

longing in the public domain after two years instead of after fifty years. I did my best to adapt our journals to the present-day hectic activities, for example by creating *Physical Review Letters*. But that is not enough. The *Physical Review* has to change further also. That journal reminds me of an old mansion, still inhabited by remnants of a family that gradually has lost its fortune and its servants but clings to outer appearances. The physics community clings to the journal's format, which is too impressive a facade for contents no longer very impressive. Just read almost any article in

the *Physical Review* of the 1920's and 1930's to see the difference.

Now a final remark. Many young people believe erroneously that wisdom comes with age. On the contrary, age brings fear of novelty and progress, fear of loss of status. Almost forty years ago I listened to the great Arthur Eddington lecturing about the fine-structure constant, 137. The little I understood was obviously farfetched nonsense. I asked my older friend, H. A. Kramers, whether all physicists went off on a tangent when they grew older. I was afraid. "No, Sam," answered Kramers. "You don't

have to be scared. A genius like Eddington may perhaps go nuts, but a fellow like you just gets dumber and dumber."

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This article is an adaptation of a talk presented 2 February at the joint New York meeting of The American Physical Society and the American Association of Physics Teachers as part of a symposium celebrating the 50th anniversary of the discovery of electron spin.

Reference

- 1 E. Chargaff, *Science* **172**, 637 (1971). □

FIFTY YEARS OF SPIN

Personal reminiscences

How one student who was undecided whether to pursue a career in physics or history and another who had not taken his mechanics exam came to identify the fourth atomic quantum number with a rotation of the electron.

George E. Uhlenbeck

In a one-page Letter to the Editor of *Naturwissenschaften* dated 17 October 1925, Samuel A. Goudsmit and I proposed the idea that each electron rotates with an angular momentum $\hbar/2$ and carries, besides its charge e , a magnetic moment equal to one Bohr magneton, $eh/2mc$. (Here, as usual, $\hbar$ is the modified Planck constant, m the mass of the electron and c the speed of light.) Sam, in his accompanying article, tells something of those times, fifty years ago. We have often talked about the circumstances that led to our idea, but it was mainly Goudsmit's recollections that have appeared in print before now—they are, however, not readily accessible in English.^{1,2,3} Although I gave a short account⁴ of the discovery of the spin as a part of my inaugural address for the Lorentz professorship in Leiden in 1955, it therefore appears to be my turn to reminisce.

I am a bit reluctant to do this; first, because my memories differ only in emphasis and in a few details from Sam's recollections, and second, because to describe the personal relationships and the circumstances properly requires, I think, almost an autobiography! However, since this is of course not meant to be a contribution to the history of the great consolidation of the quantum theory in the 1920's, I will just try to tell my side of the story, for what it is worth.

Note that I do not use the modish

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Ersetzung der Hypothese vom unmechanischen Zwang durch eine Forderung bezüglich des inneren Verhaltens jedes einzelnen Elektrons.

§ 1. Bekanntlich kann man die Struktur und das magnetische Verhalten der Spektren eingehend beschreiben mit Hilfe des LANDÉschen Vektormodells R, K, J und m . Hierin bezeichnet R das Impulsmoment des Atomrestes — d. h. des Atoms ohne das Leuchtelektron — K das Impulsmoment des Leuchtelektrons, J ihre Resultante und m die Projektion von J auf die Richtung eines äußeren Magnetfeldes, alle in den gebräuchlichen Quanteneinheiten ausgedrückt. Man muß dann in diesem Modell annehmen:

a) daß für den Atomrest das Verhältnis des magnetischen Momentes zum mechanischen doppelt so groß ist, als man klassisch erwarten würde.

b) daß in den Formeln, wo R^2, K^2, J^2 auftritt, man diese durch $R^2 - \frac{1}{4}, K^2 - \frac{1}{4}, J^2 - \frac{1}{4}$ ersetzen muß. (Die HEISENBERG'sche Mittelung²).

Dieses Modell hat sich äußerst fruchtbar gezeigt und hat u. a. geführt zur Entwirrung der verwinkeltesten Spektren.

§ 2. Man stößt aber auf Schwierigkeiten, sobald man versucht, das LANDÉsche Vektormodell anzuschließen an unsere Vorstellungen über den Aufbau des Atoms aus Elektronen. Z. B.:

a) PAULI³ hat schon gezeigt, daß bei den Alkaliatomen der Atomrest magnetisch unwirksam sein muß, da sonst der Einfluß der Relativitätskorrektur eine Abhängigkeit des ZEEMAN-Effektes von der Kernladung verursachen würde, welche in diesen Spektren nicht wahrgenommen ist.

b) Beim LANDÉschen Modell darf man das Impulsmoment des Atomrestes nicht mit demjenigen des positiven Ions identifizieren, sowie man es nach der Definition des Atomrestes erwarten würde. (Verzweigungssatz von LANDÉ-HEISENBERG⁴) — unmechanischer Zwang).

c) Bei einigen in der letzten Zeit mit Hilfe des LANDÉschen Schemas analysierten Spektren (z. B. Vanadium, Titan) stimmt das K des Grundtermes gar nicht mit dem Werte, welchen man aus dem BOHR-STONER'schen periodischen Systems erwarten würde.

§ 3. Die obengenannten Schwierigkeiten zeigen alle in dieselbe Richtung, nämlich, daß die Bedeutung, welche das LANDÉsche Vektormodell zukunfts, wahr-

§ 4. In beiden Auffassungen bleibt jedoch das Auftreten des sog. relativistischen Doublets in den Röntgen- und Alkalispektren ein Rätsel. Zur Erklärung dieser Tatsache kam man in letzter Zeit zur Annahme einer klassisch nicht beschreibbare Zweideutigkeit in den quantentheoretischen Eigenschaften des Elektrons¹).

§ 5. Uns scheint noch ein anderer Weg offen. PAULI bindet sich nicht an eine Modellvorstellung. Die jedem Elektron zugeordneten 4 Quantenzahlen haben ihre ursprüngliche LANDÉ'sche Bedeutung verloren. Es liegt vor der Hand, nun jedem Elektron mit seinen 4 Quantenzahlen auch 4 Freiheitsgrade zu geben. Man kann dann den Quantenzahlen z. B. folgende Bedeutung geben: n und k bleiben wie früher die Haupt- und azimuthale Quantenzahl des Elektrons in seiner Bahn.

R aber wird man eine eigene Rotation des Elektrons zuordnen⁵).

Die übrigen Quantenzahlen behalten ihre alte Bedeutung. Durch unsere Vorstellung sind formell die Auffassungen von LANDÉ und PAULI mit all ihren Vorteilen miteinander verschmolzen²). Das Elektron muß jetzt die noch unverstandene Eigenschaft (in § 1 unter a genannt), welche LANDÉ dem Atomrest zuschrieb, übernehmen. Die nähere quantitative Durchführung dieser Vorstellung wird wohl stark von der Wahl des Elektronenmodells abhängen. Um mit den Tatsachen in Übereinstimmung zu kommen, muß man also diesem Modell die folgenden Forderungen stellen:

a) Das Verhältnis des magnetischen Momentes des Elektrons zum mechanischen muß für die Eigenrotation doppelt so groß sein als für die Umlaufbewegung⁴).

b) Die verschiedenen Orientierungen vom R zur Bahnebene (oder K) des Elektrons muß, vielleicht in Zusammenhang mit einer HEISENBERG-WENTZEL'schen Mittelungsvorschrift⁵), die Erklärung des Relativitätsdoublets liefern können.

G. E. UHLENBECK und S. GOUDSMIT.
Leiden, den 17. Oktober 1925.
Institut voor Theoretische Natuurkunde.

¹) W. HEISENBERG, *Zeitschr. f. Phys.* **32**, 841, 1925.

²) Man beachte, daß man die hier auftretenden Quantenzahlen des Elektrons den Alkalispektren entnehmen muß. R hat also für jedes Elektron nur den Wert 1 (in LANDÉ'scher Normierung).

³) Z. B. wird nun auch die Bedeutung des HEISENBERG'schen Symbols σ (Spin) klarer, worin $\sigma = \pm \frac{1}{2}$.

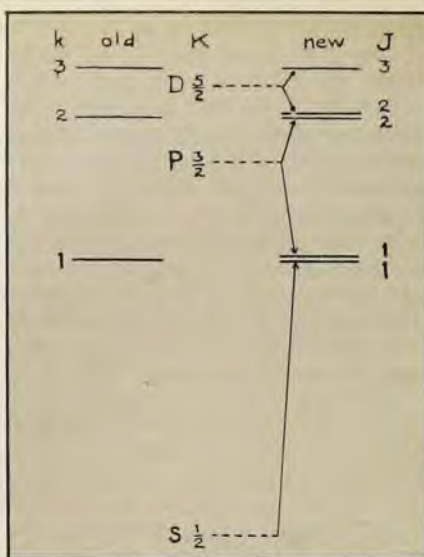
words "revolution" or "breakthrough." It was really a consolidation of many lines of thought, which admittedly occurred in the rather short period say from 1923 till 1928, but which required about twenty years of preparation. It will be a great but very difficult task to write a proper history about this period. Sam¹ is very skeptical about it and perhaps one must wait till more materials (such as the letters of Wolfgang Pauli) become available.

I will not go into the priority question. Sam has told all about this, especially in his *Delta* article,³ and I agree with his conclusions. However, a short contribution by E. H. Kennard should be mentioned.⁴ We were clearly not the first to propose a quantized rotation of the electron, and there is also no doubt that Ralph Kronig anticipated what certainly was the main part of our ideas in the spring of 1925, and that he was discouraged mainly by Pauli from publishing his results. In the memorial volume to Pauli, Kronig has written an article about the crucial period 1923–25, in which he also describes his personal experiences.⁶ In the same volume there is a very useful survey by Bartel van der Waerden,⁷ in which especially Pauli's contributions are discussed. Both articles are at most only mildly critical about Pauli's attitude about the spin hypothesis, and van der Waerden says explicitly that in his opinion Pauli and Werner Heisenberg can not be blamed for not having encouraged Kronig to publish his hypothesis. I do know, however, from a long conversation with Pauli in the 1950's during a summer school in Les Houches, that he blamed himself about the whole episode—"Ich war so dumm wenn ich jung war!" ("I was so stupid when I was young!") All I think one can say is that our proposal came just at the right time, that we had perhaps a better appreciation of its consequences—especially with respect to the fine structure of hydrogen—and, finally, that we had the luck and the privilege to be students of Paul Ehrenfest. His role in the story will become clear in the following.

Switching to paradise

Let me begin my story with some autobiographical notes. In September 1918 I started at the Technical University in Delft as a student in chemical engineering. I wanted to study physics and mathematics, but I did not have the classical education that the law required for admission to study at the University in Leiden. The work at Delft was very busy and disciplined. Every afternoon I worked in the chemical laboratory, which I especially disliked, probably because I was not very good at it.

In January 1919, the law was changed, thank God; the new so-called "Limburg law" (I will never forget the name!) allowed barbarians like me to study the sciences and medicine at the universities. I persuaded my parents to let me switch



This diagram from the 1926 Letter to *Nature* illustrates how the spin hypothesis changes the explanation of the fine structure of hydrogen-like spectra. The principal quantum number is three; the dotted lines are the levels without spin. The new levels are at the same places as in the Sommerfeld theory, but the earlier disagreements with the correspondence principle have been resolved by using the concept of spin.

to Leiden, which was easy because no additional tuition was required—I only had to change my commuter ticket from Delft to Leiden. I lived at home with my parents in The Hague.

I found Leiden a kind of paradise. We had to take only five lectures a week and one afternoon of a rather standard physics laboratory. There was a wonderful physics and mathematics library, the so-called "Bosscha Reading Room" of which Ehrenfest was the director. In physics there were three professors. In addition to Ehrenfest there was H. Kamerlingh Onnes, the famous director of the low-temperature laboratory, and Johannes Kuenen, a very fine man, who gave the first-year courses. There were few students (in my year only four) and we all knew each other. And, to top it all, there were long vacations!

Since my high-school years I had been especially interested in the kinetic theory of gases, because to me it appeared to be a theory that really *explained* the observed phenomena. In all the free time I had, I therefore studied Boltzmann's *Vorlesungen über Gastheorie*. It was hard going; I had to learn analytical mechanics and several branches of mathematics just to be able to follow the argument. But I really did not understand what it was all about. I also dipped into Gibbs's *Statistical Mechanics*, with the same experience. It was therefore a revelation for me when I got hold of the famous *Encyclopedia* article of Paul and Tatiana Ehrenfest. Suddenly it became clear what the basic problems were and what had been achieved by the founders of statistical mechanics. There were a

whole series of open problems and questions, which showed the so-called "frontier" of the subject. Of course it did not occur to me to try to answer some of these questions—I did not have the chutzpah! I was a conscientious student and I thought that I had to study all the books before trying to do anything new.

In these years I hardly knew Goudsmit, who was two years younger and was therefore just coming over the horizon. I also had little contact with Ehrenfest. He knew that I existed, and once in a while he looked over my shoulder to see what I was studying. But I was too shy to ask him questions, which was almost a prerequisite for talking to him! All this changed completely after I had passed my so-called "candidate's examination" (roughly equivalent to the BS degree), which as a conscientious student I did in the required time (December 1920).

That year I also began to follow Ehrenfest's lectures, and was also allowed to come to the famous Wednesday colloquium. I have described Ehrenfest's methods of teaching elsewhere,⁸ so not to go too far afield let me just say here that I remember those wonderful years especially because of the friendliness and feeling of community of the whole group. There was no competition. And this *all* came from Ehrenfest. He taught us that physics was not only fascinating but also fun, something we should share with each other. He had not a grain of pomposity, a trait that was (and still is!) rare among professors. We now know that, already in those years, he struggled with his feeling of inadequacy and with periods of depression, but he never showed it to us. I still remember his jokes and his laughter!

The years in Rome

For me the only trouble was that, to earn money, I accepted a job in my fourth year. I taught mathematics, ten hours a week, at the high school in Leiden. I did not mind the teaching, but I had trouble keeping order in my classes, and I begrudged the time it took. I did not get much sympathy from my father, who pointed out that, as I knew, even with a doctor's degree all I could expect was a job as a high-school or gymnasium teacher in some Dutch town. As he said, "*Tu l'as voulu, George Dandin!*" ("You wanted it, George Dandin.") When Ehrenfest asked in class, some time in the spring of 1922, whether anybody was interested in a tutoring job in Rome for a couple of years I immediately raised my hand. Thus began my Roman period, which almost changed the course of my life.

Since all this is meant to be an introduction to the wonderful summer of 1925 when Sam and I worked together, I will try to keep it short. My job in Rome, about ten hours a week, was to teach the youngest son of the Dutch ambassador, J. H. van Royen, all the subjects required in

a Dutch gymnasium except the classical languages and history, for which there was a second tutor and which took the rest of the boy's time. Every summer the boy and I went back to Holland, where he was tested to see whether he was ready for the next grade in school. And so it went for three years.

I had never been outside Holland since my sixth year, so it was a real adventure for me. I got a princely salary, and except for studying the textbooks to keep ahead of my student, I did not have much to do. The first year I started to take Italian lessons, which I kept up in the following years. This was the most intelligent thing I did in those days, and I am still proud of it. The first year I also studied hard for my doctoral examination, which (again as a conscientious student) I passed in the required time (September 1923). After that time I became more and more interested and involved in the cultural history of Italy. I travelled a lot (I could afford it!) and I always took part in the activities of the Dutch Historical Institute in Rome. My first paper was a biographical sketch of the Dutch philosopher Johannes Heekius, who was one of the founders of the Academia dei Lincei in Rome.⁹

I still tried to study the old Bohr-Sommerfeld quantum theory, using the dissertation of Jan Burgers, and I kept in touch with Ehrenfest during the summer. In 1923 I also met and became good friends with Enrico Fermi, who was already at that time an accomplished physicist. Still, even his influence did not turn me back to physics. I suppose I went through what nowadays is called an identity crisis. Anyway, when I came back in June 1925, I thought that my real interest was in the study of cultural history, and that perhaps I should forget about physics. I had a long talk with my uncle, C. C. Uhlenbeck, the professor of linguistics at Leiden, who was the wise man in our family and who knew me very well. He was sympathetic and he shared my enthusiasm for the historian Johan Huizinga. But he reminded me that if I was serious I had better start to learn Latin and Greek, and he gave me the good advice first to try to finish my studies, especially since I had never done any work in physics by myself. Of course I also talked with Ehrenfest, who somehow still had enough confidence in me to propose that we work together on a study of the various solutions of the wave equation in n dimensions; this later appeared in a joint paper in the *Proceedings* of the Dutch Academy.¹⁰ But he also told me that I had better start learning what the real problems in physics were, and that he would ask Sam Goudsmit to teach me what he knew and had done about the theory of atomic spectra.

The riddle of the gyromagnetic ratio

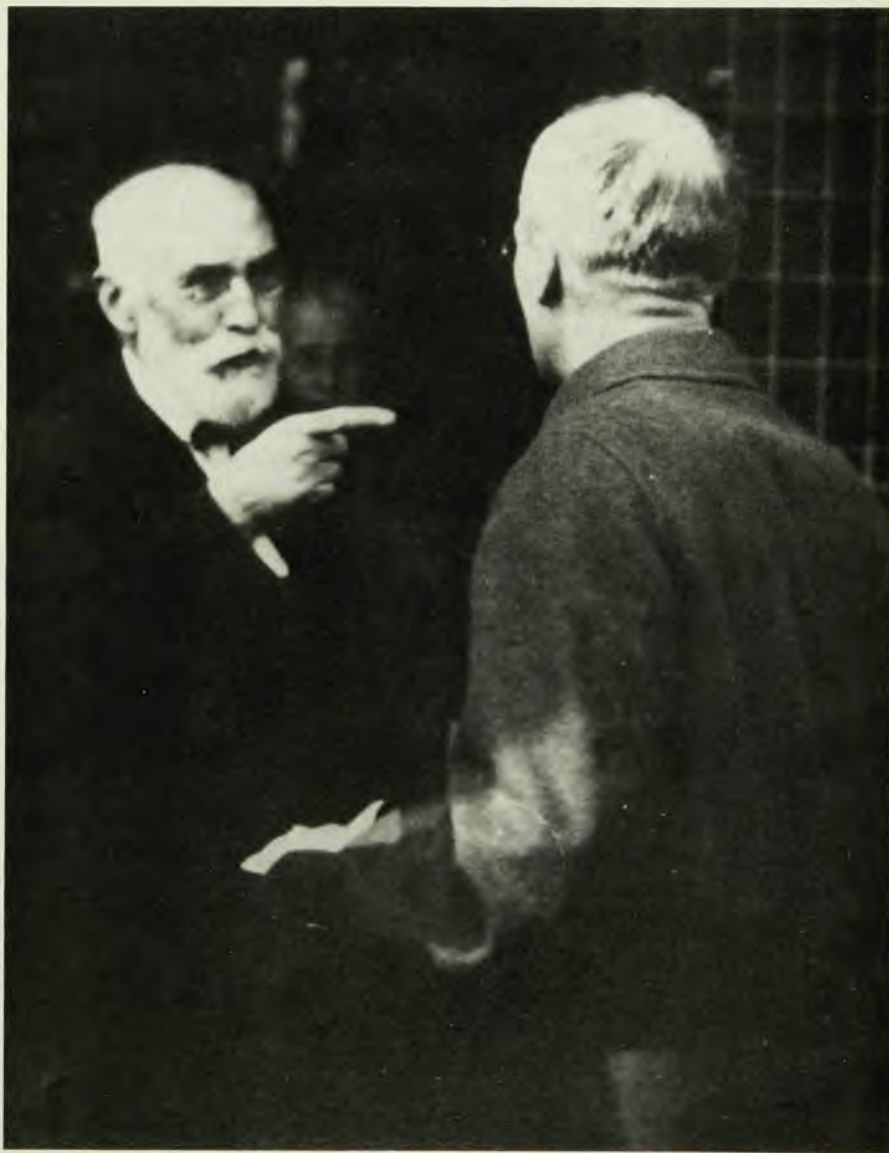
Thus began the remarkable summer of 1925. Two days a week I went to Leiden

to work with Ehrenfest on the wave equation and on the other days Sam and I got together in The Hague to talk about the recent developments of atomic theory, which as I then slowly began to realize was at that time (1923-25) the "frontier" of physics.

At this point I think I should tell more about Sam Goudsmit, especially since in his accounts^{1,2,3} he speaks rather deprecatingly about himself. It is true that Sam was not a very conscientious student and that he often had trouble passing the required examinations in the subjects that did not interest him. But on the other hand he was a very independent worker. Already in his first year (1921) he proposed a formula for the doublet splitting in atomic spectra and in the following years he wrote a number of papers on complex spectra and the vector model. This is not the place to try to describe this work,¹ so let me say only that in 1925 Sam was already a well known theoretical

spectroscopist. He was the "house theoretician" in the Zeeman laboratory in Amsterdam, where he spent the first three days of the week—returning to Leiden in time for the Wednesday colloquium. Moreover, being from the Ehrenfest school, he was a good teacher!

So that summer Sam explained to me, in a nice orderly fashion, the work of Alfred Landé, Werner Heisenberg, Pauli and others (himself included) on the vector model of the atom, of which I was completely ignorant. Again I will not go into details, so let me only remind the reader that in this model (also sometimes called "*das Rumpf-Modell*") it was assumed, say for alkali atoms, that somehow the core (*der Rumpf*) had an angular momentum $\hbar/2$ and a magnetic moment of one Bohr magneton, so that the gyromagnetic ratio was twice the classical value, $e/2mc$, for the orbital motion of electrons. This was a riddle, but with this assumption one could understand very



H. A. Lorentz makes a point. The grand old man of Dutch physics was skeptical of spin; according to his calculations the velocity of the electron's surface was ten times the speed of light.

satisfactorily the coupling of the core with the outside electron, the influence of magnetic field (the Landé formula), and so on.

I remember that I was interested, but still detached. I asked many questions, and I made notes after each session. I remember that I was especially bothered by Goudsmit's statement that the model described all atoms except hydrogen, for which the old Sommerfeld theory was valid—as though that were somehow a horse of a different color! My skepticism infected Sam, and he then got the idea of looking into the way the level scheme of the fine structure of hydrogen would have to be if it were like an alkali atom. In our next session he already had it all worked out. It is the now accepted level scheme (except for the Lamb shift), which of course also follows from the Dirac theory of the electron. We realized that although the level splittings were the same as in the Sommerfeld theory, the selection rules were different; the theory thus explained a mysterious strong line in the spectrum of ionized helium that had been observed by Friedrich Paschen. This line was forbidden in the Sommerfeld theory and could also not be explained by the influence of electric fields, as I found out from H. A. Kramers's thesis.

It was our first success. We wrote a paper¹¹ in Dutch, which appeared in *Physica*; although it did not attract any attention until much later, I was quite proud of my first contribution to physics. However, I still had not yet completely made up my mind to continue. There was an opportunity for students at Leiden who wanted to switch to the humanities to take courses in the classical languages. So in the beginning of September I started to take Latin. Unfortunately this course was not like the Berlitz school in

Rome, where I had started to learn Italian! It was very tough and, after a month or so, it became too much for me. During this time also my sessions with Sam became more and more absorbing. Ehrenfest was away, so we talked almost every day, trying to understand the ideas of Pauli.

Euphoria

Sam had earlier explained to me Pauli's criticism against the *Rumpf-Modell*, and he had told me about Pauli's proposal to ascribe four quantum numbers to each electron. He now continued with the discussion of the famous paper of January 1925 in which Pauli formulated the exclusion principle: No two electrons could have the same four quantum numbers. He explained to me how, by combining the four quantum numbers of the different electrons according to the rules of the vector model, one could understand the periodic system and the general multiplet structure of the atomic levels. Sam himself had simplified the argument by introducing the quantum numbers, n , l , m_l , and m_s (appropriate when a strong magnetic field is present) instead of those used by Pauli, and he noticed that then m_s was always $\pm\frac{1}{2}$.

I was impressed, but since the whole argument was purely formal, it seemed like abracadabra to me. There was no picture that at least qualitatively connected Pauli's formalism with the old Bohr atomic model. It was then that it occurred to me that, since (as I had learned) each quantum number corresponds to a degree of freedom of the electron, the fourth quantum number must mean that the electron had an additional degree of freedom—in other words, the electron must be rotating! Sam has written that he did not know at

that time what a degree of freedom was. This may be so, as Sam had not done his exam in mechanics yet; in fact, he never passed this exam, and as a result he did not have the right to teach mechanics in the Dutch high schools even after he got his PhD. However, this did not prevent him later from teaching the graduate course in mechanics at the University of Michigan, which he did regularly because he liked the subject so much; furthermore, it was much appreciated by the students.

In spite of this he appreciated right away that if the angular momentum of the electron was $\hbar/2$, one had a picture of the alkali doublets as the two ways the electron could rotate with respect to its orbital motion. In fact, if one assumed that the gyromagnetic ratio for the rotation was twice the classical value, so that the magnetic moment was

$$2 \frac{e}{2mc} \frac{\hbar}{2} = \text{one Bohr magneton}$$

then the properties formerly attributed to the core were now properties of the electron. The simple "anschaulichen" features of the original *Rumpf-Modell* were thus reconciled with Pauli's ideas.

I remember that when this became clear to us, we had a feeling of euphoria, but we also both agreed that one could not possibly publish such stuff. Since it had not been mentioned by any of the authorities (we did not know about Kronig, of course) it must for some reason be nonsense. But, of course, we told Ehrenfest, who was immediately interested. I am not sure precisely what happened next. Sam is wrong when he writes that he was satisfied and did not think any more about how our model could be justified. I remember that he wrote me a postcard from Amsterdam very soon afterward, in which he asked whether I

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ALBANUSWEG 10, GEDENKINGEN 2.

Den. 22/12.....1925.

Lieber Ehrenfest,

Das Aufenthalt bei Ihr in Leiden war eine wunderbare Erlebnis. Ich soll dich immer dankbar sein, dass du den Anlass war, dass ich das schöne Jubiläum von Lorentz bewohnte. Auch die Gespräche mit Einstein war ein grösster Genuss und Belehrung als ich sagen kann. Nicht weniger Freude war es für mich, den Einsatz von Ullenbeck und Goudsmit kennen zu lernen. Ich bin überzeugt, dass es ein überaus grosser Fortschritt in der Theorie des Atombau bedeutet. Auf meiner weiteren Reise fühlte ich mich ganz wie ein Profet des Elektronmagnet-Evangeliums, und ich glaube, dass es mir gelungen ist, Hei-

senberg und Pauli wenigstens davon zu überzeugen, dass ihre bisherigen Einwände nicht entscheidend sind, und dass es äusserst wahrscheinlich ist, dass die quantenmechanische

Durchrechnung alle Einzelheiten richtig wiedergeben wird. Ich freue mich sehr darauf, den Artikel von Goudsmit und Ullenbeck zu sehen. In Utrecht verbrachte ich eine sehr schöne Abend mit Ornstein.

Mit den besten Wünschen für einen frohen Weihnachten und glücklichen Neujahr für I alle von Margrethe und

Deinem

Niels Bohr

Bohr's letter to Ehrenfest. Although he misspelled both their names, Bohr was enthusiastic about the "electron-magnet gospel" of Uhlenbeck and Goudsmit. He thought it extremely likely that the quantum-mechanical calculation would reproduce all details correctly. (From the AIP Niels Bohr Library.)

was sure that the gyromagnetic ratio had to be $e/2mc$ classically—perhaps it was different for the rotation of an extended charged body. I showed this postcard to Ehrenfest, who then recalled a paper by Max Abraham¹² about the magnetic properties of rotating electrons. I studied this paper very hard and found there to my great satisfaction that if the electron has only surface charge the gyromagnetic ratio was $2e/2mc$, just as we had postulated! I think that when I showed this to Ehrenfest he thought (as he told us later) that our idea was either very important or nonsense, but that it should be published. The Abraham calculations were nonrelativistic and based on the old-fashioned rigid electron, so that they were at best only suggestive. Anyway, Ehrenfest told us to write a short, modest Letter to *Naturwissenschaften* and to give it to him. “Und dann werden wir Herrn Lorentz fragen.” (“And then we will ask Mr Lorentz.”) A letter of 16 October to H. A. Lorentz in which he mentions this (among other things) was found and shown to me by Martin Klein.

Lorentz, who was of course the great old man of Dutch physics, was retired and lived in Haarlem but gave a lecture in Leiden every Monday at 11:00 am, in which he discussed the recent developments in physics. Everybody who could possibly make it came. So when school started in the middle of October (remember, we had long vacations) I had the opportunity to tell Lorentz about our ideas. Sam was not present because he had to resume his duties at the Zeeman laboratory. Lorentz was very kind and interested, although I also got the impression that he was rather skeptical. He said that he would think about it and that we should talk again the next Monday.

In fact, when we met that day he showed me a stack of papers full of calculations written in his beautiful handwriting, which he tried to explain to me. They were above my head but I understood enough to realize that there were serious difficulties. If the radius of the electron was

$$r_0 = e^2/mc^2$$

and if it rotated with an angular momentum $\hbar/2$, then the surface velocity would be about ten times the light velocity! If the electron had a magnetic moment $eh/2mc$, its magnetic energy would have to be so big that, to keep the mass m , its radius would have to be at least ten times r_0 .

It seemed to me that if one extended the Abraham calculations properly as Lorentz had apparently done, (and published in revised form¹³) then our picture of a quantized rotation of the electron could not possibly be reconciled with classical electrodynamics. I told this to Ehrenfest, of course, and said that his second alternative had turned out to be the right one. The whole thing was non-

sense, and it would be better that our Letter not be published. Then, to my surprise, Ehrenfest answered that he had already sent the Letter off quite a while ago, and that it would appear pretty soon. He added: “*Sie sind beide jung genug um sich eine Dummheit leisten zu können!*” (“You are both young enough to be able to afford a stupidity!”)

This is not yet the end of the story. Our letter appeared in the middle of November, and soon afterwards (21 November) Goudsmit received a letter from Heisenberg, whom he knew quite well. In this letter (reproduced in reference 2) Heisenberg expressed his appreciation for Sam’s courageous idea and agreed that it would remove all of the difficulties of the Pauli theory. He especially noted that it leads to the Landé-Sommerfeld formula for the alkali doublets except for a factor of two, and he asked how we had got rid of this factor. We had not derived this formula and therefore had no idea about the factor of two. In fact, I must say in retrospect that Sam and I in our euphoria had really not appreciated a basic difficulty—one with which Pauli and Bohr had been struggling for some time:

Clearly if one formally assigns the Landé quantum numbers of the atomic core to the electron as Pauli had done, then since there is *no model*, it is quite obscure how the “core” quantum number is coupled to the orbital quantum number of the electron. Bohr had speculated about a new force—the “*unmechanische Zwang*” (non-mechanical strain)—and Pauli spoke about an intrinsic two-valuedness of the motion of the electron. In our Letter we had maintained that such ideas could be replaced by a hypothesis about the structure of the electron. This explains the rather esoteric title of our Letter: “Replacement of the Hypothesis of the Non-mechanical Strain by an Assumption about the Internal Behavior of Every Single Electron.”

Nevertheless, we had not actually explained how the basic difficulty would then be removed by the coupling of the rotational and orbital motion of the electron. Now we heard from Heisenberg that there was such a spin-orbit coupling and that it gave the right answer except for the mysterious factor of two. We still did not know how to derive the formula, but of course knowing the answer helps! Einstein, who visited Leiden every year for a month or so, gave us the essential hint. In the coordinate system in which the electron is at rest, the electric field $\mathbf{E}$ of the moving atomic core produces a magnetic field $[\mathbf{E} \times \mathbf{v}]/c$ (where $\mathbf{v}$ is the velocity of the electron) according to the transformation formula of relativity theory. This sounds learned (and in those days I liked that!), but it is of course just the magnetic field produced by the moving charged core. It is with respect to this magnetic field that the spin of the electron has its two orientations and the en-

ergy difference—the doublet splitting—could then be calculated by first-order perturbation theory. In this way we reproduced Heisenberg’s formula with the same erroneous factor of two. By the way, there is no doubt that Kronig also had done this calculation (see “The Turning Point,”⁶ page 20) and had shown it to Landé and Pauli. I find the reaction of Pauli mentioned there quite surprising, and it is certainly opposite to the sympathetic reaction of Ehrenfest to our ideas. Of course one must remember that Pauli was about of our age and was in the middle of the developments, while Ehrenfest was twenty years older and not deeply involved in what was sometimes called “spectral-term zoology.”

This brings us to the beginning of December 1925 when Bohr came to Leiden to help celebrate the fiftieth anniversary of the doctorate of Lorentz, which was a great occasion. Bohr’s visit was very lucky for us, since it gave us the opportunity to talk at length with him about our idea and the subsequent difficulties. Bohr had seen our Letter, but he still worried about how the coupling between the spin and the orbit could be understood. When we explained Einstein’s argument he was completely convinced and became quite enthusiastic. He did not pay any attention to the calculations of Lorentz, which I mentioned to him. “They raise just classical difficulties,” he said, “and they will disappear when the real quantum theory is found.” The factor of two he took more seriously, but he somehow expected that a better calculation would also make it disappear.

He advised us to go back to the spectrum of hydrogen, especially when we told him about our earlier paper in *Physica*, with which he was not acquainted. Did the combination of the Sommerfeld relativistic effect with the spin-orbit coupling (forgetting about the factor of two) lead to the fine structure of the hydrogen levels as we had surmised in our *Physica* article? Sam could show this right away, and I think that together with the general Landé-Pauli unification, it completely convinced Bohr. On his way back to Copenhagen he made propaganda for our idea, as shown in the following part of a letter to Ehrenfest of 22 December:

“... I am convinced that it implies a large step forward for the theory of atomic structure. On my further travels I felt completely like a prophet for the electron-magnet gospel, and I believe that I have succeeded in convincing Heisenberg and Pauli that at least their present objections are not decisive and that it is very probable that a quantum-mechanical calculation will give all details correctly. I am looking forward to seeing the article of Goudsmit and Uhlenbeck...”

This article was our second Letter to the Editor, this time that of *Nature*.¹⁴ It was entitled: “Spinning Electrons and the

Structure of Spectra." It was dated December 1925 and appeared 20 February 1926. Bohr added an approving postscript. Since then our idea has been more or less accepted. The only holdout was Pauli, who had *not* been convinced by Bohr and who still spoke of it as the new "*Irrlehre*" ("erroneous teachings") (see van der Waerden's article, reference 7, page 215).

And there was of course still the mysterious factor of two! It is now well known that this difficulty was soon afterwards resolved¹⁵ by L. H. Thomas, who showed that it was a forgotten relativistic effect. I remember that, when I first heard about it, it seemed unbelievable that a relativistic effect could give a factor of two instead of something of order v/c . I will not try to explain it, so let me only say that even the cognoscenti of the relativity theory (Einstein included!) were quite surprised. When Pauli understood it he finally withdrew his objections, as he mentioned later in his Nobel Prize lecture.¹⁶

This is the end of the story so far as Sam and I are concerned. I had become the assistant of Ehrenfest and in 1926 we worked together trying to digest the new quantum mechanics, and especially to understand the consequences for statistical mechanics. Sam continued his spectroscopic work, partially in Tübingen, where together with Ernst Back he worked out the theory of the hyperfine structure of the spectral lines when the atomic nucleus has a spin and magnetic moment.¹⁷ In the spring of 1927 we both spent a few months in Copenhagen, where we wrote our dissertations. We received our doctor's degrees on the same day (7 July 1927) and then in the fall we went on the same boat to the US and to Ann Arbor, Michigan, where we had been appointed as instructors of physics.

With regard to the spin of the electron, it was of course Pauli who succeeded in incorporating the notion into Schrödinger wave mechanics.¹⁸ It must have been a great satisfaction for him that it required a two-valued, or spinor, wave function. In a way it justified his old speculation on the two-valuedness of the electron motion. In his paper Pauli still had to assume the anomalous factor of two for the gyromagnetic ratio and he also had to take over the Thomas factor of two. The really complete explanation of these two factors of two, which had plagued the theory, did not come until 1928, when Paul Dirac developed the complete relativistic wave equation of the electron.¹⁹

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I am indebted to Martin Klein for a copy of the letter from Bohr to Ehrenfest; the translation from the German is mine.

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Teachers as part of a symposium celebrating the 50th anniversary of the discovery of electron spin.

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Bosscha Reading Room, the physics and mathematics library at the University of Leiden. Students without a classical education, such as George Uhlenbeck, were barred from Leiden before 1919.