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Samuel A. Goudsmit – Session I

December 5, 1963

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 circa 100 atomic and quantum physicists. Subjects discuss their family backgrounds, how they became interested in physics, their educations, people who influenced them, their careers including social influences on the conditions of research, and the state of atomic, nuclear, and quantum physics during the period in which they worked. Discussions of scientific matters relate to work that was done between approximately 1900 and 1930, with an emphasis on the discovery and interpretations of quantum mechanics in the 1920s. Also prominently mentioned are: Gladys Anslow, Robert Fox Bacher, Ernst Back, P. A. Boeser, Niels Henrik David Bohr, Hendrik Brugt Gerhard Casimir, Walter Colby, Dirk Coster, G. H. Dieke, Paul Ehrenfest, Albert Einstein, Enrico Fermi, George Hartwig de Hass, Werner Heisenberg, David Inglis, Edwin Crawford Kemble, Ivan Robert King, Oskar Benjamin Klein, Ralph de Laer Kronig, Alfred Landé, Otto Laporte, T. van Lohuizen, Hendrik Antoon Lorentz, Fraulein Mensing, Edgar Meyer, Robert Andrews Millikan, J. Robert Oppenheimer, Friedrich Paschen, Wolfgang Pauli, Linus Pauling, Isidor Isaac Rabi, Harrison McAllister Randall, Adolf Smekal, Arnold Sommerfeld, Thomas, Uhlenbeck (George's father), George Eugène Uhlenbeck, Albrecht Unsöld, W. van der Woude, Vry, John Wulff, Pieter Zeeman; Universiteit van Amsterdam, Rijksuniversiteit te Leiden, University of Michigan, Teyler's Museum, Universität Tübingen, and Universität Zurich.

Usage Information and Disclaimer:

Kuhn:

Tell me about your home. Pretend for the moment that we were doing a total biography.

Goudsmit:

From cradle to the present. The relevant thing is still that I want you to know something about the educational habits in Europe.

Kuhn:

But that's too late to start.

Goudsmit:

That's not late; that is very important, because of my family background. It is the wrong idea in America that in Europe very few people go to high school and to the university, that only the rich ones go. That is wrong. It's quite true that only few people go, but it has nothing to do with wealth: it has to do with family habits and tradition. An underpaid schoolteacher will send his children to high school and to the university, whereas in my family where there were rich merchants, I was the only one of all my uncles, aunts, cousins, in a large, large tribe, who finished high school. And I was considered a failure, because I wanted to study. My parents were not rich, but that had nothing to do with it. My cousins were much richer, and they thought it was a grave mistake.

In fact, my mother, as my sister told me only recently, wanted to take me out of high school. And my sister was taken out of high school, did not finish, and had to become an apprentice in a business somewhere. She refused, and she got a clerical job then. But in my family it was customary to get a much more expensive education than studying, namely, to be sent to some foreign country to learn the business. One cousin went to America, several went to France, some of them stayed in the Netherlands. One of my closest friends never went to high school, but went to England, and is a rich businessman in England now.

Kuhn:

What was the business?

Goudsmit:

In my mother's branch of the family, they were all in ladies' hats, in millinery. That was the way these families ran: it had. nothing to do with financial circumstances. I am sure that George Uhlenbeck's family — I visited his home — I don't want to judge it, but it can be on the tape — probably was not half so wealthy as some of my uncles. But I'm sure that his family didn't think of not sending all the children to high school, to the Gymnasium, or whatever it might have been.

Kuhn:

What was the difference in the two cases?

Goudsmit:

The difference was simply that his family had always been an educated university family. His uncle, I should know, his favorite uncle, was a famous linguist, and my favorite uncle was a wholesaler in ladies' hats and things like that. That was the difference. And they really thought that I was going to be a failure, that I'd always be a poor high school teacher instead of a successful businessman. So therefore that is important. Especially on my mother's side that attitude was very strong. My Fathers family was poorer than my mother's. My father was a little bit more interested in education. He had gone to trade school for a couple of years where he learned physics and algebra and geometry. Then be became a carpenter; that was his first job. Inter he worked in a wholesale business of hand-made furniture, which he later took over himself.

There was an evolution there, and be became what is called here a 'jobber', an agent for factories to the wholesalers for bathroom fixtures and primarily toilet seats. But it started out as hand-made, mahogany toilet seats which were made in his shop, and things like that: a most uninteresting business. My mother's business was far more interesting because you needed psycbolo, and I was always in the shop. My father and mother went twice a year to Paris to buy the new models; not what they liked, but they had to guess what the tcb women would buy. When the new models came in, I was in the shop, looked at them, made remarks as a child, and said, "Take that feather off or change it, because she won't ever sell it here," and the personnel was always upset because my mother listened to me. I used to draw sketches when I saw interesting hats in a cafe — that was when I was 10, 11 or 12 years old — and gave them to my mother and said, "This looks like a nice hat."

Kuhn:

This was a manufacturing and sales business? My mother's was purely retail sales. She had a shop, and so had all my aunts and uncles, but some of them were in the wholesale business. Their agents were Parisian houses for the ribbons and the feathers — I don't know what you call the details. How, they had another effect, namely,that I didn't even belong in university surroundings.

The schools in Holland don't educate, they teach the education you get at home. These fellows who come over here, especially the refugees, were always considered so much better educated than the American equivalents. People thought it was the schools. That is not so. The education was in the borne. In a home like Uhlenbeck's, there were always books around. In my home, there was very little of that. My parents had subscribed to a kind of a book club for a number of years. Being a traveling salesman for his business, my father read a lot on the train, and I was the other one in the family who did the reading, and nobody else did.

Kuhn:

What sort of things did you read?

Goudsmit:

Oh, I don't know. I remember having read in translation nerson, Poe, - mostly essays, I didn't read fiction in those days except the Jules Verne type of fiction, that I liked very much, and detective stories. So I was totally uneducated compared to the other people.

Kuhn:

When you say they taught in school, and educated in the home —

Goudsmit:

They trained in school.

Kuhn:

Techniques?

Goudsmit:

Techniques, ja. But the real education of these people whom you know, like Uhlenbeck, really came from the home. His father, when he retired from the Dutch East Indies, where he was a high official, worked for a great publisher, Nijhoff. That is quite different from toilet seats. It was not much education. It was a good business, until, as n Lang said to me, the bottom fell out. [Laughter] So you see, this is important. I was an exception. I was sent to the university mainly because my father always wanted to study a little more; he offered his support, over the objections of my mother, who went to high school to ask whether it really wasn't a mistake, and I heard from my sister that the high school principal said, "No, that boy should study.

It will be good for him; it would be a shame if he didn't continue." My father taught me a little algebra, a little geometry, by rote, already when I was very small. There were certain formulas he had memorized, and when Sunday morning as a small child I played in his bedroom in bed with my parents, he would teach me things like ?r2, and. the formula for the triangle, the square root of s times s-a, times s-b, times s-c. I learned that before I knew what the symbols meant. As a result, already in elementary school I was ahead in arithmetic. I did all the problems a little different from the rest of the class. That was my father's influence.

Kuhn:

I'm particularly curious about what you say now about the family background, because my impression is that in many Jewish communities, one would have a direction to learning.

Goudsmit:

No. Not in Holland, because there was no discrimination. Don't forget. In the other countries where there was discrimination, the Jews had to become doctors or lawyers, because those were the only free professions; they couldn't get jobs. In Holland, the percentage of Jews in physics, in medecine, or in law was a little larger than the average, maybe a factor of two, but that was mainly because they lived in cities, it was more a difference between urban and rural —. That was the main difference. And so that's why it was not so. And I never was aware that most of the fellows with whom I talked were Jewish, on the contrary.

Kuhn:

Were they mostly Jewish in fact?

Goudsmit:

No, no. About the percentage as the Jewish population in Holland 10%. Most of the fellows I worked with were not. And of course, in family relations, intermarriages were not very common in Holland. In social relations, you stuck to Jews a little more than otherwise.

Kuhn:

But only a little.

Goudsmit:

But only a little.

Kuhn:

Was your family religious?

Goudsmit:

My parents were not. My grandparents were, of course. They stuck to the old rules, and I had one uncle who was a rabbi, and one uncle who was an official of the Jewish community on my father's side; on my mother's side, none of them were religious. But on my father's side, a few were. My father himself was extremely liberal. But I was Bar Mitzvah, but only in a small way. So religion did not play an important role; and being Jewish also very little, So I went to the university with this handicap, being the only one and considered a failure. And it was indeed true that in Holland, what was the outlook? I would become a high school teacher.

Kuhn:

There was just nothing else.

Goudsmit:

Nothing else. All high school teachers were Ph.D.'s. And it so happened that the two teachers I learned physics from were both pupils of Zeeman. The first one was van Lohuizen, and- the other one, (Hallo), was the principal of the second high school I went to; be also was a Zeeman pupil; purely accidental, I suppose. This had really nothing to do with interest in that at first, except that when you have good high school teachers they encourage you. What happened to me is really this: first of all my father's influence; then, my sister, who is older than I am she went to high school first.

Kuhn:

Was there just the one sister?

Goudsmit:

Ja. She lives in the Bronx now.

Kuhn:

So there were really just two of you.

Goudsmit:

Just two of us. But lots of cousins and family relations. Most of my mother's family lived in Amsterdam, my father's family in The Hague. We lived in The Hague. The thing which first struck me as physics and I always remember it I think I remember it, you know bow that is, when the hypnotist comes, you probably will find out it isn't so is my sister's high school book which I read. And there I right away picked out a part of physics which fascinated me; and it was spectra; that you could tell, by means of spectra, what the stars were made of. That I found so fascinating; it was incredible.

I also read the other parts of the book, the mechanics part, and I thought it was the dullest there was and I couldn't understand it. I tried in vain to understand how an airplane stayed up in the air. I still don't quite understand it. But it was in that book. It was a book by (Boumann) one of the old physics books. A Dutch book. I still remember going to the airfields the first demonstrations must have been around 1912 or something like that in the Netherlands — knowing the names and telling my mother this is called the stabilizers, and that's called so-and-so. She was greatly in— pressed. I got it out of that physics book. But I did not understand these vectors. But spectra, that fascinated me. So I got out of the public library, lending libraries, other books on physics and astronomy at the time, trying to read. I don't remember anymore what I learned.

There was only one thing which also shook me: that was the passage of Venus before the sun; that that had happened twice, and that it would not happen again until 2002. And it suddenly dawned upon me, there was something I would never see. That made a deep impression on me. It really made me feel sad. I still remember reading that, "I'll never see that." So that was the background, bow I got interested in physics. Then, when I went to high school, I read a little more

Kuhn:

You went actually to two different high schools.

Goudsmit:

Ja. The reason is again I don't think it exists anymore — that it was possible in Holland to go to a kind, of junior high school where the whole curriculum was covered in three years. In the second year you had to decide whether you really wanted to finish or to take a complete high school. It was just one of these safeguards. If I had been a failure, my mother would say, "O.K., now you come into business, you have your diploma."

Kuhn:

You already not have gone even to this junior high school?

Goudsmit:

I might not have gone to the junior high school at all. I might just have taken some evening lessons in French or so, because, for my mother's business, you had to know

French, things like that, and bookkeeping. They learned a few things, but just what was needed for the business in my family. And all the acquaintances and all the friends I knew were that way.

Kuhn:

So it was really reading, writing, arithmetic, and beginning languages.

Goudsmit:

And beginning — or whatever was needed for the business, bookkeeping and so on. No education whatsoever. There was some, let me see. My mother used to know opera singers, and so we bad musical evenings at borne quite often. People played the piano, or brought a violin and a cello, and had a little trio. For several years we had it every Friday night. It was not very highbrow; a little Beethoven, Brahms, and so on. But I was exposed to it.

Kuhn:

Did you play yourself?

Goudsmit:

I did not play myself. My sister learned to play the piano, and I was so upset by all the tortures she went through, and I am not musical. I learned to play myself, with two fingers, tunes, but never was musical. Then my sister at one time had a book reading club when she was in high school. I'm sure that the main reason was to get some boys over. I was exposed to that a little bit, I used to listen in, and they read a few highbrow books in Dutch here and there. She liked the boys and I liked some of the girls who used to come there very much, but I was still too small then.

Kuhn:

How much younger than she were you?

Goudsmit:

Three years and a half. So that was my education: the book reading club of my sister when she was still in high school before my mother took her out and the musicales which we had every Friday night for several years, which was very nice, singing and so forth, but on a fairly low-power level. But at least it wasn't all one negative —. They were not anti-intellectual, but business came first. Then in high school, that I was interested in physics helped, because that is true, if you have good teachers, they encourage you. When I showed a little more interest than the average, then a man like van Lohuizen helped me. He gave me books, told me what books to read out of the library.

Kuhn:

Was this then in the second high school?

Goudsmit:

It was in the first high school. Then, fortunately, the second high school I went to, after I decided that I will go on, van Lohuizen taught there too. You see, I chose that second high school because he was there also the physics teacher, as well as the other man; there were two physics teachers there. He helped me, and was interested. We became friends, I went to his home. But my interest in spectroscopy was really, earlier than that, before I went to high school.

Kuhn:

But he did encourage that.

Goudsmit:

Ja, he then encouraged it very much. But he didn't know at the beginning that I was interested in that, because I tried to understand relativity. I wrote a little essay on relativity which worry that the Nazis got it completely wrong in my high school days — trying to understand what it was about it was special relativity still in those days, 1916,1917 or so. I went to art school also, because I began to draw these hats. They thought I might have a little bit of talent, and ray father always did a little drawing. He learned that too, mechanical drawing as well as hand drawing, for his business. He said, "Well, we'll send you to art school."

So I went for a couple of years, two afternoons a week, to art school. I enjoyed it very much, but it turned out, as I bad suspected, I have no talent. I learned some tricks, and so in school I used to make the best drawing, but it was the tricks again which I learned. I knew exactly with pencil and paper to draw the same thing, and one is glass and the other is wood, and I know the tricks to make the one look like glass and the other like wood. When we had free drawings in school, you had to draw something at home. And always I did something like a little silver vase and a glass one. That I used as my sketch, and I impressed the teacher that you could tell the difference; and mine were always exhibited. They never looked like the object, but it made no difference, you see. I was very good at fooling teachers that way, impressing them with a little outside knowledge which really was irrelevant.

Kuhn:

But where did you learn these tricks?

Goudsmit:

Mainly from my father; but also in the art school. You see, that was the thing which 'took' with me. I did some fairly nice sketching at times in pastel, still lifes and pastel, and I used to enjoy it, but I haven't done it since, except when I was sick. Then I was on Cape Cod, and I sketched a little bit; in fact, my daughter Esther was so surprised, she never knew I could do it. But that was the only time. This was the educational background. Then at the university

Kuhn:

I'd like to know a little bit more in educational terms, really sort of formally, what you actually knew, had learned, in the sciences, but not just in the sciences, by the time you got to the university.

Goudsmit:

There is still one other important fact. From high school, you could not go to the university. It wasn't even sure that I could go. The only thing which was open in those years was engineering, the engineering school. But just when I was in high school they changed the law. George Uhlenbeck was older than I was, and he missed out on that, and lost one or two years on that account, and went to the engineering school first, as you know, and then after the law was passed, was able to transfer to Leiden.

Kuhn:

What would you have needed to have? Was it a different school?

Goudsmit:

A different school, because they required in addition to the three foreign langages, also Latin and Greek. And that was only done in the Gymnasium, which was one year longer than —.

Kuhn:

So high school was Realschule?

Goudsmit:

Ja. So that was changed, it was possible to enter the university.

Kuhn:

What determined which of those two you selected? I'm interested —- in your case it may be fairly clear; in George's case, with a background of learning in the family, one would rather have expected that he'd have gone to Gymnasium.

Goudsmit:

Ja, I really don't know, you'd better ask him. I'm surprised about that. Sometimes it was too expensive maybe, but I doubt it very much that that had anything to do with it, because tuition is low. Maybe he didn't like languages, thought that he might not get along with Latin and Greek, and things like that. That was my trouble. One of the things, of course, which did stand out when I went to high school was that there were very few things that I could do well. Don't forget that the schools, especially in Holland, are quite different from the schools here. You learn several subjects, there are no electives, and they are tough. All "A" students don't exist.

So you never get the impression that you are good at everything. And in my case, and that's the case with many students, you drop out of more and more subjects — you just get a passing mark -_ languages, history, and all the humanities were in that class. I had great trouble with them. In fact, I had to take special lessons in English because I almost failed and, in fact, my final exam I just passed. Whereas the sciences came out all right; but with the years, when they got more and more difficult, even that reduced: biology dropped out; then chemistry dropped out. That means I did not get an NA", I got something like a "B-". So there was really nothing left over but physics and mathematics. Then when I came to the university, mathematics was my failure, and physics was left over. And if it hadn't been for Ehrenfest, I might not have succeeded in that.

So, you see, it was fairly easy to decide what you wanted to do. For the American students it's much harder, because they get all "A's" and they really believe that they are good in it, which is, of course, not true. And so that made it very much simpler. But then I had another piece of good luck. I had a friend I think it was a friend of my sister's, I'm pretty sure of that whose father was a teacher, and whose brother was a musician, who was slated to go on to the university and high school, and he was two years older than I was. He influenced me quite a bit. He was a mathematician.

Kuhn:

What was his name?

Goudsmit:

(Van Rees.) But there was one thing which disappointed me in him: he did not have research ambition. George Uhlenbeck knows him, and be was known at Leiden among the students as one of the best mathematicians, but his aim was to be a high school teacher. And indeed, when be finished, be became a high school teacher, as is customary, first in a small village in the Netherlands. When he turned out to be a good teacher, then several years later be got & job as high school teacher in Rotterdam. When that job was open, that was the job I think for which there were some overtures made, that was the job I'd get as soon as I finished, the one in the little village. That was really the horizon: I didn't know any better. But be influenced me quite a lot. He was a little bit ahead of me, he was a student a year ahead, be told me about Leiden and the people, and the studying and the pleasure of it. He had a great influence on me. I saw him very, very often.

Kuhn:

Even when he was at Leiden and you were still at The Hague?

Goudsmit:

And I was still in high school. Because he was engaged to a girl who was a friend of the girl I was engaged to at the same time, so we often went out together, the four of us.

Kuhn:

You were engaged already in high school?

Goudsmit:

Oh, gosh, I was really — I don't know what you call engaged, maybe even earlier than that. Against my parents' wishes and so — that's another story, it has nothing to do with physics. It has something to do with it, but not too much. But with all these stories and hindsight you can tell them so much more rationally than they really happened. But anyway, we were very close together, and he had a great influence on me. By the way, he was offered professorships later on, and didn't take them. Even after the war be was asked to teach at a new university in Eindhoven. He still is a high school teacher in Rotterdam. He's probably very, very happy.

Kuhn:

Does he do any research? Does he publish papers?

Goudsmit:

No, no more, But he could. He had been in those earlier years an excellent teacher. But even as a teacher he did not want to join that new university. So in that we differed radically. But he had a very great influence in the early days. I came often to his home, and he came often to my home, we went out together every day.

Kuhn:

You found the idea of teaching fairly repulsive?

Goudsmit:

No, no, not at all. My ideal was my high school teacher, I thought it was a nice life, I admired him very much. I still like teaching.

Kuhn:

That's what I was going to ask, because you do it well, and you do it with apparent pleasure.

Goudsmit:

Ja.

Kuhn:

Well, then, why do you say this was just the opposite from you?

Goudsmit:

Oh no, at that time it probably was not. You are quite right. Because as I said, my aim was to follow in his footsteps. When he left the little village, I get that job in the little village as high school teacher. Then of course things changed, and I became more ambitious and wanted to stay in research. But he had a great influence on me in the early years, and also during the early years of my studies.

Kuhn:

Now let's see if we can straighten this out a little bit more, because when you talked about the only thing one could see to do was teach high school, and indicated that your parents thought this was failure, did you to some extent also feel that?

Goudsmit:

No, I thought it was great. I thought it was a very respectable, well-paid job. I admired my- teachers.

Kuhn:

Did you have any interest in going into the business?

Goudsmit:

I had more interest in my mother's business than in my father's business; but my mother gave up the business before I finished high school, so that opportunity wasn't there anymore. My mother's business was more romantic.

Kuhn:

There was a time when you think you wanted to go into it, or thought you would go into it, saw this as a —?

Goudsmit:

The remarkable thing is that I never really thought. I always found that circumstances pushed me in one direction or another. I never made any decisions myself. At least that was the feeling I had in those days. The things I wanted to do, I knew, but I didn't know whether they were going to be hobbies, in my high school days. I wanted to solve mysteries, and there were three professions where one could solve mysteries: the police, archaeolo, or science. I was aware of all three of them. What they have in common is just solving mysteries, nothing else.

Kuhn:

Where did that interest in solving mysteries start?

Goudsmit:

That I don't know. That must be — I don't know where that is.

Kuhn:

Do you read a lot of detective stories?

Goudsmit:

Ho, no more. I never read many- of them, just a few selected ones. But that was really a driving force: anything which was mysterious, which I couldn't see, I didn't like, I wanted to solve it. Also travel I always hoped to do a lot: to see China, and see this and that. Of course, that never materialized. But it was part of it that nothing should remain a mystery for me. That was a very important driving force as a young man. That I chose physics was simply that I failed in the other. I kept the other things as a hobby, at least two of them: detective work –

Kuhn:

What do you mean when you say you failed in detective work?

Goudsmit:

There were aspects of it which I didn't like: that you could not specialize only in documents, you bad to see murderers and things like that which didn't appeal to me; a lot of drudgery. And there was no formal way of doing it, you had to start at the bottom, you had to join the police force or something like that. But I took a course in Amsterdam in scientific crime detection, and even helped the man in his laboratory a couple of times, setting up spectroscopic apparatus.

Kuhn:

What about archaeology?

Goudsmit:

It is Egyptology, of course, that it became. I read about it. You know the well-known story of how I studied it by mistake.

Kuhn:

I don't think I know that.

Goudsmit:

When I was a student at Leiden, we had that club, Huygens, where you had to give lectures. I always talked about spectra lines, because it was all I knew about, and they began to make fun of me. So when it was my turn again, I had to select another subject. But I didn't dare to talk about anything else, because I was the dumbest in the group; all the other fellows knew everything. So I announced a lecture on mathematics of the ancient Egyptians in the hope that nobody would knew anything about it, and that I could read up on it. I had read about it. When the time came I got scared, because there was Nijhoff, the son of the publisher, I'm sure he knew it all.

So I went to an Egyptian museum, and. asked the old professor whether be couldn't help me with that subject, and he said sure, and he gave me some books. He said, if you are interested, why don't you come to my lecture sometime, and I promised to do that.

You know, you have no registration in Europe, lectures are free and open . . . . I gave that lecture by the way, it was probably one of the lousiest lectures I ever gave, on mathematics of the ancient Egyptians. But during the summer I got a postcard from the man saying, “My lectures start October so—and—so.”

So I think, “I’ll be polite, I’ll return the books, and sit in his lecture.” So I go to the museum and ask the doorman where this professor Boeser lectured. He said, over there. And I open the door; it’s his office, I am the only student. And so for about two years I had every week a private lesson in ancient Egyptian hieroglyphics. He always started with a Latin proverb; he always translated it for me because he knew I came from high school and therefore didn’t know any Latin. Three make a lecture, God, the teacher, and the student. So that I couldn’t walk out, you see. And after a while it became fun, but I always remained an amateur. He finally let me go when I told him I wanted to buy some of those things and collect: that was below his dignity. But I still found a postcard from him wishing me good luck in America.

Kuhn:

That’s fascinating. By the time you finished high school, how far had you gotten in the various sciences and in mathematics?

Goudsmit:

In mathematics, just algebra, trigonometry, -—

Kuhn:

No calculus at all?

Goudsmit:

No calculus at all. A little probability.

Kuhn:

That’s pretty much the American curriculum. Is it usual not to have had any calculus by the time you get through high school?’

Goudsmit:

Ja, ja. Not at all. It’s probably a little stiffer. We knew trigonometry, we really knew how to solve all those problems. The only thing Ihave to say in favor of the Dutch system is the units. We get probably the same number of hours, but we get it fewer tlines per week, and spread out over several years. The result is you cannot afford to forget it. Here the students learn French, and they get two semesters of French all condensed in one year, and they never bear the language again, and, of course, they don't know it.

Whereas we get it one or maybe two hours a week, and spread that out over so many years that you do not have a chance to forget it. We have every week about 16 to 18 different subjects, whereas here you have every day. Here the unit is the day, you get five subjects, every day is exactly the same; in the Dutch schools, every week is exactly the same. I have at home some of these curricula for the schools, which are interesting to study. But the trouble with that system is then you cannot have electives.

It must be rather rigid, otherwise you can't do that. But I think it could be done here too, with at least part of the curriculum, and that's enough to give an improvement in the retention. It makes a lot of difference whether you get French one hour a week over seven years instead of seven hours a week over one year, a lot of difference So that's what I knew, and it wasn't very much.

Kuhn:

Well, now physics.

Goudsmit:

In physics — just elementary physics; no calculus involved. But you learned it well, problems you had to solve in high school. And so it was rather fragmentary.

Kuhn:

Chemistry, astronomy?

Goudsmit:

Chemistry, a little astronomy, a little mechanics, several hours of mechanics. Mechanics was a separate subject in our school, so-called theoretical mechanics or whatever it was called. But a little stiffer, a little better, it was not superficial. It was rather dull, let me say it that way. It didn't try to make it nice by bringing in locomotives and airplanes. Even the airplane book was vectors'. Nothing else like that. It wasn't very appetizing, but it came natural to me, and I was lazy. So I got good grades for those subjects which came naturally. I remember that I had been sick, I had the flu or something; I was out of school for a while. When I came back we had a physics exam on subjects which I had not studied with the class; nevertheless I got an "A" or the equivalent, because these things had come naturally to me.

Kuhn:

Meanwhile, what were you doing with your spectroscopy?

Goudsmit:

Reading about it, but not understanding it.

Kuhn:

What were you reading?

Goudsmit:

I was reading the popular books on astrophysics. In those days it was Flammarion who wrote popular books on astronomy and astrophysics, the creation of the universe and creation of the solar system. It was very superficial, and I have no recollection of what I really learned outside.

Kuhn:

Were you doing anything with van lohuizen about it?

Goudsmit:

No, no, he did nothing else but encourage it. The main thing he probably did for me is take me to Ehrenfest at one time, but that was after high school. You see, he still did some research, and when I was a student at Leiden he tried to tell me a little about what the kind of research he was doing which I did not understand at the time. But I remember that he was showing off with his proofs of a paper to the academy which turned out to be an important paper where be I think it was Bohr, on a visit to Leiden colloquium, which he attended from time to time, who had asked him to see whether the Zeeman effect couldn't be resolved to see that the levels were split.

And I think that was his main contribution at the time. His dissertation was rather unimportant, and probably even wrong, I think. It was spectral series, trying a better form with more constants to fit the series spectrum of lithium better or whatever it was. But this work was really a contribution. Can you resolve the levels? It was much later that I understood what he had done. The moment when he showed it to me, I didn't know what it was all about. So I went to Leiden, and like so many of these young students - -.

Kuhn:

excuse me, when do you suppose he gave you Dunz?

Goudsmit:

Later, later, when I started to write my first paper. So I was already a student then, a second year student or so. He gave me a copy of his dissertation and some other things at the time. He gave me also a copy of Fowler's book on spectral lines, which is a successor to this, which I still have, also with his name in. I'll bring it in some day, but that's not a rare book. I think everybody has Fowler's book, Fowler's spectral lines, but this may be a rare book, but that was later. So where are we now. Out of high school, I'm going to the university. Let's see, were there any troubles there? No, I went to Leiden simply because it was nearest. I commuted from my home in The Hague to Leiden, on the train every day as many students did.

Kuhn:

How long a trip was that then?

Goudsmit:

I had to go on my bicycle, or the streetcar, for about fifteen minutes to the station, leave my bicycle there, take a train which took about 20-2 minutes in the early days; lately it was speeded up now that they' re all electric, and then had to walk for about fifteen minutes to the university every day. That was good. It was frowned upon — the people who didn't live in Leiden, couldn't afford to have a room there. That was wrong, because we were a group. We always met the same people, and there was a lot of discussion going on during that half hour or twenty-minute train ride and the walk to the university. So you mingled with chemists and astronomers and other people, and just because I commuted I got to know a lot of people whom I might not have gotten to know otherwise.

Kuhn:

How did it happen that you all went at the same time if you were going to different schools?

Goudsmit:

There were not so many trains! It was like the Long Island railroad. You all got on that 8:30 train or whatever it was in the morning. And we used to complain every so often, because it was crowded, and once the complaint was so violent that they had a special car for us hooked on in The Hague and taken off at Leiden again during one season.

Kuhn:

What did you study there?

Goudsmit:

At Leiden? Well, the more or less compulsory things, I took a course in elementary physics —.

Kuhn:

Was this largely a practicum?

Goudsmit:

It was a practicum plus a good demonstration course, a more advanced demonstration course by very good teachers. I took mathematics, which was analytical geometry and calculus, rather stiff, and I did not do well in that. I had to take chemistry, including some practical work in chemistry, I had to study crystallography.

Kuhn:

That was a required course?

Goudsmit:

That was required. I was very lucky again that just by the time I had to come up for ray exams they didn't require it anymore. So I never passed the second exam in crystallography.

Kuhn:

You didn't like that subject?

Goudsmit:

No; because I had to learn all these —. You see, I have no three-dimensional vision, I have no three dimensional brain, and so I could not remember these classes of crystals and so on — very stupid. In fact, this may help you understand things, if I now can jump suddenly 40 years later for one moment. It came to me very clearly in Copenhagen at that meeting that I had made a very grave mistake in my life, again pushed by circumstances. The grave mistake is that I should have known, from the very beginning, and that Ehrenfest knew it, but I didn't realize it clearly enough, that I am not, and never was, a theoretical physicist. That I should have stuck to experimental work, or close to experimental work. And so many of the things now become clear to me because of that. But it was never as clear to me as after this Copenhagen shindig.

I don't know why that revealed it to m but it has had a very serious effect on the things I have been doing and am doing and so on. I have been always a misfit as a result, it's very sad. Ehrenfest should have explained it to me more explicitly, and be didn't. He did it only in an underhanded way by sending me away from Leiden, and making me an assistant to Zeeman three days a week. But he didn't quite say why. He should have told me, "You are not a real theorist, you'd better stick to the interpretation of experiments, you will be better there." But then, because of the spin, people always classed me as a theorist, but I never, never was, because I never could do the mathematics sufficiently well for a theorist, and I'm totally lost, of course, in modern theory, because that's all mathematics. And George knows that I don't know any mathematics.

There are even anecdotes about it in Leiden that when they asked me a question on my exam about Maxwell equations and so on, then I'd say, "Oh, that's the part George always does." That story is almost true, the real story is as follows. When I went for my final exam, we were walking towards the academy building there, Einstein, Ehrenfest, and myself —

Kuhn:

Einstein was there.

Goudsmit:

Einstein was often visiting in Leiden. He was there again for a number of months, arid we were walking, and he (Ehrenfest) said, "The trouble with you is I don't know what I can ask you, all you know is spectral lines. Can I ask you Maxwell equations and things about that? I said, "No, please don't." So it's almost true, you see. But the anecdote is a little better, "That's the part George always does." Essentially the anecdotes are always better than the true story, but that's how it happened. So at Leiden, as I was beginning to say, in the early years I was very obnoxious. I was sure I knew everything, or could know everything. It was in those years that I thought my parents were dumb, and if they just gave me a couple of years, I'll know everything.

Kuhn:

Does everything mean everything in the sciences, or —?

Goudsmit:

Well, maybe everything in the world, I can't specify it. You know I recently heard a story of a well known physicist here who, not long ago, when he was drunk, really said, "If I were ten times as smart as I am now, I'd know everything." I won't mention his name on the tape. A very nice fellow by the way, very clever, but not that clever; the factor ten is not the right factor. And there was one incident — you know there are these incidents which often you can’t forget. That was the meeting with George Uhlenbeck. George Uhlenbeck I had vaguely known, because he had gone to the same high school, the second high school, that I went to. Also I was a member of the debating club, and when he was a student be used to come back and visit the debating club occasionally.

So I had seen him; he hadn’t seen me, but I had seen him. When he came to Leiden, I had already been at Leiden a while when he entered there; we had lunch together there quite often. One had lunches also in a special way in those days. He once put me in my place, which I can never forget, but which he has completely forgotten. He was reading a highbrow book, by a Dutch writer, which I only halfway understood, but similar books had been read in my sister’s book reading club, so I knew what it was about. I made the remark to him at lunch, “How can you read such stuff, it’ all such nonsense, what this fellow writes makes no sense; I believe that only physics makes sense.”

He got mad, and said, “Here, take it. I bet you can’t even read it.” He turned the book around and made me read it out loud and I, of course, failed completely. That made a deep impression on me, the fact that he was right, that I couldn’t read it. I didn’t know how to read this good Dutch. But he has completely forgotten that. I can never forget that incident, I still see it as if it happened yesterday — in the lunch room, in front of all the other students.

Kuhn:

When you say it was different, how one had lunch, that there was something special about it—?

Goudsmit:

Well, we were poor. You brought your own sandwich or whatever it was, you bought coffee in that place where you went, except the law students were always rich, and they used to buy their lunch, and buy pastries. We were always jealous of them, we used to tease them. I remember we had a battle once, where they threw the pastries in our face from the balcony. There was a balcony in the lunch room. There was one other incident —.

Kuhn:

Why were they rich? They were just richer?

Goudsmit:

They were richer than the physics students, because they came usually from the families who were already in business and then wanted to get a law degree, not always to become lawyers. It was somewhat like here; many students go into the law school in order to go into business later on. So they were usually a richer type of student than the poor physics students who came from somewhat poorer families — not too poor. I could have bought a sandwich, I'm sure. But it wasn't done.

While I was mentioning that to Irene the other day, suddenly I remembered a funny incident that has nothing to do with physics. One of these rich students had ordered his lunch, and had a couple of fried eggs, and they fell in his lap. One of the physics students, it was a student he didn't like at all, instead of helping him, says, "Wait a minute, you forget the pepper and salt," and he speckled pepper and salt in his lap. [Laughter] That struck me as so funny, that's also one of those incidents I can't forget.

It was very mean, but that gives you a little of the atmosphere. There was also a little bit of an atmosphere of rivalry between the students who had come from high school and those who had the complete education. I did not feel that, because I always stuck with the people who had had a high school education. Most of the physics students came from the high school in that day. With that conceit came the following. There were in those days these socalled prize competitions, a hold-over from the old French academy. Ther were posted.

Kuhn:

Were they French Academy competitions or university competitians?

Goudsmit:

They were in those days of the Teyler's Museum. They were posted, but nobody ever paid any attention to them, or saw them. There were a few cases. Casimir is a famous case; his famous work that became his dissertation was the answer to one of those prize competitions. But I remember when I was in my first or second year there was one about spectroscopy. It was to find a better foundation and an extension of a rule of Sommerfeld that in the periodic table the alternate elements had either doublets or triplets in the spectra. I was sure I could solve that.

Kuhn:

You were asked to find an explanation?

Goudsmit:

Not an explanation, but a better foundation and extension, because Sommerfeld had postulated that only on the basis of two columns in the periodic table — the alkalis, and the alkaline earths. There was some rule about it, and so they had as a prize competition to extend it, and to give it a better foundation, showing whether it's really true throughout the periodic table. I thought I could solve that. The way I thought I could solve it is by finding empirical rules for these doublets, and then may be able to find bow it goes with the periodic table. I hadn't the faintest idea really what was involved.

Kuhn:

You mean, you could find the empirical rules for something like doublet separation that would enable you to pick them out?

Goudsmit:

Yes. To pick them out, and extrapolate, things like that. And of course, I did not solve the competition; but trying out these empirical rules, I found an empirical rule. I was all enthusiastic about it, I told my family about it - - of course they didn't know what it was all about and I told Lohuizen about it. He said, "Oh, that may be nice, I'll take you to Ehrenfest."

Kuhn:

This was in your second year.

Goudsmit:

It was in either my first or second year. I don't know exactly. You have the chronology. My first paper was the result of that, it was published December '21.

Kuhn:

That would be your second year.

Goudsmit:

Probably my second year. Well, I worked on it that summer I remember, towards the end of my first year, I got interested in that. So I got introduced to Ehrenfest. He was always interested in people who wanted to do something on their own. He looked it over, he bad me write it down. Then, instead of telling me it was wrong, he said, "Look, there is a paper in PHILOSPHICAL MAGAZINE which does something similar, why don't you study that?" And of course it was exactly the same. Then I got even more ambitious. I said, "Gee whiz, maybe it's wrong, maybe I can prove the man is wrong." By that time, I believe, Sommerfeld's book had appeared. That was heralded as the Bible, so I began to study that. On the basis of Sommerfeld's book I discovered, or thought I discovered the z law for the doublets or the triplets. With that I went to Ehrenfest myself. It was still wrong; but Ehrenfest at least couldn't say it wasn't new. So he let me publish it, a short note in Naturwissenschaften. But because it was wrong, the main paper was published in that obscure journal, the Archives Neerlandaises.

Kuhn:

But when you say because it was wrong, you mean what?

Goudsmit:

Well, I asked him to publish it, it could be published in the Academy. And I have the letter where he says "the Academy is filled" and I should publish somewhere else. I am so sure that that was an excuse, that he didn't think it was good enough. That's the first letter I have in these letters, a letter from Ehrenfest, "Sorry, but I can' t publish this in the Academy, but publish it somewhere else." He insisted that I write it up and get it published in that French journal.

Kuhn:

Do you remember what your original rule was, We one that was also in the Phil ..?

Goudsmit:

Ja; a rule with z^2 , a purely empirical rule. But then I got that power rule out, and tried to compare it -. You couldn't prove that was a fourth power rule, but I could prove that if you applied the fourth power rule that you got screening constants, just like for the X-ray doublets, which also were not understood, so-called relativistic doublets. And I called that the relativistic interpretation of the alkali doublets. At that time it was wrong because there was no interpretation for it. There was no reason why it should be like that. That was my first paper, and that gave me then an entree into the colloquium.

Kuhn:

Before we get you into the colloquium, how much spectroscopy had you done at that stage of the game, and where did you begin to look at tables of spectral lines?

Goudsmit:

At that time, when I began to study these doublets, I probably got the numbers out of these tables, and out of Kayser from the library.

Kuhn:

Were you already looking at Konen then?

Goudsmit:

And I was looking at Konen's book, but I didn't understand it.

Kuhn:

How did you do with Sommerfeld?

Goudsmit:

Sommerfeld. You see I was very good at picking out what I needed; I never read a book from beginning to end; I started in the middle, where there was something I knew, and then, when necessary, went further or went back and learned a little more. But there were enormous gaps in my education as a result, because I picked out just the things which would be useful to the funny little idea I had, and nothing else. I was able to avoid all the other things which were not of immediate application to those ideas, and —

Kuhn:

How were you doing meanwhile in the lecture course?

Goudsmit:

Lousy. Because as a result I neglected it. That also meant that I took a year and a half longer to pass my first exam than nomal. As you know I had gained a year in high school. I skipped the last year of high school, but that was not because I was good; that was because there was a flu epidemic and the high schools were closed. I did not have the flu. Then a student I knew in art school arid another student who is now a diannd merchant here in town said, "Can't we work together and try to do the final exam, because the three of us did not have the flu? Let's work on our own, and see whether we can't pass the final exam this year." Because they had also eased the final exams.

You didn't have to know this part of history, you didn't have to know that thing and so on. So we worked together privately in my father's shop; my father was the timekeeper, he saw to it that we really worked. We even had a little chemistry lab, up there, in his workshop because you had to know a little practical chemistry. The three of us entered the final exams of the high school, which is equivalent to university entrance, trying it out. If we had failed, we would have gone back to high school for the last year, that was all there was to it. We passed!

Kuhn:

Actually the flu epidemic was in what nonr1ly would have been your next to last year?

Goudsmit:

Ja, ja.

Kuhn:

You took the time studying, and —

Goudsmit:

We took the time studying, and took the final exam. So, you see, it didn't mean that we were any better, it was just lucky that we didn't have the flu, so that was about 1918 or 1917, in Holland, about that time; I don't know exactly when it was. I can look it up, I have my diplon somewhere still. But then I lost that year again at the university.

Kuhn:

I take it you started in the university the fall of 1919?

Goudsmit:

Probably, I did the final exam in the summer of '19.

Kuhn:

You went to Leiden in the fall, and then took your exams after what, 3-1/2 years instead of after two?

Goudsmit:

Ja, the first exams.

Kuhn:

What were you examined in for the first exam?

Goudsmit:

Well, that was mathematics and physics, and — I don't know what it was. But I didn't have to do chemistry because I was so late, chemistry and the crystallography were dropped.

Kuhn:

Already at the first exam?

Goudsmit:

Already at the first exam. These were requirements that were dropped, but I had a little more physics than normal. I don't know exactly what they examined me in, but you know how it is in Leiden, the exam is a formality usually, but you have to go to each of the teachers privately, and they give you an oral privately, not at the end of the course, but when you feel you are ready for it. And I studied like bell for some of that; I remember very well that Ehrenfest had denied me access to the colloquium until I finally passed that first exam, because I was one of the few in the colloquium who hadn't even passed the first exam. Usually you weren't allowed in the colloquium until after your so-called (Kandidats) exam.

He denied me access to the colloquium just to force me to work for my exam. And I remember I really had to cram, especially for some of the mathematics which I didn't know. I learned it quite well. I was all alone at home, my parents were away visiting my sister in Paris at the time for several months. At that time I taught myself to play a couple of pieces on the piano — a Beethoven sonata, and a little Mozart thing, just by rote. I went from my book to the piano, from piano to the books, and so on. That was all I ever knew at that time. And I remember that I went to visit my sister and she almost fell off her chair when I sat down at the piano and began to play.

I could read the notes a little bit, and I just learned it by heart. Again to convince myself that I had no talent, and why did I do it? First of all, as a psychological reflex, I think, from my cramming, and because of my mathematics friend who came(of course, from a musical family, and had taught himself the piano. I said, "Gee, if he can do it, why can't I try it?" This was again the impetus, you see. I learned pretty soon that I couldn't, But these two pieces I honestly learned by heart at that time. So, back to physics, Ehrenfest refused me access to the colloquium until I passed my exams. Then I finally did, but not too well. Then I had time again for physics. I have to look at my publications, I don't know whether I published something already before that exam, or whether there was a gap.

Kuhn:

My feeling is that you'd have taken the exam in '23. You had published the two papers on doublets.

G:

Some of these are duplicates, as was customary in those days.

Kuhn:

Two sorts of things before we get you back into your second round of physics. I'm terribly interested in this first paper, because it's not unlikely that it's got a good deal to do with what happened afterwards. How did you feel about the whole subject of the application of Sommerfeld's relativistic theory to optical doublets?

Goudsmit:

I was convinced that it was right, but not for theoretical reasons. I was a man who liked empirical relations and empirical formulas, and I was sure that it was no accident at that time, but I did not understand relativity, I did not understand the detailed mathematics of Sommerfeld's derivations. But there was that formula: why shouldn't it apply to these doublets also? I was very proud of it, proud of having a publication, and as I've often told—.

Kuhn:

But there were enough data?

Goudsmit:

No. If you now submitted the same thing to the PHYSICAL REVIEW I'd reject it. If you present it as an abstract at a meeting I'd say it s not well founded. I was very proud of the fact that it did get published, especially in the Naturwissenschaften. I even got a letter from a real physicist telling me that he was interested and wanted to get the detailed papers — Smekal — he wrote me a letter that he was very much interested in my interpretation. That made me very proud, to get as a young fellow a letter from a real physicist who had already published quite a lot.

Kuhn:

What about this business of jiggling with the quantum numbers, which you point out has got to be done?

Goudsmit:

People jiggled with quantum numbers all the time. In those days there was nothing strange about it, it was normal. Don't forget that I didn't understand what I was doing.

Kuhn:

Now you're being unfair to yourself; you're quite clear-cut about what's obviously true in this case, for example, that the doublets cannot belong to the same— that you've got to reverse the function of the inner quantum number and what had been the running number for the terms.

Goudsmit:

Ja. I thought, well, that's what it tells, but not knowing what the interpretation of those numbers was. Don't forget one didn't know what these inner quantum numbers were. The only quantum numbers one understood were the n and k of Bohr, and one didn't understand the doublets and triplets at all. But the analogy with the X-rays, that was clear to me was no accident, but why, I didn't know. And then I forgot it, then I had to work for my exams.

Kuhn:

Did you hear from anybody besides Smekal about it?

Goudsmit:

No, not a word.

Kuhn:

Nothing from Lande, nothing from Sommerfeld? In your first exposure to the colloquium, because you started going —.

Goudsmit:

Ja. In a typical European way, I understood only part of what was going on. But being exposed to it you absorbed quite a lot. Another thing happened, I had met Paschen; one should not forget that. It must have been 1921, I think. I accompanied my father to Germany in either '20 or '21, I think '21 because Ehrenfest was mad that I interrupted the lectures in the middle of the year and had to go to Germany.

Kuhn:

Were you getting lectures from Ehrenfest as early as that?

Goudsmit:

I may have been sitting in, ja.

Kuhn:

Because I think he would not have been giving one of the courses that was required for the comprehensive exam.

Goudsmit:

No, I think I sat in on some of that. I must look up in my notebook — you see, that is a thing where memory is very dangerous.

Kuhn:

You have a notebook on this sort of thing?

Goudsmit:

I have a notebook, ja. I haven't got it with me, I forgot to bring those because I didn't think of it at the moment.

Kuhn:

These would be notes on the lectures?

Goudsmit:

Yes, and they have some dates in them.

Kuhn:

If you've got notes on Ehrenfest's lectures at this time, this is something which —

Goudsmit:

They are lousy because I was such a lousy student, I didn't understand the mathematics. There are hundreds of students who listened to Ehrenfest's lectures, and there are some of them whose notes must be perfect.

Kuhn:

Yes, but most of them don't keep their notes.

Goudsmit:

That may be. Anyway [my notes] will tell you what subjects he discussed at that time, and more or less how he did it. From that point of view it may be of significance. Anyway, I think he was a little mad, and asked me where I went, and I said I went to southern Germany, so be said, "Well, you are near to Tuebingen, go and visit Paschen." I don't know whether it was during the lectures, that I was already in his lecture, or because I was in the colloquium, something like that; so I visited Paschen.

That was also a marvelous experience which I could never forget. Paschen didn't treat me as a freshman but as a physicist. He showed me the 4686 line, the famous helium line with the fine structure, which he had set up with an interferometer in his laboratory. I did not understand it. I didn't know what it was all about until I came back to Leiden. Two years later I went a whole summer to Paschen. There I learned the technique of spectroscopy. He made me build a spectrometer, told me how to measure spectra lines, what to look for — it was really marvelous. I spent the summer there.

Kuhn:

You'd had no previous experience with actually getting data yourself?

Goudsmit:

No, no, never, I didn't know how to measure anything. And I liked it very much. Anyway, I worked for exam, and I don't know exactly which summer it was now because I worked at home it may have been even later that year. Anyway, these things were important to me. Then I remember that Coster lectured at Leiden about papers by Lands and by Lande and by Millikan where they had done exactly the same with the relativistic doublets as I had done, but they had more data. Millikan had really taken the ultra-violet spectra and now they had lithium, beryllium, etc. and the whole sequence. So he had data, and bad come to the identical conclusion. T: This must have been appreciably later, though?

Goudsmit:

About 2-1/2 years later, I think, or maybe 3 years later than I did it. And Lande, also on the basis of other people's data, had come to exactly the same conclusions.

Kuhn:

This would have been late enough so that already now it's after the Rurnpf model?

Goudsmit:

No, — I don't know.

Kuhn:

Yes; the Heisenberg-Rumpf model, I think, wasn't published until '23.

Goudsmit:

When did Millikan publish his stuff, I can tell — I may have —.

Kuhn:

I may have that. [They hunt for the dates.]

Goudsmit:

I wrote a letter about it to someone once. I found it just the other day again at home. I have here a letter from (Breit) which may —.

Kuhn:

That's something also we can figure out, but my impression is that Lande's involvement with the question of relativistic versus magnetic doublets really comes after the magnetic explanation.

Goudsmit:

That may be. I don't know that anymore. I can look it up, but from memory I do not know the sequence.

Kuhn:

I really think he's got most of the g factor first and then comes back to the —-

Goudsmit:

That may be. I also do not know whether I knew Lend already or whether I got to know him later. But even that can be figured out from all those postcards, do you remember, which I have — the Lande postcards, that whole set. But anyway, Coster said, I put up my hand and said, "I talked about exactly the same thing 3 years ago" And suddenly Coster remembered, "Oh, this poor man," he said, "he talked about the same thing 3 years ago." And I thought it was hilarious, and I was rather put out, of course, at the time. But that was at a time when I was also in the middle of exams, and it didn't make much impression on me. But then, when my exam—.

Kuhn:

In the middle of the first exams?

Goudsmit:

On the first exams, ja.

Kuhn:

But then it can't have been 3 years, can it?

Goudsmit:

Sure. Because my first paper was '21.

Kuhn:

But that was already your second year?

Goudsmit:

Ja. When was the Millikan paper?

Kuhn:

Well, that I'm just not sure of.

Goudsmit:

Anyway I think I was still under the weather. I don't think I had done anything. Because I see here, my next paper was then finally on some multiplets which I had found in iron and manganese and so on. When I had time again, I began to work on multiplets, trying to find multiplets in all the spectra. Now when did I go to Amsterdam, do you know that? When did Ehrenfest finally decide that I should be an assistant of Zeemann?

Kuhn:

Well, I'll tell you what I've got on it, — these things are never altogether reliable — '23. Now that would be after your exam?

Goudsmit:

I'm not even sure that it was right after or about the same time.

Kuhn:

Probably - - you started in '19, then probably it was the summer of '21 already that you worked — or do you think it was as late as the summer of '22 that you spent really cramming for the exam?

Goudsmit:

Summer of '22.

Kuhn:

Then the following summer would be the one in which this would come, this would be all right, this would put you at Tuebingen in 1923.

Goudsmit:

Ja. Where were we, I'm lost now?

Kuhn:

Well, we haven't been quite sure either where we were. In these early things in the colloquia, what were the topics of particular interest, what were people really worrying about?

Goudsmit:

I do not know; I have no precise recollection. I always remember the bad things. This happened to me, which gave Ehrenfest an excuse to kick me out of the colloquium for a while. I had gone to the colloquium, and they were probably the most interesting things, but they had nothing to do with spectral lines. Van Lohuizen as a high school teacher couldn't always go, and he had asked me what was in the colloquia in the last few weeks, I said, "Oh, nothing of importance." Van Lohuizen, being a little naive, when he came again — I wasn't there — Ehrenfest asked, "Why didn't you come?" and then he said, "Oh, Goudsmit had said there was nothing of importance."

So then Ehrenfest stormed into the room, and said, "What did you say? You told Lohuizen that what was discussed in the colloquium was of no importance" He was furious. He said, "You'd better stay away for awhile and study a little while." So that's what I remember. But I do not know what these subjects were then: I was so narrow that if it wasn't directly connected with atomic structure and spectra lines, then it didn't stick with me.

Kuhn:

Now there were some interesting things going on in spectroscopy itself right at this time. One of the particularly interesting things surely is Lande's reinterpretation of the significance of the inner quantum numbers.

Goudsmit:

Well, I took these things for granted, you see. I never had the feeling of difficulties, never. I always had the feeling that these were empirical rules which some people understood and could interpret, if not now, then later on? My interest was really in the mechanics of the empirical rules. By empirical rules, I mean also quantum numbers. Even in the years when the Pauli principle came up, to me that was no more than an additional empirical rule like selection rules, like assigning quantum numbers. It was just an additional rule which explained a lot more and put order in this mess.

Kuhn:

But the whole Modellmaessige side of this was of no concern to you.

Goudsmit:

No concern to me. But that reflects upon me, you see.

Kuhn:

But you were also a contributor; there are different models certainly more important to Lande, but not exclusively important.

Goudsmit:

I knew the model but just did not understand it. I wrote you about that time when Lande came just the 5th of December, St. Nicholas, and that we gave him the "R" and. the "K" and the "J" in chocolate letters, which to us was marvelous, we had them on a cartoon. To him it probably merely meant that he got some good chocolate to eat.

Kuhn:

No, he remembers it; and be doesn't remember very much.

Goudsmit:

No. But that he remembers! And I took him around Amsterdam and The Hague and so forth. I was very narrow. After that, my work consited mainly of trying to find multiplets and analyzing spectra, which I thought was simple.

Kuhn:

Now how did you do that? Did you do it from tables, did you do it from pictures?

Goudsmit:

No, because I did it sometimes from tables, sometimes from pictures. I had learned from Paschen that you couldn't do it from tables. Paschen had told me, he had said, "Look at these lines, you can tell the ones which belong together by the way they look. You can never do it from tables, it's not numerology. See, this line is a little more diffused, this in the discharge comes out a little longer on the spectrogram. You can tell, look at them." And so, wherever I could, I did not go by the numbers but by the characteristics, but you could also do that from tables.

For instance, I remember once on a bet, opening Kayser's book to some complicated spectra — I don't know what it was, germanium or something — and picking out a multiplet, not by looking at the numbers and taking all the differences, but by looking at intensities. The Kayser Handbuch has numbers, and then also has next to each spectra line a little bit whether it was diffused or strong, you know, and there were some lines which just fell in a group which looked like a multiplet. That's how I found them. Then I took the reciprocals in order to see whether the wave numbers fitted, and, damn it, it was a multiplet. I didn't even publish it, it was so trivial I thought, it wasn't worth while. But I did, I began with manganese, I think, and there was iron,' and so. It was after Catalan had found the existence of multiplets.

Kuhn:

Was that itself a big source of excitement?

Goudsmit:

To me that was, of course.

Kuhn:

Where did you first know of the Catalan paper?

Goudsmit:

I think right when it appeared. We had good contact with England and so on. I think I still have some correspondence with Fowler about that in these letters. That was exciting because I'd always been interested in these empirical rules and now it gave Sommerfe1d's rule, which I tried to learn about, more sense; relativistic doublets and triplets and so on, I got that extension. From then on, it became for me a kind of numerology, but I want to say it was not numerology, because the people who tried to analyze spectra by means of numerology never got anywhere. Even at M.I.T., where they built a big machine, they never got any spectra analyzed that way.

You have to look at them. You have to look at the lines, and if you can't look at the lines themselves, then look at characteristics of the lines rather than anything else. There was a fellow in California, King, an astrophysicist. Be looked at all spectra in an oven, and classified them when they appeared, and whether they looked diffased and whether they were reversed or not. Those were the most marvelous sources of information because you couldn' t look at the lines themselves, you could tell by his classification which belonged together. So his spectra were more valuable than the very precise measurements which were published. So that's the way it was done. It was a skill.

Kuhn:

What books, what journals where did you get pictures when you used pictures?

Goudsmit:

The pictures in Amsterdam. I looked at the spectra which were measured in Amsterdam, especially Zeeman. Then the Zeeman effect of course helped. In Zeeman's lab, I told them what spectra they might look at — scandium was one of them. There were also many Zeeman effects published in obscure journals which I got hold of through Zeeman. There was a Hungarian publication which published excellent Zeeman effects; I couldn't read the Hungarian, but the tables were there. That gave the spectrum of lanthanum, and one got things like that by just looking around in the literature. I was always excited, it was like solving puzzles, nothing else. It was not difficult. Then there were these little fights with Sommerfeld. Sommerfeld tried to monopolize the information on the iron spectrum, and he lectured in Amsterdam, and I had published something on the iron spectrum.

Kuhn:

Where had be gotten the information, from Paschen?

Goudsmit:

Ja, from Paschen and other people. But he wanted to do it all, and I had published "Multiplets of the Iron Spectrum" based on some measurements made at the Bureau of Standards here. A fellow had found preliminary multiplets and I had properly assigned them and. so on. I remember Sommerfeld being quite upset because I 'scooped' some of his fellows. The one I scooped was Otto Laporte. But it didn't matter, because what I had done was again the minimum one can do, out of laziness. When Otto Laporte published his paper, it was the complete iron spectrum, nothing left undone. So I never deserved, never got any credit for being the first.

Neither did the poor man at the Bureau of Standards who did it even earlier but not so well. Maybe there's a footnote somewhere in Laporte, but I'm not even sure. Laporte is the man who really did the iron spectrum, because he did it from A to Z, everything. And it looked like quite a feat, because the iron spectrum was in the old days proverbially the most complicated spectrum. Of course from a multiplet point of view, it was the easiest, just for that reason. There are some spectra which they haven't analyzed yet because they are really complicated, like tungsten, and uranium, and so on. But iron was the simplest. So that's how it went. You see, I was a contributor only in this narrow field of interpretation of spectra, and that I knew quite well. I knew the spectra by heart, I knew all these rules by heart, but I was not a theoretical physicist, I did not know what they meant, I never felt that there were difficulties, I took the Bohr atom as something obvious.

Kuhn:

How about the more complex versions of the Bohr atom? Were you concerned with things like Ellipsenverein, these models, before?

Goudsmit:

I looked at them, but I felt that I could not contribute anything there. Therefore I say I didn't understand it.

Kuhn:

Did you think they could contribute anything to you?

Goudsmit:

That's why I read them. And I must honestly admit that I remember in the old days trying to play with models like that, but never succeeding - trying to improve it, trying to see whether one could get better results. But I never succeeded because you needed a lot of mathematics. You needed to compute orbits and things like that. I couldn't do that. I always hoped that a very sin1e model would give the right results, and it never did. I did speculate.

Kuhn:

Did you worry about helium at that time at all?

Goudsmit:

Ja, ja, but with no results. Only trying out, let's say, two circles and whether I couldn't get the radius of the one circle so that it gave the helium lines. Of course it never did, never did — pure speculations.

Kuhn:

Did you read Lande's early papers on helium, in which he tried crossed orbits?

Goudsmit:

Ja, but I didn't understand. The only paper of Lande I understood were the ones where it was numerology, the Zeeman effect, "g-factor"; by 'understood' I meant that they stuck. I knew that by heart, inside out, I could do that on a desert island, derive the formula and so on, without knowing what the things meant.

Kuhn:

What about the Rumpf?

Goudsmit:

That I did not understand. I never understood the difficulty and never was aware of the famous Pauli paper. For me, it was purely empirical rules and nothing else.

Kuhn:

You say you were never aware of the famous Pauli paper —

Goudsmit:

In which he proves that it couldn't be the Rumpf, remember, because if it were, then it could only be the innermost electrons, and then there would be a large relativistic effect. It was probably very important, it didn't hit me at all.

Kuhn:

It's important too for me that you didn't know that particular paper.

Goudsmit:

I can't say I didn't look at it, but it didn't register with me, because it was not on my level at all. Had absolutely nothing to do. Let me see what else I did in those days?

Kuhn:

I do want to get you to talk more about being in Zeeman's lab in '23. This is early still, we've skipped past it, but let's not go further.

Goudsmit:

In Zeeman's lab in '23, psychologically it was important, because the atmosphere in Amsterdam was quite different from the atmosphere in Leiden.

Kuhn:

Tell me more about that difference.

Goudsmit:

The difference was that Leiden was a rather high-brow university, and Amsterdam was a little low-brow university. I found more people of my kind in Amsterdam, I mean, the uneducated type, than I found at Leiden. It was a little easier for me to make friends there. It was also a very remarkable reaction I had: on Wednesday I traveled on the train from Amsterdam to Leiden to go to the colloquium, and I always had the feeling I had to turn a switch, already then, because now I know I cannot use certain expressions, I cannot tell certain jokes. It was very significant. As far as the physics was concerned, I learned experimental techniques of spectroscopy.

I tried to set up a couple of experiments myself. I did not quite succeed in doing that. It was Dieke who wanted me to get better data on the hydrogen spectrum, so with the help of the technicians I designed a discharge tube and a special cathode which we had read in the literature would give a very nice hydrogen spectrum and tried to take pictures of that, but it didn't come out. I tried to do it, and I probably would have gone on with it further, but then I suddenly got interested in other things, the Zeeman effect, the scandium spectrum. But I measured lines, and I helped the other people with the interpretation and advised them on what to do and bow to do it.

Kuhn:

What things? You told them to do scandium, because of its particular position in the periodic table?

Goudsmit:

Because it looked as if the multiplets were easy and that it would be nice to compare it, and because of its position in the periodic table it was one of the spectra of second — of hoeherer Stufe, with different limits and so on. I always looked for the easier things which were easy to do; and the hydrogen molecular spectrum was not an easy thing to do.

Kuhn:

The thing Dieke wanted was a more accurate molecular spectra.

Goudsmit:

Ja, That has been his interest from the very beginning.

Kuhn:

Were you tempted, did you consider going into experimental work?

Goudsmit:

[Pause] Ja. Let me say it this way. I would never have been an experimenter as such, but I would have been much better being near to experiments and help with the interpretation of experiments and directing experiments than as a theorist. The few things I did which were worthwhile were always of that kind, even later on. By telling people what would be worthwhile doing, and then giving them some advice on how to do it, and hope that they had the skill, (poor guys), so they could get it done. When they were in difficulties the only thing I could do was to encourage them, but not always give them advice.

Even when I came to Michigan, the first work was experimental; but I never got my hands dirty. I was much better at that. Even recently at Brookhaven I got really mad at the lousy way of interpreting some experiments. I insisted that they listen to me, and write it up the way I think it would be most convincing. And I think (Rothpool) listened to me and did it that way.

Kuhn:

As you worked on the Zeeman effect, by that time you had the g factor. What was that like for you? Did suddenly lots and lots of things then open?

Goudsmit:

Ja, ja. It suddenly fitted, you see. At that time I began to understand what Lohuizen had tried to do, but didn't quite succeed. So my opinion of him was a little, "Stupid, he should have done that several years earlier, but he didn't." I don't know — probably there weren't enough data.

Kuhn:

And there were some ideas that had come along in the interim also.

Goudsmit:

Ja. But you ask here whether I knew the background of that paper of Van Lohuizen. I remember very well that be told me that it was the visit of Bohr at Leiden which either Bohr himself or Ehrenfest had told him, "Look for this." But he succeeded only part way. See, what else did I do? You see, this is all spectral analysis. Then I began to get interested in intensities a little bit. I bad studied the intensities papers by Ornstein, Dorgelo, and so on, and tried to see whether they couldn't be simplified so that I could understand them. I tried to understand Kramers' thesis, but didn't, and still don't. I now know what's in it. I may probably understand it now, a little too late. I had discussed with Coster intensities in X-ray spectra at the time. Coster was also very encouraging to me. I liked him. He was a difficult man, but I got along with him marvelously. He tried to help my parents in the war.

Kuhn:

In that paper with Coster on intensities, you're also worrying, I think, about the parallels?

Goudsmit:

Ja, ja. See, I had no inkling of that, I had completely forgotten, I just had the library make a reprint for me just now, because I hadn't seen that paper maybe in 20 years.

Kuhn:

But that brings you back in some part to your earlier concern again?

Goudsmit:

Ja. But I wasn't even aware of that step in between.

Kuhn:

Can you remember anymore now that you've seen it again? Can you remember more about how that —?

Goudsmit:

Ja, I remember that it was more Coster than I, because I was not sufficiently familiar with some of the intricacies of the X-ray spectra. Sommerfeld had presented it very simply in his book, and that's as much as I knew. Of course it was Coster's main field, and he knew many more of the intricacies about the intensities and so on. He discussed it with me for several evenings. He was in Haarlem then, and I used to go up there. Those were very nice discussions. That's all I remember about it.

I always had the feeling that he was the dominant factor in that, and that he had adopted my ideas and adapted it to his field. So that paper was Coster. I must honestly say that the reason why I have forgotten so much about it is not that I haven't seen it in such a long time, but because it was primarily his contribution. He understood my ideas better than I did. He had accepted some of my ideas and said this ought to be applicable to the X-ray spectra. And again these quantum numbers show up. You see how you have to change quantum numbers around.

Kuhn:

It's exactly this aspect of it that for the obvious reason particularly concerns me. By this time, one's got now, at least for the optical series, the definitely competing theories. When does this paper go in? — just before the Pauli papers — but this whole issue of optical versus electromagnetic, versus relativistic thing is now wide open. How did you feel? Were you still pretty sure they were relativistic?

Goudsmit:

Ja. But only on the basis of the empirical relations, because, don't forget, I did not understand relativity; I did not really know what it meant. I was aware that there were difficulties there, but —.

Kuhn:

What did you know about the magnetic interaction theories?

Goudsmit:

Nothing. Except the interaction with the external field.

Kuhn:

These internal Rumpf magnetic interactions —

Goudsmit:

I did not understand, did not understand at all. And the interval rule I did not understand how that came about. I only understood interaction with an external magnetic field. I understood the Paschen-Back effect and things like that.

Kuhn:

Did you try to work an those things?

Goudsmit:

No.

Kuhn:

How about the Pauli papers in which he traces across from the Zeeman effect to the Paschen-Back effect?

Goudsmit:

Those I knew, those I understood. Those I knew very well, the sum rules. I think I wrote some similar papers or something later on, which I could only have written if - it shows that I really understood them already at the time.

Kuhn:

Yes, There is one paper like that.

Goudsmit:

The strong field, and weak field, and how they were related, how you had a one- to-one correspondence, not the transition from one to the other. That I didn't understand. For instance I also didn't understand a much older paper which I should have understood. That was on phenomenological theory by Voigt for the Paschen-Back effect and the transition from weak to strong field which is perfectly correct. It really gives The formulas are there. It really gives the whole Paschen-Back effect from weak to strong field, but I don't know what the assumptions are anymore.

Kuhn:

That thing of course goes on right through the literature. Heisenberg was very nice about this, points out that they get that same formula three times. Voigt gets it by assuming some very odd ad hoc couplets between two electrons in a classical theory. Sommerfeld applies it to terms, and then he and Jordan get it out again.

Goudsmit:

Ja, ja. And I should have known, I should have understood it. I knew of its existence, but never understood what it was all about, because I was not a mathematical physicist.

Kuhn:

How hard did you wrestle with these things? Did you try to do the Rumpf model — did you work - -?

Goudsmit:

No, I did not.

Kuhn:

Did you work very hard on the Kramers thesis?

Goudsmit:

No. Ja, on the Kramers thesis a little harder, but I had to give up. I did not learn any good mathematics for a long time. Not until I had to teach courses in advanced mechanics at Michigan. That's when I began to learn a little bit, when I had to struggle with it for some of the papers I wrote later. And it was a mistake from the very beginning to do it. I should have stuck to experiments and empirical rules. What else do you want to know, my relations with Kronig at the time?

Kuhn:

I'd like to know about those papers and about how you got together on that whole subject with Kronig, and there's the very interesting — there you're right in an area where there's real trouble, where the correspondence principle is saying something somewhat different from the Ornstein-Dorgelo theory.

Goudsmit:

The trick again was that I probably was good at guessing formulas. Kronig came to Leiden. I think he had a Rockefeller fellowship or a National Education Board Fellowship, and we bit it off very well. We saw each other quite a lot. He was very frank with me, he told me about the poetry he had written and things like that, so I felt very much attached to him in those days. We began to discuss these intensity things, and he knew all the physics behind it, I only knew the rules.

There were some rough rules for the Zeeman effect intensities based on the correspondence principle, and the question was, couldn't we pin that down, couldn't we by just guessing at least I thought of guessing, be thought of a model — find precise formulas for them. And we succeeded. But mainly, I think, I was the one who gave the clue to the formulas. I still have a letter where he says "Your remark has acted as oil on a machine," because then he left and went to Copenhagen, so we never quite finished it together. The paper was written when he had already left Leiden.

Kuhn:

Did he write it?

Goudsmit:

I think I wrote most of it that time, and sent it to him for his comments and so on, and he wrote part of it. But I think of that paper, because it was published in the Dutch Academy, I wrote most of it originally, but I cannot prove that at the moment. You have to make a word study with a computor, and that kind of nonsense. My contribution was again that suddenly I knew the formula, I knew how you had to replace L by L*1 and I just said "Now we know how these intensities" — don't forget we knew the sum rules. That we believed in, that the sum of the intensities bad to add up to a certain thing. And it was such a conlicated thin

Goudsmit:

you had five levels here, and seven levels here, and all these lines and selection rules. Is it possible to invent a formula which would just obey the sum rules and looks plausible and gives the right result? It was honestly guesswork, and b knowing that kind of formula very well, without understanding bow they came about, I was able to guess it all right. But Kronig was the one who knew the correspondence reasons for it, who knew the sum rules, you see; I was the one who was good at putting in j times j+l and showing that it just would give the right result. Things of that nature, that was my contribution.

So, you see, it was also typical for me that a1nst always I needed to work with somebody; very seldom could I do a thing alone, usually with somebody. I worked very closely with Kronig in those days. Kronig went further, of course, and also did the multiplet intensities much better. But our work was the Zeeman effect, and then, of course, other people independently had done it too, and I don't know, these formulas were rediscovered by at least 3 or 4 different people independently.

Kuhn:

We've talked in at least some detail about the part of your period at Leiden, education up to the time you took the Kandidats exams. Then what did you do?

Goudsmit:

After that I sat in on the lectures of course at Leiden, still went to Amsterdam, as you know, regularly, —

Kuhn:

You were there what, 3 days a week?

Goudsmit:

Three days, Monday, Tuesday and Wednesday I was in Amsterdam. Wednesday night I came to the colloquium and then stayed Thursday and Friday in Leiden. I have forgotten whether one went Saturday or not; probably did in the morning, I don't know. My trouble was, I did not take many regular courses, and there are big gaps in my education as a result. I am not an all-round physicist.

Kuhn:

What did you have to do, for instance, with an eye to future exams?

Goudsmit:

I almost failed. And there's another anecdote about it, in my final exam, you had to have a major and two minors. The story in Leiden is that when Sam did his exam, his major was spectra lines, his first minor was spectra lines, and his second minor was spectra lines. And it's true. First of all I had not taken all the courses which were required. For instance do you know well, I shouldn't get it on the tape because it is so shameful, that I really never took a course, and still do not know maxwell theory! Honest! It's a shame, but it just happened that Ehrenfest wasn't there, the course wasn't given, I never taught it, and I never studied it for myself. So it was quite true, when Ehrenfest walked with Einstein and myself, "Don't ask me that, I never took that course."

Kuhn:

What about Hamilton-Jacobi theory?

Goudsmit:

I learned that much later, I learned that a little bit. I took a good course in mechanics, but Ehrenfest had a fight with the professor, and I had been sick — I don't know what it was, I think it was again that flu year, or another flu year then — and stayed away, and had been stupid enough to tell the professor that I'd make up. I never did, because I got interested in research and spin and things like that, you see. But I knew it very well, I studied it. Mechanics I understood, the simple mathematics. You needed no more, at least the mechanics I know you didn't need too much highbrow mathematics.

Kuhn:

But this didn't get you into things like contract transformations?

Goudsmit:

A little bit. I knew what they were. And also I taught it at Michigan later on and had no difficulty with doing it, at least the main points I can teach still very well, but not the detailed mathematics, not the existence theory and so on. So I knew it very well, but Ehrenfest and that professor Van Woude had had a quarrel. So Van der Woude refused to give me an exam, he said, "you didn't come to my lectures." So there I couldn't finish for my doctoral. So what was to be done? Ehrenfest, of course, was a powerful man, he said, "We'll find something." So he went to the astronomers.

The astronomers were always very friendly. Ehrenfest said, "look here, this fellow, he must have his 3 subjects, and the mathematicians don't want to give him an exam. Are you willing to take him on?" And the astronomers said, "Sure, we know him, and we have listened to his colloquium lectures, he's all right, we'll give him an assignment." The assignment was the theory of Darwin and Fowler and Eddington of the star spectra. And I had to study that quite well, including the mathematics, and I learned it very well, I had to write a little essay about that.

The other subject then was experimental physics. I'd been with Zeeman, at his lab, and so de Haas was willing to consider that as requirement for experimental physics. Ehrenfest took of course, the spin work, and other things like that. So you see, it's quite true: first major was spectra lines, first minor was spectra lines, second minor was spectra lines. That's how I finally passed my exams before the thesis, the doctoral exam.

Kuhn:

When did you actually pass those? Just before, or sometime before?

Goudsmit:

No, some time before. I don't know exactly when it was, I'll have to look that up. As you know, writing a thesis can be put off in the Netherlands for a long long time if you don't want to do it. My main time was spent trying to learn more about spectra by working in Zeeman's lab, trying to write papers on spectra lines, things like that, coupling schemes, giving lectures to the colloquium or to the Dutch Physical Society about spectra lines, and to the Huygens [Club] about spectra lines.

Kuhn:

Who else was involved with this sort of work at Leiden?

Goudsmit:

Nobody. There were those experimenters who worked with Zeeman, but they did very little when it came to the theory of spectra. Zeeman himself was not active at all. He was just a gentleman who came to the laboratory one hour a day. He was very nice and friendly, would invite you to dinner - there are lots of stories about that. It was always very painful, we were poor, and it was the old Dutch habit that if you were invited to dinner at a gentleman's house the maid had to get a tip; and the tip you had to give the maid was always much more than we would have spent for our supper. So we weren't always very happy. Then we were placed among his daughters and family according to seniority. I was very lucky, I was youngest or the latest of the assistants, and I was with the youngest daughter, who was by far the most intelligent and nicest, the one I looked up this summer again.

Kuhn:

That correspondence just can't be gotten?

Goudsmit:

It can't be gotten. But I'll still try.

Kuhn:

Is there a chance that George can do something about this when he's there?

Goudsmit:

Only by asking Miss Zeeman again in Amsterdam and she is very helpful and very nice.

Kuhn:

I think the best chance is for somebody who's there for a while.

Goudsmit:

By the way, I am getting photographs of the wall. I just got a letter from (???) and I told him what I wanted again. Finally clicked you see, it took a long time.

Kuhn:

If we go much further we're going to get into spin, and I think we should save that for Saturday. Look, on page 3 of this outline, we've given a list of a lot of the things, and we ought to stay away from No. 9 and the things after that. Some of the others I've already asked you. I really just wonder whether looking at some of these great big developments of the time you were there will remind you of things.

Goudsmit:

I tried. They do not. And s imply because of w own narrowness. I did not take part in these great developments.

Kuhn:

Now, what about the Bohr-Stoner theory, because that one you were interested in?

Goudsmit:

That I again took as a new rule. You see, the Stoner sub-groups, it was natural to me. I don't know for sure, it was the kind of thing that when I read it, I might have said, why didn't I think of it? It fell in line with what I had known, but again, like the Pauli principle, to me it was just another rule for understanding spectra.

Kuhn:

Now the Bohr thing did not have the same sort of effect on you. You think it was Stoner's paper rather than Bohr's?

Goudsmit:

Stoner's paper, ja, ja, definitely. Because Bohr I probably didn't understand. But Stoner's was such a natural thing. Then also the interpretation of some of the magnetic properties, and I didn't quite get it right away.

Kuhn:

How did you talk about spectroscopy with Ehrenfest? This was not on the whole his sort of work at all, was it?

Goudsmit:

No. He let me talk about it in the colloquium, and of course he had a lot of patience. Even if he wasn't interested he didn't show it, because he always tried to encourage me. Of course it was often talked about in a derogatory way.

Kuhn:

By whom?

Goudsmit:

By the other physicists. They called it “zoology,” “the cosine pest,” and things like that. It was not considered right, but I didn’t feel it, I thought what I did was nice contributions.

Kuhn:

And it was.

Goudsmit:

But in terms of what was going on in those days it was really a minor side line. I say it was a nice contribution, but so was a nice contribution just measuring those spectra, that was just as important, because measuring them and getting them in the table, and Kayser’s Handbuch I think is also an important contribution. But is it really physics in the modern sense? I still think it is, I still think that physics is an experimental science, you must first get the data. But I was not in on the excitement, because it was outside the capacity of my brain. I met the people. I was friends with Fermi from the very beginning when he came to Leiden and so on, but the physics itself went past me.

Kuhn:

There’s one thing I ought to know more about than I do. In the earlier part of the time you were in — the ortho- and para-helium series, one’s a singlet and one’s a doublet. Do you remember how the resolution to a triplet comes?

Goudsmit:

I don’t know whether I was the first one who proposed it or so.

Kuhn:

I’m just not sure.

Goudsmit:

I’m not sure.

Kuhn:

There's an awful lot, and more, almost more in this whole spectroscopy — spectroscopy after '22 becomes incredibly complex.

Goudsmit:

I wish I could find that helium [paper], I think it is mentioned on that paper with Uhlenbeck. By the way there's one interesting thing I like about the spin which I had completely forgotten about until a year ago, because the reprints — these are the reprints of the spin paper — but if you look at the original article, there's a little appendix by Ehrenfest, which is again a typical Ehrenfest thing.

Kuhn:

Yes. That is certainly worth knowing, that there's a note by Ehrenfest.

Goudsmit:

That he had heard the same idea from de Haas, who is setting up an experiment. I don't believe a word of it, to be honest.

Kuhn:

Some of this at least, not in the form of an inner rotation in its original form does go back to the Einstein-de Haas —. That's not presumably an inner rotation, but it is an anomalous gyromagnetic ratio and gets into some of these issues.

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Samuel A. Goudsmit – Session II

December 7, 1963

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 circa 100 atomic and quantum physicists. Subjects discuss their family backgrounds, how they became interested in physics, their educations, people who influenced them, their careers including social influences on the conditions of research, and the state of atomic, nuclear, and quantum physics during the period in which they worked. Discussions of scientific matters relate to work that was done between approximately 1900 and 1930, with an emphasis on the discovery and interpretations of quantum mechanics in the 1920s. Also prominently mentioned are: Gladys Anslow, Robert Fox Bacher, Ernst Back, P. A. Boeser, Niels Henrik David Bohr, Hendrik Brugt Gerhard Casimir, Walter Colby, Dirk Coster, G. H. Dieke, Paul Ehrenfest, Albert Einstein, Enrico Fermi, George Hartwig de Hass, Werner Heisenberg, David Inglis, Edwin Crawford Kemble, Ivan Robert King, Oskar Benjamin Klein, Ralph de Laer Kronig, Alfred Landé, Otto Laporte, T. van Lohuizen, Hendrik Antoon Lorentz, Fraulein Mensing, Edgar Meyer, Robert Andrews Millikan, J. Robert Oppenheimer, Friedrich Paschen, Wolfgang Pauli, Linus Pauling, Isidor Isaac Rabi, Harrison McAllister Randall, Adolf Smekal, Arnold Sommerfeld, Thomas, Uhlenbeck (George's father), George Eugène Uhlenbeck, Albrecht Unsöld, W. van der Woude, Vry, John Wulff, Pieter Zeeman; Universiteit van Amsterdam, Rijksuniversiteit te Leiden, University of Michigan, Teyler's Museum, Universität Tübingen, and Universität Zurich.

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Kuhn:

You remembered a couple of things that you thought had been omitted[from the previous tape, 94a].

Goudsmit:

On the record, I think, to make it complete, I ought to mention two people. First, from my early youth, one person who probably had a great influence on me but I am not sure - - - my grandfather, my father's father. My grandmother was semi-illiterate. I always tell the story that when my cousins came from Paris and brought a little dog along, she absolutely refused to believe that the dog understood French. Honest! I still remember that as a child. My grandfather died before the First World War so I didn't know him well at all. I was, probably about eleven years old.

But he was a very interesting man, from a very poor family. The story is that he ran away from home and traveled a lot. When I knew him, he was the tourist guide for the two swankiest hotels in The Hague, at a time when only special, rich, important people traveled and when a tourist guide is not what it is now.

There were no tourist agencies, no buses and so on with hundreds of tourists going around, and his autograph book, which now was lost in this war, was a marvelous collection of very distinguished names. Really, a very fine man, completely self-educated, learning languages all by himself; even when he was 76 or 77, he was still studying Spanish by himself. He was the one who introduced me to the art galleries. He took me around as a little child, and said, "I want to show you these beautiful paintings." In retrospect, his taste was not what my taste is now: he would have liked Whistler's Mother. But he took me to the art galleries in The Hague.

He had traveled so much. And I remember that his death affected my father very much, it may have affected me as a result. Later on, when I went to high school and the university, my sister and I often said, If grandfather still had lived, he could have helped us, he could have told us about this, or he would have appreciated it. He would have been proud of me. So he must have had an influence on me indirectly that way, and also in introducing me to the art galleries.

Kuhn:

Your sister, I take it, did want you to go to the university, she was also pleased, and when she spoke to you about your mother's almost taking you out, she was on your side?

Goudsmit:

Completely. But she was taken out of high school by my mother. The other person I forgot again is, during my student days, the influence, the contact I had with Dieke. He belonged to the commuters, and we were together practically daily.

Kuhn:

Was he in your class in effect?

Goudsmit:

Maybe possibly a year later, I don't know exactly. But he belonged to that little inner circle around Ehrenfest. He came from Germany. He was really from Dutch parents and had a Dutch passport, but had never, as far as I know, been in Holland before until he became a student. He had relatives living in The Hague, a cousin, where he made his home and commuted. I often spent afternoons and evenings at his house, and he used to come to my house, and we worked together quite a lot and talked about physics. I still remember the day he phoned me about the hydrogen molecular spectrum, that he had found the solution. I like to tell that, because it also throws a light upon me I again must stress that I really am not a mathematical physicist or a theorist, and I did not understand all the implications and all the model but he had done something which appealed to me enormously.

The lines of molecular hydrogen fall in little groups, and people had tried to fit a formula to them, but it didn't fit very well and didn't make any sense. Dieke had gotten interested in that kind of spectroscopy just by reading about it, and once he phoned me — I don't even know whether he remembers it — and he said, "Sam, I think I have found the solution. These bands-"— the so-called Fulcher bands because of a man, Fulcher, who had measured them and tried to fit them in a formula - "that is wrong."

You must take the first line of each band, and that is a real band; then the second line of each band, and the third line of each band. It's because of the hydrogen moment of inertia being so small, the rotational distances were so much larger than in any other band spectrum compared to the frequencies." I did not understand the implication. I probably didn't even know quite what a moment of inertia was, with some exaggeration. But that was the kind of puzzle which immediately struck me as a great discovery, you see — solving a puzzle. By just arranging the lines differently, suddenly the whole thing cleared up, and as you know —.

Kuhn:

When was this now?

Goudsmit:

That must have been 1924 or so, you must find out from Dieke's paper. It must have been about that time, before he went to the States.

Kuhn:

You do something like that later with the calcium fluoride line or something of the sort.

Goudsmit:

Ja, but that was no good, that was no good; that was second-rate. I did that mainly in order to learn some newer experimental techniques in Zeeman's lab. I wanted to excite a molecular spectrum, to measure it; that was no good.

Kuhn:

The paper I'm thinking of —.

Goudsmit:

Is an experimental paper on the calcium fluoride bands.

Kuhn:

Yes, I guess that was the one.

Goudsmit:

But this Dieke — the way it hit me; that was the kind of thing I could recognize as important, whereas those Pauli papers I did not recognize as something which was a contribution because it was outside the way my brain worked. That's why I like to tell things [like this]; it was just marvelous to see people do things like this: "Now if you take the first one here, and then the second one and call that a band, the formula fits, the constants come out to the right value." So I was enthusiastic: I probably encouraged Dieke no end. You see, I had published already a number of papers on spectra, so he confided in me right away. And that became his life career - — hydrogen molecular spectrum, it was very nice. So—-.

Kuhn:

Was he in other respects also the same sort of physicist you were, or was he —?

Goudsmit:

He probably was the same sort of physicist, but he was wiser. He right away went over to experimental physics, which I should have done also, directing experiments, analyzing them. But I did not have the full opportunity, and when the spin came, henceforth, I was type-cast as a theorist. That was a pity, as far as I am concerned. But I'm sure that Ehrenfest realized it at the 'beginning; that's why he sent me to Zeeman.

Kuhn:

One thing we didn't do enough of yesterday: tell me something about Zeeman, about Paschen. I don't know whether Back was there when you made your first trip or only later.

Goudsmit:

No, he was always there.

Kuhn:

Also about Ehrenfest. George has said so much about Ehrenfest I don' t know how much you want to add, but you've said things about him. But nobody's told us much about these other men. I'd be interested in knowing what they were like as human beings, as scientists, but also what their attitude was towards this whole theoretical development that was going on in the time, the extent to which they knew it, the extent to which their work was guided by it. .

Goudsmit:

Zeeman was a man who was rather distant, very formal, very aloof, very friendly because he had learned to be friendly. He always looked to me like a kind of beneficial old rich man who looked down a little at you, very nice and friendly like a grandfather talks to grandchildren. He did not understand the modern physics of the day, there was no doubt about it. He was not active in physics when I knew him; he came to the laboratory at most one hour a day, gave a lecture on spectra lines which I never attended—maybe once— to the students, and looked around the lab, a little bit, patting people on the back, and then going home again.

Occasionally he had these semi-formal dinner parties where the assistants were invited and had to give the maid a large tip, more than it would cost us to pay for our own dinner. I was invited out to his country home a couple of times, but it was very distant. His influence on physics — he was just a European man; because he had done great work once, he was respected and invited and he traveled and he got honorary degrees. But he did not take part in anything and did not influence anything. What I learned in Amsterdam I learned from his assistants.

Kuhn:

Who were the assistants at that time?

Goudsmit:

The man at that time from whom I learned real experimental technique was a man Van der Mark who left to go to Phillips and did important work at Phillips with van der Pol the radio man, all experimental work. From him I learned most. He was the man who knew how to run the magnet and how to do everything, how to attend to a large switchboard, batteries, that all the normal theorists have usually been afraid of. I learned that, I learned to walk around, to talk to the men in the shop and tell them what tube to make for me. I learned more about the development of plates and measuring, and all kinds of techniques, mainly from Van der Mark.

Then there were some other fellows around there from whom I did not learn much. So that about Zeeman; it's very little. You can already see in that photograph I published of him — I don't know, that is really characteristic, can't help it. He was exactly the way he looked — awfully nice, but distant. I remember I was told that in later years he wanted as his successor a Nobel prize winner. That is why I was really never considered at a time when I might have been considered, according to Ehrenfest. This story was told me by others, and I don't know how reliable it is. But it's very lucky for me, of course, that I wasn't considered at the time, because then I would have, ended up in Auschwitz. In the early years I would not have been able to resist this particular position which really was the kind of thing I dreamt about as a student: if I ever get into academic work, that's where I belonged. I had the mistaken idea that there was no change in physics. I think I've told you that, it is not on the tape, but I've told it in various lectures, that my dream, when I first published these papers, was that real immortality consisted in having a footnote mention in Sommerfeld's book. And that I made it.

I wrote it even in an editorial in Physics Today, so you can find it there. And how surprised I was that Sommerfeld' s book, nobody knows of it anymore! That was really my dream; I wanted a footnote in Sommerfeld' s book, and perhaps someday I'll be good enough so that I can really get a high position in Zeeman' s lab. Maybe even be his successor. It was a kind of a dream. Some of the students who were there were assistants with me—I remember one said to me, "Sam, you look as if you belong here. When I see you come down that stairway you look just like Zeeman. You ought to be the successor here."

Kuhn:

Really?

Goudsmit:

Yes. That was Vry, V -r - y, who told me that. That made me feel good; that was 1923 or so. But I never got the job offer until it was much too late. The reason was that I wasn't good enough as far as Zeeman was concerned. He probably was right; because if I had done it, they'd still be doing spectroscopy: this is wrong. Now they do other things. Now you want to go on to other subjects. You want to ask about Paschen?

Kuhn:

Ja, Paschen and Back. Were you at Tuebingen twice before you came to this country?

Goudsmit:

I went at least three times there. Once for just a day visit, which was a very important visit, I mentioned it, where I was introduced to some of the other people. I think Back was there already. Then I was there a couple of years later for a summer. Then I was there again in 1927, and then from there I came to this country. Then I went back again after I was in this country, so I was there several times.

But when Paschen was there it was only the first two times. Paschen was one of the first really great men I knew, because he had such a liberal worldwide outlook. I do not know whether he had traveled very much, but nevertheless it was known to me already that he had been somewhat in trouble because he had kept contact with his American friends during the First World War.

Kuhn:

Really!

Goudsmit:

Yes. Many Americans had gone there. Paschen is responsible— I didn't know that at the time, of course—for the University of Michigan infra-red work. Old Randall went to Germany and learned infra-red technique with Pasehen. Paschen in those days used an old-fashioned galvanometer, a Pasehen galvnometer, and he built one for Randall, or Randall built one there under his supervision, and brought it back in his hands — so they tell at Michigan — and set it up at Michigan and that started the famous infra-red work at Michigan. I think they tried to invite Paschen a couple of times, but I don't think he traveled much. But he had a very liberal outlook. He told me already at the second visit that he had two assistants, Back and Schuler and he said, there was one thing he did not like about them: they were very narrow-minded, they were very chauvinistic. Schueler was his son-in-law, and they broke up — Schueler is still in Germany.

These people change with time — they were very friendly, absolutely. I never noticed anything of this narrow-mindedness. Back told me that he was very anti-Allies, the reason being that he came from a wealthy family in Alsace and, of course, the French —. Schueler was just German, I don't know what. Back came from Alsace. His family had large property there, which after the First World War was just taken over by the French. He lost it all, including his library, so he was always very bitter, he said. But it never showed up, because he was such an extremely friendly, kind-hearted man who'd cry at the drop of a hat at injustice. He was exaggeratedly polite. It was very difficult to get rid of him at times. When you saw him in that long street in Tuebingen at the other end, he'd have already taken his hat off, you know, that kind of a man, a little exaggerated. He was also somewhat unworldly. I can only give it by means of an example.

He was not too happy about his position, even though he did beautiful work, you know how it is. Once he was summoned by the Minister of Education, or whoever the man was who was in charge of the state of Wurttemberg, because they wanted to promote him. He . talked with the man, and everything went off quite well. Then the man said, "How much do you earn now, what is your salary?" And Back gave him a figure . Then he said, "O.K., we'll add so much, we'll make it higher now." And then when Back came home, it turned out, of course, he had given him the wrong figure, much too low and with the additions, instead of being a promotion, it was a cut in pay. Now you see that was a typical, just a typical kind of Schlemihl Back was.

Now whether that was remedied later on —- it was most embarrasing to him. He didn't know what to do about it… He didn't dare to go back and say, "Hey, I made a mistake," he didn't know — That was typical of him. According to John Wulff, he was also lazy, but I don't know whether that was really so. But you had to hold the whip over him to make him work. Back had worked with Paschen and was a precisionist when it came to the experiments, and had done beautiful high precision work in spectroscopy … higher resolution, better sources — but he was not interested in the analysis. If you told him this is interesting to do, he might do it if you kept after him; and the hyperfine structural work really only succeeded because I left a secret agent behind, John Wulff from M.I.T. After I left there was still a lot to be done.

That was now in '27 and '29. John Wulff was there, I said, "you get interested in that, this is nice work. You see that Back does it." And he did! In those days already we communicated occasionally by cable, that was a novelty, trying to get the results sent to me by cable, But Back, I liked him, a little strange as I said, a little bit of a schlemihl, overpolite, but nice. And later he got a job in a small school where he was his own boss. He liked that for awhile, but then he noticed you couldn't do nice work, so he came back to Tuebingen later on.

Kuhn:

But this was after Paschen was gone?

Goudsmit:

In those days Gerlach had already taken over. Paschen was there only the first time I was there, and then Paschen went to the Bureau of Standards in Berlin, already in '26, I think.

So when I was there in '27 Paschen wasn't there anymore, but Back was still there, and I worked mostly with Back. Lande was there, and Gerlach was there, and Fraeulein Mensing was there. Those were the people I had close contact with. Lande was difficult, Gerlach was nice and friendly, and authoritarian. Back was the one I worked with, and Fraeulein Mensing was the one I discussed physics with; she was the theorist. She worked for Lande, but she was more communicative.

Kuhn:

Are you in touch with Fraulein Mensing now, Mrs. something or other?

Goudsmit:

Mrs. Schuetz, no, she is in the Soviet zone now.

Kuhn:

Could you write?

Goudsmit:

Ja! Ja!

Kuhn:

Well, look, there's a question I think would be worth asking her —[Interruption] Just a little bit more on Back, and also on Paschen. Again, how closely was either one of them concerned with what was going on in theory? To what extent were they—?

Goudsmit:

They were not. Back definitely not. And Paschen in those days —. No, there was quite a gap between experimentalists and theorists. The experimentalists, I had the impression, were always very pleased when their experiments were important for theorists. I believe that they had the impression that after all — I still have often — experiment is more important than the theory.

Kuhn:

But I gather from what you say that Back at least, and I think the same thing'S true of Paschen, were quite willing to have suggestions from the theorists, about what it would be worthwhile to measure?

Goudsmit:

Ja! But they also did it the other way around. They believed, like I always did, that by improving techniques you would hit upon new fields and open up new questions, and also get new answers. I am convinced that they worked in that direction: get a better source, better magnet, better field, better resolution in spectroscopy; that's the main motivation. Improving the old work would give new results.

Kuhn:

When you worked with Back — this is obviously ahead of the, story — on the hyperfine structure, did he care at all? The papers, after all, were finally analytical papers, giving quantum numbers, values, and so on. Did he look at that and criticize that?

Goudsmit:

No, no, definitely not.

Kuhn:

Then his name is there because the data is there?

Goudsmit:

Because the data is there, and he would advise me about the reliability of the data. I would be willing to say the interval rule doesn't hold, or the interval rule holds with certain precision, and he'd say, "No, it does not hold, I am sure these deviations are real, because I have, measured them" So from that point of view he would criticize the statements I made about the data, he'd criticize very severely.

Kuhn:

But never the identification of a line with the following quantum numbers?

Goudsmit:

No, no. He was very little aware of that.

Kuhn:

Do you think Paschen was more aware of that?

Goudsmit:

Pasehen was more aware of that, ja.

Kuhn:

Very briefly, I think in 1925, you went to Copenhagen. Was that before —?

Goudsmit:

No, it was after the spin.

Kuhn:

Because you were there again in '27 to do the thesis, Right after the spin?

Goudsmit:

I was invited by Bohr to come to Copenhagen after he had been at Leiden to talk about the spin, so — I arrived there in January, I believe, of 1926.

Kuhn:

Then now let's go to the spin. Or not quite yet to the spin. There's this whole series of papers, your paper in Physica, your and George' s paper in Physica, both Of them now on the contradictions in existing theory. The one with George is on the hydrogen-helium spectrum, and before one gets to spin, it's important to know how those papers come into being, because they set up so much of the problem. And as you remember in your own paper in Physica [Paper No. 20] you list sort of fiye problems, and you said four of. them you can get rid of with the Pauli theory, and then there is the fifth one, which is the riddle about the behaviour of the X-ray and optical doublet. This is the one then that spin takes care of. Those are very much of a piece.

Goudsmit:

This paper wasn't bad, I read it yesterday, this summary paper [No. 20] in Physica. I'm surprised!

Kuhn:

Now that paper clearly relates to the paper with George just before it?

Goudsmit:

Ja, but this [summary paper] was worked on, you see. The summary paper, which was published just after, was the one I was supposed to discuss with George. Ehrenfest knew I had been assigned to write this.

Kuhn:

Who gives an assignment like that?

Goudsmit:

I don't know who it was. Probably Fokker who was then the editor of Physica. I had written a summary article before; I had given a talk in Amsterdam for. the Physical Society on that stuff, and they said, "why don' t you write the article?"

Kuhn:

Physica was the organ of the Physical Society?

Goudsmit:

Of the Dutch Physical Society. I think it was Fokker who asked me, and I was of course flattered and right away started to do it. Ehrenfest knew about that — it was such a srnall circle. So when George Uhlenbeck came back from Rome, Ehrenfest told me, "You get together with George, and tell him about this. Have him help you write it. In that way he can learn, can be brought up-to-date on all this vector nonsense, cosine pest, or whatever it was."

Kuhn:

Was that Ehrenfest's attitude toward —?

Goudsmit:

Ehrenfest's attitude was that it was a necessary evil to know all this. He was very nice to me, he never rubbed it in to hurt me, but you could tell. He thought that was the only thing I was good for. I remember one incident. I remember the detail. He was talking in a colloqiuium or in a discusion about a statistical problem which he hadn't been able to solve. A few days later I came with the solution, and he was so surprised, He said, "I would never have expected that you could do that." Already then, you see I don' t know what it was, I think it was one of the Bose statistics formula where he wanted a model which would give this result, because he and Einstein had guessed what formula to use, but they hadn't found a model which would give, by ordinary statistics, that same result. And I found one. I do not know exactly what it was anymore.

He was so surprised. But this stuff he'd think I knew and he didn't have to know,so he left it to me. So I had been given the task to keep George up-to-date. And I'm still surprised when I see it, because I had always in my memory an idea that there existed a summary article with George, and that this was it. That the idea was that we'd be co-authors at that time. Why I am the only author here I still don't understand. When I read it now I must confess that I have the feeling that George contributed more to it than I'm willing to admit. In this summary article there are references to papers, and, you know, I used the trick everybody used, and say, "well, we shall not do this - derivation in this article" The real reason was that I wouldn't know how to derive it. I'm sure that my reference to some of those papers by Pauli — I'm not sure, but I now guess — probably was George's influence. He had read them and told me what was in them.

I'm a little bit surprised that I'm the only author here. But already I see the influence. The only way I can find out is if I had a copy of that article I wrote a year earlier by myself, whether it has similar things or not —- My dissertation is full of references, where Ehrenfest even in the final exam said, "I know you didn't read that stuff, you wouldn't understand it. That's just for padding. Here are two of Dirac' s papers, and so on — And I did it already then.

Kuhn:

Is it possible that when you remembered last time we talked, that you, hadn't known the Pauli paper, that this means that, in fact, George brought this in and that that's why you use it here?

Goudsmit:

I would not be surprised. That is my sent interpretation. It's most likely, because it was not up my alley.

Kuhn:

Did you read the Zeitschrift?

Goudsmit:

Ja, ja!

Kuhn:

Because this was generally a very theoretical journal, as compared with the sort of thing you —-

Goudsmit:

I read it. But what does reading mean? I read only those articles which were of immediate interest to me, and which made me rush home and see whether I couldn't write another paper.

Kuhn:

But you probably found more articles that did that in other journals?

Goudsmit:

No, in the Zeitschrift. All the spectroscopy was published in the Zeitschrift.

Kuhn:

But not experimental spectroscopy?

Goudsmit:

No, experimental spectroscopy. But I was interested in the interpretation, and guides to interpretation were there, and then I'd rush to whatever it was, say the stuff that came from King in Pasadena. I think he published in the Astrophysical Journal. I said, "Gee" Maybe these people in Germany haven't seen that. And I'd get the Astroiysical Journal and see whether I couldn't get something about the iron spectrum out of that. It was very childish.

Kuhn:

What else did you get things like that out of?

Goudsmit:

Occasionally Physical Review already. I remember flattering Gladys Anslow once because I thought gee, a girl with such a beautiful name writing a paper on the doublets in sodium! She had measured — I had forgot the paper now. She was so flattered. It was one of the first papers I read on the subject. She is retired now, but she was at Smith College for many years. It wasn't until thirty years later that I finally met the beautiful Gladys Anslow; you know, it sounded to me very romantic. So I read Physical Review but mainly Zeitschrift fuer Physik, Annalen der Physik, Antrophysical Journal Phil. Mag. —those were the journals one looked through.

Kuhn:

This paper makes it very clear that there's deep trouble. It is not clear in any of your previous work I meanit's clear that there are spectra that are, giving you trouble, and that there are multiplets that are-not responding as well as they ought to to the application of the "g-factor" with Russell-Saunders coupling, this sort of thing. But here the problems clearly go deeper. Was that a surprise to you, as that something you came to realize in the course only of doing this paper?

Goudsmit:

No, I think it was my way of looking at things. I don't know, I sometimes tell my students that, when you want to get into research? that means you- want to write a paper, get your name in the Physical Review; what you do is you read other papers, but in the back of your mind, "It must be wrong," or in the back of your mind, "It's incomplete.'1 If you take that' attitude — it sounds 'like a mean attitude - - it may lead you to something where you can make a ëontril5ution. You must not take the attitude, well, so-and-so has done it, so that's the last word. That was an exaggeration of the Ebrenfest attitude. So whenever I write things I really always look for what isn't done yet, is there anything I can do? It probably was part of my own attitude, always looking for what hasn't been done yet.

Kuhn:

I would want to say that that describes a good deal of your earlier work very precisely, as I see it, but that here there is more in the sense that here you were pointing not to incompleteness, not to a wrong result, but to fundamental inconsistencies.

Goudsmit:

And that must have been the influence of George. I can't prove it; but it Wasn't in my nature.

Kuhn:

Even by the 1923 Bohr-Heft of Natturwissenschaften, which you surely read, there are plenty of people – there's a little paper by Lande on the Aufbauprinzip and what's the matter with it, and the definite inconsistency there. Was that a sort of problem that had worried you?

Goudsmit:

Ja, ja. I think it's mentioned here.

Kuhn:

So you don't think that there is a change in your own mind?

Goudsmit:

No. Except that it's better and deeper and I suspect that it is George's influence, because in 1931, on hyperfine structure, I also wrote a paper, at that Rome Conference about the difficulties, all those things which did not fit in hyperfine structure. So it was always my attitude, because that was the only way I could hope to make a contribution, to find that somebody else had done it wrong, or incompletely. So the attitude is normal, but that it comes out so well, as I see it now, must have been George, listening to him. It made it necessary for me first to organize it better because now I had a live audience to try it out on, and the questions he asked, of course, were the most important things.

Kuhn:

Do you remember particular things that came up as you went over this area?

Goudsmit:

No. Only the skepticism he had. He didn't understand it, he didn't believe it. He knew classical physics. He saw all the classical difficulties which I honestly could not appreciate. To me these Bohr rules, "Fine, well, that's how nature works. If we don't understand it, we have to wait a few years." As I told you, I didn't know the theory of electricity and magnetim well enough to really worry about the fact that the electrons should spiral into the nuc1eus.

Kuhn:

But surely that sort of a problem was very old and even George cannot have been encountering that sort of problem for the first—time?

Goudsmit:

No, but he couldn't get over it, and I could get over it. Let's put it that way: it didn't worry me. I didn't see that that was a deep problem.

Kuhn:

That obviously was a deep problem, but it was not the sort of problem that emerges in this paper, or that gives this paper its form, nor is it the sort of paper that leads from this to the hydrogen and helium spectrum paper and on into the spin. So if George was asking questions that were formative with respect to the direction in which this work goes, they would have been more about inconsistencies in the quantum theory.

Goudsmit:

No. Maybe it is that by his asking, I was able to clarify my thought, and I had still one dominating influence. I was able to brush these things aside and see only the positive contributions in ideas of spin and these vectors. That must have had something to do with that rumor — I don't know if it is a rumor that George wanted to withdraw that one time.

Kuhn:

The spin paper.

Goudsmit:

Ja!

Kuhn:

Well, we'll come back to this with him this afternoon. He says you both did. He said after Lorentz —.

Goudsmit:

Because I never saw the Lorentz business. I probably was in Amsterdam at that time. I didn't understand those things

Kuhn:

He tells the story, that when Lorentz came back, he showed that the electron couldn't have these properties because of the radius that would be necessary. The two of you went to — I think, he said, the two of you, I'm not sure —

Goudsmit:

Maybe Ehrenfest and he.

Kuhn:

The two of you went to Ehrenfest and said Lorentz has showed that it's nonsense. We'd better not publish the paper.

Goudsmit:

I don’t believe it!

Kuhn:

And Ehrenfest said, “I sent it off already.” And then he said, “You’re young, you can afford a foolish mistake.”

Goudsmit:

To me he said, “You have no reputation yet, you have nothing to lose.” That’ s what I remember, but I do not remember having wanted to withdraw it, because to me it wasn’t so important. To me it was just another rule to understand and explain the complex system of spectra. That is all it meant to me. I never really understood it in its importance at that time. It wasn’t in my nature; definitely not! It was just one of these marvelous rules; it was no more important than that phone call I got from Dieke two years earlier, saying “If you put these lines in that order instead of in that order, it suddenly fits. All the formulas and begins to make sense.” I personally never had the feeling that it was more than that.

Kuhn:

This is a little odd, in the sense that if you look at both this Review paper and the paper on the spectrum of hydrogen and helium, in both of these; I think … I’d have to study it more carefully to be sure you’ve got a scheme using Pauli and the electron quantum numbers which enables you to do everything qualitative that you do with spin; the advantage of spin, the remaining problem that spin is that by talking about the way magnetic interactions will behave, you can then get some quantitative results including z^4 and so on. But in that sense, for the analysis of spectra, in terms of what rules give you, you’ve got it already without the spin.

Goudsmit:

No, no, you do not. Because you have to asume that duplicity, every time you add an electron something suddenly doubles-up.

Kuhn:

You’re quite right, even at that qualitative level it would.

Goudsmit:

And I’m also so convinced … I can’t prove it, except with my hypnotist perhaps, that it was George who said it is spin, because when I convinced him that the electron had four quantum numbers, and when I did it my way, as I, had published it in the Zeitschrift fuer Phrsik, not the Pauli way, he said. but that means it has four degrees of freedom, something like a spin."

Kuhn:

Which do you mean by "your way" as you published it in the Zeitschrift fuer Physik?

Goudsmit:

There is one paper — and here I. am just like all the other fellows — for which I never get any credit, and that is- a paper in l925, Zeitschrift fuer Physik, where I point out that if you apply the Pauli principle, not with the quantum numbers Pauli used, but with the quantum numbers which are now used, that then everything becomes so much simpler — that you get the multiplets out the proper way at once. And I give two or three examples.

Kuhn:

Ja. This is the paper "Uber die Komplexstruktur der Spektren," [Zeits. f. Phys.32 (1925) 794-798].

Goudsmit:

Ja. And then it was, of course, immediately adopted by everybody, and poor Hund wrote a 1ong paper, but he gets credit for having analyzed all the spectra. But he had written this just before he saw mine. And so that's still done in a most complicated way. The results are, of course, the same, but he has to use all kinds of sum rules, in going from a strong field to a weak field.

I was the first to say, if you put all the spins together, you get a total spin and if you put all the orbits together, you get a total orbit. But I didn't call it spin or orbits. So that was always the paper I thought a real contribution, in my eyes, being a spectroscopist and analyst of spectra. So I told. George about it.

Kuhn:

This was presumably done before George got back from Rome?

Goudsmit:

Ja, ja. It was published in May.

Kuhn:

It was submitted in May actually.

Goudsmit:

Ja, and also published I think — I don't know for sure. Anyway it was just about the time when he came. And it was that paper I had written Kronig about. I'd sent the ideas and maybe even a copy to Copenhagen. And I'm still surprised that Kronig didn't consider this as an additional confirmation of his spin idea. But he didn't, because the answers I got from him were about entirely different things, as if he was not interested at all.

Kuhn:

At what point in this work over the paper, what role may George have had with respect to this — not yet spin, but this next big step with the hydrogen and helium spectrum, the insistence on treating hydrogen and helium the same way the alkalis are treated, which is an old idea of yours; but now in a different form because you've got more stuff ? What's interesting here is that the electron quantum numbers and so forth don't show up, what this takes off from is this paper of Wentzel.

Goudsmit:

The paper of Wentzel was one I, of course, didn't understand, but which intrigued George no end. I still don't quite understand it. People were plagued by these half-quantum numbers which showed up every so often; Schroedinger had shown it already. Wentzel had invented some new rules, where you average between whole quantum numbers in the hope that that would explain the half-quantum numbers. And really –

Kuhn:

Now Heisenberg was doing a lot of that too!

Goudsmit:

Also, ja. But Wentzel seemed to have formalized it, and probably was all wrong; we know now it's all wrong, but it must have fascinated George. I did not understand the paper, because in retrospect you must admit that this hydrogen-helium thing we could have done without the Wentzel paper. But hanging it on to the Wentzel paper and saying this leads to the conclusion that if this is true for hydrogen where one had no trouble with half-quantum numbers, everything fitted well —. If this was really fundamental, and if Wentzel is right for the complex spectra, he also ought to be right for hydrogen. If that is so, then you come to such-and-such conclusions. I haven't read Slater's paper in a long time, and I don't know what he based it on.

Kuhn:

My recollection is that he's got the same idea, that they ought to be the same. He doesn't have the Wentzel paper, I'm quite sure, nor does he have this thing that you've, got. I'm quite sure he's got nothing about the lines that do and don't show up, which makes this, I think, particularly impressive.

Goudsmit:

Of course, that impressed me again, because I knew these lines so well. That I learned from Paschen.

Kuhn:

George says, as he learned this stuff from you, that he thinks his contribution to this was that from the very beginning he said it is absurd to treat hydrogen one way and the alkalis another. Does that fit with your recollection?

Goudsmit:

Quite possible. I don't remember it anymore. But it's only in retrospect that I see that this idea shows up already in my older papers. But I do not know whether that is an accident. It certainly was not consciously so, but that it is hindsight. You can say that now that he has done this, we can trace it back to his very first paper in 1921; that may be purely coincidental, and it may simply mean that I always stuck to the interpretation o spectra. I was fascinated by that.

Kuhn:

You have no real recollection? — we touched on it yesterday — of an interim period in which the development of the magnetic interaction theory bothers you about your older relativistic interpretation?

Goudsmit:

No. I believed in that of course, from the very beginning, and then believed in it even more after Millikan and Lande independently came to the same result. No, no recollection. I never found difficulties in that because I didn't know enough, and I've often explained the success we had with the spin as caused by our ignorance. I knew the analysis of spectra and everything like that very well.

Then my previous papers — and what I told George, if you only look from the point of view of the analysis of spectra, and the formulas, then it's a perfect fit, it's almost a natural conclusion and you don't understand why I didn't do it already in May '25 before George came, or why Kronig didn't do it after I'd sent him that paper. It's a natural extension. The other people were so much more learned… They could compute the fine, structure, which we did not. But that was all they could do. They probably did not understand the Zeeman effect and the sum rules and other things. They may understand it —

Kuhn:

Heisenberg and Pauli surely did –

Goudsmit:

Pauli did, ja. And that's why it's such a surprise. But he also knew the other thing. I'm not so sure that he was as well versed in all this vector business as Lande was.

Kuhn:

By then, I think, he was, but not in the spectroscopic data.

Goudsmit:

Ja. He didn't know the "forbidden" lines and things like that. And they also knew how to compute the self-energy of an electron which was a concept which I didn't understand, even though we put a footnote in there. And I just now for the first time see a letter [of 1925] from Fermi, where he writes us about that, how do you get around…? I've never seen that before. We simply didn' t know, But that was the thing Loréntz knew, and that's the first thing he thought about. To me that "forbidden line" in hydrogen was far more important. So you see how ignorance helps.

Kuhn:

On this paper on the hydrogen and helium, do you remember other people's reactions to this? It comes so shortly before spin.

Goudsmit:

No. I don't remember at all, and I don't know why we published it in Dutch —- haven't the faintest idea…

Kuhn:

It raises a question I had meant to ask you before —- maybe we'll interrupt this, but we've got to come back right to this point —- I'm puzzled about why you published which papers where. Some of these things come out in Nature and Naturwissenschaften. Other things come out, things which in some wars look to be a good deal more important, come out just in Physica in Dutch and don't come out anywhere.

Goudsmit:

My explanation at the moment is that when you do a thing like that, you don't know how important it is, or whether it really is important. That's the only explanation I have. It's my fault, ja.

Kuhn:

What about this business of publishing in both Nature and Naturwissenschaften?

Goudsmit:

Naturwissenschaften in those days was the more natural thing for a pre1iminary note. Also Ehrenfest and Hollanders were oriented at that time towards German physics. Nature I believe, was under the influence of Bohr, who was more English-oriented. Don't forget the Nature —.

Kuhn:

But your early multiplet analysis —-

Goudsmit:

The early stuff. Early multiplet, because of Fowler. That was something along the lines of Fowler in England, so I asked Lorentz, would you p1ease send that to Nature?

Kuhn:

And you'd send it to both?

Goudsmit:

I don't know, did I send —? That was also a bad habit. Some of my papers were published three or four times, which was very bad.

Kuhn:

Some of the stuff that you and Zeeman do goes to both.

Goudsmit:

Yes, Zeeman always wanted to have it everywhere.

Kuhn:

And —. Yes, NO. 9 I would say this is, "The Magnetic Resolution of the Scandium Lines."

Goudsmit:

And 8 are practically the same, 8 and 9 are practically the same; 10 and 11 are the same; then 11 and 13 are practically the same.

Kuhn:

One of the curious things is that 8 and 9 are practically the sane; 9 goes to - Nature, and is by Goudsmit and Zeeman, 8 goes to Naturwissenachaften and is by Goudsmit.

Goudsmit:

Ja [laughter] I have no idea. I can only tell you that Zeeman had not a damn thing to do with it. That either it was one of these Ehrenfest ideas, take your hat off for a policeman, you know, like Charlie Chaplin. You have him on the job in Amsterdam, he didn't say so, but probably in his subconscious mind, he said, "We'd better publish with Zeeman" or something like that.-—I don't remember the details. Zeeman was more English-Oriented. Are these two papers identical?

Kuhn:

No. There's a little more detail in one or the other, but from our present point of view they're identical. They're reports on the same piece of work.

Goudsmit:

Ja, and one is August 9, the other August 20. Do you know whether one is signed Leiden and the other Amsterdam, or are they both signed Amsterdam? I don't know, I have no recollection. That's politics, that's pure politics, has nothing to do with physics.

Kuhn:

No, they're both signed Amsterdam.

Goudsmit:

Ja, that's where the work was done. I'm sure it's pure politics.

Kuhn:

Well, back to the spin. Again in this paper on hydrogen and helium you've got this terrible important suggestion that maybe the ortho series is triplets.

Goudsmit:

Were we the first to do that or not?

Kuhn:

I'm not sure. I don't know of an earlier suggestion to this effect; there may have been one. I rather think it's in the Slater paper also, and I'm not sure which of those two papers comes first.

Goudsmit:

I think we came a little earlier, but Dutch doesn't count. Might as well have been in Chinese.

Kuhn:

In any case, did you try to get somebody did you write and ask Back to try to measure that, or anybody?

Goudsmit:

No I probably tried to go through the literature and see whether there were any cases of it. And I didn't. I don't know why, either. Don't forget it was summer. I might have asked if the spin hadn't come along, and that took all our attention, and we were asked about that and had to go to Copenhagen.

Otherwise I am sure I would have done something else, I would have written to Back or to somebody, Zeeman. The Zeeman lab. Was closed at that time; this is summer work. I probably would have, But other things took our time and attention, so I never followed it up, but then, of course —-

Kuhn:

Do you know who first did that resolution and how long it took?

Goudsmit:

No.

Kuhn:

Of course, presumably with this as a lead, it would have been very easy to set up apparatus to get that much further resolution on these.

Goudsmit:

I don't know when it was. Then I got off in other directions. There was a paper which made me very mad, from a priority and credit point of view, by Sommerfeld and Unsoeld. They had apparently come to the same conclusion later. They gave us a footnote, I don't know whether they gave Slater a footnote on the first page, and from then on that paper of course was typically German. That's how they get all the credit, the fatter the paper, the more credit you get. There's nothing new in that paper. Then they talk about "unserer theorie." You see, from then on it's "unserer theorie." Typical nostrification paper that was. But I didn't like it at all.

Kuhn:

Do you have any recollection of how the idea of spin itself gets into this, and when it comes into it?

Goudsmit:

I only remember the discussion with George, where I tried to explain the Pauli principle to him, and point out that one quantum number is always a half, that there are four quantum numbers, that he talked about four degrees of freedom, which to me was even not a clear idea. You must honestly believe me, I am not modest, that I am a numerologist in some respects, and so four degrees of freedom, "Sure, four degrees of freedom, what the hell," you see If I don't even know what three degrees of freedom meant precisely, so four, why not, like a spin, you see, now that meant something to me.

And then it clicked again, like the phone call from Dieke. And I'm 99 percent sure that it was Uhlenbeck who mentioned that this must mean the spin, and that I picked it up in the discussion, and said, ja, now everything falls in line, how that whole vector scheme becomes so much simpler, and how you put all those spins together. I'm 90 percent sure that that's the way it happened. But for instance we don't know exactly when it was.

Kuhn:

What's your impression? The note to Naturwissenschaften is dated the 17th of October. Now how long before that do you suppose you'd had the idea?

Goudsmit:

Probably about a month. Let me see, I have some letters from Uhlenbeck at that time. That was 1925. Now let's see. [Checks through letters] No precise date, damn it, this is very rare for this is the fall of 1925, but I don't know when. It must be early fall. I wrote that down. From Ehrenfst. [See microfilm of Goudsmit Correspondence] Probably sent to me to Amsterdam. That is about the spin, you see. I wish I understood this. [Reads] Ja. See, this is an important note. And I wish the precise date. It says here, "In connection with the discussion of your, very nice idea, Uhlenbeck has made the remark that your formula,"— that is both of us, I think he means—"in connection with the Hund formula, that there is a contradiction with the sums, which are not fulfilled. If there is a misunderstanding here, and if it points at a defect in your formulas—" And then there is a note here on the side, "Uhlenbeck has just noticed that he has made a mistake. There are probably only two small deviations. But I would like to wait until tomorrow anyway."

The letter goes on here, "I request therefore that you wait with sending off the manuscript to Naturwissenschaften until tomorrow, until Wednesday evening." See, this must have been sent to Amsterdam; Wednesday evenings I came from Amsterdam to Leiden. [continues reading] "I ask you therefore to derive the permanence of the g-sum from your idea of the electron spin: A," in the case of the alkalis; B, in the case of calcium; C, in complicated cases." Now for me that was duck soup. "Perhaps there is . modification of your g1 g2, g sum synthesis" I don't know —- "I have still two or three small stylistic remarks about your letter to Naturwissenschaften. Best regards, o tentaminando." —-you see, I hadn't done my exams yet.

He rubs that in again —- "yours, P. Ehrenfest." So it seems that I wrote that, and that he just said, "Don't send it off until Wednesday because the g-sum doesn't fit," and then "Oh no, Uhlenbeck has made a mistake!" You see; I think this is significant!

Kuhn:

We must get at that letter again this afternoon with George, because this also contradicts his recollection that the paper had already gone off because Ehrenfest had, sent it in, a story which he has told often.

Goudsmit:

Ja. And here is a letter, a note from Uhlenbeck to me about things I didn't understand, 7

October 1925. [Reads] "Thanks for your letter. I have talked it over with Ehrenfest and as far as the calculation of the magnetic moment of a rotating electron, formulas from Abraham" —- of course I never understood Abraham —- magnetic moments are here, you see —- "then if all the charges are on the surface, then you get a factor two," or something of that idea. If this is right I don't know.

You see that's not a Thomas two, that's just a g of 2. "If the charge is distributed over space it's quite different," and so on. "I hope to be able to talk it over with you Saturday afternoon when I'll come to your home around 2:30 because I come to The Hague only that late. Best regards, G. Uhlenbeck." That's all I have of that period.

Then I got the letters from the outside, including a note from Fermi. Here is a copy of a letter which Bohr wrote to Ehrenfest, and he showed me, and which my ex-wife copied. I guess she wanted to keep it. And then on the back there are formulas—- the vector scheme and so on. That's probably a part of the summary article, it looks like it.

Kuhn:

That may be a part of the Phil.Mag. piece.

Goudsmit:

No, I think it's Physica;this is Physica. On the back is a letter Bohr wrote to Ehrenfest, the nice time he has had in Holland talking about the spin. I also have a copy of —-. Throw this out, that's a bill —- oh, keep the stub —- bill for a book.

Kuhn:

What book?

Goudsmit:

Egyptology, I'm sure. Ja Egyptian dictionary.

Kuhn:

When you defended your Egyptology thesis, did anybody ask you

Goudsmit:

Ja, ja, ja. They did at that time.

Kuhn:

Well now look: the Naturwissenschaften note came out and what happened?

Goudsmit:

Then we got letters, right away! From Heisenberg — and I notice, I saw a moment ago a letter from Fermi, can't find it anymore. Probably misfiled, because it had no date on it.

Kuhn:

They're in there, I found them yesterday.

Goudsmit:

And the Heisenberg letters are in here; there are copies of those too, these are the originals. And Heisenberg said, "What did you do with that factor 2?" And of course we didn't know what to do about it, and it was at that time that of course George had to take over, because that was the kind of thing he knew how to do. Then we were lucky that Einstein and Bohr and everybody came to Leiden. I think it is they who more or less solved it. And there is that letter from Bohr—-.

Kuhn:

Now when you say 'solved it' what do you mean? Because they didn't get the factor of two.

Goudsmit:

No, but they knew in which direction to look —- Bohr. That's the impression I have. There is that letter from Bohr to Kronig, in which he says —- have you got that letter?

Kuhn:

Ja, ja. But I don't think he says in what direction to look.

Goudsmit:

Ja, he says it must be a relativistic effect in there.

Kuhn:

Gee, I don't think so. G; Ja, I'm pretty sure. I have it with me. I thought I had it among this junk because I thought it was relevant. Ja! Let me see what it says. This is [26], March 1926. "Dear Kronig," it's in English "we have often missed you" was it sent to

America? Probably “I have been deeply interested in the spectral problems which you have given, so much thought. The idea of the spinning electron, and the insight it offers in all the difficulties with which we have been struggling lately has been a source of very great pleasure to me. From a discussion after your beautiful lecture on the Zeeman effect, you know how unhappy and desperate I felt over the state of things.

Certainly I had heard then that in a letter to Naturwissenschaften Uhlenbeck and Goudsmit had proposed to provide a basis for a new quantum number by ascribing a vector to the electron itself, but not understanding how this vector could materially influence the motion of the electron to which it was attached, I had thought so little about this idea that I even quite forgot to question you about this possibility in the discussion that evening. When I came to Leiden however, to the Lorentz festival, Einstein asked the very first moment I saw him, what I believed about the spinning electron.

Upon my question about the cause of the necessary mutual coupling between the spin axis and the orbital motion, he explained that this coupling was an immediate consequence of the theory of relativity. This remark has acted as a complete revelation to me, and I’ve never since faltered in my conviction that we at least were at the end of our sorrows.”

Kuhn:

But that’s not the same thing. That’s not the factor of two.

Goudsmit:

No, but that’s essentially it.

Kuhn:

No. That is, you’re right that they’re both relativistic effects, but the Einstein remark is apparently just, if you don’t understand the interaction, just remember that it’s the same thing as if the nucleus were moving around the electron.

Goudsmit:

That has nothing to do with relativity. That is Biot-Savart.

Kuhn:

Ja, or it’s the Lorentz invariance of the Maxwell equations. But I think that’s all that’s about.

Goudsmit:

Ja, but this is what convinced Bohr that one had to look at it from that point of view, I'm sure of that. Otherwise he wouldn't have set Thomas on the job of—-.

Kuhn:

Thomas' story, which I think is probably true, is that Bohr didn't. Thomas' story is quite interesting and very plausible. Thomas says "They were talking about this whole subject, and I just said, 'Look, this ought to be done relativistically.' And they said, yes, I suppose so, but we know the order of magnitude of relativistic effects, it isn't going to make any difference. A few percent."

And he thought, well, he, after all, had done all this relativity stuff before —- "that it would be nice to do it relativistiial1y, so he sat down and did it." I think he was almost as surprised as anybody else that the factor two came out. He just thought it should be, Since you knew relativity, it should be done relativistically.

Goudsmit:

"I have since had quite a difficult time in trying to persuade Pauli and Heisenberg, who were so deep in the spell of the magical duality that they were most unwilling to greet any outway of the sort. It has even been very difficult to persuade Pauli to believe in the beautiful contribution of Thomas which made it possible to obtain full quantitative agreement. And first in the very last days he has completely surrendered." "Under this situation" —- then he goes on.

Kuhn:

Did, you have arguments with either Heisenberg or Pauli? Do you remember Bohr's initial lack of conviction?

Goudsmit:

No, no. I don't remember that at all.

Kuhn:

What about Heisenberg and Pauli?

Goudsmit:

I didn't talk to Heisenberg at that time. I talked to Pauli on my way back to Copenhagen, and tried to convince him of the Thomas two in the sense that I understood it. He was not convinced, he ridiculed me.

Kuhn:

Why was he not convinced? What did he say about the Thomas two?

Goudsmit:

I didn't know enough relativity to be able to convince him. He probably made the same remarks Einstein made to me, who also wasn't convinced. Whom did I tell that the other day to? I'll have to use the blackboard for this. [Goes to blackboard.] I'd give this elementary derivation, which the highbrow say is wrong, but which I have learned from somebody in Copenhagen.

I don't know whom; here is the circular orbit, here is the electron, here is a co-ordinate system with respect to the nucleus and x and y. An electron is going in that direction. This figure is in the plane of the paper too, by the way. Now a moment later the electron is here, with the co-ordinate system in the orbit. You see, I'm moving that way. Now you make a Lorentz transformation.

And Einstein said, "No! a moment later the electron is there! You cannot apply this to the accelerated motion" that kind of thing, and he never let me go on. That it gave the right result didn't impress him, he said, "no, the electron is there." And I didn't know enough highbrow physics to put it into a language so that Einstein would be convinced. And I think Pauli also didn't understand my elementary —-.

Kuhn:

This was after the Thomas paper was published?

Goudsmit:

Was it after the Thomas? No, no, it wasn't published yet, no. You see, it wasn't published yet.

Kuhn:

Ah! Were you in Copenhagen when Thomas —-?

Goudsmit:

I was in Copenhagen when Thomas had made the discovery.

Kuhn:

How do you react to Thomas' story? That it was his insistence that it ought to be done relativistically, that other people said sure that would be nice but what the devil, we know that correction, we know —-.

Goudsmit:

See, I'm again the wrong man because I didn't understand it. Thomas was already in Copenhagen when I came, and working on it. During that January, while I was there, he had gotten the factor two. And I of course didn't understand it, I still didn't understand a word of what he says. And then it was either Kramers or somebody else who translated it for me into that elementary language which I tried to put on the blackboard: that much —I could understand. But I'm told that much is wrong.

Kuhn:

Was Pauli convinced and was Einstein convinced when the Thomas paper came out?

Goudsmit:

Pauli was convinced only in March, and there must be in here somewhere a postcard from Pauli in which he more or less apologizes. I don't know where it is, I can't find it in which he admits that he now is convinced that the factor two is right.

Kuhn:

Thomas was already at work on this before you got there?

Goudsmit:

Already before I got there, ja. Why he was there you must know from Thomas, but I don't know.

Kuhn:

This was fairly usual for Fowler's students.

Goudsmit:

Ja. Somewhere there must be the postcard of Pauli. I don't see it right now; it doesn't matter. That jibes also with the letter of Bohr to Kronig. Just about this time — this is dated March — Pauli was convinced. But I saw Pauli in February, before the Thomas paper was published.

Kuhn:

There's the diagram, on page 7 in a reprint of the summary spin paper. So it couldn' t convince anybody. I think that's the whole story.

Kuhn:

What can you tell me about Kronig in this?

Goudsmit:

Nothing than what I heard.

Kuhn:

Did he ever write you himself about it?

Goudsmit:

He never wrote me himself about it. I can only guess that he probably thought we knew about his work, or that he thought Heisenberg had told us about him. But the Heisenberg letters, which I sent him later, two years ago whenever it was, they don't mention anybody. And in Bohr's letter to Kronig, which is much older, Bohr says Heisenberg says he had heard it from somebody but he didn't know from whom, probably indirectly. Moreover, the natural reaction of Kronig was, of course, that it was wrong. So Kronig published two-papers proving that it was wrong.

He was convinced that it was wrong. If you read Van der Waerden' s paper, then Kronig was right — it was wrong. Very simple! Though he deserves great credit for believing that it was wrong. That probably is correct, I mean, from a mathematical point of view, of course. From a logical point of view of course physics is not a logical science, it goes by evolutions as you well know. Then of course it's the same with Bichowsky and Urey. They deserve at least as much credit as Kronig.

They also had the idea. They didn't publish it until later, and also, in their note, they point out that it can't be right because it doesn't give the right fine structure. You see how lucky George and I were that we didn't know how to compute it. It's pure luck. If we had known, we might have had hesitations. But we didn't. I realized it was something difficult, because I couldn' t do it, and I thought that all the other things fit so well, that if somebody who can compute it correctly does it, it probably will come out correctly.

The main thing I remember having done when Bohr and Einstein were at Leiden, was that I clearly saw how that hydrogen spectrum, which we had published in Physica, fitted in with the spin. And I still remember the diagram on the blackboard in Ehrenfest' s study, and explaining it in detail to Einstein and to Bohr, the new interpretation of hydrogen. Because Bohr hadn't seen it, it was published in Dutch.

Kuhn:

Do you think you had not really seen that relation at all, or just not seen it clearly, or what, at the time of the Naturwissenschaften note?

Goudsmit:

Not seen it clearly. I feel that I had not seen it clearly, because I would have mentioned it in the Naturwissenschaften. But I had not seen it clearly.

Kuhn:

I forget, just when was that assembly in Leiden?

Goudsmit:

November or December.

Kuhn:

To what extent was the piece to Nature a direct product of that assembly? See, that's quite different. The hydrogen is in there, you use more arguments, show more ways in-which it fits.

Goudsmit:

I believe that that came out of these discussions with Bohr and Einstein, the necessity of getting all the arguments in.

Kuhn:

was it Bohr who urged sending a new announcement to Nature?

Goudsmit:

No, I think it was Bohr who finally —- it was written in Copenhagen really, I think; what is the date on it?

Kuhn:

The date's December.

Goudsmit:

Was it really sent off in December? [checks] Copenhagen in January, you see —- ja, we wrote it in December, and I took it to Copenhagen for Bohr's O.K., and in the typical Bohr way, I remember, he changed a little word here and this little word there, suggested something here and there, then wrote this appendix. And he kept it for weeks. Then we took a taxi to the post office and had to send it on special delivery.

Kuhn:

But notice that although Bohr's letter is dated January, your letter is dated December.

Goudsmit:

December. That means it was written before I went to Copenhagen essentially.

Kuhn:

Would it be dated December if you hadn't mailed it until January?

Goudsmit:

Ja. Because I'm pretty sure that they were both mailed together.

Kuhn:

Is it possible that it was the proofs that were returned in January and that Bohr wrote his letter then?

Goudsmit:

No, because I remember discussing it with Bohr.

Kuhn:

It surprises me that Nature would print a letter with a December date line that they had not received until much later.

Goudsmit:

These old date lines were not received dates. They were really when it was written. In those days people didn't care about those details very much.

Kuhn:

You've got eingegangen dates in the Zeitschrift.

Goudsmit:

That is true. This is not an eingegangen date, because the eingegangen date was January, and I don't know when in January, late in January. I was really a little mad at Bohr for waiting so long. But I'm pretty sure that this came out of those discussions at Leiden when it was necessary especially for me to set all the little points straight, and this I was always very proud of. See, that's just the same picture as was in Physica.

Kuhn:

The same picture that's still in most books on spectra. -.

Goudsmit:

Ja, and here we knew about Slater.

Kuhn:

At this point partial anticipations of some of these things begin to come up, and you begin to know about them. For example, you begin to refer to the spin Idea as the "Compton hypothesis:"

Goudsmit:

That was Ehrenfest. I've never read it yet. It was typically Ehrenfest again.

Kuhn:

He can't have known earlier about the Compton effect. How do you suppose he heard about it?

Goudsmit:

I don't know, maybe somebody called it to his attention. But it was again: "Don't forget to quote some important people."

Kuhn:

Do you remember how the Slater thing was called to your attention?

Goudsmit:

That we probably read. Because we looked at the National Academy. I think we were aware of that ourselves. Then you hot ice that note about de Haas. Ja, that's all.

Kuhn:

Do you know anything more about the de Haas' work? No, nothing. There were more things which Ehrenfest made us quote, I think, which were: strange to me. Can't find it at the moment. But you see here are these formulas which came in that letter from Uhlenbeck which I still have.

Kuhn:

Right, in the middle of this, or between the two papers, you start taking up the coupling problem.

Goudsmit:

Ja. That's what we had really been working on. We had probably started to write that before. That came also out of the summer discussions, as a result of the summary article. See, the spin was really the thing which threw us off because of the interest of Einstein and Bohr. I must honestly admit that I at the moment didn't know that was any more important than just the coupling business. The coupling thing we had really discussed and were writing an article about. I think in the coupling paper there's also a footnote to the spin which clearly shows that it was written conceived before.

Kuhn:

I'm not clear myself where this whole business of various forms of coupling starts. It starts before this, some of it.

Goudsmit:

I have no reprint of it at the moment, so I don't know.

Kuhn:

But you've done —-. You've got a paper, either No. 22 or No. 25, in which you begin to talk about other coupling schemes.

Goudsmit:

One didn't know that they were coupling schemes at that time, I believe. It wasn't clear what the meaning was of the höhere Stufe.

Kuhn:

At least you call it coupling in the title of No. 24, which is the one you do with George. ["Die Kopplungsmoeglichkeiten der Quantenvectóren im Atom," Zs. f. Phys. 35 (1926), 618-625] And I suspect that you call it coupling earlier,because you speak of uncoupling. Now this is Heilbron's translation of paper No. 22 —- [reads] "It may however happen that the force of the ion is insufficient to uncouple R and K." Already in paper No. 22. I'm not sure, but I know that there's been some talk about —-.

Goudsmit:

Do I quote other people in that one?

Kuhn:

If you do, it isn't noted here, and I think—-.

Goudsmit:

It's a short note, it seems, it's a letter to Naturwissenchaften.

Kuhn:

I think it would be.

Goudsmit:

October, '25. Ah, but that explains it, of course! That they were simultaneous. That must have been written simultaneously, you see, that whole— we did a lot of—-.

Kuhn:

I ask this because all of these things come at once and I was wondering how they're related to eachother.

Goudsmit:

I don't know why this letter to Naturwissenschaften was not with George T. Uhlenbeck, let's put it that way. It's probably again selfishness on my part.

Kuhn:

I wouldn't think necessarily.

Goudsmit:

But anyway, you see paper No. 22 and paper No. 21, were probably thought about during the same time. That paper No. 21, was received so late in November was only because we had been too busy with the spin. But it was probably written, or at least a draft must have been written, very much earlier, because that was the main result, so to say, of our collaboration during that summer. It was a natural result, writing this article about the present state of spectra, this was a natural result to write about the doublet coupling schemes.

Kuhn:

In your thesis, you make a remark that Heisenberg in '25 first points to the possibility of other couplings.

Goudsmit:

Ja, I think that is right. I think he was really the first one to do it.

Kuhn:

You don't remember where the coupling thing really came in here? You think it was just another by-product of the summary article?

Goudsmit:

It was a more natural by-product of the summary article, the kind of thing I would have expected. No great step, but good systematization. It didn't originate with us, as you point out, it was already Heisenberg who had done it first, and I had been in this business. That was the kind of thing which was in the air, and it was natural, after studying that summary article, to write something like that.

Kuhn:

The generalization of the coupling schemes. You were probably more acutely aware than Heisenberg of how badly the other possibilities were needed in order to handle the spectroscopic results. I don't really remember, I think I've looked at it; but I don't remember that Heisenberg paper at all.

Goudsmit:

I don't remember it either, I don't remember it at all; but there's no doubt that he was the first to point out.

Kuhn:

I hate to leave spin entirely behind us, though I' m not clear that I've got anything else to ask.I'll ask one sort of question. I take it that the conversion of Bohr and Einstein, this support came so quickly that there was never any problem about their resistance? I never noticed that it wasn't a conversion. I was just telling them, and they asked questions which made it necessary to put my thoughts straight, and to bring in everything I ever knew about spectra.

Kuhn:

Now what about the Heisenberg-Pauli hold-out? Did this other you?

Goudsmit:

No. First of all, I did not learn about it; I was only told about it later on, and then I noticed it when I tried to convince Pauli on my way back from Copenhagen. But I was such a beginner. That meeting with Pauli was so strange.

Kuhn:

Had you known him before?

Goudsmit:

No. I never met him before. It was a very disagreeable experience. Later on we became good friends. That first experience was a very disagreeable experience. I have told many anecdotes about it....

Kuhn:

Tell a few now.

Goudsmit:

First of all, he took me out to lunch with people, and then told jokes at my expense and then he had the gallery guffawing, which I didn't like. I didn't always understand the jokes, I didn't understand all the German at that moment. Then he was very much upset about one thing: he looked at me. I had been a poor student, but having been invited to

Copenhagen by Bohr, my uncle had said, "You can't go like this." And he gave me money to buy a new coat. So I looked very neat when I came to Pauli, and he didn't like that. He said to me, "What a beautiful coat you have there, what a beautiful coat!"And he said it in a way that I felt very, uncomfortable.

But then there was one face- saving feature. He looked suddenly up, and said, "Oh, but you' re wearing a theoretical physics hat." [Laughter] I had one of these very old hats, you could make soup out of if you put it in a kettle with water, quite greasy. So that pleased him again. But the whole atmosphere was like that. And I never was able to convince him of anything. It was only a short stop there.

I spent probably two hours with him, that's all, then I more or less fled, went to my hotel, and the next day got on the train and went on. But later on Pauli and I hit it off quite well whenever we met. He was still mad at me for not quoting him on hyperfine structure, that is sure. But he never took it out on me openly, never was mean to me. I traveled with him, all the way to California in 1931, I think, and he was a very good companion.

Kuhn:

Was that making jokes at other people's expense, and having a sharp tongue typical of Pauli, as everybody says? I've never heard of a case in which he led a group in laughing at somebody. Was that also typical?

Goudsmit:

That I don't know; but it was very painful to me, I wasn't used to that. I don't know who was in the group any more at that time.

Kuhn:

It's the sort of thing one would be likely to erase. [Interruption, recorder turned off and on] I had just said you go right from this to byperfine structure.

Goudsmit:

Ja, I went right from that.

Kuhn:

There's a sense in which I'm puzzled that went quite so fast, because particularly with both spin now and the coupling schemes, there was a lot you could have done on multiplets that you couldn't do before. But you jumped right past multiples.

Goudsmit:

It was probably again accidental. This is what happened. I probably was overworked. Anyway, I was in a rather bad state physically at one time. In fact, Ehrenfest worried, and Mrs. Ehrenfest took me to their doctor to find out whether there was something wrong with me.

Kuhn:

What did happen? You do the summary piece on spin, and that is just about all until this piece from, Tuebingen in October of 1926. That, for you, was a slow down.

Goudsmit:

I probably had to work — didn't I have to work for exams? I probably did my doctoral — not the Ph.D., but the doctoral exam at that time. I must find the diploma for the date. That may have had something to do with it.

Kuhn:

No, you passed the doctorate. At least in your thesis you say you passed the doctoral exams in 1925.

Goudsmit:

Did I? I can't believe it. Maybe I did it wrong. Where do I say that?

Kuhn:

In the biographical account at the front.

Goudsmit:

December '25! Gee! Also everything! Oh ja, that's why I could talk with Einstein. He was there at the time, the walk with Einstein was then. That's why Ehrenfest still writes to me o tentaminando, or whatever it is in Latin you see, in October. I don't know, what did I do? I think I wasn't well. Not unlikely at that period. I was somewhat overworked, and over —-I don't know. December —- I went to Copenhagen. No, I was all right then, I was still all right then. I went to Copenhagen and was fine. I worked in Copenhagen with Bohr.

Kuhn:

On what?

Goudsmit:

That was a complete fiasco. Because Bohr knew what the next problem was, and I did not understand it, it was not up, my alley. Bohr right away understood in looking at the helium spectrum that you turn over one of the two spins, and suddenly the energy is entirely different. And every day we got together in the hope that his talking to me would put some idea in my mind which would solve that problem. It didn't. So after about five or six weeks he gave me a first class ticket back to Holland on a sleeper.

I had never been in a sleeper, that was quite something for me, with the admonition that I had to get off at Hamburg and tell Pauli about that, about the Thomas factor two. But that did not come out. And I invented all kinds of mysterious magnetic interactions, and I have a letter from Bohr in which he tells me that I'm all wet.

But I still wrote him about it later. I kept on thinking about it' You know of covrse he then called Heisenberg who indeed found the solutfrn —- the anti-symmetric eigenfunctions and so on. That was beyond me. I would never have been able to guess at anything like it.

Kuhn:

When you and Bohr worked on this, in the spring of '26 —-

Goudsmit:

January '26.

Kuhn:

Was he at all using the new quantum theory, or was he —-.

Goudsmit:

If so, it went past me. Not yet, I think. He still tried to look in terms of the model, but he knew that —-. You see that I admired so. That was the next problem that one had to understand, and that would solve a lot of things, and I am so convinced that he must have inspired Heisenberg, and that Bohr must have done things like that many times, and doesn't get enough credit. Because the most important thing in some problems is to find the problem.

Kuhn:

But that was a very old problem, that was the one that lots of people had been breaking their heads on, the helium problem.

Goudsmit:

Not in that form.

Kuhn:

Not with the spin, but with the old quantum theory treatments, the helium problem.

Goudsmit:

There wasn't even a hint, but now there was a hint and now you could pin-point the difficulty. All you do is turn over one spin and suddenly the energy is enormously different. If you believe in the spin being just little magnets it doesn't work; then the singlet and the triplet stage should be very near together. So that was a very important problem. Bohr felt that this was not the same. The helium is always still a problem.

Kuhn:

Was he pretty convinced by the time you left that it couldn't be done?

Goudsmit:

No. He was convinced that I couldn't do it. He was not convinced that it couldn't be done because then he called Heisenberg, he thought that's the man who can do it. I believe that Heisenberg followed me very soon when I had been kicked out. But Bohr had at the beginning confidence in me misplaced, and must have wasted a whole month just trying to talk with me every afternoon. That was marvelous as I remember it.

Kuhn:

We have been ignoring the fact that just at this time with all the excitement about spin, there's of course all the-excitement about quantum mechanics.

Goudsmit:

It just began. There's one incident I remember, it was just before the spin, I believe. I ought to be able to check up on that. That Heisenberg passed through Leiden and told us about it. And I remember a strange remark by Ehrenfest that he was like Sir Isaac Newton, that not only had he invented a new mechanics, but also had to invent a new mathematics to go with it.

And then the story goes, and I do not know whether it has been recorded, but it has been told many times, that indeed Heisenberg thought so. He had been in England, I think, at that time, then he came back to Goettingen, and they said, don't you know that new mathematics you invented you learned from Hilbert? You must have been asleep in his class; that's nothing but the rules of matrix multiplication.

Kuhn:

Do you know where that story comes from? You told me this once before, and I haven't been able to get any confirmation of it at all.

Goudsmit:

Doesn't George remember it? Was George at Leiden at the time when Heisenberg —- because I remember being together with Heisenberg.

Kuhn:

It may have been George, not you, who told me this story.

Goudsmit:

Ja, I told you that story. But I remember very well Heisenberg in Leiden at Ehrenfest's home. He may even have come to The Hague: with me, I have a vague recollection. He came to see me from time to time, probably was at my home, the home of my parents, having dinner there, and I remember taking him to the fireworks in (Stavene) at the seaside there.

It may have been that particular visit, but I'm not so sure, because he came to Holland during that period, off and on, and I used to take the foreigners around. I was the tourist guide. I must have inherited that from my grandfather. I had to take Lande around, I had to take Sommerfeld around, I had to take Heisenberg around, Pauli —.

Kuhn:

How did people feel about this idea of Heisenberg's?

Goudsmit:

At that moment, in the summer, that was before it was published. I have the impression that Ehrenfest was a little skeptical, but hopeful. The argumentation at the time was a little strange. It was that observable — and that didn't appeal too much to some of the people I heard talk about it. And in fact there was talk later on. So the argumentation was wrong, but the mechanics was right. That's what it amounted to.

Kuhn:

What about the Born-Jordan, and the Born-Heisenberg-Jordan pieces?

Goudsmit:

The only part I was interested in, and learned, a little about, was of course, again the spin part, the Pauli spin matrices. I still don't know it well —-.

Kuhn:

But those are much later. Those must come after the fall of '26.

Goudsmit:

I don't know. Don't forget I was only a spectroscopist, I am the wrong man to ask. The impression I got from the people I heard discuss it at Leiden was that it was an enormous amount of work, and that, it was surprising that it gave the right result, and before they really had time to digest it, Schroedinger came. And that came as a relief. I don't know how long it took to show that Schroedinger was mathematically equivalent to it.

Kuhn:

Not very long.

Goudsmit:

So then they pitied the Born-Jordan —-

Kuhn:

A couple of months.

Goudsmit:

They pitied them they said, look at all the work they have done, and how difficult it was. So that was the impression I had. They were surprised that it gave the right result, that it was so awfully difficult. They gave up the hope that they could ever learn that kind of thing. And Schroedinger came soon enough, before everybody had tried to learn matrix mechanics, that it came as a relief. But I never learned it: it was not my type of physics.

Kuhn:

When did you start learning Schroedinger?

Goudsmit:

Not until I came to the States, until I had to teach things like that; that's the only way I learned it. No, I am an experimenter. Once I would experiment, spectroscopy, nothing else.

Kuhn:

Come back then to this question now. You say you think during parts of '26 you were not very well.

Goudsmit:

I believe that in parts of '26 I was not very well. Ja, that is right.

Kuhn:

Then what happened that took you back to Tuebingen?

Goudsmit:

I wanted to do some more work, and there was no job anyway. So Ehrenfest got me a fellowship. It was an exception, because I didn't have my Ph.D. yet. But he talked the International Education Board Rockefeller people into giving me a fellowship anyway to do some more work at Tuebingen. Again, Ehrenfest understood that I had to be near the experiments. I remember that my illness —-.

Kuhn:

You were there for the whole year were you?

Goudsmit:

No, not for the whole year. I went then —- I went early. Probably I left in September '26, I left for Tuebingen. I don't know the precise date. I may still have my old passport. I went early just because I hadn't been feeling well, and nervous, and thought,"well, I'll go there, I'll go away from home, maybe it's home surroundings." I think in retrospect, knowing some psychiatrists, it may have had something to do with it.

There was one terrible incident; it shouldn't go on the tape. [Tape recorder turned off:] I remember still that getting on the train, the train took off, everything was opened up again. So I came to Tuebingen and still under the impression that I wasn't well. I was well at the time. I thought I'll take it easy. I didn't want to do much work, but I'll go to the laboratory, just sit around, have a good time, have some fun. And I had a lot of fun that time: didn't know what to do, had no plans. Then Back came to me, and said, "You know, I have some very strange Zeeman effects which I cannot analyze.

Do you want to look at them?" So I said "Sure." I remember still, I didn' t smoke much, but in those days I bought 'myself fancy long German cigarettes, and I sat there in that little room smoking one cigarette after another, against my habits, and looking at these funny plates. It was the fine structure!

Kuhn:

You had done one paper before the one in which —-

Goudsmit:

No, I had never done anything on hyperfine structure.

Kuhn:

No, excuse me. But in Tuebingen there are two papers I think that come from Tuebingen. Yes, there are two papers that come from Tuebingen.

Goudsmit:

Oh, you mean the coupling papers, ja, ja. I'd forgotten about that. [Paper No.27] Quite true. I don't know how that came about. That I have completely forgotten. I like that paper, but I have completely forgotten that I did that with Back. I'll have to read it in order to get the idea. But anyway the main thing that I remember from the very beginning he showed me these very beautiful, very complex structures. Then I remembered that there was a short note, either in Naturwissenschaften or in the Physikalische Zeitschrift.

I believe it was Joos, and later I also found that Ruark had done something similar. They had shown that some of these hyperfine structures look like miniature multiplets that you could ascribe initial level, and lower level, and have transitions, with selection rules. I thought, if that is so, then one ought to apply the Zeernan effect to that, and it's a kind of Paschen-Back effect. And it worked. We were able to analyze these structures, but the first thing we did was not yet the Zeeman effect, because it was a little difficult. It was in the back of my mind. I had tried it, we weren't one hundred percent successful.

Kuhn:

When you mean apply it, you mean analytically apply it, you mean actually put a field on and see —-?

Goudsmit:

Back had a field on, you see, and we applied the rules which we knew from ordinary multiplet to these miniature Multiplets. We were doing that. But before that, we noticed one thing, that these fine structures themselves, after you did a thing like Ruark did, and get the initial level and final level, that you could do the same as with the Zeeman effect. If you had several lines with fine structure in - the spectrum, and you analyzed those, and found that several had the initial level split the same, and others the final level split the same, you could analyze the spectrum.

So in our first paper on hyperfine structure, the emphasis was on the fact that this could be used, as formerly the Zeeman effect was used to analyze the spectrum in levels. That was the emphasis. We did that with bismuth because there the hyperfine structures were the largest; many were measured by Back, but we used also measurements made in Japan by Nagaoka. It was really used like a Zeeman effect to analyze the spectrum. And then in a footnote, I think, we tentatively say we believe this fine structure is due to a nuclear moment. And then the second paper we have finally succeeded, or are convinced that we had succeeded, in even interpreting the Zeeman effect.

And that clinched it. We had done it already at the beginning, but hadn't felt sure about it. Then we got more data, and he took more pictures, and then we had the Zeeman effect, and there was no doubt about, it, it was a spin, and the spin was nine halves, enormously large, and everything fitted so beautifully. Then several papers followed.

Kuhn:

The second paper is Tuebingen, December '27. [Paper No. 31]

Goudsmit:

It was written by remote control on new data, and that was done in part by cable, and then Bob Bacher got a thesis out of this work, out of the theoretical interpretation of these things. John Wulff was left behind in Tuebingen —- he still had a year fellowship —- to ride hard on Back. He, for a year, became a spectroscopist, but then he went back to his chemistry, and he is now I think boss of metallurgy at M.I.T. and knows nothing about spectroscopy anymore, unfortunately. But that year was really wonderful.

Kuhn:

What sort of an invo1vemeni did Back himself have with the idea of the coupling of a nuclear moment?

Goudsmit:

Nothing. His idea was to get the most beautifully resolved pictures, and do it over again whenever we wanted to, because I later discovered that some of these pictures had been published by him before. The book of Back and Landé on the Zeeman effect has a plate at the end; the last pictures on that plate were some of these strange fine structures which in the text they say we don't know what they are, here they are; bismuth line so-and-so and so-and-so, with a field and without a field. Beautiful. With a magnifying glass you could almost have made the analysis right there, but nobody had noticed it, not even I, although I had that book. In fact it had been presented to me by Lande.

Kuhn:

In the paper that Pauli felt badly about, had he pointed to this as an effect that might help with particular spectroscopic structures, or was it just something that might happen?

Goudsmit:

No, he had pointed out that these hyperfine structures might be due to nuclear spin. Definitely! And he even —- you see, there were two causes for hyperfine structure which people could think of. One was generally accepted, namely, it was isotopes: different isotopes, slightly different energy levels. And Pauli pointed out to us still a second one, and that you could tell the difference between the two by the Zeeman effect: if it was an isotope, then the Zeeman "effect would have the same structure as the individual line, because all the lines would split the same way; if it was magnetic moment of the nucleus, you would get a complicated structure.

And I had never noticed that paper. Probably came out during the period that I was under the weather, and I'd never seen it at that time. Or if I'd seen it, it was probably written in such a language that I'd said, it doesn't mean anything to me, didn't register. And it's a sad thing that nobody called it to my attention, because from Tübingen, at that time. I rent , to Copenhagen, and discussed all this stuff in great detail with Bohr, who was intrigued, Linus Pauling —- in fact, I thank them in the paper for discussions and they didn't call it to my attention, "Look,that confirms what Pauli has said a year ago, two years ago." They didn' t. Even when the first papers came out, nobody told us, "Hey, haven' t you seen Pauli's paper, why don' t you mention him?" Nobody! So I was not the only one who ignored Pauli's paper. It didn't come at the right time, that's what it means again. It was a little premature, people were not interested in hyperfine structure, the people who knew. Nuclear physics wasn't interesting yet, it was chemistry.

Madame Curie was considered a chemist at the time, so who cared about it? I believe that's the sad fate of the Pauli paper. It came at a moment when nobody cared, except a few spectroscopists, and they even hadn't noticed it, it was too learned. But I knew the paper by Joos and by Ruark where they really had found energy levels to explain the hyperfine structure, but I'd just given one or two examples. [Interruption]

Goudsmit:

You wanted me to tell you about Mililkan. Ehrenfest told me that he had written Millikan a letter about the spin, in which he said how nice it was that the person who had been the first to do the relativistic doublets was the one to collaborate in getting the spin idea. That was referring to my very first paper, of course. Ehrenfest told me later that he had gotten an answer from Millikan, but Millikan did not like that remark at all and seemed to be rather mad about it. But he never showed me Millikan's letter. So that' s the whole story. Probably one can find Millikan's letter in Ehrenfest's correspondence. I'm very curious about it.

Kuhn:

The only other thing to ask about your European period is your thesis. It was a survey?

Goudsmit:

It was a survey and a very badly written one. Ehrenfest did not like it. I violated several of Ehrenfest's rules, probably because it was done in a hurry. It is not properly divided, it has no proper index, no bold print; it's just like one long piece —- It goes on and on and on. He did not like it at all. But under the circumstances it was the best I could do. Linus Pauling liked it and translated it. I met him in Copenhagen in 1927 before coming to the States. We worked together there and I discussed various things with him.

And he had gotten hold of my thesis and was translating it, and he said "We will publish that in America." But I said it wasn't good enough, so he said, "All right, we'll rewrite it a little." And that's how that book [Goudsmit and Pauling, Structure of Line Spectra, 1930] originated. We rewrote it more than a little bit. He wrote some new chapters, and I wrote some new chapters and sent them to him to change the English.

It was done completely by correspondence. We made the final arrangement in London —- he left a few days before I did on a different ship. In the beginning, of course, he had no time and I had no time. But the book with Linus was nothing else than that I did not want the thesis to be published, in this form.

Kuhn:

You talk less about Ehrenfest than do a number of people who worked at Leiden. What did you get from working with him?

Goudsmit:

I really didn't work with him, because I didn't do anything which was in his Field. I told you that one incident about the statisica1 formula —- he was so surprised. And essentially, he sent me away; he sent me to Zeeman. That's where I belonged. But he had enough interest in my work to guide me, and he advised me on every step I took in my life.

Kuhn:

Did you feel personally close to him? Was that an important human relationship for you?

Goudsmit:

He always remained a stranger, but it was a very very close relationship from me to him. There was nothing that I did not dare to discuss with him. And I'm a little worried, therefore, about his correspondence —- the letters I wrote to him —- getting into the wrong hands. There are at least a few which are too personal, which should not be published until I am dead a hundred years.

He promised to destroy some of them, and he may have. Some of my troubles had started already that far back only I didn't know it. But I confided in him. They are purely personal and have nothing to do with physics. In some respects he has had a very bad influence on me, because I have always been the kind of person who found it easier to ask him for advice about what to do than to make up my own mind.

It was not until his death that I finally learned to make my own decisions. Up to that time —-. George, travel, things like that. You'll find it even in this correspondence. He used to call these letters 'meow letters'—like a cat wanting to go out— always complaining, always asking for help and advice. So I relied very happily upon him for advice, and in that respect it was a very important relationship. But as far as physics was concerned it was not.

Kuhn:

Did you ever work with him in the way that George did?

Goudsmit:

No. No. Maybe for an hour or so on a problem, but never more than that.

Kuhn:

When you were in Copenhagen for the second time was it entirely for the thesis or did you participate in the things that were going on?

Goudsmit:

The main thing I participated in was, probably, giving a colloquium talk or discussing the hyperfine structure, because that was my interest at the moment. Mainly I discussed this with the people who were there. I believe that I am responsible for Casimir getting into that work. In fact I found, but haven't been able to locate again, a manuscript which Casimir wrote on the subject, of which he wanted me to be the co-author because I had really done it.

I believe that preceded his famous Teyler thing. But I didn't want to be the co-author because I didn't understand it. The result was, however, very sad—-.I saw Casimir recently in Copenhagen, and we talked about the history, and he reproached me. I sat on it so long, trying to understand it, that in the meantime — Fermi published exactly the same thing. But in my book I give him credit, this formula was known to me from Casimir." [Interruption]

Kuhn:

Tell me about the American job. How did it come, how did you feel about it?

Goudsmit:

The original letters are still in there. What happened approximately the way I recall it or can reconstruct it, is that Randall was a far-sighted man who wanted theory at Michigan, that Colby was his talent scout, who had studied with Boltzmann and every year went back to Europe, had been in Copenhagen before anybody else, had been with Pauli in Hamburg, with Sommerfeld in Munich, had been everywhere as a kind of observer and student, never making an impression except on the Bohr family; they always liked him personally very, very much. He had brought Europeans to Ann Arbor, but they never stayed. In fact, Kronig was there for a while.

Kuhn:

Was he?

Goudsmit:

Ja.

Kuhn:

I'd forgotten that. I knew Klein had been there.

Goudsmit:

Klein had been there for a year, Hulthén, and they didn't stay. So Klein, who had been at

Leiden, had told Colby that maybe I would be interested in going there. And I have a letter from Klein, which is in this bundle, where he says Professor Colby from Ann-Arbor is coming and he would like to have people come to the University of Michigan, and even if you are not interested, he is such a nice man, don't fail to see him when he comes. So Colby came and talked with Ehrenfest, and asked Ehrenfest why they had so much trouble in keeping people at Ann Arbor. It was then that Ehrenfest said, you know theorists don't grow alone, you have to give them some people to talk with. I have two young fellows here, maybe they both want to go.

It must have been in the summer of '26 when I saw Colby and spent an afternoon with him, with the girl I was engaged to. I had a very nice pleasant afternoon in The Hague. I forgot more or less about it because then that fall I went to Tuebingen. Then finally all hell broke loose because I got letters from Uhlenbeck which must be in there, "Well, we have gotten an offer from Michigan, I'd like to go if you go too. Answer at once." Well, that's the kind of thing—-.

My God, I was old and conservative, and even if my family traveled, and I had a cousin in America, America had never appealed to me. If it had been Egypt or something like that, I would have gone right away, or even India, I always wanted to go to exotic places, or China, but America seemed terribly dull and uninteresting, and nothing. So I didn't answer right away. Then I got telegrams which are still in there, Make up your mind at once. In the meantime I had written to my parents, to see whether they had anything against it. I don't know what they answered. Then I said, yes, but I am engaged, and I want to get married before then. Then suddenly —-.

Kuhn:

Well how did your fiancée feel about it?

Goudsmit:

Nothing. She thought it was all right, because we still had in our minds that this was like a fellowship, you could go for two years, or one year, and then you come back, and by that time maybe Zeeman is dead or something like that, maybe a job is open. At that time I had already done the spin, so the chances of getting into academic work were a little bit better if somebody was willing to die at the right moment. I had no longer the idea that I had to go to the little high school in the little village, having had this Rockefeller fellowship; that was the first step up, so to say. But when that would come along, and where, I had no idea. And America looked like a better place to wait perhaps than to be an assistant in Leiden or Amsterdam. That's what it looked like to me, something which was temporary and waiting for the right job to open up.

So I didn't like it too much but, I was pressured and, as I said, I never made my own decisions. Ehrenfest made them for me. It was obvious that he said yes, and so I said, all right. So that's really how it happened. I was at Tübingen at the time and had to do it all by remote control, couldn' t talk to the girl I was engaged to, couldn't talk to my parents. And in those days it was an enormous decision to go to America. The way I heard it from my family, people who wanted to evade the police or the draft, they went to America in those days. But nobody in his right mind— Oh yes, my— uncles went for the diamond trade, and some acquaintances of my parents had lived here.

Kuhn:

Uncles on which side, your father's?

Goudsmit:

My mother's side.

Kuhn:

I thought they were all milliners.

Goudsmit:

No. They were married to diamond merchants or something like that. They were all merchants, but some of them went to America for the trade. Then they had acquaintances who were in antique business, and had business in America. In fact, they had got kicked out because they sold fake paintings. So it didn't look like a very interesting place to be at all at that time. But the decision was made, as I say, itwas really almost forced upon me, as you can see from the telegrams and the correspondence. I had to make hurried arrangements to get married, so I went back Christmas vacation and got married early in January. Then I went back to Tübingen, with my wife this time, and prepared. My fellowship was continued; in fact, it was raised.

You got $120 a month if you were single, and $182 when you were married, which was an enormous amount of money at the time for Europe, an enormous amount. Even though I hadn't gotten my Ph.D. yet, the Rockefeller people gave that money. So I went back to Tübingen, because we had to go to America, I decided to go to Copenhagen, and then to Göttingen; there is still a letter in there from Ehrenfest asking the Rockefeller people to allow me to change the itinerary a little bit. As a result I did not have my fellowship a full year. I had it only for eight or nine months. Then I went to Copenhagen, wrote my thesis, went back to Holland to stay with the family before going off to far away America. Then we got our degree, our Ph.D. on the same day, that was the 8th of July or something like that. It's in the thesis somewhere.

Then in September we sailed, made all the arrangements, wrote Colby to try and borrow money. I was always worried about money, how do you pay for a trip to America? The Rockefeller fellowship fortunately had paid so well, and it was so cheap to live in Tübingen, that we had saved some money, and then Colby, I think, loaned me some money to come over, which we paid back very easily. So we came to America as instructors at the University of Michigan at a salary of, I think it was, $2500 or so, maybe $5000, they made a special exception for us, something like that, but no more than that. We were met at the dock by Ophenheimer. I used that as his introduction when he lectured at Brookhaven. He had completely forgotten about that. But he met us here, and showed us around New York and all the things he did for us I mentioned. But I did not mention the real reason: there was also a lady on board he was interested in at the time. She was in the audience when I introduced him, and really I didn't mention it, but she came up to me latter. That was his real interest… She traveled on the same boat with us, and he had written to keep an eye on her —- a physicist.

I can mention on the tape. It's now Mrs. Houtermans, (Charlotta Riefenstaum). They were friends in Göttingen, and she came over on the same boat. He was really a wise man, even though Ehrenfest had trouble with him. He must have reasoned that the shock for these Europeans is a little too large. So he put us up at the Brovoert, which is the most European hotel you could find in New York at the time. But then he also took us to Greenwich Village and taught us how to eat corn on the cob. It was just the right time, in September —- excellent! So we stayed in New York for a few days —- I think Uhlenbeck stayed a little longer —- and we went on to Ann Arbor.

Kuhn:

When you say he [Oppenheimer] had trouble with Ehrenfest, what sort of trouble?

Goudsmit:

He was always so vague, his physics was so superficial. And Ehrenfest would put a piece of chalk in his hand, and say, "Now write it on the blackboard," but, of course, he couldn't do that! He talked in such generalities. So Ehrenfest —- of course, George knows much more about that than I do —- sent him to Pauli for discipline. Then he came back to Leiden and he was there —- both Dirac and Oppie were there when George and I got our degrees. So that was the introduction to America and how it happened.

So it was Oskar Klein who wrote me and Colby who talked to Ehrenfest, and of course then, the way Colby is, right away he said, "oh, you need more than one." Then he right away saw to it that Laporte also came. Then Dennison was just through with his European thing, and instead of going elsewhere, he was offered a job at Michigan, even though he was a Michigan graduate. They were wise, that they didn't always want to keep their own people, but there was a man with the proper background, so there was a group of four. And it worked. And then I didn't want to leave anymore. There were two very important things Ehrenfest told me. He had been here, and writing letters in the correspondence —- his early letters from America, and his impressions of America.

Of course Ehrenfest had a brother living here who was one hundred per cent American, the typical American. But he also went rough the list even with photographs of all the physicists he had met here, and told us a little story about each of them, their attitude. Not always favorable at all, but there were so many that it didn't register with me at all.

Kuhn:

Do you remember any?

Goudsmit:

Ja, one which was derogatory. He told me about one physicist that was very conservative, and even anti-Semitic. And this man, as he is now, is just the opposite absolutely the opposite! One of the most liberal and, even though he is a New Englander, has taken an active part in the Federation of American Scientists. You know him. He's retired now.

Kuhn:

But living.

Goudsmit:

Ja, ja, ja. And it's incredible, but that impression was that Ehrenfest had on a short visit to him, in discussing with him.

Kuhn:

I'm likely to guess wrong. Do you want to tell me for these purposes who it was?

Goudsmit:

Ja. E.C. Kemble.

Kuhn:

I would have guessed right, but it's just incredible to me that Ted should have given anybody that impression.

Goudsmit:

Yes, yes. Maybe at that time. And so I can't believe it at all. But I remember that distinctly, you see, that he said this man is i1tra-conservative, even antisemitic. That I remember because the concept in Holland was rather strange. [Ehrenfest] didn't talk about that, but he, with his Viennese background, thought it was necessary to tell me that.

Kuhn:

I should guess that he was mistaken.

Goudsmit:

I should guess. That it was not a change, that he never—-.

Kuhn:

That he would see Ted as, conservative, yes. That he was antisemitic, I think he may have been put off by the haircut.

Goudsmit:

Ja. Absolutely mistaken. So Ehrenfest wasn't always right, because never—-. I met Ted Kemble early in the game when I was here, and I never never, never met a more liberal man. In every respect. But that's typical. Anyway Ehrenfest went through that. Then the more important thing he told me was the following.

Kuhn:

Do you remember his reactions to any other Americans?

Goudsmit:

No. I don't. Because there were too many, he went through the whole list. He told me stories of reactions of Americans to Germans who had visited here, and that they were still very anti-German, that the First World War had not worn off yet, that they especially disliked the chemist Haber. But I do not know the details. But with Kemble, I'm sure he was wrong there. But I remember the name and I remember this remark. I am not wrong, my memory is not wrong in that respect. But he was.

Kuhn:

No, I'm sure he was.

Goudsmit:

But the more important thing he told me, and the thing I often repeat is the following

"When you get to America you'll say they do this wrong," "And their physics isn't any good." He said, "That is utterly unimportant. What is important to feel happy is not the state how it is, but the derivative. That is positive there. Only two countries at the moment where it's positive: Russia and America, where things are getting better all the time from the point of view of education, of physics." He was rather narrow, he did not mean politics or anything like that. "Then you feel that, you can contribute," he said. Then he said to me, "But when it comes choice of those two countries, I think you would feel better in America." But at that time he still said that both Russia and America were countries where things were going up and getting better all the time. And that is important, that you feel you can contribute.

Kuhn:

How did you feel when you got there? About America, about physics in America?

Goudsmit:

I felt pleased. First of all, as I told you, my educational, my home background was so that I often felt a little out of place with the people in Holland. I did not have that feeling here. There's one incident in my life —-it has nothing to do with quantum mechanics; it tells about me. I had done something practical, I had even made some money, just enough for a little vacation. I had found some rule for mixing oils, the viscosity of mixing oils. My now brother-in-law was in the business, he had it printed, and they sold these charts 'to' dealers, so that if they wanted to mix oil, they get the right viscosity.

It was even advertised in Nature at one time. I got just a little money out of it, and a lot of pleasure out of it. I told Ehrenfest about it, and he was intrigued; but he said, "You know, here in Leiden you don't mention that." When I came to America I was very pleased to notice that they had an automobile built in the physics building in Michigan, to do applied work. It didn't shock me at all, on the contrary. In fact, at one time, I picked up that viscosity work again and tried to go to oil companies here to see whether they were interested in developing it further. It turned out that they independently had discovered similar things, and they were printed in the handbooks. I even once tried to write a paper about it, but it was rejected by the Journal of Rhelogy.

I still have the manuscript somewhere…on oil viscosity mixes and so forth, and dependence on temperature. It made me feel good that that was not taboo. So I felt quite at home among those people. Then I like, people in general, and we were very lucky in the people we met and got in contact with in the, early days. The only black time I remember was the first Christmas vacation, because, everybody had gone somewhere; nobody was left in Ann Arbor. Our winter clothes weren't warm enough.

We had no car. We lived in two rooms, one looking out upon the hospital, the other upon the cemetery —- no kitchen, just a little gas flame to make some tea, bathroom shared with one or two other couples, icebox out on the porch with one or two other couples. It was horrible. It was at that time that there was an ad in Nature that they needed a professor in Egypt, and I wrote immediately. The correspondence is still in the file. I didn't get the job.

Kuhn:

Would you have taken it, do you think?

Goudsmit:

At that moment, yes.

Kuhn:

But you wouldn't have gotten it before April, in April would you have?

Goudsmit:

No, by then probably I wouldn't have wanted it. But I wanted it so badly. I gave as references in that letter, I think, Bohr and Einstein and Ehrenfest a whole list of the highest references. I could have gotten the job, because then I got a letter back which may be still in there, I think it's still in there, I read it not so long ago, in which they said, "Oh yes, sorry but I think somebody else (gets ???), our regards to Professor Colby." [Laughter] So he was home there too, as he always is, everywhere. That was the only black spot at the time. Then we had very good luck in that we were able to go back to Europe after about two years, I think it was. That's very important for immigrants, because then when you go back, then you can compare. And then, I think, we made up our mind that we liked it better here.

Kuhn:

Were you there for the summer only?

Goudsmit:

No. I think it was more than the summer. I don't know whether that was already on that new scheme. I don't know when that new scheme came. Well, I had a few offers then. Finally some European offers came through, one to London. But as I was here too short a time, I don't think I wanted it, and I recommended Kronig. I think he took it at that time. Then came the offer from Zürich. I don' t —know exactly what year that was, to succeed Schrödinger. I believe Ehrenfest wanted me to take it, the correspondence is in there, but I just did not see it, I couldn't. -

Kuhn:

Tell me more about why.

Goudsmit:

Because I believe I was subconsciously aware that I was not a theoretical physicist, that I would have broken down just trying to keep up the standards of Schrödinger. I could have succeeded Zeeman. That shows you what a low opinion I had of his knowledge, in spite of the Zeeman effects But I definitely felt that I was not the successor to Schrödinger. I know who recommended me highly and wanted me: that was Edgar Meyer, he was the experimintalist there, and he used to come to Tuebingen. He wanted a theorist he could talk to, and do spectroscopy with and things like that; he wanted me badly. In some later correspondence, which I only read recently, it turned out that even if I had wanted it, I might not have gotten it. Apparently they were negotiating with several people at the same time.

Kuhn:

I didn't know they did that.

Goudsmit:

I didn't know either, but I think that a week or two ago, going—through this correspondence, I found a letter to that effect, from Ehrenfest, of which he sent me a copy or something like that, giving them hell. But I didn't want that anyway. So I remember that I sent them a telegram or a letter telling them. But it had an effect on Michigan. Randall got to work, we got a raise in pay; then we got that arrangement which the dean accepted, and which was very important, to have leaves of absence, every fifth semester, with pay. So out of the five theorists, that means Colby was included in that cycle, one was always away for a semester and a summer. That lasted for about ten years until the war finally made it impossible.

Kuhn:

George did go back.

Goudsmit:

Then George got an offer from Holland, and did go back. You'd better ask him about it. I always had the feeling that it was for family reasons. But he didn't like it, as you know; he didn't at all, and she didn't like it. So when the opportunity arose to come back again to this country, they went back to Michigan, to the same house even which they had left before. He hates to move and change his habits. It was really a triumph. I'm very proud of it that I finally got him to leave Ann Arbor. He went back to Utrecht. But I still was under the system of every fifth semester going abroad for a semester and a summer, with pay. It was really marvelous, very important. So Laporte, Dennison, George, Colby and myself. That came about because I got that offer. No matter what I've done, I wouldn't have taken it, but they didn't know that. I didn't press for that; this idea was all really Randall's.

Kuhn:

How did you find the students?

Goudsmit:

Marvelous! First of all, we were so on the same level, it was so easy. The undergraduate studens at the beginning I had trouble with, because I was used to the discipline and the hostility between student and teacher of the European schools. And I treated them the wrong way, so they took the undergraduate teaching away from us at the beginning for a while, until we —-.

Kuhn:

For both you and George.

Goudsmit:

I think for both, ja. But later, after we were adjusted a little better to the American way of doing things, we got it back again. And I believe that we were very, very successful in our undergraduate teaching too.

Kuhn:

I know you both have the reputation, and in your case I know first-hand.

Goudsmit:

You mean the Harvard lectures?

Kuhn:

That Harvard semester in Physics G. I have very fond recollections of that.

Goudsmit:

But at the beginning we did it wrong. We didn't know what to do, didn't know what to require of the students, tried to introduce European systems which didn't apply at all. It took probably two years before we knew, then we were given back some undergraduate quiz sections again. And I liked it. Then in the graduate work, of course, I didn't have many students, because, like Ehrenfest, I was snooty in that respect. I wanted only students whom I could work with on an equal level, and who didn't need too much help. I didn't want to write a thesis for them, they had to come up with something, or I would give it to them and they had to do the work.

Let's see, whom did I work with? Some of them, who didn't get their degree with me, I used to work with very closely anyway. Bob Bacher was the first one. Then I had an experimental student, was [Russell A.] Fisher, hyperfine structure; (Tai Yu Wu), a Chinese — I think he got his degree with me, at least we worked quite a lot together. I believe his thesis was with me. I don't know whether (Lloyd, Young) worked with George Uhlenbeck or with me at the time. Maybe his thesis work was with Uhlenbeck, but some of his work was with me. David Inglis —- his thesis was with me.

But it was purely theoretical and I can honestly tell you that he knew more than I did. I got him interested in the problems, but he knew much more about it. In fact, I have an old notebook here I noticed that I learned it from him,some of it, —- quantum mechanics techniques.

Kuhn:

And you were learning physics all this time?

Goudsmit:

I was learning physics all that time, by teaching it, and from my students. Because I really didn't know it.

Kuhn:

You never thought of going back and taking the exams again?

Goudsmit:

I would have been scared stiff. The rest about the American episode, you know.

Kuhn:

Now tell me about some of the work you were doing. You went on with the hyperfine structures.

Goudsmit:

I went on with the hyperfine structure, ja. I think the only way to do it is to look at the papers. But that only goes up to—-.

Kuhn:

I think it goes up to about '31. I think already in this period it's probably fair to say that there are all sorts of fascinating puzzles in the spectra still left, but that, on the other hand, the fundamental excitement has probably gone out of it by the end of this. Already the interest in hyperfine structure is switching from an interest in spectra to an interest in the nucleus for example. The whole question is, when is one through with multiplets? You've got some quite ingenious stuff here to do, and gamma sum rules and so on.

Goudsmit:

There were some tricks I was very proud of. I see now, fully, how stupid they were. Because I knew so little mathematics, I always had to rely on my intuition and ingenuity arid tricks to do certain things for instance, there is a paper about guessing at the formulas going from one coupling to the other, which I thought was marvelous. I knew enough quantum mechanics to know that it had to be quadratic equations where you had two levels and cubic equations at three levels.

I knew from most elementary considerations I did that with Inglis, I think —- what it was in one extreme coupling, what it was in the other extreme coupling. That determined the coefficients without having to know any quantum mechanics And of course, the people who knew quantum mechanics thought that all very stupid. After I had done that, then they come with long formulas with all the coefficients. What upset me most: if you ever look through Condon and Shortley's book, you find several chapters on the subject, then it has a footnote, "according to Goudsmit." But then you look at it: I could never have written it, I don't understand a word of it. It's really remarkable.

Because they did it correctly, in a general way. I got the same results always by tricks whenever it was necessary, and sometimes tricks were not possible. So that was the kind of work I did. Indeed the intensities with Kronig was similar. You knew certain extreme cases, then you guessed at what you had to do. Then later people could derive it with quantum mechanics. But these London and Shortley things, it's really remarkable. I looked at it, and I said, "Gee whizz, was I that learned?"

Kuhn:

But there is this difference: although you were doing the same sort of work, and in many respects in the same way, now, at this date, one would say one could already have done it the other way. Whereas when you did the intensity things with Kronig, one could not have done it any way except the way in which you did it. Now how conscious were you and other people that already those problems were solved?

Goudsmit:

I was semi—conscious of it, but I had the feeling the people weren't interested because these problems were only of interst to spectroscopists. The quantum mechanicians already begin to get less and less interested in spectra lines. There were other problems for them like statistical mechanics which were far more interesting than spectroscopy. So they didn't care whether it went from JJ coupling to Russel-Saunders coupling, and that there were formulas like that. And when the Condon-Shortley book came out—I think they said, 'Now that's the end. No more of this. There it all is; if you want something you can find it in there, but who wants it?' There was another thing, a paper I wrote later with Bob Bacher which is not on this list, where we introduced a fractional parentage.

Kuhn:

I don't even know what that is.

Goudsmit:

Ja, you see. It's now called fractional parentage, I don't know what we called it. But again by a marvelous— I say myself it was marvelous— by an intuitive guessing game, we did the thing which they now rediscover and do with highfaluting group theory and Racah coefficients. We had Racah coefficients before Racah existed — by a similar trick. We needed that very badly. It is the followin: that if you add an electron to a spectrum, then the state of the new atom with the added electron must be related to the state of the ion.

But now the state of the ion is usually a complicated thing, it may be a triplet p and a singlet p and so on. Only in extreme cases, when can you tell on which of these original states the new state is built up. But if you have equivalent electrons, then it's a mixture. Then it is, let' s say, 6/7ths of triplet p, and 1/12th of singlet D and so on. How do you get these co-efficients? They're now called Racah co-efficients, and they have them for the most complicated cases; you just have to put it into the machinery and get it out. But we needed it for the paper, and we invented them by guess-work.

It was very nice. It was the first paper where we pointed out that this existed, that you just couldn't add an electron to an ion and say what came out of it, that you had to do all this. That was again in line with what I did, but now, of course, that' s completely forgotten because you don' t need the trick, you look it up in the tables, the Racah co-efficients.

Kuhn:

In these papers there are a number of points where you point to possible nuclear interests, electron-electron interaction, or electron-nuclear interaction, to correct or improve the Dirac equation.

Goudsmit:

Ja, I think that was nonsense. Where did I say that?

Kuhn:

Was this all window-dressing?

Goudsmit:

Absolute window-dressing, I'm sure. The only thing I was interested in was the nuclear moments. I also was probably the first ore who was able to derive the order of magnitude of nuclear moments. At least that's what Rabi claims. But I don't know whether I was really the first. By using a Landé type of formula, from the size of the hyperfine structure and a Fermi formula, or whatever—it was, I really got the first nuclear magnetic moments for complicated spectra.

Kuhn:

Would you say you were doing this to get magnetic moments or to db spectra#?

Goudsmit:

I was doing it to get magnetic moments for the nuclei; for no other reason. I did some other work. There was one little thing I always liked because it wac different. I did that Brownian motion paper with George. That was something quite different.

Kuhn:

How did you happen to do that?

Goudsmit:

It goes way back to Amsterdam. In Amsterdam in Zeeman's lab, there was one man who was doing Brownian motion, in a rather stupid way. I don't know why people were interested at that time —- He had put a little mirror on a fiber, and photographed the deflection, and got nice curve, Brownian motion, and he didn't know what to do with it.

Kuhn:

Gerlach did this one.

Goudsmit:

I was in Tübingen, and then there was a fellow with Gerlach who did the same, but better, because he used a rotation, which gave much better deflections than that stupid guy in Amsterdam.

Kuhn:

Did you suggest this in Tuebingen, or was it just a coincidence that they both —?

Goudsmit:

I think it was just a coincidence. I don't think I suggested it. I'm pretty sure it was a coincidence. I believe all this work came back because my guess, and probably a right guess, is that people were interested in better and better galvanometers, and that the limit was the Brownian motion. They observed Brownian motion in galvanometers and wanted to learn more about it in order to see whether they could improve it. As a good experimenter, you begin to investigate. Now the trouble was the interpretation of these various curves. I had looked at them, I had done it at Amsterdam already, and I had told. them that they ought to take a Fourier analysis, because they were different.

At different pressures they looked quite different. But I had no explanation. But I remember playing with the Fourier analysis. At the Amsterdam lab they had one of these planimeters with a Fourier thing where you put different things in —- a very primitive way of taking Fourier analysis —- to see whether the Fourier components were the same.

Kuhn:

You're not supposed to know anything about Fourier analysis at that time!

Goudsmit:

I .knew just enough Not much, honest, not much. But I remember playing with the instrument and trying to get the component's. I don't know whether I did it first in Tübingen and then in Amsterdam; probably first in Amsterdam arid then in Tübingen. It happened that they were both working on the same problem, and it always was back in my mind. Then when we came to Ann Arbor, I had a chance we had an office together —- to discuss it with George. He may remember that better than I do.

I said, "Even at different pressures, we know from our elementary theory that the mean-square deviation is always the same. What is different at different pressures? Why do they look so much more like pure sine curves at low pressure than at high pressure?"

Then we began to study the Fourier components of Brownian motion. I believe that I was the one who called George's attention to the problem. But he was the one who, of course, understood this field, and the computations and so on. But I learned it. I really understood that paper at that time, to my great surprise. I probably could even reproduce it yet.

Kuhn:

Your feeling is that already by the time that you got to the United States, people were no longer excited about spectra?

Goudsmit:

Yes.

Kuhn:

And that Condon and Shortley were fairly decisive?

Goudsmit:

Ja. Well not quite; I came to the United States in '27. I think about 1932 or so was the end as far as spectra was concerned. At least only detailed problems and only die-hards who couldn't do anything else would go on being interested in spectra from a theoretical point of view. The only thing I still thought might be interesting was hyperfine structure because of its nuclear interest. So at Michigan I had Fisher work on hyperfine structure at the time; and so did Inglis. But that was honestly the nuclear interest, not the spectroscopy interest. But it was nice that there was something in nuclear physics I could do. It was a little premature.

Kuhn:

At the time that you were doing the hyperfine structure with an eye to the nucleus, were you also following what was going on in nuclear theory, the development of nuclear theory?

Goudsmit:

That came a little later. The first things I was very much aware of were the papers of Elsasser because I understood those a little bit. I understood enough statistical mechanics, having been exposed to it, that I understood Elsasser's papers, I thought, in getting nuclear energy shells.

Kuhn:

But even before that, how were you about problems like the statistics of nucleus?

Goudsmit:

The statistics, no.

Kuhn:

The electron and the nucleus, the neutron, positron, neutrino, these problems.

Goudsmit:

I was aware of them, but only superficially, and was only aware of them in so far as they had something to do with hyperfine structure. I was not fully aware of the difficulty in the nitrogen spectrum; I knew about it. Molecular spectra was already too difficult for me. The alternating intensities which Dennison worked on, and Dieke, I understood a little bit, but not enough to make it my own, that I could ever have made a contribution.

Kuhn:

Wouldn't your own sort of techniques have also helped with molecular spectra?

Goudsmit:

Perhaps. But I am not sure. Remember that Rabi had promised me a bottle of champagne years later if I could use my ingenuity and apply to his work—- I think it was his molecular work at the time —- the same skill I did to getting nuclear moments from the atomic spectra. He said there must be some simple relation. The nuclear moments I got by comparing the hyperfine structure with the fine structure. You didn't . have to know anything else. I could prove that by that ratio you could get the nuclear moment. He said there must be something similar in the molecular business he was doing in his molecular beam, but I never succeeded. It turned out it wouldn't have been possible —- far more complicated. I don' t think that my skill would have helped with molecular spectra. So you see how narrow I was! But in the narrow field I pushed ahead.

Kuhn:

I realize there's one question I left out earlier: it takes us back to spin. In the Nature paper on' spin you list a lot of the reasons for it. Then you make the remark, "so far, we have not mentioned the Zeeman effect, although the introduction of the spinning electron was primarily suggested by the analysis of the anomalous Zeeman effect, shown by the components of multiplet structures." Now what does that mean? I found that hard to understand, why you would single out the anomalous Zeeman effect as the principal thing to suggest spin.

Goudsmit:

It probably is that the sentence is too condensed. I meant by that, the anomalous Zeeman effect was the reason for the vector scheme. That I knew so well, that is how it came up. I think that is what it was, and nothing else.

Kuhn:

That's right, that' s certainly clear and I should think entirely correct; but it reads as though you meant it was in particular the puzzle of the anomalous Zeeman effect.

Goudsmit:

No, it was the Zeeman effect that led to the factor with the factor two, and that it could all be now condensed into one statement, electron has a spin one half, factor two, and then the whole Landé factor, the whole business, came out.

Kuhn:

Would it be right to say that if there was a key problem that went from there to spin, it was much more the relationship of the X-ray and the doublet riddle?

Goudsmit:

No. That would not be right to say. The idea of spin, I'm sure, came in because I tried to explain the Zeeman effects and Pauli principle to George Uhlenbeck. Then after we had the idea of the spin, then I said, "Oh, but now it also explains my old doublet ideas and so on, and the X-ray doublets." That's how it went, logically, or historically. I'm sure of that.

Kuhn:

What am I forgetting to ask you?

Goudsmit:

Nothing. I still claim that these precise ideas, how they went, my memory may fail, also I still think it's unimportant. Absolutely unimportant except for a psychiatrist or a psychologist. But for the history of physics, it is unimportant.

Kuhn:

I will argue with you about that off the tape.

Goudsmit:

Another thing is that sometimes it is more logical to tell it the way it did not really happen.

Kuhn:

more logical I don't doubt.

Goudsmit:

It would have been obvious to do it from the doublets and so on. It would have been obvious for me and for Kronig to do it in May.

Kuhn:

Tell me on the tape.

Goudsmit:

The spin paper. What Ehrenfest told me at the time: "Be sure that George Uhlenbeck' s name comes first, because you have already published many papers, George has not. And the way it goes, he'll be forgotten, or dropped." And I said, "Of course, he gets first." And even so you find, especially in the early years, many references either to me alone, or even now, references where my name comes first, incorrectly. Ehrenfest was so right about that; And not only that George deserved to be first, but he made it a point, "Please," he said, "Goudsmit, make sure that George's name comes first, otherwise he'll be chopped Off in no time." You find many of the references where the names are wrong. In all our papers, George is first. But in the quotation, the other way.

Kuhn:

In the spin papers.

Goudsmit:

The spin papers, ja.

Kuhn:

I’m very glad you told me that.

Goudsmit:

Ehrenfest stressed that, you see. And if you now look at the quotations, you find it very often the wrong way around, in the old quotations.

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Samuel A. Goudsmit – Session III

December 7, 1963

Interviewed by: Thomas S. Kuhn and George Uhlenbeck
Location: Rockefeller Institute

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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 circa 100 atomic and quantum physicists. Subjects discuss their family backgrounds, how they became interested in physics, their educations, people who influenced them, their careers including social influences on the conditions of research, and the state of atomic, nuclear, and quantum physics during the period in which they worked. Discussions of scientific matters relate to work that was done between approximately 1900 and 1930, with an emphasis on the discovery and interpretations of quantum mechanics in the 1920s. Also prominently mentioned are: Gladys Anslow, Robert Fox Bacher, Ernst Back, P. A. Boeser, Niels Henrik David Bohr, Hendrik Brugt Gerhard Casimir, Walter Colby, Dirk Coster, G. H. Dieke, Paul Ehrenfest, Albert Einstein, Enrico Fermi, George Hartwig de Hass, Werner Heisenberg, David Inglis, Edwin Crawford Kemble, Ivan Robert King, Oskar Benjamin Klein, Ralph de Laer Kronig, Alfred Landé, Otto Laporte, T. van Lohuizen, Hendrik Antoon Lorentz, Fraulein Mensing, Edgar Meyer, Robert Andrews Millikan, J. Robert Oppenheimer, Friedrich Paschen, Wolfgang Pauli, Linus Pauling, Isidor Isaac Rabi, Harrison McAllister Randall, Adolf Smekal, Arnold Sommerfeld, Thomas, Uhlenbeck (George's father), George Eugène Uhlenbeck, Albrecht Unsöld, W. van der Woude, Vry, John Wulff, Pieter Zeeman; Universiteit van Amsterdam, Rijksuniversiteit te Leiden, University of Michigan, Teyler's Museum, Universität Tübingen, and Universität Zurich.

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Kuhn:

What I'd like most to try to do is to try to go back to substantially the time that you two started working together. This is presumably the time when you, George, were back from Rome, and Ehrenfest had said that Sam should teach you what has been going on, particularly with regard. to models and spectra, and you start working together.

Gousmit:

That must have been in June, the end of June, because then the academic year was over.

Kuhn:

Now Sam's impression and this adds something to what you've told me about this before, is that already at that time, he had been asked, presumably by Fokker, but it's not clear, to do this review piece for Physica. [Paper No. 20] Have you got that piece there, Sam?

Gousmit:

Ja.

Kuhn:

And a lot of the basis of this discussion was sort of working out that article, or was about ideas to which this was in any case to be a review of somewhat the ground you were supposed to be going over together. He thinks that this piece probably provided a considerable vehicle.

Gousmit:

That was the one I was writing during that summer, and which Ehrenfest said, "if you help me with that, then you'll learn at the same time all about it."

Uhlenbeck:

I have no recall that it was in connection with it, because I certainly didn't know anything about it.

Gousmit:

No. That's what I had to discuss with you, I had to teach you that, and I'm surprised at this moment reading it again, that there are things in there which I am so convinced that I didn't understand at the time; and why didn't you become the co-author? I'm sure that several things are mentioned in there which were not clear to me at that time, but I probably learned from you to understand them or appreciate them. Like the Pauli-Rumpf paper, which I had forgotten that I knew about until the other night when I reread this old article.

Uhlenbeck:

All the sophistic difficulties. I remember that we talked about that; because this summer what Sam did was give me, all the time, little lectures in the afternoon. Then of course about this, then for spectra; then it was not only spectra, also about resonance and about the influence of the polarization on resonance. All these things which were done in the current literature. I suppose that probably many of them were discussed in colloquia in Leiden while I was not there. But I don't remember that I helped writing anything, that I certainly didn't. You wrote this all alone. I didn't write anything.

Gousmit:

But the way it's written, I could only have gotten some of the .ideas by discussing it with you. Because I didn't know that much.

Uhlenbeck:

I don't know, I don't remember. I certainly didn't know as much as you did about these things at that time, that's for sure.

Kuhn:

You two are getting in the position now of trying to pass credit back and forth, and this is a useless occupation. This paper is, I think, particularly interesting because it's written about as early as a paper which reaches these particular conclusions could have written, and sets up the whole problem to which you two in your joint papers immediately go on very quickly — it sets these up very clearly. If it provided the context, or part of the context for this teaching seminar for George, it has then a double importance, as part of the background for spin.

Uhlenbeck:

Did this appear before that?

Gousmit:

Ja, ja. No, it just was printed—.

Uhlenbeck:

No, that was this paper on the Opmerking—

Gousmit:

The hydrogen —

Uhlenbeck:

The hydrogen spectrum. That is the one—

Kuhn:

The hydrogen spectrum actually comes out just a little bit before this review paper comes out.

Gousmit:

Ja. But this was written earlier, the review paper was written earlier.

Kuhn:

Yes. Let me say one of them is pages 266-270—the hydrogen and helium paper—and 281-292 is the review paper. So that really doesn't tell you anything about the order in which they were prepared. [#19 with G.E. Uhlenbeck: "Opmerking over de spectra van waterstof en helium," Physica 5(1925), 266-270. No date. August/September issue. #20 "Iets over spectra en atombouw," Physica 5(1925), 281-292. No date. October issue.]

Gousmit:

And the review paper has a footnote to our note in Naturwissensehaften "to be published".

Uhlenbeck:

Already!

Gousmit:

So I'm sure, if I taught you anything, that that is the extent of my knowledge at the time, this review paper.

Uhlenbeck:

Well,I must say, I'm surprised about this Naturwissenschaften, because that I would have placed later, certainly after that one. About this hydrogen. I still remember about the hydrogen very clearly. That came also one of these afternoons, and it must be relatively early—if I recall correctly, it came about from the point that I was so dissatisfied about the fact that one had to learn the theory for the alkali atoms and then one had to still keep the old theory for the hydrogn atom. I thought that was terrible. There should be at least some relation between the two. And that's the way it stood. At the end, when we discussed this, that it was clearly very unsatisfactory that there were two theories about things which should be similar.

Kuhn:

Did you know at that time that Sam's first article, about four year earlier, had dealt in a sense with exactly this problem?

Uhlenbeck:

Yes, yes, I knew that. Of course we talked about that too. And then Sam went home and wrote that paper. That was the "Opmerking".

Gousmit:

Which paper?

Uhlenbeck:

The hydrogen. That you wrote, very quickly, very quickly. And then we discussed it again. I think my contribution to it was mainly that I knew Kramers' dissertation. I had looked at Kramers' dissertation and found that line which Kramers simply passed over. Then it turned out that that was the line which was predicted by this scheme. That was a great satisfaction to me. And then also I told that to Sam, "look here, we have found this line which is already all the time in the books!" That was also mentioned then in the paper when it was rewritten a little bit about—. I would have placed that in the beginning of August, and I thought that the spin came afterwards.

Kuhn:

… George, there's one missing element in this, or at least, one that looks like a missing element. In the hydrogen-helium paper, a lot of use is made of a paper by Wentzel.

Gousmit:

Yes. It was a vehicle, like rice is for certain meals. You see, it really had nothing to do with it.

Uhlenbeck:

No, but I remember that we discussed this Wentzel paper and we couldn't make heads or tails of it really.

Uhlenbeck:

Well, it must be so that even at that time it took four or five weeks before these things appeared.

Gousmit:

In Physica?

Uhlenbeck:

Possibly. And in September, when I think the spin was discovered— Sam says it was in August. Then you perhaps put that note in in proof, you could have done that.

Kuhn:

Probably not, because it's numbered consecutively, it's note number 2. Maybe Physica is different from most, but usually you're not allowed to make that sort of change in proof.

Gousmit:

May use this as exhibit A? This I just discovered. It has no precise date, but we may be able to date it.

Kuhn:

This is the letter from Ehrenfst to you [Goudsmit] in Amsterdam.

Uhlenbeck:

[Reads the letter] I don't recall that.

Kuhn:

… It's this letter which helps drive home the one discrepency. You remember you told me, George, that after getting the Lorentz paper or manuscript, the two of you went to Ehrenfest and said,we'd better withdraw it. Ehrenfest said, I've already sent it off.

Uhlenbeck:

That I stilI remember very clearly.

Kuhn:

But this doesn't sound that way. And Sam's recollection is that he wasn't bothered by the Lorentz thing at all, and didn't want to withdraw it.

Gousmit:

I probably was in Amsterdam.

Uhlenbeck:

Maybe I was alone; I don't know whether we were together, but I remember that I talked with Ehrenfest. I talked, I think, with Lorentz alone, maybe Sam was not there.

Gousmit:

I don't ever remember.

Uhlenbeck:

I still remember the manuscript that he gave me. But here [in Ehrenfests' letter to Goudsmit] the Naturwissenechaften article—ja, that's mentioned.

G:

. Ja. Apparetly I was writing it, and had sent a copy to Leiaen, and be [Ehrenfest] said, "don't send it off until Wednesday night, because there are still some discrepancies." Then there is on the side, it says, "oh no, you found you had made a mistake."

Uhlenbeck:

But in this paper we didn't say anything about the g sums, did we?

Gousmit:

No, no, no. But you were checking up whether the idea would fit it. You thought you had found a discrepancy.

Uhlenbeck:

This. I have completely forgotten; I don't know what this is. I didn't even know that I did something about the g sums or found some errors.

Kuhn:

Regardless of the date, do either of you have any particular recollections of the point at which spin enters? Not as to whether it comes in August or September, but where it comes relative to the development of ideas. What, for example, is its relation to the hydrogen-helium paper?

Gousmit:

I have a recollection.

Uhlenbeck:

Ja, I also have a recollection of how it came. In my recollection, it came on an afternoon that we talked about. Then it was always this question of the 13 quantum numbers, and I still remember that I said, if there are 4 quantum numbers, there must be four degrees of freedom. Do you remember?

Gousmit:

See, it checks, that's exactly what I said, and it came when I was trying to explain the Pauli principle to you, how you get these terms with the four quantum numbers.

Uhlenbeck:

And if there are four degrees of freedom then there must be some kind of internal—. And that was really the starting point. But, and you must remember too, Sam, that we did not think of publishing that at all at the moment when we said that. Because I still remember that you. said—I think I even have a letter from you —- wouldn't it be nice if one could write such thing down, but they are clearly too speculative.

Gousmit:

I'd like to see that letter.

Uhlenbeck:

And then on1y after having talked with Ehrenfest, I think, we were encouraged to write something down. But I remember this discussion with Ehrenfest very clearly, that he immediately was, so to say, convinced. Ehrenfest was immediately convinced. /

Gousmit:

He said to me, "you have no reputation to lose yet, go ahead."

Uhlenbeck:

"Beide sind jung, genug, um sich eine Dummheit leisten zu koennen." That's what he said. I don't remember about these g sums. And it is also clear, we wrote afterwards a paper on spectra, and that was about g-sums and so on.

Gousmit:

We wrote it afterwards but it came out of the summer discussions.

Uhlenbeck:

Ja, but that was really spectroscopy. That was these different coupling schemes. That was later really. And the spin — after the Naturwissenschaften paper—stood for a while without further consequences, so far as I remember. It was not so that we continued with it, until, really, the Heisenberg letter came. Then came Heisenberg' s letter. You may have that still.

Kuhn:

It's here.

Uhlenbeck:

And that put us onto it again. There was, of course, this factor two, the Thomas factor two that he said, 'how do you avoid it?' And then it is also so that he had, of course, derived the—.

Gousmit:

But he didn't give the derivation, he just gave the result.

Uhlenbeck:

That's the way I remember it. And we didn't know how to derive that. That was not clear to us. Then we worked like a dog. And there I think that's the only place that van der Waerden is approximately right. Einstein was at Leiden at that time. I remember still that one Sunday Einstein once said "you must go take the co-ordinate system in which the electron is at rest. Then you get it. Then immediately for circular orbits it came out directly, without—. Then for elliptical orbits—I had to study perturbation theory. I did that very hard out of Born's book, and then finally with the fomulas there, you did it also for elliptical orbits without any difficulties afterwards.

Of course I didn't know that, and even Ehrenfest hardly knew that sort of thing because we were educated in such a qualitative way, always qualitative, and always with simple models, never long calculations. Then, of course, we had the same factor two, but then at least we could answer Heisenberg that we also didn't know how to get a factor two, [laughter].

Gousmit:

You might read this sentence here: that's a letter of Bohr to Kronig [26 March 1926]—that one sentence down there near the end. [Einstein's remark that the coupling between the spin axis and the orbital motion was a consequence of the theory of relativity was a "revelation"]

Uhlenbeck:

Ja, that was with Einstein, ja. This letter, has this letter. Ja, it's a very dark one why that is a revelation. Because if you thimk of it, it is so you don't need relativity theory, although I remember that we used these formulas for the transformation of the electric and magnetic fields. Although it was just Biot-Savart. But it just shows that everything was so strange. Why Bohr was so impressed by it—.

Kuhn:

That's very hard to understand.

Uhlenbeck:

Very hard to understand. One would have thought of Bohr that he would say it right away he didn't understand. And Bohr was then—that was December, I'm sure—that was the Lorentz festival. He came then to Leiden and he talked at length with us.

Kuhn:

Presumably he and Einstein were there at the same time.

Uhlenbeck:

Yes. But when he talked with us, I don't think Einstein was present.

Gousmit:

Yes, several times. I remember that drawing the diagram on the blackboard of the hydrogen structure, and that Einstein and Bohr were sitting there, but Einstein did not understand it, he did not know enough about details of spectroscopy.

Uhlenbeck:

I have not an impression that Einstein took part in the discussion very much.

Gousmit:

No, but he was present, I remember that, at some of the discussions. We went there many times, remember?

Uhlenbeck:

Many times, ja, many times. -

Gousmit:

And the blackboard was not allowed to be erased, everything was on there.

Uhlenbeck:

And Bohr insisted that now one should look, at hydrogen again. He did not know this paper.

Gousmit:

And that I told him at the time.

Uhlenbeck:

He did not know this paper. It was in Dutch of course, too.

Kuhn:

Why did you bury that paper in Dutch?

Uhlenbeck:

I don't know. That was the easiest language to write, and it was also the quickest to publish for us, I think. No real reason for it. The Naturwissenschaften, that wasn't, that —. .

Kuhn:

Ja. Your doctoral exam was in December. So Einstein was there in December, and Bohr was there too.

Uhlenbeck:

Ja, that's right, Sam had not done his doctoral examination. That was right. I had done that of course.

Gousmit:

You see how he signs that little note, (in Dutch) or whatever it is. [chuckle]

Kuhn:

It doesn't look to me as though you two are going to succeed in recreating—. Maybe we should get Sam to go to the board and give one of the lectures he gave you—.

Gousmit:

We had no board, that was one of the troubles.

Uhlenbeck:

We did it all on paper. We sat next to eachother, and he did it on paper. I don't have these things anymore. It was—.

Gousmit:

But it was the Pauli principle.

Uhlenbeck:

The four quantum numbers; that was for sure. All the further developments were, so to say, after Bohr had it accepted, so to say. There was no doubt for us, of course, anymore, although we didn't know the factor two still, at all. No idea.

Kuhn:

What is the date on that Heisenberg letter, the first of the Heisenberg?

Gousmit:

November. It was right after it appeared. I think a day after the date of Naturwissenschaften.

Uhlenbeck:

It was very close anyway.

Gousmit:

Immediately after.

Kuhn:

Yes, November 20 was the issue of Naturwissenschaften.

Gousmit:

[checks letter] And this is November21.

Uhlenbeck:

Ja, it was very quick.

Gousmit:

Immediately. Then I wrote him two letters, I don't know what's in them, and then he wrote again in December, the 12th. And that's a most interesting letter, December 12. About his own work. I think this is historically more interesting, where he says what he is doing.

Uhlenbeck:

And you went to Copenhagen I think in January?

Gousmit:

Ja.

Uhlenbeck:

And after that I didn't do spectroscopy anymore, except that we wrote this paper.

Gousmit:

But that was really written during the summer, or at the end of the summer.

Uhlenbeck:

No, this was later; it was the review paper.

Gousmit:

Oh, the review paper!

Uhlenbeck:

Then you wrote the review paper.

Gousmit:

I thought you were thinking of the Opmerking paper.

Uhlenbeck:

No, no, the review paper, that was in that spring that we—. There is nothing that one can say.

Gousmit:

There is one discrepancy, or credit. I claimed that I was the one who knew that helium Paschen line so well, and the forbidden component. You claim that you are the one.

Uhlenbeck:

No, I found that in Kramers dissertation, I didn't know that line, of course.

Gousmit:

I knew it, I had seen the—you see, the first thing I ever saw was Paschen showed me that line, and I had studied, of course, these things in Sommerfeld's book. I still—I got a Paschen reprint that I had had for a long time, with the different lines.

Uhlenbeck:

But then it must have been so that because it was in Kramers dissertation it was discussed for the Stark effect, and also a lot of it was understood, all right, but this line was standings there and was not in his calculation, and that made an impression on me.

Kuhn:

Except that in one respect at least, I think, you don't remember this quite right, George, —because at least in the paper you point out that Kramers has explained this line—I forget how—but by some sort of interaction with other electrons, and then you point out that if that were the right one, then there would have to be two lines, one of which is not observed.

Uhlenbeck:

Ja, ja, something like that. So, it's perfectly possible that Sam knew the line, and that what you did was to take the thing in Kramers' thesis and do the further analysis as to what was wrong with the way Kramers handled it.

Gousmit:

I don't see it as discrepancy.

Uhlenbeck:

That was one line which stood out.

Gousmit:

Ja, we'll have to look at it. [They look it up]

Uhlenbeck:

I would have to study this again. But the point was this, that if one tries to explain it by an accidental presence of an electric field, then necessarily also component (3D) should occur — and even with greater intensity. See especially figure 10 of the dissertation of Kramers.