# It might as well be spin

Compared to the competitive struggles of today's highly specialized physicists for recognition, the atmosphere in the "springtime of modern atomic physics" was like that of a "Peyton Place without sex."

Samuel A. Goudsmit

It was a little over fifty years ago that George Uhlenbeck and I introduced the concept of spin. The United States, celebrating its bicentennial, is only four times as old as spin—not even an order of magnitude older. It is therefore not surprising that most young physicists do not know that spin had to be introduced. They think that it was revealed in Genesis or perhaps postulated by Sir Isaac Newton, which young physicists consider to be about simultaneous. There are many other fifty-year mileposts in physics, which also have been forgotten, such as the radio-pulse experiments of Merle Tuve and Gregory Breit that later led to radar.

### Restless as a willow in a windstorm

When we reach the stage in life in which our future lies behind us, young people always ask us to look back. Most of us do not realize that we have reached that turning point until we are far beyond it; then looking back becomes a burden and often painful. We have many regrets—but never for what we did, always for what we failed to do. We realize that we failed to make use of many important opportunities, and so our looking back lacks objectivity. You must keep this in mind as you read this article, in which I propose to describe the contrast between today and the springtime of modern atomic physics, which spanned approximately the years from 1919 to the early 1930's and took place primarily in Europe.

Was it really springtime? In some respects, yes. Many little shrubs were planted that in fifty years grew into powerful trees full of fruit-bearing branches. Let me hasten to point out that at the time of planting, it was impossible to know which tree would flourish, although in hindsight it appears that some planters and observers made the right guesses, just as at the races and in the stock market.

Was it a romantic time? Were physicists better off and happier than they are today? Wasn't it exciting in those revolutionary years to have personally known Albert Einstein, H. A. Lorentz, Niels Bohr, Paul Ehrenfest, Arnold Sommerfeld, Pieter Zeeman and many others? The answer to all of these questions is, of course, that at that time the young people were not aware of or did not appreciate the circumstances in which they lived. In hindsight it must have been an unusually interesting period, even for minor participants. It is true that I was "restless as a willow in a windstorm" and often "starry-eyed and vaguely discontented" and perhaps suffered from spring fever. From an objective viewpoint however, it was merely different from today's physics; the concept of the "good old days" does not apply.

To describe the difference to those who did not personally experience that period, I must use analogies. The best analogy I have found so far is to say that the present community of physics represents life in a modern metropolis, exciting and full of frustration and dangers. In the 1920's, by comparison, we lived in a small village with its little feuds, a Peyton Place without sex. I am sure that the present generation would not have liked it, most of all because physics and physicists were unimportant to the outside world. The press did not care, the government did not care, the military did not care; isn't that awful? What is even worse, no one got his expenses paid for giving a paper at a meeting.

Marie Curie and Einstein were exceptions in that they had news value. Einstein knew how to capitalize on his fame. Once, when Ehrenfest asked him why he had gone to Spain where there was no physics of interest to him, Einstein answered, "True, but the King gives such nice dinner parties." In general, publicity was frowned upon and many of Einstein's friends tried to persuade him to shun the press. The photograph of 1923 on the opposite page was not taken because Einstein was visiting in Leiden, but because Douglas Hartree happened to pass through. It shows also how small the number of physicists was: It represents the complete class of Ehrenfest. Only half of these students were physicists, the rest were astronomers and chemists.

### Starry-eyed and vaguely discontented

To become a physics student in Europe was an anomaly in the 1920's. Physics was not a profession but a calling, like creative poetry, music composition or painting. I was considered a failure by my family. They expected me to become a businessman, as anybody who worked for a paycheck was considered a weakling. Almost all students I met came from academic families—their education came from the home, their training from the schools. As a physics student, I was considered a sort of misfit. This is quite different from what I found when I came to the US, and especially from the situation today. Physics is now a profession, like engineering or television repair, and physicists come from all walks of life. In Europe in the 1920's it was rather difficult to become a physicist. But once accepted as a serious research student, one had fairly easy access to the big shots in the field, easier than at present.

I never understood why relativity, such an abstract and difficult subject, caught the interest of the general public. World War I was followed by years of very severe economic problems, uncertainties and political upheavals in many areas. We

Samuel A. Goudsmit is visiting professor at the Department of Physics of the University of Nevada, Reno.

are living again in such a period of insecurity and again we see an increased public interest in the abstract, the occult, in extrasensory experiences and the Loch Ness monster. It scares me especially because this time the lunatic fringe includes some physicists. One of them was so taken in by the spellbinding, spoonbending Uri Geller that he wrote a book about it. The world has lost confidence in scientific and rational reasoning; physics is now hard to sell.

It was in that old protected atmosphere that George Uhlenbeck and I came up with the concept of electron spin. The number of active physicists was small, and since I had already published several papers on spectra and atomic energy levels, I was personally acquainted with several of them. But I did not think spin important enough to send any of them preprints or to write to them about it. I did not worry at all about being scooped. This had happened to me a couple of times with work on spectral lines, but there was so very much left to do that it was merely a disappointment, not a catastrophe.

Personally the spin solution gave me pleasure but not excitement. I did not appreciate its possible significance until Bohr showed such a great interest in it. There were other items in my physics work that gave me more of a thrill—for example the first experimental determination, together with Ernst Back in Tü-

**Spin, Leiden University and all that.** The class of Paul Ehrenfest (near the center) stands in front of the door of the Institute for Theoretical Physics at the University of Leiden, probably in 1923. Albert Einstein stands in the doorway, but the reason for the picture was a visit by Douglas Hartree (between Einstein and Ehrenfest). The author, Sam (then Sem) Goudsmit, is on the right. Jan Tinbergen (left, no hat) switched fields after obtaining his PhD in physics, and received a Nobel Prize in economics. The tall man next to Ehrenfest is Gerhard Dieke, who later became physics chairman at Johns Hopkins. The woman beside Einstein is Ini Roelofs; Jaap Voogt and Bernard Polak are fourth and fifth from left.

bingen, of a nuclear spin, that of bismuth. There was of course no such thing as a press conference when we discovered electron spin. For me there were no job offers either, not even as a high-school teacher.

Today, a new bump in a curve is enough to call Walter Sullivan of *The New York Times* out of bed to make sure that the work will get a headline in the paper. Today competition is fierce and often ruthless, because so much is at stake. Funding, promotion and a whole career may depend on publicity and on the *Citation Index*. In the 1920's, competition and animosities could be strong too, and sometimes affected careers, but funding was a minor consideration. There were very few academic openings and political considerations often determined who was chosen to fill them.

The published correspondence between Einstein and Max Born shows how difficult it was for Jews to get jobs in Germany long before Hitler came to power. The same was true over here, at many, but not all, universities and industrial laboratories. When my former student Robert F. Bacher was considered for a position at Cornell University in 1934, R. C. Gibbs asked me in confidence, on behalf of F. K. Richtmyer, whether Bacher was Jewish—if so, he would not have got the job. Some of these animosities had an international character, an aftermath of World War I.

The German physicist Sommerfeld published his great and influential book on atomic structure in 1921. It contained a chapter about radioactivity, which did not mention the Curies. The French were obviously offended. However, in those days the work of the Curies was considered not physics but chemistry and I very much doubt that Sommerfeld had deliberately omitted their name. But when the Dutch physicist Dirk Coster sent Sommerfeld a manuscript on x-ray energies for an opinion, he kept it so long that one of his own pupils, Walter Kossel, was able to scoop Coster. Similarly, on a visit to Holland, Sommerfeld learned that I was working on the spectrum of iron. He made sure that his pupil Otto Laporte got his results published first, making my efforts obsolete. A professor protected his pupils more than himself. These are just examples of common quibbles, minor compared to today's frantic races for a Nobel Prize.

It is sometimes pathetic to observe the present almost violent striving for publicity. The biochemist Erwin Chargaff describes it pointedly for his field and states: "That in our days such pygmies throw such giant shadows only shows how late in the day it has become.[1] What Chargaff overlooked is that pygmies also throw large shadows at dawn. This could be applied to me and several others in the 1920's, the dawn of the new physics. It is late in the day for physics too, but I am not going to predict the future—I leave that to astrologers and computer addicts. Fortunately there are still and will always be a core of physicists who pursue their science for its great intrinsic value only. They love to teach and are not overanxious to burst into print and publicity with subliminal results and half-digested ideas. We can recognize their work because the adjective "beautiful" applies to it.

There are many colleagues who believe that we received the Nobel Prize for introducing electron spin. In fact, Lee DuBridge recently introduced me as an early Nobel Prize winner; I have also seen it in print. That is all very flattering but does not supplement my TIAA pension. About thirty years after introducing spin, we got the Research Corporation Award and shared $2500; the following recipients received $10 000. Again ten years later, we each received the Max Planck Medal from the German Physical Society. The Nobel Prize was not for us—there were too many physicists who made more important contributions at that time. For example, such spectacular advances as the explanation of radioactivity by George Gamow, Edward Condon and R. W. Gurney and that of chemical binding by Walter Heitler and Fritz London were not considered worthy of the award. Even the theory of relativity was ignored; Einstein was awarded the Nobel Prize for his explanation of the photoelectric effect. The discovery of spin was the main factor for our being offered, in 1926, jobs at the University of Michigan as instructors. That was for me a far more significant award than a Nobel Prize.

PHOTO: A. PAIS, ROCKEFELLER UNIVERSITY

**The originators of the concept of spin,** George Uhlenbeck (center) and Samuel Goudsmit (right), are together, in 1926, with Oskar Klein, a Swedish physicist. Klein, who had spent the previous year at the University of Michigan, was responsible for their being invited to teach there.

#### Busy as a spider spinning daydreams

This brings me to another difference between the 1920's and today: the rapidity of change. As a young student I was totally oblivious of possible changes. As in my theme song, "I was as busy as a spider spinning day dreams." When Sommerfeld's book appeared I literally believed that being cited in one of its footnotes meant immortality. I have forgotten whether I made it in a later edition! The book has been obsolete for decades. Another dream was some day to be the successor of Zeeman at the University of Amsterdam and continue experiments on spectra and the Zeeman effect. Years later when I was offered the position, that area of physics was dead. I did not take the job. Though changes did occur in the 1920's, one could follow them more easily than today, in almost all of physics. Today, extreme specialization is a necessity for a physicist who wants to make a meaningful contribution. The different branches of modern physics now speak different languages; each uses its own jargon, unintelligible to those working in other areas.

The present generation is hardly aware that we are living in a time of rapid change, of revolution. A physicist's work may be forgotten or considered as belonging 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."

* * *

*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, $e\hbar/2mc$. (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 account[4] 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

*Sonderdruck aus Die Naturwissenschaften. 13. Jahrg., Heft 47*
*(Verlag von Julius Springer, Berlin W 9)*

**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 Vektormodelles $R$, $K$, $J$ und $m$[1]). 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 HEISENBERGsche Mittelung[2])].

Dieses Modell hat sich äußerst fruchtbar gezeigt und hat u. a. geführt zur Entwirrung der verwickeltesten 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) PAULI[3]) hat schon gezeigt, daß bei den Alkaliatomen der Atomrest magnetisch unwirksam sein muß, da sonst der Einfluß der Relativitätskorrektion eine Abhängigkeit des ZEEMANeffektes 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-HEISENBERG[4]) — unmechanischer Zwang].

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

§ 3. Die obengenannten Schwierigkeiten zeigen alle in dieselbe Richtung, nämlich, daß die Bedeutung, welch[illegible] ...NDÉschen Vektoren zukennt, wahr- [illegible] P[illegible]) hat schon einen [illegible] an Schwier[illegible]

§ 4. In beiden Auffassungen bleibt jedoch das Auftreten des sog. relativistischen Doubletts 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 Elektrons[1]).

§ 5. Uns scheint noch ein anderer Weg offen. PAULI bindet sich nicht an eine Modellvorstellung. Die jedem Elektron zugeordneten 4 Quantenzahlen haben ihre ursprüngliche LANDÉsche 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 zuordnen[2]).

Die übrigen Quantenzahlen behalten ihre alte Bedeutung. Durch unsere Vorstellung sind formell die Auffassungen von LANDÉ und PAULI mit all ihren Vorteilen miteinander verschmolzen[3]). 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 Umlaufsbewegung[4]).

b) Die verschiedenen Orientierungen vom $R$ zur Bahnebene (oder $K$) des Elektrons muß, vielleicht in Zusammenhang mit einer HEISENBERG-WENTZELschen Mittelungsvorschrift[5]), die Erklärung des Relativitätsdoubletts liefern können.

G. E. UHLENBECK und S. GOUDSMIT.

Leiden, den 17. Oktober 1925.
Instituut 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 LANDÉscher Normierung).

3) Z. B. wird nun auch die Bedeutung des HEISEN-BE[illegible] [illegible]hema III [illegible] worin [illegible]

**The spin hypothesis was proposed in this Letter,** which might never have seen the light of day because of objections based on a rigid-electron model, but it was too late to withdraw it.


George E. Uhlenbeck is professor emeritus of physics at The Rockefeller University, New York.