THE COLLECTED WORKS OF LEO SZILARD

VOLUME I - SCIENCE

Edited by Bernard T. Feld and Gertrud Weiss Szilard
with the assistance of Kathleen R. Winsor

Foreword by Jacques Monod
Preface and Acknowledgements

Introductory Essays by Carl H. Eckart
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T A B L E   O F   C O N T E N T S

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16 July 1969

Dr. Gertrud Szilard
2380 Torrey Pines Road
La Jolla, California

Dear Trude:

We're in business! The Collected Scientific Works are scheduled to appear late this winter, certainly in time to be launched (in some ceremonial way) by the Press at the Washington meeting of the APS and National Academy in April. We still need to take care of the items circled in the Table (enclosed), by September if possible.

What did you have in mind for the illustrations? Can you send me or Curly a few of your favorite photos of Leo, to be used in the book and on the jacket? A few typical (or atypical) notebook pages would be welcome. Can you have some photos made and send them along?

Now to get at the next volume.

Love,

Bernie

B. T. Feld

Cambridge Draft
March 1971

Archery

TABLE OF CONTENTS

incomplete

Contents

Vol I

1) Preface
2) Physics
Introd.
a) 15 Physics Paper
3) b) Project
Introd. by Field
Selected Proj. reports
3) Biology
Introd by Round
Biology paper
4) Patents
Introd. by Tobin
Patents

The language of space

from Angela Croome

Lt.-Colonel Alexeit Leonov, commander designate of the Soyuz crew for next summer's Apollo-Soyuz linkup (the ASTP) announced a nice compromise on the question of space communications during the joint presentation on ASTP progress at the Amsterdam International Astronautical Congress. Last year it was not known what the language of space would be, said Leonov; it had now been decided that on the mission the Russians would speak English and the Americans Russian. In an emergency each team would revert to its own language. It had not however been fixed who would take overall command in a crisis.

The 'book' of operational procedures had been written and practised in the four joint training sessions. Emergency procedures had yet to be worked out and there was further polishing of the language to be done; this had proved much more of a problem than originally envisaged (though judging from Leonov's competent performance, his English was no longer a headache). Still to be fully sorted out was the ticklish question of the interrelation of the two control centres. There seemed no clear indication that an American team of controllers would be sitting in at Baikonur next July.

Both the Russians and the Americans assured questioners that there was nothing to choose between the two sides in terms of input to the engineering and other design and operational concepts. Nonetheless behind the scenes American space planners were saying the flow of information was all one way and in the space medicine field in particular there was a great scarcity of adequate Soviet data. How had the Soviet programme eradicated early problems with motion sickness? Skylab astronauts, groggy and sick during their first week in the orbital workshop, would dearly like to know.

Medallurgy

We are agog to see whether our hint (*Nature*, **246**, 113; 1973) that the Royal Society's policy for the distribution of medals was just a trifle inward-looking has fallen on fertile ground. It's medals-time again next week and several Fellows have told us they will try and persuade Council to look further afield this year.

correspondence

Radioactivity salted away

SIR—Referring to the article "Fruits of a Faustian bargain" (*Nature*, **251**, 274; 1974) it appears that the United States propose tests in 1980 for the storing of radioactive waste so as to stop dumping it—as do the UK, USSR and other nations—in the ocean. The German authorities seem to be ahead of them, as for several years now, low and moderately radioactive waste has been stored in a salt mine close to Hannover by the Gesellschaft für Strahlen-und Umweltforschung owned by the Federal German Republic. And there is still space for thousands of barrels for decades to come. It may be taken for granted that no earthquake will occur to wreak havoc—for millions of years none has happened there, so the only thing to fear is mankind itself by trying to prove its efficiency by discovering a new and final way of self destruction.

HANS K. KOENBER

Laser & Electro-optik,
D-8000 München

Plasmid moratorium

SIR—The appeal by a committee of eminent biomedical scientists for a voluntary moratorium on an area of scientific research which may create unpredictable hazards to human health (*Nature*, **250**, 175; 1974) reminded me of a talk which Leo Szilard gave at a writers' club in Moscow in December 1960 which I attended, and where to my knowledge the question of a moratorium was raised for the first time. This story was later told by Szilard and published[1].

"A year ago last December I was in Moscow to attend the sixth Pugwash conference. And while I was there I was invited to talk to a writers' club about molecular biology, about my work. One of these writers said, 'Now what practical consequences does this have?' So I said, 'As far as I can see it has no practical utility whatever—but of course if you had asked me that about nuclear physics in the 1930s I would have told you the same thing.' And then the Russian said, 'Well in that case, wouldn't it be better if you stopped right now?'".

Yours faithfully,

GERTRUD WEISS SZILARD

La Jolla,
California 92037

[1] *Thinking Ahead with Leo Szilard*, in *Int. Sci. Tech.*, 33–38 (1962).

Sitting Bull?

SIR—I was startled to read (*Nature*, **251**, xviii; 1974) that Colorado State University is encouraging 'ethnic minorities and women" to apply for the position of Chairperson in its Statistics Department. I reject as highly improbable the implicit assumption that ethnic minorities necessarily consist entirely of males. Nevertheless, I wonder whether the aim is to encourage collective applications resulting in large-scale migrations of American Indians (that is deviations from their normal distribution). Does this mean that sophisticated statistical tests (excluding Student's *t*, of course) will then be used to select individual chairpeople from large samples? And, even if this analysis is at variance with the real object of the exercise, won't the unsuccessful applicants sioux?

Yours faithfully,

F. A. SMITH

Department of Botany,
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Adelaide, South Australia 5001

ILLL reactor?

SIR,—Although apposite and allusive alliterations are always amusing and appropriate as acronyms, Institut Laue-Langevin still satisfies many of us. It recalls that the construction of this long awaited reactor is owed to a Franco-German agreement, whilst with 1,000 visitors and 300 completed experiments in the last year, the Institut is already known as ILL the world over.

I believe we should 'let ILL alone'.

Yours faithfully,

W. M. LOMER

Institut Laue-Langevin,
Grenoble, France

Erratum

SIR In my news note (*Nature*, August 9) credit for the fallopian tube transplant experiments in animals should have gone to Dr B. M. Cohen of the University of Cape Town Medical School, not to his close colleague Dr M. Katz, who has drawn my attention to the error. I apologise to both gentlemen.

IAN RIDPATH

35 Oakwood Gardens,
Ilford, Essex

LEO SZILARD

Documents relating to the Manhattan Project

1939 - 1946

Introductory Essay by Bernard T. Feld

April 24, 1968 26-12-67 173

chap title

~~Introduction to the~~ ~~Collected Works of Leo Szilard~~ Documents relating to the Manhattan Project

Bernard T. Feld

From M.I.T. Rec. Feb 1971

no. 1

1. I. Preparation

The genius of Leo Szilard had many facets: a power of foresight, bordering on the visionary, which that enabled him to comprehend the long-range consequences of a scientific discovery or of a social situation long before they were imagined by even the most astute; an ingenuity of approach and execution ~~which~~ that led him to discover the most unexpected, but ~~yet~~ direct, solutions to difficult problems; a tenacity ~~which~~ that prevented him from abandoning difficult problems, but forced him to return and return again, devising new approaches at each attempt, and yet permitting him to leave the details of the solution in other, competent hands once the appropriate approach had been found and the path charted; and an overriding sense of responsibility, ~~towards~~ toward his work, ~~towards~~ toward science, but most of all ~~towards~~ toward his fellow men.

All these combined to make Leo Szilard an indispensable factor in the successful achievement of the first man-made nuclear chain reaction and in the vast wartime enterprise known as the Manhattan Project, which culminated in the first man-made nuclear explosion.

It was no accident that Leo Szilard was on the spot, at Columbia University, in January 1939, ready to plunge into the experimental investigations of its ramifications immediately after the announcement of the discovery of nuclear fission. He had sensed, with that uncanny instinct which never deserted him, that this was where "the action" would be; and he had been scheming and dreaming about the controlled release of nuclear energy ever since a remark by Lord Rutherford in the fall of 1933 -- ~~to the effect that talk of the large-scale liberation of atomic energy is "moonshine"~~ that "anyone who expects a source of power from transformation of these atoms is talking moonshine" -- had intrigued and goaded him into taking up the study of nuclear physics. Charac-

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that "anyone who expects a source of power from transformation of these atoms is talking moonshine" teristically, he immediately realized that, of all the available nuclear particles, it was the neutron which held the key; ~~through its~~ the neutron had the ability to penetrate matter without the energy-consuming electromagnetic interactions which prevented all but a minute fraction of the other, charged nuclear projectiles from inducing nuclear reactions. The problem was to discover a neutron-induced reaction in which at least one neutron is emitted for each neutron absorbed.

~~And~~ While Szilard was so, a refugee in England (1933-1938), with no academic base and practically no resources, he embarked on a series of fundamental researches ~~which~~ that, even though they did not succeed in uncovering the type of reaction he had envisaged, were to give him that profound understanding of nuclear phenomena which was indispensable to his exploitation of the fission reaction when it was finally discovered.

The scientific aspects of Szilard's work in England, prior to his emigration to New York at the beginning of 1938, are described in Part I of ~~elsewhere in~~ this volume. It is sufficient to observe that if his late entry into the field of nuclear physics did not permit him to participate in its most fundamental discoveries, it did not take him long to overcome the handicaps of a newcomer; ~~so that~~ by 1938 Szilard could be counted among the elite of the small international community of nuclear physicists. Still, he was always profoundly aware of his place in the historical scheme, with no illusions and ~~that~~ a delicate balance of judgment with which ~~Szilard~~ he was always able to view history. As usual, this judgment is best described in his own words, and it is ~~appropriate to~~ reproduce ~~here~~ at the end of this essay "Creative Intelligence and Society"[5] an address delivered by Szilard at the University of Chicago in 1946. There is, to my knowledge, no more succinct or penetrating summary of the history of nuclear physics from its inception until the first chain reaction, nor of his own role in this historical chain.

2. II. Genesis

Although much has been written about the genesis of the wartime atomic energy project, its prehistory -- the period between the arrival of Bohr in the United States in January 1939, with fresh knowledge of the discovery of fission, and the setting up of the official Manhattan project in 1941 -- remains somewhat obscure and confused. In part, this is a natural consequence of the fact that in science, when the time is ripe, similar ideas occur simultaneously at many sources and the rapid interchange of ideas and information serves to confuse their origins, even in the minds of the most important contributors. Furthermore, most scientists are too concerned with their current research to take the time to unravel and record their recollections of the past.

Fortunately, Szilard has left us with a clear record of the steps leading to the conception of the chain-reacting system utilizing a lattice of natural uranium embedded in a matrix of graphite. These steps are outlined in a letter written to E. P. Wigner in 1956, which is reproduced at the end of this essay, together with the documents to which it refers.

Having established, in an experiment with Walter H. Zinn, the emission of neutrons in the fission process[1], the problem was how to achieve a self-sustaining system -- one in which the neutrons emitted in a fission would be capable of inducing at least one more fission reaction. There are two requirements for such a system: first, the fast neutrons emitted in the fission process must be slowed down to thermal energies, at which the probability of producing a fission in uranium is much increased; second, it is necessary to avoid the parasitic absorption of these neutrons, both by

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number footnotes

[1] The same fact was simultaneously and independently ascertained by Anderson, Hanstein, and Fermi, also at Columbia University [Phys. Rev., 55, 797 (1939)] and, slightly before, by Halban, Joliot, and Kowarski in Paris [Nature, 143, 479 (1939)].

uranium during the slowing-down process and by the light elements ~~which~~ that are required to reduce rapidly the neutrons to thermal energies before they undergo such absorption or escape from a necessarily finite system.

After careful consideration of the possibility of utilizing mixtures of uranium and water for this purpose [Anderson, Fermi, and Szilard, Phys. Rev. 56, 284 (1939); also Halban, Joliot, Kowarski, and Perrin, ~~Journ. de Phys.~~ J. Phys. Radium 10, 428 (1939)], Fermi and Szilard eliminated water from consideration as the slowing-down matrix owing to the strong neutron absorption properties of hydrogen. (In Szilard's case, at least, consideration of the engineering difficulties involved in extracting large amounts of power from a water-uranium system were also involved; he had, from the start, in mind the necessity of producing large amounts of plutonium if the chain reaction was to be of any wartime importance.) Heavy water was also soon discarded owing to the unavailability of large quantities. Carbon, in the form of graphite, was the next choice, despite its considerably poorer energy-moderating properties than the hydrogens, both because of its availability in large quantities and of its small neutron absorption cross section (although it was not known for some time just how small the cross section is; as it later turned out, the upper limit known at that time could be accounted for entirely on the basis of impurities).

The problem of avoiding the parasitic (resonance) absorption of neutrons by uranium during the slowing-down process was more difficult. Szilard, and also Fermi,[*] [12] hit upon the solution of separating the uranium from the graphite, so that the moderating process would take place in the absence of uranium. It remained to work out the necessary proportions of the constituents and their optimum geometrical distribution, on the basis of neutron reaction properties very incompletely known

[*] [12] Although Fermi was apparently thinking in terms of alternate layers of uranium and graphite, rather than the lattice structure devised by Szilard. ~~refer to Lette,~~ (See letter from Fermi to Szilard, July 9, 1939, at end of this essay.)

and in the absence of a satisfactory theoretical framework for treating such heterogeneous mixtures.

By the end of 1939, Szilard had worked out a rough theory of the nuclear chain reaction in a graphite-uranium lattice, which served to convince him of its feasibility. This he submitted first as a letter and then as a paper to the Physical Review in February, 1940. However, being by then convinced of the favorable prospects for its success, and concerned that its publication would aid Germany in its prior achievement, its publication was indefinitely deferred at his request. Early in 1946, the editors of the Physical Review and Szilard agreed that the original version should be published with some footnotes and a "note added May 1946 " for the purpose of clarifying some aspects of the paper in the light of new information. However, the authorities of the Manhattan Project at first refused and then delayed its classification until December 1946. For reasons unknown, it was never published. This classic paper is here published for the first time.[3]

In acknowledgement of their invention, Fermi and Szilard were jointly awarded a patent on the nuclear reactor in 1955 by the United States Government.

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3 This paper received only very limited circulation in the Manhattan Project; its existence was quite widely known, as A-55, but under the confusing and wholly misguided system of compartmentalization it was not possible, even for members of the project, to obtain most reports without establishing a "need to know" their contents. Early (pre-Chicago project) reports on the chain reaction were classified under the heading A; later, a more elaborate system was established: C for Chicago, CP for physics; CE for engineering; CF for fast neutron work, etc. But the system served more to confuse than to clarify.

no 1 B.III. Attainment:

With the broad outlines of the solution generally agreed upon, Szilard was content to leave to Fermi the main responsibility for the experiments ~~which~~ that were to prove the feasibility of the graphite-uranium chain reaction and explore its properties. Although he was deeply involved in the conception of the "exponential" experiment for measuring the neutron absorption properties of graphite and for testing the multiplicative properties of sub-critical uranium-graphite systems, and he continued to take an active interest in the details of the experiments, Szilard's main efforts, from 1940 on, were directed towards planning for the earliest possible use of the reactor for the large-scale production of plutonium.

~~That, in fact,~~ The fact that enough plutonium was produced to be used militarily before the end of the war (albeit just barely), is mainly owed to the acute and incisive foresight exhibited by Szilard in the earliest stages of the project.

This may be the appropriate point for some remarks on the question of indispensability: in the history of science very few men have been indispensable; particularly in applied science, and the achievement of the nuclear chain reaction definitely falls into this category, no man is indispensable. Many men in many countries had the same or similar ideas, and some advanced them relatively far under much more adverse circumstances.[3] The remarkable exploit involved in the plutonium project was the short time span between the discovery of fission at the end of 1938 and the production by means of a high-power-producing nuclear reactor of kilogram quantities of an entirely new element, plutonium, by 1944 -- all this requiring the invention and reduction to operation of the nuclear reactor and the development of a completely new branch of engineering in a field where many of the scientific fundamentals were ~~still~~ either not

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[3] See the recent revelations on the exploits of Kurchotov and co-workers in the USSR, ~~B.A.S.~~ Bull. At. Scientists, December 1967.

yet known or imperfectly understood.

The scientists of the Manhattan project thought they were involved in a race against the Germans; they were certainly racing against time. With hindsight, one may denigrate the importance of their accomplishment or regret, as Szilard was afterwards frequently to regret, that they succeeded so well; but retrospect cannot dim the lustre of their achievement. ~~And~~ in this accomplishment no man was more important ~~indispensible~~ than Leo Szilard, and only a few -- notably Enrico Fermi and Eugene P. Wigner for the plutonium-production aspect of the project -- can be placed at the same level.

As noted, Szilard's major contributions came in the earliest stages, in 1940-1941, ~~mostly~~ beginning ~~commencing~~ even before the Columbia group moved to Chicago and was incorporated into the many-faceted Manhattan Project. It was he who first recognized the importance of securing copious quantities of pure graphite, and who initiated the steps necessary to assure this. More important, realizing at a very early stage the necessity of securing pure uranium in its high-density metallic form -- uranium metal had not previously been industrially produced, and its metallurgy was only vaguely comprehended -- Szilard initiated and personally supervised a program aimed at the industrial production of uranium metal of high purity. A brief report (A-24), reproduced later in this section ~~below~~, shows how important was his contribution to the success of this aspect of the project.

Szilard's first act on moving to Chicago was to set up a Technological Division, of which he was the first head, concerned with the anticipation and solution of the engineering and metallurgical problems involved in the achievement of a high-power nuclear reactor. To this group he recruited a number of outstanding physicists and engineers who undertook, at his instigation, important studies of such problems as uranium metal fabrication, coating and cladding, heat transfer, as well as

studies of alternative (to water) means of removing the fission-heat energy in an operating nuclear reactor.

Szilard's usefulness at this stage stemmed as much from his abilities as an engineer as from his understanding of nuclear physics. Like Wigner, whose early training in Budapest was in engineering -- a budding scientist in Hungary usually turned to engineering as the most effective entrée into science -- ~~he~~ Szilard was a superb engineer. It is therefore no coincidence that the most farsighted and imaginative, as well as solid, contributions to the solution of the project's unprecedented engineering problems came from Szilard's Technological Division and Wigner's Theoretical Division.

The papers reproduced ~~below~~ later in this section, all heretofore unpublished, represent two aspects of Szilard's contributions in this early stage of preparation for the large-scale release of nuclear fission energy in a chain reaction aimed at plutonium production. The first set are concerned with the measurement of basic nuclear constants and their application to the design of the nuclear reactor; the second represent engineering studies, some exploratory and some detailed, on reactor design.

no 1 4. ~~IV.~~ Consolidation

Once the decision had been taken, to construct a plutonium production reactor at Hanford Washington based on the graphite-uranium system with water cooling, Szilard's direct involvement slowly came to an end. The assignment of providing liaison between the project physicists and the engineers of the Du Pont Company, responsible for the construction and operation of the Hanford reactors, was given to Wigner's Theoretical Division; most of Szilard's Technological Division was incorporated into the engineering team, while some of its key members drifted off to other, more immediately pressing aspects of the project. A small group of physicists, under my immediate supervision, continued to work under his direction on some of the unsolved problems of nuclear physics only indirectly associated with the reactor.

However, even though relieved of direct responsibility for the success of the project, Leo Szilard could not divest himself of his deep involvement in its success. He assumed the role of contingency planner, devising alternative approaches in the event of the failure of the water-cooled system, which was encountering unforeseen problems and delays. His favored alternatives were a helium-cooled reactor, and cooling by a eutectic mixture of liquid Bi and Pb. Szilard devoted great efforts toward the detailed design of such reactors; for example, he and I developed a detailed design for a magnetic pump for liquid bismuth, based on a principle which had been invented by him and Einstein in 1926 for the purpose of circulating liquid refrigerants in a system without moving parts.

In this period, Szilard was intrigued with the problem of devising the most efficient system for a nuclear power reactor. In addition to He and liquid Bi cooling (he also foresaw the possibility of using the electromagnetic pump for the circulation of liquid Na) Szilard foretold and investigated the engineering problems associated with most of the reactor systems that ~~which~~ have since been seriously considered or successfully utilized for power purposes — a reactor cooled by the boiling of water; a beryllium moderated reactor; CO_2 cooling. In addition, he placed very high priority on the problem of breeding -- of designing a reactor ~~which~~ that would produce at least one nucleus of a fissile element (U^{233} from Th and Pu^{239} from U^{238} neutron absorption) for each nucleus consumed in fission.

But more and more, convinced that we had won the race against the Germans, Szilard was concerned with the problem of using, or not using, the atomic bomb in a fashion most calculated to insure peace and stability in the postwar world. In part, his concern for more efficient power reactors was motivated by a belief that it would strengthen the hand of the United States Government, in seeking a postwar system of stringent controls over nuclear weapons, to have accumulated, in the early post-war period, a substantial store of fissile material.

And thus, to an increasing extent, in the last years of the war, Szilard's efforts were devoted to the organization of a strong body of opinion among his colleagues in the project for exerting pressures on the government against the hasty and ill-considered use of the first atomic bombs. Although these efforts failed, their failure only spurred him toward~~s~~ greater efforts at political action in the postwar years.

But that is another aspect of history.*4

~~date~~?

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94 * See, for example, Alice Smith, A Peril and a Hope, Chicago: University of Chicago Press, 1965

no! ~~I~~V. L'Envoi~~e~~

I could not forego this opportunity to write ~~for~~ some very personal recollections of the unique experience I had in working in close scientific collaboration with Leo Szilard during World War II.

At our first meeting, sometime early in 1941, in response to his telephoned invitation to lunch, I succumbed, with only token resistance, to his irresistibly exciting invitation to join him in the attempt to produce a nuclear chain reaction.

At the time, I was in my second year of graduate studies at Columbia University~~,~~ and was just becoming involved in research in Rabi's molecular beam laboratory. But I had heard of, and had been intrigued by the discovery of fission -- it was in the air at Columbia -- and, more important, I was convinced that the United States would soon (should already have) become involved in the war and I was chafing for a chance to make some contribution. Fortunately, I had already hit upon a subject ~~which~~ that I thought might be suitable for a (theoretical) thesis, and Szilard convinced me that I could continue work on this subject in my spare time and helped to arrange this with Willis Lamb, who agreed to be my thesis supervisor.* 15

I never knew why Szilard chose me as his assistant; I suppose he needed someone who would to pursue some of his myriad ideas, who would follow his instructions without too much independence, who had enough competence in theoretical techniques

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15 * By fortunate chance, this worked out as anticipated, and I was able to submit my thesis and receive my degree immediately after the war.

to carry out a program of computation without too much supervision, and who understood enough -- but not too much -- to pick up a partly developed idea and carry it to completion. Whatever his original expectations, I seemed to suit him well, for we worked in close collaboration until I left Chicago for Los Alamos in 1944.

Szilard was an ideal boss. He supervised and inspired, but never held me back. He encouraged independence and enthusiasm. When I became excited about the early experiments of Fermi and his group at Columbia on the uranium-graphite lattice (the first exponential pile experiments), he encouraged me to join that group and to participate in the crucial experiments that ~~which~~ proved the feasibility of the chain reaction. By the time we moved to Chicago, I was leader of a small group who ~~which~~ carried on measurements on fundamental nuclear processes, mostly inspired by Szilard and ~~all~~ followed by him with close interest.

The day I came to work for Szilard he installed me in his cluttered room in the King's Crown Hotel, adjacent to Columbia, presented me with a 20-inch slide rule (which, until today, remains my favorite computer), outlined a program for improved computation of the properties of the carbon-uranium lattice which was to occupy me for months, and informed me that he had to leave town for a week or so. At that time Szilard was continuously traveling, to Washington to assure continuing support for the project, to the laboratories and factories capable of producing pure graphite and uranium metal to goad and cajole their adoption of programs for the large-scale production of these vital materials even in the absence of governmental assurance of support of these programs. This was the critical period of preparation for the chain reaction, and Szilard's finger was in every pie. But he did not need to be around continually in order to supply ideas and inspiration enough to occupy all the efforts of more than one eager assistant.

Characteristically, Szilard spoke very little of his earlier life and pre-war activities. Only little by little, in bits and pieces, did I learn of his work in Berlin with von Laue and Einstein -- mostly during an automobile trip from New York to Princeton on an occasion when he wanted to consult with Einstein, and on which he thoughtfully took the opportunity to introduce me to the great man. His foresight in leaving Germany immediately after Hitler's seizure of power, he explained ~~away~~ with the comment that it only had required a short airplane trip from Berlin to London, passing over that impossible jumble of frontiers and unstable political entities, to convince him that the Europe of 1932 was no place in which a sane man could stake his future. My interest in Chinese poetry -- I was reading a book of translations by Arthur Waley -- occasioned some fragmentary anecdotes concerning his acquaintance with Waley during the formation in England of a Committee to aid the exodus and placement in British Universities of young German-Jewish scholars, a project, as I learned later, for whose conception and execution Szilard was primarily responsible. And despite our very close collaboration during the period 1941-1944, I never heard directly from him of his difficulties with the narrow-minded and unimaginative higher authorities of the Manhattan Project, who could neither understand nor appreciate his independence and unconventionality. His method of teaching and guidance was by leadership and example and never by resort to preachment or authority.

After the success of the plutonium project was assured, and I departed with his blessing to work on the next stage of the project, we corresponded only occasionally, on aspects of the physics problems with which we had been mutually concerned. But I saw him from time to time, when project business brought me to Chicago, and I could not help but be infected with his deep concerns over the consequences of the forces he had been so instrumental in releasing.

After the war his scientific interests turned to biology, but on our occasional meetings he exhibited the same keen and penetrating interest in the problems of particle physics, in which I had become engaged, as he had in the problems of nuclear physics on which we had worked together. ~~And~~ He always maintained a deeply human, if apparently impersonal, interest in me as an individual.

More and more after 1946 our mutual concerns converged on the problems of controlling the nuclear genie. And so, in the last years of his life, I again became his assistant in the enterprise of building a more livable world, the ideal ~~which~~ that guided all of Leo Szilard's enterprises from his earliest days until his death. stet

November 1970

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188

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Listing of Documents Included in Part III

¶ The following list includes the ordering of the papers referred to in this introductory essay that are reproduced here in Part III.

1. "Creative Intelligence and Society," address by Szilard at the University of Chicago, July 31, 1946.
2. Szilard - Wigner letter, February 1, 1956.
3. Szilard - Fermi correspondence, five letters, July 1939.
4. Einstein - Roosevelt letter, August 2, 1939, with Szilard Memorandum, August 15, 1939, and Szilard - Briggs Memorandum, October 21, 1936.
5. Report A-55, "Divergent chain Reaction in Systems composed of Uranium and Carbon."
6. Szilard - Tate letter, February 6, 1940. A report of the patent on the nuclear reactor with a clipping from The New York Times of May 19, 1955.

Physics Reports

7. CP-285, December 12, 1941: Preliminary Report on Inelastic Collision of Neutrons in Uranium and other Heavy Elements, with W. H. Zinn.
8. CP-316, November 14, 1941: Preliminary Report on Fission Caused by Fission Neutrons, with J. Marshall, Jr.
9. CP-317, December 5, 1941: Preliminary Report on the Capture of Neutrons by Uranium in the Energy Region of Photo Neutrons from Radium Beryllium Sources, with J. Marshall.
10. CF-338, December 16, 1941: Memorandum on the Critical Condition for a Fast Neutron Chain Reaction inside a Spherical Shell of Uranium Metal, with B. T. Feld.

11. C-189, July 10, 1942: "Approximate Boundary Conditions for Diffusion Equation at Interface between two Media," with A.M. Weinberg, E. P. Wigner, and R. F. Christy.

12. CP-412, January 19, 1943: Preliminary Comparison of Radon-Boron and Ba + Be Neutron Sources, with J. Ashkin, S. Bernstein, B. Feld, and H. Kubitschek.

13. CF-1117, December 29, 1943: Neutron Emission in Fission of U^{238}, with B. Feld, J. Ashkin, S. Bernstein, L. Crentz, J. Kelsner, and R. Scalettar.

14. MDDC-1292, date unknown: Inelastic Scattering of Fast Neutrons, with S. Bernstein and B. T. Feld.

15. MDDC-1536, August 27, 1947: Inelastic Scattering of Fe, Pb, and Bi, with S. Bernstein, B. Feld, and J. Ashkin (Phys. Rev. 73, 1307, 1948).

16. Use of Threshold Detectors for Fast Neutron Studies, with B. Feld and R. Scalettar (Phys. Rev. 71, 464, 1947).

Metallurgy and Engineering Reports

17. A-24, August 16, 1911: Preliminary Report on the Melting of Uranium Powder.

18. C-130, June 15, 1942: On the Cooling of the Power Plant (with addenda C-146 and C-150).

19. CP-308, June 18, 1942: Examples of Pressure Drop Calculations in Parallel Flow Helium Cooling, with B.T. Feld.

20. CF-279, July 14, 1942: A Magnetic Pump for Liquid Bismuth, with B. Feld.

21. CP-360, November 23, 1942: Short Memorandum on Bismuth Cooled Power Unit

22. MUC-LS-60, March 6, 1945: Liquid Metal Cooled Fast Neutron Breeder.

A - Early (Pre-Chicago Project Reports)
C - Chicago
CP - Chicago Physics
CE - " Engineering
CF - " Fast Neutron work

July 16, 1969

MDDC - Manh. Distr. Declassif. doc
MUC - ? Metall. U chicago
N

Bibliography (1940-1945)

(mostly unpublished)

I. Physics

*1. Report A-55 (Declassified as MDDC-446), "Divergent Chain Reactions in Systems Composed of Uranium and Carbon", submitted to the Physical Review, Feb. 1940, but publication witheld at the author's request.

Type like this

1. 2. A-45, Suggestion for a Search for Element 94 in Nature, 9/26/41 (CN-G)

2. 3. A-56, Memorandum Raising the Question Whether the Action of Explosive Chain-Reacting Bodies Can be Based on an "Expulsion" Method, (Columbia Univ) 10/21/41

*4. CP-316, Preliminary Report on Fission Caused by Fissi on Neutrons (with J. Marshall, Jr.), 11/14/41

*5. CP-317, Preliminary Report on the Capture of Neutrons by Urnaium in the Energy Region of Photo Neutrons from Radium Beryllium Sources (with J. Marshall), 12/5/41

*6. CP-285, Preliminary Report on Inelastic Collision of Neutrons in Uranium and Other Heavy Elements (with W. H. Zinn), 12/12/41

*7. CF-338, Memorandum on the Critical Condition for a Fast Neutron Chain Reaction Inside a Spherical Shell of Uranium Metal (with B. T. Feld), 12/26/41

*8. ~~C~~ CP-189, Approximate Boundary Conditions for Diffusion Equation at Interface between Two Media (with A. M. Weinberg, E. P. Wigner, and R. F. Christy) 7/10/42

9. CP-412, Preliminary Comparison of Radon-Boron and Ra + Be Neutron Sources (MDDC-1436) (with J. Ashkin, S. Bernstein, B. Feld, and H. Kubitschek) 1/19/43

10. CF-1177, Neutron Emission in Fission of U^{238} (with B. Feld, J. Ashkin, S. Bernstein, L. Crentz, J. Kelsner and R. Scalettar), 12/29/43

* Reprinted in full in this volume

Feld

*11. MDDC-897, Use of Threshold Detectors for Fast Neutron Studies, (with B. Feld, E. Scalettar, L. Szilard,) (1943?) Phys. Rev. 71, 464 (1947) published

*12. MDDC-1292, Inelastic Scattering of Fast Neutrons, S. Bernstein, B. T. Feld, L. Szilard. (?.1943)

*13. MDDC-1536, Inelastic Scattering of Fe, Pb, and Bi. L. Szilard, S. Bernstein, B. Feld, J. Ashkin, Phys. Rev. 73, 1307-~~10~~ (1948) MonP-375 published

II. Metallurgy and Engineering

*1. Report A-24 (8/16/41), "Preliminary Report on the Melting of Uranium Powder" (CT-M)

W *2. C-130, (CE-130) On the Cooling of the Power Plant, 6/15/42; C-146, (CE) Addendum, 6/~~15~~ 24/42; C-150, (CE-150) Addendum, 6/29/42.

*3. CP-308, Examples for Pressure Drop Calculations in Parallel Flow Helium Cooling (with B. T. Feld) 6/18/42

*4. CE-279, A Magnetic Pump for Liquid Bismuth, (with B. Feld) 7/14/42

add CE-236 Aug 15, 1942 W

5. CE-271, Report for Month Ending 9/15/42(Technological Division section by L. Szilard) (Magnesium reduction) add from Wigner

6. CE-301 , Report for month ending 10/15/42 (Technological Division section by L. Szilard) (internal water cooling, Bi cooling, U^{238} fission reaction with fast neutrons)

7. CP-357, Uranium Aggregates for Power Unit (Nov 23 - 1942) ~~(no date)~~

*8. CP-360, Short Memorandum on Bismuth Cooled Power Unit, 11/23/42

9. MUC-LS-1, Memorandum on Metallurgical Problems Connected with the Power Unit which is Cooled by Liquid Metal, June 12, 1943.

10. MUC-LS-2, Memorandum Concerning Liquid Metal Cooling, 7-30-43

11. MUC-LS-5, Memo to S. K. Allison, Potentialities of a beryllium power unit, 11/15/43

12. MUC-LS-13, Memo to A. H. Compton, Designing of production units cooled by bismuth alloy, 3-28-44

N-866,

13. Ohlinger, L. A., and L. Szilard G. Young New End closure for Al cans. March 30, 1944

14. MUC-LS-19, Memo to J. Chipman, Poisoning the pile with thorium metal alloy, 4/19/44

15. MUC-LS-21, Memo to E. P. Wigner, Re: Purifying plutonium metal, 5-12-44

16. MUC-LS-22, Memo to E.P. Wigner, Peristaltic method for purifying plutonium, 5-20-44.

17. MUC-LS-23, Memo to A. H. Compton, Bismuth cooled graphite unit, 5-26-44

18. MUC-LS-24, Memo to C. M. Cooper, Swelling of slugs, 6/21/44

19. MUC-LS-27 (N-1346), Memo to E. P. Wigner, An extrusion process eliminating the weld on slugs, 7/7/44

20. MUC-LS-28, Water Moderated Pile with P-9 Core (see N-1355), 7/7/44

21. MUC-LS-41 (N-1329h), Memo to S. K. Allison, Assaying 95% enriched uranium, 8/31/44

22. MUC-LS-60, Memoranda on liquid metal cooled fast neutron breeders, 3/6/45

III. Miscellaneous

1. MUC-LS-4, Memo to B. Pregel, Source of Pb from Canadian pitchblende, 8/2/43

2

2. MUC-LS-8, Memo to James Franck, Atomic power and Germany, 12-8-43

3. MUC-LS-61, Memorandum on the development of the atomic bomb, 3/12/45

4. MUC-LS-63, Memo to W. Bartky, Postwar plans, 3/15/45

5. MUC-LS-70, Letter to F. Oppenheimer, Petition sent to Washington, 7-23-45

Wigner has in addition.

~~CE - 236~~

~~CE - 324 (as given in CE - 277)~~

~~CS - 267 (Sept 16, 1942)~~

~~N866~~ ~~N1346~~

~~N962~~ ~~N1355~~

Introduction to the Collected Works of Leo Szilard relating to the Manhattan Project

B.T. Feld 2nd draft January 5, 1968

I. Preparation

The genius of Leo Szilard had many facets: a power of foresight, bordering on the visionary, which enabled him to comprehend the long-range consequences of a scientific discovery or of a social situation long before they were imagined by even the most astute; an ingenuity of approach and execution which led him to discover the most unexpected, but yet direct, solutions to difficult problems; a tenacity which prevented him from abandoning difficult problems, but forced him to return and return again, devising new approaches at each attempt, and yet permitting him to leave the details of the solution in other, competent hands once the appropriate approach had been found and the path charted; and an overriding sense of responsibility, towards his work, towards science, but most of all towards his fellow men.

All these combined to make Leo Szilard an indispensible factor in the successful achievement of the first man-made nuclear chain reaction and in the vast wartime enterprise known as the Manhattan Project, which culminated in the first man-made nuclear explosion.

It was no accident that Leo Szilard was on the spot, at Columbia University, ready to plunge into the experimental investigations of its ramifications immediately after the announcement of the discovery of nuclear fission. He had sensed, with that uncanny instinct which never deserted him, that this was where "the action" would be; and he had been scheming and dreaming about the controlled release of nuclear energy ever since a remark by Lord Rutherford in the fall of 1933 -- to the effect that talk of the large scale liberation of atomic energy is "moonshine" -- had intrigued and goaded him into taking up the study of nuclear physics. Characteristically, he immediately realized that, of all the available nuclear particles, it was the neutron which held the key, through its ability to penetrate matter without the energy-consuming electromagnetic interactions which prevented all but a minute fraction of the other, charged nuclear projectiles from inducing nuclear reactions. The problem was to discover a neutron induced reaction in which at least one neutron is emitted for each neutron absorbed.

And so, a refugee in England, with no academic base and practically no resources, he embarked on a series of fundamental researches which, even though they did not succeed in uncovering the type of reaction he had envisaged, were to give him that profound understanding of nuclear phenomena which was indispensible to his exploitation of the fission reaction when it was finally discovered.

The scientific aspects of Szilard's work in England, prior to his emigration to New York at the beginning of 1938, are described elsewhere in this volume. It is sufficient to observe that if his late entry into the field of nuclear physics did not permit him to participate in its most fundamental discoveries, it did not take him long to overcome the handicaps of a newcomer, so that by 1938 Szilard could be counted among the elite of the small international community of nuclear physicists. Still, he was always profoundly aware of his place in the historical scheme, with no illusions and that delicate balance of judgment with which Szilard was always able to view history. As usual, this judgment is best described in his own words, and it is appropriate to reproduce here an address delivered by Szilard at the University of Chicago in 1946; there is, to my knowledge, no more succinct or penetrating summary of the history of nuclear physics from its inception until the first chain reaction, nor of his own role in this historical chain.

Item I: Creative Intelligence and Society (July 31, 1946)

(note to G. W. Szilard -- just what was the occasion of this talk?)

II. Genesis

Although much has been written about the genesis of the wartime atomic energy project, its pre-history -- the period between the arrival of Bohr in the US in 1938, with fresh knowledge of the discovery of fission, and the setting up of the official Manhattan project in 1941 -- remains somewhat obscure and confused. In part, this is a natural consequence of the fact that in science, when the time is ripe, similar ideas occur simultaneously at many sources and the rapid interchange of ideas and information serves to confuse their origins, even in the minds of the most important contributors. Furthermore, most scientists are too concerned with their current researches to take the time to unravel and record their recollections of the past.

Fortunately, Szilard has left us with a clear record of the steps leading to the conception of the chain reacting system utilizing a lattice of natural uranium embedded in a matrix of graphite. These steps are outlined in a letter written to E. P. Wigner in 1956, which is reproduced at the end of this section, together with the documents to which it refers. [ ]

Having established, in an experiment with Walter H. Zinn, the emission of neutrons in the fission process*, the problem was how to achieve a self-sustaining system -- one in which the neutrons emitted in a fission would be capable of inducing at least one more fission reaction. There are two requirements for such a system: first, the fast neutrons emitted in the fission process must be slowed down to thermal energies, at which the probability of producing a fission in uranium is much increased; second, it is necessary to avoid the parasitic absorption of these neutrons, both by

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* The same fact was simultaneously and independently ascertained by Anderson, Hanstein and Fermi, also at Columbia University [Phys. Rev. 55, 797 (1939)] and, slightly before, by Halbon, Joliot and Kowarski in Paris [Nature, 143, 479 (1939)].

uranium during the slowing-down process and by the light elements which are required to reduce rapidly the neutrons to thermal energies before they undergo such absorption or escape from a necessarily finite system.

After careful consideration of the possibility of utilizing mixtures of uranium and water for this purpose [Anderson, Fermi and Szilard, Phys. Rev. 56, 284 (1939); also Halban, Joliot, Kowarski and Perrin, Journ. de Phys. 10, 428 (1939)], Fermi and Szilard eliminated water from consideration as the slowing-down matrix owing to the strong neutron absorption properties of hydrogen. (In Szilard's case, at least, consideration of the engineering difficulties involved in extracting large amounts of power from a water-uranium system were also involved; he had, from the start, in mind the necessity of producing large amounts of plutonium if the chain reaction was to be of any wartime importance.) Heavy water was also soon discarded owing to the unavailability of large quantities. Carbon, in the form of graphite, was the next choice, despite its considerably poorer energy-moderating properties than the hydrogens, both because of its availability in large quantities and of its small neutron absorption cross section (although it was not known for some time just how small the cross section is; as it later turned out, the upper limit known at that time could be accounted for entirely on the basis of impurities).

The problem of avoiding the parasitic (resonance) absorption of neutrons by uranium during the slowing-down process was more difficult. Szilard, and also Fermi,* hit upon the solution of separating the uranium from the graphite, so that the moderating process would take place in the absence of uranium. It remained to work out the necessary proportions of the constituents and their optimum geometrical distribution, on the basis of neutron reaction properties very incompletely known

* Although Fermi was apparently thinking in terms of alternate layers of uranium and graphite, rather than the lattice structure devised by Szilard.

and in the absence of a satisfactory theoretical framework for treating such heterogeneous mixtures.

By the end of 1939, Szilard had worked out a rough theory of the nuclear chain reaction in a graphite-uranium lattice, which served to convince him of its feasibility. This he submitted first as a letter and then as a paper to the Physical Review in February, 1940. However, being by then convinced of the favorable prospects for its success, and concerned that its publication would aid Germany in its prior achievement, its publication was indefinitely deferred at his request. This classic paper is here published for the first time.*

In acknowledgement of their invention, Fermi and Szilard were jointly awarded a patent on the nuclear reactor in 1955 by the United States Government.

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* This paper received only very limited circulation in the Manhattan Project; its existence was quite widely known, as A-55, but under the confusing and wholly misguided system of compartmentalization it was not possible, even for members of the project, to obtain most reports without establishing a "need to know" their contents. Early (pre-Chicago project) reports on the chain reaction were classified under the heading A; later, a more elaborate system was established: C for Chicago, CP for physics; CE for engineering; CF for fast neutron work, etc. But the system served more to confuse than to clarify.

II. Letter from Szilard to Wigner, Feb. 1, 1956, followed by:

1. Letter to the Editor of the Physical Review by Szilard and Zinn

2. Paper in the Physical Review: Neutron Production and Absorption in Uranium, by Anderson, Fermi and Szilard.

3,4. Exchange of letters between Szilard and Fermi on the use of carbon. Permission

5. Letter from Einstein to Roosevelt and associated memoranda by Szilard. "

6. Report A-55 (note to G. W. Szilard: is there a better copy of A55 available, which includes the footnotes?) Also included are the letter to the editor, summarizing the results, which was submitted about a week before the paper, and a memorandum explaining the background, written in July 1941, which Szilard requested be attached to the project report in order that appropriate credit be given for the genesis of the ideas.

7. Report of the patent on the nuclear reactor, with a clipping from the NY Times of May 19, 1955.

III. Attainment:

With the broad outlines of the solution generally agreed upon, Szilard was content to leave to Fermi the main responsibility for the experiments which were to prove the feasibility of the graphite-uranium chain reaction and explore its properties. Although he was deeply involved in the conception of the "exponential" experiment for measuring the neutron absorption properties of graphite and for testing the multiplicative properties of sub-critical uranium-graphite systems, and he continued to take an active interest in the details of the experiments, Szilard's main efforts, from 1940 on, were directed towards planning for the earliest possible use of the reactor for the large-scale production of plutonium.

That, in fact, enough plutonium was produced to be used militarily before the end of the war (albeit just barely), is mainly owed to the acute and incisive foresight exhibited by Szilard in the earliest stages of the project.

This may be the appropriate point for some remarks on the question of indispensibility: in the history of science very few men have been indispensible; particularly in applied science, and the chain reaction definitely falls into this category, no man is indispensible. Many men in many countries had the same or similar ideas, and some advanced them relatively far under much more adverse circumstances.* The remarkable exploit involved in the plutonium project was the short time-span between the discovery of fission at the end of 1938 and the production by means of a high-power-producing nuclear reactor of kilogram quantities of an entirely new element, plutonium, by 1944 -- all this requiring the invention and reduction to operation of the nuclear reactor and the development of a completely new branch of engineering in a field where many of the scientific fundamentals were still not

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* See the recent revelations on the exploits of Kurchotov and co-workers in the USSR, B.A.S., Dec. 1967.

known or imperfectly understood.

The scientists of the Manhattan project thought they were involved in a race against the Germans; they were certainly racing against time. With hindsight, one may denigrate the importance of their accomplishment or regret, as Szilard was afterwards frequently to regret, that they succeeded so well; but retrospect cannot dim the lustre of their achievement. And in this accomplishment no man was more indispensible than Leo Szilard, and only a few -- notably Enrico Fermi and Eugene P. Wigner for the plutonium-production aspect of the project -- can be placed at the same level.

As noted, Szilard's major contributions came in the earliest stages, in 1940-41, mostly commencing even before the Columbia group moved to Chicago and was incorporated into the many-faceted Manhattan Project. It was he who first recognized the importance of securing copious quantities of pure graphite, and who initiated the steps necessary to assure this. More important, realizing at a very early stage the necessity of securing pure uranium in its high-density metallic form -- uranium metal had not previously been industrially produced, and its metallurgy was only vaguely comprehended -- Szilard initiated and personally supervised a program aimed at the industrial production of uranium metal of high purity. A brief report (A-24), reproduced below, shows how important was his contribution to the success of this aspect of the project.

Szilard's first act on moving to Chicago was to set up a Technological Division, of which he was the first head, concerned with the anticipation and solution of the engineering and metallurgical problems involved in the achievement of a high-power nuclear reactor. To this group he recruited a number of outstanding physicists and engineers who undertook, at his instigation, important studies of such problems as uranium metal fabrication, coating and cladding, heat transfer, as well as

studies of alternative (to water) means of removing the fission-heat energy in an operating nuclear reactor.

Szilard's usefulness at this stage stemmed as much from his abilities as an engineer as from his understanding of nuclear physics. Like Wigner, whose early training in Budapest was in engineering -- a budding scientist in Hungary usually turned to engineering as the most effective entree into science -- he was a superb engineer. It is therefore no coincidence that the most farsighted and imaginative, as well as solid, contributions to the solution of the project's unprecedented engineering problems came from Szilard's Technological Division and Wigner's Theoretical Division.

The papers reproduced below, all heretofore unpublished, represent two aspects of Szilard's contributions in this early stage of preparation for the large-scale release of nuclear fission energy in a chain reaction aimed at plutonium production. The first set are concerned with the measurement of basic nuclear constants and their application to the design of the nuclear reactor; the second represent engineering studies, some exploratory and some detailed, on reactor design.

III. a. Physics:

1) CP-285 (12/12/41): Preliminary Report on Inelastic Collision of Neutrons in Uranium and other Heavy Elements, with W. H. Zinn.

2) CP-316 (11/14/41): Preliminary Report on Fission Caused by Fission Neutrons, with J. Marshall, Jr.

3) CP-317 (12/5/41): Preliminary Report on the Capture of Neutrons by Uranium in the Energy Region of Photo Neutrons from Radium Beryllium Sources, with J. Marshall.

4) CF-338 (12/16/41) Memorandum on the Critical Condition for a Fast Neutron Chain Reaction inside a Spherical Shell of Uranium Metal, with B. T. Feld

5) C-189 (7/10/42): Approximate Boundary Condtions for Diffusion Equation at Interface between two Media. with A. M. Weinberg, E. P. Wigner, and R. F. Christy

(Note to G. W. Szilard: If there is not enough space, 1, 2, and 3 will suffice as examples, and the others can be summarized)

III. b. Metallurgy and Engineering:

1) A-24 (8/16/41): Preliminary Report on the Melting of Uranium Powder.

2) C-130 (6/15/42): On the Cooling of the Power Plant (with addenda C-146 and C-150).

IV. Consolidation:

Once the decision had been taken, to construct a plutonium production reactor at Hanford based on the graphite-uranium system with water cooling, Szilard's direct involvement slowly came to an end. The assignment of providing liaison between the project physicists and the engineers of the DuPont Company, responsible for the construction and operation of the Hanford reactors, was given to Wigner's Theoretical Division; most of Szilard's Technological Division was incorporated into the engineering team, while some of its key members drifted off to other, more immediately pressing aspects of the project. A small group of physicists, under my immediate supervision, continued to work under his direction on some of the unsolved problems of nuclear physics only indirectly associated with the reactor.

However, even though relieved of direct responsibility for the success of the project, Leo Szilard could not divest himself of his deep involvement in its success. He assumed the role of contingency planner, devising alternative approaches in the event of the failure of the water-cooled system, which was encountering unforeseen problems and delays. His favored alternatives were a He-cooled reactor, and cooling by a eutectic mixture of liquid Bi and Pb. Szilard devoted great efforts toward the detailed design of such reactors; for example, he and I developed a detailed design for a magnetic pump for liquid bismuth, based on a principle which had been invented by him and Einstein in 19 for the purpose of circulating liquid refrigerants in a system without moving parts.

In this period, Szilard was intrigued with the problem of devising the most efficient system for a nuclear power reactor. In addition to He and Liquid Bi cooling (he also foresaw the possibility of using the electromagnetic pump for the circulation of liquid Na) Szilard foretold and investigated the engineering problems associated with most of the reactor systems which have since been seriously considered or successfully utilized for power purposes -- a reactor cooled by the boiling of water; a beryllium moderated reactor; CO_2 cooling. In addition, he placed very high priority on the problem of breeding -- of designing a reactor which would produce at least one nucleus of a fissile element (U^{233} from Th; Pu^{239} from U^{238} neutron absorption) for each nucleus consumed in fission.

But more and more, convinced that we had won the race against the Germans, Szilard was concerned with the problem of using, or not using, the atomic bomb in a fashion most calculated to insure peace and stability in the postwar world. In part, his concern for more efficient power reactors was motivated by a belief that it would strengthen the hand of the United States Government, in seeking a postwar system of stringent controls over nuclear weapons, to have accumulated, in the early post-war period, a substantial store of fissile material.

And thus, to an increasing extent, in the last years of the war, Szilard's efforts were devoted to the organization of a strong body of opinion among his colleagues in the project for exerting pressures on the government against the hasty and ill-considered use of the first atomic bombs. Although these efforts failed, their failure only spurred him towards greater efforts at political action in the postwar years.

But that is another aspect of history.*

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* See, for example, Alice Smith, A Peril and a Hope, University of Chicago Press, ______

IV. a. Physics:

1. CP-412 (1/19/43): Preliminary Comparison of Radon-Boron and Ba+Be Neutron Sources, with J. Ashkin, S. Bernstein, B. Feld and H. Kubitschek (use abstract only)

2. CF-1117 (12/29/43). Neutron Emission in Fission of U^{238}, with B. Feld, J. Ashkin, S. Bernstein, L. Crentz, J. Kelsner and R. Scalettar.

3. MDDC-1292 (date unknown) Inelastic Scattering of Fast neutrons, with S. Bernstein and B. T. Feld

4. MDDC-1536 (8/27/47) Inelastic Scattering of Fe, Pb, and Bi, with S. Bernstein, B. Feld, and J. Ashkin (Phys. Rev. 73, 1307 (1948))

5. Use of Threshold Detectors for Fast Neutron Studies, with B. Feld and R. Scalettar (Phys. Rev. 71, 464 (1947)).

IV. b. Engineering:

1. CP-308 (6/18/42). Examples of Pressure Drop Calculations in Parallel Flow Helium Cooling, with B. T. Feld.

2. CF-279 (7/14/42). A Magnetic Pump for Liquid Bismuth, with B. Feld.

3. CP-360 (11/23/42). Short Memorandum on Bismuth Cooled Power Unit

4. MUC-LS-60 (3/16/45). Liquid Metal Cooled Fast Neutron Breeder.

V. L'Envoie:

I could not forego this opportunity for some very personal recollections of the unique experience I had in working in close scientific collaboration with Leo Szilard during World War II.

At our first meeting, sometime early in 1941, in response to his telephoned invitation to lunch, I succumbed, with only token resistance, to his irresistably exciting invitation to join him in the attempt to produce a nuclear chain reaction.

At the time, I was in my second year of graduate studies at Columbia University, and just becoming involved in research in Rabi's molecular beam laboratory. But I had heard of, and had been intrigued by the discovery of fission -- it was in the air at Columbia -- and, more important, I was convinced that the United States would soon (should already have) become involved in the war and I was chafing for a chance to make some contribution. Fortunately, I had already hit upon a subject which I thought might be suitable for a (theoretical) thesis, and Szilard convinced me that I could continue work on this subject in my spare time and helped to arrange this with Willis Lamb, who agreed to be my thesis supervisor.*

I never knew why Szilard chose me as his assistant; I suppose he needed someone to pursue some of his myriad ideas, who would follow his instructions without too much independence, who had enough competence in theoretical techniques

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* By fortunate chance, this worked out as anticipated, and I was able to submit my thesis and receive my degree immediately after the war.

to carry out a program of computation without too much supervision, and who understood enough -- but not too much -- to pick up a partly developed idea and carry it to completion. Whatever his original expectations, I seemed to suit him well, for we worked in close collaboration until I left Chicago for Los Alamos in 1944.

Szilard was an ideal boss. He supervised and inspired, but never held me back. He encouraged independence and enthusiasm. When I became excited about the early experiments of Fermi and his group at Columbia on the uranium-graphite lattice (the first exponential pile experiments) he encouraged me to join that group and to participate in the crucial experiments which proved the feasibility of the chain reaction. By the time we moved to Chicago, I was leader of a small group which carried on measurements on fundamental nuclear processes, mostly inspired by Szilard and all followed by him with close interest.

The day I came to work for Szilard he installed me in his cluttered room in the Kings Crown Hotel, adjacent to Columbia, presented me with a 20" slide rule (which, until today, remains my favorite computer), outlined a program for improved computation of the properties of the carbon-uranium lattice which was to occupy me for months, and informed me that he had to leave town for a week or so. At that time Szilard was continuously traveling, to Washington to assure continuing support for the project, to the laboratories and factories capable of producing pure graphite and uranium metal to goad and cajole their adoption of programs for the large-scale production of these vital materials even in the absence of governmental assurance of support of these programs. This was the critical period of preparation for the chain reaction, and Szilard's finger was in every pie. But he did not need to be around continually in order to supply ideas and inspiration enough to occupy all the efforts of more than one eager assistant.

Characteristically, Szilard spoke very little of his earlier life and pre-war activities. Only little by little, in bits and pieces, did I learn of his work in Berlin with von Laue and Einstein -- mostly during an automobile trip from New York to Princeton on an occasion when he wanted to consult with Einstein, and on which he thoughtfully took the opportunity to introduce me to the great man. His foresight in leaving Germany immediately after Hitler's siezure of power, he explained away with the comment that it only had required a short airplane trip from Berlin to London, passing over that impossible jumble of frontiers and unstable political entities, to convince him that the Europe of 1932 was no place in which a sane man could stake his future. My interest in Chinese poetry -- I was reading a book of translations by Arthur Waley -- occasioned some fragmentary anecdotes concerning his acquaintance with Waley during the formation in England of a Committee to aid the exodus and placement in British Universities of young German-Jewish scholars, a project, as I learned later, for whose conception and execution Szilard was primarily responsible. And despite our very close collaboration during the period 1941-1944, I never heard directly from him of his difficulties with the narrow-minded and unimaginative higher authorities of the Manhattan Project, who could neither understand nor appreciate his independence and unconventionality. His method of teaching and guidance was by leadership and example -- never by resort to preachment or authority.

After the success of the plutonium project was assured, and I departed with his blessing to work on the next stage of the project, we corresponded only occasionally, on aspects of the physics problems with which we had been mutually concerned. But I saw him from time to time, when project business brought me to Chicago, and I could not help but be infected with his deep concerns over the consequences of the forces he had been so instrumental in releasing.

After the war his scientific interests turned to biology, but on our occasional meetings he exhibited the same keen and penetrating interest in the problems of particle physics, in which I had become engaged, as he had in the problems of nuclear physics on which we had worked together. And he always maintained a deeply human, if apparently impersonal, interest in me as an individual.

More and more after 1946 our mutual concerns converged on the problems of controlling the nuclear geni. And so, in the last years of his life, I again became his assistant in the enterprise of building a more livable world, the ideal which guided all of Leo Szilard's enterprises from his earliest days until his death.

VI. Appendix

There follows a list of the reports, memoranda, letters and other documents which are listed in the retrospective files of the Manhattan project, with brief summaries of the contents of some of the more important of them. Since Szilard was habituated to memoranda, which recorded his ideas and inventions, many of which never went beyond his own files, this list is of necessity incomplete. But merely listing them provides a striking demonstration of the breadth of vision and versatility of this remarkable man.

lists

Please return to
JWS

B.T. Feld

2nd draft ~~First draft~~
January 1968
Semi-final

I. Preparation

The genius of Leo Szilard had many facets: a power of foresight, bordering on the visionary, which enabled him to comprehend the long-range consequences of a scientific discovery or of a social situation long before they were imagined by even the most astute; an ingenuity of approach and execution which led him to discover the most unexpected, but yet direct, solutions to difficult problems; a tenacity which prevented him from abandoning difficult problems, but forced him to return and return again, devising new approaches at each attempt, and yet permitting him to leave the details of the solution in other, competent hands once the appropriate approach had been found and the path charted; and an overriding sense of responsibility, towards his work, towards science, but most of all towards his fellow men.

All these combined to make Leo Szilard an indispensible factor in the successful achievement of the first man-made nuclear chain reaction and in the vast wartime enterprise known as the Manhattan Project, which culminated in the first man-made nuclear explosion.

It was no accident that Leo Szilard was on the spot, at Columbia University, ready to plunge into the experimental investigations of its ramifications immediately after the announcement of the discovery of nuclear fission. He had sensed, with that uncanny instinct which never deserted him, that this was where "the action" would be; and he had been scheming and dreaming about the controlled release of nuclear energy ever since a remark by Lord Rutherford in the fall of 1933 -- to the effect that talk of the large scale liberation of atomic energy is "moonshine" -- had intrigued and goaded him into taking up the study of nuclear physics. Charac-

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teristically, he immediately realized that, of all the available nuclear particles, it was the neutron which held the key, through its ability to penetrate matter without the energy-consuming electromagnetic interactions which prevented all but a minute fraction of the other, charged nuclear projectiles from inducing nuclear reactions. The problem was to discover a neutron induced reaction in which at least one neutron is emitted for each neutron absorbed.

And so, a refugee in England, with no academic base and practically no resources, he embarked on a series of fundamental researches which, even though they did not succeed in uncovering the type of reaction he had envisaged, were to give him that profound understanding of nuclear phenomena which was indispensible to his exploitation of the fission reaction when it was finally discovered.

The scientific aspects of Szilard's work in England, prior to his emigration to New York at the beginning of 1938, are described elsewhere in this volume. It is sufficient to observe that if his late entry into the field of nuclear physics did not permit him to participate in its most fundamental discoveries, it did not take him long to overcome the handicaps of a newcomer, so that by 1938 Szilard could be counted among the elite of the small international community of nuclear physicists. Still, he was always profoundly aware of his place in the historical scheme, with no illusions and that delicate balance of judgment with which Szilard was always able to view history. As usual, this judgment is best described in his own words, and it is appropriate to reproduce here an address delivered by Szilard at the University of Chicago in 1946; there is, to my knowledge, no more succinct or penetrating summary of the history of nuclear physics from its inception until the first chain reaction, nor of his own role in this historical chain.

Item I: Creative Intelligence and Society

(note to G. W. Szilard -- just what was the occasion of this talk?)

II. Genesis

Although much has been written about the genesis of the wartime atomic energy project, its pre-history -- the period between the arrival of Bohr in the US in 1938, with fresh knowledge of the discovery of fission, and the setting up of the official Manhattan project in 1941 -- remains somewhat obscure and confused. In part, this is a natural consequence of the fact that in science, when the time is ripe, similar ideas occur simultaneously at many sources and the rapid interchange of ideas and information serves to confuse their origins, even in the minds of the most important contributors. Furthermore, most scientists are too concerned with their current researches to take the time to unravel and record their recollections of the past.

Fortunately, Szilard has left us with a clear record of the steps leading to the conception of the chain reacting system utilizing a lattice of natural uranium embedded in a matrix of graphite. These steps are outlined in a letter written to E. P. Wigner in 1956, which is reproduced at the end of this section, together with the documents to which it refers.

Having established, in an experiment with Walter H. Zinn, the emission of neutrons in the fission process*, the problem was how to achieve a self-sustaining system -- one in which the neutrons emitted in a fission would be capable of inducing at least one more fission reaction. There are two requirements for such a system: first, the fast neutrons emitted in the fission process must be slowed down to thermal energies, at which the probability of producing a fission in uranium is much increased; second, it is necessary to avoid the parasitic absorption of these neutrons, both by

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* The same fact was simultaneously and independently ascertained by Anderson, Hanstein and Fermi, also at Columbia University [Phys. Rev. 55, 797 (1939)] and, slightly before, by Halbon, Joliot and Kowarski in Paris [Nature, 143, 479 (1939)].

uranium during the slowing-down process and by the light elements which are required to reduce rapidly the neutrons to thermal energies before they undergo such absorption or escape from a necessarily finite system.

After careful consideration of the possibility of utilizing mixtures of uranium and water for this purpose [Anderson, Fermi and Szilard, Phys. Rev. 56, 284 (1939); also Halban, Joliot, Kowarski and Perrin, Journ. de Phys. 10, 428 (1939)], Fermi and Szilard eliminated water from consideration as the slowing-down matrix owing to the strong neutron absorption properties of hydrogen. (In Szilard's case, at least, consideration of the engineering difficulties involved in extracting large amounts of power from a water-uranium system were also involved; he had, from the start, in mind the necessity of producing large amounts of plutonium if the chain reaction was to be of any wartime importance.) Heavy water was also soon discarded owing to the unavailability of large quantities. Carbon, in the form of graphite, was the next choice, despite its considerably poorer energy-moderating properties than the hydrogens, both because of its availability in large quantities and of its small neutron absorption cross section (although it was not known for some time just how small the cross section is; as it later turned out, the upper limit known at that time could be accounted for entirely on the basis of impurities).

The problem of avoiding the parasitic (resonance) absorption of neutrons by uranium during the slowing-down process was more difficult. Szilard, and also Fermi,* hit upon the solution of separating the uranium from the graphite, so that the moderating process would take place in the absence of uranium. It remained to work out the necessary proportions of the constituents and their optimum geometrical distribution, on the basis of neutron reaction properties very incompletely known

* Although Fermi was apparently thinking in terms of alternate layers of uranium and graphite, rather than the lattice structure devised by Szilard.

and in the absence of a satisfactory theoretical framework for treating such heterogeneous mixtures.

By the end of 1939, Szilard had worked out a rough theory of the nuclear chain reaction in a graphite-uranium lattice, which served to convince him of its feasibility. This he submitted first as a letter and then as a paper to the Physical Review in February, 1940. However, being by then convinced of the favorable prospects for its success, and concerned that its publication would aid Germany in its prior achievement, its publication was indefinitely deferred at his request. This classic paper is here published for the first time.*

In acknowledgement of their invention, Fermi and Szilard were jointly awarded a patent on the nuclear reactor in 1955 by the United States Government.

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* This paper received only very limited circulation in the Manhattan Project; its existence was quite widely known, as A-55, but under the confusing and wholly misguided system of compartmentalization it was not possible, even for members of the project, to obtain most reports without establishing a "need to know" their contents. Early (pre-Chicago project) reports on the chain reaction were classified under the heading A; later, a more elaborate system was established: C for Chicago, CP for physics; CE for engineering; CF for fast neutron work, etc. But the system served more to confuse than to clarify.

II. Letter from Szilard to Wigner, Feb. 1, 1956, followed by:

1. Letter to the Editor of the Physical Review by Szilard and Zinn

2. Paper in the Physical Review: Neutron Production and Absorption in Uranium, by Anderson, Fermi and Szilard.

3, 4. Exchange of letters between Szilard and Fermi on the use of carbon.

5. Letter from Einstein to Roosevelt and associated memoranda by Szilard.

6. Report A-55 (note to G. W. Szilard: is there a better copy of A55 available, which includes the footnotes?) Also included are the letter to the editor, summarizing the results, which was submitted about a week before the paper, and a memorandum explaining the background, written in July 1941, which Szilard requested be attached to the project report in order that appropriate credit be given for the genesis of the ideas.

7. Report of the patent on the nuclear reactor, with a clipping from the NY Times of May 19, 1955.

III. Attainment:

With the broad outlines of the solution generally agreed upon, Szilard was content to leave to Fermi the main responsibility for the experiments which were to prove the feasibility of the graphite-uranium chain reaction and explore its properties. Although he was deeply involved in the conception of the "exponential" experiment for measuring the neutron absorption properties of graphite and for testing the multiplicative properties of sub-critical uranium-graphite systems, and he continued to take an active interest in the details of the experiments, Szilard's main efforts, from 1940 on, were directed towards planning for the earliest possible use of the reactor for the large-scale production of plutonium.

That, in fact, enough plutonium was produced to be used militarily before the end of the war (albeit just barely), is mainly owed to the acute and incisive foresight exhibited by Szilard in the earliest stages of the project.

This may be the appropriate point for some remarks on the question of indispensibility: in the history of science very few men have been indispensible; particularly in applied science, and the chain reaction definitely falls into this category, no man is indispensible. Many men in many countries had the same or similar ideas, and some advanced them relatively far under much more adverse circumstances.* The remarkable exploit involved in the plutonium project was the short time-span between the discovery of fission at the end of 1938 and the production by means of a high-power-producing nuclear reactor of kilogram quantities of an entirely new element, plutonium, by 1944 -- all this requiring the invention and reduction to operation of the nuclear reactor and the development of a completely new branch of engineering in a field where many of the scientific fundamentals were still not

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* See the recent revelations on the exploits of Kurchotov and co-workers in the USSR, B.A.S., Dec. 1967.

known or imperfectly understood.

The scientists of the Manhattan project thought they were involved in a race against the Germans; they were certainly racing against time. With hindsight, one may denigrate the importance of their accomplishment or regret, as Szilard was afterwards frequently to regret, that they succeeded so well; but retrospect cannot dim the lustre of their achievement. And in this accomplishment no man was more indispensible than Leo Szilard, and only a few -- notably Enrico Fermi and Eugene P. Wigner for the plutonium-production aspect of the project -- can be placed at the same level.

As noted, Szilard's major contributions came in the earliest stages, in 1940-41, mostly commencing even before the Columbia group moved to Chicago and was incorporated into the many-faceted Manhattan Project. It was he who first recognized the importance of securing copious quantities of pure graphite, and who initiated the steps necessary to assure this. More important, realizing at a very early stage the necessity of securing pure uranium in its high-density metallic form -- uranium metal had not previously been industrially produced, and its metallurgy was only vaguely comprehended -- Szilard initiated and personally supervised a program aimed at the industrial production of uranium metal of high purity. A brief report (A-24), reproduced below, shows how important was his contribution to the success of this aspect of the project.

Szilard's first act on moving to Chicago was to set up a Technological Division, of which he was the first head, concerned with the anticipation and solution of the engineering and metallurgical problems involved in the achievement of a high-power nuclear reactor. To this group he recruited a number of outstanding physicists and engineers who undertook, at his instigation, important studies of such problems as uranium metal fabrication, coating and cladding, heat transfer, as well as

studies of alternative (to water) means of removing the fission-heat energy in an operating nuclear reactor.

Szilard's usefulness at this stage stemmed as much from his abilities as an engineer as from his understanding of nuclear physics. Like Wigner, whose early training in Budapest was in engineering -- a budding scientist in Hungary usually turned to engineering as the most effective entree into science -- he was a superb engineer. It is therefore no coincidence that the most farsighted and imaginative, as well as solid, contributions to the solution of the project's unprecedented engineering problems came from Szilard's Technological Division and Wigner's Theoretical Division.

The papers reproduced below, all heretofore unpublished, represent two aspects of Szilard's contributions in this early stage of preparation for the large-scale release of nuclear fission energy in a chain reaction aimed at plutonium production. The first set are concerned with the measurement of basic nuclear constants and their application to the design of the nuclear reactor; the second represent engineering studies, some exploratory and some detailed, on reactor design.

IV. Consolidation:

Once the decision had been taken, to construct a plutonium production reactor at Hanford based on the graphite-uranium system with water cooling, Szilard's direct involvement slowly came to an end. The assignment of providing liaison between the project physicists and the engineers of the DuPont Company, responsible for the construction and operation of the Hanford reactors, was given to Wigner's Theoretical Division; most of Szilard's Technological Division was incorporated into the engineering team, while some of its key members drifted off to other, more immediately pressing aspects of the project. A small group of physicists, under my immediate supervision, continued to work under his direction on some of the unsolved problems of nuclear physics only indirectly associated with the reactor.

However, even though relieved of direct responsibility for the success of the project, Leo Szilard could not divest himself of his deep involvement in its success. He assumed the role of contingency planner, devising alternative approaches in the event of the failure of the water-cooled system, which was encountering unforeseen problems and delays. His favored alternatives were a He-cooled reactor, and cooling by a eutectic mixture of liquid Bi and Pb. Szilard devoted great efforts toward the detailed design of such reactors; for example, he and I developed a detailed design for a magnetic pump for liquid bismuth, based on a principle which had been invented by him and Einstein in 19   for the purpose of circulating liquid refrigerants in a system without moving parts.

In this period, Szilard was intrigued with the problem of devising the most efficient system for a nuclear power reactor. In addition to He and Liquid Bi cooling (he also foresaw the possibility of using the electromagnetic pump for the circulation of liquid Na) Szilard foretold and investigated the engineering problems associated with most of the reactor systems which have since been seriously considered or successfully utilized for power purposes -- a reactor cooled by the boiling of water; a beryllium moderated reactor; CO_2 cooling. In addition, he placed very high priority on the problem of breeding -- of designing a reactor which would produce at least one nucleus of a fissile element (U^{233} from Th; Pu^{239} from U^{238} neutron absorption) for each nucleus consumed in fission.

But more and more, convinced that we had won the race against the Germans, Szilard was concerned with the problem of using, or not using, the atomic bomb in a fashion most calculated to insure peace and stability in the postwar world. In part, his concern for more efficient power reactors was motivated by a belief that it would strengthen the hand of the United States Government, in seeking a postwar system of stringent controls over nuclear weapons, to have accumulated, in the early post-war period, a substantial store of fissile material.

And thus, to an increasing extent, in the last years of the war, Szilard's efforts were devoted to the organization of a strong body of opinion among his colleagues in the project for exerting pressures on the government against the hasty and ill-considered use of the first atomic bombs. Although these efforts failed, their failure only spurred him towards greater efforts at political action in the postwar years.

But that is another aspect of history.*

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* See, for example, Alice Smith, A Peril and a Hope, University of Chicago Press, ______

V. L'Envoie:

I could not forego this opportunity for some very personal recollections of the unique experience I had in working in close scientific collaboration with Leo Szilard during World War II.

At our first meeting, sometime early in 1941, in response to his telephoned invitation to lunch, I succumbed, with only token resistance, to his irresistably exciting invitation to join him in the attempt to produce a nuclear chain reaction.

At the time, I was in my second year of graduate studies at Columbia University, and just becoming involved in research in Rabi's molecular beam laboratory. But I had heard of, and had been intrigued by the discovery of fission -- it was in the air at Columbia -- and, more important, I was convinced that the United States would soon (should already have) become involved in the war and I was chafing for a chance to make some contribution. Fortunately, I had already hit upon a subject which I thought might be suitable for a (theoretical) thesis, and Szilard convinced me that I could continue work on this subject in my spare time and helped to arrange this with Willis Lamb, who agreed to be my thesis supervisor.*

I never knew why Szilard chose me as his assistant; I suppose he needed someone to pursue some of his myriad ideas, who would follow his instructions without too much independence, who had enough competence in theoretical techniques

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* By fortunate chance, this worked out as anticipated, and I was able to submit my thesis and receive my degree immediately after the war.

to carry out a program of computation without too much supervision, and who understood enough -- but not too much -- to pick up a partly developed idea and carry it to completion. Whatever his original expectations, I seemed to suit him well, for we worked in close collaboration until I left Chicago for Los Alamos in 1944.

Szilard was an ideal boss. He supervised and inspired, but never held me back. He encouraged independence and enthusiasm. When I became excited about the early experiments of Fermi and his group at Columbia on the uranium-graphite lattice (the first exponential pile experiments) he encouraged me to join that group and to participate in the crucial experiments which proved the feasibility of the chain reaction. By the time we moved to Chicago, I was leader of a small group which carried on measurements on fundamental nuclear processes, mostly inspired by Szilard and all followed by him with close interest.

The day I came to work for Szilard he installed me in his cluttered room in the Kings Crown Hotel, adjacent to Columbia, presented me with a 20" slide rule (which, until today, remains my favorite computer), outlined a program for improved computation of the properties of the carbon-uranium lattice which was to occupy me for months, and informed me that he had to leave town for a week or so. At that time Szilard was continuously traveling, to Washington to assure continuing support for the project, to the laboratories and factories capable of producing pure graphite and uranium metal to goad and cajole their adoption of programs for the large-scale production of these vital materials even in the absence of governmental assurance of support of these programs. This was the critical period of preparation for the chain reaction, and Szilard's finger was in every pie. But he did not need to be around continually in order to supply ideas and inspiration enough to occupy all the efforts of more than one eager assistant.

Characteristically, Szilard spoke very little of his earlier life and pre-war activities. Only little by little, in bits and pieces, did I learn of his work in Berlin with von Laue and Einstein -- mostly during an automobile trip from New York to Princeton on an occasion when he wanted to consult with Einstein, and on which he thoughtfully took the opportunity to introduce me to the great man. His foresight in leaving Germany immediately after Hitler's siezure of power, he explained away with the comment that it only had required a short airplane trip from Berlin to London, passing over that impossible jumble of frontiers and unstable political entities, to convince him that the Europe of 1932 was no place in which a sane man could stake his future. My interest in Chinese poetry -- I was reading a book of translations by Arthur Waley -- occasioned some fragmentary anecdotes concerning his acquaintance with Waley during the formation in England of a Committee to aid the exodus and placement in British Universities of young German-Jewish scholars, a project, as I learned later, for whose conception and execution Szilard was primarily responsible. And despite our very close collaboration during the period 1941-1944, I never heard directly from him of his difficulties with the narrow-minded and unimaginative higher authorities of the Manhattan Project, who could neither understand nor appreciate his independence and unconventionality. His method of teaching and guidance was by leadership and example -- never by resort to preachment or authority.

After the success of the plutonium project was assured, and I departed with his blessing to work on the next stage of the project, we corresponded only occasionally, on aspects of the physics problems with which we had been mutually concerned. But I saw him from time to time, when project business brought me to Chicago, and I could not help but be infected with his deep concerns over the consequences of the forces he had been so instrumental in releasing.

After the war his scientific interests turned to biology, but on our occasional meetings he exhibited the same keen and penetrating interest in the problems of particle physics, in which I had become engaged, as he had in the problems of nuclear physics on which we had worked together. And he always maintained a deeply human, if apparently impersonal, interest in me as an individual.

More and more after 1946 our mutual concerns converged on the problems of controlling the nuclear geni. And so, in the last years of his life, I again became his assistant in the enterprise of building a more livable world, the ideal which guided all of Leo Szilard's enterprises from his earliest days until his death.

VI. Appendix

There follows a list of the reports, memoranda, letters and other documents which are listed in the retrospective files of the Manhattan project, with brief summaries of the contents of some of the more important of them. Since Szilard was habituated to memoranda, which recorded his ideas and inventions, many of which never went beyond his own files, this list is of necessity incomplete. But merely listing them provides a striking demonstration of the breadth of vision and versatility of this remarkable man.

lists