Foundation by the California Institute of Technology on behalf of Dr. Leo Szilard. The application is co-sponsored by the University of Chicago, the University of Colorado Medical School, the New York University College of Medicine, and the Rockefeller Institute for Medical Research.

Dr. Szilard is Professor of Biophysics at the Enrico Fermi Institute for Nuclear Studies. Prior to being transferred to this institute, Dr. Szilard was a member of the Institute of Radiobiology and Biophysics at this University, which has been discontinued. This University could make available to Dr. Szilard adequate laboratory facilities, and Dr. Szilard should have no difficulty in obtaining financial support for his work either from this University or from outside grants. However, Dr. Szilard feels that it would be inadvisable for him in his present ~~position~~ setting to try and assemble a group of research men, such as he would need to carry out an adequate program in biological research because there would be little assurance that these men if they made good would obtain staff appointments. For this reason Dr. Szilard would now prefer to work in

close collaboration with men who have already established positions at different universities rather than to build up a group of his own at this University.

I appreciate Dr. Szilard's point of view and so does the Administration of this University, and we shall be pleased to do anything that may help to make the arrangement proposed by the California Institute of Technology function smoothly.

Dr. Szilard is at present very much interested in work that is going on at this University on two aspects of protein synthesis -- adaptive enzyme formation and antibody formation. We hope that he will keep up his interest in this work also under the new arrangement and that he might get interested also in other work that is currently going on here or is planned for the future.

* * * * * * * * * * *

R e m a r k s:

To this text you could add such words, expressing appreciation of my continued presence at the University of Chicago, as you think fit. You might get some inspiration for this part of the letter from other letters of support which you will find in the back pages of the attached collection of documents which were sent to me by George Beadle.

Leo Szilard

Curriculum Vitae
(including list of Publications)

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 lthat 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 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 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. 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 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. (B1) 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 (2) 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 will fully counteract the effect of purine-type mutagens.

In a bacterial population maintained in the "Chemostat" there occur evolutionary changes (3) 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..

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

PARTIAL BIBLIOGRAPHY OF DR. LEO SZILARD*

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 Habilitationschrift 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 Nuetrons 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.

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*Some of Dr. Szilard's most important works still remain unpublishable, for reasons of national security.

(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.

(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.

PUBLICATIONS OF LEO SZILARD FROM 1948 - 1955

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-MUTAGES. 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.

The first of these papers (#17) 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 analyze 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 (#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 bio-synthesis 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/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 #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 conceiveble 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.

The Quadrangle Club
The University of Chicago
1155 East 57th Street
Chicago 37, Illinois

May 7, 1956

Dr. Linus Pauling
Chemistry Division
California Institute of Technology
Pasadena, California

Dear Pauling:

I saw you come into the lecture room when I gave a seminar at Cal Tech, but next day when I tried to reach you, I was told you had left for an extended tour.

Some time ago, you were kind enough to concern yourself with my problems and if I may, it seems that I might need your help again.

The Biology Division of Cal Tech (Beadle), the Department of Biophysics of the Medical School of the University of Colorado (Puck), and the Department of Pharmacology of the Medical School of NYU, have been discussing with each other and with me the possibility of asking the National Science Foundation to set up a Roving Research Professorship which would leave me free to pursue my scientific interests anywhere I desired and it would not be limited to these three institutions.

Apparently the officers of the National Science Foundation are interested in creating such a novel institution. The National Science Foundation would however not set up such a "fellowship" for more than five years.

The situation is complicated by the following calamity. If I stay at the University of Chicago and retire at the age of 65, about seven years from now, I would have a retirement income from Teachers Annuity of $113 per month. It is this low because my regular academic employment started in 1946.

Dr. Bernard D. Davis, head of the Department of Pharmacology at the NYU Medical School, and some of my other friends in New York, believe that it

might be possible to raise from foundations and private individuals funds to supplement what the National Science Foundation might be able to do, and create some sort of a "fellowship for life." They wish to enlist your help in this matter and you will receive a letter from Dr. Davis in the near future. He discussed with me what he proposes to do, and it sounds all right.

With kind regards,

Sincerely,

Leo Szilard

The Quadrangle Club
The University of Chicago
1155 East 57th Street
Chicago 37, Illinois

May 7, 1956

Dr. Harold C. Urey
Institute of Nuclear Studies
University of Chicago
Chicago 37, Illinois

Dear Urey:

You helped a great deal in arranging for my transfer to the Institute of Nuclear Studies and as a result my situation is much improved. Now another opportunity seems to present itself in which I think I need your help.

The Biology Division of Cal Tech (Beadle), the Department of Biophysics of the Medical School of the University of Colorado (Puck), and the Department of Pharmacology of the Medical School of NYU, have been discussing with each other and with me the possibility of asking the National Science Foundation to set up a Roving Research Professorship which would leave me free to pursue my scientific interests anywhere I desired and it would not be limited to these three institutions.

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With kind regards.

Sincerely,

Leo Szilard

Dr. Harold C. Urey
Institute of Nuclear Studies
University of Chicago
Chicago 37, Ill.

Dear Urey:

You helped a great deal
~~You were kind enough to help~~ in arranging my transfer to the Institute of Nuclear Studies. ~~This~~ which I believe, ~~is a~~ is a great improvement ~~over~~ of my ~~previous~~ situation ~~but it doesn't fully solve my problem.~~ I am now

(Teller letter)

Windsor Hotel
New York City

May 8, 1956

Mrs. Albert Lasker
3600 Prospect Avenue
Washington, D.C.

Dear Mrs. Lasker:

My present feeling is that it will not be easy to increase the number of those who become able scientists through federal aid. Probably much more could be accomplished by increasing the effectiveness of those who become scientists. But this of course is a different problem.

The following facts seem to be relevant to your problem. Roughly about 10% of those who graduate from college major in science, and roughly about 30,000 students graduate in science each year. This includes agricultural science but it does not include engineering. (You might assume that roughly about 20,000 graduate each year in engineering.)

On the basis of the enclosed pamphlet, we may assume that the number of high school graduates who would want to go to college but cannot do so because they don't have the cash, does not exceed 100,000. If we now assume that about 10% of these would major in science, and that they would all graduate, a "bill of rights for scientists" might increase the number of graduates in science by about 10,000 a year. This, if correct, would be quite substantial, but probably it is an over-estimate, for less than one-half of those who enter college do actually graduate. A $2,000 per year stipend for 10,000 freshmen would cost 20 million dollars in the first year of the plan, and later on when the plan is in full swing it would cost 80 million dollars per year.

My impression is that we would have to give much more thought to this matter before we can come up with a clear proposal on federal aid that, say,

Governor Stevenson might put forward in one of his speeches. Perhaps we can talk about this again when you are back in New York.

Incidentally, I was wrong when I told you that Charles Edison is concerned about this problem. He is merely Honorary Chairman of the Thomas Alva Edison Foundation. The man who is really active there is Charles Kettering. If you really want to go more deeply into all this, I can get you more factual information from that Foundation. But you are doing such important work in the public health field that you may not want to spread yourself thin by going too deeply into the problem of "education of scientists."

I am on the point of leaving for Washington where I expect to be just on Wednesday. I shall therefore be unable to read this letter after it has been typed and in order to avoid delay it will be sent to you without my signature.

With kind regards.

Sincerely,

Leo Szilard

j
enclosures 2

P.S. Enclosed is a memorandum I sent to the Edison Foundation. No legislation is needed for implementing this type of a proposal.

L.S.

Dictated, not re-read.

C
O
P
Y

May 7, 1956

MEMORANDUM TO: Mr. George E. Probst
Thomas Alva Edison Foundation, Inc.

FROM: Leo Szilard, The Quadrangle Club
The University of Chicago, Chicago 37

SUBJECT: How To Interest High School Students in Science

I have discussed with you and some others a specific approach to the problem of how to interest high school students in science. The proposal, in its present form, is limited to cities. It is proposed to hold in the cities classes on Saturday mornings to which high school students will be invited. These classes will be open to all high school students but admittance of students to these classes will be based on a personal interview and considerable prestige may be attached to being admitted. It is assumed that most of those who are admitted will be juniors and seniors but some sophomores and very few freshmen may be admitted also. Each class should be limited from 20 to 25 boys and girls. The classes should not start too early so as to allow on Saturday the boys and girls one hour of extra sleep and sufficient time to travel from their homes to the location of the "Saturday School."

It is essential that the teachers of these Saturday Schools have the kind of personality that will transmit enthusiasm for the subject. Again, some prestige should be attached to the selection of the teachers - engineers, college teachers, and among high school teachers those who have outstanding abilities could be used. The teachers should be paid no less than $50.00 and no more than $75.00 for each Saturday on which they serve. In a city like New York, conference rooms in the offices of large corporations who are technically oriented, such as the Union Carbide and Carbon Corporation,

Standard Oil of New Jersey, etc., might be used and these corporations might also provide teachers.

The purpose of the "Saturday School" is not to teach skills or specialized knowledge, but to give the boys and girls a feel of what science is about. Teaching could follow either of two patterns. One could take a subject like "electricity and magnetism" and teach it on the level of Faraday experiments, ending up at the end of the year with having transmitted a fair knowledge of the subject. Or else, one could follow a suggestion made to me by Dr. Maury Fox of the Rockefeller Institute, with whom I discussed this subject; take a problem that is interesting and follow through the various ramifications of this problem. This would in a natural way lead to a diversity of scientific questions. This is the way in which the most gifted students acquire a broad knowledge of science and if enough scientific problems are pursued in this manner, after a while a fairly good coverage of the field can be accomplished.

The Saturday School should include luncheon in order to give the boys and girls an opportunity to discuss with each other such scientific subjects as they happen to be interested in. It may be that the boys and girls would thus learn more from each other than from the teachers. One of the purposes of the "Saturday School" must be to bring into contact with each other young people who have in common an interest in science and who may stimulate each other and compete with each other.

It is my thought that the expenses of this operation as far as teachers' remuneration is concerned might be covered by a grant by the National Science Foundation whereas the expenses of administration might, initially at least, be covered by private foundations, such as the Thomas Alva Edison Foundation.

One might start out by setting up a "Saturday School" in New York, holding six parallel classes at six different locations, each Saturday during the school year.

In order to determine whether this should be attempted, it will be necessary first to go to a few high schools, explain the proposition to the boys and girls, and find out how many of them would want to attend such a school. If 1% respond, then it would be well worth setting up the organization here proposed.

It may be assumed that perhaps 1/3 of the pupils will show real promise and will want to work more intensely. These boys and girls ought to be taken care of Saturday afternoon on a tutoring basis by the same teacher who gives the class on Saturday morning. He would recommend to each pupil individually what books he should read and, if the pupil during his reading through the week has difficulties he would be helped over these difficulties by the tutor on Saturday afternoon and can then go on with his reading throughout the following week. Most pupils might not need such help except perhaps every second or third week. Such a tutoring program would greatly enhance the value of the "Saturday School" of science and should be regarded as an integral part of the program.

Please let me know what you and your friends think about this proposal and if you think I can be of any further help.

D
R
A
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T
2nd

Windsor Hotel
New York City
May 2, 1956

Mrs. Albert Lasker
3600 Prospect Avenue
Washington, D.C.

Dear Mrs. Lasker:

Since I saw you I have given some thought to the problem that you raised, i.e., whether we can think of any federal legislation that might increase the number of gifted students majoring in science in college. The Joint Committee on Atomic Energy has been taking testimony on this subject, but I am told that most witnesses merely describe the situation and that no one was able to make any reasonable proposal on legislation.

My present feeling is that it will not be easy to increase the number of those who become able scientists through federal aid. Probably much more could be accomplished by increasing the effectiveness of those who become scientists. But this of course is a different problem.

The following facts seem to be relevant to your problem. Roughly about 10% of those who graduate from college major in science, and roughly about 30,000 students graduate in science each year. This includes agricultural science but it does not include engineering. (You might assume that roughly about 20,000 graduate each year in engineering.)

On the basis of the enclosed pamphlet, we may assume that the number of high school graduates who would want to go to college but cannot do so because they don't have the cash, does not exceed 100,000. If we now assume that about 10% of these would major in science, and that they would all graduate, a "Bill of Rights for scientists" might increase the number of graduates in science by about 10,000 a year. This, if correct, would be

quite a substantial increase, but probably it is an over-estimate, for less than one-half of those who enter college do actually graduate. A 4000 per year stipend for 10 000 freshmen would cost $ 20,000 million in the first year and ultimately $80 million per year

My impression is that we would have to give much more thought to this matter before we can come up with a clear proposal on federal aid that, say, Governor Stevenson might put forward in one of his speeches. Perhaps we can talk about this again when you are back in New York.

Incidentally, I was wrong when I told you that Charles Edison is concerned about this problem. He is merely Honorary Chairman of the Thomas Alva Edison Foundation, and the man who is really active there in this field is Charles Kettering. If you really want to go more deeply into all this, I can get you more factual information from that Foundation. But you are doing such important work in the public health field that I am not sure that you may not want to spread yourself thin by going too deeply into the problem of "educstion of scientists."

I am on the point of leaving for Washington and

With kind regards.

Sincerely,

Leo Szilard

Dictated, not re-read.

P.S. Enclosed is a memo I sent to the Edison Foundation. No legislation is needed for implementing this type of a proposal.

Current Loss of Talent From High School to College: Summary of a Report

By CHARLES C. COLE, JR.*

ARE THERE STUDENTS of high ability not now entering college who could be induced to go to college and follow science careers? If so, what inducements would appear to be most practical in encouraging such students to continue their education? What results in terms of an increased supply of scientifically trained persons could be expected from these inducements?

Study of Loss From High School to College

These were some of the questions which the National Science Foundation asked the College Entrance Examination Board in the fall of 1954. The College Board in turn asked me to undertake a study that would measure the loss of high level ability from high school to college and that would evaluate various methods of decreasing this loss. Assisting me in this project were Mrs. Jane Blizard of the New England Institute for Medical Research, Paul Brandwein of the Forest Hills, N. Y., High School, Herbert A. Deane, of Columbia University, Robert J. Havighurst, of the University of Chicago, Mrs. Allaire U. Karzon, lawyer of Buffalo, N. Y., and Wesley W. Walton of the Educational Testing Service. A committee of prominent educators reviewed the approach, methods, and findings of the project and criticized the manuscript. The result of this study was a report, *Encouraging Scientific Talent*, submitted to the National Science Foundation in June 1955. Although the report is not now accessible for general distribution, printed copies should be available early next year.

In this report I have attempted among other things to describe the nature, characteristics, and origins of scientific ability, to bring together information on the supply of and demand for scientifically trained persons, and to suggest means for conserving the Nation's intellectual resources. The report also identifies certain deterrents to the production of scientists as well as certain factors which seem to encourage their training. Formal programs of recruitment, such as scholarship awards, the improvement of teaching, and the encouragement of a public opinion favorable to the production of scientists, are analyzed.

The rapid rate of growth in the scientific professions during the last 50 years has obscured a relative shortage of specialized persons in the labor force. As our industrialization matures and particularly when we are faced with a crisis like the Korean War, we discover that the phenomenal increase in the numbers graduating from college and graduate school has not been large enough to prevent critical shortages in certain professions. The realization is dawning on us that we need a new concept of scarcity in our society. Having temporarily solved many of the material shortages that plagued us in the past, shortages of food, clothing and shelter, of capital, of natural resources, of power, our atomic era has uncovered its special kind of scarcity—a shortage of trained ability. Decreasing the loss of talented youths from high school to college is, therefore, conservation of intellectual resources. This salvaging of capable high school graduates to higher education is a vital activity if the United States is to maintain its position of world leadership.

Questionnaire

How great is this loss of talented secondary school students? Although there have been many useful local and statewide surveys of school drop-outs and of the loss in transition from high school to college, up-to-date nationwide figures on secondary school students' educational and professional plans have been lacking. In order to learn more about vocational aspirations and college and financial plans of talented high school graduates in 1955, we asked the Educational Testing Service to undertake a nationwide research study that would involve a questionnaire and aptitude test administered to a random sample of public secondary school seniors and sophomores. The preparation and operation of the National Study of High School Students and Their Plans was under the direction of Glen Stice, Warren Torgerson, and William Mollenkopf.

*Assistant Dean of Columbia College, Columbia University.

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U. S. DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE

With the scoring of these questionnaire answers we now have nationwide figures on adolescent motivation for college, interest in science, financial plans, and parental backgrounds of the high ability students in our public high schools. In addition to revealing the extent of college-going intentions in 1955, some of these statistics throw light on the factors determining whether or not an intellectually gifted person goes to college.

The 30-minute questionnaire was completed and returned by 32,750 seniors in a random sample of public secondary schools with 12th-year students, ranked in order of size in each of the four major geographical regions. This represented a sample of approximately 2.6 percent of the public high school seniors in the United States at the time the questionnaire was administered. It was not practicable or financially possible to include private school students in the sample.

Aptitude Test

In conjunction with the questionnaire, a brief academic aptitude test was administered. This enabled us to identify the students sampled that could be reasonably considered as possessing high level ability. By so doing it was possible to apply a common test standard to all the students surveyed rather than rely on individual school estimates of high level ability. The analysis of questionnaires was undertaken only for the 9,689 seniors who scored 12 or higher on the ability test. This represented approximately 30 percent of the seniors in the sample. These students were designated "high scoring," and it was assumed that they possessed sufficient ability to do college work.

Understandably, this high scoring group was not evenly distributed geographically. A much larger proportion of the seniors in the West and Northeast scored 12 or higher on the test than in the South.

The percentage of seniors in this sample in each region scoring in the upper 30 percent is shown below:

Region	*Percent*
West	35
Northeast	34
Central	29
South	20

Hypotheses Tested

In devising the questions to be asked in the National Study of High School Students and Their Plans we wished to test a number of hypotheses about college-going. First, local studies have frequently shown that interest in college-going is greater among students from professional groups than from farm and labor classes. This hypothesis was confirmed by the national study. It reveals that 83 percent of the boys in the high-scoring group whose fathers are in medicine and 65 percent of the boys whose fathers are in scientific professions intend to go to college immediately after graduation. Only 38 percent of the sons of farmers and 25 percent of the sons of semi-skilled workers intend to go directly to college.

Another hypothesis that appears to be confirmed by the results of the study is that there is a direct relationship between interest in college-going and intellectual ability. As the following figures indicate, the higher the score on the ability test, the greater the percentage intending to continue their education:

Score on test	*Percentage planning to enter college full time, winter session, 1955*
12	34
13	36
14	41
15	44
16	51
17	54
18	58
19	62
20	69

Interest in college also appears to be closely related to parental education, classmates' plans, and the extent of high school guidance. Indeed, the questionnaire results suggest a considerable determinism involved in the process of planning for college. Insofar as guidance is concerned, the questionnaire responses revealed how different the interest in higher education is between those who have been exposed to high school guidance and those who have not. Two-thirds of the high ability boys who have discussed college "quite a lot" with their teachers or counselors are probably on college campuses this fall. Only 21 percent of those who had no such counseling intended to go to college this fall. Conversely, while only 2 percent of those who have discussed the subject a lot at school say they have no interest in higher education, 26 percent of those who have had no high school counseling about college lack the motivation to continue in school. Although these figures suggest a significant relationship one cannot conclude that there is necessarily a causal connection.

Vocational Interests

One of the questions asked of these seniors required them to think about what they would like to be 15 years from now. The answers reveal a considerable amount of realistic, practical vocational thinking by these young persons. More than 25 percent of the boys in the top 30 percent of ability said they would like to be engineers 15 years from now; 10 percent want to go into business; 8 percent into medicine; and 8 percent in other unspecified professional fields. Approximately 6 percent would like to become physical scientists. Only 1 percent aspire to become social scientists. Four percent aspire to each of the following: law and politics, white collar jobs, skilled labor, farming, and education. Insofar as the latter field is concerned, three times as many boys would like to be coaches as classroom teachers.

About 20 percent of the high-scoring girls, however, would like to be in education 15 years from now, 18 percent in white-collar jobs, and 12 percent in nursing and medical technical work. Evidently 12 percent of them desire solely to be housewives 15 years from now. No more than 1 percent of the high ability girls aspire to science or engineering, both of which continue to be masculine occupations in the eyes of adolescents.

Interest in science as a subject in high school appears to be strong among both sexes, however, if this sample of seniors with college level ability is any indication. Fifty-six percent of the high ability boys would have preferred to have taken more science courses in secondary school. Thirty-eight percent of the girls wanted more science in high school. Only 4 percent of the boys and 6 percent of the girls would have preferred less science work in school. The seniors revealed a similar attitude toward mathematics. Although their responses may have been influenced by the fact that they did not have to abide by what they said, these percentages are high enough to suggest that the high school student of high ability wants to take more science courses or realizes their importance in the world today.

The seniors who planned to attend college were asked to indicate what they would really be most interested in studying in their undergraduate years. Their answers showed the following preference among high-scoring boys: engineering, 24 percent; physical science, 8 percent; biological sciences, 2 percent; mathematics, 3 percent; pre-med or pre-dental courses, 6 percent, humanities, 10 percent; and social sciences, 2 percent.

Reasons for Not Attending College

It was also hoped that the questionnaire results would throw additional light on the extent to which financial barriers are an important deterrent to college-going among those of high ability. The seniors participating in the national study were asked, "If you do not go to college, what will the reason probably be?" No limit was put on the number of reasons an individual might circle, but they were asked to indicate their most important reason. Twelve percent of the high-scoring group reported that if they did not go to college the most important reason would be because it would cost more than they or their families could afford. Almost half of these high level ability students cited expense as an important reason why they might not get to college.

In answer to this question, about 25 percent of the high-scoring boys and 45 percent of the girls cited the lack of a college goal as possibly an important reason for not continuing their education. This suggests that there is considerable validity in believing that, despite the importance of financial need, lack of motivation for college is a stronger deterrent to college-going among those of high ability who do not go to college.

This was apparent in the answers to several other questions on the questionnaire. The seniors were asked to indicate what they would really like to do when they finished high school. They were also asked to indicate what they thought they would do after graduation. Fifty-one percent of the seniors in the top 30 percent of ability would really like to go to college full-time immediately after graduating from high school. Forty-five percent of the seniors expect to be able to do so. About 31 percent of the boys and 20 percent of the girls among the senior group scoring in the top 30 percent expect to be able to get to college by working for a while, or by combining part-time college attendance with work. About 9 percent of the boys and 18 percent of the girls say they have no interest in higher education. Half this number, or about 6 percent, would really like to go to college but see no way of getting there. The remaining 9 percent of the boys and 12 percent of the girls could not easily be classified because of the nature of their response or because they failed

to answer this particular question. However, it is evident from the replies of the latter group that although they do not intend to go to college, some of them would be interested in scholarship aid in order to do so.

Amount of Loss of Talent

It is possible to estimate the loss of talent from high school to college on the basis of these replies. It must be remembered, however, that what these seniors said they would do and what they actually did may not be the same. Follow-up studies of other groups have revealed that not all who intend to go to college even as late as their senior year in high school manage to go. In addition, the extent of college going among private school pupils may not be the same as it is among the public school population.

Taking these things into consideration, it would appear that in 1955 there are between 60,000 and 100,000 high school graduates of college ability who failed to enroll in college for financial reasons. There appears to be another group of approximately 100,000 in number who have high ability but who lack any interest in a higher degree.

Some of the principals whose schools participated in this study sent helpful comments about the factors in their communities either helping or hindering interest in higher education. It seemed evident that where there was a tradition of college going in the community, where economic conditions were good and where the school was adequately supported, adolescent interest in college was strong. In depressed economic areas or where schools were inadequately staffed or where the home had little interest in higher learning, a much smaller proportion of high school seniors included college in their plans. The economic barriers to higher education, strong though they are, are not, however, the only ones by any means. Prosperity also accounts for some of this loss of talent. One principal reported, "We are near two factories that employ many young women. They can make more within six months than a college graduate can in several years. Even the smart ones see no reason why they should go to college."

The statistics of the National Study of High School Students and Their Plans tend to validate the conclusion that every year up to 200,000 18-year-olds with college level ability are lost to higher education and presumably do not develop their talents to the fullest possible degree. This is a serious loss of intellectual resources that is all the more ironic in the face of current and future bulging enrollments. It is a costly loss in view of the Nation's shortage of scientifically trained persons.

Conclusions

The report, *Encouraging Scientific Talent*, ends with a number of conclusions. Among them are the following: There is no single dramatic solution to the problem of how to prevent the wasting of intellectual resources or of increasing the number of scientifically trained persons. There are losses to the future scientific labor force as a result of some poor teaching, insufficient guidance, and inadequate facilities in the Nation's overcrowded schools. Indeed, at present one of the most serious crises the Nation has ever faced is the shortage, almost the disappearance, of the competent, well-trained, stimulating high school science teacher. It is a breed that faces extinction. Third, although some able science students are deterred or discouraged from science or engineering careers because of the school situation, many more are turned away by deterrents in society itself. Fourth, partly because of these community and parental deterrents, lack of motivation appears to be a strong reason why a majority of the superior high school graduates fail to go on to college. Nonetheless, insufficient financial support appears to be the sole or primary reason why between 60,000 and 100,000 of these superior persons fail to enroll in colleges each year.

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THOMAS ALVA EDISON FOUNDATION, INC.

SUITE 805 8 WEST 40th STREET

NEW YORK 18, NEW YORK

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May 25, 1956

CONFIDENTIAL MEMORANDUM: ON A ROVING PROFESSORSHIP FOR LEO SZILARD

by Dr. George W. Beadle, Division of Biology, California Institute of Technology

Dr. Bernard D. Davis, Department of Pharmacology, New York University, College of Medicine

Dr. Theodore Puck, Department of Biophysics, University of Colorado, Medical School

We are exploring the possibility of setting up a Roving Research Professorship or a Fellowship-at-large for Dr. Leo Szilard, at present Professor of Biophysics at the University of Chicago. We believe such a position would offer the best opportunity for full exercise of his remarkable talents; and in addition it should eliminate his serious retirement problem, whose genesis is described below.

As we visualize this position, it would formally associate him at the outset with our three departments -- an arrangement we would greatly desire. At the same time, since we are convinced that a maximum of flexibility will enable him to function most effectively, he would be left free to pursue his scientific interests at any research institute or university of his choice. Dr. Rollin Hotchkiss, speaking for the Rockefeller Institute for Medical Research, has proposed that this institution be included also in the plan; and we assume the University of Chicago would wish to continue its association with Szilard.

Officers of the National Science Foundation whom we have approached favor, both in principle and in this case in particular, the idea of creating such a position. The financial assistance we anticipate from this agency, however, will not cover a sufficiently long period, and so we are seeking additional funds from private foundations and individuals.

There are not many scientists for whom a Roving Professorship would seem appropriate, but we believe Szilard is one. In addition to producing a number of discoveries of the highest rank, Szilard has been an unusually effective catalyst in science. In recent years he has engaged extensively in visiting other laboratories, where he has been most generous in giving thoughtful and deep attention to the work in progress. He has a unique ability to grasp instantly the most varied problems, to seize upon their significant aspects, and to apply to them unusual imaginative and critical powers. These visits have often led to valuable new experiments and have given to many young biologists a much enhanced sense of the distinction between significant and trivial problems. The Roving Professorship we are trying to arrange would regularize such peripatetic activities and would at the same time provide the opportunity, if his interests so directed, for an extended period of work at a single institution.

In order to put the problem concerning Szilard in the proper perspective, we wish to refer here to a few historical facts.

More, perhaps, than any other single individual, Szilard was responsible for getting the United States government started on the development of atomic energy. The crucial discovery was made in March, 1939, when three groups working independently (Szilard and Zinn; Anderson and Fermi; Halban, Joliot, and Kowarsky) found that neutrons were emitted in the fission of uranium. This meant that some uranium-containing system might be able to sustain a chain reaction. Shortly thereafter Fermi, Szilard, and Anderson jointly showed that a uranium-water system came fairly close to sustaining a chain reaction, and it thus became clear that the liberation of atomic energy on an industrial scale might be at hand.

These results were obtained in the spring of 1939, yet for a period of eight months, stretching from June, 1939 to March, 1940, not a single experiment on the chain reaction was in progress anywhere in the United States. No one can tell how much longer this state of inactivity would have lasted had not Szilard become convinced in July, 1939 that a chain reaction could be set up in a system composed of uranium and graphite.

Szilard convinced Einstein that this possibility must be taken seriously, and that the government ought to be so advised. On August 2, 1939, Einstein wrote to President Roosevelt:

> 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:
>
> In the course of the last four months it has been made probable - through the work of Joliot in France as well as Fermi and Szilard in America - that it may become possible to set up a nuclear chain reaction in a large mass of uranium by which vast amounts of new radium-like elements would be generated. Now it appears almost certain that this could be achieved in the immediate future.

Dr. Alexander Sachs personally transmitted Einstein's letter to President Roosevelt, together with a memorandum written by Szilard which contained information on which Einstein's letter was based.

In response to Einstein's letter, Roosevelt appointed a committee which first met in October, 1939. In March, 1940 this Committee made a $6,000. grant to Columbia University for the purchase of graphite, and only then did work on the chain reaction get under way.

A patent on the chain-reacting graphite-uranium system, subsequently issued to the government, named Fermi and Szilard as the inventors (see information released by the government and reported in the New York Times of May 8, 1955). This system was used in the pile that produced the first chain reaction at Stagg Field on the campus of the University of Chicago on December 2, 1942. And this system was also used in the Hanford plant which produced the plutonium for the atomic bomb. The same system is now used for electric power production in England. According to the Times report, "the patent is owned by the Atomic Energy Commission ... issuance of the patent establishes the priority of the Fermi-Szilard invention, and protects the government's interests."

Szilard did not profit financially from this invention, which he assigned to the government in 1943.

After the war Szilard joined the faculty of the University of Chicago and his main interest shifted to biophysics. He has made important contributions to this field. Jointly with Dr. Aaron Novick he developed the Chemostat, which permits the study of growing bacterial populations in a controlled steady state; and its use has led to important discoveries concerning mutations and adaptive enzyme formation.

At Chicago Szilard was initially attached to the Institute of Radiobiology and Biophysics, which had been established by Dr. Robert Hutchins when he was Chancellor. This Institute was recently discontinued, and on July 1, 1956, Szilard will join the Institute of Nuclear Studies of the University. While this assignment leaves him free to pursue his interests in biology, it does not provide an appropriate setting for these activities.

According to the present schedule, if he is retired by the age of 65, some seven years from now, Szilard would have to live on an income of $113 per month provided by Teachers' Annuity. (To this would be added social security benefits if he had no other earned income.) Szilard is in this anomalous position because he entered this country from Europe shortly before World War II, worked on a government project during the war, and did not take regular academic employment until 1946.

If the issue were merely Szilard's financial problem, he could perhaps solve it by taking a highly paid industrial position. Our concern, however, is to keep him active in basic science. This could be accomplished by setting up a fellowship-at-large providing for sufficiently long-term support. We believe we can count on assistance from the National Science Foundation amounting to a total of about $90,000. payable in five installments. An about equal amount

would be needed from other sources in order to secure an adequate basic income during his lifetime.

We believe the absence of an age limit in this arrangement is appropriate in view of both the nature of the position and the person involved. A fixed retirement age is customary for scientists who hold an administrative position or occupy a fixed amount of laboratory space, but neither of these considerations pertains to a fellowship-at-large. And as long as Szilard holds such a position and requires laboratory facilities for his research, he should be able to secure these on a short-term basis.

We believe it important to enable Szilard to continue to remain active in the field of science as long as possible, with adequate security, and under circumstances that permit him to be most effective. A fellowship-at-large without set age limit seems the best instrument for accomplishing this purpose. We would like to enlist the aid of interested parties in working out the details of such a plan and putting it into effect.

June 13, 1956.

1155 East 57th Street
Chicago 37, Illinois.

Mrs. Albert D. Lasker
405 Lexington Avenue
New York, N.Y.

Dear Mrs. Lasker,

This was quite a moving speech by Senator Anderson the other day; I thought he was really very effective.

Since I saw you I heard from Bernhard Dsvis. I believe I told you that George Beadle, Bernhard Davis and T.T. Puck are about to approach some foundations in trying to set up something workable for me. I now understand from Davis that you talked to him over the telephone and that you might be able to help part of the way. Davis thought that your offer to help will make it easier for them to approach other foundations.

Even though I am not really supposed to know what is going on at this level, it would be unnatural for me not to say Thank You! Hence I am writing these lines before returning to Chicago tomorrow morning.

With kindest regards,

Sincerely,

Leo Szilard

Mrs. Albert Barker
3600 Prospect Ave
Washington, D.C.

Adams 2-6888

P.S. 17

14,800

The Palmer House

CHICAGO - A HILTON HOTEL

Rockwell &
Mary Lasker &
Frank Altschul
(Teller)
Mrs. Joseph Pennoyer

Adele Levy

5[15]

LI 5-7885

3E 85
RE 47902

June 19, 1956

Harry Kalven
Leo Szilard

The Premises

1) If I were to retire from Chicago in about seven years at sixty-five years of age, I would receive from Teachers' Annuity monthly payments of $113. My father lived to be ninety-five years of age, and the prospect of living thirty years on this amount plus social security is a disturbing one.

2) I am assuming that Chicago will, in a few years, raise the retirement ~~year~~ age to sixty-eight, since this is what is being done at present at most other universities. I might, therefore, have at Chicago another ten years during which to save a few thousand dollars each year. This would not solve my retirement problem but it would make its solution somewhat easier.

The Proposal

The California Institute of Technology proposes to submit a request to the National Science Foundation for setting up a Senior Science Scholarship Award for me for five years. This proposal would be co-sponsored by the Rockefeller Institute, by the Department of Pharmacology of the New York University Medical College, and the Department of Biophysics of the Medical School of the University of Colorado.

There arises the question of whether the Institute of Nuclear Studies of the University of Chicago would be willing to be a co-sponsor also, assuming that the decision is that I should ask Cal Tech to go ahead.

As far as I understand, the National Science Foundation would be reluctant to make the contract for the Award with the University of Chicago because this might create the impression that they are subsidizing an existing professorship.

This Award would take care of my full salary for a period of five years, but there is no assurance that it would continue beyond that date. Thus if I were to receive it, I might lose five out of the ten years of security which I might have at Chicago. I hesitate to go ahead on this basis. Since you said that you might explore this situation with the University, it would now seem better to let the July 1st deadline lapse and to await the conclusions that you may reach.

The next deadline is September 15th, and if an application is made at that time, a decision will be made by the National Science Foundation about Christmas time. It was previously thought that this matter could be rushed by meeting the July 1st deadline, but this does not now seem to be advisable in any case.

Such a Senior Science Scholarship Award would represent a new institution to be created by the National Science Foundation. I understand that under its terms I would be free to collaborate not only with the above named institutions but with any institution where the pursuit of my research may take me. There would be no fixed time schedule and no fixed proportions of time set for the different institutions. According to a draft proposal which I am preparing, I would expect to cross the continent three times a year and, therefore, could convenient-

ly be in Chicago about six times in a year without substantially adding to my travel expenses. I could also retain Chicago as my place of residence, and I understand that Chicago could be so designated in the contract with the California Institute of Technology.

CONFIDENTIAL MEMORANDUM

TO: Dr. Bernard D. Davis

FROM:

This memorandum relates to the situation of Leo Szilard who, more than any other single individual, was responsible for getting the United States government started on the development of atomic energy. Szilard's present situation does not permit him to make full use of his exceptional abilities, and this is of concern to us and we understand that it is of concern to you also. We would like to try to help make arrangements that would permit the setting up of a Roving Research Professorship or Fellowship for Szilard which would leave him free to pursue his scientific interests at any research institute or university, depending on the problems to which he wishes to devote his attention. Having been something of a scientific knight errant most of his life, this kind of arrangement would give him the freedom and flexibility he should have.

Such an arrangement must not be regarded however as merely a convenience for Szilard. We understand that the Division of Biology of the California Institute of Technology, the Department of Biophysics of the University of Colorado Medical School, and your Department at New York University Medical School regard the establishment of such a "fellowship" for Szilard as a step toward creating a novel and needed institution which would fulfil a very useful function. We understand that you have discussed this problem with officers of the National Science Foundation, that they share these views, and that the National Science Foundation might assist in setting up such a "fellowship."

I assume that the University of Chicago would want to be included in what arrangement may be finally set up.

The role that Szilard played in the development of atomic energy in the United States has never been fully told. The crucial discovery was made in March, 1939, when three groups working independently (Szilard and Zinn; Anderson and Fermi; Halban, Joliot and Kowarsky) found that neutrons were emitted in the fission of uranium. Clearly this meant that uranium might be able to sustain a chain reaction, and that the liberation of atomic energy on an industrial scale might be at hand.

This discovery was made in the spring of 1939, yet for a period of eight months stretching from June, 1939 to March, 1940, not a single experiment on the chain reaction in uranium was in progress anywhere in the United States. No one can tell how much longer this state of inactivity would have lasted had not Szilard become convinced in July, 1939 that a chain reaction could be set up in a system composed of graphite and uranium.

Szilard persuaded Einstein that this possibility must be taken seriously, and that the government ought to be advised. A few months earlier, Fermi and Szilard (together with Herbert Anderson) had written a paper showing that a uranium-water system, even though not chain reacting, came fairly close to it. The manuscript of this paper served as the starting point of Szilard's discussion with Einstein.

On August 2, 1939, Einstein wrote to President Roosevelt:

> "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:

"In the course of the last four months it has been made probable - through the work of Joliot in France as well as Fermi and Szilard in America - that it may become possible to set up a nuclear chain reaction in a large mass of uranium by which vast amounts of new radium-like elements would be generated. Now it appears almost certain that this could be achieved in the immediate future."

Dr. Alexander Sachs personally transmitted Einstein's letter to President Roosevelt, together with a memorandum written by Szilard which contained information on which Einstein's letter was based.

In response to Einstein's letter, Roosevelt appointed a committee which met in October, 1939. In March, 1940 this committee made a $6,000 grant to Columbia University for the purchase of graphite, and only then, after a period of eight months of inactivity, did work on the chair reaction get under way.

According to information released by the government and reported in the New York Times of May 8, 1955, a patent on the chain-reacting graphite uranium system has been issued to the government naming Fermi and Szilard as the inventors. This system was used in the chain reaction that was first demonstrated at Stagg Field on the campus of the University of Chicago on December 2, 1942. And this system was also used in the plutonium plant at Hanford which produced the plutonium for the atomic bomb. The same system is used for electric power production in England. According to the Times report, "the patent is owned by the Atomic Energy Commission . . . issuance of the patent establishes the priority of the Fermi-Szilard invention, and protects the government's interests." Due to a peculiar set of circumstances, the government did not pay Szilard for the value of his inventions, but merely compensated him for his expenses in 1943, in the amount of $15,417.60.

After the war Szilard joined the faculty of the University of

Chicago and his main interest shifted to biophysics. He has made important contributions to this field. Jointly with Dr. Aaron Novick he developed the Chemostat which permits the study of growing bacterial populations in a controlled steady state, and its use has led to important discoveries on the subject of mutations and adaptive enzyme formation.

The Institute of Radiobiology and Biophysics, to which Szilard was attached, has, however, recently been discontinued, and on July first Szilard will join the Institute of Nuclear Studies at the University of Chicago. Szilard intends, however, to continue to pursue his scientific interests in the field of biology. While his present assignment leaves him free to do so, and while he might be given a laboratory at Chicago if he asks for it, it would be difficult for him to assemble a staff of younger people, since they could not hope for advancement at the University.

Moreover, if according to present schedules he is retired some seven years from now at the age of 65, he would have to live on a retirement income provided by Teachers' Annuity of $113 per month. To this would be added social security benefits, but only if he has no other substantial earned income. Szilard is in this anomalous position because he entered this country from Europe shortly before World War II, worked on a government project during the war, and did not take regular academic employment until after the war.

We could perhaps suggest to Szilard that he seek academic employment at some other University where the retirement age is 70 rather than 65, and that he supplement his income by acting as a consultant to a variety of projects. Or else we could suggest to him that he quit academic employment if an opportunity arises, and devote his attention to application of science rather than basic science. Either of these solutions might perhaps solve his financial problem, but they also would put an end

to Szilard's real productivity in basic science.

The need to increase the number of scientists in the United States is a much discussed topic today and is at present clearly recognized. It seems to us that this country ought to be able to afford to keep Szilard working in science.

We understand that you and some of your friends in New York are willing to explore the possibility of setting up some arrangement that would enable Szilard to continue his work in basic science unhampered by financial considerations. And if this memorandum is of any help to you in this connection, please feel free to make use of it. As stated before, a Roving Research Professorship or Fellowship that Szilard may hold without a set age limit might be the best solution of the problem. Perhaps a fund could be set up at the University of Chicago or some other university for this purpose, and the university could purchase a life annuity payable to it during Szilard's lifetime.

UNIVERSITY OF COLORADO
DEPARTMENT OF MEDICINE
4200 EAST NINTH AVENUE
DENVER 7, COLORADO

DEPARTMENT OF BIOPHYSICS

July 23, 1956

Dr. G. W. Beadle
Biology Division
California Institute of Technology
Pasadena, California

Dear Doctor Beadle:

This letter for transmittal to the National Science Foundation indicates our expectations with respect to Dr. Leo Szilard's contributions and responsibilities to this department, under the proposed arrangement for his appointment as a Scholar and Visiting Professor.

Dr. Leo Szilard possesses one of the most versatile and keenly analytic minds of our generation. His ability logically to dissect problems and to consider new and unconventional approaches to their solution is a scientific asset of enormous value.

We are expecting Dr. Szilard's specific contributions to the program of the Department of Biophysics to involve the following functions:

a) One of the main interests of this department lies in the general field of bacterial genetic processes and bacterial viruses. This area represents one of the foremost opportunities for application of molecular analysis to the problem of biological replication. The models currently developing from work in this areaare fundamental in their own right; in addition, they furnish a system which can guide genetic studies in mammalian cells, which now have become accessible to investigation by quantitative techniques like those hitherto confined only to microorganisms. The department has an excellent group of younger scientists at the assistant professorial level who are pursuing problems in bacterial genetics and bacteriophage. However,

with the turning of my own energies to problems of mammalian cell biology, there is need for mature advice from an older person actively interested in bacterial and virus replication. These younger men possess good training, drive, and real creative ability. They and their students would profit greatly from regular access to a person with the scientific maturity and intimate acquaintance with the field, which Dr. Szilard possesses, to criticize, stimulate, and assist in guiding their efforts. While we foresee that Dr. Szilard's contribution here will be mainly in the supply of guidance, criticisms, and suggestions, we hope it will be possible to provide facilities for him to undertake some experimental work himself in this area, if the occasion should arise.

b) Dr. Szilard's participation in departmental research seminars will be of great value in our training program. His ability to stimulate, challenge, and inspire graduate students is especially effective in a program like ours that is small and, to an appreciable extent, informal. His orientation toward science and its problems is one which coincides completely with the philosophy of this department; i.e., an emphasis on clearness of thinking and simplicity of operations, so as to achieve maximally definitive answers, with a minimum of manipulation and complex procedure. Both the faculty and the students can learn a great deal from Dr. Szilard, and we look forward to an arrangement that will make possible continuing opportunity for participation with him in discussions and seminars.

c) For some time it has been our hope to be able to add to the lines of this department's program a study in biological regulatory and integrative mechanisms. Such investigation would have to be of limited scope and involve new and simple approaches consistent with the scale of the department's

facilities. The time at which such an activity could start would be strongly dependent on progress made in the other programs, and on the availability of additional space and scientific personnel. Dr. Szilard has always had an absorbing interest in these problems, and has made critical conceptual contributions which have anticipated development of modern Information Theory. If our plans for this particular type of expansion reach fruition, Dr. Szilard would be of enormous assistance in the formulation of such an activity.

In addition to these specific activities in the Department of Biophysics, Dr. Szilard could contribute a great deal to many areas of this University. The faculties of the Physics Department, the High-Altitude Observatory, and many of the bio-medical departments will undoubtedly take advantage of the opportunity to consult with him regularly about their programs. His influence could have widespread effects throughout this scientific community.

Dr. Francis Manlove, Director of the Medical Center, approves of these arrangements, and is so indicating in a separate covering letter.

Sincerely,

Theodore T. Puck, Ph. D.
Professor and Head
Department of Biophysics

TTP:hw

cc: Dr. Leo Szilard

HENRY E. SIGERIST RESEARCH FUND

Department of the History of Medicine
Yale University School of Medicine
New Haven 11, Connecticut

Address correspondence to:
MILTON I. ROEMER, *Secretary-Treasurer*
2345 Rae Street
Regina, Saskatchewan, Canada

August 9, 1956

Dear Prof. Szilard –

We are writing you in behalf of Dr. Henry Sigerist - - as someone who undoubtedly feels that we owe a great debt to a great man for his leadership in medical history and social medicine.

You know that Dr. Sigerist has been in the little village of Pura, Switzerland, since 1947, writing his History of Medicine and working in other aspects of social medicine. Perhaps you also know that in October, 1954, he suffered a cerebral accident which left some residual paralysis. Nevertheless, with remarkable determination he has largely recovered and is continuing his important work.

The financial grants from foundations on which Dr. Sigerist has depended are now at an end. There is some other income, but in order for him to carry on his work, additional funds are needed. We feel certain that scores of people in America and elsewhere to whom Dr. Sigerist's intellectual leadership has long meant so much would want to help. We think you are one of these persons.

To simplify arrangements, Yale University has earmarked a fund in its Department of the History of Medicine (under Dr. John F. Fulton) for the support of Henry Sigerist. Several large donations have already been made, and we are asking friends of Henry Sigerist to contribute between $25 and $50. Smaller gifts will also help, of course.

Enclosed is an addressed envelope in which you may send a contribution. Please make cheques out to "Yale University". Such donations are tax-deductible. Also, if you would send along a few names and addresses of other friends of Dr. Sigerist, we shall write them if this has not been already done. If you have already contributed to the Sigerist Fund, kindly excuse this letter. With appreciation and every good wish.

Cordially yours,

FOR THE COMMITTEE

Robert L. Leslie

PF-56

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N.S.F.

II

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Cordially yours,

FOR THE COMMITTEE

Robert I. Leslie

PP-56

The Quadrangle Club
The University of Chicago
1155 East 57th Street
Chicago 37, Illinois

May 7, 1956

Dr. H. J. Muller
Department of Zoology
Indiana University
Bloomington, Indiana

Dear Dr. Muller:

You were kind enough to give some thought to my problem and incidentally I sought sound advice. I have pondered about what you suggested, but for the time being I think I will try to remain in biology.

A matter has now come up in which I believe I might need your help. The Biology Division of Cal Tech (Beadle), the Department of Biophysics of the Medical School of the University of Colorado (Puck), and the Department of Pharmacology of the Medical School of NYU, have been discussing with each other and with me the possibility of asking the National Science Foundation to set up a moving Research Professorship which would leave me free to pursue my scientific interests anywhere I desired and it would not be limited to these three institutions.

Apparently the officers of the National Science Foundation are interested in creating such a novel institution. The National Science Foundation would however not set up such a "fellowship" for more than five years.

The situation is complicated by the following calamity. If I stay at the University of Chicago and retire at the age of 65, about seven years from now, I would have a retirement income from Teachers Annuity of $113 per month. It is this low because my regular academic employment started in 1946.

Dr. Bernard D. Davis, head of the Department of Pharmacology at the NYU Medical School, and some of my other friends in New York, believe that it

might be possible to raise from foundations and private individuals funds to supplement what the National Science Foundation might be able to do, and create some sort of a "fellowship for life." They wish to enlist your help in this matter and you will receive a letter from Dr. Davis in the near future. He discussed with me what he proposes to do, and it sounds all right.

With kind regards.

Sincerely,

Leo Szilard

Dr. H. J. Muller
Department of Zoology (this is right)
Indiana University
Bloomington, Indiana

Dear Dr. Muller:

You were kind enough to give some thought to my problem, recently, and I am writing to you because it might be that you are in a position to help me find a solution.

(Teller letter)

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Department of Zoology
Indiana University
Bloomington, Indiana

LEO SZILARD

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The University of Chicago
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Chicago 37, Illinois

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DRAFT

May 28, 1956

To the National Science Foundation
Washington, D.C.

Attention: William V. Consolazio

I propose to serve as Senior Research Scientist At-Large for five years. I understand that under the terms of such a position I would be free to pursue my scientific interests anywhere in affiliation with universities or research institutes.

Initially, I propose to be affiliated with the California Institute of Technology, Pasadena, Calif; the Rockefeller Institute for Medical Research in New York; the Department of Pharmacology of the Medical College of New York University in New York; the Department of Biophysics, Medical School, University of Colorado, in Denver; and the Institute of Nuclear Studies at the University of Chicago, Chicago.

In general, my research will be in the field of quantitative biology, and more particularly in the problems of self reproduction, the role of RNA and DNA, protein synthesis, adaptive enzyme formations, problems of growth of micro-organisms and tissue cells in vitro, the growth of viruses in micro-organisms as well as in tissue cells in vitro, and the transformation as well as transduction of genetic characters in micro-organisms.

I expect to cooperate in these fields with R. Hotchkiss, M. Fox, N. Zinder and Paul Weiss in the Rockefeller Institute; with Bernard Davis and Werner Maas at New York University; with Leonard Lerman and T.T. Puck at the University of Colrado; Mr. Delbruck and R. Dulbecco at the California Institute of Technology; and with A. Novick, M. Weiner and H. Anker at the University of Chicago.

I propose the following budget per year:

Salary	$14,800
Three return trips from coast to coast	900
Travelling expenses to attend meetings	300
Per diem while away from my residence, 270 days at $12.00 per day	3,240
Cost of transportation while away from my residence, 180 days at $2.00 per day	360
Secretarial services while away from my residence, 100 hours at $2.00	200
Total	$19,800

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Secretarial services while away from my residence, 100 hours at $2.	200.
Total	$19,800.

CORRECTED

MEMO TO DR. G.W. BEADLE May 29, 1956

I just talked to Chicago to find out precisely what my status will be as of July 1st of this year. I shall be Professor of Biophysics in the Institute of Nuclear studies with a starting salary of $11,000. This salary is on 3/4 year basis which represents a change from the old 4-E type of contract to the new 3-Q type of contract. The new contract leaves me free to retain outside earnings and to take full employment for 1/4 of a year. In addition, the University of Chicago pays a amount equal to 5% of my salary into a retirement fund and this will be increased, I am told, very soon now to 7½%. Salaries will also be increased but probably rather slowly and we may assume that my average salary for the next five years would be about $12,000.

This is approximately what I have told you in New York, with one minor difference, but the present version is supposed to be the final one on the new contract.

CALIFORNIA INSTITUTE OF TECHNOLOGY INTER-OFFICE MEMO.

To Szilard From Bender Date

Subject Your letter

I have your letter and the copies I have from Congolazio. We are agreed here to go ahead and formulate a proposal and we understand why Chicago should not do it.

I think you should get all relevant material to me as soon as possible. I have letters from Davis, Hotchkiss and I believe Rusk

-over-

He'll be in Japan in Sept and would like to be finished with the proposal week before then.

Do you want to take up with Chicago the question of their participation and a possible leave-of-absence. Or do you want me to write to someone there. If the latter to whom should I write.

Regards

Bea

LEO SZILARD

1155 East 57th Street
Chicago 37, Ill.
May 31, 1956

Dr. Rollin Hotchkiss
Rockefeller Institute for Medical Research
York Ave. and 66th Street
New York, N.Y.

Dear Hotchkiss:

Enclosed is a letter which I sent to Beadle. Would you let me have your reaction to it and the budget proposal and perhaps drop Beadle a line also, in order to accelerate the proceedings?

This is a good occasion for me to thank you for your very kind help in this matter.

With kindest regards,

Sincerely,

Leo Szilard

LEO SZILARD

1155 East 57th Street
Chicago 37, Ill.
May 31, 1956

Dr. T.T. Puck
Department of Biophysics
Colorado General Hospital
Denver, Colo.

Dear Puck:

Things have been going rather well. Among other things there was a fortunate chance occurence involved. I went up to a meeting to Boston, and on my way back it so happened that Bronk Rabi and I took the same train. The conversation covered a great variety of topics and I was very favorably impressed with Bron's attitude towards many of the problems upon which the conversation touched. Soon thereafter things began to happen at the Rockefeller Institute and you may have seen Hotchkiss's letter to Beadle.

I have seen Mr. Consolazio of the Foundation in Washington, and I am sending him a copy of the enclosed material for his comments and suggestions. The probability is that he will want the proposal to remain as it stands. I would much appreciate your looking over the enclosed material and letting Dr. Beadle have any comment that you may have with a copy sent to my Chicago address.

Sincerely,

Leo Szilard

LEON SZILARD

1155 East 57th Street
Chicago 37, Ill.
May 31, 1956

Dr. W.G. Beadle
California Institute of Technology
Pasadena, Calif.

Dear Dr. Beadle:

Attached you will find corrected version of the little memo that was clipped to my letter. The correction is not an important one.

This is a good occasion to thank you for having spent so much time with me in New York on this matter and for having handled it so effectively.

I sent a copy of the letter which I sent to you, and the attached proposal also to Mr. Consolazio with the request of letting me have his comments on the details of the proposal. I shall let you know if I hear from him.

With best wishes,

Yours,

Leo Szilard

1155 East 57th Street
Chicago 38, Ill.
May 31, 1956

Mr. William V. Consolazio
National Science Foundation
Washington 25, D.C.

Dear Mr. Consolazio:

Enclosed is a copy of a letter which I wrote to Dr. Beadle. I would very much appreciate your criticism and any suggestion which you might care to make. Should anything be expanded? Should any item from the budget be cut or reduced? I might call you on Friday to get your comments on this. Otherwise a letter from you would reach me next week in Chicago.

I enjoyed the conversation in Washington very much, and might again come down for advice, just in order to get more of the same.

With best wishes,

Sincerely,

Leo Szilard

LEO SZILARD

1155 East 57th Street
Chicago, 37, Ill.
May 31, 1956

Dr. Bernard Davis
Department of Pharmacology
Medical College
New York University
31st Street and First Ave.
New York, N.Y.

Dear Davis:

Enclosed is information that I sent to Beadle. If you have any comments will you send them on to Beadle with a copy to me in order to accelerate the procedure?

Sincerely,

Leo Szilard

NATIONAL SCIENCE FOUNDATION
WASHINGTON 25, D. C.

June 18, 1956

Dr. Leo Szilard
1155 East 57th Street
Chicago 38, Illinois

Dear Doctor Szilard:

I have your draft on the proposal dated May 28, 1956, respecting the Senior Science Scholarship Award or Senior Research Scientist At-Large.

The question is not one as to whether I accept or object to the budget submitted for you know I am supposed to have no opinions until after the proposal has been reviewed. In spite of what I may have said when you and I talked about the matter several weeks ago in Washington, my own personal opinion at the present is that the sum total should not exceed $100,000. If an overhead is involved then this should be included in the total. I would suggest something like the following:

Budget For 5 Years

Salary	$75,000
Travel and Subsistence	$12,000
	$87,000
Overhead	13,000
	$100,000

Note that I have lumped travel and subsistence together and not explained it. The explanation I would put in the text, namely, that you expect to stay short or long periods in the various localities mentioned. As for the text, I would like to see a few words of explanation as to why you think it is to your benefit and to the benefit of science to undertake the task outlined in preference to the present arrangements you now have at the University of Chicago. What I am saying is that it would be to everyone's advantage if you said a few more words about the undertaking, so that those unfamiliar with your plans can have reasonable information to reach a justifiable decision.

With respect to Dr. Wilson's suggestion that this proposal be prepared in time for the December Board Meeting, considering all the time that has elapsed, one must admit that his estimates are better than mine. Thus, if you will prepare the following proposal ~~some~~ time so that we have it by the 15th of September, I am sure we will be able to have it evaluated and processed in time for the December meeting of the Board.

Sincerely yours,

William V. Consolazio
Program Director
For Molecular Biology

NATIONAL SCIENCE FOUNDATION
WASHINGTON 25, D. C.

June 19, 1956

Dr. Leo Szilard
1155 East 57th Street
Chicago, Illinois

Dear Doctor Szilard:

Now that I am back in my office and have had an opportunity to discuss this proposal on the Senior Science Scholarship with Dr. Wilson, I would like to make a few more remarks.

I suggested that it would be wise if you indicated the benefits that would result from such a program to you and to science. Dr. Wilson's suggestion goes even further in that he is of the opinion that it might be wise if the collaborators involved were to submit a sort of testimonial, at least a letter indicating what they foresee your responsibilities to be and the value of such an arrangement to their university. I think that Dr. Wilson has a point and one that I would like to recommend to you. The timing he suggested has further corroboration in that it is his intention to which I subscribe that this sort of activity will require Divisional Committee approval, since we are embarking in a new activity.

I will be interested in your comments.

Sincerely yours,

Bill

William V. Consolazio
Program Director
For Molecular Biology

St 3 2140
Ext. 3528

1155 East 57th Street
Chicago 37, Illinois
June 21, 1956

Dr. George W. Beadle
Division of Biology
California Institute of Technology
Pasadena, California

Dear Dr. Beadle:

I received two letters from Consolazio, of which I am enclosing copies. As you will see from these, the deadline for filing our proposal is no longer July 1st but September 15th. This will give me more time to discuss matters with the University of Chicago. I am nevertheless anxious to have a final draft proposal in your hands as soon as possible so that I can forget about it. We shall not be able to complete the proposal though since -- as you see from Consolazio's second letter -- supporting letters from Hotchkiss, Puck and Davis will be needed. These I propose to gather early in September and have them sent to you.

I talked to Consolazio today over the telephone to find out in greater detail something about the points which he raised in his letter. I find that the proposal which we may submit in September need not go to any panels but will go to the Division Committee. Consolazio said he would like you to explain why he does not think it advisable for them to make the contract with the University of Chicago.

Could you possibly let me know where you expect to be during the first two weeks of September, provided you know this?

With kind regards,

Sincerely,

Leo Szilard

m
Encl.

1155 East 57th Street
Chicago 37, Illinois
July 2, 1956

Dr. George W. Beadle
Department of Biology
California Institute of Technology
Pasadena, California

Dear Dr. Beadle:

I have both of your inter-office memos. Regarding the supporting letters from Davis, Hotchkiss and Puck, I could ask them early in September to write these and to send them directly to Consolazio with a copy to you. It may be possible to get these letters written earlier but Davis is in Woods Hole, while Hotchkiss is in Denver, and neither of them may have the stationery of their lab with them. Even if the supporting letters do not get written until September, we can still refer to them in our proposal, as long as we know that they will be forthcoming. This is clear at present for all of them except the University of Chicago where nothing can be settled before July 20th. Harry Kalven of the Law School of the University of Chicago had been discussing the issues involved with Warren C. Johnson, the Dean of the Physical Sciences Division, and the two of them will be talking to the Central Administration after July 20th. In the meantime there is no need for you to write to anyone in Chicago.

I shall prepare a draft proposal along the lines suggested in Consolazio's letter, and send the draft to you as well as to Consolazio for any further comments. You might want to bring this proposal into a form suitable for submission to the National Science Foundation through Cal Tech.

With kind regards,

Sincerely,

Leo Szilard

m

(Do not forward; hold for arrival)

Quadrangle Club
University of Chicago
Chicago 37, Illinois
July 2, 1956

Dr. Rollin D. Hotchkiss
Rockefeller Institute
66th Street and York Avenue
New York City, New York

Dear Hotchkiss:

I am advised by Consolazio that the deadline for the proposal that Cal Tech is supposed to submit to the National Science Foundation has been shifted to September 15th, and that the proposal will come before the board meeting in December.

According to Consolazio's attached letter (see underlined passage), it will be desirable for you to write another supporting letter which should be addressed to the National Science Foundation and which would be included with Beadle's proposal.

I realize that it might be inconvenient for you to write such a letter before Labor Day, particularly since you may not have your institute's stationery with you. If such is the case, I assume that you could send your letter in September directly to William V. Consolazio, National Science Foundation, Washington 25, D.C., and a copy of it to Beadle. Beadle himself will be in Japan in September, and would want to send Consolazio his proposal sometime in August.

I shall keep in touch with you about this matter and can remind you from time to time about it. However, if you do

not expect to be in New York soon after Labor Day, would you drop me a line now at my above address?

With kind regards,

Sincerely,

Leo Szilard

m
Encl.

Quadrangle Club
University of Chicago
Chicago 37, Illinois
July 3, 1956

Dr. T. T. Puck
Department of Biophysics
Colorado General Hospital
Denver, Colorado

Dear Puck:

I am advised by Consolazio that the deadline for the proposal that Cal Tech is supposed to submit to the National Science Foundation has been shifted to September 15th, and that the proposal will come before the board meeting in December.

Further, according to Consolazio's attached letter (see underlined passage), it will be desirable for you to write another supporting letter which should be addressed to the National Science Foundation and which would be included with Beadle's proposal.

Could you, therefore, write such a letter at your convenience and send it on to Beadle?

I expect to be in Denver once or twice between now and September and plan to attend your lectures on tissue culture in August. (I have joined the Institute for Nuclear Studies on the first of July and am now on a 3-Q contract.) I shall attend Cy Levinthal's symposium at Ann Arbor which starts on the 9th of July.

I hope all goes well with your experiments.

With kind regards,

Sincerely,

L. Szilard

m
Encl.

CALIFORNIA INSTITUTE OF TECHNOLOGY INTER-OFFICE MEMO.

TO L S FROM GWB DATE July 5

SUBJECT

I have your June 2 letter and will sit tight until having your draft of a proposal.

I agree the supporting letters can come later.

Regards
Beadle

NEW YORK UNIVERSITY-BELLEVUE MEDICAL CENTER
NEW YORK UNIVERSITY COLLEGE OF MEDICINE
550 FIRST AVENUE, NEW YORK 16, N. Y.

DEPARTMENT OF PHARMACOLOGY

July 5, 1956

OREGON 9-3200

Dr. Leo Szilard
Quadrangle Club
University of Chicago
Chicago 37, Illinois

Dear Szilard:

I just received your letter of July 2. I fortunately have on hand all the materials necessary for sending a neatly typed letter to the NSF, including even Miss Stern who happens to be here this weekend. I'm therefore sending a letter to the NSF today in support of Beadle's application.

I think you might enjoy a visit to Woods Hole. The weather has been cool, and there are biologists here in great variety and number. I hope we will see you here; if not, I look forward to seeing you in New York in September.

Sincerely,

Bernie

Bernard D. Davis

July 10, 1956

Dr. G. W. Beadle
Biology Division
California Institute of Technology
Pasadena, California

Dear Dr. Beadle:

This letter for transmittal to the National Science Foundation indicates our expectations with respect to Dr. Leo Szilard's contributions and responsibilities to this department, under the proposed arrangement for his appointment as a Scholar and Visiting Professor.

Dr. Leo Szilard Possesses one of the most versatile and keenly analytic minds of our generation. His ability logically to dissect problems and to consider new and unconventional approaches to their solution is a scientific asset of enormous value.

We are expecting Dr. Szilard's specific contributions to the program of the Department of Biophysics to involve the following functions:

a) One of the main interests of this department lies in the general field of bacterial genetic processes and bacterial viruses. This area represents one of the foremost opportunities for application of molecular analysis to the problem of biological replication. The models currently developing from work in this area are important in their own right; in addition they furnish a system which can guide genetic studies in mammalian cells, which now have become accessible to investigation by quantitative techniques like those hitherto confined only to microorganisms.

Such studies with mammalian cells are now in progress in this department. Their ~~mutation~~ initiation has somewhat weakened the force of its investigational program with bacteria and bacteriophage. The department has an excellent group of younger scientists at the assistant professorial level who are pursuing problems in bacterial genetics and bacteriophage. These younger men possess good training, drive, and real creative ability. They and their students would profit greatly from regular access to a person with scientific maturity and the intimate aquaintance with the field, which Dr. Szilard possesses, to criticize, stimulate, and assist in guiding their efforts. While we foresee that Dr. Szilard's contribution here will be mainly in the supply of guidance, criticisms, and suggestions, we hope it will be possible to provide facilities for him to undertake some experimental work himself in this area, if the occasion should arise.

b) Dr. Szilard's participation in departmental research seminars will be of great value in our training program. His ability to stimulate, challenge, and inspire graduate students is especially effective in a program like ours that is small and to an appreciable extent informal. His orientation toward science and its problems is one which coincides completely with the philosophy of this department; i.e., an emphasis on clearness of thinking and simplicity of operations, so as to achieve maximally definitive answers, with a minimum of manipulation and complex procedure. Both the faculty and the students can learn a great deal from Dr. Szilard, and we look forward to an arrangement that will make possible a continuing opportunity for participation with him in discussions and seminars.

c) For some time it has been our hope to be able to add to the lines of this department's program a study in biological regulatory and integrative mechanisms. Such investigation would have to be of limited scope and involve new and simple approaches consistent with the scale of the department's facilities. The time at which such an activity could start would be strongly dependent on progress made in the other programs, and on the availability of additional space and scientific personnel. Dr. Szilard has always had an absorbing interest in these problems, and has made critical conceptual contributions which have anticipated development of modern Information Theory. If our plans for this particular type of expansion reach fruition, Dr. Szilard would be of enormous assistance in the formulation of such an activity.

In addition to these specific activities in the Department of Biophysics, Dr. Szilard could contribute a great deal to many areas of this University. The faculties of the Physics Department, the High-Altitude Observatory, and many of the bio-medical departments will undoubtedly take advantage of the opportunity to consult with him regularly about their programs. We expect his influence to have widespread effects throughout this scientific community.

With regards,

Sincerely,

Theodore T. Puck, Ph. D.
Professor and Head
Department of Biophysics

TTP:st
cc: Dr. Leo Szilard

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With regards,

Sincerely,

Theodore T. Puck, Ph. D.
Professor and Head
Department of Biophysics

TTP:st
cc: Dr. Leo Szilard

NATIONAL SCIENCE FOUNDATION

WASHINGTON 25, D. C.

July 11, 1956

Dr. Leo Szilard
The Quadrangle Club
The University of Chicago
Chicago 37, Illinois

Dear Doctor Szilard:

With respect to the draft of the proposal submitted on July 5, 1956, let me state at the very onset that it reads well. I have only one suggestion and that is that you attach a curriculum vitae and bibliography.

With respect to the budget I would agree with you that the overhead is quite out of order and, personally, I would like to see some adjustments made, especially since Cal Tech in this case will profit by the arrangement without having to assume any responsibilities, except to handle your vouchers. However, this is a matter beyond my control and one that you might consider raising with Cal Tech.

I would like, however, to raise a more delicate question and that is the issue of the travel and subsistence item of $14,000. Personally, I am of the opinion that scientists as a group are not as well paid as they might be and should not be asked to make any more sacrifices than any other professional group. However, to the uninitiated and especially to scientists an item of such a size might prejudice the proposal. My past experiences tell me that your fellow peers might react negatively to the program if such an item of the size under discussion should appear. It has been a Federal practice that an individual in Federal employ is expected to assume some sort of permanent residency after he has been away from his home-base for a period of time. If this were the case in this particular situation, an adjustment would have to be made with respect to travel and subsistence.

If it is a matter of making both ends meet, you should seriously consider adjusting the salary item upward. At any rate I would suggest that you try the present budget item out on your colleagues for size and see how they react to such an item as the one presented.

Sincerely yours,

Bill

William V. Consolazio
Program Director
For Molecular Biology

The Quadrangle Club
The University of Chicago
Chicago 37, Illinois
July 26, 1956

Draft
Memorandum

To The
National Science Foundation
Washington 25, D.C.

At present I am a member (Professor of Biophysics) at the Enrico Fermi Institute for Nuclear Studies at the University of Chicago. This position, while it may be regarded as very desirable in many respects, does not represent the proper setting for the pursuit of my biological interests.

I take the liberty to submit herewith a memorandum in which I propose to serve as a Senior Research Scientist-At-Large either for five or for ten years.

I understand that under the terms of such a position I would be free to pursue my scientific interests anywhere in affiliation with universities or research institutes. Initially, however, I would be affiliated with the California Institute of Technology, Pasadena, California; the Rockefeller Institute for Medical Research in New York; the Department of Pharmacology of the Medical College of New York University in New York; the Department of Biophysics, Medical School, University of Colorado, in Denver; and the Enrico Fermi Institute for Nuclear Studies at the University of Chicago, Chicago.

In general my research will be in the field of quantitative biology. More particularly, I expect to be concerned with the formation of adaptive enzymes in microorganisms, and the formation of anti-

bodies in spleen cells *in vitro*, problems connected with the growth of microorganisms and tissue cells *in vitro*, the growth of viruses in microorganisms, as well as in tissue cells *in vitro*, and the transformation as well as the transduction of genetic characters in microorganisms.

I would expect initially to cooperate in the Rockefeller Institute with R. Hotchkiss, M. Fox, N. Zinder and Paul Weiss; at New York University with Bernard Davis and Werner Maas; at the University of Colorado with Leonard Lerman and T. T. Puck; at the California Institute of Technology with Max Delbruck and R. Dulbecco; and at the University of Chicago with A. Novick and H. Anker.

In the following I wish to present two points of view that might be relevant in judging the usefulness of my serving as a Senior Research Scientist-At-Large.

(a) At present certain branches of biology in which I am interested are in rapid progress. The problems of protein synthesis, the role of RNA and DNA, and the general problems of self-reproduction, differentiation and aging are rapidly becoming open to attack by means of new techniques. In part, this is due to progress in the field of microbiology and, in part, it is due to progress in the techniques of animal cell cultures, as well as other fields. This appears to be a situation where it would be of great advantage for anyone interested in general biological laws to be able to shift from one biological material to another, as the problem demands, and to be free from the limitations of using just the few techniques which any one person can master. This ideal is not fully attainable for anyone short of being made head of a research laboratory of some size created *de novo*, and thus being enabled

to assemble, so to speak, from scratch a sufficiently large and varied team, and at the same time being given an administrative setup which leaves the head of the laboratory free of administrative duties in order to enable him to effectively work with such a team. However, as a Senior Research Scientist-At-Large, it might be possible to approximate somewhat this ideal situation (which is probably not fully attainable in the United States). As a Senior Research Scientist-At-Large, it should be possible for me to establish collaboration with a sufficiently large and varied group of scientists scattered across the country in different laboratories, and at the same time I would remain free of administrative duties.

(b) As a Senior Scientist-At-Large, it should be possible for me to acquire intimate knowledge of experiments conducted with a great variety of biological material and diverse techniques, and thereby to be in a position to try to function as a "theoretical biologist." This statement requires a qualification:

Biology has not quite reached the stage which was attained by physics half a century ago when enough facts were established to permit a theoretical physicist to come up with significant insights on the basis of the established facts. Yet in biology we might be very well on the verge of a similar situation, and a few scientists who are so inclined may attempt to act, for a period of time at least, as theoretical biologists. This means that it might be well at present for a few scientists to put less emphasis on their own experiments and spend more time trying to keep in close touch with the experiments of others in the hope of being able to recognize new patterns and to gain insight into some general biological laws that have so far not clearly emerged. It may be that the

main difference between 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 feel the need to cut down the number of possible avenues along which his further thinking may be tempted to wander.

With respect to the issue of whether the fellowship should be given for five or for ten years, I wish to present the following point of view:

If the fellowship is given for ten years, it would take me to the age of 68 years, which is the present retirement age at most universities. On the other hand, if the fellowship is given for five years, I would have to contemplate returning to the University of Chicago when the fellowship lapses. While I understand that the University of Chicago would be agreeable to my returning to it at any time I desire to do so, prior to reaching retirement age, yet should I return to Chicago after a lapse of five years there would arise the question of how to spend the remaining time in a productive manner.

L. Szilard

BUDGET

On a one-year basis:	
Salary	$14,800.
Travel and Subsistence*	2,500.
Contribution of University to retirement fund, T.I.A.A., in the amount of 7-1/2% of salary	1,110.
General overhead in the amount of 7-1/2% of salary	1,110.
	$19,520.
On a five-year basis, this would amount to a total of	$97,600.
On a ten-year basis, this would amount to a total of	$195,200.

*For explanation of travel and subsistence item, see attached sheet.

Estimate of Expenses for Travel and Subsistence

I estimate transporation expenses on the basis of three return trips per year from coast to coast to be $900.

To this must be added travel expenses to attend meetings which I estimate to be . . . . . $300.

This gives for transporation per year a total of . . . $1200.

The amount of subsistence while travelling, I estimate as follows: I may be away most of the year from my home base (Chicago) where I pay rent all year around. Unless I stay in any one place long enough to make other arrangements, I might have to stay at hotels. If, instead of eventually concentrating my activities either mainly on the East Coast or the West Coast, I should keep on commuting between the two coasts, and particularly if my activities should gradually extend not only to New York and Pasadena but also to Boston and Washington, as well as Berkeley and Palo Alto, then my expenses for hotel accommodations and meals at hotels might amount (on the basis of $12/day for 270 days) to . . . . . $3,240.

Transporation in cities away from my home base (on the basis of $2. per day for 180 days) might amount to $360.

Secretarial services, while away from my home base, calculated on the basis of $2. per hour for 100 hours per year, might amount to . . . . . $200.

Thus, subsistence while travelling might amount per year to a total of . . . . . $3,800.

This means a grand total for travel and subsistence per year of $5,000.

On the assumption that it should be possible to economize by travelling on non-scheduled airlines, by staying with friends rather than staying at hotels, and by gradually concentrating my activities so that fewercities need to be covered, it is conceivable that the expenses may be cut down to half of this total. Therefore, in preparing a budget, I have assumed a yearly expenditure for travel and subsistence of $2,500.

Leo Szilard

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 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 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 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. 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 ultraviolet light can be reactivated by shining visible light at them. (B1) 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 ultra-violet 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 (2) 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 will fully counteract the effect of purine-type mutagens.

In a bacterial population maintained in the "Chemostat" there occur evolutionary changes (3) 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.

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.

PARTIAL BIBLIOGRAPHY OF DR. LEO SZILARD*

A. Physics

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

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

(4) Zeitschrift fur 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 Habilitationschrift 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.

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*Some of Dr. Szilard's most important works still remain unpublishable, for reasons of national security.

(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.

(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.

of Leo Szilard,

Publications from 1948 to 1955

B. Biology

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. Dynamisc of Growth Processes. Princeton University Press, pp. 21-32, 1954.

8) 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.

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 exponentially 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 gorwing 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 groupof compounds described in these papers, all of them ribosides of purines which in small quantities will completely counter-

act 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 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} 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 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 thefinal product at one of the <u>early</u> steps of the metabolic pathway leading to Tryptophane.

In paper #8, 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.

The Quadrangle Club
The University of Chicago
Chicago 37, Illinois
July 27, 1956

Dr. Warren C. Johnson
Dean, Division of Physical Sciences
University of Chicago
Eckhart 111
Chicago 37, Illinois

Dear Dr. Johnson:

I understand from Harry Kalven that he had a detailed discussion with Dr. Harrison and that the way is clear for us to go ahead with filing an application with the National Science Foundation. Kalven told me that he will send you a memorandum of his conversation.

I am now sending to George W. Beadle at the California Institute of Technology the enclosed draft. The application to the National Science Foundation will be made by the California Institute of Technology which will function as the contracting agency.

I understand that it would be desirable for them to receive supporting letters from those institutions which co-sponsor the application to indicate what benefits the co-sponsoring institution expects to derive from the proposed arrangement. I would, therefore, appreciate your writing such a supporting letter to Dr. Beadle. If you are in doubt what to say, this matter can be postponed until the end of August when I expect to see you and when we can discuss the details. In that case, the supporting letter will have to be sent directly to the National Science Foundation, Attention: Mr. William V. Consolazio; Washington 25, D.C., since Beadle will have gone to Japan.

Many thanks and kind regards,

Sincerely yours,

Leo Szilard

m
Encl.

CALIFORNIA INSTITUTE OF TECHNOLOGY

Division of Biology

August 24, 1956

TO: Bernie Davis
Rollin Hotchkiss
Ted Puck
Leo Szilard ✓

The suggestion from NSF now is that the Szilard proposal be made as a straight request for research support in theoretical biology. This will avoid certain serious difficulties.

The title could be "Research in Theoretical Biology." I'm not sure this is the best that can be done so make any suggestions you like.

For Roving Professor we can substitute research, research support or some other appropriate expression.

Omit last paragraph of page 1.

Omit page 4 and explain travel allowance in footnote -- very briefly.

Pages 10-11 and 14-15 duplicate somewhat and this will be fixed.

Omit next to last paragraph in my letter and change last paragraph to "will be able to make the requested grant.

Page 19. This letter will be replaced.

Page 21. Change G. W. Beadle to California Institute of Technology; change Roving Professorship etc. to research support. Suggest next to last paragraph of Davis letter be omitted or, perhaps better, modified.

Will you let me know as soon as possible what your reaction is to the proposal as a whole and also suggested modifications. I get off August 31 for Japan. Also please tell me to whom final proposal should be sent for signature if you have not already done so.

Regards,

G. W. Beadle

L.S. Is "Research in Theoretical and Quantitative Biology" a better title?

The Quadrangle Club
The University of Chicago
Chicago 37, Illinois
July 27, 1956

Dr. George W. Beadle
Division of Biology
California Institute of Technology
Pasadena, California

Dear Dr. Beadle:

I am sorry that things in Chicago have been moving so slowly due to vacation absences. Warren C. Johnson, Dean of the Physical Sciences Division, who is in the direct chain of command, is away at present, but before he left he had a brief conversation with R. Wendell Harrison, Vice-President of the University. Harry Kalven of the Law School saw Johnson before he left and subsequently he discussed all issues with Harrison in detail. (Kalven will send you a memorandum of his conversation for your information.)

I understand from Kalven that we can now go ahead and file an application with the National Science Foundation. The University wishes to cooperate in every possible way, and accordingly I shall suggest to Warren Johnson that he send you, upon his return, a letter that will establish that the University of Chicago is a co-sponsor of the application along with the other co-sponsoring institutions. If Johnson's letter does not get written in time, he may send it directly to the National Science Foundation.

We might avoid raising the issue of a leave-of-absence altogether because the University of Chicago is willing to assign me to the new "job". If this should prove unworkable, however, they could give me a leave-of-absence of indefinite duration.

I understand also that, as a courtesy, the University of Chicago might take on the job of handling the accounts. Since Consolazio tells me that the National Science Foundation would have no objection to the California Institute of Technology giving the University of Chicago the sub-contract, we might later on decide to let Chicago handle the vouchers. However, we can cross this bridge when we get to it; i.e. sometime in December or January.

Enclosed is a draft with proposed budget attached representing my contribution to the application that is to be filed with the National Science Foundation. If you think that some passages had better be changed, please do not hesitate to mark them up and return to me one of the two copies that you will find enclosed. I plan to call you over the telephone early next week to find out if everything is now under control.

Enclosed are also two copies of a curriculum vitae including a list of publications. Consolazio asked that such a document be submitted together with the application.

If there is anything else that needs to be done, you can let me know when I call you.

With kind regards,

Sincerely,

Leo Szilard

m
Encl.

CALIFORNIA INSTITUTE OF TECHNOLOGY INTER-OFFICE MEMO.

To Szilard From Beadle Date

Subject NSF Proposals

Give put into standard form the Szilard proposal. It will be dictated soon - in tentative form - to be typed out on participating institutions + key persons. It should come to you later this week.

Regards

B