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PEOPLE IN THE EARLY DAYS OF QUANTUM MECHANICS

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Talk at the Symposium

It is a pleasure to be once again at this Institute and to take part in the celebrations of the work of Professor Supek and I think, also of his birthday, which from my perspective indicates that he is now entering early middle age. So I would like to take this opportunity to add my best wishes to those which have already been expressed. It seems fitting on this occasion to talk about old times, in fact even little older than the 50 years that Professor Supek has been active, so I will tell you a little about what I remember of the very beginnings of the quantum mechanics.

I grew up in a suburb of Berlin, and it would be nice to be able to say that I chose physics because I could see that such exiting things, such developments, were happening at that time, but it would not be honest. In fact, I wanted to be an engineer, which is natural for a boy growing up in a technological environment, when airplanes were new, radio was new, the telephone wasn't that old. But my parents decided that I was not fit to be an engineer, probably just not very good reasons. I listened, and I chose then for the next best thing, which seemed to be physics. I didn't know much about what was going on there. So I entered the University to Berlin, which was my home town, because I was considered too young to leave home. There I listened, amongst others, to the lectures by Max Planck, which were the worst lectures I ever sat through. He would read from one of his books, and if you had the book you could sit there and follow it word by word. Of course, we knew he was very famous, but we didn't know what he as famous for. The earliest indications I had of something new happening that I hand't heard of at school appeared in lectures by Bothe in a course called X-ray physics. Words like the Bohr atom and the quantum of action, and so on, appeared for the first time.

After a year of study in Berlin, I decided I was old enough to leave home, and I went to Munich, to study under Sommerfeld, who was then probably the best teacher of theoretical physics in the world. Figure 1 shows Sommerfeld on the left talking with Pauli. Pauli looks in this picture very respectfull and modest, which was unusual. But that happened always when he was talking with his old professor;

Fig. 1. Arnold Sommerfeld and Wolfgang Pauli.

he called that his Schülerkomplex. Sommerfeld, as you see, was rather short and looking important, and had a big moustache. In fact, we used to call him "the upper half of Hindenburg". And from his, here in the picture slightly pompose appearance, you could believe that he had the title of "Geheimrat", which was a good German courtesy title for academics and other intelectuals. He liked to use it. There was a story that an American visitor on his first encounter with Sommerfeld didn't know this, and he called him Herr Professor. But then, on the next occasion (after it had been explained to him) he said Geheimrat, and Sommerfeld said "Your German is making rapid improvements". But as teacher he was different. We never used the title Geheimrat in his department and he was very approachable. Figure 2 is a picture of him on skis, and you can, I think, see that he was not the world's best skier. This was near the hut he shared with his chief mechanic. He occasionally invited students up there for weekends, and that's

Fig. 2. A. Sommerfeld on skis.

how I came to take this picture. As you can see also, he doesn't look very much like a Geheimrat. He was an excellent teacher of undergraduates, and he never let you forget that theoretical physics was essentially an empirical subject, and his books, in which he wrote down his lecture courses, are still very usefull today. And also, in research he had a talent for picking problems which were interesting enough to spend some time on, and yet easy enough for inteligent graduate students to make some progress with them. He was very good at mathematics, he loved it but he was not pedantic. I remember one occasion, when lecturing about the then new electron theory of metals, he dropped a factor of two quite early in the calculation.

But it didn't seem to matter, until he came to the result which was something well known, I believe it was the Wiedemann-Franz law, in which the numerical factor was well known. We could see he as approaching an answer which was wrong by a factor of two. Eventually he saw it himself and without a moment's hesitation he said: "And then we must remember there are as many electrons going from left to right, as from right to left."

And that was also the occasion when I really started to learn the new quantum mechanics which then had just begun. In Munich we followed the wave mechanics, Schrödinger's approach, which to Sommerfeld was much more concrete and also dealt with differential equations with which he was a master. That was my first occasion to give a seminar. He said to me, there are these papers by Dirac and by Jordan on transformation theory, which we haven't yet understood. Could you read then, and perhaps explain them to us. This was quite a tough assignment for a student of two years standing. But I managed somehow. I learned a lot from that exercise, but whether the audience learned something is hard for me to say.

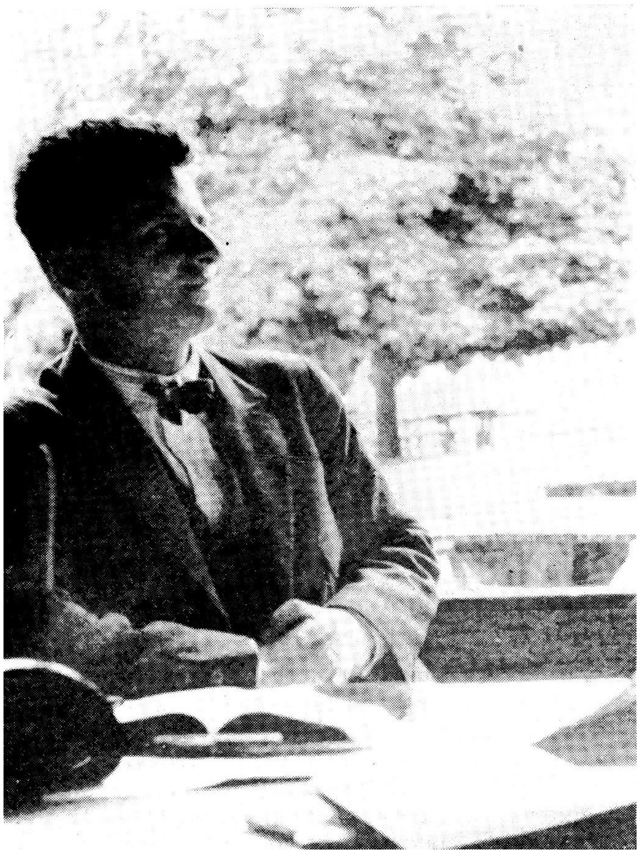

Fig. 3. Hans Bethe.

There were many other interesting people in the department. Figure 3 is a picture of Hans Bethe, who was a year older than me, a year senior to me, and that seemed a very big difference. He seemed very wise compared to me, and I could learn a lot from him. Well, we remained good friends and today he is still a year older than me which now makes rather less difference, but I can still learn a lot from him.

I would have continued there, but, after I have been in Munich three semesters, Sommerfeld went on a sabbatical leave to America, and sent me to Heisenberg in Leipzig. Heisenberg was a very different person from Sommerfeld, certainly nothing like a Geheimrat about him, and he also had a very different attitude to physics. He didn't really like mathematics, it was a tool, a neccessary tool. He never would do mathematics for its own sake. In fact his approach to a physical problem was to look at it, and intuitively decide what was the right answer. And then he would look for a mathematical technique which would give him that answer. Now, that maybe a good method when you have as powerfull imagination as Heisenberg had. It is a little dangerous for other people to imitate. Figure 4 shows a picture of him. There on the right in front is Heisenberg, next to him it's me, in case you can recognize, then above is Gentile, he was from Italy, and George Placzek, a very charming and very wise physicist. Further, Giancarlo Wick, Felix Bloch, Viki Weisskopf and Saurer, the German physicist. Felix Bloch worked in Leipzig and was just then writing his famous paper about the motion of electrons in periodic

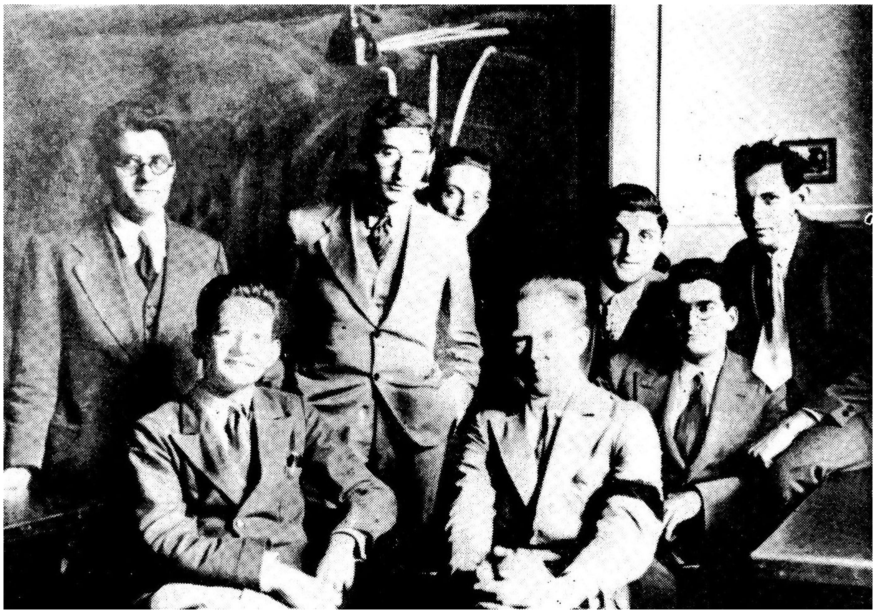

Fig. 4. In the front row are shown Werner Heisenberg (right) and Rudolf Peierls (left), and in the rear row, left to right are G. Gentile, George Placzek, Giancarlo Wick, Felix Bloch, Viktor Weisskopf and F. Sauter.

fields. In Figure 5 he is seen at a conference in Zürich in 1929. That was at the lake, at a bathing place, where we went in the intervals of the conference.

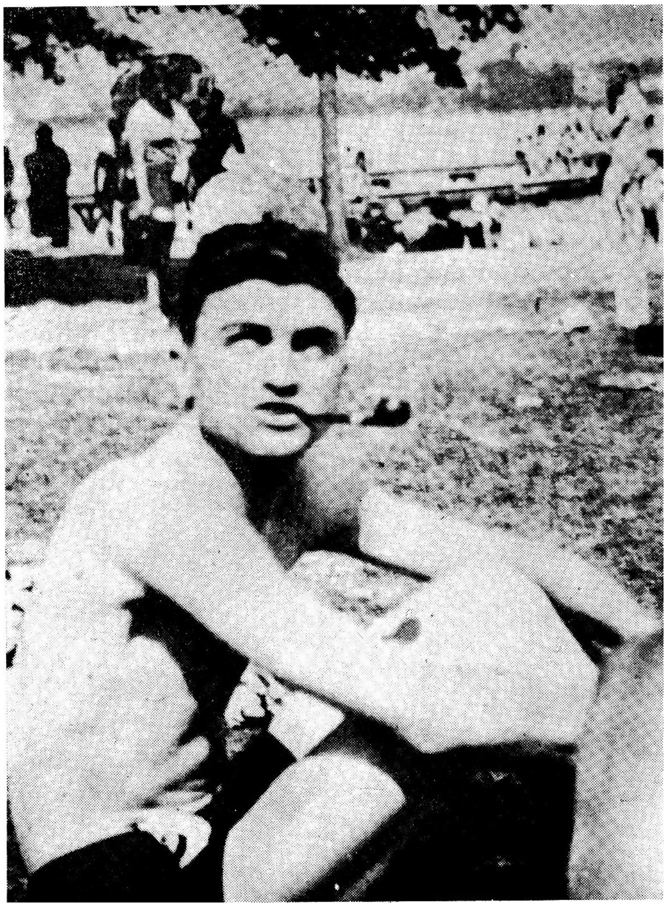

Fig. 5. F. Bloch at the Zürich Lake during a break of the 1929 Conference.

In intervals of the work in the Institute we went downstairs to play table tennis and Heisenberg's ambition to win at table tennis was more obvious than his ambition to be a great physicist. It was a sensation when a Chinese visitor came who could beat Heisenberg at the game. I believe that later on when he was on a tour around the world, on the long voyage across the Pacific, he practiced table tennis every day to make quite sure this wouldn't happen again.

That was the time when I was able to write my first paper. It was to do with the so called anomalous Hall effect. As you know, the Hall effect is the deflection of the electric current in magnetic field. If you pass electric current through a piece of metal with a magnetic field at right angles, then there is a lateral electric potential difference which you can understand because moving electrons are deflected in the magnetic field. But some metals show the opposite sign, as if the current was carried by positive carriers. My job was to try to understand this on the basis of the Bloch theory and indeed one could see that this happened. In modern language this simply means that if an electron band is nearly filled, what determines the conductivity are the empty places, the holes, and they behave indeed like the absence of the negative charges, i. e. like positive charges. So, it was a pleasure to be able to write my first paper.

Then, after I spent two semesters in Leipzig, Heisenberg went on sabbatical leave and on his advice I went to Pauli in Zurich. I think I have every reason to be gratefull for the system of sabbatical leaves and the invitations from the American Universities, because they provided me with a unique combination of distinguished teachers.

Pauli, of course, was a very different person. He was extremely critical and if he approved your answer to a problem then you could be sure the answer was right. I think there are very few mistakes he ever made in his writings. And if you went to him with an idea, then after a little while you came back either chastened, because the idea was wrong, or you were confirmed in your confidence that you had something what was worth pursuing.

Figure 6 shows another picture of Pauli. Here he is in the swimming place

Fig. 6. W. Pauli at the Zürich Lake during a recess of the 1929 Conference.

in Zürich, at the same conference. His was probably not the most fashionable swimming suit of '29. He took part in some of our activities. I persuaded him also to go sailing on the lake. Figure 7 shows Pauli with some other visitors, Robert Oppenheimer, Rabi and another physicist, not very well known.

Pauli was famous not only for his contributions to physics, including the exclusion principle, but also for his hurting remarks to people and there are many of such stories around. I must mention some, at least. You can't talk about Pauli without mentioning some of these. For example, when Landau, the Russian physicist, came to visit Zürich, one day he was arguing all afternoon with Pauli and at the end he said: "Professor Pauli, you are not going to say that all I said was nonsence." "Far from it" said Pauli, "far from it. What you said was so confused, one couldn't tell whether it was nonsence".

He was once visiting another university and in the evening he wanted to go to the cinema. It was his habit to go out in the evening, and when he came back at eleven or so, he would start working. He was doing most of his work during the night. Therefore, of course, he would not get up very early in the morning. It was said that once he was asked to attend a meeting at nine o'clock in the morning, and he said: "Oh no, I can't stay up that late!". Well, on that occasion he wanted to go to the cinema and a local man explained to him how to get to a good film. Next day he asked him did he find it all right. "Oh yes", said Pauli "it was easy, you express yourself quite intelligibly when you don't happen to talk about physics".

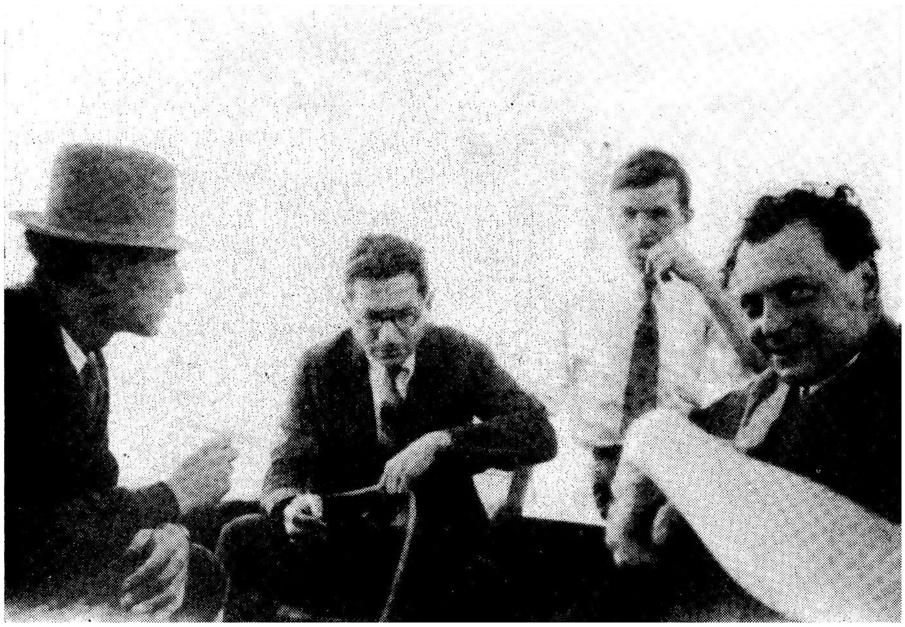

Fig. 7. Robert Oppenheimer, Isidore Rabi, W. Pauli and another physicist (at the rear) in a boat on the Zürich Lake in 1929.

Probably, his most severe remark was to Stueckelberg. He was a very distinguished theoretical physicist, not really adequately recognized. Anyway, on an occasion, Stueckelberg said in the discussion: “Don’t go so fast, I can’t think as quickly as you.”, to which Pauli said: “I don’t mind if you think slowly, but I do object when you publish more quickly than you can think.”

Perhaps his last critical remark was when he was shown a paper by a young theoretician. He looked at it, and shook his head: Sadly, he said, das ist nicht einmal falsch. “That is not even wrong”.

I wrote my Ph. D. thesis under Pauli and then stayed on for three years as his assistant. In that position, of course, you were exposed to that kind of treatment and you had to accept it. People had to accept it, but the funny thing is that none of these people, none of the victims of these remarks, ever resented it. Nobody ever grudged for this treatment. The reason is probably that one realised that Pauli was equally critical of himself, he had the same critical attitude to his own faults and his own ideas. Anyway, a lot of us were exposed to this, but I didn’t suffer too badly. When I started as his assistant he invited me one evening to his apartment and gave me a lot to drink. He claimed that I was drunk, but I wasn’t really. But he looked at me and said: “No, you are not any more interesting when you are drunk”.

So, as I mentioned, one of the visitors to Zurich at the time was Landau, the Russian physicist, who was very young and already a great authority. He had a remarkable capacity for understanding physical problems and solutions. His method of reading a theoretical paper was to look briefly at the problem and at the method and assumptions used by the author. Then he would sit down to solve the problem himself. And if his answer agreed with what was in the paper, then it was obviously a good paper. That, of course, is the way to understand really what goes on, but we are not all capable of handling every paper that way.

Landau was very systematic, not only in physics, but in all matters, including practical problems of life. That extended, for example, to his classing physicists into classes. Bohr and Rutherford were in the first class. Einstein was in the class by himself. And Landau modestly hoped that he might eventually be found to be in class two. And his theories extended to the human relations, to relation beetwen a man and a woman. He called that a situation, and a situation could be satisfactory or unsatisfactory. And when he found in his aquaintance an unsatisfactory situation it would be of course his duty to inform the people concerned. This did not always make him friends. He also had very strong views, for example, about beards. Particularly beards of young people he regarded as a survival from the Victorian age. We had then in the lab in Zürich a young man who in fact didn’t have a beard, but he had very pronounced side-burns. So one day Landau phoned this man’s wife, whom he hadn’t met, and he said: “This is Landau and I’m ringing to ask when you will get your husband to shave off his ridiculous side-burns”.

Figure 8 shows a picture of Landau in company with the well known astrophysicist Ambartsumian, and Bronstein, a very brilliant young physicist who

Fig. 8. Left to right are shown Lev D. Landau, Nina Kannegiser, S. A. Ambartsumian, another scientist (in the rear), Genia Kannegiser (later Mrs. Peierls) and M. Bronstein.

Fig. 9. L. D. Landau on skis.

perished under Stalin, about whom a very nice biography has just been published in Russia. As you can see Landau had a tendency to pull funny faces.

Figure 9 shows Landau on skis. Again, not the most brilliant skier as you can see, but gallantly ready to try everything and to take part in all activities. One of Landau's belief was that theoretical physicists do anything useful only while they are young. I imagine he later modified that view a little when he got older. But there he was, and when the name of some physicist came up, he asked who he was and he was told it was a young theoretical physicist of 26, 27 . . . and where he was, so Landau said: "So young and already so unknown?".

Then, amongst other visitors to Zürich was Gamow, who was already famous for his theory of α-decay and many other contributions to nuclear physics, but also famous for his very pronounced sense of humour. If any of you have come across his books on Mr. Tompkins, which are a sort of a lighthearted and popular approach to atomic physics, you will know what I mean. This came out also when we were on a walk in the mountains. There was Leon Rosenfeld, Gamow and I. When we came up to the top of a mountain with the very picturesque name Piz Dadaint, Gamov pulled some papers out of his pocket, which were the beginnings of the letter to Nature, about some nuclear physics problem. He had left off the last sentence, so he could on this mountain top write it dated from this Piz Dadaint. And he added an acknowledgement to Rosenfeld and to me for the opportunity of working there. Figure 10 shows us on that mountain top.

Fig. 10. George Gamow and Leon Rosenfeld are shown on top of the mountain Piz Dadaint (the third companion, R. Peierls, took the photo).

Figure 11. is a picture of Gamow talking with Pauli. For once, Pauli was right correctly dressed.

Fig. 11. W. Pauli and George Gamow.

In those days, of course, people liked to travel, but there were then no foundations or other bodies anxious to pay your fares to distant places. You had to go under your own steam, mostly sitting overnight in the corner of the third class railway carriage. There were four classes, but the fourth class didn't go on the long distance trains. So it was a pleasure to be invited to a conference in the Soviet Union, in Odessa, where, at least in the country, you were the guest of the organizers. So I went there. Figure 12 shows us on the beach in Odessa with Pauli in the swimming suit that we had already met, talking with Frenkel and Tamm, one of the most charming theoretical physicists, Franz Simon, the low temperature experimentalist, and Simon's wife. There is also the little Frenkel, now a professor of physics. You may wonder that we seem to spend so much of our time in swimming suits and near the water, but that's understandable, because first of all that's

Fig. 12. Left to right are shown Charlotte Simon, son of Frenkel, W. Pauli, Ja. I. Frenkel, I. E. Tamm and Franz Simon.

a good way of talking about physics informally, but also it is more likely on those occasions that you have a camera handy then in the lecture room.

Another place to which physicists liked to travel included, of course, Copenhagen. Niels Bohr was one of the most charming of physicists at heart, in addition to be one of the most distinguished. You can see him in the picture shown Figure 13. He is in the garden of his country house in Tisvilde. I am sorry, this photo is technically very poor, but you can just see Bohr there holding a football. Further, you can see Pauli who turned the back to us, Heisenberg, Gamow and Landau, and the rest are little Bohrs, including Aage who became his father's successor and a Nobel prize winner. Picture shown in Figure 14 was taken on the same occasion, but is slightly better. Bohr and Heitler are having a serious talk, so it shows you that one didn't only play football there. Although football was important, Bohr was very good at it, but not as good as his brother, the mathematician Harald Bohr, who was of almost professional standard. There was a story that Harald Bohr was sitting in the tram car with his mother. They didn't talk, so it was not obvious that they belonged together. When he got out first, one of other passengers turned to the mother and said: "Do you know who's been sitting next to you, the famous footballer Harald Bohr".

Figure 15 shows a picture of Heitler and Rosenfeld in the lunch room of the Institute, obviously talking about an important problem in physics. Bohr was an extremely kind person, but also not willing to alow any statement to pass if it wasn't

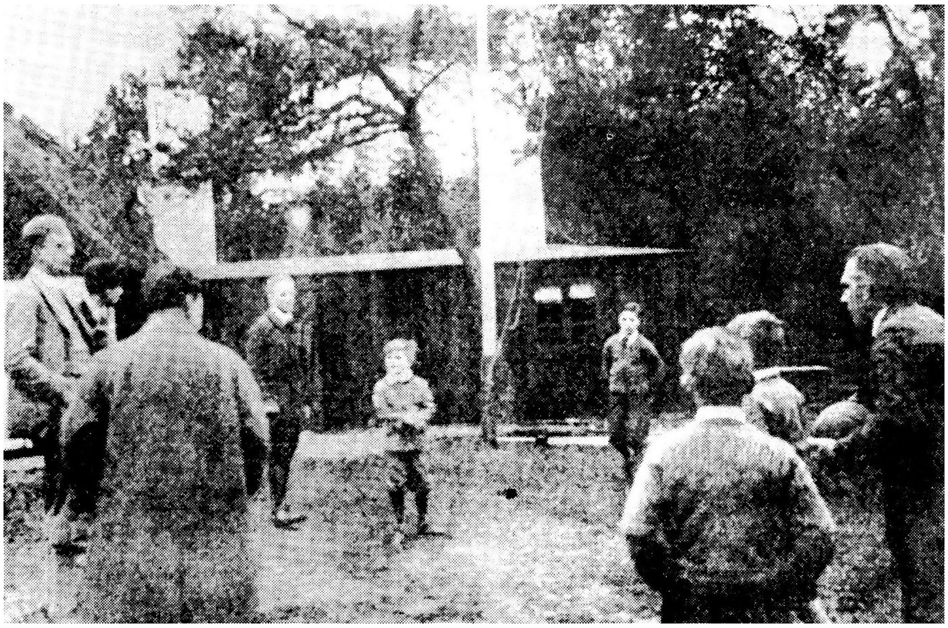

Fig. 13. Niels Bohr (shown at right) holding a football and his four sons in company of (left to right) G. Gamow, L. D. Landau, W. Pauli (with his back turned to camera) and W. Heisenberg.

completely accurate. And this combination, that he didn't like to hurt people's feelings with his insistence on accuracy, led once to the memorable remark, "I'm not saying this in order to criticise, but this is sheer nonsense." He was fond of saying that truth and clarity are complementary. Meaning, that if you want to say something that is very clear, than it can't be quite accurate. Or, if it is to be accurate, than it has to be involved in so many conditions and clauses that you are losing clarity. In writing his papers he tended to go to the limit of extreme truth, so his papers, as many have found, are not always very easy to read. They had very complicated genesis. He would start dictating the first draft. He liked to walk around when he thought of writing and so he always got somebody, some visitor or some other physicist to write things down to his dictation and then when this was crossed out, and rewritten several times, it would be typed and then, in the typed form you could recognize better what it looked like, and there were still changes and it had to be retyped several times, and then eventually it went to the publishers and went sometimes through twelve sets of proofs before Bohr was satisfied. Apparently, his attitude to other things was similar, as shown by the story that he once walked around the site where the new building of the Institute was going up and the old foreman, who knew Bohr very well, said: "Professor Bohr, you see that wall over there, if you want to move it again, you better be quick, because in a few hours the concrete will have set."

Fig. 14. N. Bohr and W. Heitler at Bohr's place.

He, of course, also tended to be somewhat absent-minded and he would in those days smoke his cigar and always, when he started, he would say: "Have you got a match?", and somebody would produce a box of matches. He would light his cigar and put the box in his pocket. Then after a few minutes, when the cigar had gone out, he would say: "Have you got a match?" and so we came always to these discussions armed with a good supply of matchboxes.

I also treasured for a long time a piece of chalk which was blackened at one end. Obviously he had his cigar and the chalk in one hand, and in trying to light this cigar he applied the match to the wrong object.

But, he was also very concerned with other people. This was the time when Hitler's regime began to dominate Germany and many physicists were losing their jobs. So these conferences in Copenhagen became the occasions not only to find out what was the latest in physics, but also where were the best opportunities for finding jobs or survival for the displaced physicists from Germany and later from Austria.

Fig. 15. W. Heitler and L. Rosenfeld in the lunch room of the Theoretical Physics Institute in Copenhagen.

Another important person of those days was Fermi, who is seen in the picture in Figure 16, again at the '29 conference, talking with Kronig. They make a rather nice pair. Fermi was a physicist with an outstanding command of theoretical physics. "When you went to ask him about a problem, the chances were he would take from the shelf a notebook, and turn to a page where he had worked out the solution to that particular problem you were asking.

But as you know, Fermi also became a leading experimentalist, pioneering the use of neutrons for nuclear physics. In doing that he made important discoveries. But he missed discovering fission. He did bombard uranium with neutrons, but he insisted on interpreting the activities which resulted as due to transuranic elements. And when in fact Ida Noddack, a German chemist, pointed out the possibility that this might be fission, he refused to accept it. It is quite interesting to speculate what would have happened if, as was quite possible, he had discovered fission several years before it was in fact discovered, so that all the discussion about this could have taken place before the Second World War started. But that's an idle speculation.

The following story about Fermi doesn't relate to the period I am talking about. Much later he was present at Los Alamos, at the first test explosion of an atomic bomb. Of course, everybody was anxious to know what had been the power of the explosion. There were many instruments for that purpose, but it took time for the

Fig. 16. Enrico Fermi and R. de L. Kronig at the 1929 Zürich Conference.

measurements to be evaluated. Fermi was the first to have a good estimate and he did that by arming himself with a few little pieces of paper. When the blast from the explosion reached him, he released the pieces of paper and watched how far they were carried by the airflow and from that he could determine the intensity of the blast wave, and that in term gave him an estimate of the energy. I do not know whether to admire him more for his ingenuity of thinking about this method, or for his control in letting go the pieces of paper at the right moment. I think, if I tried that I would have let them go either too early or held onto them too long.

One other nice story about Fermi is in connection with the work on atomic energy. There was a meeting of a committee, where there were also some military people present. There were some discussions about how a certain experiment would turn out, and Fermi said that it would be a miracle if it gave a positive answer, and some general present asked him: “What do you mean by a miracle?”. “Oh,” said Fermi, “anything that has a probability of less than twenty percent”.

I spent six months working under Fermi in Rome, when I had a Rockefeller fellowship. Of course, in those days the salaries of scientists were not very generous. Generally, the prospect for science wasn’t even as good as it is today, although we complain today about our chances. At the time the universities were generally static, they did not expand, so there were vacancies only when somebody resigned, or retired, or died, and you didn’t expect that your Ph. D. would assure you the possibility of getting into a research career.

And even when you could get a position, the rewards were not very generous. In fact, when we were in Rome, we discovered what was the level of a professor’s salary in those days and we asked our friends how could they manage to support a family on this kind of salary. And the answer was: “Well, if you want to be a university professor you must have what they called “un piccolo patrimonio”, a small private fund. Although Fermi, I believe, did not have one, he somehow managed to live on his salary.

Well, after spending six months in Rome I went to Cambridge in England. I had decided to divide the year of my Rockefeller fellowship between Rome and Cambridge, following the example of my friend Hans Bethe who had done the same thing. Althought he had decided to spend the winter in Cambridge and the summer in Rome, I thought it was better to do it in the opposite order. Of course, my main contact in Cambridge was Dirac. Figure 17 shows a picture of him on the river. Dirac is with Chandrasekar the astrophysicist, and Viki Weisskopf who is pushing the boat. That’s the so-called punts which are used on the river there, which are very flat boats because the river is shallow and you push them forwards with a pole. It is obvious that Weisskopf was very pleased having mastered this art. It is an art, because standing at the back of the boat makes a rather unstable situation, because the boat tends to go the wrong way, so you have to be clever.

Now, about Dirac there are also many stories. First of all it’s believed that he was very silent and woudn’t speak, but that isn’t quite true. He could be very articulated when he had something to say. He didn’t like to make idle conversation. There are many stories illustrating this. One of my favourite ones was told by Mehra, the historian of science who as a young man came to Cambridge and was anxious to meet Dirac. So, a friend arranged for him to have a dinner in St. John’s College where he was placed next to Dirac. There was silence and Mehra felt obliged to say something, perhaps about the weather. So he said: “It is very windy today.” Dirac didn’t answer but he got up and went to the door. Mehra thought already he had somehow offended him. Dirac opened the door, looked out, came back, sat down and said: “Yes”.

Another nice occasion was when he was walking with a colleague in Cambridge, and the colleague had something in his pocket that made a noise. He said: “I’m

Fig. 17. In front are shown P. A. M. Dirac (left), S. Chandrasekar and (in the rear, pushing the boat) V. Weisskopf.

sorry about this noise, I have some bottle of pills in my pocket, and I took some because I have a cold, and so the bottle is no longer full and it rattles". Dirac considered this statement for a while and then he said: "I suppose it makes maximum noise when it is half full". When I heard the story I was intrigued because I didn't know whether this conversation took place before or after Dirac developed the hole theory. Because I thought, it might have given rise to that idea. But I discovered later, it occured really after the paper on the hole theory.

There is another characteristic story about Dirac. He once came to dinner in a friend's house, but the hosts were late. So he was shown to the sitting room where there was the old grandmother sitting there and knitting. Neither of them said anything for a while and Dirac was just watching her knitting. Then he said: "There must be two ways of doing this". Now, any woman who can knit knows there are indeed two ways, but Dirac decided this entirely on basis of topological grounds, just by watching her.

I have one more picture with Dirac, Figure 18. Here he is with my wife and me in the open-air zoo near London. He was very kind, he knew that we didn't have a car, so he took my wife and myself out on this excursion. And you see, he is taking very seriously the question what animals we should look at.

Fig. 18. Left to right are shown P. A. M. Dirac, R. Peierls and Mrs. Peierls in the Whipsnade zoo.

I think, I used up the available time. I also come to the end of what might be resonably described as the early days of quantum mechanics. So, I better stop at this point.