NOTES ON SZILARD'S THERMODYNAMIC PAPERS

The two papers:

I: Über die Ausdehnung der phänomenologschen Thermodynamik auf die Schwankungserscheinungen,

II: Über die Entropieverminderung in einem thermodynamischen System bei Eingriffen intelligenter Wesen,

are closely interrelated, though each makes a distinct and highly original contribution to thermodynamic theory. These notes will try to place these papers against the background of both earlier and later contributions by others.

There are really two thermodynamic theories: the phenomenological and the statistical or atomistic. They were constructed simultaneously and largely by the same men. The phenomenological theory starts from the principle that heat is energy, and always flows from hot to cold regions. By ingenious and abstract arguments, many diverse phenomena are brought under this principle, and remarkable conclusions are reached. An abstract concept of entropy is developed, and the basic principle is reformulated by Clausius as

<u>The energy of an isolated system remains constant, and its entropy always increases</u> -- or, in ideal cases, remains constant.

The abstract elegance of this theory, together with its great predictive power, has impressed many scientists, including Max Planck. As he repeatedly stressed, one will not readily accept any theory unless it is demonstrably in accord with Clausius' principle.

On the other hand, its abstractness has led to the search for alternatives, based on hypotheses concerning the interactions of the atoms that constitute all matter. Heat is then not an abstractly defined form of energy, but the random motion of the atoms. Entropy is related, according to Boltzmann, to the probability of a given state of motion of the atoms, and Clausius' principle is seen as a consequence of the tendency of a complex system to reach its most probable state.

Attractive as this seems at first, by the beginning of the twentieth century many had recognized that it merely replaces one abstraction, entropy, by an equally difficult one, probability. Moreover, generality is lost, since the Boltzmann principle is founded upon an atomic model. Hence Planck and others who were actively questioning the applicability of Newtonian mechanics to structures of atomic size, were always careful to integrate their results with the phenomenological thermodynamics.

Probabilistic thermodynamics differs from the phenomenological theory in one important respect. The latter considers thermodynamic equilibrium

as static, a state of no change. In the atomistic theory, equilibrium is a state of motion. The modern art form of the mobile illustrates this concept: after a mobile has been set in motion and left to itself for a time, its constantly fluctuating configurations illustrate thermodynamic equilibrium.

These fluctuations can also be observed in more interesting circumstances. The botanist R. Brown discovered that the small particles of a colloidal suspension are in constant irregular motion. Einstein[1] developed a theory of this as an example of thermal agitation, and Perrin[2] used his equations to obtain good experimental values of Avogadro's number and of Boltzmann's constant. Smoluchowski[3] studied the thermal fluctuations in density, and W. Schottky[4], the thermal agitation of electrons in conductors. Today, numerous other examples are known.

While Szilard was a graduate student at the University of Berlin, these phenomena were under active discussion. Perrin and others cited them as "proof" that atoms "really" exist. It is therefore not surprising that they engaged Szilard's attention. In the above mentioned paper I, he showed that these fluctuations can be included within the framework of the phenomenological theory without making any reference to atomic models. This surprised many, including Einstein and v. Laue; it is not surprising that the latter readily accepted the paper as satisfactory for a Ph. D. thesis.

In an interview recorded in 1963, Szilard mentions that this paper was written during the Christmas recess of 1921, though it was not published

until 1925. He also records that the paper II was written about six months later; it was not published until 1929. It was accepted as his Habilitationschrift in 1925, when he was appointed Privatdozent at the University of Berlin.

The second paper concerns the paradox invented by Maxwell. Maxwell[5] imagined

"a being whose faculties are so sharpened that he can follow every molecule in his course, and would be able to do what is at present impossible to us ... Let us suppose that a vessel is divided into two portions A & B by a division in which there is a small hole, and that a being <u>who can see the individual molecules</u> opens and closes this hole, so as to allow only the swifter molecules to pass from A to B, and only the slower ones to pass from B to A. He will, thus, without expenditure of work raise the temperature of B and lower that of A, in contradiction to the second law of thermodynamics."

Smoluchowski was, perhaps, the first to recognize the fallacy in this conclusion. The being (or demon) must be considered as a part of the system--otherwise it is not isolated. Moreover, the demon's metabolism must be included among the processes that occur in it. Obvious as this may seem today, forty years elapsed before it was explicitly noted. In the 1920's, this resolution of the paradox still did not immediately convince many physicists.

Szilard seeks for the essential mode of interaction that enables the demon to decrease the entropy of the remainder of the system. He finds it in "a kind of memory", which is inherent in "measurement". Then he concludes that ignorance of the metabolism of the memory process need be no hindrance to his project. One can imagine inanimate mechanisms that have this ability to remember. Their "metabolism", being simpler, can be subjected to detailed analysis--and this is the subject of the paper. He postulates that any diminution of entropy in other parts of the system will be compensated by an increase in that of the mechanism. Then this postulate is verified in an example.

During the same decade in which Szilard published these ideas, an apparently unrelated theoretical development began. Communication engineers needed a precise definition of "information". This started with the general notion that information is the commodity that telegraph companies transport from one place to another. The more refined definitions were developed by H. Nyquist[6], R. V. L. Hartley[7] and others. This lead to the recognition that messages with a high probability of occurrence (such as conventional greetings) convey little information.

Perhaps John v. Neumann (whose friendship with Szilard dated from their days at the University of Berlin) was the first to recognize this intrusion of probabilistic ideas into information theory. This connects that theory with

thermodynamics. Moreover, there is a strong analogy between Szilard's formulae for the entropy generated by the demon, and those of Nyquist and Hartley for information. N. Wiener[8] had also proposed similar formulae.

This was soon seen to be of fundamental significance. C. E. Shannon[9] then established the complete and detailed relation between information and entropy.

This was followed by a whole genre of papers[10] similar to Szilard's. It is also significant that the inanimate memory imagined in II has been realized in the modern computer--the "thinking machine".

By this time, Szilard was occupied with other matters. However, in his last paper, "On Memory and Recall", he returned to the metabolism of memory, and shows that he intended to take an active part in the study of these processes in living organisms.

Carl Eckart
May 12, 1967

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Bernie: Leo always sent this out c̄ his reprints — because it explained things better than his paper to non-mathemat.

THE NEW SCIENTIST, 12 FEBRUARY 1959

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Bio 9

A theory of how we age

from JOHN LEAR, our American correspondent

AGEING is plainly a matter of arithmetic. We add one year to another until our time expires. Everyone knows this, and insurance salesmen earn a pleasant living by averaging out the figures and cajoling the rest of us into wagering where we as individuals fit into the scheme. But only a very imaginative man would think of systematizing these mathematics into one equation to comprehend the interval between the cradle and the grave. In all of American science, there are few minds creatively abundant enough to put such a thought into action if the idea did strike. One of these rare intellects graces the disarming corpulence of Dr. Leo Szilard, who has used it to work out mankind's first scientific theory of how we grow old.

It seems to me inevitable that this latest of the Hungarian-born theorist's long line of brilliances will in time be recognized as a major contribution to human thought. It appears, however, with almost innocuous modesty in the pages of the January, 1959, *Proceedings of the National Academy of Sciences*. A dry and frugal footnote tells us that the work was done while Dr. Szilard—ordinarily occupied with the affairs of the University of Chicago's Enrico Fermi Institute of Nuclear Studies—was serving as a consultant to the basic research programme of the National Institute of Mental Health at Bethesda, Maryland. The N I M H is, of course, keenly conscious of the relationship between advancing age and mental illness at a time when the population of this country is ageing markedly.

"This paper represents an attempt to describe a hypothetical biological process that could account for the phenomenon of ageing", the introductory sentence says. "Ageing manifests itself in much the same general manner in all mammals, and we are in a position to learn enough about the ageing of mammals to be able to test the validity of a theory that leads to predictions of a quantitative kind—as does the theory here presented."

Dr. Szilard's thinking takes off from the fact that the basic hereditary unit in our makeup, the gene, can be responsible for the synthesis of a specific protein molecule, and that this molecule in many instances has a specific catalytic function in one of the chemical processes of the body. When the gene mutates, the change renders the gene incompetent to bring about the process for which it ordinarily would be responsible. When the bundle of changes inside us grows sufficiently large, we are incapacitated, we are crippled, and ultimately we die.

How do we begin to die? Just where does the mysterious stroke of death originate? The theory assumes not one all-powerful, felling blow but a series of minor hits. The hits occur at random, with a speed that remains constant from the moment we are born. One hit disables one chromosome, to the extent of knocking out one of the working partners of a pair of genes harnessed to a given biological task. The chromosome continues to function, however, until the second partner also suffers a hit.

Each of us possesses altogether about 15,000 genes. The Szilard theory assumes that only 3,000 of these are important to the healthy life of an adult. To distinguish these effective operatives from all the other genes, the 3,000 are dubbed "vegetative" genes and the mutants of these "vegetatives" are called "faults".

If this were the whole story, the variation in the length of our lives would depend entirely on accidents, murders and wars. But this is not the whole story. The whole story begins generations before we are conceived. It is handed down in the genes of our forebears. By the time the chromosomes containing the 3,000 "vegetative" genes reach us, a certain number of hereditary "faults" have already occurred. In terms of age, it is as though we have already been scraped by the edge of Father Time's scythe before we emerge from the womb. Some of us are born relatively old people.

Whatever individual age one of us has at the beginning of his own phase of the eternal process known as life, that age increases progressively with the number of "vegetative" genes that are disabled by the mysterious hits of time against the species. The progression is not entirely steady, for the really "old" newborn die off rapidly in the first year of existence and then at a slower rate to the age of ten years. After that time, inherited "faults" increase the death rate "only in conjunction with the hits of time, and they increase it appreciably only above 40 (years of age)". From then on, the surviving fraction of vital genes "decreases with age at an accelerating rate".

Here Dr. Szilard begins his ageing equation by writing the symbol "f" to represent the surviving fraction of genes. When "f" reaches a certain critical value, symbolized as "f*", "the individual . . . dies . . . within the year".

"Thus, in its crudest form," Dr Szilard notes, "the theory postulates that the age at death is uniquely determined by the genetic makeup of the individual."

This, however he adds at once, "cannot be strictly true, for, if it were true, identical twins would die within one year of each other". And the fact is that female identical twins die at differing intervals averaging out to three and a half years. So the reasoning has been refined to account for the variance in manifold ways, including the shielding effects of environmental conditions "prevailing at present in the United States, where essentially no adult dies for lack of food or shelter and no adult has a reduced propensity to procreate because of his inability to provide food or shelter for his offspring".

In underpinning his theory mathematically, Dr. Szilard takes a hypothetical, genetically perfect, white female and designates her age of death as the "lifespan of the (human) species". He finds that she, at 50 years of age, would have the same physiological age as today's average woman of 35 years. Her most probable age of death would be 92 years, twelve years greater than the age at which the average woman dies today.

The Szilard equation encompasses other women by postulating a basic time interval of the ageing process and defining this interval as the difference between the life-expectancy of the genetically perfect woman and a woman whose genetic makeup includes one "fault".

Dr. Szilard stipulates that genes "vegetatively" incapacitated by the hits of age are not thus rendered impotent in their power to duplicate themselves in future generations.

Experiments are now under way in mice to provide supporting evidence for the ageing theory, mice being better than hamsters or dogs for the purpose because the number of their chromosome pairs is closer to the number of man's.

1/20/67

Graduate School
INSTITUTE OF
MOLECULAR BIOLOGY

UNIVERSITY OF OREGON

EUGENE, OREGON 97403

Dear Trude,

It was very good to see you, and particularly good to see you in fine spirit. I look forward to my next visit. Meanwhile please do call me if I can be of any help with anything.

I enclose a xerox of Delbruck's Christmas card as I promised.

Love,

Aaron

LEO SZILARD

Published Papers in Biology

1949 - 1964

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Please return to GWS

Aaron Novick

The great sense of adventure so typical of everything Leo Szilard did is also seen in his work in biology. For many years he had been interested in a wide range of problems in biology and medicine, but he was preoccupied in the thirties and early forties by the developments in physics whose awesome implications for mankind he so clearly foresaw. Although the political consequences of these developments continued to occupy a large fraction of his attention until his death, he managed after the war to turn all of his scientific energies toward understanding the mysteries of biology.

In his characteristic way he sensed that something exciting was going to happen in biology and that it would occur through the study of viruses and bacteria. He decided to set up a laboratory at the University of Chicago where President Hutchins had appointed him professor jointly in biology and sociology. One Spring evening in 1947 after a meeting of the Atomic Scientists of Chicago, while we walked together through the campus, Leo asked me whether I would be interested in venturing into biology with him, having decided to seek the collaboration of a younger colleague. Although he cautioned me to consider this carefully and pointed out many risks, I knew this was precisely the chance for adventure in science I had always dreamed of. He said that he would try to find some kind of position for me in the University and that meanwhile we should both take a three-week summer course in bacterial viruses which Max Delbruck had organized at Cold Spring Harbor.

To our great pleasure we found that the course gave stimulating entry to a new and exciting field in which the participants were held to very high standards by Delbruck's leadership. The clarity of the ideas being developed and the

solidity of the experimental information made it possible for newcomers like us to engage in interesting and useful research following this brief course.

One of the interesting people we met at Cold Spring Harbor was Jacques Monod, who was passing through on his way from a symposium. Leo had been fascinated by Monod's observations published in his thesis that bacteria, offered the two sugars glucose and lactose at once, would consume the glucose preferentially and utilize the lactose only after the glucose was exhausted. While discussing this with Monod, Leo suggested a scheme which he argued would oblige the bacteria to use both sugars at once, unless for some unknown reason use of one necessarily excluded use of the other. In this scheme he proposed to grow the bacteria in a continuous culture device into which glucose and lactose were to be fed continuously. He anticipated that the concentration of glucose in the growth vessel would fall as the bacteria consumed it to some point where the bacteria would begin to use the lactose. As noted below, this idea led to his discovery of the chemostat principle the following summer.

We had hardly returned to Chicago, where we had not yet set up a laboratory, when Leo suggested that we go back to Cold Spring Harbor to do some work. I took leave from my job at the Argonne Laboratory and back we went. In a few weeks we found ourselves doing experiments intended to clarify the mechanism of mating in phage which Delbruck had just discovered. But we had time only for a primitive beginning.

In January of 1948 we started our own laboratory in the basement of the former small synogogue of a Jewish orphanage whose buildings had been taken over by the University of Chicago.

Leo had been unsuccessful in securing a regular University appointment for me so I was employed as a research associate on a research grant he had obtained.

Subsequently I did obtain an appointment, but during all this period he was unhappy about my relatively low salary and insisted upon supplementing it out of his own pocket, although he too was astonishingly poorly paid. ¶ One of the first things we did was to look into the mechanism of the curious phenomenon of photoreactivation -- the reversal of the lethal effect of ultra-violet light by irradiation with visible light--which had just been discovered by Kelner for bacteria and Dulbecco for phage. Our conclusion, that UV produced chemical changes ("poison") which account for the lethal effect and are destroyed through the action of visible light, was a correct anticipation of present knowledge of this phenomenon.

Leo was disturbed by the fact that Delbruck and Luria hesitated to accept Lederberg's discovery of genetic recombination in bacteria. We designed what we felt should be a convincing experiment. When we completed these studies Leo reported the results to Delbruck and to Luria in a letter in which he offered to eat his hat if this was not genetic recombination. As I recall, Luria was convinced by our findings but Delbruck remained skeptical, urging us to continue our studies. But we discovered that Lederberg had already performed an equivalent experiment whose results were contained in a table in one of his papers which typically had far more information than was seen by the usual reader.

Leo was always fascinated by paradoxes and was struck by one reported by Delbruck in his studies of genetic recombination in phage. Study of this mystery led us to the discovery of phenotypic mixing in viruses and to the realization that the genetic component of a virus is distinct from its phenotype. Hershey's subsequent brilliant demonstration of this separability and his identification of DNA as solely responsible for the genetic component was, of course, one of the dramatic discoveries of molecular biology.

During the summer of 1948 we took C. B. Van Niel's delightful course in microbiology at Pacific Grove, which again was a course set up to educate a heterogenous group, generally including distinguished scientists as well as Stanford undergraduates. While there, Leo again thought about the continuous culture of bacteria. He had been annoyed by the fact that to get the actively growing bacteria required for most phage experiments, it was necessary to inoculate a culture two and one-half hours earlier. Ideas for experiments occurred to him so quickly and so urgently that a delay this long was intolerable to him. A continuous culture system he proposed would provide an ever present source of growing bacteria. In thinking about the operation of such a system and perhaps recalling the idea he had proposed to Monod, he discovered the chemostat principle. In great excitement he described to me his realization that in a continuous culture system a stable state should be achieved if the turn-over rate in the growth flask was less than the maximum bacterial growth rate. The bacteria eventually ought to grow at a rate equal to the turn-over rate. In this state the concentration of the chemical factor limiting the bacterial growth rate would be set at a constant concentration by the bacteria themselves. So he proposed to call the apparatus based on this principle the "chemostat".

On our return to Chicago at the end of the summer we quickly verified the chemostat principle and designed a suitable apparatus. Nevertheless we were refused an NIH grant on the grounds that it would not work. Later the grant was made, with apologies Although we never used the chemostat as a source of bacteria for phage experiments, nor until much later for forcing bacteria to consume simultaneously glucose and lactose, we did use it for a series of genetic and physiological studies, some of which led to interesting discoveries.

We saw that observation of the accumulation of mutant bacteria in a population in the chemostat would give a far more accurate measure of rate of mutation than could be obtained with existing methods. Our discovery that mutation seemed to occur at a constant rate per hour independent of the growth rate of the bacteria drew considerable attention but did not provide the useful insights we wanted. We did find that a number of nontoxic compounds like caffeine increased mutation rates sharply and that some of the purine nucleosides had the novel contrary effect, they decreased rates of mutation whether spontaneous or induced by the caffeine-type mutagens. We came to realize that mutation rate depended on the chemical environment in the cell. Observation of the accumulation of mutants for longer periods of time led to the discovery of the phenomenon of evolution in the chemostat. Here was, in a sense, evolution in a controlled laboratory system.

The chemostat turned out to be very useful for the study of control. mechanisms. By accident we observed that a tryptophan-requiring mutant excreted large quantities of a tryptophan-precursor when growing at very low tryptophan concentrations. Further we found that a sudden raising of the concentration of tryptophan immediately stopped the synthesis of this precursor. Clearly the inhibitory effect of tryptophan had to be on the activity of the enzymes in the tryptophan pathway rather than on the formation of the enzymes. Extension of these studies to an amino acid not required by the bacteria (arginine) showed again that an amino acid could block its own synthesis by interfering with the activity of the biosynthetic enzymes. We did not know that it was only the first enzyme that was affected, as Umbarger subsequently showed, but it was clear from our experiments that the inhibitory effect could not simply be a damming up of the system by mass action.

I can recall so many ideas Leo did not develop, mostly for lack of time or because of bad luck. Quite early he saw how useful it would be to be able to transfer by some kind of printing process all of the colonies on one plate to another. He built plates holding many needles - like a flat porcupine - but we made only limited use of them. Lederberg's ingenious independent development of this technique using velveteen and his applications of it showed how powerful it is and made museum pieces out of Leo's plates.

Another powerful idea which we tried to develop with not much success was to find a system where one could select a rare mutant and an equally rare reverse mutation to wild type. Seymour Benzer's discovery of the r_{II} system in phage where this can be done and his brilliant application of the techniques to improve our understanding of the structure and function of the phage chromosome, show how great was Leo's insight into what is important.

Our active collaboration came to an end in the summer of 1953, when I went to the Institut Pasteur for a year and Leo turned his energies toward politics and the threats facing mankind. Our friendship continued, of course, and we frequently sought each other out for the exchange of ideas.

Although he no longer had a laboratory and although he spent increasingly greater amounts of time trying to save the world, as he put it, he was still able to be very productive in biology. Among his attempts to save mankind were inventions of many ingenious birth control devices. One astute idea was to solve the psychological problems presented by sterilization through the preservation of sperm obtained prior to the sterilization operation. He abandoned these researches when he learned that the problem had just been solved in England for bull sperm and the method was coming into commercial use.

In his typical fashion he continued to seek out the most active areas of biology and to be totally generous with his ideas. So often as was the case in physics great discoveries seemed to follow soon after his visits to a laboratory.

During this later period he wrote a number of theoretical papers. The phenomenon of ageing had long fascinated him. At one point he considered a population of bacteria in the chemostat as a model of an ageing system in that in time the fraction of cells which were mutant would increase. But this idea was discarded when we discovered that in a growing population evolution invariably occurred, i. e., a population and its accumulated mutants eventually become replaced by a new population. He remained convinced by the basic idea and prepared the theoretical papers on ageing and on the effect of paternal age on sex ratios at birth.

Leo was particularly interested in the mechanism by means of which the synthesis of enzymes in cells is controlled. Here for him was not only the key for understanding the astonishing ability of mammals and many other vertebrates to recognize foreign proteins and to make specific protein antibodies to inactivate them but also the ultimate problem of differentiation of cells seen in multicellular organisms. Thus, he wrote the two theoretical papers on control of enzyme formation and on antibody formation.

In his last work he turned to the problem which in a sense began his career when, as a physicist, he raised the question whether the brain violated the laws of thermodynamics. Now he sought to understand how the brain could "learn." With his typical great simplicity he applied the ideas he had developed for the control of enzyme formation and antibody synthesis to describe how a neuronal system could learn. It is too early to tell how close to the truth he had reached, but from what has been discovered since his death and from his incredible record I would give odds that he was right.

Novick
Biology

Proceedings of the National Academy of Sciences, 35:591-600, (Oct.) 1949

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By Leo Szilard

(Communicated April 27, 1964)

*Posthumous

LEO SZILARD

Published Papers in Biology

1949 - 1964

Introductory Essays

~~I. Jacques Monod~~

II. Aaron Novick

Revised version (3rd)

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LEO SZILARD: NUCLEAR PHYSICS PAPERS

By Maurice Goldhaber

In this section, I shall deal with Leo Szilard's publications in the field of nuclear physics which stem from his brief but historically important "English period." Since I was partly responsible for fanning his interest in nuclear physics at that time, I am glad to have been asked to introduce his work in this field.

Leo Szilard had wide interests; few fields of human endeavor escaped his enquiring mind. When confronted with a problem, he tried to isolate its essential core, and especially this was so with scientific problems. In contemplating the life of a person with such a fertile mind, it is of interest to see what problems he found important enough to pursue to completion. This is best shown in his published papers. His published scientific papers are few in number. In each one he tried to make an important point, to prove something of general interest and consequence.

Let us consider here Szilard's contributions to nuclear physics and chemistry. His first experiments in these fields were carried out in London. He had come there early in 1933 from Berlin, where I had met him while he was Privatdozent and I was a student. I was struck by the way he would talk to young people without stuffiness or condescension. I came to England in the spring of that year and entered Cambridge University in the fall as a research student. I saw him often during his years in England, mostly in London and occasionally in Cambridge or Oxford. We usually discussed progress in nuclear physics and the "state of the world." It is not widely appreciated that Szilard, who started out to be a theoretical physicist, had great powers of observation as his two "accidental"

discoveries discussed below illustrate: one of the first examples of an artificially produced nuclear isomer (in indium) and, independent of Fermi and Amaldi, the first observation of "neutron resonances."

~~Physics 5~~ - Chemical Separation of the Radioactive Element from its Bombarded Isotope in the Fermi Effect
by Leo Szilard and T. A. Chalmers
Nature, 134:462 (Sept. 22) 1934

The discovery described in this paper has become known as the "Szilard-Chalmers effect." Its genesis is typical of Szilard's way of thinking and acting. He was at that time in London. He was contemplating (he told me once) the possibility that some of the neutron-produced radioactive isotopes discovered by Fermi and his collaborators might make medically useful compounds. However, Szilard realized immediately that chemical compounds exposed to neutrons would be disrupted by neutron capture. Characteristically, he turned this apparent defeat around which led him to a brilliantly simple method of isotope separation . At that time any isotope separation was still a major feat. He therefore proposed to a young physicist, T. A. Chalmers, who was working at St. Bartholomew's Hospital, that they should investigate the possibility of concentrating I^{128} produced by an (n,γ) reaction in an iodine compound. They started with ethyl iodide and precipitated the "free" radioactive iodine, which does not exchange with the iodine in the ethyl iodide, as silver iodide. This method is now widely used. Its mechanism has been widely studied and there are many variations in the means of production of the radioactive isotope [e.g., (γ,n) reactions, conversion electrons accompanying isomeric transitions, etc.]. There is some retention of the radioactive isotope in the starting compound under certain conditions, as well as exchange of the recoil atoms; this allows promising medical applications, and thus comes back to Szilard's original hope.

~~Physics 6.~~ - Detection of Neutrons Liberated from Beryllium by Gamma Rays: A New Technique for Inducing Radioactivity
by Leo Szilard and T. A. Chalmers
Nature, 134:494-495 (Sept. 29) 1934

Soon after the observation of photo-protons from the photo-disintegration of the deuteron was reported, Szilard and Chalmers used their newly developed method of concentrating radioactive iodine as a sensitive way of detecting photo-neutrons from beryllium disintegrated by radium γ rays. These photo-neutrons have turned out to be a very useful source of fairly low energy neutrons (a few hundred keV). They are produced by γ rays above the (γ,n) threshold of Be^9, which has the lowest known threshold for stable isotopes. A Ra-γ-Be photo-neutron source was often used by Szilard in his later researches, particularly in fission experiments.

~~Physics 7.~~ - Liberation of Neutrons from Beryllium by X-Rays: Radioactivity Induced by Means of Electron Tubes
by A. Brasch, F. Lange, A. Waly, T. E. Banks, T. A. Chalmers, Leo Szilard, F. L. Hopwood
Nature, 134:880 (Dec. 8) 1934

This paper well illustrates Szilard's mode of research--ahead of his time: If γ rays, at that time available in very limited sources, can disintegrate beryllium, why not use powerful high energy X ray sources? The only source of this kind well known to him was at Berlin, but distance was no barrier to him; nowadays researchers take this for granted. He got in touch with his former colleagues there and arranged to have bromoform exposed to photo-neutrons produced from beryllium by X rays with energies exceeding 1.5 MeV. The bromoform was flown to London where the radio-active Br was separated by a "Szilard-Chalmers" separation. A weak activity decaying with a 6-hour period was observed. (We now know that

slow neutrons induce in Br activities of 4.5 h and 35.3 h half life besides shorter lived activities, another early example of nuclear isomers.) The question of whether a sharp energy threshold exists is raised in this paper. Szilard has recounted the fact that at that time (because of a mistake in the measurement of the Be^9 mass) it was not certain that a sharp threshold would exist, and that Be^9 might be metastable. This led him to the first speculations on nuclear chain reactions. The threshold for the photo-disintegration is now known to be 1.66 MeV.

~~Physics 6~~ - Radioactivity Induced by Neutrons
by Leo Szilard and T. A. Chalmers
Nature, 135:98 (Jan. 19) 1935

Here Szilard and Chalmers report an interesting observation in indium exposed to neutrons. They found a new period which they estimated to have a 3½ h half-life (now known to be 4.4 h). This period had been overlooked by Fermi's group (who had found half-lives of 13 sec and 54 min), and Szilard and Chalmers correctly concluded that "one of the two indium isotopes is activated with more than one period." They realized that this could not be due to one of the processes already established by Fermi and his collaborators, since the new activity was not "water sensitive", and since In seemed too heavy an element to undergo (n,p) or (n,α) reactions with Rn-α-Be neutrons. Their "Letter" ended characteristically with the remark that their findings "seem to deserve further investigation, for which adequate instruments of observation are not at present at our disposal."

Thus ended the short but exceedingly fruitful collaboration between Szilard and Chalmers. Decades later, Chalmers told me how fondly he remembered those days.

~~Physics 9~~ - Absorption of Residual Neutrons
by Leo Szilard
Nature, 136:950-951 (Dec. 14) 1935

In 1935, at the invitation of Professor Lindemann (later Lord Cherwell), Szilard moved to Oxford, where he enjoyed the hospitality of the Clarendon Laboratory. The work on "Absorption of Residual Neutrons" appears to be the only experiment which Szilard carried out without a collaborator. He reports the discovery that a slow neutron beam, filtered by cadmium and detected through the 54 min activity in indium, is selectively absorbed by indium. He observed a similar effect in iodine and correctly concluded "that some elements have fairly sharp regions of strong absorption in an energy region for which cadmium is transparent." To carry out these experiments cleanly he used a "paraffin wax tube of 13 cm inner diameter and 20 to 40 cm length" to lead the neutrons from a radon-beryllium source to the detector. I have found this invention quite useful for researches in which the fast neutron background had to be reduced, and "descendants" of this invention are now used at many reactors. The discovery of "residual neutrons" (resonance neutrons) was made independently also by Amaldi and Fermi. These observations proved of great importance in stimulating the development of the Breit-Wigner formula and Niels Bohr's compound model of the nucleus.

~~Physics 10~~ - Gamma Rays Excited by Capture of Neutrons
by J. H. E. Griffiths and Leo Szilard
Nature, 139:323-324 (Feb. 20) 1937

By making use of a paraffin tube filled with lead to reduce both fast neutron and γ-ray background from a radon-beryllium source, Griffiths and Szilard made some interesting observations on neutron-capture γ rays. This is still a very active field of research. They found that the effect produced in a γ-ray counter does not differ much, per neutron captured, for a great variety of elements. One deviation from the "normal" effect they correctly interpreted as due to an undiscovered long-lived activity in chlorine (now known to be the 3×10^5-year activity in Cl^{36} produced with an (n,γ) cross section of 44 barns in Cl^{35}). Other deviations they ascribed to strongly absorbing impurities. They remarked: "The values obtained indicate that more than seven quanta are emitted from cadmium per captured neutron, but this result requires confirmation by an independent method which is now being attempted." I do not remember hearing about this method, and Szilard appears to have become interested in other things. The average number of quanta per neutron captured is now known to be about three to four.

~~Physics 11~~ - Radioactivity Induced by Nuclear Excitation:
I. Excitation by Neutrons
by M. Goldhaber, R. D. Hill, and Leo Szilard
Physical Review, 55:47-49 (Jan. 1) 1939

While Szilard was at Oxford, he and I often discussed the puzzling "extra" activity in In which he and Chalmers had observed. Sometime during 1937 we decided to collaborate on a systematic investigation of this activity, and R. D. Hill, who had come to the Cavendish Laboratory as a research student, joined us in these experiments. In December 1937

Szilard left England for the United States. Before leaving he came up to Cambridge for a farewell dinner with me at Magdalene College. At dinner he met I. A. Richards (of Basic English fame) and learned that Richards was going to the United States on a visit. Szilard at that time was rather pessimistic about the future of Europe and quipped to Richards that he had better buy a one-way ticket. (Later Richards became a Harvard Professor.) After Szilard left, Hill and I continued to investigate the indium activity until the spring of 1938. At that time I visited the United States for the first time to see this country and science here for myself. Szilard met me at the boat and introduced me to New York. During the Washington meeting of the American Physical Society, Professor F. W. Loomis, Chairman of the Physics Department of the University of Illinois, offered me a position as Assistant Professor and Szilard strongly advised me to look into this carefully. After a visit to Urbana-Champaign, I accepted the position, returned to England and immigrated to this country in time for the academic year in the fall of 1938. I arrived at Illinois shortly before the Munich crisis. Szilard spent the time of the crisis mostly in my new apartment in Champaign and we listened to the news on a radio which I had bought for this purpose. Hill had come along with me to Illinois and together with Szilard we wrote up our results on the indium activity.

We could show that this activity was due to a new process: the excitation of In^{115} by inelastic neutron scattering into an isomeric state. It was later shown that this isomeric state can be excited by many types of radiation (γ, p, α, e, etc.), and many other examples of such an isomeric activity have since been found. In^{115m} was probably the first well understood isomer, and thus the observation of Szilard and Chalmers turned out to be very important for the development of systematic isomer research.

LEO SZILARD

Published Papers in Physics

C. Nuclear Physics

1934 - 1939

Introductory Essay by Maurice Goldhaber

LEO SZILARD

Published Papers in Physics

B. Experimental work with X-rays in crystals

1925 - 1926

[No introduction]

Bernie: is there a connection between these two papers and the patents of the same period ??
German, U.S. and Holland
re Discharge tubes

LEO SZILARD

Published Papers in Physics

1925 - 1939

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Von L. Szilard

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* Posthumous

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by Dr. Leo Szilard and T. A. Chalmers

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DETECTION OF NEUTRONS LIBERATED FROM BERYLLIUM BY GAMMA RAYS: A NEW TECHNIQUE FOR INDUCING RADIOACTIVITY

by Dr. Leo Szilard and T. A. Chalmers

Letter to the Editor

(Dated Sept.17, 1934)

Nature, 134:880 (Dec. 8) 1934

LIBERATION OF NEUTRONS FROM BERYLLIUM BY X-RAYS: RADIOACTIVITY INDUCED BY MEANS OF ELECTRON TUBES

A. Brasch
F. Lange
A. Waly
T. E. Banks
T. A. Chalmers
Leo Szilard
F. L. Hopwood

Letter to the Editor

(dated Nov.26, 1934)

Nature, 135:98 (Jan. 19) 1935

RADIOACTIVITY INDUCED BY NEUTRONS

Leo Szilard
T. A. Chalmers

Letter to the Editor

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Nature, 136:950-951 (Dec. 14) 1935

ABSORPTION OF RESIDUAL NEUTRONS

Leo Szilard

Letter to the Editor

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Nature, 139:323-324, (Feb. 20) 1937

GAMMA RAYS EXCITED BY CAPTURE OF NEUTRONS

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RADIOACTIVITY INDUCED BY NUCLEAR EXCITATION:
I. EXCITATION BY NEUTRONS

M. Goldhaber, R.D. Hill and Leo Szilard

(Received November 5, 1938)

Physical Review, 55:799-800, (Apr. 15) 1939

INSTANTANEOUS EMISSION OF FAST NEUTRONS IN THE INTERACTION OF SLOW NEUTRONS WITH URANIUM

Leo Szilard
Walter H. Zinn

Letter to the Editor

(dated March 16, 1939)

Physical Review, 56:284-286, (Aug. 1) 1939

NEUTRON PRODUCTION AND ABSORPTION IN URANIUM

H. L. Anderson, E. Fermi and Leo Szilard

(Received July 3, 1939)

Physical Review, 56:619-624, (Oct. 1) 1939

EMISSION OF NEUTRONS BY URANIUM

W. H. Zinn
Leo Szilard

(Received August 14, 1939)

LEO SZILARD

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A. Thermodynamics

1925 - 1929

Introductory Essay by Carl H. Eckart

Carl Eckart: Introduction: Please return to GWS

I

Final

NOTES ON SZILARD'S THERMODYNAMIC PAPERS

The two papers:

I: Über die Ausdehnung der phänomenologischen Thermodynamik auf die Schwankungserscheinungen,

II: Über die Entropieverminderung in einem thermodynamischen System bei Eingriffen intelligenter Wesen,

are closely interrelated, though each makes a distinct and highly original contribution to thermodynamic theory. These notes will try to place these papers against the background of both earlier and later contributions by others.

There are really two thermodynamic theories: the phenomenological and the statistical or atomistic. They were constructed simultaneously and largely by the same men. The phenomenological theory starts from the principle that heat is energy, and always flows from hot to cold regions. By ingenious and abstract arguments, many diverse phenomena are brought under this principle, and remarkable conclusions are reached. An abstract concept of entropy is developed, and the basic principle is reformulated by Clausius as

<u>The energy of an isolated system remains constant, and its entropy always increases</u> -- or, in ideal cases, remains constant.

The abstract elegance of this theory, together with its great predictive power, has impressed many scientists, including Max Planck. As he repeatedly stressed, one will not readily accept any theory unless it is demonstrably in accord with Clausius' principle.

On the other hand, its abstractness has led to the search for alternatives, based on hypotheses concerning the interactions of the atoms that constitute all matter. Heat is then not an abstractly defined form of energy, but the random motion of the atoms. Entropy is related, according to Boltzmann, to the probability of a given state of motion of the atoms, and Clausius' principle is seen as a consequence of the tendency of a complex system to reach its most probable state.

Attractive as this seems at first, by the beginning of the twentieth century many had recognized that it merely replaces one abstraction, entropy, by an equally difficult one, probability. Moreover, generality is lost, since the Boltzmann principle is founded upon an atomic model. Hence Planck and others who were actively questioning the applicability of Newtonian mechanics to structures of atomic size, were always careful to integrate their results with the phenomenological thermodynamics.

Probabilistic thermodynamics differs from the phenomenological theory in one important respect. The latter considers thermodynamic equilibrium

as static, a state of no change. In the atomistic theory, equilibrium is a state of motion. The modern art form of the mobile illustrates this concept: after a mobile has been set in motion and left to itself for a time, its constantly fluctuating configurations illustrate thermodynamic equilibrium.

These fluctuations can also be observed in more interesting circumstances. The botanist R. Brown discovered that the small particles of a colloidal suspension are in constant irregular motion. Einstein[1] developed a theory of this as an example of thermal agitation, and Perrin[2] used his equations to obtain good experimental values of Avogadro's number and of Boltzmann's constant. Smoluchowski[3] studied the thermal fluctuations in density, and W. Schottky[4], the thermal agitation of electrons in conductors. Today, numerous other examples are known.

While Szilard was a graduate student at the University of Berlin, these phenomena were under active discussion. Perrin and others cited them as "proof" that atoms "really" exist. It is therefore not surprising that they engaged Szilard's attention. In the above mentioned paper I, he showed that these fluctuations can be included within the framework of the phenomenological theory without making any reference to atomic models. This surprised many, including Einstein and v. Laue; it is not surprising that the latter readily accepted the paper as satisfactory for a Ph. D. thesis.

In an interview recorded in 1963, Szilard mentions that this paper was written during the Christmas recess of 1921, though it was not published

until 1925. He also records that the paper II was written about six months later; it was not published until 1929. It was accepted as his Habilitationschrift in 1925, when he was appointed Privatdozent at the University of Berlin.

The second paper concerns the paradox invented by Maxwell. Maxwell[5] imagined

"a being whose faculties are so sharpened that he can follow every molecule in his course, and would be able to do what is at present impossible to us ... Let us suppose that a vessel is divided into two portions A & B by a division in which there is a small hole, and that a being <u>who can see the individual molecules</u> opens and closes this hole, so as to allow only the swifter molecules to pass from A to B, and only the slower ones to pass from B to A. He will, thus, without expenditure of work raise the temperature of B and lower that of A, in contradiction to the second law of thermodynamics."

Smoluchowski was, perhaps, the first to recognize the fallacy in this conclusion. The being (or demon) must be considered as a part of the system--otherwise it is not isolated. Moreover, the demon's metabolism must be included among the processes that occur in it. Obvious as this may seem today, forty years elapsed before it was explicitly noted. In the 1920's, this resolution of the paradox still did not immediately convince many physicists.

Szilard seeks for the essential mode of interaction that enables the demon to decrease the entropy of the remainder of the system. He finds it in "a kind of memory", which is inherent in "measurement". Then he concludes that ignorance of the metabolism of the memory process need be no hindrance to his project. One can imagine inanimate mechanisms that have this ability to remember. Their "metabolism", being simpler, can be subjected to detailed analysis--and this is the subject of the paper. He postulates that any diminution of entropy in other parts of the system will be compensated by an increase in that of the mechanism. Then this postulate is verified in an example.

During the same decade in which Szilard published these ideas, an apparently unrelated theoretical development began. Communication engineers needed a precise definition of "information". This started with the general notion that information is the commodity that telegraph companies transport from one place to another. The more refined definitions were developed by H. Nyquist[6], R. V. L. Hartley[7] and others. This lead to the recognition that messages with a high probability of occurrence (such as conventional greetings) convey little information.

Perhaps John v. Neumann (whose friendship with Szilard dated from their days at the University of Berlin) was the first to recognize this intrusion of probabilistic ideas into information theory. This connects that theory with

thermodynamics. Moreover, there is a strong analogy between Szilard's formulae for the entropy generated by the demon, and those of Nyquist and Hartley for information. N. Wiener[8] had also proposed similar formulae.

This was soon seen to be of fundamental significance. C. E. Shannon[9] then established the complete and detailed relation between information and entropy.

This was followed by a whole genre of papers[10] similar to Szilard's. It is also significant that the inanimate memory imagined in II has been realized in the modern computer--the "thinking machine".

By this time, Szilard was occupied with other matters. However, in his last paper, "On Memory and Recall", he returned to the metabolism of memory, and shows that he intended to take an active part in the study of these processes in living organisms.

Carl Eckart
May 12, 1967

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Rec'd by S.W.

after Title Page

~~Proposed Format~~

~~for~~

~~VOLUME TWO~~

~~of~~

The Collected Works of Leo Szilard

SOCIAL AND POLITICAL PAPERS

Beyond Science--Toward a Livable World

PART ONE:	Szilard's "Version of the Facts"
PART TWO:	Political Issues
PART THREE:	Social Issues

August 20, 1975

PART ONE: Szilard's "Version of the Facts".

Chapter Headings

PART TWO: Political Issues

Chapter Headings

I AFTER HIROSHIMA: THE FIRST YEAR

Hiroshima Through Passing of Atomic Energy Act

Alerting the public to the meaning of atomic energy and fight for civilian control.

(August 6, 1945 to 1946)

II CALLING FOR A CRUSADE

From Passing of Atomic Energy Act through First Russian Atomic Bomb

Exploring means for international control; intensification of educational efforts directed at the public--including members of Congress--and organization of scientists for this purpose.

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III NUCLEAR ESCALATION

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This bibliography is limited to scientific and technical papers and reports. It includes all known published papers and a selection of the important unpublished work.

Papers preceded by an asterisk are printed in this volume for the first time. Some of the reports so designated, however, have had limited distribution in duplicated form.

Between the years 1940 and 1945, papers were withheld from publication and issued as classified reports by agencies of the United States Government concerned with nuclear energy during the second World War. These reports bear various code numbers. After World War II the reports quoted here were declassified and released as documents of the Atomic Energy Commission.

The bibliography of Leo Szilard's writings on subjects other than science, such as memoranda on operational and political aspects of the Manhattan Project and other general and political writings, such as his articles in the Bulletin of Atomic Scientists, as well as fiction will be presented in succeeding volumes.

KEY TO ABBREVIATIONS

Cold Spring Harbor Symp. Quant. Biol. = Cold Spring Harbor Symposia on Quantitative Biology

Phys. Rev. = Physical Review

Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences

Zeits. Physik = Zeitschrift für Physik

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Szilard letter to C. S. Wright, British Admiralty (February 26, 1936) 000

* * * * *

At various times during his life, and for various reasons, Leo Szilard thought about writing his auto-biography.

The first thought seems to have accurred ~~duringxthe~~ in 1943, after the chain reaction had been demonstrated at Stagg Field on the Campus of the University of Chicago. Szilard visited Professor Hans Bethe of Cornell University and this is the conversation he had with him : " Bethe asked Szilard how things were going .....

(9 lines.)

From unpublished manuscript "Excerpt of Voice of the Dolphins. Appendix I."

After the end of the war, in the early 1950's the thought occurred again and again and source and background materials were ca fully preserved and put together. The problem became acute ~~inxthexx~~ late in 1959 when Szilard learned that he was suffering from a very serious illness which would sharply reduce his life expectancy. Afte the initial shock, with charecterstic courage and resourcefulness, within twenty-four hours Szilard adjusted to the new situation and set up a system of priorities : after putting his personal affairs in order , he would finish the "Antibody Paper"(2), then complete the manuscript on which he had been working for the Bulletin of Atomic Scientists (3) and -"if there is still time " - he would dictate his memoirs.

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Scientific Research Institutes and programs both here and abroad,(5)(6) and finally, after he joined one of the Institute he helped to create, just a few short months before his death, in working on a theory of the Central Nervous System, which he had been incubating for many years, and which to his great joy suddenly took shape in his mind and was , as first approximation written down, in his last manuscript, submitted just five weeks before he left us. (7).

During this period of intense creativity, there was no time to look at the past - and the "Book" was never written.

At this time, therefore, we can anly , with apology and regret, present the "Companion Volume". We feel an obligation to do this however : for although most of the material in these two volumes has already been published elsewhere, it covers different fields and has appeared in so many different publications, that no one has ever seen it in its entirety. Szilard's concern with productive activities and with "the world" enviable, was so much greater than his concern about himself that there never even existed, during his life time, a comple complete bibliography. The foregoing material was therefore compiled from my archives, complemented from other sources with the kind and laborious help of many of our friends. (See acknowledgements).

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Szilard was very meticulous about the material he presented for publication, and with few exceptions - such as necessity for secrecy first self-imposed, then by regulation during the period he worked on nuclear energy - could publish anything he wanted to. He was also very selective about the quality of the thoughts which he wished to present as the very small number of his scientific publications indicates. We feel therefore very comfortable about presenting the material which he himself has selected for publication.

The transcription of the tapes and broadcasts - some of which he has never seen - had to be edited by others , and we hope very much that it has been done in a way of which he would approve. We present them,because fragmentary as they are , they contain facts and thoughts which we very much want to share with others, and above all, because we want the Voice of Szilard

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Gertrud Weiss Szilard, M.D.

(Mrs. Leo Szilard)

Mount Vernon, N.Y.
July 1965

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→ Preface 4 copies

Introduction

At various times during his life, and for various reasons, Leo Szilard thought about writing his auto-biography.

The first thought seems to have occurred in 1943, after the chain reaction had been demonstrated at Stagg Field on the Campus of the University of Chicago. Szilard visited Professor Hans Bethe of Cornell University and this is the conversation he had with him :

'Bethe asked Szilard how things were going in the Uranium Project at Chicago and Szilard said that decisions were reached in the most peculiar manner and, accordingly, the decisions reached were most peculiar. "Do you think that the project is making the wrong decisions?" Bethe asked. "Some of the decisions are wrong and some of them are right" said Szilard "but they all have one thing in common, they are all based on false premises."

"What is going on is so peculiar" Szilard went on, "that I have just about decided to keep a diary. I don't intend to publish it; I am merely going to record the facts for the information of God." "Don't you think that God knows the facts?" Bethe asked. "Yes," said Szilard "He knows the facts, but He does not know this version of the facts."' (1)

After the end of the war, in the early 1950's the thought occurred again and again, and source and background materials were carefully preserved and put together. The problem became acute late in 1959 when Szilard learned that he was suffering from a very serious illness which would sharply reduce his life expectancy (perhaps to less than a year). After the initial shock, with characteristic courage and resourcefulness, within twenty-four hours Szilard adjusted to the new situation and set up a system of priorities : after putting his personal affairs in order , he would finish the "Antibody Papers" (2), then complete the manuscript on which he had been working for the Bulletin of Atomic Scientists (3) and -"if there is still time " - he would dictate his memoirs.

(italics)

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During the last three and a half years of his life, when physical disability no longer presented a major problem, Szilard's concern with the future of mankind and with science took on renewed and intensified vigor and he pusued with relentless energy several activities which grew out of his speech given at colleges all over the country (5); he made efforts to create

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Scientific Research Institutes and programs both here and abroad, (5)(6) and finally, after he joined one of the Institute, he had helped to establish ~~create~~, just a few short months before his death, he ~~in~~ worked ~~ing~~ on a theory of the Central Nervous System, which he had been incubating for many years, and which to his great joy suddenly took shape in his mind and was , as first approximation, written down, in his last manuscript, submitted just five weeks before he left us. (7). (7)

During this period of intense creativity, there was no time to look at the past - and his "version of the facts" ~~the "Book"~~ was never written.

At this time, therefore, we can only , with apology and regret, present the "Companion Volume". We feel an obligation to do this however : for although most of the material in these two volumes has already been published elsewhere, it covers ~~xxxxxxx~~ different fields and has appeared in so many different publications, that no one has ever seen it in its entirety. Szilard's concern with productive activities and with "the world", enviably, was so much greater than his concern about himself that there never even existed, during his life time, a ~~comp~~ complete bibliography. The foregoing material was therefore compiled from my archives, complemented from other sources with the kind and laborious help of many of our friends. (See acknowledgements). We hope that this compilation is fairly complete although we cannot claim 100% at this time and may add even plus/minus 1%.

While Szilard never found time to write his Auto-Biography, he enormously enjoyed talking to people, and on such occasions he frequently reminisced. During interviews, when I saw that "the going

Material in subsequent volumes, as it comes to our attention.

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PREFACE

A volume of Szilard's Collected Works containing his scientific papers was published several years ago.[1] In the preface to that book I quoted a paragraph from a speech which Szilard made in 1953 and which we would like to repeat here:

> "When I was a young boy, I had two great interests in life; one was physics and the other politics. I kept these two interests in two compartments, and it never occurred to me that these two compartments could ever merge into one. To my interest in politics, I probably owe my life; and to my interest in physics, I owe my livelihood. I owe my life to my interest in politics because it enabled me to recognize in 1930 what was going to happen in Germany."

According to the duality which Szilard keenly perceived, this volume is devoted to the second aspect of Szilard's life, his interest and concern with people and their problems. It presents a selection of Szilard's writings on social and political matters, bringing together already published materials, scattered in many different publications--some no longer available--and a selection of articles, stories, speeches, correspondence, transcripts of tapes and broadcasts,

and other documents which have not appeaered in print so far.

Arranged in informal chronological fashion, they cover Szilard's childhood and adolescence in Budapest; the First World War when he served in the Austro-Hungarian Army; his education and the development of his career as a physicist in Berlin; his emigration to England in 1933 where he at first worked to establish a committee to find positions for scholars displaced from Germany; then became interested and very much involved in nuclear physics. And yet another emigration, his third, to America, where he became even more involved in nuclear physics which was soon to result in the liberation of nuclear energy, and thereby merging with politics. Szilard's role in the production of nuclear bombs and his fight against their use occupies a considerable portion of this volume. And after Hiroshima, there was another switch in career to the newly developing field of molecular biology, in which he participated with great enthusiasm.

And through it all, even before the Nuclear Age, innumerable schemes to "save the world" by rational means. These range from attempts, in 1930, to cultivate an intellectual elite, "der Bund"--which remained a dream--through Pugwash Movement and "Angels" Project to the establishment, in 1962, of the Council for Abolishing War. The Council continues to date, fourteen years later, as the Council for a Livable World, a patent political organization run largely by the team which Szilard selected and brought together.

30. Pimentel,

As in the Scientific Papers several of Szilard's friends and colleagues have generously ~~and with compassion~~ contributed sensitive introductory essays to some of the chapters. They have thereby made the papers come to life.

In 1940, in a semi-serious mood, Szilard formulated his own "Ten Commandments." He wrote them in German and was never satisfied with any attempts at translation. They were therefore published only in the German edition of his book The Voice of the Dolphins. (They were translated posthumously for distribution to his friends). We have inserted them here as frontispiece because they reflect Szilard's spirit like a portrait.

In looking over the assembeld materials one finds a consistent and persistent trend in the development of Szilard's thoughts and actions. In editing his writings it was difficult and sometimes impossible to avoid duplication. We recognize this and it does not trouble us too much, because indeed it is the perseverance and commitment to a purpose which characterizes Szilard and his work.

make a box like Vol 1

Gertrud Weiss Szilard, M.D.
Program in Science, Technology and Public Affairs
University of California, San Diego

Par Doc

G.W.S.

per hec

Dr. Joel Bernstein
Office of the Foreign Secretary
Nat'l Acad. of Sciences
2101 Constitution Ave ~~NW~~
Washington, D.C. ~~20037~~ 20418

Dear Joel:

Enclosed is a manuscript I have just submitted to Science on energy use in rural India. It may be of interest to some of your panels and study groups and, if so, you are welcome to make as many copies as you want and distribute them. I am also taking a few copies ~~of the~~ to the meeting of Jerry Piel's study group tomorrow.

With best regards,

Sincerely

1. The Collected Works of Leo Szilard: Scientific Papers, Bernard T. Feld and Gertrud Weiss Szilard, eds., (London, England and Cambridge, Mass.: MIT Press, 1972)

~~Ordot Ro Ten Communications:~~

Footnote 6 p. 3

1

~~Ref~~ Leo Szilard "Die Stimme der Delphine"
Wilhelm Heyne Verlag, München 1976

Omit

Another man Szilard approached was Thomas Edward Allibone, director of the High Voltage Laboratory at Metropolitan Vickers Company. According to William B. Thompson, over lunch one day --

Allibone asked Sir George [Thomson] if in 1936 he would have been prepared to give a thousand pounds for the patent rights on a scheme for slowing down neutrons by allowing them to diffuse through hydrogenous matter. Sir George's response was, "No, I don't suppose I would," and Allibone's reply was, "I didn't, when Szilard made me that offer."[1]

I - 45.6

1. W. B. Thompson, professor of physics, University of California at San Diego, to Mrs. Gertrud Szilard, March 27, 1968.

D for GWS

SOURCE NOTES:

Numbered in order of appearance.

[1] Notes, "Rough Draft, Outline for Book," June 1960.

[2] Interview taped in New York, May 1960.

[3] Slightly abridged from interview taped in Washington, D. C., 1963.

[4] "Biographical Data and List of Publications of Leo Szilard from 1922 to 1945," written by Szilard sometime after 1955.

[5] Abridged from ms, "Book. Apology (in lieu of a foreword)," probably 1951. conflicts this version of the book

[6] Draft for magazine article, dictated in 1960.

[7] Draft, Nov. 7, 1953, for a speech delivered at Brandeis, Nov. 19, 1953.

[8] Memorandum to A. H. Compton, Nov. 12, 1942.

omit [9] "Memorandum About the Einstein Letter," ~~Aug. 19, 1955.~~ Substitute April 18, 1955 Look up title

[10] Magnetic recordings made in May, 1956.

[11] Slightly abridged from interview given Mike Wallace, WNTA-TV, Feb. 27, 1961.

[12] "The Story of a Petition," July 28, 1946.

[13] Interview in New York, July 29, 1960; printed (with changes and abridgements) in U.S. News and World Report, Aug. 15, 1960, p. 68.

[14] "Answers to Questions," dictated May 9, 1963, probably a condensation of [3]

OUTLINE FOR BOOK (June 14, 1960)
to June 23

1. Childhood 1898 - 1908

2. ~~Adolescence~~ Adolescence 1908 - 1914

3. The First World War 1914 - 1920

4. Berlin 1920 - 1933

5. England 1933 - 1938

6. America 1938 through November 1940

7. America November 1940 to December 31, 1941

8. America December 31, 1941 to December 2, 1942

9. America December 2, 1942 to August 6, 1945

10. America August 6, 1945 to the passing of the Atomic Energy Act (July 26, 1946 - JWS)

7/12/67

PART I DOCUMENTS

I: 1-18 through January 1939

II: 19-52 December 1938-July 1939

III: 53-79 April 1939-December 1939

IV: 80-100 February 1940-October 1940

V: 101-109 December 1940-February 1944

VI: 110-127 August 1944-August 1945

From Spencer W. 3-23-76

FOOTNOTE FORMAT

The copy I was sent was inconsistent about the following items. We should agree on a format, preferably in consultation with the press: Commas after journal titles? I prefer e.g. Physical Review 55: 702-712 (1939), with no comma after Review. Note that colon is italicized (?). Rx Like Vol I

Book reference to "p. 59-75" or "pp. 59-75"? o.k.

Period after title of books? I prefer e.g. The New World (Washington, D.C.: USAEC, 1977), with no period after World. o.k.

This is for text as well as footnotes: should not all cabinet officers be capitalized, thus: Secretary of the Interior.

There are lots more of these points that could be picked and worried at, but these were the most obvious ones. Better to deal with them now than in proof.

Further note to Christa: please use italic *1*, not *l*, in typing italic numerals.

Picky, picky, picky. The retyping etc. was all really an excellent job, very error-free and well-organized. Regards, best regards, to all.

Spencer

Another point — I haven't been consistent about putting signatures to letters in parentheses. I suggest eliminating parentheses altogether, e.g.

John Jones (John Jones Director)	→	John Jones Director

From Spencer W. 3-23-76

THINGS STILL TO BE DONE WITH PART ONE

-- Add doct. numbers to Chapter VII.

-- Introduction to docts. to Chapter I should be re-edited if desired (Trude) and retyped in italics. (pp. 74-75)

-- Note 3 to doct. 48 (old no. 55) o.k. and note 1 to doct. 100 (old no. 112) o.k. were not typed and sent to me; please type these up.

-- Retype doct. 113. It would also be preferable to retype docts. 109, 111, and 114.

After these items are taken care of, Part One can be sent to the press. It's not the sort of copy they like to have, but they should be able to work with it. Obviously, if time allows there's a great deal of further retyping that ought to be done. Perhaps you could start with the worst pages and work your way through until time runs out.

-- Send me copies of all retyped pages. The originals are easier to work with than Xeroxes. Also send a copy of anything that is changed, however minor, except renumbering of pages. Finally, of course you'll send Xeroxes of all front matter (preface etc.).

In principle we should do the following editing, although in practice this may be left to MIT Press copy editor. I list them in case there's opportunity to take care of it, and to keep in mind while doing any retyping. (see next page)

FURTHER CORRECTIONS TO SZILARD VOL. 2 5/25/76

p. 133-35:delete note 1 (I can't identify Aschner).

p. 146: second sentence should read at end: "to be financial or otherwise unscientific."

p. 207, note 3: In the New York Times of Feb. 5, p. D9, Waldemar Kaempffert reported, "hope is revived that we may yet be able to harness the energy of the atom." A "remotely possible atomic powerhouse" was depicted.

p. 246: Restore first three lines of the missing paragraph, up to the visa matter (omit beginning "I have..."). Place four dots and footnote at the end of excerpt, thus: ....[4] have to add footnotes again

p. 247: Change note 4 to read: we omit some sentences concerning visa problems.

p. 498, note 6: John Marshall, Jr., physicist.

Our Method
Our Style

Suggested material for editorial preface SW 3/15/76

This volume lays before you Szilard's way of approaching the world's problems and his ideas towards solving them. We have not attempted a critical edition of his main works, still less a complete collection of his massive correspondence and writings. We have tried to use only a minimum of scholarly apparatus and to let Szilard himself speak.

The text consists of Szilard's own words, spoken or written, drawn from many kinds of sources. Correspondence is selected from copies he retained; he saved enough to show the full range of his private writings, at least after 1932. For speeches we use drafts he kept in his files, and for published speeches, or writings we use the final printed form rather than a draft, except as otherwise noted.

The editors' own work is confined to five categories: (1) We have written introductions and headnotes, always in italics, intended to put Szilard's words in their historical context. (2) We add footnotes, always the editors' rather than Szilard's unless otherwise noted, to explicate obscure points and to identify references and people. We identify persons by their position at the time Szilard mentions them; the reader should use the index, since names are sometimes identified only by context. (3) We make additions and grammatical replacements in the text, always in square brackets, and deletions, always marked by an ellipsis, thus: ... We silently delete the signature (typically Leo Szilard) from letters he wrote. (4) We silently correct obvious and unimportant errors in spelling and punctuation. (6) We refer the reader to documents which are printed in this volume, using italicized parentheses, thus: (Doc. 1); the table of contents shows where the documents are to be found. The only exceptions to these rules are in the tape-recorded recollections gathered in Part One, in which we have made numerous minor nonsubstantive changes to turn the loose grammar of speech into something a bit more coherent.

COMMENTS TO PART I BY GWS, FALL 1975

Page*

1 After first paragraph insert the following: "Very often it is difficult to know where one's set of values come from but I have no difficulty in tracing mine to the children's tales which my mother used to tell me. My addiction to the truth is traceable to these tales and so is my predilection for 'saving of the world.'" [1]

3 Document 01: Maturity Certificate of public school (Realiskolai) dated June 16, 1927. Could be a document or reproduced as facsimile.

7 Document 02: Record Book of Budapest Technical College (Jozsef-müegyetem). This gives the courses and names of teachers and grades for the years 1916 to 1919. This may be reproduced in facsimile. This seems important because same school attended by Wigner and I believe also Teller and von Neumann. Perhaps final certificate only would suffice.

14 This Kaiser Wilhelm Institute was called Institut für Physikalische und Elektrochemie. (Dr. Eisner, Athens)

check Duke papers

---

*Page numbers refer to final draft page numbers.

Page

16 Insert line 12 after "so I thought I would just think whatever comes to my mind," paragraph from p. 18 "I went for long walks and I saw something in the middle of the walk. . . . until I had the whole theory fully developed.[3]

17 Footnote 6. I believe that sources [3] and [14] are identical, i.e., [14] was dictated by Szilard from [3]. I excerpted the story of physics b for use by introducer in Volume One. Check whether pp. 14, 15, really come from [2].

21 Add sentence to end of first paragraph "'As long as you pray to God and ask him for something you are not a religious man' he said to me and I share his view."

24 10th line from bottom, no bracket.

25 Insert footnote: Dr. Jacob Marshak, now professor emeritus, UCLA Graduate School, Management

Doc. 3a . This was inserted for possible inclusion here.

Doc. 4 See letter from Esther Simpson re. beginnings of Academic Assistance Council.

Page

30 Eliminate "around" first line.

Doc. 5. Is this necessary? Also chronologically slightly out of place.

General: Should one note somewhere that the correspondence from Book Folders and History Folders were those selected by Szilard himself as important.

32 Footnote 23 add "See also letter by Szilard to Samuel Glasstone, dated Jan. 15, 1957, reprinted in Intellectual Migration, p. 102.

Doc. 9 Eliminate last paragraph and footnote 3.

36b Omit docs. 15a and 15b, correspondence with Simon.

Doc. 9. The mentioned Polanyi letter would be more interesting if we can find it.

Do we have letter to Lindemann or just ~~a~~ the telegram which is Doc. 16

Doc. 18. This is a classic. We should have date when Czechoslovakia was forced to accept the Berchtesgaden demands.

Doc. 19. This is just a reminder to go back to original correspondence with Navy and perhaps mention somewhere in the footnotes that the Army had declined to s accept this patent.because it was of no interest to them.

Page

37a and 37b Don't put description of Wigner in here because it interrupts the suspense. Will have to find a spot earlier where this may fit better.

Doc. 22. Letter to ~~Louis~~ Lewis Strauss, dated Jan. 25, 1939, should have an introductory note, such as Szilard describes these new developments in a letter to Louis Strauss with whom he had been in touch in an effort to obtain support for research in this new field.

41 Szilard mentions here that Liebowitz "was not poor but he was not exactly wealthy." Footnote identifying Liebowitz - wherever it is - should be corrected accordingly.

Doc. 24. Regarding footnote 3 we have not found clipping of Feb. 4, 1939.

Doc. 25 Do we need permission?

General: Which documents should be reproduced in German as well as in English?

Which title should be in caps, e.g. page 43.

44 Footnote 36, add source [8]

Page

44 Add dates in square bracket.

Doc. 26 Where does this belong in text?

Doc. 40. Footnote 1. Should it stay?

46 Does document 27 really fit here and should it not come with Doc. 26 inserted earlier for which we have no page number?
At end of page, Docs. 33-52 are listed. 33,34,35,38 39 and 40 have already been filed. Do all the others really fit in this sequence? There is no doc. 51.

Doc. 53 This is copy of a letter which was copied from a copy. Where is the original from which the last 3 paragraphs were omitted? Couldn't find in Book Folder 3

Doc. 54 does not belong here. Should be fitted in in Doc. II folder.

Doc. 55 Not sure whether footnotes should be included or omitted.

49 Footnote 45d is not quite clear. Is this the right order now?

page

48 End of first para. insert: [as a guest, March 1 to June 1, 1939].

Do not delete sentence "Some very simple calculations which I made early in July showed that the graphite-uranium system was indeed very promising." although it seems like repetition.

Doc. 58 We might consider reproducing part of the handwritten German as facsimile. (see below *)

Doc. 59a This is Szilard's letter to Einstein in German dated July 19, 1939 to which he appended his Szilard's draft of a letter addressed to the secretary of state.** This is Doc. 59a. Because this sequence of events is so important I think that the Szilard letter of July 19 could be printed both in the original German and the English translation.

Doc.58 * This was the first version of the letter ~~of~~ to the Ambassador of Belgium dictated by Einstein and taken down by Wigner during the first visit.

Doc.59 ** And Szilard's English translation of the draft of the letter to the Ambassador of Belgium reproduced as document 58.

page

53 Delete second paragraph from source [9] because it is not correct and therefore confusing. Also omit footnote 47. Would be better to quote from memorandum dated April 18, 1955 (speech over the radio on the evening of Einstein's day of death). If no suitable para. found there, footnote 47 should read "On this second visit with Teller Einstein apparently dictated another draft this time for a letter to the President of the United States which Szilard took down in his handwriting in German. (Doc. 60). ~~and later translated~~. This might be reproduced in facsimile. This handwritten draft was redictated by Szilard himself and typed in German after he returned to New York, and is titled "Die in Peconic aufgesetzte Formulierung:" [ ]

I think we could eliminate the excerpts from the early draft by Szilard for Einstein's letter to the President. One might perhaps consider bringing one page of facsimile of the English translation in Szilard's handwriting.

Doc. 61 This is a letter by Szilard to Einstein dated Aug. 2, 1939 with the following enclosures:

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Page 94
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COMMENTS TO PART I BY GWS, FALL 1975

Page*

1 Insert after 2nd ¶. ~~After first paragraph~~ insert the following: "Very often it is difficult to know where one's set of values come from but I have no difficulty in tracing mine to the children's tales which my mother used to ~~tell me.~~ [2] My addiction to the truth is traceable to these tales and so is my predilection for 'saving of the world.'" [1]

3 ~~Document 01:~~ Maturity Certificate of public school (Realiskolai) dated June 16, 1927. Could be a document or reproduced as facsimile. date only in Curr. Vitae

7 ~~Document 02:~~ Record Book of Budapest Technical College (József-müegyetem). This gives the courses and names of teachers and grades for the years 1916 to 1919. This may be reproduced in facsimile. This seems important because same school attended by Wigner and I believe also Teller and von Neumann. Perhaps final certificate only would suffice. Also info Curric Vitae check this c̄ Teller

14 This Kaiser Wilhelm Institute was called Institut für Physikalische und Elektrochemie. (Dr. Eisner, Theisen) will check c̄ Nark Paper.

~~Document~~ 04: becomes illustration, also check data for C.V.

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*Page numbers refer to final draft page numbers.

Page

16 Insert line 12 after "so I thought I would just think whatever comes to my mind," paragraph from p. 18 "I went for long walks and I saw something in the middle of the walk. . . . until I had the whole theory fully developed.[3]

17 Footnote 6. I believe that sources [3] and [14] are identical, i.e., [14] was dictated by Szilard from [3]. I excerpted the story of physics for use by introducer in Volume One. Check whether pp. 14, 15, really come from [2].

Re Pr!! check.

21 Add sentence to end of first paragraph "'As long as you pray to God and ask him for something you are not a religious man.' he said to me and I share his view."

24 10th line from bottom, no bracket.

25 Insert footnote: Dr. Jacob Marshak, now professor emeritus, UCLA Graduate School, Management

Doc. 3a . This was inserted for possible inclusion here. omit

Doc. 4 See letter from Esther Simpson re. beginnings of Academic Assistance Council.

Doc 2 : ask Delbrück —

Page

30 Eliminate "around" first line.

Doc. 5. Is this necessary? Also chronologically slightly out of place. o.k.

General: Should one note somewhere that the correspondence from Book Folders and History Folders were those selected by Szilard himself as important.

yes in preface or ed. note

32 Footnote 23 add "See also letter by Szilard to Samuel Glasstone, dated Jan. 15, 1957, reprinted in Intellectual Migration, p. 102.

Doc. 9 Eliminate last paragraph and footnote 3.

36b Omit docs. 15a and 15b, correspondence with Simon. The mentioned Polanyi letter would be more interesting if we can find it.

Do we have letter to Lindemann or just ~~a~~ the telegram which is Doc. 16

Doc. 18. This is a classic. We should have date when Czechoslovakia was forced to accept the Berchtesgaden demands.

Doc. 19. This is just a reminder to go back to original correspondence with Navy and perhaps mention somewhere in the footnotes that the Army had declined to s accept this patent.because it was of no interest to them.

Page

37a and 37b — Don't put description of Wigner in here because it interrupts the suspense. Will have to find a spot earlier where this may fit better. ?

Doc. 22. Letter to ~~Louis~~ Lewis Strauss, dated Jan. 25, 1939, should have an introductory note, such as Szilard describes these new developments in a letter to ~~Louis~~ Lewis Strauss with whom he had been in touch in an effort to obtain support for research in this new field. ?

had been in touch c Strauss before Jan 25, 1939

41 — Szilard mentions here that Liebowitz "was not poor but he was not exactly wealthy." Footnote identifying Liebowitz - wherever it is - should be corrected accordingly.

Doc. 24. Regarding footnote 3 we have not found clipping of Feb. 4, 1939.

Doc. 25 Do we need permission? Yes — we have address

General: Which documents should be reproduced in German as well as in English?

Which title should be in caps, e.g. p age 43.

44 — Footnote 36, add source [8]

Page

44 Add dates in square bracket.

Doc. 26 Where does this belong in text?

Doc. 40. Footnote 1. Should it stay?

46 Does document 27 really fit here and should it not come with Doc. 26 inserted earlier for which we have no page number?

At end of page, Docs. 33-52 are listed. 33,34,35,38 39 and 40 have already been filed. Do all the others really fit in this sequence? There is no doc. 51.

Doc. 53 This is copy of a letter which was copied from a copy. Where is the original from which the last 3 paragraphs were omitted? Couldn't find in Book Folder 3 Sp will look

Doc. 54 does not belong here. Should be fitted in in Doc. II folder. omit

Doc. 55 Not sure whether footnotes should be included or omitted.

Re find + send to Sp.

49 Footnote 45d is not quite clear. Is this the right order now?

page

48 End of first para. insert: [as a guest, March 1 to June 1, 1939].

Do not delete sentence "Some very simple calculations which I made early in July showed that the graphite-uranium system was indeed very promising." although it seems like repetition.

Doc. 58 We might consider reproducing part of the Wigner's handwritten German as facsimile. (see below *)

Doc. 59 This is Szilard's letter to Einstein in German dated July 19, 1939 to which he appended his Szilard's draft of a letter addressed to the secretary of state.** This is Doc. 59a. Because this sequence of events is so important I think that the Szilard letter of July 19 could be printed both in the original German and the English translation. Can delete Dukas provide original - very good xerox copy

Doc.58 * This was the first version of the letter ~~of~~ To the Ambassador of Belgium dictated by Einstein and taken down by Wigner during the first visit.

Doc.59 ** And Szilard's English translation of the draft of the letter to the Ambassador of Belgium reproduced as document 58.

page

53 Delete second paragraph from source [9] because it is not correct and therefore confusing. Also omit footnote 47. Would be better to quote from memorandum dated April 18, 1955 (speech over the radio on the evening of Einstein's day of death). If no suitable para. found there, footnote 47 should read "On this second visit with Teller Einstein apparently dictated another draft this time for a letter to the President of the United States which Szilard took down in his handwriting in German. (Doc. 60). ~~and later translated~~. This might be reproduced in facsimile. This handwritten draft was redictated by Szilard himself and typed in German after he returned to New York, and is titled "Die in Peconic aufgesetzte Formulierung:" [ ]

do repro as figure

also illustration

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(delay)

fwS
Lock

will take

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o.k. get photo or very good xerox

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Folder: A more complete identification of persons (Placzek, Urey)

Where will the Bound go?

Last Doc. in Folder VI. ? give name - either all or none

Retype documents: 119, 119 a, 120, 121, 122, 124, 125 (or better copy) 126, 127,

Good copy of 115, correct date 118

Illustration: Receipt of Classf. Mat. 7-19-45

June 13, 1964
Speech

LEO SZILARD'S INFLUENCE ON PHYSICS

If one thinks of Szilard as a physicist, the immediate question is: Was he an experimentalist or a theorist? And then it becomes clear that Szilard cannot be considered within the confines of any one category. His work as a physicist had biological overtones; and when he was being most coldly logical, there were warm undertones of the man and the citizen. This awareness of organic interconnections was the cause of that far-sightedness which led him to anticipate so many things that later came about, and perhaps others that are yet to come.

In his earlier years, Szilard was not a prolific writer, and a simple list of his publications will give a false impression of his influence on physics during his stay at the University of Berlin. Someone with a sense of esoteric humor bestowed upon him the title "Katholisator", explaining that, in medieval universities, this was the official who had to make sure that students and visiting scholars met the right people. This duty Szilard imposed upon himself. Many physicists of all countries remember the easy energy with which he performed i[illegible] and thus helped unify the profession with ties of personal friendship. He had long since established a relation of mutual respect with everybody at the University, including Max Planck and Albert Einstein.

That was the time when the "new" quantum theory was being constructed. Szilard did not immediately publish in this field, but he sat, with Erwin Schrödinger, in the front row of John von Neumann's seminar. And it was Szilard who asked the penetrating questions, while the others tried, as best they could, to follow into the unfamiliar reaches of Hilbert Space.

Despite the preoccupation of physicists with these exciting developments, Szilard's Habilitations-schrift was the subject of lively discussion and even debate, in Berlin, even before it appeared in the Zeitschrift. I believe this paper was written sometime in 1925 and submitted to the editor in January, 1928, but it was not printed until March, 1929. The reasons for these delays can be only vaguely surmised.

Today, we can formulate Szilard's thesis in three words: "Thinking generates entropy" -- and no one will contest it. It is difficult to recapture the unwilling attitude of those past times. Perhaps a quotation from Smoluchowski[1] (himself a major contributor to thermodynamic theory) will help: "... There can be no automatic perpetuum mobile, but one might well be possible if it were suitably operated by intelligent beings." Many less careful physicists tended to replace "might well be" with "would certainly be". This had the sanction of James Clerke Maxwell, who had imagined a sub-microscopic, but superhuman, "demon" to operate the device. Hence the debate.

Szilard's matter-of-factness[1] contrasts strikingly with both of the above formulations: "The accomplishment of any physical effect, by action of the sensory and motor nervous systems, is always accompanied by a degradation of energy, quite apart from the fact that their very existence depends upon a continual dissipation of energy."

And, even after we had convinced ourselves of the correctness of Szilard's analysis, we remembered it only on that one day out of every year or so, when we lectured on the relevant part of thermodynamics. Not until nearly twenty-five years later, when others were groping in this same

area, did we realize that we had witnessed the laying of the cornerstone of psychophysics.

For, in this paper, Szilard eradicated the ancient dichotomy of mind and matter, just as Einstein had already eradicated the less ancient dichotomy of energy and matter. If this seems too sweeping, let it be admitted that this iconoclasm was inherent in the Zeitgeist of the early twentieth century. But no physicist attacked the tradition so directly and with such technical elegance, as Szilard did in this paper.

Nearly forty years were to elapse before he returned to this problem and began work on a more detailed solution.

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Even before Hahn's discovery, Szilard had thought about the possibility of a self-sustained, neutron-induced, chain reaction, yielding immense amounts of nuclear energy. He was therefore quick to begin experimenting with fission. Needing a powerful source of neutrons, he went into personal debt in order to rent a gram of radium. In these days of generous support for research, this must sound strange to young ears. But, we should remember that the increasing financial sources for

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In broad outlines, Szilard's part in the Manhattan Project is already a legend. He persuaded Einstein to write Roosevelt; the first patent on nuclear energy names Szilard and Fermi as joint inventors; he (with others) tried to communicate with Truman before Hiroshima. But the details are more numerous, and not insignificant; and it will be some time before they are all described in one document.

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At one stage, these efforts resulted in an organization known publicly as Section S-1 of NDRC, and internally as the Uranium Section. It had several subsections, and both Szilard and Fermi were members of most of these, including the Theoretical Aspects Subsection. This Subsection met at irregular intervals in a small room at the Bureau of Standards in Washington.

It is very difficult to separate the contribution of these two men to the work of this Subsection. All major topics arose during dialogues between Fermi and Szilard, and only gradually did the other members join in the conversation. But, after each meeting, they returned

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Charles Cox
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In 1959 when L.S. learned that he had a very serious illness with a very small probability of cure through radical treatments he asked "How much longer do I have to live if nothing is done" and was told "perhaps six months".

Within twenty-four hours after the shock he said "Allright, I have set up a list of priorities of things I will do in the time which is still left to me: after straightening out legal and financial matters, I shall finish the Biology manuscript on which I have been working and get it ready for publication; then I shall work on the political article I have been thinking about and after that is done - if

there is still time left - I shall work on "the" "Book"

"The Book" was his history of the past, originally conceived during the time of the Manhattan Project when beset by the daily frustrations and admin. problems of this period L.S. said during a particularly trying episode to a friend "When this is all over, I am going to write this story so that God will know the facts" his friend said "don't you think that God knows the facts" whereupon L.S. replied "Yes, but not <u>my version of the facts</u>".

Although source and background materials were carefully preserved and filed by L. S. "My Version of the Facts" was never written. When I, in some distress, complained to Eugene Wigner, during L. S.'s illness in 1959 that I ~~was~~ had been not able to persuade him to get him to write ~~this~~ this book, Wigner replied "I think I can explain this; you see, Leo was always more interested in the future than he was in the past."

That this interpretation was quite correct, was proved in 1960 when L. S.'s condition improved contrary to expectation following

some less radical therapy and when it appeared that we had more than the predicted six months of life left, L.S. was approached by several interested persons ~~to~~ who urged him to write his auto-biography: he consented to an "oral history" to be edited and expanded into an autobiography, and in preparation a tape recorder was placed into his hospital room. It, however, remained unopened for several weeks.

One morning when

I arrived at the hospital Leo greeted me with a big smile and said "You will be pleased. I used the tape recorder very early this morning to dictate something that came to my mind. Listen:" He turned the instrument on and off came, the first draft of "The Voice of the Dolphins" — the history of the next 25 years.

The "oral history interview" never materialized, due to

pressures of other interests, Leo was seldom ready, and on some occasions when he expressed readiness the project was unable to meet his date.

However, L.S. was frequently approached by the press, radio and television for interviews on specific subjects and at times in talking about his ideas necessarily in response to questions, talked about how these ideas developed and how he developed. Some of

these interviews have been preserved, on tape and in transcriptions and although fragmentary and in no sense a replacement for the comprehensive "auto-bio-graphy" which we all hoped for, we catch glimpses of how L.S. saw himself, and with respect to some events, obtain his "version of the facts".

L.S. was a controversial figure and ~~meant~~ had different meanings ~~for~~ to different people. In addition, because of his

unique personality and ways of doing things, he became a legend while still alive. Many legends - truth mixed with fiction in ~~different~~ varying proportions - are circulating and multiplying.

It will take laborious and painstaking work on the part of historians ~~over a long period of time~~ to sort + scrutinize all the evidence + ~~sort and~~ analyze all the facts of this ~~in a~~ ~~comprehensively~~ rich life. In the mean time, while the

so that a comprehensive biography can be written.

In the mean time, and in answer to many demands it seems appropriate to present as much of the material as is readily available in L. S. own words, ~~not~~ primarily in order to perpetuate his thoughts and ideas

memories are still vivid, there is a strong urge on the part of many people, to combine these memories into a composite portrait, into a biography of L. S.

Writing a biography <u>is</u> like painting a portrait and the result is an expression of the personality + ideas of the biographer as well as of the (biographée) person about whom he writes. L. S. therefore will emerge in many different

~~versions~~. portraits, painted in different styles.

My hobby has always been photography, rather than painting (perhaps because of a lack of talent for the latter). I would like therefore, before these portraits are created, and perhaps as a safer source material for them, to present a photograph - letting L. S. emerge himself, showing his ~~own~~ ideas and stories in his own words, ~~and putting~~ as many as

we can readily find at this time

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AARON NOVICK
Institute of Molecular Biology
University of Oregon, Eugene, Oregon

Phenotypic Mixing: Leo Szilard's Iransition to Biology

PHENOTYPIC MIXING

The discovery of phenotypic mixing between related bacterial viruses was a wonderful introduction to biology for Leo Szilard and me. What could be more appropriate for a physicist and a chemist who wanted to become biologists than finding that a virus can change its spots?

I had been trained as a physical organic chemist, although as a child I had first wished to become an astronomer. I turned to chemistry largely in response to the inspiration provided by Frank Westheimer, with whom I studied, rather than because of an intense interest in chemistry. In Westheimer's laboratory at the University of Chicago, I had my first glimmering of the possible excitement of studying biology at the molecular level, when he proposed an experiment in 1941 that might have given more information about the sequence of amino acids in the protein silk fibroin. The results of that experiment were not promising, however, and because of the pressures of the war situation I turned to other things to complete my training. In March of 1943 I joined the Atomic Energy Project at the University of Chicago and was abruptly immersed in the developments which even then held awesome portents for mankind.

Leo Szilard was a fabulous character even among the many legendary figures then associated with the "Project." He had provided not only the principal initiative to get it started in the first place, but also continued to provide momentum by appearing wherever there was a block to its progress and intervening with sound good sense and ingenious suggestions.

Szilard preferred the company of the younger men to that of his own age group and never succumbed to the stuffiness that often comes with middle age. No doubt he seemed eccentric to many, certainly so to the generals, because he declined to confine his work to his office, or his thinking to prescribed subjects. Conversations with him could occur anywhere, on any subject. One might run into him on a street corner, in a restaurant, or wherever he could sit in a sunny and quiet spot. He was often to be found near the tennis courts at the Quadrangle Club, sitting with his face toward the sun, asking questions and listening with half closed eyes in a conversation that might be on child-rearing or aging, physics or sociology, medicine or law. He was fascinated by the laws of man as well as by the laws of nature. Thinking was his greatest pleasure, and he enjoyed thinking about an incredibly wide range of subjects. It was in such contexts that I got to know him, for he enjoyed advising the young and was excellent at it. Whether the problem was personal or scientific, one found oneself editing the nonsense out of one's thoughts when talking to him.

True to his character, at the end of the war Szilard turned his energy and ingenuity toward helping mankind in its moral and political adjustments to the technology of atomic energy. During this period I became active politically, along with many other of the younger scientists. One spring evening in 1947, as we were leaving a meeting of the Atomic Scientists of Chicago, Szilard approached me and asked whether I would care to join him in an adventure into biology. Despite his cautioning me to think his proposition over carefully, I accepted immediately.

Apparently he had been considering a move to biology for some time, in part because he saw that the heroic era of nuclear physics, where one man could contribute significantly, had ended and in part because he sensed that biology was on the threshold of a revolutionary era much like that of physics in the preceding two decades.

But at that time it was unheard of for biology departments to appoint physicists, or even chemists, to their staffs. Fortunately, Robert Hutchins, Chancellor of the University of Chicago, appreciated Szilard's greatness and had the courage to appoint him as Professor of Biology and of Sociology, with specific departmental affiliations to be worked out later. Szilard was placed administratively in the new Institute of Radiobiology and Biophysics, one of three institutes set up at Chicago to adapt to post-war scientific developments. Szilard tried without success to get an appointment for me in that Institute. But I was so excited about the prospects of working with him in biology that I was happy to join him even in a position where I would be dependent upon his research grants for my support. He was, however, never pleased by this arrangement and, because he felt I was being underpaid, insisted on supplementing my salary from his own pocket.

Szilard proposed that we get started in biology by taking the Cold Spring Harbor phage course that had been recently started by Max Delbrück. So we enrolled for the summer session of 1947, which was to have been taught by Mark Adams. But because Adams became ill, Delbrück delayed his first post-war return to Europe and stayed on at Cold Spring Harbor to give the course. We were as delighted as the many other proselytes, initiated by Delbrück, to discover a biology that had been made comfortable for people with backgrounds in the physical sciences. In that three-week course we were given a set of clear definitions, a set of experimental techniques and, most importantly, a set of core problems to be clarified and understood. It seemed to us that Delbrück had created, almost single-handed, an arena in which we could work, and after three weeks we felt ready to embark on our own without further preparation.

Evidently Szilard regarded Delbrück highly. This was easy for me to see since usually Szilard only listened to people as long as they had something to say that interested him and made sense. This meant that he sometimes turned away in the middle of conversations. But whenever Delbrück was talking, he stayed to listen.

We had hardly returned to Chicago that summer, where I was trying to complete my work in nuclear physics, when Szilard persuaded me to return with him to Cold Spring Harbor to try there some genetic crosses between differently marked phages. These primitive experiments had to be abandoned for lack of laboratory facilities when we returned to Chicago that fall.

We arranged to start work in our own laboratory in January of 1948. The Institute of Radiobiology and Biophysics was located in a former synagogue of a Jewish orphanage whose buildings had been taken over by the University, and our laboratory was a room in the basement with a very low ceiling laced with pipes.

We decided to study mutation in bacteria and tried a number of schemes to find a simple and accurate way of measuring mutation rates. We also looked for cases where we could study forward and reverse mutation at the same genetic locus.

We were both impressed by Joshua Lederberg and the implications of his by then two-year-old discovery of genetic recombination in bacteria. Delbrück, however, had reservations about Lederberg's interpretations. Because of our very high regard for Delbrück's opinions we tried to design an experiment that, we hoped, would test Lederberg's conclusions and convince Delbrück. For this purpose we compared the results of a bacterial cross with those of the reciprocal cross and found clear evidence of genetic recombination, independent of the particular markers used. Szilard wrote to Delbrück and to Luria reporting the results and offered to eat his hat (which, it must be admitted, he never wore anyway) if this was not genetic recombination. Delbrück replied that recombination or not, one still did not understand the basic nature of the phenomenon and he urged us to continue our studies. But we were by then being attracted by other problems and certainly had little incentive to continue the work when we learned that in essence our experiment had been done already and reported as part of a table in a paper by Lederberg (Lederberg, 1947).

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As part of our schooling the preceding summer at Cold Spring Harbor we had studied attentively the 1946 Cold Spring Harbor Symposium volume which contained the papers that all the early phage workers considered basic to the new field later to be called molecular biology. We were particularly fascinated by the Delbrück and Bailey paper (Delbrück and Bailey, 1946), which reported the discovery that a single bacterium can be infected simultaneously with two genetically distinct phages and that new phage genotypes issue among the progeny of this mixed infection. A paradox presented in this paper quickly caught our attention. Like many theoretical physicists Szilard loved paradoxes—they are certain indications of error in understanding and they provide solid food for thought.

The paradox was this: When bacteria, mixedly infected with phages T2 and T4, are plated before lysis, those yielding T2 are scored as plaque yielders on plates seeded with B/4, those yielding T4 as plaques on B/2, and those yielding both as clear plaque formers on B/2+B/4; those yielding T2 or T4 alone give turbid plaques on B/2+B/4. One would expect that the number of plaques on B/4 should be *greater* than the number of clear plaques on B/2+B/4, since the latter plate indicates only those T2 yielders which also yield T4. But, as Delbrück and Bailey pointed out, the number of plaques on B/4 is generally much *less* than the number of clear plaques on B/2+B/4.

Our interest in this paradox was revived by a talk with Luria who had come to Chicago to give a memorial lecture for Louis Slotin, a physicist who had been killed in a laboratory accident at Los Alamos. He told us that the discrepancy was much greater for UV-irradiated T2.

It is difficult to recall the line of our reasoning, which must have followed from the inference drawn by Delbrück and Bailey that the presence of B/2 on the mixed indicator plate raises the plating efficiency of the liberated T2 particles. Apparently, we must have hit upon the idea that the effect was on a sub-class of the liberated T2 particles and that the critical factor was B/2 itself rather than some chemical it made. By studying the process in liquid suspension rather than on a plate, we found that the proportion of T2 type phages capable of plating on B/4 was strikingly increased by one step of growth of the mixed phage lysate on strain B/2, and we were able to show that strain B worked as well for increasing the proportion of such phages.

Reproduced here are some of the first notebook entries on our work of that May in 1948. Szilard, who made these records, always used the right-hand page first. This allowed him to examine simultaneously a page, and either the page which preceded it or the page which followed it, without having to turn a page. He sometimes reminded himself that he was following this convention by putting a symbol resembling an inverted N in the upper right-hand corner.

As can be seen from these notebook entries, we soon realized that we were dealing with phenotypic mixing. That is, the resolution of the paradox was that, among the phage progeny of a bacterium simultaneously infected with T2 and T4, there appear phage particles having the T2 genotype but carrying the T4 phenotype. In further experiments, which were never published, we were able to demonstrate that these T2 particles with T4 host range are far more sensitive to neutralization by anti-T4 serum than normal T2, showing that the T4 phenotype was probably the consequence of a T4 protein in these particles. We evidently had the correct insight into the nature of these particles, because we explained to our friends that they arose in the following way: At some point in the development of the phage in the host bacterium, the phage particles put on coats, much like customers leaving a restaurant. For some unknown reason there seemed to be an excess of T4 coats.

In retrospect, it is clear that we did not appreciate sufficiently the importance of the clue provided by this discovery. We were even uncertain at first whether to publish it and reported it only in the "Phage Information Service," which circulated privately among the members of the Phage Group. Apparently we were not alone in our lack of appreciation, because Delbrück subsequently advised us to forget about publishing it. Finally, we did publish our results (Novick and Szilard, 1951), though whether through appreciation of their importance or through some kind of vested interest I no longer recall.

Fortunately, A. D. Hershey did take the result seriously. He wrote to us describing his finding of phenotypic mixing between T2 wild type and T2h its host range mutant. I like to believe that this effect led him to search for the physical basis of phenotypic mixing which culminated in his fancy use of the Waring Blendor to separate viral germ from viral soma, an experiment which provided the ultimate proof that the genotype of a phage is established by its DNA (Hershey and Chase, 1952).

Of course, another reason why we did not pursue the implications of our findings on phenotypic mixing is that we became interested in other things. Szilard had been intrigued by Monod's discovery that bacteria fed a mixture of glucose and lactose at first consume only the glucose and metabolize the lactose only after they have depleted the glucose (Monod, 1942). Szilard argued that if the glucose concentration of the medium were lowered sufficiently, a point should be reached below which the bacteria *would* use both sugars at the same time. This point could be found, he reasoned, if the bacteria were grown in a continuous culture apparatus in which both sugars were fed simultaneously at less than saturating concentrations.

This reasoning led him to think about how a continuous culture apparatus might work, and it was not long before he discovered the principle of the chemostat. With an appropriate apparatus we were able to demonstrate that the principle worked (Novick and Szilard, 1950a). We realized that the chemostat could be used not only for the physiological studies for which it was invented but also for an accurate measure of mutation rate. So we embarked on a study of bacterial mutation. Our very first finding, namely that the spontaneous mutation rate is a constant per hour (rather than per division period) over a wide range of bacterial growth rates in the chemostat (Novick and Szilard, 1950b), was understandable neither then nor subsequently.

During our studies of mutagenesis in the chemostat we found that our tryptophan-requiring strain of *E. coli*, when grown on a limiting supply of tryptophan, excreted great quantities of a substance which appeared to be a tryptophan precursor. The production of this substance could be stopped instantly by the addition of tryptophan to the culture, indicating to us that an amino acid can inhibit its own formation by a direct effect on one or more enzymes in its biosynthetic pathway (Novick and Szilard, 1954).

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In late 1953, my collaboration with Szilard came to an end when I left Chicago for Paris to spend a year at the Institut Pasteur with Lwoff, Monod, and Jacob. Szilard continued to maintain his interest in biology; but, as the nuclear arms race of the nineteen fifties developed, he devoted a large fraction of his energies to attempts to divert mankind from this disastrous course. Nevertheless, he managed to keep up with the explosive development of molecular biology during this period. He was a frequent visitor to the most active laboratories, both in the United States and in Europe and on these visits contributed original and useful ideas. He also wrote a number of theoretical papers during this period, including an ingenious theory of ageing (Szilard, 1959). He constructed a theory to account for control of enzyme synthesis in bacteria (Szilard, 1960a), a theory which he used also to explain the production of specific antibodies in vertebrates (Szilard, 1960b). In the last months of his life he turned to a problem that had been of great interest to him for many years, namely how the brain works. He developed a theory of the nature of the synaptic links between neurons and how neural networks could be constructed to account for learning (Szilard, 1964) and he was writing a second paper when he suddenly died in the spring of 1964. Szilard had evidently become a biologist, although I suspect he always continued to think of himself as a physicist merely interested in biology.

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