

UNIVERSIDADE ESTADUAL DE CAMPINAS INSTITUTO DE FÍSICA
GLEB WATAGHIN

F609 A – Tópicos de Ensino de Física

**MOTOR E BALANÇA DE CURIE COM OUTROS
MATERIAIS**

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1) Introdução

A proposta principal é construir dois modelos experimentais que se relacionam entre si. Um deles é o pêndulo de Curie que possui uma base firme de madeira com um ímã fixo em uma das extremidades de modo a atrair um pedaço de níquel seguro por um fio de cobre. O outro é o motor de Curie, um aro de níquel preso a um eixo giratório que também é atraído por um ímã próximo. O níquel apresenta temperatura crítica em que perde suas propriedades magnéticas; desse modo ao ser submetido a essa temperatura o pendulo oscilará, bem como o aro lentamente começará a girar em seu eixo.

O principal objetivo é trazer para os alunos, principalmente do ensino médio, por meio da peculiaridade do comportamento dos materiais no experimento, o interesse ou a curiosidade em saber o que realmente acontece (atomicamente falando) com os materiais ferromagnéticos ao serem submetidos a essas temperaturas conhecidas como temperatura de Curie (T_C). Mostrar que há muito mais além do que nossos olhos podem ver.

2) Teoria

Magnetismo e elétrons:

Os materiais magnéticos, da magnetita às fitas de vídeo, são magnéticos por causa dos elétrons que contêm. Os elétrons podem produzir campos magnéticos de duas formas envolvendo momento dipolar magnético:

- Momento dipolar magnético de spin: Um elétron possui um momento angular intrínseco conhecido como momento angular de spin ou simplesmente spin, representado pelo símbolo S . Associado a este spin, existe um momento dipolar magnético de spin intrínseco, representado pelo símbolo μ_s . (O termo *intrínseco* é usado para indicar que S e μ_s são propriedades básicas de um elétron, como a massa e a carga elétrica.) Os vetores S e μ_s estão relacionados através da equação:

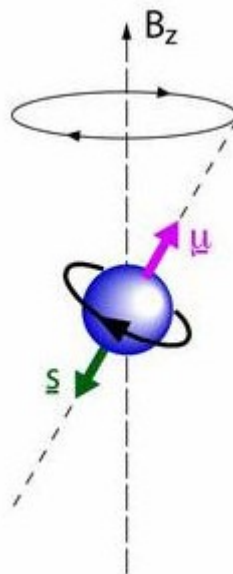
$$\vec{\mu}_s = \frac{-e}{m} S,$$

onde e é a carga elementar ($1,6 \times 10^{-19}$ C) e m é a massa do elétron ($9,11 \times 10^{-31}$ Kg). O sinal negativo indica que μ_s e S têm sentidos opostos.

A existência do spin do elétron foi postulada por dois estudantes de doutorado holandeses, George Uhlenbeck e Samuel Goudsmit, para explicar alguns espectros

atômicos. A teoria quântica do spin foi formulada alguns anos mais tarde pelo físico inglês P. A. M. Dirac, que desenvolveu (em 1929) uma teoria quântica relativística do elétron.

Uma interpretação clássica do spin seria imaginá-lo como um movimento de rotação do elétron em torno do próprio eixo; entretanto, este modelo clássico, assim como o modelo das órbitas que será citado abaixo, não está de acordo com os resultados experimentais. Na física quântica, o spin é considerado uma propriedade intrínseca do elétron; é simplesmente impossível interpretá-lo classicamente.

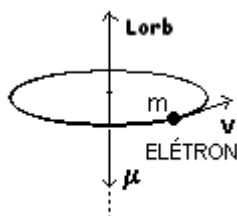


Momento dipolar magnético de spin

- Momento dipolar magnético orbital: Quando faz parte de um átomo, um elétron possui um momento angular adicional que recebe o nome de momento angular orbital, representado pelo símbolo L_{orb} . Associado a L_{orb} existe um momento magnético dipolar orbital μ_{orb} ; a relação entre as duas grandezas é a seguinte:

$$\vec{\mu}_{orb} = \frac{-e}{2m} \vec{L}_{orb}$$

onde o sinal negativo mais uma vez significa que as grandezas tem sentidos opostos.



Momento dipolar magnético orbital

Diamagnetismo:

No diamagnetismo, momentos dipolares magnéticos são produzidos nos átomos do material apenas quando este é submetido a um campo magnético externo; a combinação destes momentos dipolares induzidos resulta em um campo magnético de baixa intensidade em sentido contrário ao do campo externo, que desaparece quando o mesmo é removido.

Paramagnetismo:

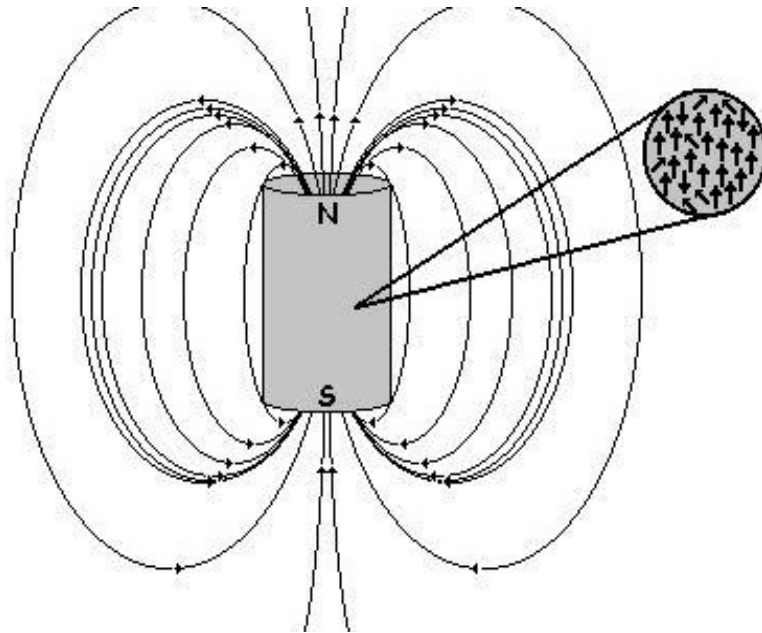
É observado em materiais que contêm elementos da família dos metais de transição, das terras-raras e dos actínídeos. Os átomos destes elementos possuem um momento dipolar magnético diferente de zero, mas, como os momentos dos átomos estão orientados aleatoriamente, o campo magnético resultante é praticamente zero. Entretanto, um campo magnético externo pode alinhar parcialmente os momentos dipolares magnéticos atômicos, fazendo com que o material apresente um campo magnético resultante no mesmo sentido que o campo externo, que desaparece quando o mesmo é removido.

Ferromagnetismo:

Quando falamos de ferromagnetismo em nossas conversas do dia-a-dia, quase sempre imaginamos uma barra imantada ou um ímã em forma de disco (provavelmente preso a uma porta de geladeira). Ou seja, fazemos a idéia de um material ferromagnético tendo um magnetismo forte, permanente, e não um material diamagnético ou paramagnético tendo um magnetismo fraco, temporário.

Ferro, cobalto, níquel, gadolínio, disprosio e ligas contendo estes elementos apresentam ferromagnetismo por causa de um efeito físico quântico chamado de acoplamento de troca, no qual os spins dos elétrons de um átomo interagem com os dos átomos vizinhos. O resultado é o alinhamento dos momentos de dipolo magnético dos átomos, apesar da tendência aleatória de colisões atômicas. Este alinhamento persistente é o

que dá aos materiais ferromagnéticos o seu magnetismo permanente.



Alinhamento dos momentos de dipolo magnético

Se a temperatura de um material ferromagnético for elevada a certo valor crítico, chamada de temperatura de Curie, o acoplamento de troca deixa de ter efeito. A maioria destes materiais torna-se então simplesmente paramagnéticos; isto é, os dipolos ainda tendem a se alinhar muito fracamente com um campo magnético externo, e a agitação térmica agora consegue quebrar mais facilmente o alinhamento.

Veja na tabela abaixo a temperatura de Curie para alguns materiais:

Material	T_C(°C)
Fe	770
Co	1115
Ni	354
Gd	19

CrO ₂	113
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3) Descrição do trabalho

Lista de materiais utilizados:

- Madeira para a base dos dois experimentos
- Velas
- Imãs
- Rolamento de disco rígido + bolacha de madeira (eixo de rotação)
- Fios de cobre e de níquel
- Pedaco de níquel (moeda)
- Haste de ferro para o pêndulo

Pêndulo de Curie:

Utilizando a haste de ferro como suporte para o pêndulo, amarra-se na mesma os finos fios de cobre junto com a moeda de níquel presa na outra extremidade (um furo com broca de furadeira foi utilizado para deixá-la com uma abertura que facilitasse o serviço). Além disso, com o auxílio de uma serra manual, cortou-se a moeda de modo que a superfície de contato com a chama seja maior e a temperatura de Curie seja atingida com mais facilidade. Aproxima-se então o imã de ferrite de modo que não entre em contato com a moeda, deixando esta apenas deslocada devido à atração magnética. Acende-se então a vela para aquecer a moeda até a temperatura de Curie correspondente.

Observa-se após certo tempo que a moeda desloca-se ligeiramente e tende a cair devido a força gravitacional, porém é logo atraída novamente pelo imã devido o resfriamento do material. Este processo tende-se a repetir até a chama da vela se esgotar.

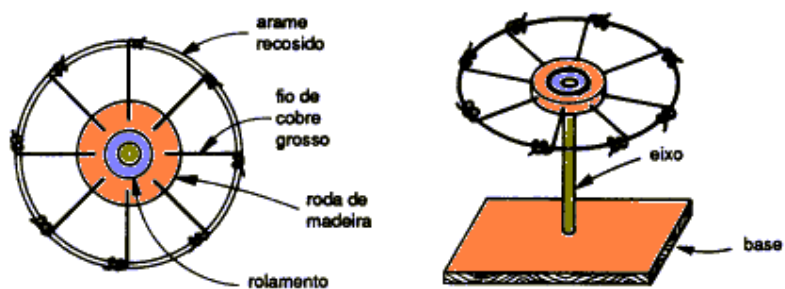
A vela pode ser ajustada manualmente, portanto não se encontra fixada. Algumas caixas de fósforo podem servir de apoio para isso.



Pêndulo de Curie finalizado

Motor de Curie:

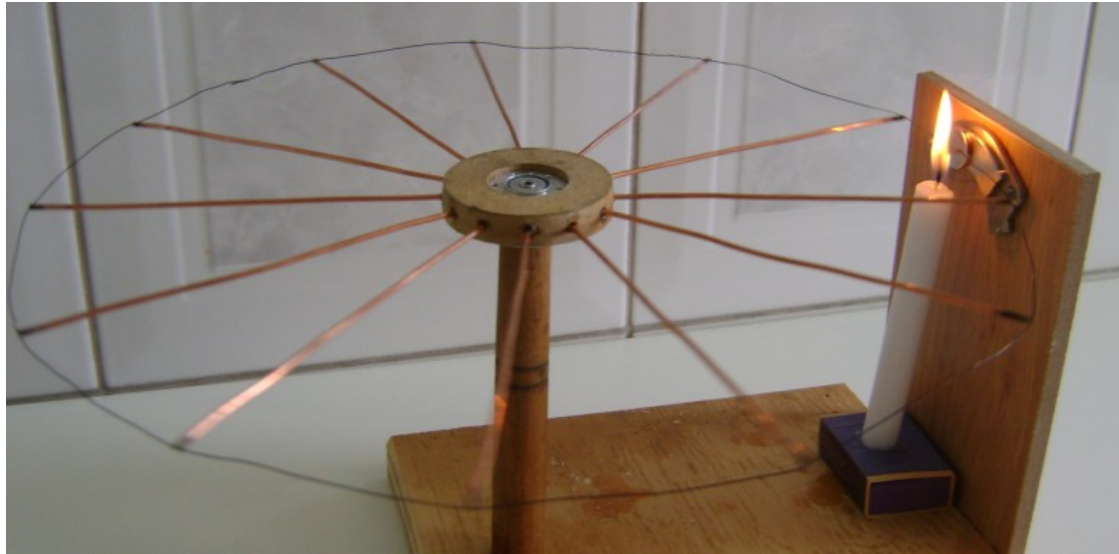
Primeiramente monta-se o esquema da figura abaixo:



Montagem do rotor

O ímã de neodímio é colocado então próximo ao aro de níquel, sendo, portanto, atraído por ele. Numa região próxima do ímã, aproxima-se a vela de modo que quando a temperatura de Curie desta região do níquel é atingida, perdem-se as

propriedades ferromagnéticas e, portanto, deixa-se de ser atraída, isso determina um torque em relação ao eixo, que faz com que o disco gire.



Motor de Curie finalizado

Pela foto acima pode-se observar que as bases e a fixação do ímã foram feitas de madeira, bem como a bolacha em volta do rolamento de disco rígido. Para fixar o eixo de rotação foi utilizado parte de uma colher de pau de cozinha. Os fios de cobre (2,5 mm) serviram de aro para que o fio de níquel pudesse ser enrolado.

4) **Resultados atingidos e dificuldades encontradas**

Após a coleta dos materiais e a montagem dos experimentos, foi observado o que realmente se esperava. No caso do pêndulo, obteve-se total sucesso na observação dos fenômenos. O mesmo pode ser dito ao motor de Curie, que de certa forma superou as expectativas pelo fato de girar muito rapidamente em relação ao que se esperava.

Durante a busca de material, conseguir níquel foi a tarefa mais difícil, que teve que ser adquirido por meio de compra online. Dez moedas colecionáveis de níquel puro (comemorativas dos 150 anos da independência) foram comprados no site www.todaoferta.uol.com.br. O fio de mesmo material (80% 20% - 0,35mm), que após muitas buscas pela internet, foi doado pela empresa Cable Tye Comercial e Industrial LTDA www.cabletye.com.br.

Outro material de difícil acesso (rolamento de hd) felizmente foi fornecido pelo orientador, juntamente com os ímãs de Neodímio encontrados no mesmo disco rígido utilizado.

Com relação à montagem experimental, a única dificuldade considerável é que o Pêndulo de Curie pode não funcionar corretamente dependendo da posição do ímã e da vela. Por isso, ambos os materiais não são fixados nas bases de madeira do experimento, ficando a cargo do usuário adequar as melhores posições dos materiais.

5) Bibliografia

A maior parte da pesquisa bibliográfica foi feita na internet (sites de busca). Parte foi feita na biblioteca do Instituto de Física “Gleb Wataghin”.

Algumas palavras chaves: “Pêndulo de Curie”, “Máquina de Curie”, “Pierre Curie”, “Efeito Curie”, “Temperatura de Curie ”Níquel””, ”Magnetismo da Matéria”.

Coloco aqui os sites mais visitados durante a busca de informações, juntamente com uma breve descrição de sua importância.

- 1- http://www.feiradeciencias.com.br/sala13/13_26.asp - Site do professor Luiz Feraz Netto que faz uma breve introdução do experimento e fornece algumas dicas de materiais a serem utilizados.
- 2- <http://www.explicatorium.com/Pierre-Curie.php> - bibliografia de Pierre Curie (ver anexo[1])
- 3- <http://pt.wikipedia.org/wiki/Ferromagnetismo> - breve explicação do que é ferromagnetismo e mais interessante por fornecer uma tabela de diversos materiais e seus respectivos pontos de Curie.
- 4- **Moysés Nussenzveig - Curso de Física Básica 3 - Eletromagnetismo; Editora Edgard Blucher Ltda (1981) – capítulo 11 – teoria relacionada.**
- 5- **Halliday, Resnick e Walker; Fundamentos da Física - Vol. 3, 6a edição; Editora: Livros Técnicos e Científicos. Capítulo 32. – teoria relacionada.**
- 6- **Halliday, Resnick e Walker; Fundamentos da Física - Vol. 4, 7a edição; Editora: Livros Técnicos e Científicos. Capítulo 40. – teoria relacionada**
- 7- pmei.unicamp.googlepages.com/acos-congresso-10.pdf – este arquivo .pdf se encontra em separado junto com o arquivo no Ensino aberto. Durante as buscas por materiais de baixo ponto de Curie, encontrei este artigo falando sobre temperaturas de Curie em ligas do tipo Fe-(Nd_{0,5}+MM_{0,5})-B. A utilização deste material não foi possível por se tratar de ligas especialmente tratadas e não são disponíveis fios do material para ser acoplado no motor do experimento.
- 8- <http://www.youtube.com/watch?v=RWrTvB-oK94> – vídeo demonstrativo de

um pêndulo de Curie.

- 9- <http://www.lorentz.leidenuniv.nl/history/spin/goudsmit.html> - Texto de Samuel Goudsmit falando sobre a descoberta do spin do elétron. (ver anexos [3])

6) Agradecimentos

Gostaria de agradecer aqui ao meu orientador Professor Mauro Carvalho que disponibilizou sua marcenaria caseira e a madeira para os dois experimentos, além do imã e o rolamento do disco rígido.

Gostaria de agradecer também a empresa Cable Tye Comercial e Industrial LTDA que gentilmente forneceu o fio de níquel (realmente difícil de encontrar) essencial ao experimento.

7) Declaração do orientador

Meu orientador realizou os seguintes comentários:

Sobre o relatório parcial:

“O aluno Rodrigo César de Campos Ferreira está desenvolvendo seu trabalho com bastante desenvoltura, procurando referências, materiais e alternativas para obter resultados mais expressivos. Acredito que tudo isso resultará num bom trabalho de instrumentação.”

Sobre o relatório final:

“O aluno Rodrigo César de Campos Ferreira, RA: 074352, desenvolveu seu trabalho com muita independência e responsabilidade. Durante todo o semestre me procurou para discutir os problemas que surgiam e teve muita iniciativa na procura de materiais e bibliografia. Foi um aluno exemplar e merece, em minha opinião, a nota máxima no trabalho”.

Anexos

Um pouco sobre Pierre Curie:

[1] – retirado de - <http://www.explicatorium.com/Pierre-Curie.php>

Pierre Curie (Paris, 15 de maio de 1859 – Paris, 19 de abril de 1906) foi um físico francês, pioneiro no estudo da cristalografia, magnetismo, piezoelectricidade e radioactividade. Obteve o prémio Nobel da Física em 1903, juntamente com a sua mulher Marie Curie, outra famosa física: "em reconhecimento pelos extraordinários serviços que ambos prestaram através da suas pesquisas conjuntas sobre os fenómenos da radiação descobertos pelo professor Henri Becquerel".

Pierre foi educado em casa pelo seu pai, e nos primeiros anos da sua adolescência revelou uma forte aptidão para a matemática e a geometria. Aos 18 anos já tinha obtido o equivalente a um grau superior, mas não seguiu imediatamente para o doutoramento por falta de dinheiro. Em vez disso, trabalhou como instrutor de laboratório.

Em 1880, Pierre e o seu irmão mais velho Jacques demonstraram que se gerava um potencial eléctrico quando se comprimiam cristais, a piezoelectricidade. Pouco depois, em 1881, eles demonstraram a existência do efeito inverso: que os cristais podiam ser deformados quando submetidos a um campo eléctrico. Quase todos os actuais circuitos electrónicos digitais recorrem a este fenómeno.

Antes dos seus famosos estudos de doutoramento sobre o magnetismo, ele concebeu e aperfeiçoou uma balança de torção extremamente sensível para medir os coeficientes magnéticos. Os investigadores que o seguiram nesta área utilizaram regularmente uma qualquer variedade deste equipamento. Pierre Curie estudou o ferromagnetismo, o paramagnetismo, e o diamagnetismo para a sua tese de doutoramento, e descobriu o efeito da temperatura sobre o paramagnetismo que é actualmente conhecido por lei de Curie.



Pierre Curie (1859 - 1906) Físico

francês.

A constante material da lei de Curie é conhecida como a constante de Curie. Ele também descobriu que as substâncias ferromagnéticas apresentam uma temperatura crítica de transição, acima da qual as substâncias perdem o seu comportamento ferromagnético. Esta temperatura é conhecida por ponto de Curie.

Pierre Curie enunciou em 1894 o "Princípio universal de simetria": As simetrias presentes nas causas de um fenômeno físico também são encontrados nas suas consequências.

Pierre trabalhou com a sua mulher [Marie Curie](#) no isolamento do polônio e do rádio. Eles foram os primeiros a usar o termo 'radioatividade', e foram pioneiros no seu estudo. No seu trabalho, incluindo o conhecido trabalho de doutoramento de Marie, usaram um electrómetro piezoeléctrico de precisão construído por Pierre e pelo seu irmão Jacques.

Pierre Curie e um estudante seu foram os primeiros a descobrir a energia nuclear, ao identificarem a emissão contínua de calor das partículas do rádio. Ele também investigou as emissões de radiação das substâncias radioactivas, e conseguiu demonstrar, com o recurso a campos magnéticos, que as emissões apresentavam carga positiva, negativa ou eram neutras. Essas emissões correspondem às partículas alfa, beta e radiações gama.

O casal Curie autenticou a médium Eusapia Paladino, numa carta a Georges Gouy, datada de 24 de Julho, 1905, por sessões supervisionadas por eles próprios:

Foi muito interessante e, realmente os fenômenos que vimos pareciam inexplicáveis como truques, mesas com quatro pernas suspensas, movimentos de objetos até a certa distancia, mãos que beliscam ou acariciam a pessoa, aparições luminosas. Tudo num local preparado por nós, com um pequeno número de espectadores, todos conhecidos nossos e sem qualquer possível cúmplice. O único truque possível é o que poderia resultar da extraordinária facilidade da médium como mágica. Mas, como explicar o fenômeno quando se está segurando as mãos e os pés dela e quando a luz é suficiente para se ver tudo que acontece?

O casal confirmou a genuinidade de Paladino em outra carta, em 14 de abril de 1906, poucos dias antes de Pierre morrer, novamente a Georges Gouy:

Tivemos mais algumas sessões com a médium Palladino. O resultado é que esses fenômenos realmente existem e não é mais possível para mim duvidar disso. É improvável, mas existem, e é impossível negar isso, após as sessões que tivemos, em condições controladas. Uma espécie de membros fluidos destacam-se da médium (principalmente dos braços e das pernas...) e empurram com força os objetos. Esses membros fluidos se formam em geral sobre um pedaço de material negro... Mas algumas vezes eles pulam para o ar aberto. Não tenho dúvida que depois de algumas boas sessões, você se convencerá... Você, que tem uma intuição tão grande, com tanta frequência sobre os fenômenos, como explica esses deslocamentos de objetos de uma distância, como concebe que a coisa seja possível? Existe aqui, em minha opinião, todo um território de fatos inteiramente novos, e estados físicos no espaço, dos quais não temos qualquer idéia.

Esta informação consta no livro de Susan Quinn, chamado "Marie Curie, Uma Vida". Pierre

Curie morreu em 19 de Abril de 1906, em resultado de um acidente de viação quando atravessava a *Rue Dauphine* em Paris durante uma tempestade. A sua cabeça foi esmagada pela roda de uma carruagem, escapando a uma provável morte por envenenamento por radiações como a que veio a matar a sua mulher. Os restos mortais de Pierre e Marie foram depositados na cripta do Panthéon de Paris em Abril de 1995. O curie (Ci) é uma unidade de

radioactividade correspondente a 3.7×10^{10} desintegrações por segundo. O nome da unidade foi originalmente atribuído, em homenagem a Pierre Curie, pelo Congresso de Radiologia de 1910.

A filha de Pierre e Marie Curie, [Irène Joliot-Curie](#) e o seu genro, Frédéric Joliot, foram igualmente físicos destacados, que se dedicaram ao estudo da radioactividade.

[2]- Retirado de <http://plato.if.usp.br/1-2005/fnc0376n/na5/node5.html>

Spin do elétron

O elétron (e outras partículas como o próton e o nêutron) possui um outro grau de liberdade além dos representados pelas três coordenadas espaciais. Ele têm um outro momento angular, adicional ao momento angular orbital, que chamamos de *momento angular de spin*. Não se trata de um momento angular devido a rotação do elétron em torno de si mesmo: até onde sabemos o elétron não tem estrutura. Além disso, se fosse um momento angular de rotação no espaço, ele seria quantizado em números inteiros de \hbar . Uma componente qualquer do momento angular de spin

do elétron, S_z , por exemplo, entretanto, assume os valores $\pm \frac{1}{2}\hbar$, ou seja múltiplos semi-inteiros de \hbar .

Mas o momento angular de spin é um momento angular e as variáveis dinâmicas S^2 e S_z associadas têm auto-valores exatamente similares aos correspondentes do momento angular orbital L^2 e L_z :

$$S^2 \rightarrow s(s+1)\hbar^2 \quad L^2 \rightarrow \ell(\ell+1)\hbar^2 \quad (1)$$

$$S_z \rightarrow m_s \hbar \quad L_z \rightarrow m_\ell \hbar \quad (2)$$

$$m_s = -s, -s+1, \dots, s-1, s \quad m_\ell = -\ell, -\ell+1, \dots, \ell-1, \ell \quad (3)$$

com a diferença de que ℓ pode ser qualquer inteiro não negativo, $\ell = 0, 1, 2, \dots$, enquanto para o

spin do elétron $s = \frac{1}{2}$. Por isso, para um único elétron, só há dois estados de spin possíveis,

$S_z = +\frac{1}{2}\hbar$ e $S_z = -\frac{1}{2}\hbar$, a que também nos referimos como *spin para cima* e *spin para baixo*, ou

$spin +$ e $spin -$, ou \uparrow e \downarrow .

Assim, para a descrição do estado quântico de um elétron é necessário, além de especificar a função de onda $\psi(\mathbf{r})$, especificar o seu estado de spin. Um estado de spin genérico é uma combinação linear dos dois auto-estados de S_z , \uparrow e \downarrow .

No caso de auto-estados da energia, eles estarão associados a quatro números quânticos, os três da auto-função mais um para especificar o spin. Assim um auto-estado de um elétron no átomo de hidrogênio, por exemplo, pode ser especificado por n , ℓ , m_ℓ e m_s . Na teoria de Schrödinger a energia é independente do spin, e as funções de onda são idênticas para os estados com $m_s = +\frac{1}{2}$ e $m_s = -\frac{1}{2}$. Os auto-estados podem ser:

$$\psi_{n,\ell,m_\ell,+}(\mathbf{r}) = R_{n,\ell}(r)Y_{\ell,m_\ell}(\theta,\phi)$$

$$\psi_{n,\ell,m_\ell,-}(\mathbf{r}) = R_{n,\ell}(r)Y_{\ell,m_\ell}(\theta,\phi),$$

ambos com energia

$$E_n = -\frac{e^2}{8\pi\epsilon_0 a_0} \frac{1}{n^2}.$$

Assim, a degenerescência de um nível n , que era n^2 devido à multiplicidade de ℓ e m_ℓ (n valores para ℓ e $2\ell + 1$ valores de m_ℓ para cada ℓ), passa a ser $2n^2$ devido à degenerescência de spin.

O momento magnético de spin do elétron

A relação entre momento de dipolo magnético e momento angular orbitais do elétron já foi discutida anteriormente:

$$\mu_{Lz} = -m_\ell \frac{e\hbar}{2m_e} = -m_\ell \mu_B.$$

Já vimos que o momento angular de spin não é um momento angular usual. Esta peculiaridade também se reflete no momento magnético a ele associado. O fator de proporcionalidade entre momento magnético e momento angular de spin é aproximadamente o dobro do mesmo fator para o momento

angular orbital. Embora a componente z do momento angular de spin seja $\pm \frac{1}{2} \hbar$, o momento magnético associado é $\mu_{S_z} = \pm \mu_B$ (e não metade de μ_B , como seria obtido fazendo $m_\ell = \pm \frac{1}{2}$ na expressão para μ_{L_z}). Este fato só tem explicação na teoria quântica relativística. A relação entre spin e momento magnético se escreve:

$$\mu_{S_z} = -g_s m_s \mu_B,$$

onde $g_s = 2.0023$. Normalmente usamos a aproximação $g_s = 2$ para o spin.

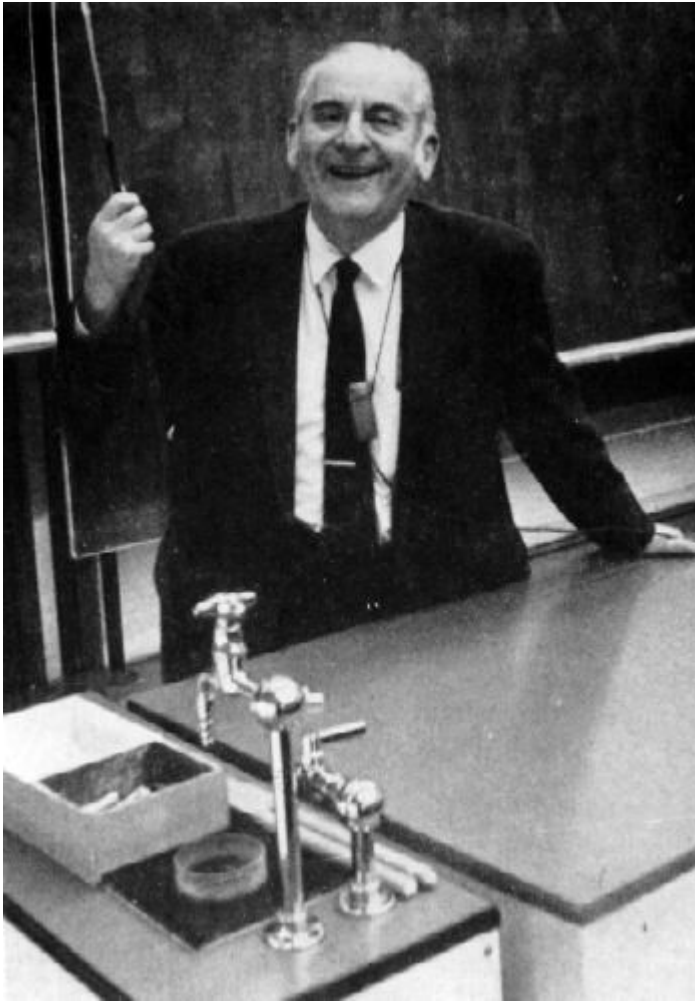
[3]- Retirado de <http://www.lorentz.leidenuniv.nl/history/spin/goudsmit.html>

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



Goudsmit delivering his lecture in 1971.

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 others who also made contributions. They can't help it since that is the way 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 professor. And Ehrenfest was always worried when we interrupted our classes when we had to go 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 structure 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".



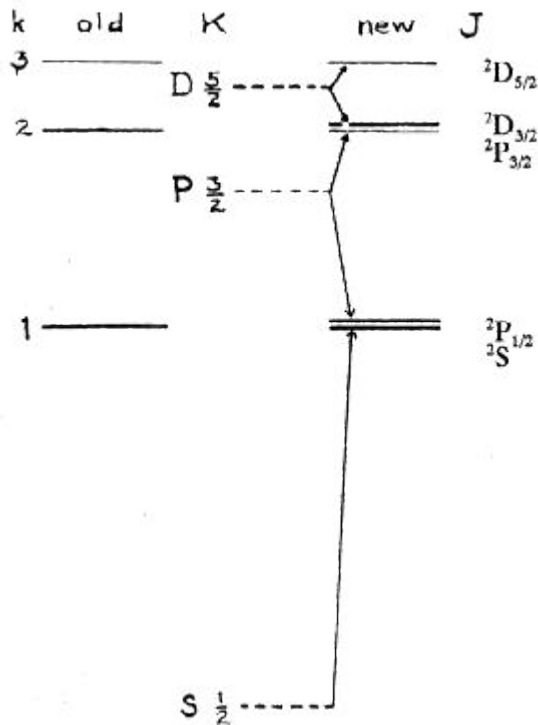
Leiden 1924. From left to right: Dieke, Goudsmit, Tinbergen, Ehrenfest, Kronig, Fermi. Note: Tinbergen later changed from physics to economy and became the first Nobel laureate in economy (1969).

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.

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 all these things about the spectra, and I then said: "That fits

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 quantum number $n=3$, presented by Goudsmit in the form in which it appeared in the original publications of 1926. 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 used for 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 $3^2P_{1/2} \rightarrow 2^2S$ in which the total angular momentum is conserved and L changes by plus or minus 1.

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 it 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 it 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 celebrities, 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 the 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.

Refer

ences

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[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.

