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George Uhlenbeck – Session I

March 30, 1962

Interviewed by: Thomas S. Kuhn
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Abstract:

This interview was conducted as part of the Archives for the History of Quantum Physics project, which includes tapes and transcripts of oral history interviews conducted with ca. 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: Henri Abram, Niels Henrik David Bohr, Max Born, Louis de Broglie, Max Delbruck, Paul Adrien Maurice Dirac, Tatiana Ehrenfest, Paul Ehrenfest, Walter M. Elsasser, Enrico Fermi, Ralph Fowler, Samuel Abraham Goudsmit, Werner Heisenberg, Oskar Benjamin Klein, Hendrik Anthony Kramers, J. P. Kuenen, Otto Laporte, Hendrik Antoon Lorentz, J. Robert Oppenheimer, Wolfgang Pauli, Isidor Isaac Rabi, Harrison McAllister Randall, Julian Schwinger, Arnold Sommerfeld, Llewellyn Hilleth Thomas; American Physical Society meeting (Boston), Huygens Club, Kapitsa Club, Rijksuniversiteit te Leiden, Technische Hogeschool Delft, University of California at Berkeley, and University of Michigan.

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Uhlenbeck:

Ehrenfest was a man who always had to get it out of his toes. He had, somehow, no technique. Nothing was in his fingers. He always had to think it out completely from the beginning. Although he knew mathematics it was not simple for him. He was not a computer. He could not compute. That's the one thing I never learned from him. I had to learn it all by myself later on. There were people like Sommerfeld and Born, who immediately took up an idea and made calculations. He was a little bit frightened and also disgusted by them. Always "diese komplexen Integrale." Sommerfeld, as soon as an idea comes out, in the next paper does some complex integrals, computes all kinds of things. That was also true of Born. In several of my visits with Ehrenfest to Gottingen the contrast between these two was very striking. Born, on the other hand, was slightly afraid of Ehrenfest. Born was afraid that he'd make the wrong calculations, and then Ehrenfest would, by simple models, by simple logical algebra, show him that it was wrong. Of course I don't know whether that happened, but he was somehow a little bit cagey about it. Ehrenfest on the other hand was of course impressed by all the mathematics.

We had the colloquia and seminars — Wigner was also there — he can still remember that too, I am sure. Here Ehrenfest always wanted to have the simple point, "what is the point". Can one say that, so to say, in a few words or with a very simplified model so one could see what the point was. Often he was unable to do it, usually unable. And then Ehrenfest … jumped around. It was always so. It was disgust, really, all the time, all the time. And he never stopped. In a certain sense it was also his tragedy to be interested, because you see when he got older it got more and more difficult for him to get it out of his toes, to learn it all. People like Sommerfeld and also Lorentz, and surely Born, could always fall back on the technique. They could make long calculations. Maybe in the fundamentals it was not very new, but it was always interesting. The electron theory of metals of Sommerfeld was not at all original with him. The idea came from Pauli and Lorentz originally. But then he worked it out so far that it was a whole school after a while. The Sommerfeld electron theory of metals was due to this remarkable facility of Sommerfeld of working things out completely and in great detail.

Then was the problem of quantum mechanics and Ehrenfest. I think he always hated it. Oh, and then there was this whole generation which came with it. All these youngsters, who had, with great facility, made these calculations because it was, so to say, a technique which was given you, and you didn't have to understand much. You just computed, and you did this and you did that and everything came out. Ehrenfest said. "Diese Klugscheisser." "Always so clever they were! And nobody understood anything." Which was partially true and partially wrong of course. There was, therefore, a mathematical apparatus built with Hilbert's basis, with operators, which had a sort of abstractness. It was so against his creed that I am sure that he suffered from it. I remember that we talked. so much about one of his last papers. He was rather proud of it. That was the

"Erkundigungsfragen". That was a typical paper of Ehrenfest. That was, I think, the last one he wrote. It was in '30, I think. I was in Leiden. I went with him to Gottingen. Then he wrote this paper in his inimitable way. He wrote a series of questions. Very humbly he says, "Einige Erkundigungsfragen". He wanted 'only a few points of information.' Then he wrote, typically, number 1, number 2, etc. The paper is still worth reading. Pauli took it up, and answered it, point by point, very respectfully and, nicely, and also very clearly. It was so nice of Pauli. It was very sad that it was not yet known. The answers to these questions were not quite known hen. That gave Ehrenfest quite a kick, "that it nicht tun was. Gar nicht tun, nicht tun." This is typical now too — this remarkable facility that certain people have with the mathematical formulas. There is, of course, always this tendency. It's like a machinery which runs wild. It doesn't churn up anything of real value. That was not true of quantum mechanics, of course, because it was really something which any child could use if he knew anything.

Kuhn:

Did he see this about it?

Uhlenbeck:

Well he understood it all right. He said he was just too old. It was against his creed to really take part in it. Although he did take part in it. We, in those later years, did all these calculations. Ehrenfest had a good education all right. So he could finally work with spherical harmonics and what not if it was absolutely necessary. He did it too! But mainly I was the one who did all these exercises following the papers of Schrodinger when they came out — the perturbation theory and what not.

Kuhn:

Was there an intervening period in which you started doing the matrix mechanics and then switched?

Uhlenbeck:

Those came quickly after each other. We studied matrix mechanics too. The Drei Manner Arbeit — Born, Heisenberg, Pauli — written by Born, Heisenberg, and Jordan. But with it you still couldn't make little problems for yourself, because it was all so unwieldy. As a result the young people did not have the feeling then that they had the key. That came really only after Schrodinger. Then people had the feeling. The equivalence of the two was I think first shown by Pauli… Well it was Eckart who showed it too, but he was, of course, still in the wilds. America was in the wilds. He did it remarkably on his own. But Pauli in a letter to Schrodinger showed the identity. That's also mentioned in one of the Schrodinger papers — that Pauli saw it right away. I still remember that I then computed the intensity of the Balmer lines. You had all these formulae already. It only was those damn difficult integrals! I liked, already at that time, to compute such integrals, and I did it. But I made a mistake. I made a mistake. And out of the mistake it followed that even for the principle quantum number there would be a selection rule.

Now that was of course nonsense, it couldn't be. Ehrenfest says, "The complete theory, pfft." He couldn't see what the error was. He said, "well you write it all down, nicely, as a letter to Born." I was a young student and I did it. I wrote these integrals nicely, and wrote it all out. And Ehrenfest sent it off to Pauli with a little footnote, in which he said, "Bitte, behandle die Tiere sanft." This is only said in wildlife, … Pauli then answered right away. I still have that letter of Pauli. He says, "Ja, ich habe mich auch mit diese Integrale umgeschlagen." Then he pointed out very carefully what my error was. It was just a substitution. I had forgotten to take something into account. Then he gave the answers, which were also the first answers. He had found those in Copenhagen — typical Paul — when he was in one of these cafes. He was always computing in these cafes. He said … "How to do it." And he gave me the answers. Then there was also a little postscript, so far as I remember, to Ehrenfest, saying that this letter was very sanft. It was really very nice of Pauli to write me without any sarcasm whatsoever.

Kuhn:

I didn't know he could do that. We hear so much of his sarcasm.

Uhlenbeck:

Oh yes, he did. He was really at the bottom a very friendly fellow. He was a man who in his early days always — and it was always clear, at least to me — tried to see whether a person had an area of sensitivity. If he found it, then he pushed, certainly. If he didn't find it, well, then he looked at you. He always said something, and then he looked at you to see whether that hit something or not. I always had to laugh because I saw that he wanted to do that. I remember in Ann Arbor then I told him, "ja", I was working on Brownian motion. Paul remarked: "Desesperance physique! Typische desesperation physique!" Then he looked at me. I said, "Es ist schon wahr. Es ist schon wahr." Then he saw that I was completely untouched about it because I realized that it was so. So afterwards he always said to Elsa, "Ja ja, that Uhlenbeck, such a starken (grad)!", because he couldn't get my goat. He was a charming man. I was very close to him. Especially during the war. I have such a beautiful letter. He was at Princeton and I was in the Radiation Laboratory. He had a rather bad time. He was here and was quite unhappy. He was alone at Princeton, really, and there was really nobody to talk to. He was a refugee in a sense, and he didn't think that they treated him very well.

Well we were both of course in the midst of the war. Ivy whole family was in Holland and in the Dutch East Indies. No I liked him very much really. He was much sharper to the older people than to the younger people. Born was really afraid of him. Ehrenfest too, although he got along with Ehrenfest very well. They were always kidding each other –- in a sharp way all right. Of course, with regard to that kind of wit Ehrenfest was his equal. He could do it back just as readily. There was this famous story about what Ehrenfest said of the Pauli effect. I don't know whether you have heard of it. This was a famous story. The Pauli effect was that when Pauli came into the laboratory, everything went wrong. He was extremely proud of that. That, he thought, was really remarkable, and he believed it really true. I still remember that in Ann Arbor we went to the lake. On the way back the car broke down. Pauli got out of the car muttering, "Pauli, it is the Pauli effect." Although it was a joke, of course, yet he felt, "well, maybe something — ." Not at the same time, but an analogous happening Ehrenfest said, "Ah, Pauli. This Pauli effect is very trivial. You know, this is only a special case of a very well-known effect, that ein Ungluck kommt niemals allei." And Pauli objected to say "It comes naturally." Ja, that's a famous "Ehrenfestis." Ein ungluck kommt niemals allein. When Pauli comes in, and everything else comes also bad. He believed in Jung, and he was a very good friend of Jung. He had a little bit of mystical connection. I had long talks about that with him. One of the remarkable things in which he believed was extra-sensory perception. He believed in that. Whether it was proved, he was not sure. But that it existed, he had no doubt. And that it could exist. He says that there was something like that which is so completely separate from physics. He had also a remark he once told me when we talked about biology, and about the problem of life.

This was in his youth, that I talked with him about that. He told me he did not believe that in all these questions of evolution everything went by random mutations. That was just at the period that Bohr and Heisenberg also and of course Delbruck began to think about complementarity and these things. And Pauli said, "Ja, diese wage are just as strange, just as separate from physics as extra-sensory perception." "Delbruck could believe! Delbruck believes it!", he said, but he couldn't believe that by just random mutation, by just pure chance, one could get this whole development. He could not believe it. He had read a lot in these things, too. He was a very cultured man, a very cultured man. He had all kinds of technical examples which I didn't know anything about, but apparently about which he had talked with Heisenberg and with Bohr. These three were rather close. They talked about many other things. Bohr and Pauli were very close.

Kuhn:

It bothered him not at all that there should be realms quite outside of physics?

Uhlenbeck:

No, no.

Kuhn:

He felt no conflict?

Uhlenbeck:

I don't think he had really systematically thought about these things as if trying to make a unified philosophy. That I don't think he did. Maybe he did. But anyway, he was not convinced that with the quantum theory, so to say, the last word was spoken, or that it was even possible, in that sense. Pauli's paper on Kepler is most remarkable. I recommend it to you if only for the foreward. I read it with great interest. There is first, in this book, a paper by Jung. This is a typical Jung paper. It is so mystical and also a bit confused, that one doesn't know one way or the other what this really asserts. After he gives this account, Pauli's paper is a kind of an appendix which is written with the typical Pauli clarity. At least you know what he asserts. It has these mystical points, but at least one knows what he asserts. And, of course, it is a very nice paper. He has also papers — which I have never really seen — on the development of physics in the 19th century. Always with Pauli you at least know what he says. Which is very often not the case with some.

Kuhn:

Particularly with people who take so much of that attitude. Did you see signs of this same thing in his physics?

Uhlenbeck:

No. I don't think so.

Kuhn:

You would say that these were totally different aspects of the personality and kept very separate.

Uhlenbeck:

Well, he had this feeling of depth. One of the nicest things of his is in the Bohr Festschrift in which he writes a paper on the CPT theorem. Then he gives a quotation which perhaps would please Bohr, because it shows what Bohr always says a quotation from Schiller — …, "even if it is not quite clear, if it has only depth." Only by such remarks can you see that he had these things. Here, this is this interesting quotation: //This is about the neutrino. This is a letter which he wrote to Lise Meitner: "The gravity of the situation … is illuminated by the pronouncement of my respected predecessor … who recently said to me …, "Oh, it is best not to think about it at all."// No, the influence of Pauli should not be underestimated. It's really so deep, you know, on whole generations. More than anybody else, almost more than anybody else. Certainly more than Heisenberg and Dirac. He knew everything, and had a critical opinion about it. In addition he had of course enough original thought … — maybe no finally fundamental contribution after the exclusion principle, but —…

Kuhn:

When you say influence, then you mean mostly direct influence on people?

Uhlenbeck:

On people, yes. He was the conscience of physics. That's what everybody said. He was the conscience of physics. Ehrenfest was the Socrates of modern physics. He always questioned and said that he did not understand. But Pauli was the conscience. Pauli really, finally, accepted it. He was not easy to convince, of course, not easy to convince. But finally he accepted it. Then it was surely right, then it was really very strong.

Kuhn:

How did he feel about the relation between the new and the old? How did he feel about complementarity.

Uhlenbeck:

Completely convinced.

Kuhn:

Was he quickly convinced?

Uhlenbeck:

I think so. I don't know, precisely, that went, because I wasn't there at that time. The matrix mechanics he took up immediately, and he gave the solution to the one problem that was really hard, namely the hydrogen atom. That's what he did right away. The second Born book of quantum mechanics, which was completely matrix mechanics was, therefore, already a little bit out of date when it appeared. There is a rather scathing review by Pauli of that book. It came out in Naturwissenschaften. Born was very unhappy about it. In the book wave mechanics was not mentioned, although it was already known at that time. Pauli said that all these problems like the hydrogen spectrum were clone in this complicated manner by means of matrixes, which was really not to be defended. And this criticism, he said, is not due to the impotence of the man who writes the review. A typical Pauli paper! [coughing] He then immediately took up this wave mechanics.

Rather interesting and ironic in a sense is the Pauli spin paper, which was really a rather profound paper, because of the two valuedness of the wave functions. Pauli told me that he never thought that that was such an important paper, afterwards. But he says, "Ja, Es war viel wichtiger als ich dachte." He has told me that. And the point was that he was the last who accepted the spin, the last! He only did it after the Thomas precession was elucidated. Then he said "all right," but he still did not quite like it. At this time I was in Gottingen. Oppenheimer was there, and also Lande, and they all said 'well now we should make a quantum theory of it.' It was, of course, completely dark to me, how all that should be done in wave mechanics. Then Pauli did it. It was very profound really, because that was a big step to go from the scalar psi to the two component psi. I remember that I studied that paper very hard, and it was very clearly written. Except that it was so full of these transformations of these wave functions — the Caley parameters as I remember, which, of course Pauli knew. It sounded profound to me, and difficult too. Although again, it was that it was so clear that afterwards you could use it. Many people did of course.

Kuhn:

How did he feel about Dirac?

Uhlenbeck:

He had a great admiration for Dirac. This kind, of dream-like way of using mathematics and then getting something out. 'The Dirac papers', he said, 'you must read all of them.' He did, and he studied them backwards and forwards. One of the remarkable things about Pauli, really — in contrast to anybody else that I know of among these great physicists — is that he read everything. Everything! I've still got memories. He was at Princeton and there was the Physical Review. He began to read it, certain papers of coarse only. He didn't only read it, but he had paper there with a notebook and he made calculations. The whole paper I think he did that until the end, you know. All the renormalization, all these things. He followed them precisely. He knew them in detail. Which really very few people do. Who nowadays reads a paper? Very few people. And he did it until the end really. I was so surprised when I was in Holland, and I wrote a few papers on the quantum theory of the non-ideal gas. They were all right, I was rather proud of them too. But there were still difficulties. And then Pauli came out, and he said "Ja, dies war nicht tun." He had given colloquium lectures on them. He had read them precisely. Now, as a consequence of his following the literature so carefully everybody wanted to know his opinion. He fitted it right in where it belonged.

Kuhn:

How did other people feel about Dirac? Delbruck relates that to Bohr it was pure formalism.

Uhlenbeck:

That was of course quite different. Bohr has also not mathematical. Pauli was — just this dream-like mathematics. Mathematics guided him. Ehrenfest even read. Dirac's book. He studied it very hard — "ein greuliches Buch" — he thought it was simply terrible. "You can't tear it apart," he says, "You can't tear, it apart!" He had to follow it completely from beginning to end to understand it. He says "Ja, Dirac's book, genau wie Gips." So smooth. You couldn't tear it apart. He wanted of course to chop it up and say "What's all this assertion? What has he not done? Can't one —?" And it had this classical character that you could only say it this way and no other way.

Kuhn:

Then he said "Genau wie Gips", did he mean the character of the writing was the same?

Uhlenbeck:

I think he was speaking of the presentation. This kind, of didactic way, and smooth, as if it was absolutely necessary that this was the only way to develop the subject and there was no other way. That was something which Ehrenfest couldn't stand. He always says you must be able to say everything in three different ways. "Komme nicht auf einen (???)", he'd say. In Dirac's book, everything was so linear in the logic. And that he disliked, but he admired it too, of course, tremendously. Ein gremliches Buch, but he studied it very hard. I still hope that I can get it out of Ehrenfest's wife a copy of Dirac and a copy of Gibbs, which are simply full, full with Ehrenfest's notations. Everywhere. The whole thing — it's full. I think it is an historical document. But to get them out of Mrs. Ehrenfest is not so easy. She is not easy to handle. It was so difficult to get the letters out of her.

Kuhn:

I take it from what Martin says that there are still quite a few letters.

Uhlenbeck:

Sure, quite a number left. I only hope that they won't be destroyed. Still, Dirac was so different from the other people because he was always a little bit off to the sides — and because he was so silent. He didn't discuss very much ever as I remember, although I saw him only once in a while. He was in Leiden several times. Oppenheimer of course knew him rather well. Although I think he also had trouble with him in his Cambridge time.

Kuhn:

You know, I showed Maria Mayer that chapter of the Born biography about Oppenheimer. She says she doesn't remember it at all. Maybe it was she, but she doesn't remember.

Uhlenbeck:

I was in Gottingen, after I wrote my dissertation. That was a rather hectic year — the second year I was assistant to Ehrenfest. I was assistant to Ehrenfest for two years. Really, the second year I worked on the statistical mechanics of quantum statistics, the subject of my dissertation. Then it was very clear that if I stayed in Leiden as his assistant — you know you worked every day — I would never write it. I had to write it, because I had a job already at Ann Arbor. That was open to me before I got my Ph.D. I wanted very much to do it. Sam, after some hesitation, did too. So we both had to write our dissertations. Ehrenfest said, 'well, you go to Copenhagen. Get out of here and write it there.' Then under high pressure I wrote it, in two months I think. After that was over, I went to Gottingen — from Copenhagen to Gottingen, and after to Leiden. There was Oppenheimer. He was, so to say, clearly a center of all the younger students. I knew him there already, and in the early Oppenheimer period he was really a kind, of oracle. He knew very much. He was very difficult to understand, but very quick, and with a whole group of admirers. So that must have been the year that Born talks about. Maria was there too, and Joe Mayer, and Nordheim was there, and several other people. Robert was really one of the leaders there among the younger students. He had done, I think, his degree with Born, maybe half a year before. Again the one that appreciated it immediately was Pauli. He was the only one who understood it.

I doubt that Born understood it properly because it was really very complicated. It was on the continuous spectra, the normalization of the wave function of the continuous spectra, and so on. That of course was very good. Later one could do these things so much simpler. I stayed in Leiden for about a month together with Oppenheimer. He was, for a while, assistant of Ehrenfest… I don't know exactly the time. I went to America in '27, in August, together with Sam — on the same boat. In New York Robert received us. He was extremely nice, and put us up in a hotel and took us out to a remarkable place in Brooklyn. It was all so strange. Alice was rather ill. She was sea-sick all the time. But anyway, afterwards we went to Ann Arbor and he went to Berkeley. But then he went back again to Europe, and he was Ehrenfest's assistant, I think, it was for about three or four months. Then he got this terrible pneumonia. He had terrible trouble with his health, and he had to go to the mountains. He went to Chile. I think he still has a very good memory of Leiden and of Ehrenfest. In contrast, before him Ehrenfest had had as his assistant Elsasser. Elsasser was a typical German, in a sense… Ehrenfest didn't think so in the beginning, but found out while Elsasser was there that he was a ein "Klugscheisser."

Kuhn:

A what?

Uhlenbeck:

A "Klugscheisser." One with these cracker jacks which Ehrenfest couldn't stand very much. Ehrenfest was so sensitive. I wasn't there, but I heard from good authority that Ehrenfest was so afraid of Elsasser that he didn't dare come to the institute. Elsasser had to leave because it was an impossible situation. Ehrenfest stayed home. He didn't come to the institute. Now Elsasser had clearly to resign, and he did — after half a year, I think. That was the end of Elsasser, and then Oppenheimer came. I don't know precisely the time though. Oppenheimer liked Ehrenfest, and he was very patient, very patient. Ehrenfest didn't understand Oppenheimer at all well, but he at least was willing to try. He was very patient. Only the people who were so clever and didn't want to talk further about it or try to make it clear were of the type that Ehrenfest couldn't stand. Robert wasn't like that, although he was certainly —

Kuhn:

Well he's not always patient with people.

Uhlenbeck:

Then he was. Yes. That's why he had so many students. The impatient Oppenheimer was the post-war Oppenheimer… He was quite different before the war. I had much contact with him before the war, before I went to Utrecht in '35. A typical example of the situation was this famous Ehrenfest-Oppenheimer paper. Martin Klein had so much trouble with it. Ehrenfest was in Ann Arbor and there was this question: how to prove that if you have composite particles of which each component has the Fermi statistics, then the compound particle could have Bose statistics if composed of an even number — how do you prove that? There was a theorem which, also, was usually attributed to Elsasser. And there was a little proof of Wigner, which was only for very special cases. Ehrenfest had great trouble with it, how to prove it. He did not like to say, "Well, if you interchange the two, then it is an even number of interchanges, and an even number minus one to the even power is plus one, so it must be plus." That he didn't like. It had a certain phony character, you see. He says, "But now suppose I take this one and do it another time," and he says, "It is clearly approximate only, because it must be so that the states of these two compounds are the same. If they are not in the same state it is not true. Then they are different particles." He says, "How does that come about?" We talked about it at the summer school, and then Ehrenfest went to the west coast. There he again was very much together with Oppenheimer. And then they wrote this paper which is an always quoted paper about the theory, because it is 'proof' of the theory.

This is a paper which nobody has read, nobody. And now Martin Klein tries to read it, and it is extremely difficult. I, unfortunately, have no reprint of it. I think one could probably figure it out, but it was completely written by Oppenheimer. The proof is in none of the books, but there is a very interesting passage about it, which I mentioned to Martin, in Kramers' book. Kramer knew it was a bit of difficulty, but all the other Klugscheisser just think not. Very interesting. But this shows that they must have talked very long with each other about it. Then, perhaps, Ehrenfest understood what Oppenheimer wanted to do, and left it with him to write it. It is in absolutely unEhrenfestian style. An Ehrenfest article you cannot miss because it has a grossen, grossen style… Oppenheimer and Ehrenfest got along very well. They liked each other very much. Robert has also great respect for him.

Kuhn:

How well did you know Born?

Uhlenbeck:

Not terribly much. I was never very close to him. In the old days I met him very often. In the late '30's we were both working on condensation theory — the Mayer theory. I had worked on it with Karl, you see. Born did it at the same time (with Fuchs). Several papers. They were not very good in my opinion. Furthermore, one of the nicest proofs was one I had given him. He gives me reference, but I had of course intended to write it in the paper with Karl. I told him that, and he just printed it in his paper. This point was mathematical proof, really, which we succeeded in doing extremely elegantly by means of (Lagrange's) theorem. I told it to Born at the (Von Laus) Congress and he was very interested. He says, "Please write it to me." And I wrote it to him. And then, well, he must have considered that — I mean it was not that he didn't mention me. He, of course, mentioned me all right. Still, it was a little bit tedious, I thought, because our paper appeared afterwards, and in it, the same proof appeared… It was something which I think still now that I would never have done myself without asking beforehand. But he was then also in a difficult period. He was then at Edinburgh… He wanted to do too much. He wanted to do his theory of liquids and of superconductivity and of field theory — he wanted to do all the things. He was ambitious all right… None of them panned out, really. And that bothered him. He was of course also (half-professor) all right. When I was in Gottingen I went often to his lectures. His lectures were always like colloquium talks. It was always recent papers which he simply read and somehow digested a bit, and did all the calculations on the blackboard. And this was very instructive. It was always very mathematical. One had the feeling that (you took part more). But I didn't really know him. Neither did I know Sommerfeld really very well, although I met him several times. We were anti-German, everybody around was anti-German. That was just the (???). We were too close to them. Sommerfeld was in the Ann Arbor summer school, and there he was extremely nice. He was really a remarkable gentleman. So Prussian! He made a Prussian impression. He was very short. But he was always so straight — this military bearing he had.

Kuhn:

But he was very different with students?

Uhlenbeck:

He was very nice with students. La Porte was one of his students too, of course… La Porte always tells that they worked in the evenings together. Around 10 o'clock Herr Professor stopped, and they drank a glass of wine together. They did that so all the time, or they met in a cafe with the students. No, he must have been a very nice person. Pauli and Sommerfeld were once together in Ann Arbor. Firstly, Sommerfeld was the only one to whom Pauli didn't say "du". Secondly, as Sommerfeld came in, Pauli always stood up. It was very striking. He always stood up, because it was Herr Professor. That was still so strong in him that he could not. He had all kinds of reservations about many of the things, but he had this inherent respect from his early days in Munchen.

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Abstract:

This interview was conducted as part of the Archives for the History of Quantum Physics project, which includes tapes and transcripts of oral history interviews conducted with ca. 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: Henri Abram, Niels Henrik David Bohr, Max Born, Louis de Broglie, Max Delbruck, Paul Adrien Maurice Dirac, Tatiana Ehrenfest, Paul Ehrenfest, Walter M. Elsasser, Enrico Fermi, Ralph Fowler, Samuel Abraham Goudsmit, Werner Heisenberg, Oskar Benjamin Klein, Hendrik Anthony Kramers, J. P. Kuenen, Otto Laporte, Hendrik Antoon Lorentz, J. Robert Oppenheimer, Wolfgang Pauli, Isidor Isaac Rabi, Harrison McAllister Randall, Julian Schwinger, Arnold Sommerfeld, Llewellyn Hilleth Thomas; American Physical Society meeting (Boston), Huygens Club, Kapitsa Club, Rijksuniversiteit te Leiden, Technische Hogeschool Delft, University of California at Berkeley, and University of Michigan.

Kuhn:

George, I would be very grateful for anything you can do to illuminate this question of knowledge and conviction about the anomalous gyromagnetic ratio of the spinning charge.

Uhlenbeck:

Ja, well let me tell you. Let's not talk completely about the spin, perhaps yet. But I can tell you that when we thought about it, we had no idea of publishing. It was too strange, and we were so young. This I have already told at several occasions. What we did, of course, was tell it to Ehrenfest. Ehrenfest was immediately interested. He said we should write something up, and that was the Naturwissenschaften letter, which was really completely written by us. Now about this gyromagnetic ratio, we simply postulated it. Because, taking it over from the core, we had of course to make the same assumption for the electron. I still remember that Goudsmit was not with me all the time that fall. I was assistant of Ehrenfest, Goudsmit was assistant of Zeeman. He was three days of the week in Amsterdam, and then he always came to Leiden for the colloquium and, stayed also, I think, a few days in Leiden. There is a certain vagueness in my memory at which time we started really to write it up. That means at which time Ehrenfest told us to do it, because he surely did. I remember that Goudsmit wrote me a letter, in which he made very primitive models. This whole Abraham business was really quite straight-forward. If you have a (point) electron moving in an orbit then you get gyromagnetic ratio of e over 2mc. That was of course standard. Sam wrote me a letter in which he did something that was very primitive, and he says "I don't know why, but for a continuous distribution," — he certainly didn't use these words! — "might it not be different?"

Kuhn:

Continuous charge distribution?

Uhlenbeck:

Continuous charge distribution, and mass distribution too. I thought about it, and I talked with Ehrenfest… He immediately said that had been done. He said to look in the early papers of Abraham. I was, of course, at the same time in charge of the library. So I made a search of all the early papers. There was this paper, and there was the factor two, for the case that the charge distribution was a surface distribution. You see, the charge had to be only on the surface of the sphere. The mass, of course, had to be uniform. Abraham had also made calculations for a uniform distribution; you then get another factor. In fact, by changing the distribution, you can get anything between one and two. Anything. Well, that you got at least two for one case, that was very encouraging at that time. That was why we mentioned it in the Naturwissenschaften note. Now Lorentz knew that right away. We gave this note to Ehrenfest, and he says, 'Now you must also talk with Lorentz about it,' and we did. Lorentz was retired already. He was in charge of the Teyler Institute in Haarlem which was a kind of retirement job… He lived in Haarlem, but he came every Monday to Leiden and he gave a lecture from 11 to 12 Monday morning. It was the Monday morning lecture of Lorentz. There you had to go. Ehrenfest simply drove you to it. People came from all over.

Lorentz there talked always about his recent work or recent literature. It was always very beautiful, beautiful lectures. Anyway, that was why he came to Leiden. One of these Monday mornings then we saw Lorentz and told him about this idea. Lorentz was not discouraging. He was a little bit reticent. He said that it was interesting and that he would think about it. He immediately, of course, knew about Abraham, and he also then thought he should do some calculations about it. And he did. It was so typical of Lorentz, that he immediately made very extensive calculations on the classical theory of rotating electrons. I think already the next week, but maybe two weeks later, he gave me such a stack of papers with long calculations. Large white paper, I still remember. He tried to explain it to us — to me, but it was so learned for us that I — By the way, he published these things. It was one of the last things Lorentz published. It was this contribution to the Como Congress. The only thing which was clear out of his explanation, the only thing I really remember, is that he painted out this famous difficulty, that the magnetic energy would be too large. If you believe in equivalence of mass and energy, you have to take the radius of the electron extremely large, you see, in order to get in. Otherwise you have, beside the electric energy, magnetic energy, and it would be much too heavy. I took the old radius, and then the electron was as heavy as a proton. So it looked nonsense.

This was later pointed out also by Fermi, in a short note to Nature, I think. But this was the difficulty. I remember that we were not even very depressed by it, because we thought it was probably nonsense anyway. Otherwise people would have done it already. And so we told it to Ehrenfest, and that I still very well remember. We told it to Ehrenfest. Lorentz was, of course, the god nearly, also for Ehrenfest, and certainly for everyone in Holland — the absolute authority. "Lorentz has shown us that this is nonsense." And so we said to Ehrenfest, "We'd better not publish that note." And then Ehrenfest said, "I have sent it away already weeks ago and it will appear next week" And then he said to us – I don't know whether Sam remembers, but I remember — "Well, Sie Beide sind jung; Sie konnen eine Dummheit leisten." And he went on, and that was all. Then the next week really it came out. And the funny thing was that three days after that Sam got a letter from Heisenberg. Sam got a letter from Heisenberg! In which he says, "very interesting," and he said, "what do you do about the factor two in the fine structure formula?" That was the Thomas factor, but at that time we had not derived the fine structure formula… In the note we only said that qualitatively everything was then in order, especially that the fine structure was proportional to the fourth power of z, and that, as also mentioned by Lande, the doublets were inverted. Which was right, you see. I have made the note in there too.

I know that this whole passage that Lande mentioned — I remember it quite well. It happened, of course, after this note in Nature, because St. Nicholas Day is December 5 in Holland, and then it had already appeared. So he must have not remembered that it had already appeared. So he must have not remembered that it had already appeared. Anyway, we got this letter of Heisenberg, and he says, "What do you do with the factor two?" Now this was much more precise, of course. But we had only these qualitative considerations… And then we had to derive it ourselves. That was quite a struggle. I think I did that finally. I really learned to do a little bit of perturbation theory, which I didn't know at that time very well. I did it only for circular orbits. And there came this remark, which Van der Waerden makes so much of, but I don't think it was so important really. That was, namely, that if you sit on the electron you don't have a magnetic field because of the relativistic transformation. Or much more simply, because of the Biot and Savart Law of the rotating positive charge. I remember that Einstein mentioned that. Then it is relatively straight forward, of course, although still not obvious, how to do the perturbation theory. You have to compute the average energy then of that thing. And I did that, and then I got, of course, also the fine structure formula with the factor two wrong. And so, I don't know whether Sam answered Heisenberg. I certainly did not do that…

Kuhn:

What was the relation of the piece in Nature and the piece in Naturwissenschaften?

Uhlenbeck:

I will tell you. That came about as follows. Bohr came to Leiden. It must be either November or December of that year. You see, this all happened in the fall.

Kuhn:

This was after the Naturwissenschaften piece was out?

Uhlenbeck:

Ja, Ja, after that. It was the golden jubilee of Lorentz's doctorate in Leiden, a big occasion. Bohr came then and he always stayed at Ehrenfest's house. We talked then with Bohr really very long — whole mornings — and Bohr especially urged us to look back at hydrogen spectrums. Now we had done that before of course, but only on a formal basis. That was mainly Sam's contribution. That was the first paper we wrote together. It appeared in Dutch only… We postulated that the spectrum of hydrogen had to be, as it is now. That appeared already, I am sure, in August or so of that year. That was the first paper we wrote together, and Sam mainly wrote it. The only contribution I made to it was really because I was so dissatisfied. Sam gave me lectures all the time, because I was, of course, in these things a complete beginner. But I was so scathing about treating hydrogen one way and the alkali spectra another way. I said that must be nonsense, you must do it at least in the same way. And then Sam, I think in a very short time —

Kuhn:

This was really the fine structure, the difference in the treatment of the fine structure.

Uhlenbeck:

The fine structure, yes. Then Sam, because he had known so much about this formal spectroscopy, guessed how it was. And then, I remember that I still looked through the (dissertation cards). I had all these things. The only further contribution I made to the paper, was that I saw there was a line in the hydrogen fine structure which Kramers did not explain. He did the Stark effect for hydrogen, but of course without spin. He explained all kinds of things of the Paschen measurements, but he did not explain this outstanding line. He didn't say anything about it. It was just this line which was possible in a new interpretation, because the selection rules changed. You see, it was a forbidden line previously, but now became allowed. And it was clearly there. We mentioned that in the paper as an argument for this new interpretation.

Kuhn:

… What is the gist of that paper? Is it a treatment of hydrogen in terms of a Rumpf rather than in terms of the relativistic —?

Uhlenbeck:

No, no. It was purely formal. It was simply how the energy levels should be, and how it then could be conducted with alkalis. Without interpretation of what quantum numbers meant. Just a j, then there was an l, of course, but what they meant, we didn't say. Of course the connection with the relativistic Sommerfeld formula was not in this first paper. That appeared in the Nature. Of course that was on the insistence of Bohr. I remember that in these discussions with Bohr, we finally came then to this famous picture about how it was that it split and that you just got the old one back, but now with the double degeneracy.

Kuhn:

That all came out during these conversations with Bohr?

Uhlenbeck:

Yes, and that was in Leiden. Then Bohr left Leiden and he went to Germany. He wrote then also to Ehrenfest that he felt as a pop-gun on the spin. He gave lectures about it, talked about it to everybody, and really because of that everybody accepted it. Although there was still the factor two. Except Pauli of course. He didn't believe it.

Kuhn:

What did Bohr say about the factor two? What was the situation?

Uhlenbeck:

Well, he says, that will come in order. You see, he was also completely uninterested in the classical difficulties. Completely uninterested! I remember that I talked with him about what Lorentz said. He said, "this is of course not classical, and so that one has not to think about it in those terms." He was, of course, worried He always was worried about such difficulties, but he had such a feeling that that was the answer to many of these difficulties. I think with regard to the Nature letter, in memory, he essentially wrote it. After these computations. Probably Kramers helped. We did really very little with it except that we had this picture, and we had also told him about Heisenberg's calculation and the factor two and so on. That was all in this letter. But the style of that letter is essentially Bohr. You must ask Sam whether he wrote anything of it. My memory is that I certainly didn't write one word of that letter. Although it was signed by Sam and me with an appendix of Bohr.

Kuhn:

Yes, I know.

Uhlenbeck:

Which was of course very nice. Although the contents, we knew, completely in Leiden. That was in January, I am sure. Then, because of this, Sam went to Copenhagen. This was January or February I think of '26 — must be '26. It was the Lorentz fellowship. At the 50th doctors' jubilee thing, there was a Lorentz fellowship created, and he was one of the first Lorentz fellows. And there he worked, unfortunately not successfully, with Bohr, on the helium spectrum. There was this great difficulty which now we know is exchange energy. Sam tried in all possible ways to make models by which it could be understood. So he came back a little bit discouraged, I think, in the spring of that year, without further success. But at that time, when he was there, Thomas was also in Copenhagen. Thomas had this factor two. That paper of his was so learned at that time. He was also such a remarkable — he gave, then, a speech about it in Leiden. I still remember it very well, because he couldn't write on the blackboard. It was just physically impossible for him to write on the blackboard. Everything came out so large. It was very remarkable. Kramers then also tried to simplify it very much. Anyway, we were able to simplify it sufficiently — Sam and me. Then we wrote our third paper on the spin, which is not very well known. That was a review paper, which Sam was supposed to write but which we wrote together. That appeared again in Dutch, and in it is the simplified derivation. There, so to say, also the whole connection with x-rays and with alkali spectrum, and how everything then tied together was presented. That was the most complete presentation of the spin, as we knew it. That must have appeared around June of '26. Then Sam went home, because then he went to Tubingen. That's where he did, in the fall of '26 and '27 already, hyperfine structure with Back. I think that was the time. While I, together with Ehrenfest, in the fall of '26 started to do essentially the quantum statistics. That became my dissertation. Then Sam and I were together again in Copenhagen. We both, of course, had to write our dissertations.

Kuhn:

Returning to the question of the anomalous gyromagnetic ratio, if one doesn't worry about self-energy, electromagnetic mass, the derivation is almost trivial.

Uhlenbeck:

Absolutely! It was trivial.

Kuhn:

And in that sense one doesn't need the Abraham paper. In fact the Abraham paper is vastly more difficult and is almost about something else.

Uhlenbeck:

I don't think that had much influence at all. It was only that, you see, in our naivete, we thought that it was impossible to get it, because one always thought of point electrons moving. Ehrenfest remembered this paper. He knew the literature very well, of course, and he remembered this paper. We looked it up, and there was the factor two. And it was a sort of encouragement that it was therefore not so classically impossible to get it.

Kuhn:

But even though you then supposed a body with continuously distributed mass with all the charge concentrated on the surface?

Uhlenbeck:

Ja!

Kuhn:

This didn't seem so odd as the —

Uhlenbeck:

Well, this was always the classical picture. We always thought that it was a little sphere. It was only a kind of small encouragement, and that was why we mentioned it. But then Lorentz of course came along, and says, "But if you think of it that classically, then you have all these problems." … So I don't think it had further any effect at all afterwards. I think that both in the Nature paper and in the third paper we didn't even mention Abraham any more. We simply postulated that these were probably intrinsic properties of the electron.

Kuhn:

This also points to another problem that's bothered us. Why didn't anybody get hold of this idea for the. Rumpf? Is it that the Rumpf was never thought of, conceptualized as a single body?

Uhlenbeck:

Right. It was always separate particles moving.

Kuhn:

And the Rumpf angular momentum, the Rumpf quantum number, represented a sum of the angular momentum for a group of discrete particles.

Uhlenbeck:

Always. Right.

Kuhn:

You never thought of it as a core or a sort of equivalent single particle rotating?

Uhlenbeck:

Never. Never. I don't think that was ever an idea. And therefore factor two was always funny.

Kuhn:

What sorts of problems were most on your and Sam's minds, that seemed most unsatisfactory about the Rumpf model, problems in which you saw that the spinning electron might do something for you?

Uhlenbeck:

About that it is so difficult to say. Since we are at it we might as well tell it. I can tell you how it came about, you see. I was of course in Italy for three years, teaching the son of the Butch ambassador. That was a job I had gotten from '22 to '25. I was a very dutiful student always. I took my exams in time. When I had this job, I also did my exams. I worked on it there, and then I came back on a vacation and did the intermediate exams and final exams and so on.

Kuhn:

These were the exams for the doctorate?

Uhlenbeck:

These were the doctoral examination, which was before the thesis. That is, in Holland, separated. You see, there is a doctoral examination and then there is a dissertation.

Kuhn:

How many subjects did you have to cover? How broad were those doctoral exams?

Uhlenbeck:

Oh, quite a lot of mathematics, and theoretical mechanics, which was taught by a mathematician. Then there was theoretical physics. It depended on the major and the minors. But the traditional one was if you were theoretical physics. Then you took surely mechanics and mathematics as minors. You had to do quite a lot of mathematics — function theory and what not.

Kuhn:

No electromagnetic theory?

Uhlenbeck:

That we did with Ehrenfest. The mathematics was function theory, advanced analysis, so to say; differential geometry; and then this mechanics…

Kuhn:

Then the theoretical physics exam included the electromagnetic theory?

Uhlenbeck:

It was Ehrenfest. Ehrenfest was always extremely cavalier about it. I mean if he trusted someone, then you had hardly to do any exams. And he said, well you know it.

Kuhn:

What did he want to make sure you knew?

Uhlenbeck:

He essentially gave only two courses. The one year it was Maxwell's theory, which always ended up with a little bit of special relativity; the other year he gave statistical mechanics, which ended up with a little quantum theory and, atomic structure. And that was essentially all that he taught. It was four hours a week. I had done the exam, and so I was what they call a "doctorandus", which means I was ready for my dissertation. I did this relatively early, but then I liked it so much in Italy that I stayed an extra year. It was against the advice of Ehrenfest, but I did it anyway. The third year I didn't do any physics whatsoever. The second year I knew Fermi, and I had a seminar with Fermi in Rome. But the third year I didn't do anything. No physics whatsoever. So then I came back. That was in June. We always came back in the summer because the student had to do his examination to the next grade. This was, in fact, the last one, because this time he had to do the final exam for the gymnasium and he went through. So I was really finished. It was clear that I wanted to get my dissertation. But I was really at that time in a kind of a shaken position. I was so fascinated by history I thought I should study it. This was even practically impossible, because for that you had to have classical languages, which I didn't have. So I began to study Latin. While I was assistant to Ehrenfest I did that. But then that was all soon over of course — because of the spin, essentially. I came down with Ehrenfest, and he says, "all right." I think he had his doubts at that time about me. I was too elegant, because I had lots of money in Italy. This was because of the devaluation of the lire. You see, I was paid in Dutch guilders. I had, thank God, saved money. But then he spoke of the possibility of assistantship. He says, "Anyway, now, you have to learn a little bit of what is going on." I knew Sam, who was younger than I was. He was two years after me really, but he was at that time already an expert.

Kuhn:

You had known him before you left?

Uhlenbeck:

Oh yes, sure. Only as a younger student, so to say — not very intimately. Now Sam was not a dutiful student. He never could do exams. He had, not even done his doctorandus exam, which I had done already a few years ago. This was because he was so afraid of the professor in mechanics. That's a very interesting sidelight. Finally we had to push him through his doctorandus exam. Ehrenfest was only finally able to do that by letting him drop mathematics or, rather, mechanics as a minor. But he had to have two minors, so he took experimental physics and astro-physics as his two minors. This was of course at that time very strange, because as a result he was not allowed to teach mechanics or mathematics in the Dutch high schools. Of course as soon as he was in Ann Arbor, he was the one who always gave the course in theoretical mechanics… He did it always with great pleasure. I always kid him that he has no right to teach it. According to the Dutch law he is not allowed to teach mechanics. Anyway, he had already published several papers. He knew Heisenberg and Hund, and he knew all the spectroscopy of course very well.

Kuhn:

Well he was already clearly driving toward spectroscopy?

Uhlenbeck:

Oh yes, that was his specialty. Especially this kind of formal business, this looking with the help of numbers through the experimental material, and getting some regularities out. In that he was really a past master.

Kuhn:

And in this you had never been interested?

Uhlenbeck:

Not interested? I didn't know it. I just didn't know it. Then Ehrenfest gave Sam the task of teaching me. That was one of Ehrenfest's pedagogic principles. He always wanted to have people work together in pairs. That was his method. You always have to do it together — that was his principle. So that summer from June on, Sam came. I think we came together twice a week. The other days I went to Leiden to start working with Ehrenfest on something quite different — partial differential equations, the wave equation, properties of the wave equation, which I was also very much interested in. Sam just lectured to me. We were together sitting in a room, and he told about all these successive points, and he did that very beautifully.

Kuhn:

This was all on spectroscopy and atom models?

Uhlenbeck:

Spectroscopy and atom models, and so on. I learned it quickly because of that, you see. I remember that then very soon came about all these questions of the old duality, Nichtmechanische Zwang. Somehow there was something mysterious going or because of the two orientations of the core. There were papers by several people, I think also by Bohr — which really escaped us altogether. But we studied them — especially Pauli's papers. Especially Pauli's papers on the exclusion principle. That was already known to us. There, especially, the four quantum numbers appeared, which for the first time, in a formal way, are attributed to the electron. There was this hocus-pocus of combining them so as to explain the periodic system, the Zeeman effect, and the transition to the Paschen-Back effect. This was all in the Pauli papers already. Sam had studied those, and we always came back to them. Now the date is — I am in conflict with Sam about the date. He has a very good way of dating it, because he says it; was at the same day that there was a tornado in Holland. I don't remember that quite. But anyway, I thought it was earlier. He may have been right that it was August. It was one of these times. Then came this idea that if there were four quantum numbers then it couldn't be solved with the three degrees of freedom. Now that would only be there if it was really rotating, if it was not a mass point, but something which rotated. We always looked upon it as really a rotating thing. It was quite clear that if you then have it rotating, and if you give it as nowadays half quantum number — in those days it was still one — either way it had two orientations only. This was then the explanation of the alkaline doublets. That occurred immediately to us. The anomaly of the gyromagnetic ratio was still difficult, but then came this period — which must have been in September — when we talked with Ehrenfest about it. The academic term had already started. I was already then in function, because Ehrenfest had then begun to get confidence in me, and I became his assistant right away. And then Sam went to Zeeman, so it must have been in that period that Abraham and also this connection with Lorentz developed. That must have been, I would think, the end of September, beginning of October.

Kuhn:

You told me before, in talking about the hydrogen paper, that in your discussions with Sam you kept insisting it was nonsense to treat hydrogen one way, the alkalis another. As you learned the whole of spectroscopy and models with Sam, or from Sam after the return from Italy, were there other things which you or he or both of you felt to be wrong?

Uhlenbeck:

Well, that's so mysterious, you see. The thing with hydrogen was, of course, that you had the Sommerfeld theory. This was a difficult theory, but it was solved, so to say. One didn't think about it anymore. Then there was this other one, where you had the vector model. This seemed to be completely different. Every other word was the vector model, of course, because that was the only thing at hand. And then you had to do such hocus-pocus, you see, changing the quantum numbers once in a while from j^2 to $j^{2-1/4}$. Nobody was satisfied with it, but that was, so to say, the spirit of the times. You had to guess it somehow. I only remember that my great dissatisfaction was that hydrogen was, so to say, left aside. I thought it was terrible that in the simplest case there, somehow, the model did not apply. There you had another theory. That was this paper which I have, surely, here. It must have appeared in August, really. Sam may have been right, that all this appeared a little bit earlier than I think. But that, surely, is the first paper we wrote together. It appeared in Physica.

Kuhn:

One of the problems with the Rumpf that Bohr and Lande raised was that if you specify the quantum numbers and then ionize the atom, you ought to be able then to predict what the total angular momentum of the next element over in the periodic table is.

Uhlenbeck:

That was the Aufbau principle.

Kuhn:

Right. And that doesn't work. Was this a bad problem for you people, also?

Uhlenbeck:

Oh yes, that was one of the great riddles. They had several names for it, we mentioned it also in our letter… It certainly was a difficulty, but not so sharp, because one did not know what to do with it. It was certainly not with us the starting point for our theory. I think the starting point was really Pauli's paper, and the four quantum numbers to one electron. Of course then, already, the Aufbau principle became that of two (electrons). Of course Pauli had also a very important paper — which we knew too — showing that the rumpf couldn't have a regular momentum because of the relativistic effects. Of course these are isolated things, so to say. There was of course discontent with them, but one didn't know what to do with it for sure. We certainly didn't know what to do with it.

Kuhn:

I take it that very often what precedes a basically new idea is a generally diffused sense that something's wrong. I've been trying to give more structure to this sense of something wrong, even though it isn't a particular thing being wrong that leads to a particular idea, but the general sense that things are not going as they should be.

Uhlenbeck:

There was, of course, this general feeling. First of all, it was a central problem just what the proper quantum theory should be. That was general; that everybody had.

Kuhn:

Do you mean quantum theory for the atom, or does this sweep in the whole question of black-body radiation, specific heats?

Uhlenbeck:

For the atom, the structure of the atom. How, really, the correspondence principle should be sharpened, how one could get all these remarkable regularities in the spectra, that was the central problem of '24, '24 for sure. How it had to be done, that of course maybe some of the great people, as Heisenberg, had some vague, ideas, but we certainly didn't know. And in such a case, you see, there were also, in a sense; no sharp contradictions. The situation was always really such that you had to take for granted certain things which somehow seemed strange. It was not such that you could say, as with the Michelson-Morley experiments, there was a sharp contradiction.

Kuhn:

There wasn't with that either.

Uhlenbeck:

Well, with a certain disrespect to Lorentz, I think there was. At least for some of the people there was. The situation is there similar as it is today really, I think. Now one knows also that this is not the final theory, and therefore no one knows what to do about it. At that time one had not the feeling — at least we didn't have the feeling that one was very close. But then I was of course a complete beginner, and in a sense Sam was too. Sam knew the spectroscopy, but he didn't, know all this theory of the multiple periodic systems, and what not and what not. Celestial mechanics, the helium spectrum — all these things were for Sam, and at that time for me too, completely new. We just didn't know those things.

Kuhn:

Were you concerned about some of the other outstanding problems? The dispersion problem was particularly messy.

Uhlenbeck:

No, we didn't know that very good.

Kuhn:

You were aware that it was troubled.

Uhlenbeck:

We knew about Kramers' paper, but did not know it very well, because that was very difficult stuff. For us that was very difficult stuff, you see. And the Raman effect and so. We knew that it was there, and we knew roughly how to describe it, but the mathematical formulas were not known to us at that time — to Sam and me. Of course it was known to other people. This was also interesting what Lande says about De Broglie. That surely was unknown to everybody.

Kuhn:

About what?

Uhlenbeck:

The thesis of De Broglie. Nobody spoke about it, not even Ehrenfest. Nobody! I didn't even know of its existence before Schrodinger. And I am sure that it was not discussed in a colloquium. Ehrenfest's colloquium was, in that respect, perfect. Every new thing which came up. So I think he must be wrong.

Kuhn:

I think very probably; though, clearly, there is a difference. Einstein got excited fairly early about it. You knew Einstein wrote to him about it?

Uhlenbeck:

There must be people who knew about it, all right. Maybe Ehrenfest knew it too, but I don't remember that he ever drew our attention to it in any way.

Kuhn:

What about the problem of the corpuscularity of light?

Uhlenbeck:

I discussed this with Ehrenfest quite often, because he had this paper with Epstein. We had several of these Duane-like explanations. I still have notes about that. We tried then to get intensity formulas, which was not so easy as one thinks. But nothing came out of that… But it was still in that sense separate from atomic structure. Atomic structure was one way. All those questions of photons was another way, another part which was perhaps also dark, but had nothing to do, or not in an immediate sense to do, with the spectral problems. There you have, of course, not only the Land interval rule, but you have also the intensity rules of Ornstein and Burger, which Sam had worked on. He and then Kronig. Fermi wrote about it. All from the correspondence principle guessing from the correspondence argument, … which one was remarkably successful with. You see Sam was there really very good. He had these formulas, guessed by means of certain measurement that you have to have. Still, it was clear that this was all, so to say, clever guessing. That there should be a theory, everybody thought, surely necessary all right. But it was far away; we thought it was far away.

Kuhn:

A little bit like the renormalization problem when people learned how to get rid of (infinities.)

Uhlenbeck:

Ja, ja, I mentioned that in my talk at Leiden too. Because in the atmosphere at the time there was such a remarkable analogy, too. The years of '26, '27 and the years '48, '49. There was the same type of example. Now it will really go. It had also the following effect, that both the quantum theory time and that time simply produced the generation. There was the quantum theory generation, and there was the renormalization generation. A whole generation of theorists really came to the fore around '48. Kroll, Karplus, Case — the whole present theoretical staffs in all American universities come from the '48 period.

Kuhn:

But that one hasn't been nearly as big and fruitful…

Uhlenbeck:

No, because it petered out. It petered out. It turned out not to be such a break-through. That's a terrible word to use about this.

Kuhn:

That's very expressive.

Uhlenbeck:

Ja, but only now you think of missiles. But it was not, of course, like the quantum mechanics was.

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George Uhlenbeck – Session III

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Interviewed by: Thomas S. Kuhn
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Abstract:

This interview was conducted as part of the Archives for the History of Quantum Physics project, which includes tapes and transcripts of oral history interviews conducted with ca. 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: Henri Abram, Niels Henrik David Bohr, Max Born, Louis de Broglie, Max Delbruck, Paul Adrien Maurice Dirac, Tatiana Ehrenfest, Paul Ehrenfest, Walter M. Elsasser, Enrico Fermi, Ralph Fowler, Samuel Abraham Goudsmit, Werner Heisenberg, Oskar Benjamin Klein, Hendrik Anthony Kramers, J. P. Kuenen, Otto Laporte, Hendrik Antoon Lorentz, J. Robert Oppenheimer, Wolfgang Pauli, Isidor Isaac Rabi, Harrison McAllister Randall, Julian Schwinger, Arnold Sommerfeld, Llewellyn Hilleth Thomas; American Physical Society meeting (Boston), Huygens Club, Kapitsa Club, Rijksuniversiteit te Leiden, Technische Hogeschool Delft, University of California at Berkeley, and University of Michigan.

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Kuhn:

One of the things we haven't talked about is education, curriculum, and books. Another one is this whole question, and you're in just the right period for it, of how people felt about the state of physics — the problems and the pressure on the problems.

Uhlenbeck:

Only for Leiden I can tell you… Let me first tell shortly a little bit about education, because I don't think there is very much to say about that. In one respect, I was a little bit different because of this period in Italy. Which of course the usual student did not have. That was just very good luck that I got that. And the other point was that I started to be a chemical engineer only out of a kind of desperation, because I was at that time not allowed to study in Leiden because of the law. It was a law that everyone who wanted to study at the University needed a classical education, must have gone to the gymnasium. I had gone to what Germans call "Reale Schule"… There we had no classical languages. And these people were only allowed to go to the technical universities and not to the University. My parents were also much more inclined that I would become an engineer, because it sounded much more safe as a profession. So I started in Delft as a chemical engineer. Then, however, this first fall the law was changed, and that was the famous law ("limbo"). Everybody was waiting for it. And then at last also the people from the Reale Schule were allowed to study the exact sciences, and I think also medicine — that I am not sure of — at the University. However, they could not yet study law. That was after a semester, roughly, in Delft.

I was rather unhappy there. That was a typical technical university grind education. Lots of lectures, laboratory and what not. I was finally allowed to go to Leiden. And there with regard to the education, I found it quite heavenly. Mainly because there were practically no lectures, you had nothing to do. There were only lectures in mathematics, analytic geometry and calculus and analysis and so, and they were only about four hours a week altogether. Two hours geometry, two hours analysis. Then there was physics. The general lecture, precisely as in Germany. There was the big lecture with demonstration, which I thought was quite dull and I didn't go to that. I didn't have to. I mean there was no attendance, nobody took any kind of responsibility for the student.

Kuhn:

Who gave the big physics lecture?

Uhlenbeck:

Kuenen, who was a very good physicist. As a general lecturer he was a little bit slow. The only other thing he gave — but that was already the next year — was thermodynamics. That I attended, and that I liked much better. In that he also did much better, I think. Then you had to do this practical, set up experiments. But since I was a first-year student, I was allowed to do that only one afternoon a week, which was just fine for me. Just one afternoon, there was no more. So I had an enormous amount of free time. There was this library which Ehrenfest had started. I didn't get in touch with Ehrenfest at all. But I studied for myself Boltzmann's gas theory, I remember, and mechanics as much as I needed. It was quite heaven.

Kuhn:

This was still first year?

Uhlenbeck:

First. And then the second year the question of the exam began to dawn. You see, there were no exams.

Kuhn:

What level of the sciences had you reached before you went to Delft?

Uhlenbeck:

Quite a bit. The physics and so in high school was quite good. In fact my teacher in high school was quite a good physicist. He had a Ph.D. and had also published things. He was a very shy man. I had the greatest admiration for him as a boy, and he partially determined my direction. He seemed so learned, and that attracted me. Anything which seemed learned. He also gave me the first calculus while I was still in high school. That was not taught, but I studied it for myself during the summer before the last year, so I knew calculus when I came to Leiden. But the general level was not so high. One learned in mathematics, elementary mathematics, geometry and algebra, trigonometry. And physics also, I would say, was just about equivalent with the first year physics, or sophomore year's physics in Michigan. That we got in high school.

Kuhn:

But not with as much mathematics as the sophomore year?

Uhlenbeck:

Well, in Michigan there was also no calculus in the general course. Now perhaps, they have introduced it, but at that time there was no calculus. There were problems, but no practical work. The only practical work in high school was in chemistry. Otherwise not. And all the languages, we had three languages, which everybody at the end of high school could read fluently. So then the calculus was, of course, taken up again at the University… Analysis and infinite series and so. But no theoretical physics except this thermodynamics. That was a little bit theoretical. But then after two years, or in the third year, I finally did it — … I was a very dutiful boy. I did my exam in fine in the third year, and the last three months of that period I had to work. The examination consisted only of oral examinations. All separate topics. On analysis you went to the professor, made an appointment, and talked for half an hour while he asked questions. Same in geometry. Same in physics, for this general course and thermodynamics. So that was a little bit of the pressure; when one was under pressure. Because then one has to know everything at the same time… You see the whole method of education consisted of doing two exams. One was called "Candidates" which you did after two or three years. And then again two or three years later you did the doctorandus examination. In between there was never any examinations, never! And also no attendance, in courses or anything. Of course one knew what one had to know, because that was essentially the content of the lectures. Whether you had gone there or not gone there, there, there was always someone who took notes. Notebooks, notebooks.

Kuhn:

How about problems?

Uhlenbeck:

No problems… There were problems in class, but never assigned problems.

Kuhn:

How do you feel about that now?

Uhlenbeck:

I love it. I never did it myself very well, except if I am forced to, because that is so schoolboy like, you see. And that was, of course, what I liked. They treated us completely as grown-ups. You could take the responsibility. You made your own problems…

Kuhn:

Did the teacher suggest that one do this?

Uhlenbeck:

Not at all! They lectured and went away… Ehrenfest always didn't believe in problems. He always said the only problems which are good to do is those you could do yourself. He never wanted to make a problem course in which the problems are assigned, that he never did. He was very much against it, in my opinion, so far as my memory goes. Anyway, I did this exam, and the second year was more of the same. We went on with calculus, and we learned a little bit more of geometry.

Kuhn:

Did you begin to get some special subjects?

Uhlenbeck:

No, thermodynamics was the only special subject. I was, at that time, unsatisfied with the mathematics, mainly because of the lack of rigor. I was so extremely attached to rigor at that time. I am not at all anymore, but at that time I thought that everything should be precise. There was one man (Proster) who gave a special course on the foundations of analysis. And that I thought was marvelous. I still have the notes of it, because it was straight forward business — Bolzano-Weierstrass and the Dedekind cut, and all that sort of thing. But I thought all that was wonderful, because it was so rigorous. Anyway, that was only a special course and no exam was required in it.

Kuhn:

Did the general physics course go on in the second year?

Uhlenbeck:

Yes. It was a two year course. Then there was also thermodynamics. Optics only in the general course, and of course in the practical. That I remember. The second year you had to work at least two or even three afternoons to get ready, because there was an assigned number of these experiments which you had to do before your exam. You had to give reports on them, written reports on the results. These experiments were quite interesting, for me, mainly because there was lots of formulas in them. You had to check and see whether you could put in experimental values and it would come out. All about diffraction, and all kinds of things. Then I wanted, always, to derive these formulas too. I still remember that the reports of my experiments were simply, for three-quarters of the time, small dissertations which I started. Kuenen was then very much impressed that I gave all the derivations. It was not required, but it was my hobby to do that. But otherwise I learned a lot for myself at these times. A remarkable amount, really. Maxwell theory I did by myself, optics, lots of diffraction theory.

Kuhn:

What books did you use?

Uhlenbeck:

I had a little book. I think it was Clemens Schaefer. It was not a big book, it was a thin one. I still remember, it had such a red cover. Of course, for myself, I pored through books. The gas theory of Boltzmann I practica1iy copied! Complete excerpts. And I began with Gibbs, already, at that time.

Kuhn:

This was before you knew Ehrenfest?

Uhlenbeck:

Well, I knew him in the second year. I saw him, we talked, but I did not know him well. After this exam, which was so far as I remember around Christmas in my third year, I started with Ehrenfest. I had already started, that third year, to follow his course, I think.

Kuhn:

What subjects were you examined in?

Uhlenbeck:

I was examined only in analysis, geometry, and in the physics course — physics and thermodynamics by Kuenen. There were three people, three professors. One (???) in geometry, Kluyver in analysis, Kuenen in physics. Ehrenfest was not an examiner for the first exam. He, as I told you, gave always these two lectures, essentially. Once in a while he did, so to say, collect a group of students and then give something extra, like partial differential equations of physics. I remember that in the third year — the second semester, after my exam — he did that, and I worked for that quite hard, too. Then I began to get acquainted with him. But I did not start to work with him yet, really at all, because his method was that he always worked with one man. That was someone else at the time, you see, not me.

Kuhn:

I see. He had really just one student at a time.

Uhlenbeck:

At most two.

Kuhn:

In your first three years how many other students of physics were there?

Uhlenbeck:

When I arrived, which was in '18, just five of us were starting, of which after a while I think one dropped out. One was an experimental physicist; two were mathematicians, of which one, Grosemann, is now professor at Leiden — a very well-known man. I was the only one who wanted to do theoretical physics. Already when I arrived I knew that. Somehow or another, that was the most learned subject, I thought.

Kuhn:

And these five were all in the same class. You were all the same year?

Uhlenbeck:

Yes. Of course, because it was always a collection of various years, I would say that in the mathematics courses there were certainly not more than twenty. In the big physics course there were more, because chemists and so had to follow that, and there were many more chemists. So it was a very relaxed sort of education. I think it was very fine. Mainly possible because the numbers were small.

Kuhn:

Did all of the people, or most of the people, work as hard, do as much that they were not being asked officially to do? Did they just take it for granted that you did this?

Uhlenbeck:

Well, it was not taken for granted, but most of them did. Most of them had certain special interests. Sam, already before the first exam, I'm sure, started to read Sommerfeld's book, Atombau. I am sure that even at that time when he did his exam, he was younger than I was, two years younger, so I then hardly knew him. He was still a beginning student, but I know that in his second or third year he published his first paper. He was so good in seeing equivocal relationships. He had found that the alkali doublets followed this Sommerfeld fine structure law for relativistic doublets. And that shouldn't be so… Ehrenfest was so conservative he thought it shouldn't be published, so he let him publish it in a very obscure Dutch journal. Then of course two or three years later it was rediscovered by Lande. Now we know that it has to be so. But he did the purely empirical. He was very much, I think, under the influence of a high school teacher from (Lohuyzen), who was also a spectroscopist. So he was there very much interested in everything which had to do with empirical regularities.

Kuhn:

We had really just gotten you then through the first exam.

Uhlenbeck:

And then we had that seminar. Then in that semester I got, suddenly, an offer to become a high school teacher. That was my fourth year. I was a high school teacher at the gymnasium in Leiden for about 12 hours a week. Mainly I did it for money, because I kind of hated it. I taught only mathematics, nothing but mathematics. At the same time I followed, of course, Ehrenfest's lectures, and those also of Lorentz. I told you about Lorentz. That was the Monday morning lecture, the famous Monday morning lecture, 11:00 every Monday. He talked on the recent literature which he worked out in his own way. It was told, and it may be true, that what he always did was to look at the literature and see what it was about. Then he closed it and did it his own way. That he presented in lectures. And the lectures were really quite remarkable. At least my memory of them was that they were so clear that at the end of the hour you didn't know any more what it was all about. Nothing went wrong, so to say. In one of these things I was able to really pin it down only by making careful notes. You went already quite early to it. Ehrenfest was absolutely insistent that you went. That was one thing that he looked that everybody was there! But for the first time, since it was of course not a required thing, I just listened to it, made a few notes. I don't remember, even, from the beginning years what a doublet was. I had this tendency that you forgot it again.

Kuhn:

Did those lectures range over a large range?

Uhlenbeck:

Oh yes. It varied from month to month, because it was all recent stuff. Quantum theory, so far as it was. Not all this spectroscopy. That Lorentz never did, though he knew it, I think, very well. Everything with regard to the crystal structures. I remember that the papers of Born and Lande were discussed, and various other things. Relativity theory some. But anyway, I followed of course mainly Ehrenfest's lectures, and I went to the Wednesday colloquium. That was also a famous institution. The Monday Morning lecture of Lorentz, and the Wednesday evening colloquium. This lasted from about 8 to 10.

Kuhn:

Ehrenfest organized a colloquium?

Uhlenbeck:

Yes. That was his colloquium, and he even made notes. He had a book in which he took attendance; he had notes on the lectures. And there was always much discussion, mainly from Ehrenfest. He always asked questions, and he did it really marvelously, because he often simply interrupted the speaker, and especially one of the younger ones. He said "Now you stand aside. Now I will talk." And then he summarized it, you see. I know it has happened with me and certainly with many people, that at that time you understand it all! How precise it was. And many of the things in the colloquium I still remember; certain points I will never forget. Of course in the beginning you didn't understand much. But by a kind of diffusion process you got finally what Ehrenfest also called the "jargon", the jargon of modern physics. That you began to pick up, so to say, and then in later years you asked questions and took part. There was much discussion, always much discussion.

Kuhn:

Were the presentations usually from students?

Uhlenbeck:

Students sometimes, but there were also always guests. There were also experimental results from the low temperature laboratory which were presented. It was not just theory, but also not just experiment. There were no experimental details. That was never discussed. But always, so to say, the point. "What's the point?" "What's the point?" And always recent literature too. He himself talked. Not very often, but he talked once in a while. Those were also always hard points. I remember the lecture on the Nernst theory. For the first time I understood it. That was every Wednesday, religiously. It was never omitted. Wednesday by Wednesday. They drank tea beforehand, and then there was the colloquium. You sat around in a room. In the middle of the room there was always the recent issues of all the leading journals, new books which had appeared were there — all just to let the people see what the recent stuff was. That was the task of the assistant I did that later, I was not yet assistant, of course. That was a marvelous colloquium. That was also the year that I was a high school teacher, and for the first time I lived in Leiden. Then I had the money to have a room. Before that I commuted to the Hague, and lived with my parents. I still remember that then also in that fourth year Ehrenfest suddenly came in and said, "Well, he was inquiring about someone who wants to go to Rome, to be a teacher." The remarkable thing was that nobody reacted except me. I thought it was wonderful, and so I put my hands up. I got this job which was at my fifth year, and then I was there for three years, until '25. That fifth year was the year that I should do my second exam. Of course I followed Lorentz and Ehrenfest, then also further mathematics of the same people. That was differential geometry; theoretical mechanics, given also by the mathematicians; and so-called higher analysis, by De Kluyver. Again, in each of these topics you had to do an oral exam. I did them in my fifth year, partially by coming back from Rome — once at an Easter vacation — and then also early in the summer. We came back always in June, and then I could still do some of these exams.

Kuhn:

But how did you do the lectures?

Uhlenbeck:

I didn't do the lectures. Well, I had the lectures so far as … my fourth year. I got notes from people, and I studied then for myself out of some books. I don't even know which, precisely… I studied a bit in Whittaker, I remember — Whittaker and Watson. I studied this book on differential geometry, of Eisenhart. That was for the other exam. For the theoretical mechanics, I studied Appell. But that was really when I was in Rome. Of course at that time books were mentioned a lot, but not a single course followed a book at all. So it was only meant as collateral readings. Neither did Ehrenfest, of course, follow a book, but he was always very insistent that you looked at books. He says he thought it was terrible, since the invention of the printing press, that he would say something before the blackboard which was in a book. He says, "Everybody can read!" "So why should I parrot what is in a book." "All that I can teach you," he said, "is everything in between the books. In between the books. That's what I must tell you." But he certainly hoped and required that you looked at books, in various portions. So I looked at very many books, really. But, then, I was a bookish fellow. Very bookish in contrast to Sara. So I remember still that I studied even Kirchoff's lectures, which Reiche mentioned. They're very difficult, but I studied them anyway, especially optics. At the end of the fifth year I more or less squeaked through the second exam…

Kuhn:

I want to interrupt before you get into the frame of mind of the fifth year. In your fourth year, before you went to Italy, when you were going to the colloquia and to the Lorentz lectures, what seemed the big problems?

Uhlenbeck:

It's difficult to say, because I was still just a school boy. The only thing which was clear is the quantum theory, because that was always at the end of the Ehrenfest lecture. He talked about some of the quantum theory things, that that was the new thing and that was what people were interested in. That was, of course, also discussed at the colloquium very often. But I don't think that in my fourth year I had already impressions of what the central problem was. Everything seemed interesting to me at that time. Relativity theory was also touched upon by Ehrenfest — always at the end of the Maxwell theory — but only just to the Lorentz transformations and all these paradoxes. He had all kinds of models that tried to explain. Ehrenfest had of course the tendency to do everything as "anschaulich" as possible — his models. On the other hand, therefore, he didn't hope that you even knew everything. There were only a few things that you had to know. That was Hamilton theory, Maxwell equations, some portions of statistical mechanics — these you learned very well. There was not the feeling — at least I didn't have the feeling — that there was, so to say, a frontier, except that it was quantum theory and atomic structure and so on. One knew that everything was very mysterious. As a student you know that was mysterious, of course. I myself didn't work at all on anything at those times, except learning the books. And already from my early days my main interest was still kinetic theory and statistical mechanics. Especially Ehrenfest's encyclopedia article made a big impression on me. It was so clear, and I was just ripe for it, which was very good… Sam was much more specialized, and I'm sure that he knew at that time already much more. Not at that time, but at the same period of his development — he was two years after me. He knew already much more about atomic structure, because he had studied Sommerfeld's book.

Kuhn:

You got a good deal of relativity also from Lorentz?

Uhlenbeck:

Well, Lorentz, once in a while, discussed problems in relativity, and even general relativity. I then did not understand really, although I thought I understood when he explained it. In the Lorentz lecture, of course, no holds were barred. Everything was discussed, everything! And he computed everything.

Kuhn:

Did Lorentz himself have any feelings about his own version and earlier work toward relativity, as against the Einstein formulation?

Uhlenbeck:

I think he had, but I certainly don't know that of my personal experience. The contact with Lorentz at that time was non-existent. You went to the lecture, but he was, so to say, the god. He gave his lecture and then he went home again to Haarlem. Of course he talked with Ehrenfest, no doubt. He did not come to the colloquium, of course.

Kuhn:

What about the aether?

Uhlenbeck:

He never talked about that.

Kuhn:

What about the rest of the students, just getting it through Ehrenfest? Was the aether physical for you?

Uhlenbeck:

I can't say. Ehrenfest's inaugural address was about this. I still remember having read it. He talked about it a little bit, but we certainly did not have strong convictions one way or another. But relativity was for me, and I think for most of the students, on the horizon of our attention. The thing which we had to work on, if there was anything, was statistical mechanics and quantum theory…

Kuhn:

So special relativity was not yet classical?

Uhlenbeck:

Oh no, no. And it also was not so known, you see, that one felt that it was a kind of an instrument. Nowadays, of course, all people know it so well, but at that time we knew it just a little bit.

Kuhn:

Well, I think there are some schools in Germany, where it was much more closely built into their curriculum.

Uhlenbeck:

I suppose so. Ehrenfest was the opposite of a systematic man. He was not systematic. He always wanted to have the points "at was the point? As soon as it became so to say technical machinery, then he left it. He just left it. He told me later. This is now neither here nor there, but he told me later that both with regard to the quantization of the rotator and also with regard to the Langevin formula for magnetism, that he did it all first, years before! Some of it was published, I think. But he did it only on the plane, because there it was easy! And he never did it even for three dimensions, you see, because then he saw the point. He saw immediately the point that it was discrete, and that you then have also an effect on the specific heat. Then later on it was of course done much better, by Reiche especially, and so he is never even mentioned… And that was typical of him. As soon as it became a longish calculation, then he didn't do it. He just didn't do it.

Kuhn:

Now I'll let you go to Italy, or come back from Italy — whatever you like.

Uhlenbeck:

Let me talk one bit about Italy. That was my fifth year. I then did the exam, and I did not do, very much else. I learned Italian.

Kuhn:

Oh, you did the exams the first year you were in Italy? I see.

Uhlenbeck:

The second exam, the doctorandus. Ehrenfest always said he had a kind of faith in me which was based on nothing, he said. I didn't do even an exam with him, although he was my principal man. He was then, of course, present. The only thing he let me do, which was also required. The exam at that time was then divided into two parts. One of them was just very short, an hour at the most, in which one was again formally a little bit quizzed… A little bit on analysis, and a little bit (???). There I did pretty well, so he was all in favor of — In analysis I didn't do so good. Ehrenfest asked a little bit about Maxwell equations — oh, very little. That you have to get two what they call ("scriptions"). It was not quite a problem. You had to write part of it as a problem and part of it as an essay. I got one in mathematics and one in physics… You were simply given a problem and told you had three days to write it. And you could have access to books, and even friends of course. It was very important that you have! Everybody helped, you see, everybody helped!... And then you wrote it out — the physics. Mathematics — it was a tough problem that even with my friends we only solved half. And the mathematician Kluyver, who had his doubts about me, let it go at that. But he was not quite content, really. Ehrenfest gave me for a scription something on which I had talked in the colloquium, so I wrote it out a little bit further. I still remember what it was. It was on the dynamical theory of x-ray reflection which I had studied a little bit at that time. And that was perfectly okay.

Kuhn:

On the dynamical theory of x-ray reflection — you mean what?

Uhlenbeck:

Ewald's stuff, you see. The question that the reflection could not simply be done geometrically by the Bragg reflection, but that you must think of partial waves. That had certain consequences which Ehrenfest was at that time interested in. And I wrote about it. Then the second half of the exam was then a short discussion of these scriptions, and then it was through! I was through and I got my diploma. Without doing an exam with Ehrenfest, no oral exam!

Kuhn:

What was the point of the scriptions? If you could bring in all your friends and so on?

Uhlenbeck:

It was tradition. Had been done for fifty years like that. Everybody did it, so this was the way it was done. Two scriptions, you had always the scriptions. One in your major subject and one in one of the minors. My other minor was theoretical mechanics. Mathematics and theoretical mechanics. Then on your diploma, which was the thing which was required by law for anyone who wanted to teach in high schools, it was then indicated that you had taken these minors. And so I was allowed to teach in all the high schools and gymnasiums. I was allowed to teach physics, mathematics, and theoretical mechanics, which were always separate subjects, even in high schools.

Kuhn:

Did theoretical mechanics by that time definitely include Hamilton-Jacoby theory?

Uhlenbeck:

Yes. Not very much of it. We had contact transformations and so on. That was done by a mathematician.

Kuhn:

What did you learn those out of?

Uhlenbeck:

Really out of his lectures. Since I had to study most of it for myself, I studied most of it out of Appell, which was the book which also Van der Woude recommended, and I still have the book… I studied those volumes — much of it, not all of it. But then it was interesting. I did this exam early in September, as soon as school was over. I was really expected to be back in Rome already, but the ambassador gave me then a little bit longer so that I could do this exam.

Kuhn:

Tell me now what the job in Rome was.

Uhlenbeck:

This was the ambassador, von (R____) was his name. He wanted his two sons to have a Dutch education, but he wanted them also with him. So he hit upon the scheme to hire two people. He had plenty of money because of his American wife. He hired two people, one for the classical languages, Latin and Greek. This was always half the time of the curriculum… And the rest of the time was everything else. I was hired for everything else. And so I taught this boy mathematics, and some physics. Of course the older boy was already quite through. He is now an ambassador in Washington. But this younger brother was the one whom I had mainly. I taught him everything else, except modern languages, because for that he had again his own tutors. Furthermore he knew the modern languages much better than I did, because the boy spoke fluently Italian, French, English. The only language he was not so good in was Dutch! Because he was never in Holland. He always made mistakes in Dutch, which I had then to correct. And that was a wonderful job. I did not live with him. The last two years — two and a half years in fact — there was only one boy, and he can take only so much. And we had to divide it in two. So it amounted really to about every morning or every afternoon, and then the other part, the other fellow came.

So I had, again, an enormous amount of free time in Rome. Then after this exam, Ehrenfest said that I should look up Fermi. He gave me a letter and also a series of questions. That was typically Ehrenfest. This was in connection with the paper of Fermi on the proof of the ergodic theorem… Ehrenfest was so impressed by that paper, he wrote a paper himself, which is also in the Collected Works, and sent a copy of that with some questions. I was supposed to look up Fermi, who was then what they call "littera docente" in Italy. And of course in this whole physics building, I still don't know precisely where it is. Anyway, I looked him up, of course, and Fermi was clearly very alone. He had two people who were at least about his own age, and with whom he could talk. Those two were Pontremoli and the other was the man who is now professor at Torino, Persico. I talked a lot with Fermi. I remember that he walked home with me and talked, and I have, even, the memory that he talked with me about the Fermi statistics. And so that is where I really learned to know Fermi. I told him about Leiden very much, and that, I am sure, made him decide to use the half a year of his fellowship that he still had. That was, of course, an international Rockefeller fellowship, of which he spent one half year in Gottingen and had quit.

Kuhn:

What did he say about why he quit Gottingen?

Uhlenbeck:

He never said. I know only indirectly that he was not happy there. And then we said we should have a little seminar, because then I talked about the Ehrenfest colloquium and how fine it was. But I was of course, compared to him, a complete beginner. He was younger than I was, but he was in a sense a wonder child, like Pauli. At Pisa he wrote about general relativity and what not. He knew everything. And as I said, I think this must have been before he began to tell me something about these electrons in a harmonic oscillator field, and then, of the exclusion principle. I didn't quite understand. I may have been mistaken, maybe it was later. But it must be about that time though. Anyway, we had then a little seminar, with Pontremoli, Persico, and me, in this old building. Of course Fermi always talked. It was really a little lecture that he gave us. And what we talked on was the classical perturbation theory, three-body problem, analytical mechanics, all applied to the quantum theory. You see, all these things which one now finds in the first volume of Born. Fermi knew everything, and he talked about these things… This was the topic of our seminar. He was so much ahead of all three of us, that he was the one who talked all the time. That was also the only year, really, I think the only year, that I followed lectures in Rome. I followed the lectures of Volterra and of Levi-Civita. But that was all in mathematics… This was the second year. And then I came back in June again, and of course I went to Leiden to see Ehrenfest. Ehrenfest said, "Now you've got to come back, now this should be over." But then I didn't want to really, I liked it so much. The boy had still one year to go, and this ambassador wanted very much not to change. I just stayed over, and that was the third year. And then the third year I didn't do any physics at all… Fermi was gone now, because Fermi got then this job in Florence. It may be that he went to Leiden first, but the last year in Rome I never saw Fermi. I think he was a semester in Leiden, and then went to Florence as a professor of something. He didn't get any money in Rome so far as I know.

Kuhn:

You said the other day that there was no physics in Italy.

Uhlenbeck:

There was Corbino. He was the professor, and he was only an experimentalist. There was certainly, except for Fermi, no one who knew anything about physics.

Kuhn:

Do you have any notion what the curriculum was like?

Uhlenbeck:

No, not at all, because I never met any students. I don't think there were any students! To tell you the truth, in physics. There were certainly some in mathematics. The second year there was also Stark — or was that the third year? Stark — from M.I.T. We came then to Rome to study with Levi-Civita. Of course all these people then I got to know. I mean there were always so few, and if there were Dutch people I knew then through the Dutch Institute… There were always other Dutch people, but mainly art historians and what not. I knew them all; I still remember some of them.

Kuhn:

Then it was then you got so interested in history?

Uhlenbeck:

Then I had so completely lost touch, you see. And I read all these things. I was shaken. That third year shook me, so to say. I didn't know what I should do. I had, thank God, my uncle who was professor of linguistics at Leiden, a very fine old gentleman. He was in sympathy with me. I knew him so well. He was one of the men for whom I had such an admiration. And he said, "Yes, well maybe that is so, maybe you should do —" But then there was this difficulty about the classical languages, I had to learn. And he said, "But why don't you anyway first go with Ehrenfest and see whether you can write a dissertation. You'll have, at least, your Ph.D." The future was always high school teaching, that frightened me too. There was no future except high school teaching. And I had done that a year and I didn't like it at all.

Kuhn:

Would that have been less true — in history?

Uhlenbeck:

No, also true. But in history you had other possibilities. You could get into institutes like the Rome Institute, which I liked very much. A man there made me write this first paper. He was a Dutch man who was one of the founders of (???). So I did it. I went to Ehrenfest, and Ehrenfest said, "All right, let us try." And we started to work on these problems in wave equation — one of the first papers. And he told Sam to get me acquainted with what's going on.

Kuhn:

Is there more that you could say about that time with Fermi? About Fermi's state of mind, his interests, things he felt or expressed about the state of physics?

Uhlenbeck:

I don't think that he felt strongly — at least he never told — that there was such great difficulties that something had to change. He never said something like that. He may have held that though.

Kuhn:

Are there other things that you remember about him? What sort of a person was he?

Uhlenbeck:

Extremely mild. As I said, the impression was that he was lonely. I was a young student, clearly not his intellectual equal, but he really went out with me and talked with me. Of course, there was nobody he could talk to, you see. Here was somebody at least who listened and tried to understand — that was my Ehrenfest education. So I told him about Leiden. My impression was that he was kind of intellectually lonely at that time, which was '2l. You know, I mentioned that in this lecture. It made a big impression on me that Fermi apparently said to people that he found himself in Leiden, and that there he got a confidence to work so completely for himself. And that was due to Ehrenfest. And that was the thing which he did not get in Gottingen… Of course, I knew Fermi much better in his American period, because he came to Ann Arbor a couple of summers, and I even worked together with him. I wrote a paper together with him, but that's another story, so to say. In my Rome period I had not much intellectual contact because I didn't know enough at that time to really appreciate. I have this vague memory that he told me about the Fermi statistics. I think that is a topic for a paper, of early '25… I have the memory that he told me. But otherwise, golly, we talked about all kinds of things and also about the Italian situation, in which he was also not happy. There was so little future for him at that time, you see. On the other hand, of course, one must remember that this was the revolutionary period in Italy. I mean I saw the march through Rome. I was there. And I listened to Mussolini's first speeches in Rome. And I saw all these black shirts in the streets of Rome… It was very exciting, those revolutionary times. Well, so that's my education. Afterwards, of course, after this summer and the spin, everything about history was forgotten. Then it was so clear that I should go on.

Kuhn:

Well you had gotten a good deal of material that was relevant to what you did with Sam, hadn't you, from the Italy seminar with Fermi?

Uhlenbeck:

Not much, no, no. Because you see with Sam we mainly talked on these empirical regularities in spectra. All the coupling schemes. Russell-Saunders coupling and how the vectors — always the vector models we talked about, and the various experiments related with it and so. That was the main thing that started us off. And also, you see, the Pauli papers were all of this formal and numerological way. Some physics, some numerology. You had, so to say, to smell your way through it. It was very strange business for me. I never really quite learned it. Sam was of course very good at that.

Kuhn:

Did Ehrenfest do any of that?

Uhlenbeck:

No, it was also not an Ehrenfestis, but he always wanted to hear about it.

Kuhn:

Did Sam do it for Ehrenfest?

Uhlenbeck:

No, no. Sam did it all originally on his own… He got his degree with Ehrenfest, yes, but he was never a student of Ehrenfest in the sense that he went through this period that they worked every day together. You see that was the Ehrenfest method. I told you of that already. And that of course I did. I did that for two years — worked practically every afternoon with bin. That of course made an enormous difference for me. Sam talked a lot with Ehrenfest when he was in Leiden, but he was never his assistant. Then he also went to Tubingen, although his dissertation was of course with Ehrenfest. We got the degree the same day. Ehrenfest wanted that. He said, you must do it on the same day, because at the dissertation the professor is always supposed to give a speech about the students. And he says he did not want to give two speeches. He wanted to modulate it a little bit, first the one and then the other. This was also not quite kosher for the Dutch tradition. They didn't like it. Ehrenfest, of course, was so un-Dutch in many respects. And I remember that our dissertations — the dissertation is always defense of thesis… You have always these assertations, at the end. That's typical Dutch method. And that gives really maybe something to talk about. And that lasts only about half an hour or forty minutes… Then, the faculty goes out and afterwards they come in again. It is given to you with the speech. So I think Sam was the first. No, I was the first. And then Ehrenfest said, "You go out, you go out, and we take Sam." And then at the end, I still see us sitting next to each other in front of this whole row of professors. And then Ehrenfest made the speech to both of us. We had at that time already the job to go to

Ann Arbor. That was already settled, that was settled in the spring… Ehrenfest was responsible that we got it.

That was marvelous, because as a result we didn't have to do high school teaching. You see we had immediately a place where it was at least a university. The way that came about — I still remember. Colby, from Ann Arbor, was then in Europe, looking around really for people for Ann Arbor to succeed Oscar Klein, who was also two years in Ann Arbor, I think. There he discovered the wave equation. Colby came to Ehrenfest, and we were also present. Ehrenfest gave him an impassioned speech, in which he said that this was a very bad idea, to try to get one man to Ann Arbor. Because that was — nobody there — that was just wilderness — you must at least have two, he says, otherwise they have nobody to talk to. Even better, more than two. And he was very serious. He could talk so seriously about how science develops. He made an enormous impression on Colby. We walked home with him, and he says "Yes, he's a great man. He's really a great man." As a result, two or three weeks later, we got both an appointment to Michigan as an instructor. And we both accepted. I accepted immediately. Sam had still a little bit of hesitation, but he did it too. And we went to America on the same boat in August. And then began the Michigan period for me, which was '27. But of course then the quantum mechanics was already there… You see the first Heisenberg paper, we knew about, but it was completely — at least to me — completely strange.

Kuhn:

Did you know it was to be taken seriously?

Uhlenbeck:

Everything which Heisenberg did had to be taken seriously, because Heisenberg, Pauli, and, of course, Bohr were the gods. They were the people who knew everything.

Kuhn:

But this was already clear about Heisenberg?

Uhlenbeck:

Oh yes. Sure, sure. Absolutely. Of course in a minor sense also Lande, but still not like Pauli, or Heisenberg, or Bohr. So we knew that this paper was probably very profound. And I remember having looked at it, but I couldn't make head or tails of it really. And my impression, and I think also Sam's, was that this was just some more of the same thing, funny rules about the unmechanischen Zwang, and this and that. We did not understand really, but since it was always written by Bohr or Heisenberg, we knew somehow that it should be taken seriously. But it never, so to say, sank in, this sort of thing. I don't know the precise order, but I think the Born-Jordan paper came before

Schrodinger, but not much before. I remember studying that a bit, and also with Ehrenfest. Ehrenfest then began to see a little bit about matrixes and so on.

Kuhn:

Had he known about matrixes?

Uhlenbeck:

Yes.

Kuhn:

Had you had them?

Uhlenbeck:

Yes. I'm sure that we learned that very quickly then. What you had to know was so little. We studied it, but it was so clear that you couldn't make any problems with it, even for yourself. Everything became these infinite numbers of equations that you had then to solve, and so nobody knew exactly how to do it. One had the impression that there was a real advance, all right. I think that was clear. But it was still so strange that one couldn't do very much with it. And that changed with Schrodinger. Because that was '26, yes, it was in March '26. And then with Ehrenfest we studied every paper, and made little problems. We did the perturbation theory even before the next paper appeared where the perturbation theory was done. We made little problems, and I told you about the hydrogen atom and the intensities. There for the first time I learned to handle a little bit all these special functions. It was so good that I was so dutiful. I had studied those things out of Whittaker-Watson. And so I became the expert on special functions. I just knew what a spherical harmonic was and so on. And that was wonderful, that was very wonderful. And that lasted, I think, about six months. It was the whole second semester, or the first semester of '26. Then — that was very important — was when Klein came. He was also a Lorentz Fellow. Through the Lorentz funds, of which I have already told you about, Oscar Klein was brought to Leiden. And I stayed with him. We stayed in the same rooming house… He had come back from America. He was, I think, in Copenhagen, and then Ehrenfest invited him to stay.

I have the impression that it must have been at least a month, maybe even longer, but it was certainly a month. We had, all the time, these discussions with Klein. Every afternoon. And Klein of course had in Ann Arbor found a wave equation too. He knew De Broglie. And he had written down what now is called the Klein-Gordon equation. He did it of course immediately relativistically. I think he had also the non-relativistic form. But he had not solved it. He did not have the hydrogen atom. And furthermore he was in Ann Arbor, so that was really the wilderness. I think that was one of the reasons why he wanted to come back to Europe. Then he had also these ideas about five-dimensional relativity, and we discussed about that. It was very interesting… The fifth dimension had something to do with the mass I think, at that point. I wrote a paper with Ehrenfest on this five-dimensional stuff, typical Ehrenfestian paper. He was then also working on the Compton effect, and on the radiation theory — what now is called the korrespondenzmassige interpretation of the radiation phenomena. He wrote that at that time, also, two of his well-known papers about it. Very soon he had of course the formula for the intensity of the Compton effect.

Well later on he did what is called the Klein-Nishina formula. But that was not done in Leiden, that was later. But he did it for the thing without spin too, I'm sure of that. The connection with dispersion theory became then clear to me — how they hang together. That was a very, very interesting time. I had two good friends in Leiden, with whom I always had dinner together. Both of them experimentalists, (Neyroff and Meersma). And I still remember one time after these discussions with Klein in which he had told about his five-dimensional relativity and how out of that quantum conditions could come. You see, from the periodicity condition in the fifth dimension you got the quantum conditions. And I was so excited. I told them, "Very soon we had the world formalized. We will know everything! Everything will be known at that time." Well, it was a beautiful exaggeration. That was a little bit the feeling — that somehow maybe we know everything now.

Kuhn:

Did you all feel that way? Did Ehrenfest and Sam?

Uhlenbeck:

Well, Sam did not take part much in that. I think he must have been at Tubingen then. Ehrenfest of course took part. But Ehrenfest was always much more skeptical than I was. He was a complete skeptical man about these things. He was very excited about it, and interested.

Kuhn:

When you say skeptical do you mean he was very skeptical about the world formula or that he was skeptical about wave mechanics?

Uhlenbeck:

No, no, he was not skeptical about wave mechanics. He knew that this was really an enormous advance. But the feeling that one knew everything, that of course I am sure he didn't have. I mean that was more the younger generation which got this feeling.

Kuhn:

Had you felt before that things had to break, loose?

Uhlenbeck:

Not really, because again I was just not up to it, you see. People who feel that there is something going on must have been people who really completely knew how the situation was. I would think those would be people like Heisenberg and Pauli and Bohr, especially, and perhaps Dirac. I didn't know that Dirac had this feeling. But people like me — we knew that it was very confused. In discussion with Sam, I was very scathing about all this unmechanischen Zwang, which I didn't understand what it all was, you see. But I wouldn't have dared to say that! That was what fools said. If you didn't understand what you were told, clearly there must be something to it. But that — in the summer during the spin period — that it was very confused was very clear. And that, therefore, something in the future had to be verified, that was of course also clear. But that this would imply a revolutionary change in the foundations — that for me was far too specific. I think only a few people had that, and these were the people who finally did it. All the others thought it's all very difficult, and maybe you just go on with what there is. The usual people only think the next step. But I don't think it was general.

Kuhn:

The Compton effect must have been your fourth year.

Uhlenbeck:

The Compton effect we knew, that we knew. That was also discussed by Ehrenfest.

Kuhn:

Did that come as a big surprise?

Uhlenbeck:

Oh, it was very interesting, but one knew that sometimes one has to work with photons. That we learned in a colloquia — these things like photoelectric effect and so on. That there were photons, that one knew.

Kuhn:

There was no question whether the photon aspect was fundamental? You knew the Einstein paper on the energy fluctuation?

Uhlenbeck:

I don't know whether I knew it, but surely Ehrenfest knew this sort of thing backwards and forwards. No, no, no! I remember even that he talked of one aspect of this fluctuation paper in his lectures. You see, when you take the Wien formula, then it becomes just independent fluctuation of particles. And that I remember Ehrenfest did in his lectures… The other term was too complicated to do. And that was, so to say, the classical interference theory, which Ehrenfest knew too, of course very well, but I don't remember that we ever had that. The notion that there were photons, that there was what we now call duality, that was hammered into us, as being the situation at present. Maybe not satisfactory, but these facts were there. So the Compton effect was also discussed, and I certainly studied it. Of course there was this basic question of the conservation laws of momentum and energy that were there applied… It was, of course, quite new really, but it didn't strike us as extremely new. So we took it more or less in our stride, I would say. The Compton effect we took in our stride in Leiden. One aspect of that period which I can probably reconstruct if I think longer about it, is the effect of the visitors. There were always so many visitors who lectured. And that was also due to Ehrenfest. He was in a certain sense a central figure. People came, or he invited them. Bohr — I told you already about Bohr, in '25, at the Lorentz festival — and Oppenheimer, and we had a little conference with Dirac. That was later, of course. Pauli came, I think probably also Heisenberg. Then the long visit of Klein.

Kuhn:

Did students come from all over Europe also?

Uhlenbeck:

No. There were always foreign students, but they were mainly people who worked in the low temperature laboratory. There was (???), for instance. I remember that I talked with (???) in Leiden. He was then working at the low temperature laboratory. And there were sometimes foreign students. Yes, while I was there there was always one or two, but nobody whom I really remember, except Oppenheimer. He was there for a period, but never for very long. I knew younger students, I remember.

Kuhn:

Did you know Kramers in that period at all?

Uhlenbeck:

Yes, I knew Kramers much better later on. But he came, and gave a speech — in which connection I don't know. This must be '26 or '27 — I was still assistant to Ehrenfest. In it he talked about the WKB method. He made the impression, so to say, that he knew everything which there was, so to say. This was the first impression that he always made; he was a little bit pedantic. But it was only a first impression. I mean, just the way he talked, you see. He always talked so (???). But I can't say that I learned to know him very well in this Leiden period, because he was of course at Copenhagen. He was then in Copenhagen, except towards the end. He became professor in Utrecht, and I think I was still in Holland, when that happened, although I am not certain. It may have been when I was already in Ann Arbor. But then we got him to this summer school in Ann Arbor. He gave one of the early summer schools. And then I learned a lot from him. Later of course we were colleagues for four years in Holland. I succeeded him in Utrecht. You see, after the death of Ehrenfest, Kramers went to Leiden. And then after a long — it was about a year of interregnum in which nobody was there — finally it was offered to me. And after a long discussion with Kramers whether I should do it or not, I finally did it, of course. I was four years there. I saw him very often during those four years. We also had a colloquium; we called it the "K" Club, Planck club. All the professors, theoretical physics and all, came together at each others' houses.

Kuhn:

How many of you were there then?

Uhlenbeck:

About five or six, because also the assistants came. Each one had an assistant. So the people who were there were always Kramers, Kronig, Fokker, myself, plus assistants — Kahn, Belinfante, and some other people. — Pauli was also present several times at this "K" Club. That was every month. And then we went to the different towns. I was in Utrecht, Kronig was in Groningen, Fokker was of course in Leiden, Kramers was in Leiden. And that was very fine. But that was '35 - '39, which was much later. Then I knew Kramers very well; I know his family also very well.

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George Uhlenbeck – Session IV

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Interviewed by: Thomas S. Kuhn
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Abstract:

This interview was conducted as part of the Archives for the History of Quantum Physics project, which includes tapes and transcripts of oral history interviews conducted with ca. 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: Henri Abram, Niels Henrik David Bohr, Max Born, Louis de Broglie, Max Delbruck, Paul Adrien Maurice Dirac, Tatiana Ehrenfest, Paul Ehrenfest, Walter M. Elsasser, Enrico Fermi, Ralph Fowler, Samuel Abraham Goudsmit, Werner Heisenberg, Oskar Benjamin Klein, Hendrik Anthony Kramers, J. P. Kuenen, Otto Laporte, Hendrik Antoon Lorentz, J. Robert Oppenheimer, Wolfgang Pauli, Isidor Isaac Rabi, Harrison McAllister Randall, Julian Schwinger, Arnold Sommerfeld, Llewellyn Hilleth Thomas; American Physical Society meeting (Boston), Huygens Club, Kapitsa Club, Rijksuniversiteit te Leiden, Technische Hogeschool Delft, University of California at Berkeley, and University of Michigan.

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Kuhn:

There's clearly a whole shift of perspective on what it means to have statistics involved there. Didn't it occur while you were at Leiden with Ehrenfest?

Uhlenbeck:

I mean my dissertation was on it, you see. That was all done in the fall of '26. You see, the spring of '26 was the spin, really, and the quantum mechanics, especially the Schrodinger equation. Ehrenfest and I worked extremely hard on the Schrodinger equation and the selection rule for the principal quantum number which I thought I had found but which was wrong… Sam was then for two months, as I told you, in Copenhagen. I think with the Lorentz fellowship. He tried to work on helium, without much success at that time.

Kuhn:

This was helium using the Schrodinger —?

Uhlenbeck:

Using the spin and always the model, you see. Because then it was clearly that the difference between the singlet and triplet level must be due to a difference of the spin orientation. And then the question is, "why is it so large? Because the magnetic interaction was of course very small. And then he and Bohr worked very hard to think of some kind of a model where you could magnify that, without much success. And quite about the same time, or maybe a bit later, Heisenberg — it must have been in April, May, something like that — gave the correct explanation, as an exchange attraction. So that year or that spring it was mainly Schrodinger.

Kuhn:

what about the Bose-Einstein paper?

Uhlenbeck:

That didn't at that time involve us at all, at least not in that spring. That came then in the fall, you see, because then it was also more or less clear. Ehrenfest knew that very well of course, the Bose papers. He had been involved in the controversy, that this was not something which was obvious, but that this was really a new assumption. That was in his papers. And at that time also the Schrodinger paper appeared, which was really the paper just before the quantum theory series. I think it was in Phys. ZS. And that we studied also, and that made a great impression both on Ehrenfest and on me, because it was so clear. And then in the fall Ehrenfest and I said … that we must now put everything together; he thought that that would be a proper thesis for me, to make this nicely systematic and see how it can be all said from one point of view. And there was of course always these two aspects of these statistics. Either you look upon the radiation as quantized oscillators, and use the good old statistics. Or look upon it as photons, and then do something funny with the counting of states. At that time also — It must have been beginning '27 — the Dirac paper came out, which translated it in terms of the symmetric eigen function. We worked on it. It was mainly a systematic paper. You did everything the same way; you did it always with the method of steepest descents, with the (Fowler) method, although that was against Ehrenfest's grain… Too many complex integrals. But I insisted on it, that we do it. Because I like these things, and it was also quite simple. And then to my great distress — I was just then starting — (Ehrenfest said, "Now start typing it up",) And Fowler came out — R. H. Fowler with really about the same thing as we did. And that was a little blow, but, Ehrenfest says, "Well don't bother. We will not write a paper about it, but you will write a thesis about it anyway." And that was '27. I was all this time assistant of Ehrenfest, of course.

Kuhn:

You had Fermi statistics in this too.

Uhlenbeck:

Fermi statistics was there also. And there again, as with the radiation, you must either speak about what we called the spin oscillators. They could only have two states. Or you must do it with particles. Of course in a sense it was formal, because even if you do it with oscillators you must, for material systems, put in that the sum of the particles is n; which is of course for such an oscillator view completely unnatural. For the radiation you didn't have that. If you then translate it to the Bose ideal gas, it was a bit formal, but still you could always say it this way.

Kuhn:

Was this aspect of the counting bothersome? Did it trouble you?

Uhlenbeck:

No. You see, you knew what had to come out, so it was clear how you had to do it. And the basic assumptions were on the microscale, that means on the energy levels in the box. You must say that the distribution of particles on these levels is, for Bose statistics, always one; for Fermi statistics it must be zero as soon as there are two particles, and otherwise it is also one; and Boltzmann is n factorial over — And already if you say it that way, you see clearly that the Boltzmann statistics are always in between the two results. Only at the end of that, and it was of course incorporated in my dissertation, it became clear to us that in a certain sense the Bose and the Fermi statistics were the only ones. Because they are the symmetric, and antisymmetric eigen functions, and there was so to say nothing in between that you could really do.

Kuhn:

That was partly due to the Dirac paper, I take it.

Uhlenbeck:

Partially to the Dirac paper. And then immediately we said, but how should you then say the Boltzmann statistics? And then it was very clear that you had to say, "Well, you have to take all the eigen functions. Not only the symmetric ones, but you had to take them all. Well it was a little exercise to show that that's equivalent to Boltzmann statistics. And that Ehrenfest and I wrote a little note about. That was one of our collaborations. And that was an excitement; that was very interesting. That was all before my dissertation. Then Ehrenfest went away. I think he was in Paris, or something or other. And I got suddenly a typical Ehrenfest postcard. Maybe I still have it, or I gave it to Martin Klein. He says, "Fermi statistics means the impenetrability of matter." Exclamation sign; exclamation sign. "See you in Leiden. Come 10 tomorrow morning." What he had seen was the following — unfortunately on a one-dimensional model. If you take the combined wave function and require that the wave function has to be zero if the coordinates of two points are the same, then they cannot (penetrate); that's the impenetrability. Then he thought that the Fermi statistics followed from that, which is true in one-dimension, but not in two or three. For a while we were very excited about it. It was typical for Ehrenfest that now he saw why. Of course! Matter with impenetrability is a fundamental physical action and that it had Fermi statistics as a consequence, that was a great thing. Then we wrote a paper immediately, also, on something which always bothered him. That was the Einstein mixing paradox. You take say two Bose gases or two Fermi gases, and you take a mixture of these, and then you think of it that particles become more and more alike. Now there was of course the well—known Gibbs mixing paradox, that the entropy changes by R, but this had no effect on any physical properties of the mixing. (Even things which were very close were of course physically undistinguishable in the old statistics from their equal, although the entropy made a jump.) That was an old question, and that we knew alright.

Kuhn:

Had this question been worried about very much?

Uhlenbeck:

Ja, in the old days people worried a bit about it…It was clear that it was due to the counting. The number of ways changed suddenly because you make suddenly two things indistinguishable, where previously they were distinguishable… We had heard of it, and Ehrenfest had explained why in his lecture, and that was all. But here then, something different happened. Now, as soon as the two particles were equal, in the Fermi gas, the Pauli principle begins to work, and begins to push them up to higher energies suddenly. And that means that suddenly the pressure will jump. That seemed paradoxical — the Einstein paradox. We wrote a paper on this, which was based really on this impenetrability idea. This was the reason for it; that they suddenly became impenetrable. Now all that was wrong. But one must say of Ehrenfest, he is one of the few people, when they write a wrong paper, write a note about it, telling that it was wrong. And he took it all back. And that is in his collected works, both papers, and also his retraction. It then became clear relatively fast afterwards that this in one dimension helped so much, but that in two dimensions, when you make it zero only on a little sphere, that doesn't (change the frequencies) very much. And therefore it was not the correct (proof). But it was, for a moment an excitement. It was about April of that year, I think. Being Ehrenfest's assistant, I was in charge really of taking care of several of the colloquia for the younger students. He had always this pedagogical principle that everybody had to teach the younger students.

It had always been done in colloquia. I had always to run some of those for the younger students. And I was anyway quite busy. It was clear that I would never write a dissertation if I stayed in Leiden. And then he sent me off with a Lorentz fellowship. He said now you go to Copenhagen and there you write your dissertation. And Sam was there then too, and then for two months we did essentially nothing else but writing. That was all. It was high pressure. Boy was it high pressure, writing this dissertation. And the last day of the academic year — it was already all arranged, Ehrenfest had made the dates fixed, so it had to be printed, and a correction had to be read, and the whole book had to be read — Well, it all came through, and we got the degree on the same day. At that time we had already the job in America, so we left in August, just about a month after our dissertation. Oh, and then there was also, with regard to all these statistics, there was also the question of the Einstein-Bose condensation, which of course Einstein had predicted. And that was a curious argument. I tried to do it a little bit better, and then thought that I could prove that it was wrong, that there was no discontinuity whatsoever, that there was no Einstein condensation. And Ehrenfest was highly impressed and he believed it too.

He had some correspondence with Einstein about it, which Martin Klein at the moment is studying. It was a very humorous exchange of letters. One of them had all such citations from Goethe in it. Well — I forget. Very funny — typical Ehrenfest…He was so to say kidding Einstein about it, that he made this mistake, you see. He says, "Ja, but when the great men so to say sleep, then there is something for younger people to do." But Einstein did not give in. He said "No, it is not wrong. It is not wrong." "You have not understood it," he says. And he never completely gave in, although he says, "Ja, it was not quite right the way I said it." And of course at present we know really that we were both right. Namely, that if you do the Bose statistics with a finite number of particles, there is no Einstein-Bose condensation. The condensation is always a limit property, for infinite number and infinite volume, but with finite density. And Einstein had always silently assumed that, without making it clear. And I don't think he was clear that that was essential. At least he didn't say it in so many words. I put it in my dissertation. There is a part in my dissertation in which I said there is no Einstein-Bose condensation. Then later when I worked on the condensation problem, then I understood really. When I finally understood that it was a limit property, then it was clear that it was still existent. So I was in a sense fundamentally wrong, but not so to say the mathematics. That was also part of that same period, that I found that out.

Kuhn:

George, I've got a question. I don't know whether I can make it clear. Today one speaks of particles that obey Fermi statistics, particles that obey Bose statistics…In talking about the derivations with you, I just wanted to say, "Now what's the situation? I'll write down a different sort of probability formula." You wanted to say, "It's a question as to whether these obey Boltzmann statistics as oscillators or whether they obey the Bose statistics as photons." Now, at the beginning how was that? Did one start very early thinking of these as different sorts of statistics?

Uhlenbeck:

Ja, well you see, Ehrenfest — I may even still have notes about it — each year had this course in statistical mechanics which ended up so to say in quantum theory, and then off into newer things. He touched upon them…

Kuhn:

Now this would be before '25? Before the Bose papers?

Uhlenbeck:

Well, maybe at the same time. That I don't know. It was just a lecture. Because that was part of his controversy with Bose, and also with other people I think. And he hammered always that if it is photons and if you could believe for these photons that there were particles like the Boltzmann particles in the gas, then you would get the Wien limit law.

Kuhn:

That's a very old derivation.

Uhlenbeck:

A derivation that we learned. Then he said that if you want to, with this picture, get the Planck radiation formula, you must do something really radical in the distribution function. And he considered, I think, the whole Bose derivation as a kind of an ad hoc case; in order to get the right answer, which I think it more or less was. I mean there were many confusions in it, but of course it helped very much that he knew what the answer was.

Kuhn:

What about the slightly earlier interpretation in terms of photo-molecules?

Uhlenbeck:

That I never heard of; that we never heard much about…

Kuhn:

It's a fairly brief development, but it may well have something to do with Bose's route to the statistics.

Uhlenbeck:

Ja, I don't know anything about that, how Bose came about, because he was simply not known. An unknown man he was when his paper came out… But the only memory I have really very sharply is that the Schrodinger paper in Phys. ZS, was, even for Ehrenfest, clarifying. He may have known it, but it was for him a very clarifying paper; and certainly for me. That was the paper which was really the starting point for my dissertation. But about the photo-molecules, I never heard anything.

Kuhn:

In the early days of the Bose statistics Einstein talks about a material gas with the property of photons. Is that thought of as purely an exercise?

Uhlenbeck:

It sounded extremely speculative. And I think with Einstein it also was pure speculation in a sense. He says, "Why not particles too?" But I think even in my dissertation I thought there were no examples. I thought Bose statistics only photons; Fermi statistics everything else; which was not right, of course. Then there was the Elsasser theorem, which later Ehrenfest and Oppenheimer wrote about. Say if you take a hydrogen molecule, that would have Bose statistics because it has an even number of Fermi particles. That means in the translational motion it is Bose statistics. That I am sure came much later. That was I think '28 or something like that…I have correspondence with Martin Klein about that, about how that came precisely about. Because I remember talking with Ehrenfest about that during one of the summer schools in Ann Arbor, so that must have been '28, '29; and then for the first time there were really — at least a priori — cases where it should be so. Although of course there was no empirical argument.

Kuhn:

No, I asked about that because just thinking and talking about material particles does mean that you're thinking about a sort of statistics which material particles may satisfy. On the other hand you might say, "Photons are not like material particles so we count them differently". If you take the second of these approaches, then you're likely not even to set up the Bose material gas…The Einsteins' papers came after de Broglie,…and I think this was, so to say, the first realization with him of the wave —

Uhlenbeck:

— particle duality. If really particles have also wave properties, then gas particles should also behave in some sense like separate waves. And therefore the distribution should be similar to Planck. That was extremely bold, of course, that it was immediately connected with the wave-particle duality. And that was also clear in the Schrodinger paper. There was always the duality question.

Kuhn:

Let me just tack on one other question. There's a reference in something of Planck's in '23 perhaps in the Bohr Heft of Naturwissenschaften — about the interpretations of quantum mechanics which undermine causality.

Uhlenbeck:

Well I've no memory of it. I don't think it was an issue for us. I mean these questions were — Of course Ehrenfest was always interested in fundamental questions always. But he was not a philosopher. I think as soon as it got so philosophical, then he was —. He wanted always to know is there a sharp point. Is there something by which it goes one way or another and one can decide? So no, I have no memory about that at all. I don't even know Ehrenfest's reactions to the uncertainty relations. He probably would have liked it. He was extremely proud, extremely proud, about one of his last papers, and that was the Ehrenfest theorem; the wave packet in general moves like a classical particle; You can deduce this from the Schrodinger equation by thinking of the Schrodinger equation as a heat conduction equation. (And so he immediately would know in general what the wave packet is.) And he then derived that average value of the velocity and of the acceleration and so on obeyed the ordinary classical mechanical rules. That's nowadays called the Ehrenfest theorem. That was one of his last papers. It was after I had gone. It was I think maybe '29. And he was very proud of that. It was also an extremely acute paper. He was at that time so sensitive about, so to say, the big shots and the things they said. And they were all very nice about this paper.

Kuhn:

With all the personal factors that were involved in his suicide, did, do you suppose, any sense of the new physics having left him behind also play a role there?

Uhlenbeck:

Oh ja, I mean, he had such a feeling that he couldn't do it anymore. And then he had the feeling that he was the professor, that there was only one, and that there were all these smart youngsters who should have his job. He was perhaps still good to teach, but he couldn't take part in it. That was certainly an aspect of it. It was not the only one, but certainly an aspect, because he was very depressed, boy, was he depressed about these things. He couldn't understand it so quick. He wanted to have such Gefuhl, such feeling. No, for him it was really very hard to get used to this sort of confusion without sharp models and without (Anschaulichkeit) without knowing what you precisely did. (That was terrible for him.)

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George Uhlenbeck – Session V

December 9, 1963

Interviewed by: Thomas S. Kuhn
Location: Rockefeller Institute, New York, New York

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Abstract:

This interview was conducted as part of the Archives for the History of Quantum Physics project, which includes tapes and transcripts of oral history interviews conducted with ca. 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: Henri Abram, Niels Henrik David Bohr, Max Born, Louis de Broglie, Max Delbruck, Paul Adrien Maurice Dirac, Tatiana Ehrenfest, Paul Ehrenfest, Walter M. Elsasser, Enrico Fermi, Ralph Fowler, Samuel Abraham Goudsmit, Werner Heisenberg, Oskar Benjamin Klein, Hendrik Anthony Kramers, J. P. Kuenen, Otto Laporte, Hendrik Antoon Lorentz, J. Robert Oppenheimer, Wolfgang Pauli, Isidor Isaac Rabi, Harrison McAllister Randall, Julian Schwinger, Arnold Sommerfeld, Llewellyn Hilleth Thomas; American Physical Society meeting (Boston), Huygens Club, Kapitsa Club, Rijksuniversiteit te Leiden, Technische Hogeschool Delft, University of California at Berkeley, and University of Michigan.

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Kuhn:

George, I think there's one thing that is sort of a complete blank in the way we worked before and which I'm trying to pick up systematically with people because it's useful comparative material. This is this question of early life, as though you were going to give us a biography, but obviously with an idea to the sorts of things that determined your interest in science -– I know that it wasn't very clear for a long time that you were going to be a scientist – how this related to family background, and things of this sort. So you could start pre-natally, or in the crib, or in kindergarten. Tell us something about the sort of family you came from, who was interested in the sciences.

Uhlenbeck:

You see, I of course was born in the Indies, in Batavia, and the whole family is military, both on my mother's side and on my father's side. My grandfathers on both sides were all military men and, as a consequence, had their careers in the Indies where I was born. A consequence of that was also that they always got out early because one year in the Indies counted as two years' service so after twenty years my father retired although he was only forty-two, and he did it mainly for the education of the children.

Kuhn:

You mean he retired?

Uhlenbeck:

He retired and went to Holland because as a military man the last place we were at was a small village in Sumatra where there were hardly any schools, so I came to Holland when I was six years old and we lived in the Hague. My father had all kinds of accessory jobs because the pension was not very high but still it was enough. So my whole early education was in the Hague where I went all through elementary and high schools; and, ja, I was a very dutiful student, very dutiful. I always worked very regularly and I was always very good in the class and so on. I was certainly not clear until the last years in high school what I was going to do; then mainly through my sister who was five years old, —

Kuhn:

Was that your only sibling, or did you have others?

Uhlenbeck:

No, I have two younger brothers, but that was my older sister who had great influence on me at that time. Well, where were we? We were about at my high school. Now let me tell a little bit about one teacher I had: that was Mr. (Borgesius) whom I mentioned in the introduction of my thesis. He was a very shy man and also he had no order in his class, but he had the reputation in the school — mainly through my sister — that he was the most learned man in the whole staff; and for me at that time, and for quite a while, anything which was learned was good for me. So I tried to get in touch with him, but since he was very shy and I was, of course, much shyer even, it was a very strange connection I had with him. Once in awhile we came together, but neither of us said very much, but it had the result that I learned calculus when I was in high school because he gave me books, you see.

Kuhn:

Was his field mathematics?

Uhlenbeck:

No, he was a physicist, really an experimental physicist, and had done a very excellent dissertation; he took extreme care with demonstrations which were simply beautiful but were lost to the students because he had no discipline. I still remember that he gave these lectures and worked these demonstrations and only a few people would listen, so to speak. But because of him I learned calculus and he also gave me lectures of Lorentz; you see, for many years, Lorentz gave elementary physics lectures in Leiden, just as Feynman does now at Caltech. He thought that was what he did. These lectures came out in a two-volume book. They were really remarkable — I don't know whether I have it anymore, I don't think so. They used only a little bit of calculus which was taught by Lorentz himself in the beginning few pages and it was completely original — original lectures really. I got the Feynman lectures; they are quite different but they have that same quality of suddenly someone who is really a very great physicist suddenly thinking that he should tell it to the youth, so to speak. This book, by the way, afterwards, after many editions, came into the hands of a committee and now it is a committee book and just as bad as everyone else, but the first editions I still think were classics. He called it Beginselen der Natuurkunde. I went through that while I was still in high school, mainly through this teacher, which had as its consequence my knowing calculus and this thing; I had practically copied this book with all its notes. No problems, there were no problems, which was also very typical.

Kuhn:

Typical of Lorentz?

Uhlenbeck:

Yes. There were problems in the calculus book which was also by Lorentz. [Lehrbuch der Differential und Integralrechnung (1907)]

Kuhn:

Was it?

Uhlenbeck:

Yes. He wrote a book on calculus in which I did all the problems and those went very far really and were very interesting. I finished high school and then I think, I told you already, I didn't start in physics but rather in chemical engineering.

Kuhn:

I wanted to ask you a question. You did tell me that; how did it happen, George, that you went to high school instead of gymnasium?

Uhlenbeck:

First of all, because it was one year shorter and no one of our family, so to speak, had gone to gymnasium, and —

Kuhn:

Had they gone to high school and then to military school? You had an uncle in the University —?

Uhlenbeck:

Yes, I had an uncle in the University, of course, who had also great influence on me, but a bit later really; he was the great linguist. But it was not discussed; it was just the obvious thing to do that one went to the high school. I was not clearly a man who was interested in languages and so on and I also didn't want to go into medical school or law or something like that; that was also very clear. Well, when I was 13 I just went to the same high school my sister went to and that was it.

Kuhn:

But as of that time it meant that you were closing off the possibility of going to the University?

Uhlenbeck:

Yes, I could go to the technical high school, of course, to which I went. Then, as I told you, this law was changed and I was very unhappy in Delft mainly because it was such a mechanical sort of business, all these many lectures which one had to go to and chemical laboratories which one had to take and at which I was not very good at all. I didn't like that, so after a semester, then again because my sister knew that I was unhappy, she persuaded my parents that I should go to the University then.

Kuhn:

We talked a reasonably large amount about the University already; I'd like to see if we could explore more of this earlier period. Your family was generally a good deal oriented toward learning? Would you say they were interested in scholarly pursuits?

Uhlenbeck:

No, they weren't. I think one of the disappointments of my parents was that none of their sons went to the military schools; only one of them wanted to, but he couldn't because of his eyes, and I didn't want to so he [my father] thought I should just become an engineer, which seemed much better to him than to be a high school teacher. But it was clear that since there was very little money, I had very soon to get some kind of a job. It was already quite a sacrifice for the family to let me study, you see. My sister never studied; she got an education to become a biology teacher, which she did. But I was then supposed to go to the University.

Kuhn:

Did she go on after high school?

Uhlenbeck:

She went on after high school and then studied biology with a teacher in the Hague to get this degree which gave her the right to teach biology in high schools. When I finally went to Leiden she went then too for a year.

Kuhn:

To the University?

Uhlenbeck:

Yes. But the University was seven years while Delft was five years. These were the arguments in which it seemed to my parents very important that I get through with that as soon as I could; therefore there was a little bit of reaction from my going to the University, but not really. I didn't have to fight very much about it because it was clear that I was unhappy and so I was allowed to go to Leiden at that time. I stayed, of course, always at home; we commuted by train to Leiden or to Delft in those cases, and that was really the way it went. Then when I came to Leiden that was, so to speak, paradise for me because there were hardly any lectures there. I didn't get in touch with Ehrenfest at all in the early years, but I worked by myself very much and the lectures didn't bother me very much because there I was helped by the fact that I knew calculus already so most of it was very simple, and so I had lots of free time, an enormous amount of free time -– which I didn't have in Delft at all. You had to do a bit of experimental physics, but you were not allowed to do it more than one afternoon a week; it was forbidden to do it more than one, which was all right with me. I did it once a week, one afternoon a week. Those two years before my first examination -– there were no examinations -– one must not underestimate that business. There were no course examinations, no constraints whatsoever; nobody even knew whether you went to the lectures and I very often didn't at that time.

You could just do what you wanted and what I did at that time mainly was to study Boltzmann's gas theory which I picked up second-hand -– I still have the copy — in the Hague, I thought, "Jesus," that looked to me the most learned theory; if I could do that, then I would really know something. Now with regard to the kinetic theory, my interest was already aroused in high school, at least when I studied these lectures of Lorentz's; I was very much impressed by kinetic theory, mainly because I thought it was an example of theory, it explained something. Also, in that same book of Lorentz's there were in the electricity part, models, so to speak, which were part of the mechanical models of the ether which he used mainly to illustrate the various relations. Those I thought were just swindle — those had nothing to do with reality — but the kinetic theory I thought was a theory, so I learned that already, and when I got this gas theory I really studied it; I copied the book practically and, as a result, I had to learn analytical mechanics because that was needed for it, so I studied that a bit for myself.

Kuhn:

What did you study that in?

Uhlenbeck:

I studied it out of a book by [P.] Appell not a big book but rather a precis which I only went through enough so that I could follow Boltzmann again with it. Also I studied Maxwell theory a little bit out of this little book by Schaefer; it was also a very thin book.

Kuhn:

It was Clemens Schaefer?

Uhlenbeck:

Clemens Schaefer, yes, but he had this very thin book at that time which was in a red cover. All that I did essentially for myself, but you see, there were no lectures in physics because Ehrenfest lectured after the first exam. The only lectures in physics were the general elementary course which still used Lorentz's book which at that stage was diluted, so to speak, and already terrible. There was one lecture by [J. P.] Kuenen in thermodynamics. That was the only one I followed in physics, really, and although it was all right he was such a hesitant lecturer and went so slow for me that I learned something about thermodynamics, but it //had little effect on me//. The one thing I worked very hard in for the University was the experiment, because there you had this way as I told you, where the first year you could go once a week and the second year twice a week. You had to do, I think, about forty experiments in which for each of them there was written out a little syllabus in which all the formulas and so on which one had to use were mentioned. Then you had to write a little report for each thing, and those I took enormously seriously because, as I said, I was very dutiful. I derived all these formulas; I wanted to derive them all. That's one of the reasons I did this Maxwell theory. And I wrote it all at length. That made for me personally a good impression because Kuenen got so impressed by that; he saw those things, and as a result, the third year, through the efforts of Kuenen, I got what you call a fellowship which was quite exceptional in those days. It was a state fellowship, which meant, on the one hand, that you didn't need to pay the tuition, which amounted, I think, to about a thousand guilders, and that was a godsend for my parents. Towards the end of the second year I had to take this exam, and that was terrible for me. But I finally did it together with a good friend of mine -— we took it on the same day -— a man named Nijhoff. I especially had connections with Nijhoff who is the son of the big publisher and who was the brother of one of the great poets of Holland. I admired that poetry very much. I was always with him; he was older than I was but started to study philosophy, then went into experimental physics. I learned lots of things from him; I remember that he talked about philosophy, Spinoza, with me and we were always together and took exams on the same day. I didn't know Sam [Goudsmit] very well yet.

Kuhn:

When you say the exams were dreadful for you, why?

Uhlenbeck:

Well, because there you have to do these orals; they were all oral, and that means you have to know a lot of things ready and especially in mathematics it was very hard for me that I had to know all the convergence criteria, series, and whatnot, because, of course, the second year the mathematics went further than calculus. It was just that this kind of cramming I thought was very disgusting but I did it and it was precisely on time. I took the exam on time, but afterwards I was a little bit overworked; I was very tired. That's the time when I first got in contact with Ehrenfest; that was after. I had very little contact with Ehrenfest all the time and he told me once that he was so surprised at himself that he always had some confidence in me although he didn't really know me at all, but I think it was mainly because of Kuenen and because I was so dutiful.

Kuhn:

You mean you had little contact with him then really until you came back from Rome?

Uhlenbeck:

I talked with him all right, once in a while, and he had a little seminar that semester in my third year which I took part in and which I even gave a little talk and all that, but I was at that time rather tired and I didn't, so to speak, do anything more than follow the lectures of Ehrenfest, and the mathematics tool. It was at that time that Ehrenfest suddenly mentioned this job in Rome. He had had a request from the Dutch ambassador and I jumped at it.

Kuhn:

I forget; did you finish your third year before you went to Rome or did you just do half of it?

Uhlenbeck:

No, I finished my fourth year, I'm sorry, because during my fourth year I was a teacher in the gymnasia in Leiden for 12 hours, and I didn't like that either because I didn't have much discipline in my classes and it was quite a strain on me. That was the fourth year, and it was at the end of the fourth year -– so it was not the third year — that Ehrenfest mentioned this job, which implied, of course, that I had already done a year and a half after my first exam. At that time you were supposed to do your second exam after five years so he said I had to do the second exam at the end of this year and I worked very hard in Rome. I came back at the end of this year and I worked very hard in Rome. I came back during the vacations to do the exams and I just managed to do that somehow.

Kuhn:

You and other people keep referring to Huygens' [Club] as though I knew all about it and I don't.

Uhlenbeck:

Well, that was a dispute as they call it; it existed quite a long time and I became a member of it in my third year as also Sam did some few years later. There were students in it all about the same level, some a little older, after the first exam. In my memory we got together once every week or once every other week at each other's rooms and one of us gave a little talk; then there was usually a second little talk, the main talk, a little one, and then what you call improvisations and so on. It was very nice, and we —

Kuhn:

Was it all in the sciences?

Uhlenbeck:

Yes. There were always a few chemists, astronomers and mathematicians in it. I gave several talks in it and worked very hard at it. We had a little blackboard which we carried about and once a year there was a big yearly festival.

Kuhn:

How large a group was it?

Uhlenbeck:

I would estimate about 20, not more certainly.

Kuhn:

How long did one stay in it before one made room for some others?

Uhlenbeck:

Until the doctoral exam; then you became "outlaid" and you came once in a while. Even went I was working for my dissertation I still came there and then I was one of the "outlaid". Ehrenfest encouraged it very strongly and, in fact, he said it had to be extended and that was one of the things I did when I was his assistant. I started, together with others, to adopt for the young student a similar one which was called the Leiden Jar. There what he wanted — what he always wanted — was the students to teach each other, so the Leiden Jar meetings were essentially meetings of the young students in which either part of the lectures or questions about the lectures which the students followed were, so to speak, repeated, or where they did some other things; and the people who took charge of it were members of Huygens. It was a hierarchy, you see, and the best of the Leiden Jar became members of Huygens; that was the scheme. But as a result, the people were always talking together. Ehrenfest came once in a while, suddenly popping in at a Huygens meeting or coming to such a Leiden Jar meeting. He was very much in favor of that; this was, so to speak, his method of teaching because although he would always (with) one man he still had this responsibility and he thought that he could just let it go on from there.

Kuhn:

At what point did you make major contact with your uncle who, you say, influenced you so much?

Uhlenbeck:

During my third year and also the fourth year.

Kuhn:

He was at Leiden?

Uhlenbeck:

Oh, yes. He was in very delicate heath and he was very often overworked; he somehow managed to go on pension when he was only 61 and after he became a pensioner he became so fine! Then he did lots of work and died only when he was 85 and had a wonderful time -– lived in Switzerland most of the time. But this duty as a professor he just couldn't stand although he was a wonderful lecturer according to everybody. Anyway, for quite a while, during the days I was in Leiden, I had lunch at his house and he was very family-conscious. He was the one who made the whole family tree and took care of it as it came in this little book on families; he went and looked through the old documents in Germany and everywhere else.

Kuhn:

Was he your mother's brother or your father's?

Uhlenbeck:

He was a cousin of my father; he was on the same level as my father.

Kuhn:

Not really an uncle then?

Uhlenbeck:

No, although we always called him Uncle Cornelius, C. C. Uhlenbeck. He had no children. He was an expert in linguistics, a great Dutch linguist at that time, and he was a man who was very fond of literature and so on, all the literature. I still remember that when I came there I went to his study which was on the first floor and was full of books and there he sat; then I talked with him and very often he started reading. I still remember his reading all these Sanskrit things to me; I didn't understand a word but it sounded simply magnificent. Then we had lunch and took a little walk in the garden where we talked on everything; he was a very great influence on me, of course, as a young man. After lunch I went back to the school.

Kuhn:

One thing I realize I don't understand here: in your last few years at least in high school you were sure you wanted to do science?

Uhlenbeck:

Somehow science, yes. Physics already; you see, because of this (Borgesius) it was clear that I had to do physics and really also theoretical physics was also clear because that looked much more learned than experimental. As another example of my -—

Kuhn:

But later you were a lot less sure of it?

Uhlenbeck:

At Rome. The last year in Rome shook me up very hard; then I didn't know anything about what I was going to do.

Kuhn:

What was it about the experience in Rome that shook you so?

Uhlenbeck:

I got completely dissociated from physics; I didn't do any physics at all because I didn't know anything. The second year at Rome I still had contact with Fermi but not later and the people I saw were all of another direction. So somehow the study of physics disappeared and I didn't do anything for a whole year, no reading. I, or course, had my job to do which was relatively simple but I read all kinds of Burckhardt and Mommsen and history of art -— I had a very good friend who was an art historian with whom I went around — and all kinds of things for a whole year, you see. So at the end I didn't know if

I should do it at all? Thank God, the one that that kept me back was that I didn't know Latin and Greek and it was clear that if I wanted to do history — which I wanted at that moment mainly through the influence of the books of Huizinga which I then thought were so magnificent that that was really the thing to do — I had to learn that. I still remember that I talked with my uncle about it; he thought very highly of Huizinga and was a colleague of his at Leiden. He said, "Ja, that is very fine; of course, that is very profound stuff, but you have to learn Latin and Greek." So I started to learn Latin; I took lessons in it as soon as I came back but my uncle said, "Try anyway also to see whether you can still get a Ph.D. in physics because that sounds, so to speak, more practical." And that's why I went to Ehrenfest and Ehrenfest said, "All right." He needed an assistant at that time and so he asked me after a month of so if I wanted to become his assistant. Then I started working with Sam and with Ehrenfest all the time and in the fall I still started to learn Latin, but very soon it was far too difficult for me and I had so many things to do that I never even went over the hump with Latin and then whole thing disappeared. Then I was simply back in the groove again. But that was the year which made me uncertain about it.

Kuhn:

Through all of this time it was most likely that you would wind up as a high school teacher?

Uhlenbeck:

Yes, but that was very, very terrible to me because I thought it was a terrible job. I had done it a year and I thought it was a terrible job. That was also one of the things which bothered me very much at that time, the future of it, you see. Thanks God, there were no financial difficulties anymore; that I never had any more.

Kuhn:

Because of the assistantship and that sort of thing?

Uhlenbeck:

First, the high school teaching, then, of course, Rome -– I got a princely salary for that and so I saved a lot — and then I became an assistant and even had money saved and so on, so I was completely independent from the end of my third year on.

Kuhn:

Did you still live at home?

Uhlenbeck:

Not any more, no. I lodged in Leiden and came home on weekends. During this Leiden period I was always together with Nijhoff and with Wiersma. Wiersma was a very good experimental physicist; he became a professor in Delft later on. Both are dead now, Nijhoff died very early, Wiersma just after the war. But then at that time, of course, after the spin which happened the first three months I was back essentially, then, of course, there was no doubt anymore about what I was going to do. Then, of course, it was a godsend for me that we got this offer to come to America; I didn't have to be a high school teacher!

Kuhn:

I gather from Sam that you were clear from the start that you wanted to take that?

Uhlenbeck:

Yes! Right away, I said right away; Sam was a little bit more doubtful but he did it too finally.

Kuhn:

Did you think of that at the time as being a permanent move to America, or were you —?

Uhlenbeck:

Yes, more or less. Ehrenfest always said —. We got the job partially through Ehrenfest. Sam may have told you about how we finally got this offer because we were relatively well-known then among the younger generation. Randall at Ann Arbor wanted to build up theory -– maybe you know it already?

Kuhn:

Yes, you've talked a bit about it and Sam has also talked about how Colby came, and —

Uhlenbeck:

So after the decision was made, Ehrenfest said, "Of course, you should go there; all the younger people who are really good should go away, and then you should afterwards come back if they want to make you professors somewhere." That was, so to speak, another scheme of Ehrenfest's; he thought you should send them all away and then you could call them back, but the last one didn't happen, of course. That was the trouble with his scheme, although I did it, partially because I had that in mind and partially through Kramers who also, so to speak, pulled the "duty-racket" on me. He said it was my duty to come back to the fatherland in '35 and I did, although I didn't want to at all.

Kuhn:

You didn't want to even then?

Uhlenbeck:

But I thought it was my duty. Something which was perhaps another small point and which was, I think, characteristic of my views was that everything which was learned was good. So in my third or fourth year -- I don't recall which -- I studied [Newton's] Principia Mathematica extremely hard; it has three volumes and I didn't go through the three volumes, but I went almost through the first volume only. It was really because it absolutely looked like abracadabra — all these things which looked very learned to me. So together with Nijhoff I studied it. In a mathematics colloquium I also gave a couple of lectures about it and I still very much remember the reaction of the old mathematician, [J. C.] Kluyver. He was really the best mathematician in Leiden; he was an old man but he was really an old-fashioned analyst, so he listened and he was a very sarcastic man. He said, "Well, that's very interesting that you can do this all by these symbols on the board, but now, if all the mathematics really is just this combination of symbols, then one could do it on a machine! We could do it on a machine and we would never have to think again!" And that shocked me so much, so terribly, that I thought, "Jesus, if it is that mechanical —!" That put a damper on the whole enthusiasm for Principia Mathematica. "If a machine can do it, then it is clearly not very learned."

Kuhn:

Where did you get, do you suppose, this deep interest and value for anything learned?

Uhlenbeck:

Very young, I think, and I think mainly through my sister; she was always impressed by it and it was because of her, I think. I don't know why or how it came, but it was just somehow that anything which —. Well, also perhaps through my uncle who was considered by the family as an enormously learned man and everybody in the family looked up to him.

Kuhn:

But he was admired for his learning in the family?

Uhlenbeck:

In the family, yes. My father sometimes made fun of him a little bit, too, but he was still Uncle Cornelius. Ja, he was clearly so learned and knew everything, so to speak, and could learn —. There were all sorts of stories in the family about him -– how many languages he spoke; they were all apocryphal. He didn't know so many languages, although he, of course, spoke most of the languages very fluently and also Russian. But he knew many, of course, like a linguist knows them. So I think mainly because of these two influences on the side and also because of Nijhoff for whom I had enormous respect in my youth because he had all this knowledge about philosophy which I tried to read but had not —.

Kuhn:

Had you know him before the University?

Uhlenbeck:

No, not before, but the first years at the University; I knew him right away when I came. The point was that in a certain sense it went contrary to Ehrenfest and I think it was extremely healthy for me that I then got in contact with Ehrenfest who did not want to do anything learned; if you couldn't say it simply, if you couldn't be to the point, then he didn't want to hear it, and anything which was, so to speak, long-winded and learned, he made immediate fun of. And as a result, and since he was finally the man who certainly had the most influence on me, this counter-acted this thing of mine considerably. You see, in mathematics for instance, in the old days, I was the one who wanted to have it absolutely rigorous; now, after Ehrenfest, I think it is bad when it is very rigorous! So it was very good for me when Ehrenfest really took me in hand in that respect. Every time I worked with him —. He also used his assistants sometimes to read papers which he wanted to know. Then we had to tell him about them from notes we made and so on, and then, of course, if I didn't say the point — Jesus! -– or if I hadn't understood the point!

Kuhn:

Do you remember any of the papers you read for him this way?

Uhlenbeck:

Oh, yes, many. About the Compton effect and about these experiments of Bothe and — what was it?

Kuhn:

Geiger. Well, if you read the Compton effect this would be not right at the time but later papers on the Compton effect?

Uhlenbeck:

Later papers, yes.

Kuhn:

Did you read the Dirac paper?

Uhlenbeck:

No, not Dirac. Of course, what we went through right in the beginning was all the Schrodinger papers, one by one, all of them.

Kuhn:

But this would already be a little later?

Uhlenbeck:

That was in '26; my assistantship really began in '26. In '25, the first three months there was the excitement of the spin thing and everything, but in '26 when Sam really went to Copenhagen, then I was really the one with whom Ehrenfest worked.

Kuhn:

Did you, do you think, spot the Schrodinger papers with the very first one, or did it wait for the second?

Uhlenbeck:

No, right from the first, of course.

Kuhn:

Do you think you went over the first paper carefully even before the second one came out?

Uhlenbeck:

No, that is not so, because the first one had, of course, the hydrogen atom in it and that was already a bit learned. Even Ehrenfest did not know that quite. But then the next one, which had Hermite polynomials, we went through and then came back to the first one.

Kuhn:

Do you think you were interested in the first one from the start?

Uhlenbeck:

Oh, yes.

Kuhn:

The first one was very strange and it's the reactions to this that I'm trying to get at. The derivation of the equation seems to be nothing; I mean, it looks like nothing at all. At least I haven't talked to anybody who felt as though he understood it.

Uhlenbeck:

I don't recall that in detail. I think we studied it. You see, at that time the man who knew a lot of mathematics and whom Ehrenfest always asked was [J.] Woltjer, a theoretical astronomer. His son is now a professor in astronomy at Columbia. Ehrenfest always used him instead of the professional mathematician with whom he didn't get along so well. I still remember that Woltjer talked to us about the first paper and about the mathematics, this complex integral, but Ehrenfest of course -– "Ach, die komplexen Integrals" -– that was always hard on him.

Kuhn:

What I'm really trying hardest to spot is to see whether one could still remember whether one was interested in the first paper before the second one came out.

Uhlenbeck:

Well, I think one was extremely impressed that the Balmer series came out and that, of course, came from the first paper so it was clear; while from the matrix mechanics it was, as Pauli would say, an "analytisches Kanstatuck" to get the Balmer formula out. Pauli did it, of course, too. But here it was, so to speak, with more or less traditional mathematics. And then the next one — you know, they came issue by issue — we went through very, very carefully. Perturbation theory. Then, of course, I learned for the first time what the spherical harmonics was; we didn't know that.

Kuhn:

Tell me something about the nature of the interest in these problems with the wave equation that you worked on with Ehrenfest. What was that about? What motivated that work? What was the point of it?

Uhlenbeck:

Ehrenfest always read or wanted to know the recent developments. It was also clear that this was a very important recent development; furthermore, it used mathematics with which everybody was more familiar, and I think that anybody at that stage couldn't help saying when you saw the papers of Schrodinger, "Golly, this is suddenly a breakthrough." Then also very soon came the connection with matrix mechanics.

Kuhn:

Actually I didn't mean those papers. You're involved presumably even before the Schrodinger equation with this problem of a theorem of Lorentz's on wave equations.

Uhlenbeck:

Oh, the wave equations. That was a completely different sort of —. Ehrenfest's great trouble was always to see whether we could get a dissertation together, and he was a man who thought that at least one should try to get a subject which was not, so to speak, in the center of interest, but in which he was interested enough. Already there had existed for awhile his paper with [H.] Bateman ["The Derivation of Electromagnetic Fields from a Basic Wave-Function," Proc. Eat. Acad. Sci., 10 (1924), 369-374]. He was interested in just the mathematical properties of the wave equation, and his friend Herglots had also done some method. He thought it would be very good if we together would try to study all these methods of solutions of the Cauchy problem, essentially, and see how they were related. Purely mathematical.

Kuhn:

That's not like him, is it?

Uhlenbeck:

Yes, that was like him -– the systematic touch he had, especially when there were two methods which gave the same answer; then he wanted to know why. What is the point of the similarity, so to speak. Of course, then he wanted to know what the point was in each method and how it hung together, and that was the way he started it already that summer; then immediately he had to do it with n-dimensions because it was typical of Ehrenfest that he was always interested in the fact of dimensions. His famous paper why does space have three-dimensions; it's a famous paper which made an enormous impression on me. ["In What Way Does It Become Manifest in the Fundamental Laws of Physics that Space Has Three Dimensions?" Proc. Amsterdam Acad., 20 (1917), 200-209].

Kuhn:

When was that done?

Uhlenbeck:

That was around the same time, I'm sure.

Kuhn:

So this whole questions about the difference between odd-dimensional and even-dimensional spaces –

Uhlenbeck:

That was very important for him, you see: why is Huygens' principle not valid in even dimensions? So that's the way we started it, and then out of it really came these two papers. Finally we put it together that it clearly was not going to be my dissertation, after three months. So we wrote it up and out of it we also had to make this Lorentz Featschrift for Lorentz and then this "Stelling" of Lorentz ["Over een Stelling van Lorentz en hear uitbreidung voor meer-dimentionale Ruimten," Physics, 5 (1925), 423-8.] was really an outgrowth of that same study, which I wrote up alone for this. So that was a purely mathematical intermezzo in my work which had nothing to do with Schrodinger. Then in '26 Schrodinger came and we worked very hard at it, very hard. I told you this story about Pauli.

Kuhn:

I don't know that you did.

Uhlenbeck:

Well, that was a very typical story. Since I then learned at that time —

Kuhn:

Oh, the computing story, the integral —. Yes, yes, that story you did tell me. You did a paper with Ehrenfest on de Broglie's phase waves in five-dimensional space. ["Graphische Veranschaulichung der De Brogliesschen Phasenwellen in den funfdimensionalen Welt von O. Klein," Z. fur Phys. 39 (1926), 495-8.]

Uhlenbeck:

Yes, that was because of Oskar Klein. I have, by the way, a manuscript here of a paper by Ehrenfest, Oskar Klein, and myself; it never got published, but I have it here and it might be of interest.

Kuhn:

I would think it would be.

Uhlenbeck:

Oskar Klein came to Leiden on a Lorentz fellowship, I would think, in June or so of that same year.

Kuhn:

'26?

Uhlenbeck:

'26, yes, and stayed about a month together with me. We stayed together in the same apartment. You know surely from him that he came from Ann Arbor and that he had the wave equation, of course, the relativistic form. He had done quite a lot and we talked about it with Ehrenfest every day. Out of that these things came out, you see, and I was very much impressed by Oskar Klein at that time.

Kuhn:

Were you impressed with this approach, the five-dimensional approach?

Uhlenbeck:

Yes, with the generality of it, because it seemed then that one was very close to a world formula — one equation containing everything, you see. I remember that I had the feeling that "Golly, we now perhaps know everything."

Kuhn:

Did Ehrenfest share that?

Uhlenbeck:

No, of course not; it was I in my youthful —. Of course, Ehrenfest was very much interested in these five dimensions because of this view that quantum conditions could somehow be understood as a periodicity condition in the fifth dimension, and that is when we wrote these papers. Then all three of us would write up what we did in these discussions and that's what I have, but it was mainly Klein; that is why Ehrenfest didn't want it and so it was dropped, but it may still have some interest.

Kuhn:

Yes, it would be worth having. If I remember correctly, and I'm not sure on this five-dimensional relativity version that I do, one of the reasons that Klein kept on with it after Schrodinger was the hope that it wouldn't demand the transition to configuration space. Now I may have this wrong.

Uhlenbeck:

No, I don't think so; I think it was mainly the connection with general relativity which he always had in mind. I have forgotten a little bit how it was, but I think it was always this connection with general relativity which Klein is still thinking of. In his last thing he says we have to go back to the absolute space of Newton.

Kuhn:

Oh, does he? That I didn't know.

Uhlenbeck:

Because the vacuum really has everything in it nowadays and that would play the role of the absolute space of Newton; it's one of his last statements.

Kuhn:

I'll be damned; that I didn't know. In Leiden, as you were doing the Schrodinger equation, what about the question of the interpretation of the wave function?

Uhlenbeck:

That also came relatively quickly. That it was probabilistic we knew very soon.

Kuhn:

Well, of course, the Born paper itself comes fairly soon.

Uhlenbeck:

Right. And although Born and Ehrenfest were so antipodal, still that was one of the papers which we discussed very much and which I had to study -– the Born collision paper. I don't think we had very much our own opinion about that; at least I didn't have. I accepted the probabilistic interpretation very soon. It was probably in connection with that — that was in the summertime — that Ehrenfest said what we should now do was to see the consequences of all these things to statistics. And that was what we did. That was then the whole task which came in the fall of '26. It was until '27 that we worked on that.

Kuhn:

When the Bose-Einstein statistics came out, you were still in Rome, weren't you? That was '24, '25.

Uhlenbeck:

Yes, I hardly knew it.

Kuhn:

When you got back, however, clearly Ehrenfest was already deeply involved with that?

Uhlenbeck:

Oh, he would have been since he knew about it; he always had his doubts about it.

Kuhn:

Yes, well that's what I wanted to ask you. I mean, initially that comes out without any benefit of the wave equation, no question of symmetry properties or anything else of the sort, but a new game to be played for possibly non-existent particles. How did he feel about that?

Uhlenbeck:

Oh, he hated all these arguments that this was the right way of counting. There were several of these arguments that because the particular were indistinguishable you had to count it this way. I still remember that he said, "If Boltzmann heard that, he would turn over in his grave." Just because they were identical you had to count like Boltzmann! But then he was, I think, impressed by the pre-quantum mechanics Schrodinger paper nad that is what we studied very hard because he knew, of course, that somehow you have to have the Planck radiation formula and that therefore if you quantize the waves, then the

Bose statistics is the natural way; so we thought for awhile.

Kuhn:

"Natural way" in what sense? That it's the one that gives the right results?

Uhlenbeck:

It gives the right results for Planck, right? And therefore, since that was right, and since matter was also waves, it seemed natural also to quantize.

Kuhn:

Well, to say that matter is also waves is not yet quite clear?

Uhlenbeck:

Well, with Schrodinger and de Broglie and so it was clear. You see, the particle-wave duality was, of course, in everybody's mind, but still everybody was more inclined to say that it was either one or the other. It was clear that if there was a wave motion and a wave equation for an ideal gas, so to speak, then one should use the same way as one does with electromagnetic waves and one then got the Bose statistics formula. That was in Schrodinger's paper. Schrodinger's was a very fine paper, — really, one of the very clear and very good ones. This connection then with the symmetry properties in configuration space was troublesome to us for awhile and I remember that for quite awhile we had some doubts whether there was only one anti-symmetric and one symmetric —. It was all in that summer that we learned that from Dirac and Heisenberg and that we got back to the Fermi paper.

Kuhn:

You hadn't known the Fermi paper?

Uhlenbeck:

I knew it, but it looked strange to us, so to speak; it was clearly —. Furthermore, he did it so strangely at that time because he put — as a vessel he took a harmonic oscillator which was already a bit strange for us to do. But then it was clear how it hung together, that there was an interpretation in μ-space, an interpretation in γ-space, that are all in my dissertation and which I then clearly, a la Ehrenfest, distinguished. That was a very fine time for me, this fall of '26; we worked all the time on these statistical questions. We had the disappointment that the results, at least, which we had, were also published by R. H. Fowler and typically, as Ehrenfest said, he just made it learned and unclear, but he had the same results all right. As a result we didn't publish anything except for the flurry of the interpretation of the Pauli principle, the impenetrability of matter. I think I have the postcard from Ehrenfest about that which was wrong, which was just plain wrong.

Kuhn:

It's the thing that works in one-dimension but not from there. In one of the papers you do do with Ehrenfest, there is a remark that the Pauli principle was "anschaulich" before it came to depend on symmetry properties, but now it's no longer clear why there should be such a thing, which is presumably before this postcard. Now, one of the things I'm curious about — and it's the sort of mental transition that it's hard to pin down or get information about — is the concern of a number of people — I know Heisenberg was concerned for some time -– to understand 'why the Pauli principle'?

Uhlenbeck:

Me, too.

Kuhn:

And clearly the transition to symmetric versus anti-symmetric wave functions was no answer to it; it changed the problem, but it left you with the old questions. By the time I get to learn physics, this is no longer a problem; there is no answer to it but now one speaks of Fermions and Bosons and that's all. How did that transition take place? Where does that vanish as a problem without ever being solved?

Uhlenbeck:

Well, I think it became clear, and surely slowly, that the choice of the statistics, so to speak, could not be decided on the basis of quantum mechanics alone, that it lay outside of it. People felt that they couldn't find an explanation and therefore they gave it up. People slowly gave up trying to find some kind of an explanation although Pauli, of course, kept going at it and his paper on the connection of spin and statistics was one of his answers, which, at least, he thought very highly of. I think it was a very important paper also in the '40's; that was, of course, Solvay Congress of 1940.

Kuhn:

But there's a perfectly real sense in which the spin and statistics connection still doesn't answer the question and makes it more fundamental if you like; it relates things, but really if you asked more physicists today what the answer to this question was, they wouldn't tell you it was unanswered; they'd tell you it wasn't a question.

Uhlenbeck:

Oh, I think it depends whom you ask; I mean, I think that it is certainly a question. I would still call it a problem but it is such a deep one that perhaps only later one can see; it is clearly not that one can do it simply. People very soon gave up and Ehrenfest, too, after the debacle of the impenetrability; this is one of the things, you know, with Ehrenfest. Nowadays and also in the old days people didn't do that; if they made an error they didn't take it back in print; he did that; he wrote a little note and he mulled this through and why and so on. I thought it was very fine that he did that. He put it all out, so to speak; nobody worried about it. Although it went on afterwards for quite awhile. Jaffe worked on it and you still can [see] that it's not wrong, what Mr. Jaffe did; by making the forces very strange, entirely strange, you could still enforce this anti-symmetric business.

Kuhn:

You were really already gone, or about to leave, by the time of the Como Conference and the '27 Solvay Congress, weren't you?

Uhlenbeck:

Yes. By the way, these papers that Lorentz gave me [i.e. Lorentz' calculations on a rotating charge] were finally published; they are in his collected papers. I think he gave them in the Como Conference, or at least a reworked version of it, but he did that in less than a week or two weeks at the time I talked with him — all these calculations, you know.

Kuhn:

I asked you particularly about Como and Solvay because the whole question of the working out of the interpretation and then the relative end of the argument sharply after Solvay is a very interesting sort of point. But really you escaped that entirely. So as far as you were concerned, this was never a great big issue. Did you ever get concerned with measurement problems, for instance?

Uhlenbeck:

I tried to read Bohr and somehow it was clear that that was very profound, but I was not, so to speak, conversant with it at all end I never became very conversant with it at all.

Kuhn:

When did you see the Heisenberg uncertainty principle paper? After you got to this country?

Uhlenbeck:

No, I think I saw that while I was still in Leiden, but I can't be quite sure about it.

Kuhn:

You might well have seen a manuscript or a proof.

Uhlenbeck:

I don't think that it made an enormous impression on me somehow; it looked very fine but I was at that time quite pragmatic about things, you see, and also for my personal work. After coming to Ann Arbor, it appeared very clearly that I knew very little; I knew this quantum mechanics a bit and I had dutifully followed lectures, but then suddenly I had not only to give lectures but suddenly I also had Ph.D. students and that worried me sick! I didn't know what to do with them. So it was clear that at that time I went through a minimum and I had to learn all kinds of things; I was therefore mainly interested in getting certain problems which I could do with the student preferably, and there, of course, I finally got out of it all right with the help of Fermi, who came for the summer schools there and with whom I worked very hard. Then I got positron calculations; I learned Dirac theory only at Ann Arbor, I learned the Dirac paper. I knew it —.

Kuhn:

Which of the Dirac papers?

Uhlenbeck:

The electron paper. It came out in '28 and I saw it, of course, but I couldn't understand it. I didn't know what to do with it, but then finally through Fermi and the positrons and so on, I got familiar with it and I made long calculations with it, trying to check every Oppenheimer calculation about it because he always published it with only the result which was very nice, if you don't like problems. Once I worked a whole summer with Fermi on these things, and then I had students —.

Kuhn:

Now, when you say 'the positron', you mean the hole theory before the positron really?

Uhlenbeck:

Well, they came almost very soon together; there was about six months, maybe, in between.

Kuhn:

Oh, no. More than that. That is, the equation itself is ‘28 and the hole theory is ‘30 and the positron isn’t until late ‘32.

Uhlenbeck:

Ja, maybe so; I don’t have the dates in my head.

Kuhn:

But there is a lot of intervening work, the Klein paradox and so on.

Uhlenbeck:

I learned that then immediately afterwards because there was even a Physical Society meeting in Boston in which there was a series of invited papers on the positron theory. I was one of the speakers; I gave the general introduction and then came Anderson and then came Oppenheimer. But then I was more or less conversant with it.

Kuhn:

That would have been ‘32 or ‘33.

Uhlenbeck:

It was at that time that I worked very hard on these things; and then I was completely, so to say, on my own feet; then I knew what to do.

Kuhn:

Were you involved with that, though, enough earlier to have been bothered by the hole theory?

Uhlenbeck:

Well, I knew that there were difficulties but I didn’t actively take part in it at all. I’m sure that Robert [Oppenheimer] talked with me about it because he was then, of course, in the midst of it, but I had no real opinions about it. That was this little note with Fermi; we wrote a note to it. This summer with Fermi was a wonderful experience because he was so concrete, you see; he has one of the most concrete minds I know. We did all these calculations on the blackboard and then made notes and then it was very clear that to explain something all these approximations were, so to speak, uncertain, and that we should do the solution of the Dirac equations for a potential which was not Coulombian but was, so to speak, the statistical potential. Well you can't do it, of course; I said, "Fermi, you can't do it." He said, "What do you mean you can't do it? You do it numerically." I said, "numerically!" He said, "yeah," he thought a little bit, and said, "It will take about a week for one wave function." And we worked every afternoon — he punched and I wrote down the numbers and really in a week we had the curve and we knew how far therefore, for this wave function, it compared with all these approximations which one turned out; these approximations were quite bad, you see.

We left it at that; it was at the end of the summer that we did that. But that one could push it that way was again a revelation for me — that you could simply sit down and compute. And he was so fast, you see — boy, was he fast! -- he was really astonishing. I'm sure that because of that I really read immediately and in great detail his beta radioactivity papers which I worked through backwards and forwards and then I went on with it with [E. J.] Konopinski; we did the derivatives which were all wrong, but it was still a very great excitement for the moment. Yes, those were really the main influences for me, I think. Ehrenfest in the beginning, then I had to learn how to compute, which took me a couple of years. Thank God for the Brownian motion; it was for me very fine that there was such a field that people didn't work at but in which there were still many things to do, so I wrote then these papers on Brownian motion. I still remember that Pauli said, "Brownian motion – Desperazions Physik," he said, "Desperazions Physik." But that didn't bother me.

Kuhn:

Why did you do that? Because it offered fascinating puzzles still?

Uhlenbeck:

There were definite questions, there were various questions, you see, which were not answered, and it came also partially through the work in Michigan on the noise questions, Johnson noise. They made very fine experiments there and I was then clearly the man, since it was statistics and I was supposed to be the theoretical adviser about it. So I studied all that very carefully and that brought me to the Brownian motion. I got a dissertation out of it for someone and then when I went back to Holland — the second time, I think, or the first time — I worked with Ornstein on it where I did most of the work, but it was the methods which he had invented, all right. At that time, of course, nobody looked at that paper really; it only became known in the war when all these noise questions came up again.

Kuhn:

Tell me about your impression of physics in this country when you got here. What was the transition from Europe to Ann Arbor like for you?

Uhlenbeck:

Well, we were on the outskirts, we were in the provinces, that was very clear. In a respect it was very nice because you were not in a center -– nowhere in America, not only in Ann Arbor. The only feeling of being in the center again was during the summer schools because of all the people who came.

Kuhn:

Did you two start the summer school?

Uhlenbeck:

No, no. Randall started it and there also was a summer school before we came, but as soon as we came, then it became really quite a center of activity of the department. Of course, Ehrenfest came, and Fermi, Kramers, Pauli, Sommerfeld, Dirac, and everybody came; then, of course, the summers were extremely busy and you worked very hard because you went to the lectures, too, but at the same time, you had to give lectures because that was, so to speak, the way it was set up. Then, of course, during the fall you started to digest what happened during the summer with all these things: Quantum Electrodynamics of Fermi which I edited completely because he sent me the notes and I wrote part of it, correcting his English and so on because his English then was not very good. That was this Reviews of Modern Physics paper which then I learned of course very well. So the center was still Europe. That's also why Randall said we had to go to Europe every two years and he convinced the administration of that: that's what I also did. Then really the shift came when I went back to Europe because all the people like Bethe and so on came slowly to this country and, of course, nuclear physics started. I still remember that at one of the last Physical Society meetings before I went back to Utrecht, they asked me to give them an after-dinner speech. Then the main point I tried to make there was just this change, because I said when I was in Leiden up to '27 the Physical Review was one of the funny journals just like the Japanese, where you looked at it once in awhile but you never really considered it very much; then in '35 when I left it was quite different and it was then already one of the central journals. That was an example of how things had changed; just in these eight years really the whole shift came.

Kuhn:

Rabi made an interesting and, I think, controversial point yester in discussing this rapid development of American physics. He said he felt that in spite of the great contributions made by the refugee and the emigrant physicists that actually they scarcely played an important role until the war and after the war because they were in many ways still isolated enough and unaccustomed enough to the situation here: And it's his impression really that the first great impact was made by the Americans who, like himself, had spent a year or so abroad, had come back, and been able to teach this very large number of students who were here; so he would say that there was a really major affect from his own experience, Condon's, Oppenheimer's, and so on.

Uhlenbeck:

I think Oppenheimer's school was very, very decisive because Oppenheimer in those days, as soon as he came back, I think, in '28 or so, had an enormous number of students and also [post-graduate] people, research fellows and so on, and he had an enormous activity. There then, at Berkeley, was one of the great centers — [W. H.] Furry was there, and [A. T.] Norsieck and [Willis E.] Lamb — all these guys came from Oppenheimer so Oppenheimer was able to bring what he had learned, to bring this excitement, over there, and I think what Rabi says is true. It was only much later that, so to speak, the real refugees – we were not refugees —, but people like Bethe and so on, really took part. Rabi himself then, I think, started a school at Columbia which also had a large influence. He really discovered Schwinger. Did I tell you? I always had to tell the people at Columbia that Schwinger was a great man. I was always the one Rabi asked, you see, and I still remember in '35 — I think it was before I went back to Holland or maybe earlier — oh, no, about '36 when I went to the summer school in Ann Arbor – he wanted to say, "Well, you have to talk to Schwinger; here's a young man and he says that the Bethe-Heitler formula is not correct and you'd better talk to him about it." And I remember that I said, "Fine", and Schwinger came and he was such a young man; this may be wrong, but it looked to me as if he were still in knickers! He was a very young fellow and he began to talk about this Bethe-Heitler calculation which I had also been through very carefully because that was one of the things I had to do when I worked with Enrico [Fermi]. I think I convinced him that it was not wrong and that there was on some point in his argument which I didn't quite understand but which I thought could not be right.

The only remarkable thing was that after ten minutes, we talked as complete equals; I mean, he knew clearly just as much as I did about it and it was very, very nice. And at the end I said to Rabi, "Well, you'd better take him; he is really one of the very good men." Then I was, of course, again in Columbia in '38 and Schwinger was again in trouble; he could get his Ph.D. because he didn't go to lectures of the mathematicians and he didn't have enough credits. So Rabi had told Schwinger that he had to go to my lectures at Columbia; of course, he didn't because it was early in the morning, and I asked Rabi, "What shall I do?" because I was, of course, perfectly willing to give him an "A" on that because he needed the credits. Rabi said, "No, that you shouldn't do; you give him an exam and you make it a tough one." So I did, we made an appointment, and, of course, he knew everything; he had somehow gotten the notes! I also cleaned up a couple of derivations which I had done a little bit sloppily and which he had done much better and again it was so that he knew everything, so I said, "Fine, I will tell Mr. Rabi." I could now with good conscience give him an "A" and that helped him in getting his degree.

Kuhn:

What were the lectures on?

Uhlenbeck:

Statistical mechanics, which was not his field. And then he did the deuteron work and on that he got a dissertation; I think they by-passed the mathematicians because he had gotten the credits partially from me. So I was always involved in saving Schwinger somehow; even during the war I was involved in it because then he was at the radiation laboratory. He was at Chicago with the atomic energy business but he didn't like that, so, typically Julian, he just hopped in a car and went to Boston. He didn't tell anybody -– he just went! He came to Rabi, who was then at the radiation lab. And Rabi said, "Well, all right, of course, you can work here." And that he liked; he was in my group and he did all these mathematical problems on wave guides which was very good, of course.

Kuhn:

Did he ever do the computations on those?

Uhlenbeck:

Oh, yes. He was a real computer, really remarkable. He was mathematically and technically really remarkably good.

Kuhn:

I meant did he do the actual numerical work?

Uhlenbeck:

As far as it was necessary, yes; the experimentalist finally only came to him in the evening at about 4 o'clock. I finally got him to give a seminar on it at 4:30. Julian was always out of breath when he came in, but then I had a certain influence on him so he did it and very conscientiously. But then the people in Chicago got mad that he had left and through channels it was told that Schwinger should be sent back to Chicago, that he was needed there, and that especially I should be reprimanded for having taken him away. So I was brought to Lee DuBridge and he said, "Well, they tell me you have seduced Schwinger into working at the radiation lab." I said, "Nothing of the kind! He just came. I had nothing to do with it: he just wants to do it." "Well," he said, "people are very mad and you've got to go to Chicago and put the thing right." So I had to go and spend a day or so in Chicago talking to Eugene Wigner and all those guys; I just told them how it happened and, thank God, they weren't mad anymore and dropped the thing. I was complimented by Lee DuBridge for my great diplomatic gifts in dealing with Schwinger who didn't even know about it; he was just sitting there! So I had to save him from going to atomic energy work which he just somehow didn't like.