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Hendrik Casimir - Session I

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Interviewed by: Thomas S. Kuhn, Leon Rosenfeld, Aage Bohr and Erik Rudinger
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Abstract:

Part of the Archives for the History of Quantum Physics oral history collection, which includes tapes and transcripts of oral history interviews conducted with circa 100 atomic and quantum physicists. Subjects discuss their family backgrounds, how they became interested in physics, their educations, people who influenced them, their careers including social influences on the conditions of research, and the state of atomic, nuclear, and quantum physics during the period in which they worked. Discussions of scientific matters relate to work that was done between approximately 1900 and 1930, with an emphasis on the discovery and interpretations of quantum mechanics in the 1920s. Also prominently mentioned are: Homi Bhabha, Niels Henrik David Bohr, Paul Adrien Maurice Dirac, Paul Ehrenfest, Albert Einstein, Walter M. Elsasser, Enrico Fermi, Ralph Fowler, George Gamow, Samuel Abraham Goudsmit, Walter Heitler, Hendrik Anthony Kramers, Lev Davidovich Landau, Hendrik Antoon Lorentz, Walther Nernst, Wolfgang Pauli, Rudolf Ernst Peierls, Max Planck, Ernest Rutherford, H. Schüler, J. Solomon, Otto Stern, George Eugène Uhlenbeck, and B. L. van der Waerden.

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Casimir:

I had very little in the way of documents. I am not that sort of a man; I don't keep files; I usually don't keep letters very long and so I'm afraid, — I had one or two letters written by Pauli, but they were much later; I sent them to Mrs. Pauli. I don't think I have any letters of Ehrenfest left, with the exception of a letter Ehrenfest wrote to Bohr to introduce me. At that time, of course, I wasn't allowed to see it but then did see it much later. It naturally wasn't very relevant to the development of quantum mechanics. So I don't think that I have any concrete material; the only thing I could have is certain recollections, certain indications — perhaps I could indicate some sources where, I think, you might find something.

Kuhn:

I would be very grateful for anything of this sort. I think that we might very well sort of go through it roughly chronologically. Could we start with how did you get into the sciences in the first place, with respect also to interest in this sort of work in Holland in general?

Casimir:

First of all is the question of why I chose theoretical physics. It is a little bit difficult to say; some people have a very pronounced gift in one direction, but during my school years I often hesitated between different subjects, not being "outstandingly good" in one particular field. Sometimes I thought that I would study languages; and sometimes I thought that history would be interesting, and then I veered toward science and mathematics. I had, first of all, a very good physics teacher at school, and that always helps. There were a few other factors. One thing was that my father, who was a rector of a large secondary school in The Hague, had a very great admiration for Lorentz, and also knew Ehrenfest personally very well. It was a rather progressive school and Ehrenfest was very much interested in modern methods of education. Also my father was appointed as a part-time professor of education at Leiden. Now, this was an unheard-of-thing at Leiden, because one didn't consider education at all a subject for a university. And when you have these part-time or extraordinary chairs, a small board of what we call 'curators' or supervisors has to be formed for this particular chair. Lorentz was so kind as to cover this enterprise with his authority,so he was in the first committee dealing with the special chair for education and psychology at Leiden, for which my father was extremely grateful, because he was a little bit regarded as an outsider at Leiden University. And the fact that he was, so to say, somewhat protected by the authority of Lorentz, which was unassailable, of course, helped him a great deal. So he always 'held up to me' that to be a theoretical physicist would be about the best thing you could do. Another thing was that I felt attracted toward physics and also that I was not very practical and not very good with my hands, so it had to be theoretical physics.

Kuhn:

Did you see anything yourself of Lorentz and Ehrenfest?

Casimir:

Lorentz, no. I met him later but we will come to that later in the story. Ehrenfest mainly came to our home when I was still very young; later he lost interest or perhaps the school my father vas running was going a little bit too much into a conservative pattern for his taste. I remember him from when I was a child. One of the things was that he would always drop in unexpectedly and if there was no one at home he would collect my sisters and myself around the piano and start playing children's songs and would have all the children singing with him. This I remember very distinctly: Ehrenfest hammering away on the piano, Dutch children's songs and all of us singing with him. That is an early recollection. I don't remember much afteward, although he may have been there occasionally. Then, when I intended to go and study physics, the first thing I did was to go and see Ehrenfest who, of course, received me well, knowing my father well. I also remember our first conversation. He said, "You should not study theoretical physics but become an architect." I was rather surprised, but it was one of his pet theories that special gifts and aptitudes should reveal themselves early in life. He said, "I remember you as a kid, and the one thing you were always playing with was little bricks and wooden blocks, building towers and houses and so on. It was obvious to me that you should become an architect." Then he started inquiring whether I had done anything in physics. And I said that I really hadn't, apart from what you had to do in school and always getting high marks without much trouble. But that wasn't sufficient; he said "I don't think you're going to be a theoretical physicist, in any case not with me. But you better talk with one or two of my people." So he introduced me to Goudsmit and Uhlenbeck. I remember going to Uhlenbeck who started to explain a little bit what theoretical physics was about; and I went to Goudsmit who told me a little bit about the recent success of Uhlenbeck and himself with the spin of the electron. This was 1926, summer of 1926; the final examinations of secondary schools are always in July and when I had passed that examination, and, so to say, before deciding whether I would go to Leiden, my father said that I had better talk with his old friend Ehrenfest and he sent me to Uhlenbeck and Goudsmit, and that was that. I said that I should like to try anyway, and so that was the beginning. The choice you make of your subject at Leiden comes only after a first examination, the so-called candidates so it was not particularly urgent. — And that was how I started studying physics at Leiden.

Kuhn:

Apparently you had a variety of interests of which physics was only one; did you think seriously of not doing physics after you had this initial discus sion with Ehrenfest?

Casimir:

No, no. I thought that in any case I would do physics. I also thought that I would be able to do it. Also, coming from a school-teaching family in those days, the possibilities for a physicist were not extremely great but you could always get a teaching position at a secondary school, and at that time I thought I would be quite satisfied with that. I liked the subject and I had no objection against becoming a school-master. So "I'll take physics anyway and we'll see how it will work out" and so that was that. It was two or three years at Leiden. before you took the first examination. I took it in the summer of 1928, two years later. And until that time mainly you had just fairly element work — fairly elementary mathematics and all that sort of thing — which kept you more or less busy. However, the number of students then was very limited; let's say that the number of mathematics, physics and astronomy students together was in my year perhaps 12 or so. Ehrenfest, although his teaching duties only started when people had passed this first examination, always took a keen interest in the young people. He was pretty well the only professor who really took an interest in them; he always kept his assistant quite busy keeping an eye the young people, giving informal lectures to them, which were not in the curriculum, on recent advances in physics, having the young people themselves give talks on fairly new things, having them look at the journals and see what was appearing, and all that kind of thing. There was an institute of theoretical physics and since there were so few students there, the library and reading room was a kind of center for all the students in the department who would come there and read things there and have a look at new books and new periodicals coming in and so on. The first year I was there, uhlenbeck was Ehrenfest's assistant. You know Uhlenbeck, of course, and you can understand that he did this remarkably well, organizing small informal discussion groups, and all that sort of thing. I remember that he himself gave talks on, I think, spectroscopy and the vector model, if I remember rightly, but I wouldn't know for certain. In any case, one was brought into contact with new things; Ehrenfest himself would from time to time give this sort of very informal talk to young people to get them interested. I remember that I had not been there very long when I myself gave a talk, in one of these discussion groups, on collisions of the second kind, which was then a fairly new phenomenon. It was rather well recieved, and after that, I was admitted to the famous Ehrenfest colloquium. I think that this was in the middle of my first year already. I was, of course, highly flattered. It really was a most interesting center, where you got to see, I would say, most of the prominent physicists of the day and could listen to discussions and so on. And Ehrenfest always managed that colloquim in such a way that everyone got something out of it. It was really a quite remarkable institution, you, Léon, will remember also, in the early days. As a colloquium it was quite unique. It was of course very much a creation of Ehrenfest's personality; he could make life very tough for his speaker — he disliked any form of bombast, you see — and if he felt that a man was covering up his ignorance with big words, he would just tear him to pieces. With younger speakers, if they spoke badly, he would just take the job away from them and he would tell the story. Certainly it was always extremely fascinating. You had to attend every time; if you didn't, then you were taken off the list. Well, once or twice it could be done, but you would have to have a pretty good excuse.

Kuhn:

How many were there at the colloquium?

Casimir:

At that period I would say perhaps 20, 25, something like that. You certainly learned there how to give a lecture, even how to use a blackboard. There was one thing there; if there was a famous invited speaker, then he had to write his name on the wall; therefore on that wall there was a beautiful collection of famous names: Bohr, Einstein, Schrodinger, Max Plank, Dirac, and Heisenberg. Now that whole institute has been torn down ; it is a pity. But the wall has been well preserved. Incidentally, but that's a bit later, on that wall among the great names you find one deep scratch which is still there. And that deep scratch in the wall has a curious history. It was made by a rather poor student of physics who had failed an examination or pre-examination with Ehrenfest. The student was also rather given to drinking; when he had to come to Ehrenfest he thought he had better get some courage so he came reeking of Bohls at 9:00 o'clock in the morning.Ehrenfest was very strict teetotaler and also a man with a very sensitive nose and so the student, also not having answered the first two questions to Ehrenfest's liking, was kicked out of the room and told that he might try again next year but not before. The man was so mad that he went to that wall and among Bohr and Einstein and the others he wrote his name, which was (Mirlo), and then made a very deep scratch below it with his pocket knife. I was Ehrenfest's assistant at that time, and I found his signature there. It would have been just too bad to have Ehrenfest see that his sacred collection of names had been defiled in this way so I very carefully wiped off the name. But after all, I didn't have anything like plaster of Paris so I couldn't fill up the scratch, and I don't think that anyone did afterwards. The name disappeared but the scratch —. Afterwards the man went to Amsterdam and I understand that during the war he did rather well in the resistance movement and was shot by the Germans. So the scratch on the wall is kind of a monument to an unknown soldier. You should get a photograph of the wall of names. It is interesting because the dates are always added on which they gave their colloquium lecture so therefore it is a certain time scale: 'when did Dirac come to Leiden? when did Max Planck lecture in Leiden?' Einstein came very often. Of course, or fairly often. Einstein was even a kind of honorary professor at Leiden, you see, being a great friend of Ehrenfest. And I think there was a period when he came a few days every year to give lectures. I remember having seen him at Leiden at least on two occasions. one thing I remember, it was either in the winter 1926-27 or the next year,when Planck came to leiden as a visitor and Kramers gave a lecture. I don't remember if Planck gave some lectures also, but Ehrenfest thought it would be a nice idea to have some discussions on new quantum mechanics. That was then still very new, of course, and the interpretation was very new at Leiden, so he asked Kramers to give a lecture. I think that

Kramers was then still at Copenhagen, but he was, more or less, in the process of moving from Copenhagen to Utrecht. Do you remember exactly when Kramers was appointed at Utrecht?

Kuhn:

Heisenberg came here [Copenhagen] to replace him in, I think, 1926-1927; late in 1926 would be about the last that he was here.

Casimir:

Maybe then he had just arrived at Utrecht, although I have a feeling that he had not yet settled down at Utrecht, but I don't know for certain. Well, Kramers started to explain the ideas about wave-packet and about probability and the first discussion of uncertainty principles and that sort of thing.

Kuhn:

That's not 1926-1927, because the uncertainty principle itself comes well along in 1927, so it would have to be 1927-28.

Casimir:

Right, so it would be 1927-1928, my second year at the university. Then he must have been at Utrecht already but still in very close contact with Copenhagen. There are a few things I just remember. I remember Kramers' opening sentences: "Ich fuhle mich heute wie ein Kind, das seinem Vater sein Spielzeug zeigt. Ich warte schon, er wird schon horchen (eben weil er ein Vater ist.) Kramers had his misgivings about the quality of the work and so on. I think it was also on that occasion that Ehrenfest asked Planek whether he didn’t regret the more deterministic theories of classical physics. And Planck very courageously said, ‘No, because I cannot regret things which I have seen to be wrong.” I don’t remember whether he said, “Sachen von denen man eigesehen hat, dass sie falsch sind, kann man nicht bedauern,” or whether he said, “Sachen von denen man eingesehen hat, dass sie falsch sind, soil man nicht bedauern”; perhaps it was the second thing, I don’t know for certain. Incidentaily, in my first year, I also went, a little bit brazenly, to a few of the famous “Monday Morning” Lectures of Lorentz. I remember that Lorentz then was lecturing on the classical theory of the spinning electrons. I think that there is even something in his collected works about that, where he had taken up the work of Uhlenbeck and Goudsmit. I don’t think I could really follow it in detail then, I was a very young and very shy student, so I was slightly taken aback when I went to the lecture and Lorentz had the habit in that lecture of always introducing himself to all the people who came to that lecture, and it was a bit of a shock to me. He was very kind and referred to my father, whom he knew well. It was remarkable; I think that even later in life, Lorentz, if he had become acquainted with someone in that way, would never forget him afterwards. He had apparently a prodigious memory and always knew everyone he had seen once without mixing them up. That must come in very handy. I wish I had that kind of memory. I'm terrible at remembering people. Now let's go back to quantum mechanics.

Kuhn:

Let me throw in one question before we turn more particularly to quantum mechanics. Clearly, if you were in the colloquium by the middle of your first year, you were learning an awful lot of physics very fast?

Casimir:

Fairly fast, yes, skipping a lot.

Kuhn:

I take it then that you were doing a lot of reading of physics on your own. And I wonder what sort of books that Uhlenbeck or Ehrenfest would have sent a student in your position to in order to learn about physics and particularly modern physics fairly rapidly?

Casimir:

Yes, well, I didn't really start on quantum mechanics, of course. What did I read?

Kuhn:

If you yourself gave a paper in your first year on collisions of the second kind, you were doing something with quantum mechanics.

Casimir:

I think I got that from Franck and Jordan's book, Anregung von Quantenspruengen durch stoesse, which Rosenfeld will remember from his Goettingen days.

Rosenfeld:

Yes.

Casimir:

I don't think I read the whole book, but I read chapters of it; I remember studying Goudsmit's doctoral dissertation which came out a little bit later, and also Hund's book on spectral lines I remember studying rather carefully. I got my Maxwell theory from a very small German book by Clemens Schaefer which is a nice little book, and I read the first chapter of Lorentz' theory of electrons, which is a wonderful introduction. I couldn't date all the things exactly, of course. I worked through most of Jeans' kinetic theory of gases, I remember, and then of course there was a little bit of outside reading; the first thing I read was Sommerfeld's Atombau und Spektrailinien, during the summer vacation of 1926. That was in many ways an eyeopener to a young student and it must have had great influence on many people. I think also that I began reading Weyl's Raum-Zeit-Materie rather early, and although I wasn't able to understand everything, still I got something out of it. And, of course, when one had to prepare such a thing on collision of the second kind, one had Franck and Jordan's book, but they always encourage you to read at least some original papers. Sometime in '27, I think, or it may have been '28, Pauli came to Leiden and gave a lecture on his theory of the spinning electron. I remember especially that Ehrenfest very much emphasized — that must have been in '28, but the date can be inferred from the wall in Leiden — the question: 'How is it possible that you have these equations and that they are invariant? I have always been taught that the only possible equations in physics are tensor equations.' That was then this question of the two valued representations of the rotation group in which Ehrenfest was always very much interested and which played such an important part in the whole development of the formalism. I remember that Pauli said that you can show that they are invariant, "und das stimmt schon; es gibt so ein Art (von Blaettkrusten); das hat Weyl mir irgendwie erklaert." Pauli himself had not perhaps at that time —. Or else he didn't want to speak about it. But it was before Weyl's book appeared, I think, or at least still very early in the days of group theory. For Ehrenfest this was extremely important and I think the work that van der Waerden, for instance, later did on spinors was really directly under pressure that was brought to bear by Ehrenfest who was fascinated by this formal question, and I think it came up for the first time during this Pauli lecture in the coiloquium. During my second year at the University Elsasser became Ehrenfest's assistant and that didn't work out too well; Ehrenfest and Elsasser didn't get along well together. He had admired Elsasser's suggestion, concerning these interference effects very much, but the two of them didn't hit it off too well and Elsasser didn't do as much, I would say, for students and so on, as Uhlenbeck had done, although he could have been very useful in many ways. One weakness of Ehrenfest's way of doing things was that people didn't get much training in mathematical and computational techniques, and also he very often looked at the formal structure of the formula rather than at quantitative things. He had a famous habit of writing 4π in quotation marks and that could be 4π or 1/4π or perhaps $4\pi/c^2$.

Kuhn:

Now that's an Ehrenfest story I hadn't heard before; that's quite leading and interesting.

Casimir:

Yes, the 4π in quotation marks. And, as a matter of fact, I don't think that he himself had a very outstanding feeling for orders of magnitude. When I came to Bohr, one of the things which impressed me so much was that Bohr had an almost uncanny feeling for possible orders of magnitude and whether a certain effect might become important or would be small, and also how that would be related to experimental possibilities. I remember Ehrenfest' s saying, "As for myself, I'm not an experimentalist, but I should like to have more feeling for what you can do and what you cannot do." And he said: "I was so surprised the other day when someone had to measure a very small current, and I think that man said, 'I think I will measure it with a quadrant electrometer.' I was completely surprised because I hadn't realized that very small currents are best measured with such an instrument." That is quite remarkable. Elsasser, from the Sommerfeld school, might have been very useful in running a seminar where people would have learned to work out problems quantitatively a little bit more and to do some more mathematical physics, which was not Ehrenfest's way of doing it. What was important from his point of view was formal and logical structure. I remember, but that was in my third year, Ehrenfest said, "Well, you are wasting your time; you are listening to my lectures on electricity and magnetism, so I will examine you." So he examined me in Maxwell theory, walking up and down the corridor without paper or blackboard, and I had to recite formulae; he would say things and I would answer, and he would catch me on some paradoxes and so on. He said, "Well, there are a few things there you still don't know," but I think he said something like, "Die Musik hast du schon verstanden." [The music] of Maxwell's equations, yes! That was in the old Institute of Theoretical Physics in a very small corridor and he was partly sitting on the bannister, talking about Maxwell's equations. It was also during my second year that Lorentz died. When I had passed this first examination in June '28, Ehrenfest took me along with him to Goettingen just to listen to a few lectures and to learn German a little bit better. I remember a little bit of the atmosphere there, and I listened to lectures by Born and by Franck. I think it was the next summer that Ehrenfest began to talk with van der Waerden on the spinors.

Kuhn:

Where did he talk with van der Waerden?

Casimir:

At Goettingen, I think. Ehrenfest liked to go to Goettingen every year at the summer semester and I was there twice, once in the summer of '28 and, I think, also the next year. An interesting period in leiden was when Dirac was there; Dirac spent quite some time there. That must have been the spring of '28. When did the first edition of his book appear?

Kuhn:

1930.

Rosenfeld:

Yes.

Casimir:

Dirac spent quite a few months at leiden working on his book and, as a matter of fact, the first chapters were then presented as lectures at Leiden. I remember these lectures and that I was still very much at the beginning of the game, although I had read something on quantum theory. In a way I was perhaps not supposed to go there. I remember definitely that Rutgers was there and that he taught Dirac to ride a bicycle, which Dirac hadn't done before.

Kuhn:

This would be before your time in Copenhagen?

Casimir:

That was definitely before my time in Copenhagen.

Rosenfeld:

I remember Dirac in Goettingen later, in either '28 or '29; he was working on his book.

Casimir:

He went to Goettingen after he had delivered these lectures at Leiden, I think, and I would almost think that it was '28 and not '29.

Rosenfeld:

No, it could not be 1929. In the summer of '28 we went for a tour with Rutgers, Dirac and Tamm.

Casimir:

Yes, because Tamm was also at Leiden then, so it must be '28. Before that he spent quite some time working on his book, giving lectures and so on. I also recall that they were very beautifully presented and that Ehrenfest's attitude was quite amusing because Ehrenfest was always interested in seeing how people's minds worked. Now in the case of Dirac, he had worked on these chapters in the book and then it was presented in perfect form. You know the habit of Dirac: if you wouldn't understand things, he would not offer any explanations but would very patiently repeat exactly the same thing, and usually it worked, but it wasn't quite Ehrenfest 's way of doing things. I remember that once Ehrenfest put a question to Dirac to which Dirac had no immiediate answer and so Dirac began to work it out on the blackboard; he covered the entire blackboard with very small things. And Ehrenfest was right behind him trying to see what he did and exclaiming, "Kinder, Kinder! Schaut jetzt zu. Jetzt kann man sehen, wie er es macht" That was very typical. Now it must have been '28 because I know that I went to these lectures with a somewhat bad conscience because I had to read for this first examination and this was quite outside the range of the subject, of course. Then later, in '28 I started really working with Ehrenfest — I had been to Goettingen and then in September I really started working on theory. I was one of the few students Ehrenfest had. Rutgers was one, and there were one or two others besides me. During that winter Oppenheimer spent quite some time at Eindhoven, becoming quite great friends with Rutgers but not hitting it off too well with Ehrenfest — not too badly either. Perhaps Oppenheimer himself was not exactly what I would call "on top of his form" that winter.

Kuhn:

By the time that you really began to follow the work in quantum mechanics closely quantum mechanical theory — did problems like the interpretation of the Schrodinger equation seem pretty well solved?

Casimir:

I think they were still being debated. I listened in on the "fringes" during these first two years at the university and I remember when the first results from interference were told in the colloquium and so on. I also remember discussions where people, even Uhlenbeck, were not quite certain that these waves which you use mathematically would really behave as normal waves and show normal interference patterns when scattered by crystal lattices and so on. So the thing was still being debated and certainly was not in every way clarified, but I think the thoughts were gaining ground very rapidly. We youngsters, as I recall, did not quite understand why people of the older generation thought these things so hard to work with, because we found it in a way easier. It's the same thing you have nowadays with modern field theories and what not. When I started to work with Ehrenfest in '28, he was then very much interested in applications of group theory; that was just becoming important in those days. And I think on various occasions he invited Wigner and also Heitler to Leiden to explain things; of the two, Wigner of course being the greater physicist and Heitler being a much clearer lecturer in explaining group theory on, let's say, a relatively elementary level. Ehrenfest's way of working, by the way, was often to take a paper that he thought important, for instance, older papers like Dirac's transformation theory papers, and to say to his students, "Now we are going to read this paper together and see whether we can understand it and whether we can understand all the steps involved." So you would sit by yourself and study it; perhaps you would see him for a few hours a day when he was really interested, and sometimes you wouldn't see him for quite some time. His idea of studying physics was especially to read what he considered the important papers and if you got stuck, then you could look at a text book or a handbook or something and see what was there, or refer to older papers; but the best thing was to try to read this fairly advanced paper, which in those days was still quite possible. Of course, then you would skip a lot of things and there would be terrific gaps in your knowledge, but it was a way to get people to the front of science rather rapidly. Another recollection I have of these days was when Weyl's book on Gruppen theorie und Quantenmechanik came out, which I studied rather carefully. And I think I not only learned a certain amount of group theory then, but also a lot of quantum mechanics; that has been an extremely valuable book to tue. But I also remember that Rutgers and I read the first papers of Wigner on the application of group theoretical method, and so on, together. I remember how beautiful I thought this was, particularly in Weyl's book where it is well written down, because I had not studied spectroscopy in great detail but sufficiently so to appreciate the vector model and all that sort of thing, and then to see how that came from the theory of representations gave me a great kick.

Kuhn:

Do you remember particular papers that you worked through with Ehrenfest that he thought were worth doing this way?

Casimir:

I think we worked together on some of the papers on the transformation theory and Wigner's early group theoretical papers; those are the ones I remember, but there must have been others.

Kuhn:

Was there any concern still at Leiden with matrix mechanics as an alternate to wave mechanics, or was the approach pretty much dominated by wave mechanics throughout?

Casimir:

In my recollection, it was pretty much dominated by wave mechanics, and matrix mechanics, so to speak, only came back through group theoretical arguments, if I may put it that way. Of course, there were not many people who were actually working out concrete problems with quantum mechanics at Leiden in those days. Early in '29 Ehrenfest took me along with him to a Copenhagen meeting —

Rosenfeld:

That was the first meeting?

Casimir:

Of this somewhat extended type, yes. I remember travelling with Ehrenfest to Copenhagen very well; I also remember Ehrenfest's words which I put in my letter to Mrs. Bohr after his [Bohr's] death: "Das wichtigste Erlebnis, das es fuer einen jungen Physiker geben kann, ist Niels Bohr kennen zu lernen." Ehrenfest had suggested that I might stay some time, but Bohr didn't know whether that would be a good idea or not; in any case, however, I went to that first meeting, which was an interesting one.

Kuhn:

What was the general subject?

Casimir:

[To Rosenfeld] Would you remember? Was it at that meeting that Heitler spoke about the theory of the chemical bond?

Rosenfeld:

Yes.

Casimir:

That was one thing, and the magnetic moment of the electron was another. The Dirac theory of the electron existed already, of course, because it was published in '28 and in Weyl's book, which was out at this time, there was the Dirac theory of the electron with four-dimensional spinors and so on.

Kuhn:

It would have been out perhaps a full year at that time.

Casimir:

Those were a few of the subjects. There was the most remarkable incident of Heitler' a lecture on chemical binding, which you will remember. You haven't heard it? It's really a remarkable story. I had already heard from informal discussion with Pauli and so on that Pauli didn't like this approach to the theory of the chemical bond. Heitler gave his lecture, and then, somewhat tired, he sat down on a chair; now you know that Heitler is a little man, and as he was sitting there, Pauli came at him with rather violent remarks. His argument was roughly this: We know that this particular approximation is wrong in the case of large distances, for then there is always a an der Waals attraction, no repulsion, no saturation. We also know that this particular approximation is quite wrong when the two nuclei are very close together, when we get repulsion. And Pauli, as usual, walked up and down, and then approached Heitler rather threateningly, saying, "Nun gibt es eine an die Gruppen glaubende Physiker appeilierende Aussage, die behauptet, dass trotzdem in einem Zwiscbengebiet diese Annherung wenigstens quantitativ das Richtige geben soll," and at this particular moment, the chair on which Heitler was sitting collapsed under him and Heitler fell over backwards. The audience roared with laughter and said, "Pauli effect!" I don't know whether anyone engineered that; it would, of course, not have been beyond Gamow to do it, but I don't think he had an opportunity. But it was also characteristic of Pauli who was very much against a lot of mathematics on uncertain foundations. We know that he was not against mathematics nor against hard work; he even told me later that if he were younger, what would interest him would be to develop numerical methods for calculating atomic and molecular spectra, but he wouldn't do it now, he said, though it might have interested him when he was twenty. He didn't like this sort of mathematical thing where you have no idea about the approximations involved; that was why he also disliked much of solid state physics: Ich mag diese Physik des festen Koerpers nicht; zwar hab' ich damit angefangen." Then, of course, Gamow was there who had just published his theory on alpha radioactivity that winter. He paid us a visit in Leiden that winter also, and I remember showing Gamow the Rijksmuseum at Amsterdam where we ran into Oppenheimer and exchanged a few words. Oppenheimer was also at Leiden then, but he was having a day off. Well, the result was that I stayed in Copenhagen until the summer and came back to Copenhagen again the next September, I think.

Rosenfeld:

I do remember that at that meeting Goudsmit put to you the problem of calculating —

Casimir:

Yes, that is certainly one of the things that is interesting because Goudsmit wanted to calculate the hyperfine structure or S states or hydrogen-like atoms or — no — of the alkali halides [alkali metals], so you had only one electron. There is a difficulty because if you write interaction between magnetic dipoles, you get a divergent result. I had read Weyl's book carefully, and Dirac's paper too, so I knew that you could have a current associated with a spin; in fact I remember that it struck me as a very important sort of thing that one should not think of an electron as a little magnet, but that a hydrogen atom in its ground state had such a beautiful current distribution extending over the whole of the wave function and going around like rotating spheres. Now if you take that, it's very easy to calculate the magnetic field at the center and then, applying normal ideas, to calculate the interaction; so during that meeting I derived this formula with $8\pi/3$ times $\Psi_o{}^2$ times for the interaction, and Goudsmit provided an estimate for the $\Psi_o{}^2$ with the n^3 and the Z_i and the $Z_{external}$ squared. This is a well-known kind of Landé formula ... That was my first original work in physics. The fate of that was that I wrote a paper, which was probably rather badly written, and sent it to Goudsmit who left it on his desk for a year or so. In the meantime Fermi had published this theory, but I think I had it not quite a year before; and I'm still proud to say that, although the paper was badly written, I had it in a more general form at once for arbitrary l and j and so on, and I think that in a way, using group theory methods, I had a rather more elegant derivation. That was the origin of the certain interest I always kept in hyperfine structures, and it also made me aware of this difference between dipoles and currents about which a lot of nonsense has been written in physics, because it's always a slightly tricky point. That summer I was again in Goettingen and that was the summer when Ehrenfest got van der Waerden to write about spinors. Or perhaps that was a year later. Yes, he got van der Waerden talking about spinors then, but the paper was later. At this time there was Bohr's question about the measurement of the electron; I stayed at Copenhagen and I remember that Bohr sometimes used me a little bit as a kind of "reverberator' or listener.

Kuhn:

When you say the 'problem of measurement of the electron' do you mean to determine the spin of a free electron?

Casimir:

Yes, whether you can determine the spin of a free electron. And then to discuss all kinds of contraptions and so on.

Kuhn:

Do you know if that problem begins for Bohr only after the Dirac electron?

Rosenfeld:

Of course.

Casimir:

Yes.

Kuhn:

There's a problem one can formulate before, but there is suddenly a much better reason for formulating it afterwards.

Rosenfeld:

It was directly an outcome of Dirac's paper; it was then that the question came up.

Kuhn:

Yes, that makes very good sense, but it would not have had to be the way it happened. In particular, one could have begun to wonder after the Pauli paper, though, of course, then Dirac gets this without putting it in, this question has a new forcefulness. So this problem was one with which Bohr was working very actively?

Casimir:

Yes, very actively.

Kuhn:

What else was particularly concerning him at this time? Was he, for example, at all concerned with the developments that were going on quite rapidly in quantum electrodynamics?

Rosenfeld:

That came a bit later, I think.

Casimir:

I think that came a bit later.

Kuhn:

The whole issue of second quantization which was very alive in just the years you were there was not one that —?

Casimir:

I don't think it was one that played a great role here at Copenhagen. I must get these periods straightened out a little. I came here in the spring of '29 and stayed until the summer; then I was again in Copenhagen some of the academic year 1929-1930 following; but I also had to prepare my second examination at Leiden, so I wasn't there the whole time. I passed that second examination at Leiden in the summer of 1930.

Then I joined Ehrenfest on a trip to the United States, to Ann Arbor, where I also had the privilege of being present when Fermi came to America for the first time in his life and delivered his famous lectures on the quantum theory of electromagnetic fields, about which I might have a few things to say later. Then after that summer, I went again to Copenhagen and spent most of that academic year there working on my thesis which was published in the subsequent academic year, which would be ‘31-'32. I got my Doctor's Degree in the autmn of 1931 and then worked for one year at Leiden. The summer of ‘32 I spent some time at Berlin-Dahlem with Lise Meitner; just to get the thing fixed, I also got into contact there with the spectroscopist Schuler, a wel1-known hyperfine structure spectroscopist. And I still had my old interest in hyperfine structures, slightly frustrated because of my first publication. Schueler had some rather curious things, perturbations of mercury lines, which I helped him unravel, developing a theory of interconfiguration interactions in hyperfine structures. I remained somewhat in correspondence with him and later, he was the first to find clear indications of quadrupole effects, of cosine square effects, in hyperfine structures. And I then worked out the theory for the interaction of electrons and quadrupole moments and derived the formula for hyperfine splitting due to quadrupole moments, but that was quite a few years later, in ‘36 or so when I was already back in Leiden; and that was about the end of my relations with hyperfine structures.

Kuhn:

Was it after that that you went to Zurich with Pauli?

Casimir:

Yes, that was later. I worked at Berlin in the summer and then I got a letter from Pauli asking whether I wanted to become his assistant, so I was with Pauli in '32-'33. Then I went back to Leiden after Ehrenfest's death and stayed at Leiden until ‘42. So that is just a rough time scheme, and now let us see what I remember. That first year when I spent quite some time in Copenhagen, in the season 1929-30, there were not so many visitors. I think Gamow was mainly away most of the time in Cambridge; if I remember rightly, Bohr really had introduced Gamow to Rutherford and to Cambridge, feeling that here was an important thing. After all, Gamow's little paper provided the solution to a problem which had been very explicitly put by Rutherford, because he was always worrying about how it was possible that you get alpha particles with this certain energy which would not be sufficient to surmount a potential barrier, whereas from scattering experiments you knew that you had a Coulomb field to quite short distances, and so this turning was very important. I think Gamow himself, of course also realized the possibility of the inverse process where you could shoot electrons into nuclei. So I believe that Gamow's presence and ideas at Cambridge did a lot to interest Rutherford so that he encouraged Cockcroft and Walton to go ahead with their experiments, because without this idea it would have been ridiculous to try to get nuclear disintegration with energies as low as those Cockcroft and Walton had; and probably they even had less than they thought they had.

Kuhn:

The Gamow work had actually been done before you were here?

Casimir:

Yes, because it was very new when Gamow visited us at Leiden before first visit to Copenhagen, some time in the very early winter of ‘29. Then Gamow was also here at Copenhagen where I met him, but be went again to Cambridge. Although the things such as the uncertainty principle and so on were more or less well established in Bohr's way of thinking, he was still spending quite some time, I think, thinking about special examples, working them out, solving apparent paradoxes, and so on. Then this question of measuring the spin of free electrons was an important thing. Another thing that be was rather concerned with was the Dirac theory, how good it was and how bad it was, and with the Klein paradox which was then formulated here

Kuhn:

I take it that what particularly was on his mind, then was the negative energy states?

Casimir:

Yes. I remember when Dirac’s first paper came on what were then believed to be electrons and protons, Bohr was particularly vexed by Dirac’s remark that this infinite charge, being entirely homogeneous, would have no influence at all. Bohr said, "That's quite impossible, for the divergence is infinite then,” which, of course, it would be. And so this whole question —

Kuhn:

Dirac immediately responds to that by suggesting that one reformulates one's notions as to what Maxwell’s equations are about by supposing it’s only deviations from the average distribution that —

Casimir:

Well, I think he never quite liked the idea.

Rosenfeld:

At one time, much later, be speculated on the possibility of using that sea of negative electrons in order to get the conservation of zero point energy by arguing that one had to count all the kinds of elementary particles, each contributing self energy, either positive or negative, and all those self energies can —

Kuhn:

When was that?

Rosenfeld:

That was quite a bit later, in '46.

Casimir:

Well, that's one thing. Another thing which came before that time of course and partly at Leiden already was the begining of solid state physics, electrons in solids, which wasn't studied very much at Copenhagen. There's one thing I remember which Ehrenfest was very interested in. Ehrenfest liked to "help out," let us say, great paradoxes and problems which you had to solve and one of the things which was always heavy on his mind was the existence of a positive Hall effect in some conductors; that was a problem which had also troubled Lorentz a lot. I don't know whether you have seen the address I gave at the Academy of Science at Amsterdam when Peierls got the Lorentz medal.

Rosenfeld:

No.

Casimir:

Peierls got the Lorentz medal last autumn at Amsterdam and I had to make the presentation there and then I looked up a few of the other things, and I found that Lorentz himself had been very worried about the existence of the positive Hall effect. I think it is not always sufficiently recognized in the literature, at least not in the literature on semi-conductors, that it was Peierls who first introduced the idea of a positive hole. Of course Heisenberg had worked on almost-filled shells in atoms and pointed out that they almost behaved like positive particles, but Peierls was the first to have almost-filled bands in conductors and to point out that they behaved so as to get a positive Hall effect, and were positive holes, really; and that was quite a bit before Dirac's first publication on electrons and protons. It's not too trivial because there are two ideas in it, of course, since you always have to have the combination of two things: namely, the particle itself in the top of the band behaves like a thing with negative mass because the energy is curved the wrong way; and secondly, an empty spot in a filled band behaves like one particle ... So there are these two elements of the negative mass and the almost-filled shell which, of course, are not in Heisenberg's theory of the almost filled shells, and really the positive electron in an ordinary band precedes the positive electron by quite a few years. I found it rather amusing to put that straight historically; I don't think there is the slightest doubt about it.

Rosenfeld:

The time lag must not be very —

Casimir:

No, the time lag between Dirac's electrons and protons and Peierls' paper is perhaps something over half a year.

Rosenfeld:

Was Heisenberg's paper before Peierls' or was it after?

Casimir:

I don't know, but Peierls said that his ideas had been much influenced by Heisenberg's. But he definitely was the first one to introduce this notion, which is an interesting feature there. I think it was also that year that I did a lot of work with Bohr and Bohr had to write an appraisal of the work of candidates for a chair of theoretical physics at Stockholm, the candidates being Klein, Faxén, Wailer, and, I think, Enskog. That was terrible because you know how it is in Sweden; people apply for such a job and then official experts are appointed who must write reports; and so we got huge packets of papers written by Klein, Wailer, Faxén, and Enskog. Bohr thought it would be nice if Klein got this sort of thing, but Bohr, of course, also thought that, after all, you had to read all these papers and see what was in them and then write an appraisal, so I acted then more or less as his secretary and we read most of these papers. That was not so bad; we started to analyze them and to speak about them sometimes here end sometimes at his country house. The trouble really began when Bohr started writing his appraisals; that was when I really learned Danish, especially Danish laudatory adjectives in their different shades of approval. I remember that he spent a very long time thinking about one of Klein's papers, saying, "Ja, det er et smukt arbejde," and I said, "That will never do." ("Det er et vigtig arbejde.") "Take it away, it won't do," and so on. Finally it came out as "(???) arbejde." I said," I'm afraid I don't know the word," and the only explanation be gave was, "(???) (???) (???)." [Casimir and Rosenfeld laugh.] I also remember that Bohr had a curious habit that things must be sent away on a Saturday evening so they could just catch the train to Hamburg; he said that if you get things away on Saturday you gain one day because they travel on Sunday, so finally we got it off and I think it was the ninth or tenth version of the manuscript. Dear Miss Schultz was also almost exasperated by that time. It was a beautiful piece of writing and Klein got the position. Whether it was worth all the trouble I have my doubts, but I remember that when I once made a very slight suggestion in that direction it was not well received. "Now don't desert me," Bohr said. I even have a book at home which he presented me when this work was finished, so apparently it had been very difficult for him too. I got a book with a beautiful inscription saying that it was thanking me for "..." [Casimir here says two lines of Danish.] Well, that was one thing. There were, I think, other things he had to write in that period. I myself became interested in a problem of an asymmetric top, an asymmetric rotator, which had been one of the things studied very extensively by Kramers together with Ittmann at Utrecht. They had done it the hard way, in a way, tackling the differential equation with Landé functions, adding quite a few new results about Landé functions to existing knowledge and so on. Then Huang found that you could write down this kind of matrix equation, and while I was here, Klein found that you could write down these equations quite simply by matrix methods. I then got interested in extending that a little bit more so as also to be able to calculate intensities. I also became interested in the connection of this formalism of Klein's with group theory, because, I think, you can say that operators of angular momentum in a rotating frame of coordinates — that's rotating with a rigid rotator — and in a stationary frame of coordinates, correspond to infinitesimal rotations of the two parameter groups — the one which you multiply on the right hand and the one which you multiply on the left hand. So you could work out the connection between the theory of the rotator and representations of the rotation groups. I started to work on that during that first winter at Copenhagen. Bohr, of course, was not so much interested in these mathematical things, but I still remember he very kindly looked at this sort of thing and even helped me get that paper, which was the first I had ever published, into shape.

Kuhn:

What sort of criticisms and suggestions was he able to make in connection with the paper?

Casimir:

The kind of things, I remember, are the way of aligning the formula, the way of stating certain things, the way of formulating an introduction, and so on, rather than the mathematics, really. I then went back to Leiden to pass that examination and that summer I traveled with Ehrenfest to Ann Arbor, Michigan, where Fermi was as well as Uhlenbeck and Goudsmit.

Kuhn:

I want very much to come back to that point, but let me emphasize one more thing before we pass away from this period in Copenhagen. This really goes back to something

Professor Rosenfeld and I were talking about a day or so ago, which was on the whole the lack of any very early response after the Como meeting to the difference between the uncertainty principle in Heisenberg's formulation of it and the elements that Bohr was adding through complementarity in his own Como paper. I think that Professor Rosenfeld has said himself that he had really not seen that there was something more and deeper than Heisenberg had gotten hold of. Many people seem to feel, on the one band, that that whole story of the interpretation is done after Como and Solvay in the fall of '27; yet clearly Bohr was still very much concerned, in the time you were there, with some of the analysis of measurement problems, and I am still very much unaware of how that deeper element enters the consciousness of physicists and the extent of Bohr's own concern With it.

Casimir:

It was certainly always there that winter as well as the next winter when I was again at Copenhagen, as were Landau and Gamow. It is difficult; he bad been producing, roughly within that period, various articles also — one in Naturwissenachaften — and then rewriting certain things later for the university yearbook, and he sometimes gave lectures and sometimes a discussion with philosophers and so on, and I remember his always coming back to a screen with two holes. It was one of the favorite ones that was always there.

Rosenfeld:

And then it was the year in which he was tackled by Einstein; the Einstein box was in 1930 at the Solvay Conference.

Casimir:

Yes, and when was this Einstein proposal which, I think, appeared later in a modified form in a paper by Einstein, Rosen and Podolaky where gravity was brought into play?

Rosenfeld:

That was later.

Casimir:

But I also remember a colloquium when Einstein was at Leiden and I was again at Leiden; that must have been the winter of '31-'32 when Einstein spoke about his misgivings on the theory. I remember that Ehrenfest was presiding, and Einstein presented these things while I had the task of trying to defend quantum theory. Being well coached by Bohr, I could, of course, show that there was really no paradox, that it works out all right, Einstein listened very patiently and then he said, "Ja, ja. Ich weiss schon, die Sache ist schon widerspruchsfrei. Das ist schon richtig, aber sie enthaelt trotzdem eine gewisse Haerte." That must definitely have been in the winter of '31-'32 because then I was again at Leiden. We also made as a toy a box illustrating one of these paradoxes. [To Rosenfeld] Do you remember the one?

Rosenfeld:

Yes, that must have been in ‘31 because it was for the anniversary of the Institute.

Casimir:

Yes, with the photographic shutter, the little lamp, the spring balance and the portrait of Einstein. A very nice contraption-it might still exist.

Rosenfeld:

It may still exist. I hope so.

Casimir:

I also think that Bohr was always struggling writing these papers to find, let’s say, better formulations of these things, to look at the relation with other fields of physics, also to look at various examples and to discuss them, and so on. This, of course, was taken up later with Rosenfeld in connection with the electromagnetic field. I don’t think he himself was at that period very much taken up by the more technical applications of quantum theory and, quantum mechanics.

Rosenfeld:

No, I even remember a remark made by Landau when Landau first came here: “What’s Bohr doing? What’s Bohr doing?” And we said, “He’s discussing those cases of complementarity.” "Oh, yes,” said Landau, “but that’s not physics.”

Kuhn:

What year was that?

Rosenfeld:

1930 or ‘31.

Kuhn:

About the same time as the Landau-Peierls paper. Is that also wasting time? I was trying to see how it would relate to the Landau-Peierls paper. Well, it's right in here that there's a problem I think we probably can't answer, but it may show some in the correspondence. The history of the battles with Einstein and with the other old hold-outs is fairly clear even if not in all its detail; it is also fairly clear that by 1930, and in some cases already by 1929, for a number of people these problems had already ceased to be physics. Furthermore, I think they had ceased to be physics before the depths of Bohr's Como paper were at all realized, that they had ceased to be physics at a point which is really represented by Heisenberg's uncertainty principle paper and that doesn't include the notion of duality and doesn't really include complementarity at all. Already at this point a number of physicists have cut off and let go of it; now somewhere in between there is the introduction and growing conviction about this extra element of Bohr and it is very hard to get any notion for the structure of that development.

Casimir:

Yes, I would say that is very difficult. You cannot attach it so easily to one very concrete paper publishing one concrete formula and so on, so that is what makes it so difficult. I think that Bohr's own thinking about these things gradually developed; he always went round and round that sane subject, studying other examples in a slightly different way, taking new problems, looking at new things coming into physics from this point of view. Apart from those people trying to fight against it in physics — "the Einstein group" some older people who might still need some convincing, and, even worse, philosophers because they were standing outside physics, I would say that most physicists would take it for granted, whereas Bohr kept thinking and reformulating these things. And I would say that that was an important part of his thinking and his activities during most of the time I spent with him in the spring '29, summer of the next academic season, and most of the academic season after that.

Kuhn:

And that really gets practically up to the time when you do the paper with him?

Casimir:

Yes.

Kuhn:

Now let me get back to the time you went to Ann Arbor.

Casimir:

There were a few interesting things there perhaps. Ehrenfest perhaps did not have so many new things to give there; I remember his not being satisfied with the statement, that if you have an even number of particles obeying Fermi statistics then the composite system made of them will behave as if it were Bose particles. Ehrenfest went by car with Dieke from Ann Arbor to California, and there is a paper written later that year by Ehrenfest and Oppenheimer in which that was worked out, though perhaps not in the most elegant way. One could do it better these days. Fermi impressed everyone very much by his very clear lectures on quantum theory of the electromagnetic field; of course before that, the Heisenberg-Pauli theory and so on had been also reported at the Copenhagen Conference, but I would say that people found it difficult to work with. Then there was the Dirac theory of radiation, and I think this Fermi publication turned it into a sort of tool which you could easily solve problems with.

Kuhn:

I take it this was to some extent your first real involvement yourself with problems of quantum electrodynamics. Do you remember what people at the Conference thought of the state of the field? Was it already relatively clear that both the zero point energy problem and the self-energy problem were not going to respond to further treatment?

Casimir:

I would think so, yes. I think this was fairly clear.

Kuhn:

I would say that in the Heisenberg-Pauli paper, although they were very careful to make all these remaining problems explicit, the tone of the paper is one of great encouragement, as if to say, "And in the next formulation we may have it so that these will —"

Casimir:

Yes, optimism was probably there to a certain extent. It may be true that the ideas of self-energy were not so clear. I'm coming back to a moment later when I speak about working with Pauli. That was one thing. Fermi, of course, spoke even more Italian then than he did later, and that was very amusing. He had very great difficulty with dipthongs but he would pronounce them with great energy, saying, "Nah-oo psah-ee ees a fyounk-shun that ees kone-teen-youss and fin-ih-tah een the nay-boor-hood of the or-ee-gee-nah." That was of course perfectly understandable once one got accustomed to it; there was a one-to-one correspondence and for us it was very easy, but for Americans it would be a little more difficult, of course. Also Onsager was there at that meeting and tried rather unsuccessfully to get Ehrenfest interested in his ideas of time-reversal and timesymmetry and irreversible thermodynamics, derivation of the Kelvin relations for the thermoelectric quantities on the basis of symmetry between past and future; and Ehrenfest didn't catch on to that, which is remarkable. "There might be something in it," he said, but he wasn't really much fascinated by it. Well, that doesn't have much to do with quantum theory. I don't remember too many other highlights there; as I said, remember these very brilliant Fermi lectures.

Kuhn:

You don't particularly remember any discussion about the remaining problems in quantum electrodynamics?

Casimir:

No, at that moment, no. I think that Ehrenfest from time to time extended a word of warning and said, "You will get your difficulties with the point electron," but that was about it. After this visit to the United States I was again in Copenhagen where Landau and Gamow were also Gamow was working on the first edition of his book on the nucleus while Miss Swirls, who later married Jeffreys, was trying to brush up his English and convert it in reasonable form. Landau was working on a number of problems, not getting much in shape for publication that year at Copenhagen. He was very critical of everything. Part of the time A. H. Wilson was there, the man who started semiconductor theory and whom Landau thought a complete fool, his favorite pastime in the evening being to tease Wilson and try to make him furious, in which he often succeeded. The Landau-Peierls paper was one which Bohr didn't like at all; he thought it was not well analyzed and I think he felt a little bit that it encroached upon his favorite field with fairly uncareful preparation.

Kuhn:

And this even at a time, from what I gather from what Professor Rosenfeld writes about Bohr's knowledge of the field when they took it up together, when Bohr's own preparation to work on this problem was in some sense still inadequate.

Casimir:

I remember that with Landau the question was always, 'who is going to make quantum electrodynamics?' So he didn't feel that what existed then was really the answer. So at that moment it was fully realized that there were these remaining problems of infinite energies and so on and so forth. Landau always raised the problem of who was going to make quantum electrodynamics.

Kuhn:

It was Landau who raised this problem rather than Bohr raising it with him?

Casimir:

Yes, it was Landau who raised it.

Kuhn:

Did Landau think that the key to it might very well lie in this sort of problem as illustrated in the Landau-Peierls paper?

Casimir:

That I don't know. I don't think that he thought that was the way in which you ought do it.

Rosenfeld:

No, their paper was more descriptive, I think; they wanted to show that the present approach, which is still a current one, after all, was completely wrong.

Casimir:

I went back to Leiden at Ehrenfest's request in the academic season of '31-'32 and I first finished my thesis, which dealt with this question of asymmetric tops and rotations and representations of the group of rotations and that sort of thing; I developed some general mathematical theorems on irreducible representations which were later found to be rather useful. I don't remember very much about that particular winter after I finished my thesis; I don't think I did anything very important myself, and nothing very much was going on in theory at Leiden at that particular moment, As a matter of fact, when summer came, Ehrenfest sent me away from Leiden, saying that I wasn't doing anything there. That was when he first sent me to Berlin and I already told you about this episode with hyperfine structures and so on; then Pauli asked me to become his assistant at Zurich.

Kuhn:

Whom did you succeed there?

Casimir:

Peierls. This was in a way interesting. Pauli had just finished his Handbuch article on quantum mechanics so it was in state of affairs where you could regard many chapters as closed, so to speak. This was the paper of which he always said, "Es ist nicht so gut wie die erste Auflage, aber immerhin doch noch zweimal besser als sonstige Darstellungen der Quantenmechanik," with due modesty, because he didn't really think it was as good as the first one. Pauli was looking a little bit for problems at that moment; he didn't like, let us say, more applied work such as solid state physics and a number of other things, and he became interested in rather abstract formalisms. He was giving a lecture on general relativity and he played around with formalisms of quantum mechanics and five-dimensional relativity and that sort of thing. I think this occupied most of his time and effort during that period. I remember some discussions with him about this question of self-energy in which he mentioned — I think it is even in his Handbuch article — that you have, of course, an infinite self-energy. Now you may ask what happens to a bound electron. Peierls, at his request, had worked out, but I think had never published, that. He found that, if you take bound states, still the self-energy diverges in exactly the same way as it does for a free electron. I also remember that Pauli and I discussed then that it might be possible therefore to take the difference of these two divergent sums in a certain way so that they would compensate one another for the high frequencies and that you might get a finer difference which then could be interpreted as a difference in electromagnetic mass in a bound state and a free state, and that that then might have a physical meaning. I remember considering working out that problem Just for the fun of it; that would have given you the Bethe approximation for the Lamb shift and we discussed that in '32. Our attitude was that you could calculate such a thing, but that it was rather doubtful whether theory was good enough to give a definite meaning to such a quantity, although to speak with Bohr, "Man sollte darauf vorbereitet sein, dass Abweichungen von dieser(weisen)Abnung vorkommen koennten." So there was just a ghost of a hint in Pauli's Handbuch article where he says this. These five-dimensional things didn't carry him very far, of course.

Kuhn:

Did he hope that they would do something for the quantum electrodynamics problem?

Casimir:

Yes, I think so. I also remember at one moment he said that this must be the source of terrestrial magnetism because he thought he had found some coupling terms between gravity and the electrons. I think I helped him work out the orders of magnitude. There were quite a few powers of 10, some thirty or forty, I believe, missing. And there was a rather curious episode with the two papers of Pauli and Solomon.

Kuhn:

This I don't know at all.

Casimir:

That was a very formal paper on some formulation of Einstein; Einstein had a four-one dimensional, if I remember rightly, and Solomon had written a rather formalistic paper on tensors and so on, and the first thing that I had to do when I came to Zurich as Pauli's assistant was to check over some of the results in the second proofs, which I did. Solomon was a very good theoretician, but in this paper he had been rather sloppy, and as a matter of fact, although the general thought and pattern of the paper was perfectly all right, most of the formulae were wrong in the first edition. So Pauli said we have to write to the Journal de Physique telling them they had to stop publication and wait until we put that right. Then a terrific thing happened; a letter came that they had waited so long for the second proof, and since there had been so few corrections in the first proof, they had printed the thing. So the second paper, Pauli-Solomon number two, was written with an elegantly phrased introduction that although the general ideas and so on were all right, there were certain defects in the actual calculations in the previous paper. So Pauli took it up again from the very beginning. Pauli was really a little bit embarrassed about that: "Was muss ich noch sagen? Muss ich jetzt Ihren Namen nennen? Das ist naemlich so peinlich, aber die Formeln sind jetzt alle von Ihnen; also was muss ich jetzt?" [General laughter] So I graciously consented not to be quoted in that paper and to be a ghost writer for Pauli and Solomon Number 2. What I also remember of that Zuerich time is that Otto Stern, who was a good friend of Pauli, came there and told about his experiments with the proton magnetic moment and so on, which in a way was quite a sensation.

Kuhn:

Do you remember particular reactions to that?

Casimir:

Pauli, of course, I think, knew that you could, in a formal way, write extra terms in a Dirac equation so as to get higher magnetic moments, so that in a way consoled him. I remember that I worked a bit, apart from these things, on various problems of theory of radiation, radiation damping and damping in successive states and so on. And Pauli was much interested in mathematical problems at that time, and I remember when I came to Zuerich he said, "Now there is one unsolved problem in group theory which is a problem of the complate reducibility of representations which is not quite satisfactory because it is proven by Weyl, but only with a kind of integration over group space and not by purely algebraic methods." So be put me to work on that and I was able to do it easily for the rotation group and for a number of other cases for other groups; then I wrote to van der Waerden who completed the proof. It was later simplified by Brouwer. The basis of it was really an operator that I had introduced in my thesis when I was working on rotators. Then there was this problem in connection with radiation which again was connected somewhat closely with these self-energy things. I did some work on the scattering of radiation by bound electrons, a correction to Klein-Nishina formula when electrons are bound, So Pauli was rather interested in these problems and always asked, "Where are the limitations? To what extent can you rely on such formalisms and where can you not?" And let's say, as I said before, some vague ideas of renormalization were present, but one didn't really see how to do it and how to do it relativistically, and so on.

Kuhn:

You're there really when the problem of the negative energy state finally comes out experimentally, aren't you?

Casimir:

I don't know if I was at Zurich then or whether I was already back at —. When was the positive electron? When was Anderson's paper?

Kuhn:

Anderson's paper was '32. I think the Blackett-Occhialini paper is already early '33. I'm not sure of those dates now.

Casimir:

Of course, in '33 I went back to Leiden after Ehrenfest's death. I had intended to stay one more year in Zuerich, but Ehrenfest brought much pressure to bear on me to come back to Leiden. This is again part of the Ehrenfest tragedy that his plan to commit suicide, I think, was one which he had had for a very long time; and he also knew that it would take quite some time, that there would be a gap, before a successor was appointed, and so he wanted at least to have me there, a young man, to tide things over. So I was back at Leiden in September '33.

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Hendrik Casimir - Session II

July 6, 1963

Interviewed by: Thomas S. Kuhn, Leon Rosenfeld, Aage Bohr and Erik Rudinger
Location: Institute for Theoretical Physics, Copenhage, Denmark

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Abstract:

Part of the Archives for the History of Quantum Physics oral history collection, which includes tapes and transcripts of oral history interviews conducted with circa 100 atomic and quantum physicists. Subjects discuss their family backgrounds, how they became interested in physics, their educations, people who influenced them, their careers including social influences on the conditions of research, and the state of atomic, nuclear, and quantum physics during the period in which they worked. Discussions of scientific matters relate to work that was done between approximately 1900 and 1930, with an emphasis on the discovery and interpretations of quantum mechanics in the 1920s. Also prominently mentioned are: Homi Bhabha, Niels Henrik David Bohr, Paul Adrien Maurice Dirac, Paul Ehrenfest, Albert Einstein, Walter M. Elsasser, Enrico Fermi, Ralph Fowler, George Gamow, Samuel Abraham Goudsmit, Walter Heitler, Hendrik Anthony Kramers, Lev Davidovich Landau, Hendrik Antoon Lorentz, Walther Nernst, Wolfgang Pauli, Rudolf Ernst Peierls, Max Planck, Ernest Rutherford, H. Schüler, J. Solomon, Otto Stern, George Eugène Uhlenbeck, and B. L. van der Waerden.

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Kuhn:

When we quit yesterday, you were toward the end of talking about Pauli and Zurich; I thought that before we went back, which I hope we will do, let me ask you just a little bit more about that particular period in terms of some of the technical concerns. You had spoken of Pauli's recently having finished the second Handbuch and looking for topics and the concern with relativity, the five-dimensional formulations, and so on. What can you tell me about Pauli's feeling at that point about field theory and its state and the extent to which it was likely to work out?

Casimir:

It is rather hard to say. I dont think Pauli really believed very strongly in this sort of five-dimensional theory; he liked it mathematically, and occasionally he thought there might really be good physics in it, but I don't think he really expected that it would solve fundamental difficulties. Also, I think, he was extremely well aware of the difficulties of theory of radiation and radiation field and so on. I told you that we were speaking of various things about radiation formula and such, and about what the limitations would be; so he was well aware that the theories you had at that moment would not be sufficient to explain everything, but in those days, there were very few physical effects that would require, so to say, this next order of magnitude. I do not remember any clear-cut idea, but I remember working with him, and that is perhaps characteristic, because he rather suggested certain problems to me. The first question was a purely mathematical one, that is, of proving the complete reducibility of a useful representation, which I did. I worked on this five-dimensional theory. Then I did something on theory of radiation and radiative damping. You remember [Rosenfeld] we had some correspondence there; what was called "korrespondenz-maessiges Verfahren", a kind of makeshift procedure for getting intensities of radiation and that sort of thing, and there were certain paradoxes there which I did some work on. I think at the end of that paper I made a certain remark that corrections to formulae might arise which would be outside the scope of the theory of radiation of those days; let's say, in any case, that these remarks were approved by Pauli and must in a way have expressed also some of his feelings about the situation. It might be worthwhile to look at that and see exactly. I remember writing something there which is fairly cryptic but which in a way gives this sort of idea, in the Zs. fur Physik. Then the next problem was one on scattering, Compton scattering and Klein-Nishina scattering with bound electrons which I have talked about, where the question arose as to how to possibly formulate in a relativistically invariant way this scattering by electrons in a certain state of movement and so on. I think Stueckelberg went a little bit further along these lines later. There was already something which came a little bit in all these things, which, I would say, pointed in the direction of renormalization theories and that it might work a little bit along those lines;however, I think that Pauli hoped that you would have a more important breakthrough with a more consistent formalism, but he didn't know how to tackle that.

Kuhn:

Would you think that the five-dimensional attempts, through perhaps without great optimism, were nevertheless attempts in this direction, attempts to see whether they might provide a key?

Casimir:

A little bit, perhaps, but Pauli was not a very happy man in that particular winter; his first marriage had just broken up, he was drinking rather heavily, and he felt neither happy about life nor about physics, I would say. So it wasn't one of his most productive periods. I remember one remark he made when we came home from a drinking party; he was slightly tipsy and be started to speak about all kinds of problems, and among things, he said, "Wir leben in einer fuerchterlichen Zeit, in einer kulturlosen Zeit. Ich weiss genau, was jetzt kommen muss, aber das sage ich nicht. Wenn ich es sagen wuerde, wuerden die Leute sagen, ich bin verrueckt. Dann mache ich eben lieber generale Relativitätstheorie, aber ich weiss genau was kommt. Ich weiss es genau. Das sage ich Ihnen, dann vieleicht ein anderes mal. Gute Nacht." He has never told me, but one had almost the feeling that he had very definite ideas. I would have liked to know what they were like. "Ganz genau, dass es schon kommt." I asked him later, I think, but he never told me.

Kuhn:

You take it that when he said he knew just what was coming he meant what was coming in physics, or —?

Casimir:

No, no. That related to the world in general, but since he didn't want to pose as a prophet about mankind in general, he thought he might just as well play around with these formulae of general relativity.

Kuhn:

This is really addressed to both of you. I think I have noticed a great many quite different attitudes among different people about the relationship of relativistic to Newtonian formulations. For some people, knowing relativity meant that you could use Newtonian things only as an approximation, but if you really wanted to do fundamental work it had to be relativistic. I think this is most pronounced among the people who were influenced by the English at Cambridge. One the other hand, you get another group of people who were perfectly willing to think of these as sort of equa1ly fundamental but applicable to different sorts of problems; they were not bothered by the fact that there are things you can do in non-relativistic quantum mechanical formulations that you simply cannot do with the same power in the relativistic formulations, and who will therefore think of these as somewhat on a par and will use sometime one and then the other. In that sense, from one point of view, these people would be said to be inconsistent in their mental commitments in the field.

Rosenfeld:

Yes, surely at that time, one was very conscious of the fact that the methods of quantum mechanics which one tried to extend to field theory were essentially non-relativistic and so one had to prove explicitly every time that the result was nevertheless covariant. It was very clumsy, so from that point of view, that is the great progress made by the new methods. However, I must point out there that Weiss, I think it was in '36, under the inspiration of Dirac, had produced essentially the kind of relativistic manifestly covariant formulation that we have now; so the problem was recognized, but it was not regarded, from what I can see, as a fundamental —. One was convinced that the whole thing could be formu- lated relativistically so that was not the main problem. But one was worried all the time, especially when one was dealing with divergences, just about separating out what could be the result of an essential physi- cal divergence and what could be just the result of clumsy mathematics.

Casimir:

But I think that also those people who were quite willing at first to work with formalisms that were non-relativistically invariant realized that it would be necessary in some way or another to extend the formalism so as to become relativistically invariant; sometimes it was done in a rather clumsy way and then later it was done in a more elegant way. I don’t think there was ever anything (to do with) commitments to fundamental ideas, that is, as far as special relativity is concerned. It is a different story when it comes to general relativity. There I remember that at one time Ehrenfest and I entertained some hope that another version of Einstein, the (Vierbein) theory, might have something to do with the Dirac theory of the electron, because the (Vierbein) theory and the spinning electrons seemed to go well together. And at one time, one had the feeling that it would be difficult to write the Dirac equations in such a form that they were covariant in general relativity, and then some sort of very vague idea of the spin of an electron being a kind of gyro-compass for the (Vierbein) floated around among many people. I don’t know whether they got much into print, but I know that several people entertained ideas along those lines. I think it was the Russian Fock who then showed that it was possible to write the Dirac equation in a form that was covariant also for general linear transformations. But, of course, I would say that, even today, there is not much connection between the general relativity theory of rotation and quantum theory. Well, you [Leon Rosenfeld] had been amusing yourself quantizing gravitational fields at that time.

Rosenfeld:

But at that time that was for another purpose.

Casimir:

I know. But as far as special relativity goes, I would say that there is not a difference in commitments, but only a difference in attitude. Some people say, "Well we'll let it go for the time being and see what we can do."

Kuhn:

Heisenberg had told me of his dissatisfaction with Jordan's additions of a quantized field in the final section of the papers he wrote with Born and also with Born and Heisenberg. Heisenberg was opposed to the addition of relativistic considerations, not because of the odd or difficult mathematics that would have to be used, but because the theory then might be of a totally different nature. Thomas, on the other hand, had no compunction about introducing relativistic considerations, since, so far as he was concerned, relativity had been shown to be correct. In fact, Thomas has told us that in discussions he held at Copenhagen, physicists demurred at his suggestion to tackle problems relativistically, since they held that the results would be more correct by only a few percent. Thomas had no notion that one would get a larger correction, but when he worked out the problem relativisticaily, the factor two dropped out. Now, the question is: since the attitudes of Thomas and Heisenberg seem to be so markedly different on this matter, I wonder whether other physicists, like Pauli, Emrenfest, or Bohr, also held different points of view about the use of relativistic considerations?

Casimir:

Yes. Of course, when I came to Pauli, so much had been done there relativistically, that, in a way, that was settled. Bohr, I would say, was not so much concerned with that question. Ehrenfest, I think, was; he wanted to see the thing relativistically, because this whole question of invariance and formalism counted heavily with him. He was very glad for the Dirac theory, when —. I just remembered yesterday evening that when I had to pass my second examination, my master's examination, I had to write something like a master's thesis. I produced something on hsow the Dirac theory could be relativistically invariant, so to say; this question of not being tensors. I didn't develop this spinor formalism then, which, I think, was just a little bit later, but I worked out something which probably existed already and was very similar to quaternions in three- dimensional space, taking the Dirac matrices and the product of Dirac matrices, to use them for infinitesimal rotations and also for finite rotations and how you could write Lorentz transformations of four- and six-vectors by multiplying them with Dirac matrices and then with the factor before and the factor behind being two-valued representations; I worked out a number of problems that way and showed that you could derive the addition of velocities and so on with this sort of quaternion formalism. He was very much interested in that sort of thing and was always asking how it would be when it was relativistically invariant; when that very idea, as I say, gave rise to Dirac's equations, that was one thing he was very keenly interested in. When I started to learn about quantum mechanics, some of the main steps had of course been taken already; there was on the one hand always the further extension of the mathematical formalism in which group theory, for instance, had played an important part. Ehrenfest was always very much interested in that and he struggled with it and he had some difficulties in familiarizing himself with that sort of thing; both Pauli and Kramers played an important part. Bohr, of course, was not really very much interested, but since I was then working on my thesis which contained things about rotation groups and so on from time to time, he would very kindly ask me,"Hvordan star dat (???) rotationsvaesen" which I think clearly shows that he did not have a high opinion of this sort of thing, although it was an amusing game. There are a few of these things, for instance, which all of a sudden become common knowledge and where it is not always quite easy to find out who did it first; for instance, this question that the eigenfunctions of a symmetric top are the representations of the three-dimensional group of rotations and that spherical harmonics, when they transform an arbitrary rotation, then the coefficients of transformation are given by the eigeafunctions of a symmetric top, and that these also are spherical harmonics on the three dimensional plane in four dimensional space which is the space of the four Eulerian parameters. One day I knew it and one day everyone knew it; I don't know where that appeared, but probably, if you looked hard, you would find it in much older literature, but it was never in standard textbooks on spherical harmonics and so on, until suddenly one knew that this was the case. Incidentally, that formed the basis of this work I later did when I was with Pauli on reducible groups because, since these representations are eigenfunctions of a symmetrical top, you can then say that the matrix elements of irreducible representations are eigenfunctions of a certain Hamiltonian, and then you put to yourself the question whether for other classes of groups you can find a similar Hamiltonian of which the matrix elements of irreducible representations are eigenfunctions; and I was able to construct such an operator for any semi-simple group, and this then formed the basis of subsequent proofs of irreducibility and so on. But as I say, Bohr would inquire sometimes about "rotationsvaesen", but that was the extent of it. Another question, of course, was the rapidly growing application to all branches of physics where Sommerfeld and his school played a predominant role as well as did Heisenberg and his people at Leipzig. England contributed too, to some extent, with Mott and so on. Pauli was not so very much interested in these things, though occasionally some of his people would do very good work along those lines. And in those days Bohr was not particularly interested either, although, I think, he had this idea that the application of wave mechanics to motion of electrons in metals would be important. There must be a letter or inscription of Pauli in connection with Bloch's first paper on electrons where he says, if I remember rightly, something like, "Aus diesen Abhandlungen von Bloch wirst du sehen, dass dein Sieg Uber die Physiker vollstaendig ist." or something along similar lines. Where did I see that? Was it in something he wrote on these papers or in a little letter? There must have been some sort of an argument going on, probably in connection with Sommerfeld's fairly primitive application of Fermi statistics where people said, "This is no good," and Bohr said, "Well, it is a good beginning and it must be some" Then Bloch's paper came out and then I think Pauli acknowledged that Bohr had been right. But where I saw that remark? I didn't invent it myself and I'm quite certain that it must have related to this sort of thing. This, of course, was only natural, since Bohr's thesis, after all, was on electrons in metals and, though it is very little known, since it is written in Danish, in my opinion, that thesis is a very great masterpiece; it is a very beautiful analysis of many things and you find a lot of things there to which people came back later.

Rosenfeld:

He was keenly aware of the impossibility of explaining those things by classical theory.

Casimir:

Oh, that's everywhere in his earlier writing, wouldn't you say?... Then there was in this period when I started this question of the foundations and thinking about complementarity and so on. We were speaking about fields and so on, and later we knew Bohr came back to this question of electromagnetic fields; when I was with him, you might say everything had been done, the uncertainty principle was clear, complementarity to a certain extent for normal one-particle mechanics was clear, but I think Bohr kept going around thinking through these examples, discussing the apparent paradoxes, trying to find different formulations. This was not only a question perhaps of trying to see the thing more clearly or to get more to the bottom of things, but in a way, although there was very little mathematics involved, it was also the elaboration of a certain technique of thinking and of developing really a way of tackling problems without much mathematics; you could see all the things and predict all the things; at this Bohr was extremely ingenious. I think you might say that really in these years he sort of developed that art of making fairly precise qualitative statements about what quantum mechanics would predict without really doing much mathematical work.

Kuhn:

Who followed him closely in that?

Casimir:

I think there is hardly anyone who really could do these things Bohr did. Also the way he played with orders of magnitude — all these things were very simple. For instance, the thing he taught me and which I found extremely useful in later life was that you always have these different characteristic lengths, the classical radius of the electron, the

Compton wave length divided by 2Pi, the Bohr radius, always differing by a factor of 137. In any kind of formula he would always twist things up and down so that you got these things, so he would never write quantities e and c and h with large numbers of 10 to the high powers; he would always combine them so as to get these dimensionless things out. That was a simple art, you might say, but a very definite one. and I would say that it was really an art developed to a high degree of perfection by Bohr. The same thing held with energies: with mc^2 you have $1/2\alpha^2 mc^2$ and then you have your fine structures and so on, and the hyperfine structure with the m/M, Then, of course, as regards the question of radiation, I think one of the things Bohr was always aware of and which you find in his first paper on the quantum theory of the hydrogen atom is: if you take a classical orbit of an electron, it would of course radiate, but this radiative damping is only a slight correction because the fine structure constant is so small, you might say, that this radiative damping can be neglected. That was one of the main arguments for him in making a theory of the atom. "These radiative effects," he said, "are so small and therefore you can, for the time being, use ordinary mechanics and forget about electrodynamics." I think during the period that I still was at Copenhagen, he said, "But these radiative effects are small, so for the time being, I will first clarify in my mind the particle without its radiative reactions"; and later, of course, he came back to these questions of radiation. You might say it was a little bit late, but even though quantum theory was fairly complete in '28 or '29, the number of people who had really mastered it was rather limited. You see it when you look at the literature on other experimental subjects; to the experimentalist it was still very much a closed book. And on the other hand, let's say, that the group of people who really knew how to manipulate quantum theory didn't know too much about more pedestrian branches of physics, so it was quite some time until things were worked out and until it was a generally recognized discipline. At Leiden, of course, Ehrenfest worked on it, but it was still regarded as a somewhat esoteric subject and I think this was true in most centers. And, as I say, those people who were proficient in the formalism of quantum theory were not always too familiar with classical physics and with experimental effects that might be interesting to discuss.

Kuhn:

Do you remeither any particular examples of that lack of familiarity with classical physics?

Casimir:

No, not at this moment, but I have the feeling that if I thought hard I could find them without much difficulty. I think that would be fairly easy. For instance, I think Sommerfeld played a very important role there. Another thing, of course, which came in those days was the application to nuclear problems and that, among other things, was something Bohr was very much interested in. We discussed that already, his relations with Gamo his introducing Gamow to Cambridge, and so on. It is perhaps interesting that a problem that Gamow Landau and I discussed frequently in Copenhagen was the question of the electron. In those days there were no neutrons, and so one still thought that the nucleus had in some way to be built of protons and electrons; it was clear that alpha particles and also protons might be treated with wave mechanics applied in a normal way, but it was also evident that you could not trap an electron in a thing as small as a nucleus without getting into terrific difficulties.

Kuhn:

You say this is a problem that you, Gamow, and landau discussed; when would that have been?

Casimir:

That was this last winter when I spent most of my time at Copenhagen, 1930-31, and both Gamow and Landau were there. Gamow, who was writing his book on the structure of the nucleus, marked all passages relating to beta structure and to electrons carefully with beautifully drawn skull and crossbones in his manuscript, and I remember Cambridge University Press asking whether they were permitted to replace the emblem by a simple asterisk! [laughter]

Rosenfeld:

I think he had a stamp made.

Casimir:

Right, he had a stamp made. Gamow wrote a letter saying that we had no objection to the asterisk and I remember that I contributed to that letter the sentence: “It has never been my intention to scare the poor readers more than the text itself will undoubtedly do." [laughter]

Kuhn:

What ways did you see as possible ways out of the problem of the electron in the nucleus in that year?

Casimir:

I would almost say none. On had the feeling that something entirely new would have to come. But another thing which appeared in some of the literature in those days was the question whether, for electrons outside the nucleus also, there would be new factors or forces in its interaction with the nucleus. You said: "in order to trap an electron in a nucleus something extra must happen; what about an electron outside the nucleus?”

That question came up in connection with internal conversion of gamma rays and in connection with hyperfine structure; with internal conversion, it was thought at the time that you could not explain the large experimental internal conversion coefficients. I wrote a short note where I had calculated it relativisticaily and found that the conversion coefficients were much too small, and I remember were then bad. The idea that there must be something extra, we didn't know what it is, but it certainly would be proportional to the intensity, to the square of the wave function, at the nucleus, so add something proportional to Ψ_o^2 and that would be the effect. It was then shown by Mott and his group that you could explain conversion coefficients. I had used an asymptotic expansion which was all right for high energies, but the energies for which things were measured were not as high as all that, and there was the other question that four-poles and magnetic octopoles had much higher conversion coefficients. There is a funny point there; we had thought of that, but I had shown that there is no difference in conversion coefficient for whatever multi-pole you take. That was even right for this asymptotic expansion I had used where essentially you take things in the wave zone when the field behaves like 1/r, but the point is that whereas for dipole radiation, this asymptotic method was not really good, but not very bad either, its convergence was worse and worse as you got to higher poles; and so whereas asymptotically, it would have been right and there would have been no effect, or a factor of two perhaps, when you really worked it out properly for the dipoles, yet there would have been a much larger factor for the higher multipoles because the things converge so badly. This is a little episode which just shows that you must be careful with these things. Another place where it came up was in connection with hyperfine structures where people always thought you could not explain hyperfine structures. And then came Goudsmit pointing to special perturbations, and Fermi and Segre who analyzed certain things further, and you always came to the result that if you did it properly, you did not have to introduce anything but normal electromagnetic interactions in order to explain the interaction of nuclei with outside electrons. I think that’s true even today because the nuclear interactions exist, of course, but they are weaker than the other things so they could give at best only very snail corrections.

Kuhn:

Let me come back to a question which I asked a few minutes ago and which I possibly put badly. In this work that Bohr was doing in connection with the measurement problem, I asked whether anybody else was really following him on this and you said no, in the sense that he was so much the master of this. I was really more interested in finding out who else was interested. You spoke of landau’s reaction yesterday — "That’s not physics.”

Rosenfeld:

But that was before.

Casimir:

Yes, but all the same Landau was interested. I think Pauli was interested. Most people would explain to Bohr what they had been doing and then Bohr would look at it from this point of view; that would help greatly to clarify ideas about the things.

Rosenfeld:

I always had the impression, even in later years, even the last time I saw him in Kiev, that landau always spoke of those points as subtle points, which meant he didn't want to learn too much about them. But on the other hand, Pauli, and Dirac too, in his own way, were very much interested. Somebody asked Dirac about some point that raised some doubt about the question of complementarity and I remember Dirac's saying, "That is right.""How so?" said the questioner. "That is right because Bohr says so and he has thought about it."

Casimir:

Another thing which has just occurred to me in thinking about these Gamow things and theory of alpha radioactivity is that it is curious that, although this is a very good explanation, a lot of bad papers have been written about it. Gamow's first paper, of course, was mathematically far from perfect. Then wanting to put things right, von Laue wrote a paper which really wasn't very much better; Born later published an extended paper which was not very good because the solution was not unique.

Rosenfeld:

You set it straight?

Casimir:

There were others who did it right and in Kramers' book it's all right, but there were always a number of papers about it and Pauli was always extremely amused when another publication about it appeared. He would say, "Es Gamow't wieder." But it's very curious that a somewhat elementary textbook problem should have given rise to so much difficulty, and that is probably because the whole formalism had been developed very much in connection with Hermitian theory, Hermitian matrices, eigenfunctions and that sort of thing, and therefore, this sort of pseudo-eigenfunction with an exponential decay didn't quite fit the classical picture of transformation theory, Hermitian matrices, and so on. I think you can find this sort of thing even up to modern days.

Rosenfeld:

Yes. It is the problem we discussed recently in the more general context of nuclear reactions and it is a complicated thing because this exponential decay is only part of the phenomenon. There is also the expansion of the wave packet which complicated the thing.

Casimir:

Probably it is true that even in pure mathematics the theory of pseudo-stationary states is not so well developed. You come across the same sort of thing in what you might call a classical problem; if you try to calculate laser modes — an interferometer where you build up a standing wave. But it isn't really standing because there are always certain diffractions so the waves go off to infinity sideways but only to a limited extent. If you try to do that problem mathematically you run into rather complicated things. There are extremely clumsy and laborious calculations published by people of the Bell Labs, and so on on that sort of thing and I've been thinking a little bit about it, but it is not quite easy. But I like that sentence, "Es Gamow't wieder". Shall we speak a little bit more about Pauli? There are a few more things. He was working on these relativistic things and he was interested in problems of radiation, as you can judge from these things he suggested to me that I might take up; they were partly my own ideas, but be certainly encouraged that sort of thing and there was a little pre-shadowing of a sort of coarse renormalization. He was not too much interested in solid state work, or, in any case, he disliked it, feeling it wasn't sufficiently exact; he had some work that he had done with Guettinger on problems of rotating molecules and similar things in changing fields and also these questions of magnetic atoms in rotating fields with adiabatic transitions or whether the thing flips over — work which, in a way, foreshadowed much work which later became very important in connection with problems of paramagnetic resonance and all that sort of thing. There is quite a bit of his in those early papers which now becomes very relevant. I also remember giving to a rather poor student as a thing to do for his engineering degree a problem which, if the student had been able to do it, would have led to the student's writing the paper which was later written by [Richard] Becker, [Gerhard] Boiler and [Fritz] Sauter referring to super conductivity. Namely, it was said that in a super conductor the field is zero. Pauli said that this was all nonsense. The argument had been that if the field changed in a super-conductor there would be an electric field and that would lead to an infinitely high current. Pauli said this is not so because there would be an inertia of electrons and you would have to write that mdv/dt is equal to electric charge times electric field. So he put the problem to one of his students to work out the behavior of a body when the electrons behaved along those lines, but this was a rather poor student and nothing came of that. Then the problem was later solved by Becker, Heller and Sauter, which was again a forerunner of london's theory. You can, of course, put london's theory in a more philosophical form; you can also say that london simply took the Becker, Heller, Sauter theory and arbitrarily put the integration constant equal to zero — which is also an adequate description of london's theory. So that is remarkable. There were a few other things he did not like. He always thought that studies on gas discharges were an awful part of physics which he didn't like.

I also remember that he was chairman of the physics coilloquium and some people had asked for a lecture on modern types of radio tubes-appreciate that I said ‘tubes’ and not ‘valves’, as I would normally. Anyway, a man prepared the lecture there and I don’t remember it well, but I believe it was a pretty good one. He gave a summary of all kinds of complicated arrangements of grid structures and this and that and Pauli was sitting there, enjoying himself thoroughly: “Das ist aber lustig, das ist aber lustig, Ich verstehe Uberhaupt kein Wort. Das ist aber wunderbar. Verstehen Sie ein Wort? Das ist aber lustig Kein Wort — was sagt er da? Hab ’ich nie gehoert, was er uberhaupt sagt — nichts verstehen!” And it went on that way until finally he said, very politely: “Ich hoffe dass die jenigen unter uns, die sich einen Vortrag ueber diesem Gegenstand gewuenscht haben, jetzt befriedigt sind.” A most remarkable session! He didn’t even think it was bad physics; he just thought it extremely amusing that he, Pauli, should not be able to understand even one single word of what was being said. That was a colloquium on radio tubes. I remember another seminar where I think I reported on the work of the Mott group on internal conversion using higher multipoles and where we got into a violent argument concerning multipole developments of radiation. That was very funny because he wanted—. If you have the radiation of the nucleus, one wants to expand the e to the ikr in powers of r, but there is the other question that you can have the thing without a steady dipole moment which still emits pure dipole radiation, and Pauli for some reason was against that. He thought you shouldn't call that a dipole, and I remember we had quite a discussion.

Rosenfeld:

I had once a terrible ordeal —

Casimir:

later I wrote a short piece on multipoles in Helvetia Acta and in the introduction I mentioned this battle I had with Pauli. Afterwards I went to him and we kept more or less shouting at each other, but finally we agreed and it was also a question of nomenclature. Then in order to make up for it, since he felt he had perhaps gone a little bit far and that this time I was right [he did the following]. He had just gotten his letter from Springer [Verlag] and he said “Schauen wir mal zusammen, was der Springer mir fuer das Handbuch bezahlt”; it was a mark of great confidence that he showed me his earnings there. In a way he almost repeated the famous story he told about Nernst. Pauli loved to tell it and referred to it as “mein beruehmtes Gespaerch mit Nernst”. Somehow he had met Nernst somewhere, I don’t remember exactly where. Nernst said “Herr Pauli: Ja, ich habe damals bei dem Physikertag einen Vortrag von Ihnen gehoert.” I think it was Physikertag, but I'm not quite sure — "Das war an sich ganz gut. Etwas schillerhaft, ganz gut, und wo sind Sie jetzt?” And Pauli very politely, said, “Ich bin jetzt in Zurich.” Nernst: “Ja, so? Sind sie droben beim Herrn Meyer” — da war ja ein Herr Edgar Meyer, ein grosser Physiker.” oder bei Herr Scherrer?" Pauli: “Nein, nein. Ich bin dort als —” Nernst: “Da haben Sie dann ein Institut?” Pauli: “Nein, ich bin

Theoretiker." And then Nernst started to understand that Pauli really had a chair there: "Ach, so Sie sind also Ordinarius." Then Nernst, feeling that he hadn't been sufficiently polite, said: "Aber sagen Sie mir mal, lieber Herr Kollege, koennen Sie denn davon leben?" Das war das beruehmte Gespraech von Pauli mit Nernst. This change: "etwas schuelerhaft" to "Lieber Kollege, koennen Sie davon leben?" Also during that year Bhabha came to Zuerich with an introduction by Fowler; Fowler didn't like Bhabha very much and thought him rather conceited and not so very capable, and he wrote a letter to Pauli — I don't know whether it still exists-where he said, "You can be as brutal to him as you like." Pauli liked this at once, so that already put him into sympathy with Bhabha. Bhabha did some work there and, of course, he was quite capable, but be reminded me in those days of the savage in Huxley's Brave New World, the man who is discovered there and only speaks Shakespearian English Bhabha had studied Dirac's book and studied it well, and it was the only form of quantum theory he knew, so that was really like this savage out of Huxley. I told him so; I think he got the point and I think it helped him some. Bhabha and Pauli became very good friends.

Kuhn:

I wondered a bit about that. I wondered a good deal who else. The texture, the flavor and the approach of that book are so different from that of almost anything else that it's very hard to see its influence.

Casimir:

Which book?

Kuhn:

The Dirac book. I know of almost nobody who does think that way, and I'm much interested to know that Bhabha —. Were there other people?

Casimir:

I would say there always were but in a somewhat mitigated form. You win notice that, for instance, one influence of the book is that people started to use matrix elements in the Dirac way, instead of having subscripts and that, I would say, was almost universally done, so there there was definitely an influence. But as a matter of fact, when I had to calculate this scattering of Compton radiation by bound electrons, I told Pauli that I had to sum over states of positive energy of the Dirac electron, and Pauli said, "Well, there is a method of Dirac to do that which is somewhere in the Cambridge proceedings. Let's look, it up." That was the idea of introducing a projection operator, not for the relativisitic case, but be does it there for some other examples. He may also have done it for the relativistic case; that I don't remember, but in any case I used that in this little paper, saying we would carry out the summation using a method due to Dirac, Cambridge Phil. —. And I then explained the method, but in a little more "down-to-earth" terms whereas in this Dirac publication it had been fairly incomprehensible. People have often ascribed the trick to me, but it is really a method due to Dirac. But Pauli was very good in that way; he was, of course, terrific in all respects, but if you were working on something, he would always say, “Yes, I think you could use there a method Mr. So-and-So must have used.“ “Let’s look at this paper of Dirac” was just one example, but I think it was also true in many other cases that he knew of some way of calculating an integral or some way of solving this or that problem.

Rosenfeld:

Oh, yes.

Kuhn:

I've heard it said that once he read a paper he remembered from then on its content, the journal, the author, the page.

Casimir:

I don’t think that would be right; I don’t think he would remember the page or even all the details, though he could probably reconstruct it if it were mathematics, but he would not have it at his fingertips. He would know the gist of the argument, the author, and roughly where the paper was published. As a matter of fact, his normal lectures on theoretical physics were very bad — or perhaps not very bad, but not good — unless he really prepared them. If he took the trouble to prepare them well, they would be excellent, but very often he didn’t do that, and just before a lecture he would glance through some lecture notes and say, "Das werden wir schon fertig bringen.” But it isn’t as if he had the mathematics at his fingertips in such a way; let’s say that, from that point of view, Fermi was much better. I think if you took. one of the reasonably advanced things in theoretical physics and asked Fermi at short notice, or perhaps even to give a lecture on it, I think you would get an almost perfect lecture with the formula nicely in place, good derivations, and all that; and that was certainly not the case with Pauli. I also remember a lecture he gave on relativity in which he was deriving the red shift, and somehow, which often happened to him, he got the sign wrong so that it became the Violetverschiebung. Then he began thinking about that, not saying very much but just going to and fro, changing a plus into a minus sign and back again,’ making a few gestures, saying a few sentences which had neither head nor tail, and that lasted. for quite some time, Finally, with a few sign shiftings here and there, the desired sign appeared there, and he said, “Ich hoffe also, dass sie jetzt alle deutlich gesehen haben, dass es sich wirklich um eine Rotverehiebung handelt." I remember a curious thing happened there. I spoke about Stern who measured this proton moment, and there was a nice little trick there of which the answer was due to Fermi. In these measurements of Stern you had to correct for a rotational magnetic moment for the two nuclei and the electron cloud, and

Stern had asked how large this correction was. Some theoretical people had taken approximate wave functions for the outer electrons, regarded the whole as a solid charge, and then worked out the rotational moment. Then Stern had some more subtle methods to really determine these rotational moments which came out quite differently; this seemed rather incomprehensible, and Pauli couldn't offer an explanation. Then came a letter from Stern that Fermi had explained it. As he said, "Die Elektronen rutschen," and the question is: if you have this rotating system, you really have to say that the cloud is not really quite going around with the rotating nuclei, but if you really see what you are doing when transforming to a rotating system of coordinates and so on, you might say that the "Elektronen rutschen", a backward movement, which meant that this moment was very much smaller than you would expect otherwise. It's a rather subtle point, and I remember Pauli was rather interested in that because I had written my thesis on the rotating molecules. He said, "Sie haben ja ein Buch darueber geschrieben, und der Stern sagt, 'sie rutsehen'; erklren Sie mir das." Well, when you knew how it was, it wasn't so difficult to explain. That was quite an amusing effect in those days. Of course, another thing was that Pauli had learned to drive a car and that gave life a peculiar flavor; I always claimed later, and Pauli did not deny it, that there was a tacit understanding between us that I wouldn't say anything about his driving as long as he wouldn't say anything about my physics. Without being unduly proud of my physics, I think I could say without boasting that it was slightly better than Pauli's driving, which isn't saying much because Pauli's driving was just awful in those days. I said that much later when we were together after the war and somebody asked, "Didn't you have a very difficult time with Pauli?" I said no, because he wouldn't say much about my physics if I wouldn't speak about his driving. "Ja, ja," said Pauli, "das ist richtig, so war es. Ich fahre jetzt kein Auto mehr, ich fahre kein Auto. Ja, ja Sie machen ja auch keine Physik mehr; also die Sache stimmt noch immer." And, of course, the high spot in this car-driving of Pauli was the Physiker Tagung at Luzern; Pauli had driven us to Luzern with Bloch and David Inglis and on the return journey there were both Bhabha and Elsasser, one of whom had missed the train or something and the other of whom was with us also when we drove to Luzern. Driving to Luzern was all right, and then we're eating in the evening, and Pauli was drinking orange juice but it was clear that he didn't like it. He said, "This (evening I'm) all right." Then all of a sudden Pauli switched and ordered himself a whiskey soda, so we kept an eye on him and when he had ordered a second whiskey soda, and I think, a third one, his passengers consulted and we said, "Now this is getting dangerous, so what are we going to do?" We made a plan to offer him drinks, get him quite drunk, and then Inglis would drive the car home; Inglis was one of those Americans who have been born and bred in cars and, even more American, a very good and competent driver, so we felt quite happy. The first part of the operation went according to plans, Pauli got quite drunk, and we said, "Now wouldn't it be better if Inglis drove the car home?" Pauli said, "Oh, no, no question of that. Ich fahre, und ich fahre ziemlich gut ." Well, all trains had gone by that time and also we felt we couldn't leave him alone. "Ich fahre nach Hause. Wenn Ihr nicht mitfahren wollt koennt Ihr hier bleiben." To let him go alone, we felt, we couldn't do anyway, so there we were and, I think, we somehow packed ourselves with four people in the back seat and Pauli driving with Inglis sitting next to him, ready to try to save something if something could be saved. It was really awful. He started by sounding his horn, he skidded against one curb and across the street to the other until he got going, and then he started screeching around corners. And he was a bad driver in the first place Inglis sitting beside him tried to sober him somewhat while Pauli kept saying, “Ich fahre ziemlich gut,” and when Pauli went around corners in a terrifying way Inglis would say very severly, “Das heisst nicht gut fahren.” There was one incident where the full moon came over the top of the hill and Pauli swore at the driver for not dimming his headlights And another one where he said, “Wir machen jetzt einen Abstecher,” and he went along a little trail which ended unexpectedly At the wagonshed of some farmhouse and where he also began swearing, saying, “Was fuer ein Unglueck war das?” and “Ein Wagen schon ueber meinen Abstecher?” We got home somehow, but we never forgot it. I remember seeing Inglis again in '48 or '49 and I said, “Dave, do you remember the drive with Pauli from Luzern to Zurich?” and he said, “Will I ever forget it?” Well, that has nothing to do with physics. But as I said, Pauli wasn’t quite a happy man in those days; I don’t think he really got much fun out of this sort of thing. Well, Kramers —

Kuhn:

When did you first meet him?

Casimir:

I mentioned this first lecture that he gave when I was a very young student. And then later when I worked

Kuhn:

You think that he had probably just come to Utrecht then?

Casimir:

Yes, I think so. Then later I suddenly knew that he was at Utrecht when I started to work seriously with Ehrenfest in the autumn of ‘28; he and Ittmann came regularly to Leiden from time to time.

Kuhn:

Then before Kramers I gather that there had not been an awful lot of contact between Utrecht and Leiden?

Casimir:

No.

Kuhn:

But there was fairly regular contact then?

Casimir:

There was then contact, but it was not very regular. Then I saw him off and on, and I saw more of him later in ‘34, of course, when I was his assistant at Leiden, and in ‘33 in that intermediate period after Ehrenfest’s death. Of course, the early days of quantum mechanics, so to say, coincided with his settling in at Utrecht, which may, in a certain way, have slightly hampered his own work. But I have always felt that Kramers was definitely one of the outstanding masters of the art and had an understanding of the theory and a mathematical skill that were quite outstanding. I would say that in virtuosity in solving certain problems, I think, he could compete with Pauli; he was extremely good at that sort of thing. And I have always felt that his contributions in those days were not quite on a par with his understanding of the theory and its principles and his command of mathematical methods. Why is that? He devoted a lot of time, for instance, to this problem of the asymmetric rotator and to developing the theory of Lame functions and so on, which were all very clever but not very useful. It was very difficult work and well done, but you might ask if it was entirely worthwhile. There is some rather complicated work on paramagnetism of oxygen where some group theoretical methods are being used and where you have the feeling, “Is it worth all the trouble he is giving to it at this moment?” One thing that is evident is that he wasn’t out for cheap success, or, in other words, to solve problems that he felt were easy to solve was a thing that didn’t amuse him. He picked things that were difficult, and I would say that he sometimes picked them almost for their mathematical difficulty rather than for their physical content or importance. He was almost a bit quixotic there, I would say.

Rosenfeld:

Yes, I quite agree with that.

Casimir:

In Leiden they had some beautiful large barrel-organs, the kind of thing they also have in Amsterdam, and that was the one thing Kramers loathed — this sort of easy, simple, and mechanical music. I was always amused by it, finding it so crazy when some of the classics were perverted on such a (“piriment”?) , as they call it in Amsterdam. But Kramers really suffered when he heard such things. I sometimes said to him about his physics too, ‘The trouble with your physics is that you don’t like draaiorgeltje.”

Kuhn:

What would he respond when you talked about his physics in that way?

Casimir:

He would just smile. Of course, I wouldn't really criticize it. For instance, if you would conare Kramers and Bethe, the output of Bethe in those years was very much higher, but I would, say really that the mathematical skill of Kramers, though be did not have Bethe's speed and fluency, when he did solve a problem, was really more ingenious, and his physics was definitely more subtle and more profound.

Rosenfeld:

I remember one case in which he seemed to have doubts about what he was doing; that was in the work on order-disorder.

Casimir:

Yes, he also put a terrific effort into the order-disorder problem.

Rosenfeld:

I remember that he once asked me if I knew something about some recondite part of mathematics because he had just reduced the order-disorder problem to one special question involving, I think, number theory. I said nothing, and then he said, "Don't you think the problem is worth putting this mathematical effort into it?" I had not said anrthing at all, I had the impression that he himself felt doubts about pursuing that and that he was a bit conscious of treating the problem as a kind of challenge to his mathematical subtlety.

Casimir:

He tried to draw me into that work and I didn't respond much; for one thing, it was too difficult for me. This type of mathematics is not the sort of thing I can do, and somehow, I wasn't very much fascinated. I said, "well, here we have the system of these equations, we have a good approximation at high temperatures, a good approximation at low temperatures, and somehow, they must be tied together." But of course, the interesting thing for him was to see whether you could find exactly by what kind of singularity; since these solutions were quite different, it was also fairly evident that there must be some sort of singularity. I was not so much interested in this, unless one could see a very elegant way of doing it, with a very new type of mathematics or something; but to do a lot of struggling with complicated and ever-more complicated formulae, to try to get the thing was something I was not much attracted to, and that was, of course, what he did. Then he put quite some effort into developing his own brand of group theory, so to say, his (psi-eta) methods, which is good formalism but it was really only used by his pupil [Hendrik] Brinkman and perhaps by (Wolfe) and one or two other people. But no one really used this formalism, though he did very clever things with it, for instance, the coupling between two vectors when it is not a Lande coupling, but also a higher order coupling, let's say, with second or third harmonic of the mutual angle between the things. Heisenberg, in one of his papers, uses one of these higher interactions and writes down a general formula which is all wrong and which he then applies only in one special case where the result is right! That's characteristic of Heisenberg.

Kuhn:

To what extent do you suppose this characteristic of Kramers' work at Utrecht and Leiden may relate to a perpetuation of the division of functions that had come when he worked with Bohr? That is, he was, I take it, the person who did the mathematics in his work with Bohr, and I wonder whether he may have to some extent drifted in as being the mathematician.

Rosenfeld:

I think he had a bias toward mathematics from the start. I remember Bohr telling me that when he came here and asked whether he could work with Bohr, Bohr called his brother Harald who was the appriser, the practical man who made the practical decisions. "He told me such learned things that I could hardly understand," Niels said to Harald, "and what can we do with such a mathematician?" Then of course Harald said, "Well, take him." But it was characteristic that Kramers came to Bohr with some ideas of his own of applying mathematical techniques to quantum theory.

Casimir:

Of course, later he became interested in a lot of things relating to more practical work, too; for instance, work on magnetism and the work he did with Becquerel on the theory of magneto-rotations, and later also, in advanced kinetic theory, there is his work with Kistemaker which is very beautiful work. But also there, I would say, he was interested in the theory of non-uniform gases, the whole Enskog-Chapman story and so on, from which he derived new effects; his approach was always a little bit mathematical, I would say, and he liked to create mathematical methods of his own, as he did in playing with invariants where he worked out these new formulae and so on. He did not always find followers for his particular way of doing it. Some of the things are very elegant; for instance, there is a beautiful little paper on the theory of bands in one-dimensional periodic structures to prove that if you have an arbitrary periodic stcucture in one dimension, you always get bands of energy levels separated by empty states. This he proved by very general methods and very elegantly. Also in his book, of course, there are a lot of things which you don't find elsewhere and where he gives very elegant proofs, very beautiful ways of calculating normalization integrals and so on; he put a lot of time into writing that book in later years and in thinking over once more the basic principles of theory. Thinking about that and writing the book brought him really to the first steps in renormalization theory. But his own way of tackling it, by trying to separate the internal and external fields, did not turn out to be very fruitful. I think you might say that in some ways he was more a mathematician than a physicist, but then really that is saying too much; but let's say that he put a rather great emphasis on mathematical theory. He was afraid of over-simplification, but that sometimes led to his making things a little bit complicated. He was not tempted by easy or cheap success but always wanted to do difficult and beautiful problems. And sometimes it may have been a bit of a question of good luck or bad luck in certain of the things he tackled.

Kuhn:

Did he talk at all about his time in Copenhagen?

Casimir:

I had not talked with him much about that, though we spoke sometimes about personal circumstances, recoilections and so on. Of course, his wife always felt that he had been insufficiently recognized, but then she was that sort of woman.

Rosenfeld:

He did not give the impression of holding a grudge on that score?

Casimir:

Oh, no. Not at all, not in any way. But he wasn't that sort of man, anyway.

Kuhn:

Something you said yesterday made me think that perhaps there were other things that you had to say about the earlier period, which is why I —

Casimir:

I don't think so. Who was it who told that the difference between Kramers and Heisenberg, when they were both here in the days of the dispersion formula,was that Kramers always had the door of his office open and Heisenberg always closed?

Rosenfeld:

I hadn't heard that.

Kuhn:

I hadn't either.

Casimir:

I heard that somewhere. So Kramers really spent a lot of his time helping other people, and he was extremely well liked by the whole Scientific community, who thought he was a great help in many ways, whereas Heisenberg worked more for himself. I heard that remark somewhere and there may be some truth in it. He certainly must have given a lot of his time to the problems of experimental physicists and mechanics and so on.

Rosenfeld:

And Christiansen. He was a great friend of Christiansen. He was also traveling around through the country giving popular lectures on physics. There are some gibes about it in Pauli's letters.

Casimir:

Also in those days of silent movies he worked on explaining a film that had been made on Einstein's theory of relativity; in those days a film was accompanied by an "explicateur" and Kramers did that to earn some extra money. In those days you also had to have an orchestra, so first you had to have a musical introduction, and Kramers said, "We selected the 'Ouverture Egmont' to introduce that film." I asked him why Egmont and he said, "It's a good introduction to relativity." I have not quite been able to understand why, but probably that is due to my lack of musical feeling; anyway, Egmont was chosen, perhaps because the orchestra probably had a rather limited repertoire. Kramers later said he could never hear Egmont being played without thinking of aspects of relativity, and when the last chord crashed, he would have to step forward and say "Mine Darner og Herrer" and begin to speak about relativity. He also told me that that was the occasion upon which he acquired some of the professional's attitude when you know you can give the same performance any number of times, which we usually don't reach as physicists.

Rosenfeld:

There used to be a huge piece of quartz crystal on the mantelpiece in Bohr's office, and he explained that this actually had been stolen from the mineralogical museum; when Krarners went around lecturing about physics, he carried it in his coat pocket to show what nature could do with atoms.

Casimir:

There is still the whole question of the relations between Ehrenfest and Pauli, which are well known and have often been told, of course. That was very curious. Ehrenfest had great admiration as well as great liking for Pauli, but at the same time he felt slightly ill at ease with him. The result was that they were always making nasty remarks to one another, some of which have become quite famous. One of the more funny things is this, and I think the piece of writing is worth reading. When Ehrenfest presented the Lorentz medal to Pauli at the Academy of Science in Amsterdam he made the presentation and made a beautiful address to Pauli about Pauli's work and so on, This probably is in Ehrenfest's collected papers, but there is one little thing which is not there, This being a rather formal occasion, Ehrenfest had said it was rather inportant that Pauli come in a nice black suit or something suitable for the occasion and this Pauli at first refused to do, but then said, "Well, I'm wining to do that but you will have to mention this in your presentation address." And Ehrenfest did, but it is not in the printed version, but it is almost. [leafing through Ehrenfest' s Collected Scientific Papers to p.619. After explaining that it is because of the exclusion principle that a piece of metal is so voluminous, Ehrenfest says "Sie muessen zugeben, Herr Pauli: Durch eine partielle Aufhebung Ihres Verbotes koennten Sie uns von gar vielen Sorgen des Alltags befreien; zum Beispiel vom Verkehrsproblem unserer Strassen." And then he said in his presentation, "Und auch von dem Problem, wie man die Kosten fuer schoene neue schwarze Festkleidung etwas herabsetzen koennte." Pauli, of course, had been waiting for it. That was quite interesting, and interesting also is this paper he wrote quite later on, "Einige die Quantenmechanik betreffenden Erkundigungsfragen" to which Pauli also wrote an answer; I think that answer is also in a way quite significant, because you would see some of Pauli's attitudes there. Well, that's about Kramers. Ehrenfest, of course, was in a way a magnificent lecturer. I wouldn't call it a shortcoming, but I have mentioned that he stressed the logical content and the structure of the theory rather than its quantitative applications; still, he always looked for very striking, graphical ways of expressing himself, illustrating things by means of simple models, etc.

Kuhn:

To what extent do you suppose the problems of converting to quantum mechanics contributed to the discontents that ultimately led to his suicide? I gather the conversion would have been difficult for anybody but perhaps more difficult for Ehrenfest's sort of mind than it would have been for some others because of the mathematics it involved.

Casimir:

I don't think that the questions of the interpretation of quantum mechanics and so on contributed to it; what certainly did contribute to it is the feeling that he could no longer master the most advanced stages of the theory. He felt that some of the mathematics, some of the more difficult developments of group theory, some of the field quantizations and all that sort of thing, be didn't really understand. Either that he didn't understand that in the way that he wanted to understand things, or — [Break to answer phone] About Ehrenfest's last years. There was suddenly this feeling which I had, so to say, seen coming, of no longer being able to grasp modern developments, no longer being entirely on top of his subject, getting a little bit out of the development; that was a thing he found difficult to accept. After all, he still knew a lot of things, he could still lecture on a lot of things, he could still criticize a number of things, he could even have done a lot of things himself, but he always had had this wish to be able to understand the latest developments of physics, to try to bring them into a very clear-cut form. He was willing to leave some of the mathematical techniques and all that to others, but he felt that things were escaping him. Einstein once wrote somewhere a necrology where he put this as the essential point. He said this was a man who felt he had to be a great teacher and when he felt that his powers as a teacher of the most modern things were declining, be went out of life. I don't think it was a simple as all that, but this feeling, which I suppose every one has occasionally and which everyone has around 50 or 55, suddenly played a role; that was one thing. I think the whole question of National Socialism in Germany certainly certainly must have played a role, too. He was very much attached to Goettingen, always went there and took his students there, and to see that all that type of German cultural life was being destroyed was certainly something he took very much to heart. His whole attitude to the Jewish question was a remarkable one; he was in a way very sensitive there. Whether he believed it or not, he always pretended that he really thought that being of Jewish descent was almost indispensible to doing anything worthwhile, in science at least. That was a very remarkable trait of his; I hadn't seen it so pronounced in anyone else. I remember my friend Rutgers telling me that he found Ehrenfest looking and looking again at a photograph of Lorentz and discovering, to his satisfaction, something in his face which he thought more Jewish characteristics. He said to Rutgers, "Ja, ja. Das war doch einer von uns. Das muss einer von uns gewesen sein." To me, he used to say, "Wenn du nur ein Viertel juedisches Blut haettest. Und wenn es nur ein Achtel waere, da koennte noch etwas aus dir werden. Du bist aber viel zu Arisch." In my family it's really a rather difficult thing, even with a lot of good will, to discover any Jewish blood. In this Frisian peasant stock of mine it was a little bit hard even for Ehrenfest. Very remarkable — not quite funny perhaps, not really. But you can understand under those circumstances what the whole of Hitler and National Socialism must have meant to him, so that was the second question. Then there was the whole question of his marital situation and his not being able to make a decision as to whether he would go away from his wife, to whom he was very much attached, or not. That situation may well have given him the final blow; he was somewhat entangled at that time with another lady who in a way was quite a nice woman. I once had a heart-to-heart talk with her for a whole evening. She had a lot of Ehrenfest's papers which he left with her and they are now with Martin Klein. She was quite a nice and well-bred person in a way, but —

Kuhn:

Did he talk to you at all about his feelings about Einstein and particularly about Einstein's failure to follow the new interpretations?

Casimir:

No, not in particular. He was, of course, at the Solvay conference when these things were going on, and there, I would think, his great admiration and friendship for Einstein notwithstanding, he sided with Bohr and he thought Bohr came out on top. On the other hand, he took Einstein's objections more seriously than many other people did. I told you about this colloquium where Einstein spoke about these things and said, "Die Sache ist schon widerspruchsfrei, aber hat innnerhin ein gewisse Haerte." Well, the younger generation hardly felt 'diese Haerte' and I think Ehrenfest recognized that there was a bit of 'Haerte' there, that it wasn't quite so nice to have to accept these things. I don't think that that was a thing which unduly upset him in any way, but he did feel, of course as you see from the difragen, a little bit unhappy about some of the aspects of quantum theory. You could also see in Ehrenfest's later years that he tried to maintain his own style and that it became an effort, that it was no longer quite natural. You must have noticed that in reading. He was forcing himself to his old vivacity and vivid ways of expressing himself while he was really rather miserable in many ways. A curious thing just before his suicide was that the man who realized most clearly that something was very wrong with him was, of all people, Dirac. I think Mrs. Bohr told me that. Ehrenfest was at Copenhagen. very shortly before his return to Holland and the disaster, and I think, Dirac said, "This man is mentally very ill and one shouldn't let him go alone." I think Mrs. Bohr told me that. That it was Dirac is very curious, though, of course, he had known him and had been at Leiden with him, but you would perhaps at first sight not expect him to have such insight into human nature. I think what finally triggered the thing, as this mistress of his told me, was that he couldn't make up his mind whether he would do the one or the other. She said that finally he was relying on her more and more and yet was in a way so independent mentally that she couldn't stand it any longer and said, "Decide one way or another, but stop this sort of "zwaaien zo" as we say in Dutch. Then he didn't know what to do; but this I think was only secondary, although it triggered the thing. Well, that's that; I don't think I know much more that would be relevant. [Another break] In Gottingen in those days people were amusing themselves with the following game: all normal mathematical symbols, plus and minus, a sigma, a product sigma perhaps, and a factorial are allowed; try to write all numbers using the figure '2' four times. For instance, write 1 as equal to 2 plus 2 over 2 plus 2; 2 is equal to two over two plus two over two, and so on. Some people had gotten up to 20, some to 24, and some to 30 and Dirac had been thinking about that problem quite a bit. They asked him whether he had done anything with that and he said, "Yes," so they asked him how far he had gotten. "I can write the number n with [four] figure '2's'," Dirac said, and produced the following formula: which was much admired but which took all the fun out of the game. No one played it after that

Kuhn:

For the recorder, it's minus log to the base two of log to the base two of the nth square root of two. When was this that this game was played?

Casimir:

I have been in Goettingen twice and this must have been the second time I was there, so that was summer '29. But it was strictly within the rules of the game.

Kuhn:

Is it? The use of the n on the right hand side —

Casimir:

But you don't write the n really, so if you ask "Would you please write 935," you would just take 935 roots. Definitely, square root was allowed, and it came in very handy sometimes. I do remember the letter quite well; there was something about the "Ritterschaft" and that sort of thing with which Ehrenfest was thoroughly amused. I had been here for some time and I had just completed the thesis, which I defended in October, so it was just the question that Ehrenfest didn't have many collaborators at that time and he would have liked me to have been back at Leiden, perhaps slightly in connection with this fact that be was beginning to feel just a little bit uncertain about himself, because the thing repeated itself later in '32. I came back to Leiden then, was there that winter, and I had thought that I would have liked to stay on a little bit in Copenhagen, but Ehrenfest said, "No, I need you here at Leiden." So I spent that winter at Leiden. I then got this offer to come to Pauli and Ehrenfest, I think, wasn't quite happy about the work I had been doing at Leiden that winter, and rightly so probably. So I went to Zurich and again that question arose; Pauli wanted to keep me at Zurich one more year whereas Ehrenfest again wrote a rather emphatic letter saying that he wanted me back at Leiden. There, I think, he was already thinking about taking his own life and he also knew that it would take some time before a successor was appointed, so he wanted to have someone there in the theoretical physics department. That was another thing when he insisted on my coming back, so this happened twice, once when I was at Copenhagen and once when I was at Zuerich. But in a way, it's quite understandable since he felt that most of his people had gone away and that he would like to have some assistance and,as I say, he was already feeling a little bit uncertain of himself anyway.

Rosenfeld:

Did that letter from Klein help?

Casimir:

No, he laughed a lot and thought it was very amusing, but I went back to Leiden all right; I think it straightened things out. There was a little bit of ill feeling here and there and then some arrangent was made. But that was that question.