4. Berlin 1920 - 1933

Stenotape 1963 tape
(Rec'd by Carl Eckart)

Excerpts from Taped Interview, Dr. Leo Szilard, 1963 (physics.

. . . . . Well, my studying was interrupted by the war. When I was eighteen years old, 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 soldier 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 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.

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 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. 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 Schrödinger. 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 pupil 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.

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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 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 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 laws 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 any 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 than were available up to that time. There had 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 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 refrigerator 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.