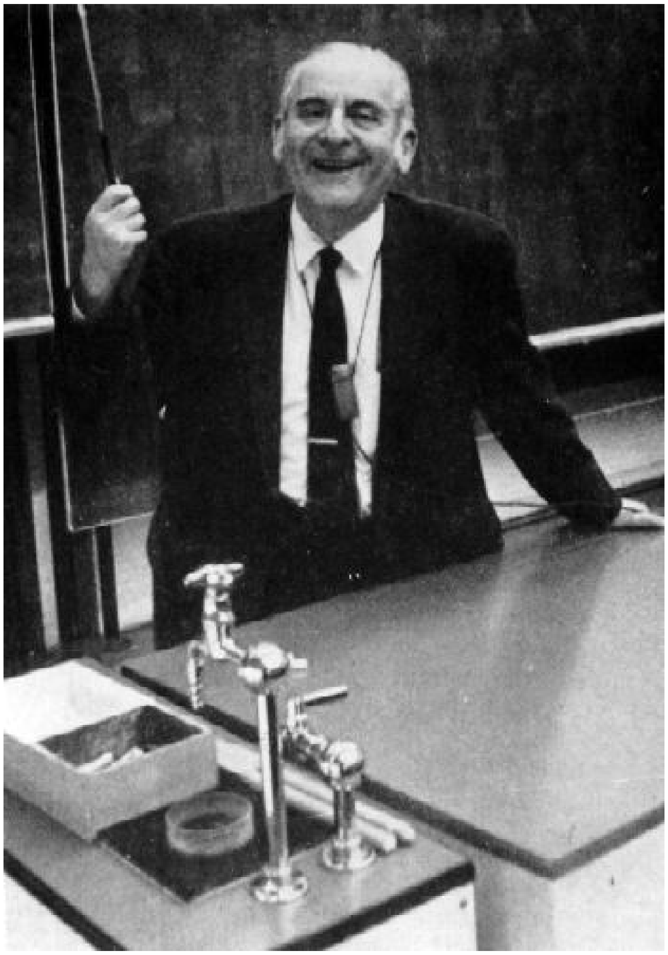
The discovery of the electron spin

S.A. Goudsmit

The golden jubilee of the Dutch Physical Society in April 1971 was concluded with a lecture by Samuel Goudsmit on the history of the discovery of the electron spin. Actually, his could hardly be called a polished lecture; it was a grandiose artistic performance, full of wit and emotional involvement. Goudsmit, then at the end of his scientific career, gave a very personal account of how chance and the guidance by Ehrenfest, their far-sighted supervisor, led him and Uhlenbeck to formulate their remarkable discovery. When, in connection with the present book [*], the question turned up how to discuss the early history of electron spin, my thoughts returned to that day, nearly twenty five years ago, when I had been impressed by Goudsmit's truly humane wisdom. After weighing various alternatives I thought: why not let the master speak for himself? Thus I came to translate Goudsmit's historic lecture. Its text was not meant to be published as a paper, but Goudsmit subsequently consented to its publication from a tape recording [1]. Apart from a few minor changes I have tried to present Goudsmit's very personal style by giving a literal translation of the words spoken in Dutch. A number of references to the papers mentioned by Goudsmit have been added.

J.H. van der Waals

Today I will talk a little about history. The history of the discovery of the electron spin by George Uhlenbeck and myself. That is tricky; I don't like the history of physics, I have always been against the way in which the historians wrote about it in earlier days. Nowadays it is better; someone like Martin Klein, that is real, he brings something new. But the earlier historians always described physics as if it had been done by three or four people and they forgot that these famous people could only do their work because of the many other who also made contributions. They can't help it since that is the way



Goudsmit delivering his lecture in 1971.

they have learnt it from the ordinary historians. You hear about a man like Hitler .... and they forget the millions who lent him the necessary support.

Then, today, there are other people who are interested and they are the psychologists. They want to know why you became a physicist, why you did all you have done, and start to interrogate you about that. They want to know about your family, hoping that your grandfather was a great chemist or a great mathematician, and then they are always terribly disappointed when they come to me. Because, when I first registered as a student in Leiden the Beadle said: "The Rector would like to see you a moment". He took me to that room with all those portraits of famous people and there, next to the portrait of Hugo de Groot, hung a large painting of a famous jurist. "Here", he says, "is your grandfather", I reply: "I have never heard of this man". The great jurist's name being Goudsmit, my reply made him angry. Actually, with my own family the psychologists could do nothing. My grandfather was a tourist guide in Hotel des Indes in the Hague, my mother had a millinery, and my father a wholesale business in seats ...... for water closets.

What the historians forget - and also the physicists - is that in the discoveries in physics chance, luck plays a very, very great role. Of course, we do not always recognize this. If someone is rich then he says "Yes, I have been clever, that is why I am rich"! And the same is being said of some one who does something in physics "yes, a really clever guy.....". Admittedly, there are cases like Heisenberg, Dirac and Einstein, there are some exceptions. But for most of us luck plays a very important role and that should not be forgotten.

And this is relevant because, when I went to Leiden, I ended up with Ehrenfest. Ehrenfest's classes were small and one had a very good interaction with one's profess And Ehrenfest was always worried when we interrupted our classes when we had to g somewhere. Once I had to accompany my father to Germany, because of his business, and then Ehrenfest said: "Do you again have to interrupt your classes?" But my father could not travel alone. Then he asked: "Where are you going?" When I told him, he said: "Nearby is a university and there is a spectroscopist, Paschen. You are interested in spectroscopy (I had become interested in it through my high-school teacher Lohuizen), go and have a look". That was important; I have done it. I went to visit Paschen, who did not treat me as a student but as a colleague. And he showed me the experimental set up which he had for the study of the spectral line of ionised helium, which entirely confirmed Sommerfeld's relativistic electron orbits. I did not understand a bit of it. But, I think, I managed to hide my lack of understanding and after my return to Leiden I have nicely studied all this. One of the things which stuck to me is that in Paschen's experiments on the helium line, its fine stucture and the relativistic explanation, there was a forbidden component which was obviously present. The following summer I was sent for a stay to Paschen, and Paschen and Back have taught me the techniques of spectroscopy. And when I talked to the theoreticians about that forbidden component .... but you know how theoreticians are ...... they then say: "Poor experiments". That forbidden line already was an important milestone. I shall recount a few more of these milestones.

If I talk in the first person, then there are two reasons. First: lack of modesty, and second: as I tell that history, I can only speak about my own experiences. You know, when Uhlenbeck tells the history of spin then he tells a different story. I don't think either of us lies. But if someone is lying then it is a little more I than he.

I was interested in spectral lines and the first thing I did .... I found a formula for the doublets in the spectra, claiming that it was exactly the same formula as used by Sommerfeld for the X-ray doublets. And I told this to Ehrenfest. At that stage it was all wrong but Ehrenfest never discouraged anyone and said: "That's nice, we'll publish it". And there was a short little piece in "Naturwissenschaften" and a very lengthy article in "Archives Néerlandaises des Sciences exactes et naturelles", which was published in Holland in french to be sure that nobody would read it. Of course, as a young student I was very proud of it.

Now, what happened? Two and a half years later exactly the same work was done, the very same formula, by Millikan in America, and Koster gave a seminar about it in Leiden. Of course he did not know that I had already done so. At the end of the seminar I said: "I have spoken about the very same, here, two and a half years ago".

Now there is an important point I want to make. Do I have to get recognition from the historians that I was the first? I had simply guessed it while Millikan, when he obtained the formula, had new experimental material which demonstrated its correctness. One did not understand that the formula was correct, but the new experimental data made it clear that he was the one who had the right formula. He had reasons for it, I had simply guessed, I could not even convince Ehrenfest, and it was published in french .....

In these days Kronig came from America and he came to Leiden; we collaborated in spectroscopy and worked on the intensities in the Zeeman effect for which we found the exact expressions [2]. Of course, it was quite different from today; there was no quantum mechanics at the time, don't forget that this did not yet exist! One had to guess these little formulae; one developed a feeling for them. It is just as with veterinary and human medicine. People can tell one where it hurts, but a veterinary doctor has to guess where it hurts. A horse or a cow cannot tell that. And so it is with these little formulae. It is really curious ...... it was a kind of numerology, and it is a miracle that we arrived at the correct expressions which later could be derived by quantum mechanics. Now, when it is derived it becomes quite simple. If one knows some mathematics, then one can derive all those things. We had to guess at them; I had a feeling for that. And that is the way Kronig and I did those things.

Well, Ehrenfest soon found out that I was not a proper theoretician and then he sent me to Amsterdam. Three days a week I was part-time assistant with Zeeman and things were quite different during those days. For instance, Wednesday evenings I took the train back to Leiden and then had a feeling I ought to switch; the jokes one heard and recounted in Amsterdam could not be used in Leiden. That was not done; they were not proper enough. In Amsterdam it was quite friendly. Professor Zeeman, of course, was somewhat more formal than I had been used to.

And I did something else at the time. The Pauli principle was published early in 1925 [3]. I am convinced that although it is one of the most important publications in physics, who reads it now, of the younger generation, will find it hard to understand. Even that one will not understand it all. And I wrote a note in May [4] that the Pauli principle became easier to understand when introducing different quantum numbers. The quantum numbers I used for Pauli's principle were m_L and m_s; m_s being always the same, plus or minus 1/2. (In those days it was slightly different, one used 1 and 0, but that does not really matter.) And if you used these for the Pauli principle, then it became much simpler ...., as one does today of course. You people don't know that such a change was necessary but Pauli had introduced different quantum numbers. As a mathematician said, the change amounted to a simple linear transformation - which is trivial, mathematically trivial of course, but not so for the understanding and in teaching.

Well, I had introduced those quantum numbers, but if I had been a good physicist, then I would have noticed already in May 1925 that this implied that the electron possessed spin. But I was no good physicist, I am no good physicist and thus I did not realize this.

I sent my note to Copenhagen to get an opinion from Kramers and Kronig; Kronig then having left for Copenhagen. I received a long letter from Kronig about other things but he did not say anything about my note. That did not interest him, apparently. This is another important point, besides Paschen's forbidden line, the forbidden fine-structure component. That was all in the spring of 1925. Then Uhlenbeck appears on the scene.

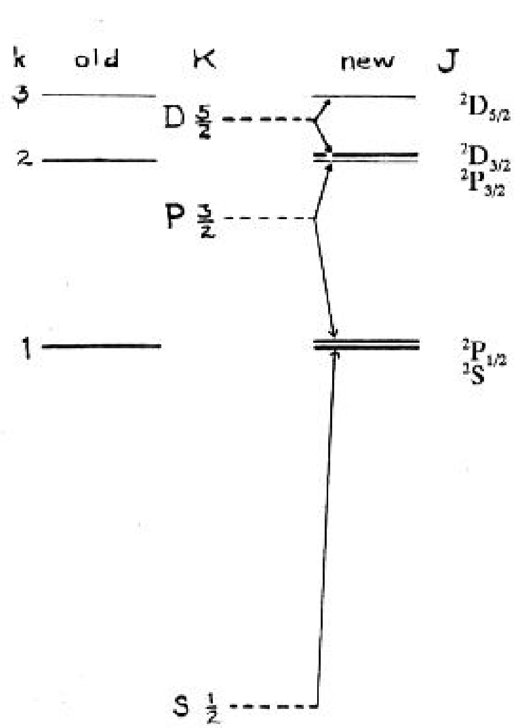
George Uhlenbeck had interrupted his research to become tutor of the children of the Netherlands ambassador in Rome. He must have done this very well because one of them later made it to ambassador in Washington, Van Rooyen. But, as Ehrenfest said: "there in Italy he has learnt nothing of those new things, there they only know classical physics". And George Uhlenbeck, who was there, has also studied classical physics; when he came home in the summer Ehrenfest said: "You should work together with him for a while, then he may learn something about the new atomic structure and all that spectral business". What he clearly thought, of course, was: "Perhaps I might learn a little bit of real physics from Uhlenbeck".

Leiden 1924. From left to right: Dieke, Goudsmit, Tinbergen, Ehrenfest, Kronig, Fermi. Note: Tinbergen later changed from physics to economy and became the first Nobel laureate in economy (1969).

So Uhlenbeck knew nothing about the new physics, but yet he did an important thing for the modern physics that was to come. Ehrenfest had written him a letter in which he said: "I have read an article by a young man, it looks nice and one ought to try and see him". Well, in those days, when your professor wrote you, you did it. And George Uhlenbeck went to see that young man; the young man just came back from Germany and was totally discouraged. He had spent a semester in Göttingen and there they had given him a treatment: "Well that man cannot know anything, besides being too small he never studied anyplace worthwhile". So the young man really got discouraged and meant to give up physics. But Uhlenbeck said: "Don't do that before you first talk to Ehrenfest; come and see Ehrenfest." And the man came to Leiden and stayed for two or three months with Ehrenfest, of which I can show a picture. A well-known picture that you may have seen before: there is that young man, Enrico Fermi. And under Ehrenfest's encouragement it dawned on him that he really was a competent physicist. And if you look at Fermi's career .... those are the days in which he really became a great physicist.

In any case, Uhlenbeck came to the Hague - where I lived and he lived there too. I had promised to write a short article for "Physica", then in Dutch, and I did it together with him, which was really great. Because he knew nothing, but was so good; he asked all those questions I had never asked, and from that collaboration to make things clear emerged a few, as we now know, important results. One of the first results that came out was a new interpretation of the spectrum of hydrogen. We had Sommerfeld's hydrogen spectrum, and for formal reasons and because I had investigated all those things, we obtained a new interpretation of the hydrogen spectrum. The new term scheme. I have a picture of it, but you know it because that is what you learn today. On the left is the old Sommerfeld scheme, on the right the real one .... And the curious thing is that I, because I knew all these intensity rules and so forth, had already guessed the correct formulae. That was my contribution; that I knew which formulae one had to take. One took the classical expressions and instead of integral quantum numbers one put in half integral quantum numbers and did a few other things, it was like magic, but it nevertheless precisely fitted, and what I found so delightful - if you really believed it - then the "forbidden" line which Paschen had seen was not forbidden but a real spectral line which ought to be present and that gave me a lot of fun.

And this, of course, is something I want to say again; people don't believe it. In the beginning when you do something you never know whether it is important or not, and we absolutely had no idea that a new interpretation of the hydrogen spectrum was important. Therefore, this was published in "Physica", in Dutch [5]. We also had an article about those quantum vectors **L** and **S**, the coupling of quantum vectors, all that tommy rot, I don't know how you call it, and that was sent off to the "Zeitschrift für Physik". Do you note the difference? We did not know what was important. Everyone worked on those quantum vectors and that was published in the "Zeitschrift für Physik". The hydrogen spectrum was published in "Physica", but you note, this spectrum pointed in the right direction.



The old and the new term scheme of hydrogen [5]. The scheme shows the multiplet splitting of the excited states of the hydrogen atom with principal

When the day came I had to tell Uhlenbeck about the Pauli principle - of course using my own quantum numbers - then he said to me: "But don't you see what this implies? It means that there is a fourth degree of freedom for the electron. It means that the electron has a spin, that it rotates". Now, I can also exactly tell you the difference between Uhlenbeck and me as physicists. In those days, all through the summer when I told Uhlenbeck about Landé and Heisenberg, for instance, or about Paschen, then he asked: "Who is that?" He had never heard of them, strange. And when he said: "That means a fourth degree of freedom", then I asked him: "What is a degree of freedom In any case, when he made his remark, it was luck that I knew *quantum number n=3, presented by Goudsmit in the form in which it appeared in the original publications of1926. The assignment in the current notation has been added at the right. With the development of quantum mechanics the notation changed. The quantum numbers L and J now usedfor the orbital and total angular momentum, respectively, correspond to K-1/2 and J-1/2 in the figure. The "forbidden component" referred to by Goudsmit is of the type 3 $^2P_{1/2}$ --> 2 ^2S in which the total angular momentum is conserved and L changes by plus or minus 1.*

all these things about the spectra, and I then said: "That fits precisely in our hydrogen scheme which we wrote about four weeks ago. And if one now allows the electron to be magnetic with the appropriate magnetic moment, then one can understand all those complicated Zeeman-effects. They come out naturally, as well as the Landé formulae and everything, it works beautifully".

And that was it: the spin; thus is was discovered, in that manner. Of course we told Ehrenfest about it and then summer was over and I went again to Amsterdam and various episodes followed. Naturally, I found it wonderful, because in the formalism which I knew it fitted perfectly. And the rigorous physics behind it I did not fathom. But Uhlenbeck, being a good physicist, started to think about it. ...... "A charge that rotates"......? He claims that he then went to Lorentz and that Lorentz replied: "Yes, that is very difficult because it causes the self energy of the electron to be wrong".

And Uhlenbeck also tells you that .... We had just written a short article in German and given to Ehrenfest, who wanted to send it to "Naturwissenschaften". Now it is being told that Uhlenbeck got frightened, went to Ehrenfest and said: "Don't send it off, because it probably is wrong; it is impossible, one cannot have an electron that rotates at such high speed and has the right moment". And Ehrenfest replied: "It is too late, I have sent it off already". But I do not remember the event, I never had the idea that is was wrong because I did not know enough. The one thing I remember is that Ehrenfest said to me: "Well, that is a nice idea, though it may be wrong. But you don't yet have a reputation, so you have nothing to lose". That is the only thing I remember.

Well the note was submitted and published [6]. Directly, the next day, I received a letter from Heisenberg and he refers to our "mutige Note" (courageous note). I did not even know we needed courage to publish that. I wasn't courageous at all. I think I still have Heisenberg's letter. In it he writes a formula .... I did not understand a bit of it. And then he says somewhere: "What have you done with the factor 2?" Which factor? Not the slightest notion, and the formula given without derivation.

I told you, the spin fitted nicely into the whole formalism. But, of course, we also ought to have made a quantitative calculation of the size of the splittings. If one believed in the spin, then the spin can be "up" or "down", and what is the difference in energy - does it come out correctly? We had the formulae already, but was it possible to derive these formulae? We did not do that because we imagined it would be very difficult. Now every beginning student does it; what do you call him? ..... a freshman, a greenhorn? He manages, but we didn't know how to do it, and therefore we had not done it. Luckily we did not know, because if we had done it, then we would have run into an error by a factor of 2. That would not have fitted, but we did not know; all other things fitted perfectly, yet this does not.

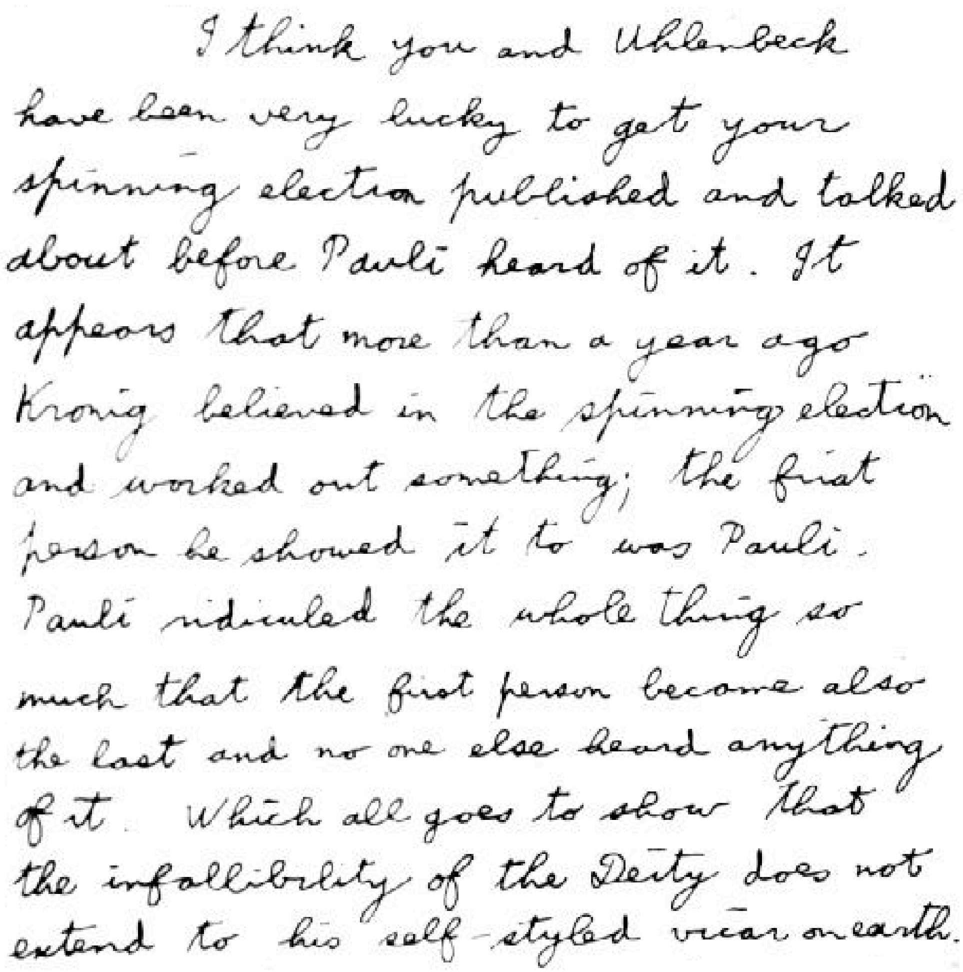
Well, we were discouraged but, again, it was a matter of luck. Just in those days Lorentz celebrated the fiftieth anniversary of his doctorate. And Bohr and Einstein and many other great scientists came to Leiden. And Bohr had seen our note and was quite interested. Every day we had a meeting, a get together with Bohr, Einstein and Ehrenfest about the problem of the spin and all those things, at Ehrenfest's home. There we learned a lot.

In passing I have to mention a typical Ehrenfest anecdote, not such a nice one, perhaps. Lorentz lived in Haarlem and all these celebreties, Rutherford, Madame Curie, Bohr, Einstein and very many others travelled by train, a special train, from Leiden to Haarlem. And the week before one of those rare fatal train accidents had occurred and I said to Ehrenfest: "Wouldn't it be dreadful if that train had an accident?" And Ehrenfest replied: "Yes, that would be dreadful, but think of all the young physicists who then could get jobs ......".

Bohr was highly optimistic, in particular when he saw that I had already all the formulae for the fine structure. And he thought perhaps, that it [i.e. the factor 2] is something trivial; probably something relativistic. I have never understood the argument precisely. When Bohr and Einstein were talking together at the Ehrenfests', I did not understand a bit of it.

Anyway, Bohr made one mistake. Instead of Uhlenbeck he invited me to Copenhagen, to see if I might learn something there. That did not work, of course, and after six weeks he presented me with a first-class railway ticket, to go back to the Hague. But in Copenhagen there was a young man, Thomas, who was thoroughly acquainted with the theory of relativity. While I was there he worked out that Heisenberg's factor of two - which seemed lost - actually represented the relativistic factor and everything was in order [7].

The man who never cared to believe in the spin was Pauli. And then Bohr said: "On your way home you should stop off at Hamburg and explain the factor 2 to Pauli". I have tried to do so, but because I did not really understand it myself I, naturally, was unable to explain it to Pauli .... But Pauli did not want to believe it; on my return Einstein still was in Leiden and I had to explain it to him too, which went even worse. I did not manage, but later I received a postcard from Pauli that he had seen Thomas's work and that he believed in it.


I think you and Uhlenbeck
have been very lucky to get your
spinning electron published and talked
about before Pauli heard of it. It
appears that more than a year ago
Kronig believed in the spinning electron
and worked out something; the first
person he showed it to was Pauli.
Pauli ridiculed the whole thing so
much that the first person became also
the last and no one else heard anything
of it. Which all goes to show that
the infallibility of the Deity does not
extend to his self-styled vicar on earth.

Part of a letter by L.H. Thomas to Goudsmit (25 March 1926). Reproduced from a transparency shown by Goudsmit during his 1971 lecture. The original is presumably in the [Goudsmit archive] kept by the AIP Center for History of Physics.

Well, that would have been the end, as I thought myself. Thomas was to return to England and wanted to travel via Holland to visit me, so he wrote me a letter. Here is a part of that letter. I think this represents an important point, and in particular the historians, naturally, enjoy such a thing. The historians, they always try to trace someone who, somewhere in a dark chest, has already hidden Einstein's theory. But this they also found wonderful.

Now this is dead certain. If Kronig had not left Leiden and had stayed with Ehrenfest, then things would have taken another course. Ehrenfest would have encouraged him and said: "That you ought to publish". With Pauli, of course, it was entirely different. But admitting the great difference in this respect, if one looks objectively .... In the days that Kronig had that idea then the new interpretation of the hydrogen spectrum did not exist, m_L and m_s did not exist, and he may not have known about these forbidden components because they did not interest him. Thus, actually, the material that convinced people that it was right simply did not exist. Also, Kronig was not really th first. The first one to publish about the quantized electron - Kronig did not do so - was Compton. For reasons that were totally erroneous, he had said some four years before in the "Journal of the Franklin Institute": "Perhaps there exists a quantized rotation of the electrons". But the reasons he had given were wrong and unconvincing.

Then there was a short paper by Kennard, an American physicist, who had slightly more convincing arguments, but insufficient to make people believe him. Urey had thought about it but did not publish it. When Kronig read our paper he published two articles to prove that we were wrong; in "Nature" and in the "Proceedings of the National Academy" in Washington .... Therefore, I find it a little strange if some historians say: "Kronig did it, really, you people did not do it". That is the same historian who says .... that is merely a linear transformation and, therefore, a trivial contribution.

That is the way the history looks and it is a somewhat curious history. Who, precisely, should get credit for it? Such things are not possible without also giving credit to all other people who have contributed. But one aspect stands out which is of particular importance for young people. First: you need not be a genius to make an important contribution to physics because, I do admit, the electron spin is an important contribution. That I know now, then we did not know, but now I do. They all told me so.

Then I want to say one more thing: even if you make a minor contribution, if it is not important, then this gives an enormous satisfaction. Therefore I do believe that one should not always aspire to tackle what is most important, but try to have fun working in physics and obtain results.

References

[*] Foundations of Modern EPR, edited by G.R. Eaton, S.S. Eaton, and K.M. Salikhov (World Scientific, Singapore, 1998).

[1] S.A. Goudsmit, *De ontdekking van de electronenrotatie*, Nederlands Tijdschrift voor Natuurkunde **37** (1971) 386.

[2] S. Goudsmit and R. de L. Kronig, Naturwissenschaften **13** (1925) 90; Verhandelingen Koninklijke Akademie van Wetenschappen **34** (1925) 278.

[3] W. Pauli, Z. Physik **32** (1925) 794.

[4] S. Goudsmit, Z. Physik **32** (1925) 111; the relation with our current notation is somewhat less trivial than suggested by Goudsmit in his lecture.

[5] S. Goudsmit and G.E. Uhlenbeck, Physica **6** (1926) 273.

[6] G.E. Uhlenbeck and S. Goudsmit, Naturwissenschaften **47** (1925) 953. A subsequent publication by the same authors, Nature **117** (1926) 264, is followed by a very interesting postscript by N. Bohr.

[7] L.H. Thomas, Nature **107** (1926) 514.