No one in his right senses in Hungary, no matter how much he was interested in physics, would major in physics in Hungary. I myself majored in electrical engineering but when after the First World War, I went to Berlin to continue my studies, the attraction of physics became so great that I dropped my studies of engineering and set out to obtain a Doctor's Degree in physics at the University of Berlin. I very nearly did not make it, but then, suddenly, my work began to turn out very well. I got my degree and I was regarded as a young man of great promise by those whose opinion I valued most highly. In the 1920s physics was the king of the sciences and Berlin was a great center of Physics.

Ever since I was 13 I was interested in physics and in public affairs, but I kept these two things sealed in water-tight compartments and it never occurred to me that these two interests of mine would ever meet. Because of my interest in public affairs, Nazism in Germany did not come as a surprise to me. In 1933, when Hitler took office, I kept two suitcases packed in my room in the Harnack House, the faculty clubhouse of the Kaiser Wilhelm Institutes, and after the Reichstag was put on fire I picked them up and I took a train to Vienna. There I tried to appraise what may be in store for those scientists and scholars on the staff of German universities who would be unacceptable to the Hitler Government. Some organization ought to be set up abroad so I thought, preferably in England, which would undertake to find positions for those who will be forced to resign from their university positions in Germany. A chance encounter with Sir William Beveridge was instrumental in the setting up of such an organization in London and it also landed me in London in the late spring of 1933.

# IV. Berlin, 1920 - 1933 (1)



The collapse of the Austro-Hungarian Army was followed by a troubled period in Hungary, and ended with the Communist government of Béla Kun which lasted about four months. This government lasted too short a period of time to be able to do anything except hold office. During this period the things which have deteriorated during the war deteriorated even further, and I made up my mind that I wanted to leave Hungary in order to study in Germany. One year

One year before I had been drafted I entered, as a student, a Hungarian Institute of Technology in o rder to study electrical engineering. My real interest at that time was physics, but there was no career in physics in Hungary. If you studied physics, all that you could have become was a high-school teacher of physics, not a career that had any attraction for me. Therefore,I considered seriously to do the next best thing and to study chemistry. I thought that if I studied chemistry I would learn something that is useful in physics and I would have enough time to pick up whatever physics I needed as I went along. This I believe in retrospect was a wise choice, but I didn't follow it; for all those whom I consulted impressed upon me the difficulty of making a living even in chemistry and they urged me to study engineering. I succumbed to that advice, and I cannot say that I regret it, because whatever I learned while I was studying engineering came me in good stead later after the discovery of the fission of uranium.

During the troubled times of the Communist regime of Béla Kun, I made a strenuous effort to obtain a passport and to go to continue my studies of electrical engineering in Germany. One or two days after these efforts were successful, the Communist regime collapsed and was replaced by the regime of Horthy. Thus I had to start from scratch in my quest for a passport, but through the help of friends I got one rather quickly and I left Hungary to go by way of Vienna to Berlin. This was about the worst time after the war because of the coal shortage. There was a shortage of food and there was a shortage of coal; because of a shortage of coal, travel was slow, and as a matter of fact it took me two weeks to get from Budapest through Vienna to Berlin.

I stayed in Vienna only for a few days, as long as it was necessary to make arrangements for the trip to Berlin; but during those few days I was greatly impressed by the attitude of the Viennese, who in spite of starvation and misery were able to maintain their poise, and were as courteous as they have always been, to each other, as well as to strangers.

In Berlin I had to face new difficulties. The number of foreign students who were admitted were limited. The attitude towards foreign students was not friendly in this respect, and I had in Hungary considerable difficulty in obtaining a German visa. I first tried to get a German visa to go and study in Munich. I was told that in order to obtain such a visa, I must first furnish proof that I have been admitted as a student at the Institute of Technology in Munich. I wrote to the Institute of Technology and received the reply that they would admit me as a student if the police would give me a permission of residence. I wrote to the police in Munich asking for such a permission, and was told that if I were admitted as a student at the Technische Hochschule the police would give me such a certificate of residence but otherwise they wouldn't.

There was no legal way of solving this dilemma. But there seems to be in operation a law of nature which says that if all legal ways are barred in a righteous cause, the righteous cause finds some illegal ways to achieve its purpose. In this particular case, the illegal ways consisted in a forged telegram I procured through friends at Army Headquarters in Budapest, which advised me that I have been admitted to the Technische Hochschule of Berlin-Charlottenburg as a student. On the strength of that telegram I obtained a visa, the German visa, and left for Berlin in order to a apply for admission to the Technische Hochschule of Berlin. This permission I finally got, but not without difficulty and not without having to bring to bear all the pressure I could through such private connections as I was able to muster in the city of Berlin.

Berlin at that time lived in the heydays of physics. Einstein was there, Max Planck and Von Laue were at the University of Berlin, and so was Walter Nernst; and Fritz Haber was at that time director of one of the Kaiser Wilhelm Institutes. Engineering attracted me less and less and physics attracted me more and more, and finally the attraction became so big that I was physically unable to listen to any of the lectures through which I sat more or less impatiently at the Institute of Technology.

Even though all arguments by the conscious spoke in favor of getting a degree in engineering rather than getting a degree in physics, whatever considerations went on, on the subconscious level, argued for the opposite. In the end, as always, the subconscious proved stronger than the conscious, and it made it impossible for me to make any progress in my studies of engineering. Finally the ego gave in, and I left the Technische Hochschule to complete my studies at the University, some time around the middle of '21.

A student of physics had in those days in Berlin great freedom. Boys left high school when they were eighteen years old. They were admitted at the University without any examinations. There were no examinations to pass for four years, during which time the student could study whatever he was interested in. When he was ready to write a thesis, he either thought of a problem of his own or he asked his professor to propose a problem on which he could work. At the better universities, and Berlin belonged to them, a thesis in order to be acceptable, had to be a piece of really original work. If the thesis showed the student to be really able, and was accepted, the student had to pass an oral exam.

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(end of this tape)

By the time I was thirteen I was very much interested in physics and when I reached college age I would have studied physics had there been some way of earning a living as a physicist in Hungary. A physicist could earn a living only by becoming a high school teacher and this particular career did not attract me. So I did the next best thing to studying physics; I studied electrical engineering. In 1919, I went to Berlin to continue studying electrical engineering, but in Berlin the attraction of physics became too much for me. After one year of study at the Institute of Technology in Berlin I quit and moved over to the University of Berlin to study physics. This was the hey-day of physics in Berlin. Max Planck, Max von Laue, Walter Nernst, and later on, Ervin Schraedinger taught at the University of Berlin and Albert Einstein was there, attached to the staff of the Prussian Academy of Sciences. At Christmas, 1921 (K.W.) ~~1931~~, I decided to tkae a few weeks vacation from working on a research problem assigned to me as my doctors dissertation by Professor von Laue. I thought I would just loaf for a few weeks and think about whatever comes to my mind. I started to follow up some curious ideas which came to me and within three weeks I had a paper written on a subject completely unrelated to the assignment I had received from Laue. I didn't quite dare to take this paper to Laue, but I went and spoke to Einstein about it. At first he was quite incredulous and thought that what I claimed to have done couldn't be done. It didn't take him, however, long to get the point. Encouraged by Einstein, I telephoned Laue and asked him whether I might bring him a paper to look at and to decide whether it might be acceptable as a thesis, in lieu of the assignment which he had given me. That evening, I took the paper to his house ~~; he lived~~ in one of the suburbs of Berlin. Next morning, the telephone rang. It was Laue; he called to tell me that my thesis had been accepted.

Tolole 4. D Stenorette 1963 tape
Re-typed 1967

Excerpts from Taped Interview, Dr. Leo Szilard, 1963 (Physics)

. . . . . . . . . . . Well, my studying was interrupted by the war. When I was eighteen years okd, I passed the usual exam which ended the highschool career and then I wanted to study engineering. I went to the Institute of Technology in Budapest for a year, but then I was recruited as a soilder and I didn't leave the army until the war was over. When the war was over in 1918 there was rather a lot of disorder in Hungary and the atmosphere was not very conducive to learning. In any case, the schools in Germany were very much better than in Hungary and I tried to get into a German school. There was, as you may remember, soon after the war ended in October 1918 a communist government in Hungary under Bela Kun which lasted for about four months, and during that time I made great efforts to get permission to go to a German university and just as I got the permission, the Communist regime ended and another regime took over, the regime of Horty and I had to start from scratch. But about Christmas time 1919, I left Budapest to go to Berlin where I wanted to continue to study engineering and I did that at the Institute of Technology in Berlin - Charlottenburg.

In Berlin at first I went to the Institute of Technology. But I really lost interest in engineering, it was too much what we might call routine application of already establishedk knowledge and the attraction of physics became b then very great. But physics was centered not at the Institute of Technology but at the University of Berlin. This was really the hey-day of physics in Berlin: at that time there were in Berlin von Laue, Max Planck, Walter Nernst, Fritz Haber. And while I was still in Berlin, a few years after I arrived, there came Erwin Schrodinger. And of course, there was Einstein. Einstein was not at the university, he was a full-time member of the Prussian Academy of Sciences and I was not an actual pupil of Einstein. I was a regular pupli of the university, and at some point rather early I went to von Laue who was Professor of Theoretical Physics and asked him whether he would give me a problem on which I could work to get my doctor's degree.

Let me tell you, maybe, the story of my doctor's thesis.

Let me tell you, maybe, the story of my Doctor's thesis. I had this problem which von Laue gave me but I couldn't make any headway with it. As a matter of fact, I was not even convinced that this was a problem that could be solved, and I forced myself to work on it, but it just wouldn't go at all. And this went on for about six months. Then came Christmas 1921, and I thought Christmas time is not a time to work, it is a time to loaf, and so I thought I would just think whatever comes to my mind. And pretty soon things began to come into my mind, in affield completely unrelated to the theory of relativity, and within three weeks I had produced a manuscript of something which was really quite original. But I didn't dare to take it to von Laue, because it is not what he asked me to do. There was a seminar for students which Einstein held at that time, which I attended and after one of these seminars, I went to him and said that I would like to tell him about something I had been doing, and he said, "Well, what have you been doing?" And I told him what I have done. And Einstein said, "That's impossible. This is something that cannot be done." And I said, "apparently no, but I did it." So he said, "How did you do it?" Well, it didn't take for him five minutes or ten minutes to see and he liked this very much. So this then gave me courage and I took the manuscript to von Laue. I remember that I caught him as he was about to leave his class and I told him that while I didn't do - write the paper which he wanted me to write, I wrote something else, and I wondered whether he might be willing to read it, and tell me whether this could be used perhaps as my dissertation for my Doctor's degree. And he sort of looked somewhat quizzically at me, but he took the manuscript and next morning, early in the morning, the telephone rang. It was von Laue who said, "Your manuscript has been accepted as your thesis for the Ph. D. degree."

The subject, well up to the time that I wrote this thesis, it was generally believed that the laws which govern the thermodynamical fluctuations must be derived from mechanics and that they transcend what is called the second law of thermodynamics. And I showed that the second law of thermodynamics was much more than just a statement about the average values; it also covers the loss which governs the fluctuations - the thermo-dynamic fluctuations. Now this was not really the beginning, it was not the

cornerstone of a new theory, it was rather the roof of an old theory. However, about six months later, I wrote a little paper on a rather closely related subject; it dealt with the problem of what is essential in the operations of the so-called Maxwellian demon, who guesses right and then does something, and by guessing right and doing something, he can violate the second law of thermodynamics. And this paper was a radical departure in thinking, because I said that the essential thing here is that the demon utilizes information to be precise information which is not really in his possession, because he guesses it. And that there is a relationship between information and entropy, and I computed what this relationship was. Now, this paper no one has paid <u>any</u> attention to, until, after the war, information theory became fashionable. Then the paper was rediscovered and now this old paper, to which I would think for over 35 years, nobody paid any attention, is a cornerstone of a modern information theory.
Question: What is information theory?)

Well, the theory is embodied in my Doctor's thesis. Yes, I went for long walks and I saw something in the middle of the walk and when I came home I wrote it down, and next morning I woke up with a new idea and I went for another walk, and it crystallized in my mind and, in the evening I wrote it down. Well, it was an onrush of ideas, all more or less connected, which just kept on going until I had the whole theory fully developed. It was a very creative period. In a sense, the most creative period in my life, where there was a sustained production of ideas.
. . . . . . . . . . . . . . No, that was considerably later. This was maybe 1928 or 1929, when I began to think what might be the future development of physics. Disintegration of the atom required higher energies that were available up to that time. There has been no artificial disintegration of the atom and I wa s thinking of how could one accelerate particles ~~of the atom and I was thinking of~~ to high speeds, and I hit upon the idea of the cyclotron, maybe a few years before Lawrence, and I wrote it down in the form of a patent application which was filed in the German patent office. It was not only the general idea of the cyclotron, but even the details of the stability of the electron orbits, and what it would take to keep these orbits stable, all this was worked out on this occasion.

. . . . . . . Yes, Einstein and I had an idea of how to pump liquid matters and this we patented also, again in Germany, and we wanted to use it to make a household refirgerator without moving parts and, as a matter of fact, we built one refrigerator which was based on this principle. It was not very practical, because mechanical refrigerators which have moving parts function really quite well and are not too noisy, and so this principle of pumping liquid matters had no application until atomic energy came along. But then, you see, after the war, they began to build pumps of this sort, and they are really quite useful.

From 1963 Tape.

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(1963 ? Transcribed 28 pages
~~Sc. + Techn.~~) March 1965
last 5

Taped Interview - Dr. Leo Szilard

S: When I was between ~~the~~ age~~s~~/ ~~of~~ 6 and 10, I was very often ill. I had ~~lots~~ all sorts of colds and bronchitis and I did not go much to public school until I was 10 years old. But when I was 10 years, I was over this spell of illnesses and then I went to ~~a~~ the regular - it was not a gymnasium, it was what you would call Realschule, where emphasis is placed not on classic languages like in the gymnasium, but on things like - more on science. ¶ I was the oldest of three children, and we lived in a house which belonged to ~~a~~ family ~~that~~ in the broadest sense of the term the house belonged originally to my grandparents, then it was inherited by three sisters of whom my mother was one, and this sister had the whole floor. It was a house with a large garden in a - the cottage district of Budapest.

More on science.

Voice: ---

S: All children ~~did~~ do a great deal of reading between the ages of six and ten.

Voice: Not in this country, anyway. It ---

S: In Europe they do. You see, there was no television at that time and I remember that I was ~~very~~ already very intensely interested in physics ~~by the time~~ when I was 13. — V: Why — At that time, I ~~had~~ got a few playthings, which are sort of physical playthings, playthings in physics, and I remember how overjoyed I was. ~~At last~~ and also I read books, then I begin to read books where I try to understand elementary physics.

Yes, the public school where I was aged 10 - I went to public school.

Well, my studying was interrupted by the war when I was 16 years old - no, I was 18 years old, I passed the usual exam which ended the high school career and then I wanted to study engineering and I went to the Institute of Technology in Budapest for a year, but then I was recruited as a soldier and I didn't leave the army until the war was over. I went through Officer's School and I didn't have a very hard life, in the army. ~~I~~ But returned when the war was over in 1918, there was rather a lot of disorder in Hungary. There was not much - the atmosphere was not very conducive to learning. In any case, the schools in Germany were ~~really~~ very much better than in Hungary and I ~~applied~~ tried to get into a German school. There was, as you may remember, when the war ended in October 1918, there was soon thereafter a Communist government in Hungary under Bela Kun which lasted for about four months, and during that time, I made great efforts to get permission to go to a German University and just as I got the permission, the Communist regime ended and another regime took over, the regime of Horthy, and I had to start from scratch. But about Christmas time, 1919, I left Budapest to go to Berlin where I wanted to continue to study engineering and I did that at the Institute of Technology in Berlin - Charlottenburg.

Voice: I cannot imagine you as a soldier. What kind of a soldier were you?

S: Well, I was highly respected among my colleagues, because I understood how the telephone worked.

Even then, technical knowledge was regarded as being of some importance in the artillery at least where I was with - in the officers School for artillery and this kind of knowledge was valued.

I did not take kindly to military discipline, but on the other hand, you see, we were sort of a privileged group in the officer - the boys who went to officer's school were a privileged group, and they were not very hard ~~living~~ driven as far as military discipline ~~as far as living~~ was concerned.

No, that story is not quite correct. What happened was this, that we had to fill out during the ~~in~~ Manhattan District ~~this~~ work endless pages of questionaires for clearance purposes and one of these questions was, "What are your hobbies?" and this is where I wrote "The baiting of brass-hats." But It was not the Hungarian officers I had in mind at the time.

Yes, the Horthy regime was, in the beginning, at least, ~~anit~~ anti-semitic but by that time I have - I didn't see much of the ~~hostile~~ Horthy regime, I left at that time.

<u>Voice</u>: Did, ---did,---

<u>S</u>: No, because I just didn't stay in Hungary long enough. Under that regime.

Before the first world war, there was so little anti-semitism in Hungary that I remember a series of articles which appeared in one of those high-brow journals devoted to the question, "Is there anti-semitism in Hungary?" And I recall one writer who said that he didn't think there was, even though it may take several years before a stupid Jew will become a member of the Cabinet.

I never saw any example of anti-semitism, no.

Voice: ---Germany, (In Germany, then.)

S: In Germany, again I left before anything happened. No, in Berlin~~'s~~ (at first I went to the) Institute of Technology.- But I really lost interest in engineering, it was too much, what you might call routine application of ~~all of the~~ already established knowledge, and the attraction of physics became then very great, but physics was centered ~~,~~ not at the Institute of Technology, ~~,~~ but at the University of Berlin, which was in Central Berlin, and this was really the he~~a~~yday of physics in Berlin. At that time there were in Berlin, ~~Van Laue~~ von Laue, Max Planck, Walter Nernst, Fritz Ha~~r~~ber, and while I ~~w~~as still in Berlin, a few years after ~~he~~ I arrived, there came Erwin Schrödinger. And of course, there was ~~Ashton~~ Einstein. ~~Ashton~~ Einstein was not at the University. He was a full time member of the Prussian Academy of Sciences and what's (my own -) I was not a (actual) pupil of ~~Ashton~~ Einstein, I was a regular pupil of the University, and at ~~that~~ some point, rather early, I went to V~~an~~on L~~aue~~aue who was ~~a~~ professor of ~~theory of~~ theoretical physics and asked him whether he would give me a problem on which I could work to get ~~a~~ my doctor's degree.

Let me tell you, maybe, the story of my Doctor's thesis. I had this problem which Von Laue gave me ~~and~~ but I couldn't make any headway with it. As a matter of fact, I was not even convinced that this was a problem that could be solved, and I forced myself to work on it, but it just wouldn't go at all. And this went on for about six months. Then came Christmas, 1921, and I thought Christmas time is not a time to work, it is a time to loaf, and so I thought I would just think on whatever comes to my mind. And pretty soon things began to come into my mind, ~~and~~ in a field completely unrelated to the theory of relativity, and within three weeks I had produced a manuscript of something which was really quite original. But I didn't dare to take it to von Laue, because it is not what he asked me to do. There was a seminar for students which ~~Ashton had~~ Einstein held at that time which I attended and after one of these seminars, I went to him and said that I would like to ~~talk over with him~~ tell him about something I had been doing, and he said, "Well, what have you been doing?" and I told him what I ~~was on.~~ have done. And ~~then he~~ Einstein said, "That's impossible, ~~that~~ this is something that cannot be done." And I said, ~~"But~~ "Apparently not, but I did it." So he said, "How did you do it?" Well, it didn't take for him five minutes or ten minutes to see and he liked this very much. So this ~~thing~~ them gave me courage and I took the manuscript to ~~Van Laue~~ Von Laue. I remember that I caught him as he was about to leave his class and I told him that while I didn't do - write the paper which he wanted me to write, I wrote something else, and I wondered whether he might be willing to read it, and tell me whether this ~~can~~ could be used perhaps as my dissertation for my Doctor's degree. And he sort of looked somewhat quizzically at me, but he took the manuscript and next morning, early in the morning, the telephone rang. It was ~~Van Laue~~ Von Laue, ~~he~~ who said, "Your manuscript has been accepted as your thesis for the Ph.D. degree."

The subject, well ~~up to that time~~, up to the time that I wrote this thesis, it was generally believed that the laws ~~that~~ which governed the thermodynamical fluctuations must be derived from mechanics and that they transcend what is called the second law of thermodynamics. And I showed that the second law of thermodynamics was much more than just a ~~mundane~~ statement about the average values, it also covers the loss which governs the fluctuations, the thermo-dynamical ~~the~~ fluctuations. Now this was not really the beginning, ~~it was rather the~~ it was not the cornerstone of ~~the~~ a new theory, it was rather the roof of an old theory. However, about 6 months later, - I wrote a little paper which - a rather closely related subject; it dealt with the problem with how it is, what is essential in the operations of the so-called demon, who guesses right and then does something and by guessing right and does something, he can violate the second law. Of thermodynamics. And - this paper was a radical departure in thinking because I said that the essential thing - theories - that the demon utilizes information which is not really in his possession, because he can guess it. And that there is a relationship between information and anthropy, and I computed what this relationship was. Now, this paper has - no one has paid any attention to this paper until, after the war, information theory became fashionable. Then the paper was rediscovered and now this old paper, to which for over 35 years, nobody paid any attention, is a cornerstone of modern information theory.

Voice: What is information -- --

S: Well, the theory is embodied in my Doctor's thesis. Yes, I went for long walks and I saw something in the middle of the walk and when I came home I wrote it down, and next morning I woke with a new idea and I went for another walk, and it crystallized in my

~~it has crystallized in my mind~~

mind and in the evening I wrote it down. - Well, ~~the sequel was the~~ it was a ~~undiversified~~ an onrush of ideas, all more-or-less connected, ~~it~~ which just kept on going until I had the whole theory fully developed. It was a very creative period. In a sense, the most creative period in my life, ~~it~~ where there was a sustained production of ideas.

Voice: Experience?

S: Well, I wouldn't say - you are not lonely when you create, you know, you are fully occupied, anyway, and very happy, and -

Voice: I mean, you are by yourself?

S: But you are alone, surely.

No, that was considerably later. This was ~~the year,~~ maybe 1928, 1929, when I begin to think what ~~the~~ might be the future development of physics. ~~Nuclear fission~~ Disintegration of the atom required higher energies than ~~was~~ were available up to that time. There has been no artificial disintegration of the atom and I was thinking of how could one accelerate particles to high speeds, and I hit upon the idea of the cyclotron, maybe a few years before Lawrence , and I wrote it down in the form of a patent application which was filed in the German patent office. It was not only the general idea of the cyclotron, but even the details of the stability of the electron orbits, and what it would take to keep ~~them -~~ these orbits stable, all this was worked out on this occasion.

Yes, ~~actually~~ Einstein and I had an idea of how to pump liquid matters and this ~~was~~ we patented also, again in Germany, and we wanted to use it to make a household refrigerator without moving parts and, as a matter of fact, we ~~did run a~~ built one refrigerator, which was based on this principle. It was not very practical, because mechanical refrigerators which have moving parts function really quite well and are not too noisy, and so this principle of pumping liquid matters had no application until atomic energy came along. But then, you see, after the war, ~~there begin to be~~ they began to build pumps of this sort, and they are really quite useful.

Why did I change to biology? Well, I wanted to change to Biology when I left Germany in 1933, because I was interested in it. And, as a matter of fact, I went so far as to talk to A. V. Hill who was one of the grand old biologists of England at that time. He was not very old, but he was one of the senior highly respected scientists, and he thought it wasn't such a bad idea. He, himself, used to be a physicist, before he began - made his rather famous experiments in physiology, and he thought, he said, "Well, if you really want to do that, I'll get you a position as a demonstrator in physiology, and you will just - a few days before you demonstrate, you will read up on the subject and you will be perfectly able to teach the students and this would be a good way for you to both earn a living and learn the field. I would probably have done that if I hadn't - if physics hadn't become too exciting; but in 1933 - latest '32 - early '33 - I'm sorry, late in '33, early in '34, I think it was, the Joliots have discovered artificial radioactivity. And in the fall of 1933, when I was in England, it occurred to me that something like a chain reaction might be set up in matter, if we find an element that, when bombarded with neutrons, would emit neutrons - so that it would emit, say, two neutrons for each neutron absorbed, or something of this sort, then we could make a chain reaction. Now, with artificial radioactivity available, there was a tool given how - one could - easily discover neutrons, because there are a number of elements which become radioactive when they are bombarded with neutrons. Now, the fact that the chain reaction might be possible, the fact that we have a tool, with which we can observe neutrons by virtue of their producing radioactivity, because otherwise you can't see neutrons so easily, you know, you can't smell them, you can't touch them, you can make them visible in a Wilson Cloud ~~glass~~ chamber, but that was a rather cumbersome technique, so I saw the possibility of a chain reaction, saw the tools available, to make an inquiry into this subject, and this seemed to be so attractive to me that I gave up the idea of going into biology and went, instead, into nuclear physics, which was a new field for me.

<u>Voice</u>: ---search for the

<u>S</u>: No, this I think is a misquotation. I never said there are mysteries that ~~can't~~ can be solved. However, I think all physicists who are successful in their field share the belief that if anything exists, it should be possible to explain it. And it is really this kind of conviction which the physicists who moved into biology, carried with them. And the physicists who moved into biology and became successful in biology, became successful not because they applied their knowledge in physics, but rather because they brought in to this field of biology, the attitudes which a physicist has and an essential feature of this attitude is a conviction that if something exists, we ought to be able to explain why it ~~walks~~ works the way it ~~walks~~ works.

This conviction that ~~it's~~ if there is some mystery, you can solve the mystery if you only investigate it properly, comes from the success which you experience in physics. You are confronted with an odd phenomenon and after a while you understand it. It is this kind of experience which brings this conviction; you go through this experience when you begin to go to a university where you are - particularly ~~when~~ if you go into a university in a country like Hungary where they don't overfeed you with knowledge, you more-or-less have to invent, yourself, the solutions to the problems. The problems - you quickly see what the problems are, but in a country like Hungary where they don't teach you too much, you have an opportunity to find the solutions yourself, before you are told that somebody else has already solved that problem. And through this process, you grow and you experience your own strength and you get self-confidence and you end up with the conviction that there is no problem too difficult, too incapable of a solution.

But I didn't go into biology until after the second world war. And during the second world war, it was already clear ~~decreed~~/~~to~~ that atomic energy will become big business, that physics will be very important, that there be change in physics, that much of physics will go into the building of big accelerators, a rather different kind of life for a physicist from the life to which I was accustomed, and I remember that during the war we had --oh, in the Manhattan project, we had once-a-month, a meeting where one of the members of the project talked to the rest of the people, ~~it was~~ usually to several hundred people, and on this particular occasion, Fermi mentioned that he had repeatedly thought that he would go into biology, and before the war he had already set himself some sort of a date on which he might want to make the change, but now he was no longer certain when he would do it or whether he would do it at all. And I said ~~when he was discussing~~ then in the discussion that ~~it~~ with me it is just exactly the other way around, that I had vaguely thought of going into biology but I had never been able to set a date before, but now I think I know when I am going into biology, - when the war is over, and all the big companies who had some role in the development of atomic energy ~~would~~ will advertise, ~~but all~~ the role they had played in the field of neutronics, that if this happens, then I think that I will feel the time for me has come to leave physics and go into biology. When the war was over, I was appointed professor of bio-physics at the University of Chicago before I had done any work in biology, at all, and I -just on a hunch - I called up one of my colleagues, a physical chemist, Dr. Aaron Novick, who was at that time working in the Argonne National Laboratory, and I don't know why I thought he might be interested, I just picked up a phone and said, "Look, Novick, I have decided to go into biology and what I want to do is this - I want to move into micro-biology and start taking a few summer courses so I ~~can~~

22 learn the technique, I believe this field is a very fertile field for the kind of people who are trained in the exact sciences, because most of the people who have been working in it come from those branches of biology where ~~a~~ quantitative determinations are not usual, and how would you feel about coming along?" And he said, "I would like that very much," so I said, "Well, how much do you

23 earn?" What is your sal, and he said how much it was, and I said, "Well, no, I can't do that from my budget but I can pay you about a thousand dollars less. Will you come?" and he said, "Yes, I will come." And so Novick and I started out together in biology and worked together for about five years. He is now Director of an Institute of Molecular Biology at the University of Oregon in Eugene, Oregon. Dr. Aaron, A-Aron Novick. Well, the experimental work was in molecular - in - for a number of years I

worked on micro-biological problems together with Dr. Novick, but then the Institute in which I worked in the University of Chicago, it was called the Institute of Bio-physics, no, it was called Institutee of Radio-biology and Bipphysics - became wobbly and there was talk that the Institute might be dis/olved. So I was not able to really ~~let him~~ feel that I ought to build up a staff and bring in young people # to whom I couldn't promise any advancement, ~~you see,~~ if the Institute vanced ~~were~~ in fact disaolved, and this was the time when I began to work on problems which were more theoretical problems where no new experiments were immediately needed, at least in the beginning or were not, - no experiments were needed, and the first work of this category was the theory of the aging process. ~~classes~~, All mammalians, as you know, go through a characteristic process which is called aging, and well, I can't say that my theory solved the problem. It might turn out that my theory is not right, however, it is the only theory in this field at the moment which is capable of making quantitative predictions. And the second theoretical paper which I wrote was on induced enzyme ~~the Youth and Their~~ Formation, in bacteria, and a paper which grew out of it, is closeley related with it ~~It's called~~, ~~it~~ was published in the same issue of the proceedings of the National Academy, ~~it~~

gave a theory of antibody formation. You know that if you inject an antigen into a rabbit, if you inject a foreign protein into a rabbit the rabbit responds by forming an antibody which can percipitate this antigen. There are a great number of interesting phenomena related to this # very simple fact, and it requires - we need something like a theory of antibody formation if we want to account for a variety of the phenomena which are connected with this, - again connected with this simple fact.

You are asking what's the difference between a scientist and an engineer ~~and I say~~ no. A scientist has to be interested also in the details, just as an engineer, but what a scientist wants to find out is how things work; he wants to understand what are the basic equations, which hold true in nature, where an engineer starts with ~~his~~ these equations and he applies ~~his~~ these equations ~~he knows~~ in order to make ~~ing~~ machinery which will perform the task that needs to be performed. So an engineer takes off where the scientist stops. Science produces basic equations where an engineer applies the equations. Well, the scientist, of course, always works on the frontiers of knowledge. The areas where the answers we already know are of no interest to scientists; it is interesting to him and if he doesn't know how it works, he will read the book which tells him how it works. But this is not creative work. Creative work begins when you know already all that is known and you begin to find out new things by working with the frontiers of that established knowledge. The frontiers of established knowledge.

Around 1945, I found the mysteries of biology more interesting than the remaining mysteries of physics, though there are still plenty of mysteries remaining in physics. But the mysteries which are remaining in physics are mostly - ~~with~~ at very high energies which do not normally occur on earth, so really what you do there, is go to a lot of trouble to create particles which are much faster than anything than we normally can observe here on earth, and so you go to great trouble to observe something that puzzles you and then you try to find an explanation. But, in this area it is not any more so very clear what it means, unless you have explained something because, before this earlier time, in physics, explaining something always meant explaining something new in terms of something old and familiar, the way Newton explained the motion of the planets in terms of familiar phenomena of falling apples. ~~that~~ The apple grows on a tree, falls, and is a ~~planet~~ accelerated in motion, and what happens, the behavior of the apple on ~~the~~ earth is a familiar phenomena which enables Newton to explain the motion of the planets. Now in high-energy physics, this is not so, whatever explanation we mean, the explanation is not ~~cast~~ couched in terms of everyday phenomena which you can observe with the naked eye, in your everyday life. Explanations mean something more subtle there. And just what it means is not quite clear.

You are asking me whether what attracted me to biology was the fact that if you understand the life phenomena, ~~it~~ if you can be of help to your fellow-man. The answer is no. I was interested in biology for the sake of the ~~entrancing~~ intrinsic interests of biology without any reference ~~for~~ to what it might do for mankind. As a matter of fact, perhaps I ought to tell you a story. This was when I ~~was in~~ visited Moscow, I only have been to Moscow once. The first and last time I was there was in November 1960, when I went to attend the ~~B~~ Pugwash Conference, but I stayed over for about a month, because I wanted to have private conversations with our Russian colleagues, in order to understand better what is in their minds, and while I was there, I was invited to address a Writer's Club. There were about 50 people there, Russian writers, and I didn't know what I should talk to them about. So I decided I would tell them something about the development of modern biology, molecular biology, in the United States. This was a very rapid and dramatic development, which took place here and started somewhat before the last war, and in the fifteen years which followed the war, rather remarkable and very fast advances ~~have been~~ were made, and they ~~expressed~~ listened with great interest, ~~in fact~~ and finally, one of them said, "Well, Dr. Szilard, tell us. What practical applications do you see? To what practical applications can these, as you say, major discoveries, be put?" And I said, "That's ~~easy~~ really,. I didn't see any practical applications of these discoveries, but," I said, "this may not mean very much, because if you had asked me in - around 1925, or any time before 1932, what practical application ~~did~~ I see for nuclear physics, I would have given you the same answer, that I didn't see any." Where~~fore~~upon, another Russian got up and said, "Well, in that case, Dr. Szilard, would it not be better if you stopped doing what you are doing right now?" I answered that maybe it would.

Well, there is a fascination ~~in~~ with uncovering the mysteries, but if you know for sure that you are treading on dangerous ground, you probably would stop. However, you never know that, - in advance, until it is too late.

You are asking what is the situation now, that so much of science is controlled by government, ~~and I say~~ the answer is really that ~~but~~ honest-to-goodness science is not controlled by the Government. This kind of science still goes on at the universities, which are not controlled by the Government, and you can get ~~even~~ easily the Government grants for it, but the mere fact that, the Government gives you money for doing what you are doing, doesn't mean it is controlled by the Government. As a matter of fact, I would say that scientists cannot be bribed, because they will willingly accept the money, but they won't deliver the goods.

You are asking whether German scientists during the war were probably interested in atomic energy in this regard. The German scientists did work on atomic energy, but they worked in a rather half-hearted fashion. ~~There weren't really~~ They worked without any clear goal of developing a bomb or anything of this sort. Somehow they took one look at the uranium-carbon system, ~~just~~ did one experiment, and decided that it would not work. Whereas, as you know, this was ~~[illegible]~~ precisely the main line of the uranium work which we adopted in the United States and which culminated in the chain-reaction system that was set up in Chicago on December 2, 1942, and led to the construction of the reactors at Halfort, which made the plutonium that went into the Nagasaki bomb.

Well, you are asking what was my motivation in convincing Einstein that he should write a letter to the President. My motivation was that I, as many of other colleagues who knew Germany were convinced, the Germans are working very hard on setting up a chain reaction and constructing an atomic bomb, and we thought that if we did not do this, we should have no defense against atomic blackmail by Germany.

Well, Germany has got the atomic bomb. It has not got only one, but a large number of them, they undoubtedly could have conquered the world. And they could have done it without firing a shot. All they had to do was drop one bomb on one city and show what it does.

Well, you were asking why I was opposed to the dropping of the atomic bomb on Japanese cities. Well, ~~simple as it sounds~~ is because it was ~~so~~ unnecessary. The war against Germany was won. It was quite obvious that Japan was not going to win the war. Well, it was also obvious that the Japanese must know this, or if they don't know it now, they will know it a month later. ~~Well, you reach~~ Once you reach a situation where a nation knows that it cannot win the war, then you can end the war not - then you don't need military means to end the war, then you ~~don't~~ need - then you can end the war ~~with~~ by political means, provided only that you are willing to negotiate peace. If you demand unconditional surrender, then you can't win the war, because no nation will surrender unconditionally unless it is absolutely forced to do so.

Voice: Didn't you say also that---

S: Yes, of course, one of the important considerations was that it would be a fatal mistake to create a precedent for using atomic energy for purposes of destruction. There was no valid military reason for doing it, and there was every - every other reason for not doing it.

You say that I am able to foresee future political events and you are asking whether this is - what do you say what it is - intuitive----

Voice: Intuitive or thinking through logically?

S: Or thinking through logically. I think that all thinking is intuitive. Thinking does not consist in a sequence of syllogisms. You - something - ~~unless~~, on a subconscious level, create a pattern of ideas which are then projected out on the conscious level, and this is what thinking is. You can later on go in the right on - if this happens in mathematics, you start out by knowing that a certain ~~thing~~ theorem is true, and then you can sit down and begin to work and sometimes you have to work very hard to prove that it is true. And the way you prove it, may be quite different from the way you arrived at it, on the subconscious level. On the subconscious level you may have arrived at it through a series of steps, none of which was really foolproof, and as a result of that, when you are convinced that you have found a theorem you cannot be absolutely sure that you are right. If you are a good mathemetician, you are right in 95% of the cases. If you are not so good a mathemetician, you may be right in 50% of the cases, or even less.

You say, I have been so accurate in the past and what do I see in the future today? Well, first of all, let me caution you you don't get brighter when you get older, and as you get more involved and less detached you - the validity of your judgement may suffer, but right now, I would say that my prediction for the United States is rather grim. I think that more likely than not, this country will come to grief, and this prediction is based on two things. First, the accumulating of stockpiles of bombs, both here and in Russia, will create a situation in which America and Russia will be just about equally powerful within 2 or 3 years, this will take place, This doesn't mean that Russia will have as many bombs as we will have, or as many rockets. We may have ten times as many as they have. But this kind of superiority is no longer relevant when Russia and America are both able to destroy the other nation to any desired degree, and they are able to do it even after they have undergone an attack, an atomic attack. Now, the bombs alone don't go just off, you see, no nation will attack another nation just out of the blue sky. It takes some disturbance, some provocation, before this takes place, and if we had a universally accepted - if we had universally accepted principles of international justice, if we had a code of behaviour which the nations follow, then I think having a stockpile of bombs here, a stockpile of bombs over there, will not really be very dangerous.

But, there is no rule of conduct which we follow and the Russians follow and thereby avoid getting into conflict with each other; we don't believe very much in principles of - of course, no nation can act on principles alone. It is obvious, expediency must always be taken into account by the Government, but we follow an extreme of emphasizing expediency and disregarding principle. You can see that, even - take the case of Cuba, for instance; the President is asked at his press conference whether he would be in favor of instituting a blockade of Cuba, and he will say, no, he is not in favor of that, because a blockade of Cuba would be an act of war, and it would be not in the national interest, at this time, to institute such a blockade. Now, this sounds very satisfactory except it implies that if it were in the national interest - or if it will be in the national interest a year, or two, or three years hence, then a blockade would be instituted even though this is an act of war. You see, there is just no mention of the fact that a freedom of - decision, that we must impose some limitations on our freedom of action, and in a sense, we have done that when we have subscribed to the Charter of the United Nations. We cannot institute a blockade of Cuba in order to bring about the downfall of

Castro, and do this without flagrantly violating the United Nations Charter. But the principle is not so important to us as success. Now, if we were really ~~on the sides~~ omniscient, then we could follow expediency without coming to grief. But the trouble is that when nations follow the principle of expediency, more-often than not, what they do is not expedient, and this is so because it is so easy to base sound reasoning on the wrong premises. And there are just innumerable examples in history how nations came to grief. In the first world war, Germany came to grief because it went into the war - deliberately went into war on the assumption that Great Britain will remain neutral. It was expedient to send the German troops marching into Belgium and the Germans were convinced that England would remain neutral, but when the Germans marched into Belgium ~~unift~~ England intervened. Similarly in the second world war, Hitler acted on the premise that when it comes to war with Russia, within two or three or four months, Russia can be defeated. This was based on his appraisal of Russian military strength, and this appraisal was not so crazy, because the American Government's appraisal of Russian military strength was quite similar and the British Government's appraisal of Russia's military strength was no different. Well, here was a premise, the premise was wrong. If the premise had been right, Hitler would have won the war, but the premise was wrong and the four months had passed. The war was not over. Winter came. The Germans troops were not equipped for fighting a long war during a hard Russian winter, and this is how Germany came to grief. I think that the actions of Governments are more often than not, are based on the wrong premises, and if there is an exaggerated tendency to disregard principles and to act on expediency, it is ######### almost a foregone conclusion that a nation #### which does this will come to grief.

You are asking me if there is a link between creativity and ethical purpose. I cannot answer it straight, but this is perhaps what I should say.

4, Berlin, 1920-1933 (1) and (2) Notes

~~Notes to p. 11~~ (~~Youth section~~)

Béla Kun's government lasted from March to August of 1919.

Szilard's Record Book from 1916 to 1919 from the Josef Technical Institute in Budapest is available. It gives courses, grades, and signatures of faculty.

Transcript sheets from the institute in both Hungarian and German show courses and grades, 1916 to 1919. (Xerox copy of German version attached).

Since 1894 an important academic occasion in Hungary has been the annual Eötvös mathematics competition, open to all university freshmen. Szilard entered this competition in his first year at the Josef Technical Institute.

In his preface to the American edition of the Hungarian Problem Book,* Gábor Szegő writes: "The publication of the problems and the names of the winners was from the beginning a public event of first class interest." Szilard won second prize in the contest in 1916, and so started his public career. (Testimonial attached)

K.W.

* Hungarian Problem Book, edited by G. Hajós, New York, Random House, 1963.

# Übersetzung.

Z. 2570/1920. Abgangs-Zeugniss von der Maschineningenieur-Abteilung der „Kön. Josef" Technischen Hochschule. Herr Leó Szilárd, geboren zu Budapest, im Jahre 1898, hat als ordentlicher Studierender der Maschineningenieur-Abteilung der Kön. „Josef"-Technischen Hochschule in den Studienjahren 1916/17, 1917/18 u. 1918/19, insgesamt während sechs .. Semestern die nachstehenden Vorträge besucht.

| Studien Jahr | Namen des Vortragenden | Gegenstand d. gehörten Vorträge | Note | Anmerkung |
|---|---|---|---|---|
| 1916/17 | Kürschák | Analysis u. Geometrie I. | 6 | — |
| " | Tötössy | Darstellende Geometrie | 5-5 | — |
| " | Szarvasy | Chemie ...... | 6 | — |
| " | Schauschek | Zeichnen ...... | 4 | — |
| " | Bresztovszky | Mechanik I. Cours ... | 6-4 | (II. Semester) |
| " | dtto | Festigkeitsversuche | — | (II. " ") |
| 1917/18 | Rados | Analysis u. Geometrie II. | — | — |
| " | Réthy | Mechanik II. Cours ... | 5-6 | — |
| " | Pfeifer | Chemische Technologie | 4 | (I. Semester) |
| " | Wittmann | Physik I. Teil | 6 | — |
| " | Jónás | Maschinenzeichnen ...... | 6-5 | — |
| " | Rejtő | Giessen u. Bearbeiten d. Metalle | 4 | (II. Semest.) |
| " | dtto | Werkstättenübungen ... | 12 (zwölf) | Halbtage. |
| " | Wittmann | wissenschaftliche Theorien d. Elektrotechnik (Physik II.) | 3 | — |
| " | dtto | Elektrische Messübungen | — | — |
| " | Herrmann | Maschinenelemente I. ... | — | — |
| " | Bresztovszky | Eisenconstructionen | — | — |

Hat dem I. (ersten) Maschineningenieur-Rigoros nach neuer Prüfungsordnung am 16. Juli 1919 gut entsprochen.

| Studien-Jahr | Name des Vortragenden | Gegenstand der gehörten Vorträge | Note | Anmerkung |
|---|---|---|---|---|
| 1918/19 | Bresztovszky | Eisenkonstruktionen | — | — |
| " | Herrmann | Maschinenelemente II. | — | — |
| " | Rejtő | Bearbeitung v. Holz u. Metall | — | (I. Semest.) |
| " | Bánki | Hydromaschinen, Compressoren, Dampfturbinen ...... | — | — |
| " | Rejtő | Spinnen u. Weben faseriger Stoffe | — | (I. Semest.) |
| " | Weissmahr | Elemente der Geodaesie ... | — | (II. " ") |
| " | Herrmann | Hebezeuge ...... | — | (II. " ") |
| " | Sándy | Bauencyklopaedie ... | 5 | (II. " ") |
| " | Rejtő | Werkstättenübungen ... | — | — |
| " | dto | Mechan. technolog. Übungen | — | — |
| " | Zipernowsky | Einrichtung elektr. Anlagen | — | (I. Semest.) |

Anmerkung: „6" ist die höchste, „2": die niedrigste Note.

Sein sittliches Betragen war den Vorschriften der Technischen Hochschule ........ entsprechend. Budapest, den 23-ten November 1920... Rejtő m. p. dzg. Rektor: Dr Béla Bresztovszky m. p. dz. Dekan.

Ford. ügyszám 239/1920.

Hivatkozással a nagymélt. magy. kir.: igazságügyi Ministeriumnak 5194/912 .. sz. jogosítványára, tanúsítom, hogy jelen fordítás a hozzáfűzött eredetivel mindenben egyező. Budapesten, 1920. (ezer-kilenczszáz húsz ..) évi november hó 29én (:huszonkilenczedikén:)


Kraft der mir von Sr. Excellenz, dem Herrn kön.: ung. Justizminister sub Z. 5194/912 .. erteilten Befugniss beurkunde ich, dass vorliegende Übersetzung mit dem angehefteten Original in Allem übereinstimmt. Budapest, den 29-ten (:neunundzwanzig:).. November 1920 (: Tausendneunhundertzwanzig:).—

Díjjegyzék.
ford. és hit. —
bélyeg —
Összesen: —


kir. közjegyző,
kön. öff. Notar.

M-27

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Erklärung.

Ich bestätige, dass der Studierende der Technischen Hochschule zu Budapest Leo Szilárd gelegentlich des Physikalischen Landeswettbewerbes für Studierende (1916) mit dem zweiten Preise durch die Ungarische Gesellschaft für Mathematik und Physik ausgezeichnet wurde.

Budapest, den 14. Oec. 1919 Dr Leopold Fejér m.p. o.ö. Professor der Mathematik a. d. Universität Budapest, Sekretär der Ungarischen Gesellschaft für Mathematik und Physik.

Vorstehende Abschrift stimmt mit der mir in Urschrift vorgelegten Hauptschrift wörtlich überein.

Charlottenburg den 3 Januar 1920

Pakscher

Notar im Bezirk des Kammergerichts zu Berlin



**Kostenberechnung.**

Der Wert des Gegenstandes beträgt 200 M

| | | |
|---|---|---|
| 1. Gebühren nach § 5 der Gebührenordnung und §§ 23. 52 d. [illegible] Kostengesetzes vom [illegible] u. 6. 7. 18 | 2 M | 8 |
| 2. [illegible] (§ 20 d. Geb.-Ord. [illegible]) | " | 80 |
| 3. Stempel frei | " | " |
| 4. | " | " |
| Zusammen | 2 M | 80 8 |

Der Notar.

Pakscher

Justizrat

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~~Notes to p. 12 (Youth section)~~

Nicholas Horthy de Nagybanya. He entered Budapest in November 1919, after Rumanian forces had defeated Kun, following a counter-revolution. He was made regent and head of state in 1920, and remained so until the Germans forced him to resign in October 1944, after he had surrendered to the Russians.

(Columbia Encyclopedia, 3rd ed., New York, Columbia University Press, 1963)

K.W.

~~Notes to p. 15~~ ~~(Youth Section)~~

Szilard's Ph. D. degree was granted Cum Laude by the University of Berlin on August 14, 1922. The thesis was published in 1925: "Über die Ausdehnung der Phänomenologischen Thermodynamik auf die Schwankungserscheinungen," Zeitschrift für Physik, 32:753-788 (Heft 10), 1925.

K.W.

A. Berlin, 1920-1933 (7) Notes.

~~Notes to p. 16 (Youth section)~~

"On the Decrease of Entropy in a Thermodynamic System by the Intervention of Intelligent Beings," by Leo Szilard. Zeitschrift für Physik, 53: 840-856, 1929.

This paper was translated from German into English and published posthumously in Behavioral Science, 9:301-310 (Oct.) 1964.

Szilard's cyclotron patent application was filed in the German patent office on January 5, 1929 (Application No. S 89288 VIIIa/21g). A few weeks earlier, on December 17, 1928, he had filed an application describing a linear accelerator for particles (Application No. S 89028 VI/40c).

K.W.

~~Notes to p. 17~~ (~~Youth section~~)

Szilard and Einstein were joint holders of seven German patents covering pumps, liquid metal pumps, and refrigerator systems using them. These were dated from 1927 to 1930. In addition, Szilard held some dozen other German pump and refrigeration patents, granted during the same period.

K.W.

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# HUNGARIAN PROBLEM BOOK

BASED ON THE EÖTVÖS COMPETITIONS, 1906–1928

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