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GEORGE EUGENE UHLENBECK
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A Biographical Memoir by
GEORGE W. FORD

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BY GEORGE W. FORD

GEORGE EUGENE UHLENBECK was a distinguished theoretical physicist, perhaps most widely known for the discovery of electron spin, made together with Samuel Abraham Goudsmit while they were both graduate students in Holland. In a long career he made important contributions to atomic and molecular physics, nuclear physics, and above all, statistical mechanics. He was a superb lecturer and expositor, with a deep grasp of his subject and an orderly delivery of great clarity, spiced with subtle humor.

He was born December 6, 1900, in the East Indies, in Batavia, Java (now Jakarta, Indonesia). His was a military family that had served for generations in the army of the Dutch East Indies. His father, Eugenius Marius Uhlenbeck, was a lieutenant colonel, while his mother, Anne Marie Beeger, was the daughter of a major general. At the time of his birth there was an older sister, Annie, born five years earlier. Later there were two younger brothers: Willem Jan born in 1906 and Eugenius Marius born in 1913. The latter became well known as a linguist, expert in the Javanese language and professor at the University of Leiden.

Because of his father's duties, the family moved about and George's early years and schooling were in the small towns of the East Indies. Then in 1907, in large part to get

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better schooling for the children, his father retired from the army and moved the family permanently to Holland, where they settled in The Hague. His father's pension was not very high, so the family's circumstances were modest, yet adequate. George attended elementary and high school in The Hague. He was a good student, very dutiful, but he showed no predilection for science until his last years in high school. Then his sister Annie urged him to contact A. H. Borgesius, who taught physics in the school and had a reputation as the most learned person on the staff. Apparently Borgesius was very shy and George, the young student, was shy as well, so they had a strange connection. Yet Borgesius gave him books to read, among them H. A. Lorentz's *Beginselen der Natuurkunde* (2 vols., 1908, 1909), based on lectures on elementary physics given to high school students in Leiden. He studied this carefully and was especially taken by the discussion of kinetic theory. Later he worked through Lorentz's *Lehrbuch der Differential-und Integralrechnung* (1900). As a result he knew calculus when he took his final high school examination in July 1918.

At that time, by law entrance to a Dutch university required knowledge of Latin and Greek, taught in a *gymnasium* but not in the higher *burgher* school that George had attended. He was not interested in a military career, which his parents might have wished, and so it was that in September 1918 he entered the Institute of Technology at Delft to study chemical engineering. He was not happy there, finding the prescribed curriculum onerous: the many lectures one had to attend, the required chemistry laboratories. Luckily, that autumn the law was changed to allow those in such fields as physics to enter universities without satisfying the language requirement. With the help of his sister he was able to persuade his parents to allow him to leave the Delft institute and enter the University of Leiden in January 1919.

Leiden seemed to him a paradise. There were few lectures and in none was attendance required. There were no examinations. One afternoon a week he worked at the prescribed laboratory exercises, but that was all. So he had much free time, which he put to good use. He began a study of Boltzmann's *Vorlesungen über Gastheorie* (1898). He found this difficult to grasp until his brother-in-law, Harm Buning, told him of Paul and Tatyana Ehrenfest's article in the *Enzyklopädie der Mathematische Wissenshaften* (1911). This was a revelation; he saw what Boltzmann was about. In order to work his way through these works he made a study of analytical dynamics, using a thin book by Paul Appell. He also followed some of the mathematics lectures and lectures on thermodynamics by Professor J. C. Kuenen. He worked hard at the required laboratory exercises, one afternoon a week the first year, two the second. Being, as he says, a dutiful student, he wrote careful reports with derivations of all the formulas.

In December 1920 he passed the *candidaat* examinations, which were oral examinations in mathematics and physics and marked the end of the undergraduate years. As a graduate student he began to follow the lectures of Paul Ehrenfest, who was to have such a profound influence on him. These were electrodynamics one year and statistical mechanics with some quantum mechanics the next. He also attended the famous Wednesday evening Ehrenfest colloquium, to which one was invited only after passing the candidate examinations. There was also the Huygens club. Members were graduate students in the sciences who met weekly at one anothers' rooms to hear talks and discuss their studies. Ehrenfest encouraged this, feeling that an important part of education was students learning from one another.

In his first year of graduate studies, largely through the influence of Kuenen, he received a State Fellowship. This paid his tuition and was a great help to his parents. He was able to have a room in Leiden, no longer commuting daily from The Hague. It was at this time he came into closer contact with his uncle (actually his father's cousin) Cornelius C. Uhlenbeck, a distinguished linguist and professor at Leiden. He went once a week to have lunch with his uncle and was much impressed by his uncle's extensive library and the atmosphere of learning. From September 1921 to June 1922 Uhlenbeck taught 12 hours a week in a high school in Leiden. Although this was a great financial help, he found high school teaching distasteful; maintaining order among the mischievous young women was difficult. At the end of that year, in his lecture Ehrenfest asked if anyone was interested in a job in Rome. Uhlenbeck raised his hand and in September he began as a tutor to the younger son of the Dutch ambassador.

He was three years in Rome, returning to Holland in the summers. The first year he began the study of Italian, first at the Berlitz school, then with a private tutor. He eventually became fluent, reading Dante in the original. However, he did not neglect his studies of physics. In September 1923 before returning to Rome, he took the examinations for the degree of *doctorandus* (roughly equivalent to the master's degree in an American university). Unlike the earlier *candidaat* examination, these were not entirely oral but required a pair of essays, one in mathematics, the other in physics. For physics he submitted an essay on the dynamical theory of diffraction.

His second year in Rome was the "Fermi year." When he left Holland that fall, Ehrenfest asked him to look up Enrico Fermi, with a letter and a list of questions about Fermi's paper on the ergodic theorem, written earlier that year during

a visit to Gottingen. This he did and the two soon became good friends. Uhlenbeck spoke to Fermi in glowing terms about Ehrenfest and the atmosphere in Leiden. They even organized a small Ehrenfest-style colloquium, at which Fermi did most of the talking. A result of this was that Fermi in the following year used the remainder of a fellowship from the Italian Ministry of Public Instruction to spend the fall term in Leiden with Ehrenfest. In later years Fermi credited this contact with Ehrenfest with giving him a renewed confidence in his scientific abilities. He and Uhlenbeck also discussed the troubling political situation; Mussolini's march on Rome had occurred only the year before. During this his second year in Rome, Uhlenbeck also followed lectures in mathematics by Tullio Levi-Civita and Vito Volterra.

The following summer in Holland, Ehrenfest urged Uhlenbeck to come back to his studies; it was time to bring the Roman episode to an end. But he had liked Rome so much, and the ambassador was anxious for him to continue, that he decided to return for a third year. From the beginning things were different. Fermi was no longer there, away first in Leiden and then to a position in Florence. Then Uhlenbeck became fascinated by the study of art history, reading voraciously. He associated mostly with members of the Royal Netherlands Institute in Rome, which he thought was a wonderful place. Indeed, his first published paper appeared at that time, devoted to a topic in art history.

When he returned to Leiden in June 1925 it was with the intent of studying history. Among others, he spoke of this with his uncle Cornelius Uhlenbeck, who was sympathetic but pointed out that knowledge of Latin and Greek was required. His uncle also suggested that he go ahead to get the Ph.D. degree with Ehrenfest, since that was somehow more practical. George spoke with Ehrenfest about this and he agreed to try. So that summer Uhlenbeck began to

study Latin with a tutor in The Hague. At the same time Ehrenfest arranged for him to meet regularly with his fellow student Samuel A. Goudsmit, who would tell him about the recent developments in atomic spectroscopy. The two were acquainted since they had been students in the same high school in The Hague and had also met during Uhlenbeck's second year in Leiden, but this summer a bond of friendship formed that was to last throughout their lives.

With Sam's help George learned quickly. They wrote together a short note on the spectrum of hydrogen and helium in which they took account of the two-valued quantum numbers introduced earlier that year by Pauli and gave what are now the accepted assignments of the energy levels. Then, in September while Sam was explaining the Pauli principle using the two-valued quantum number assigned to the electron, George remarked that there must be a corresponding degree of freedom: the electron must rotate. When they came to Ehrenfest with this idea he was a bit skeptical but told them to write a brief note that he would send off to *Naturwissenschaften.* The appearance of this note caused a sensation, tinged with some skepticism.

At that time Niels Bohr traveled by train to Leiden to take part in the celebration of the 50th anniversary of H. A. Lorentz's doctorate. He was met at stations on the way by Pauli and by Heisenberg who asked what he thought of spin. He was noncommittal. At Leiden he was met by Ehrenfest and Einstein, who explained how the moving electron feels an induced magnetic field, giving rise to the spin-orbit coupling. With this Bohr became an advocate of the spinning electron. While in Leiden he spent a good deal of time with Goudsmit and Uhlenbeck, urging them to write a note to *Nature*, which he followed with a note of his own. Later L. H. Thomas, a young English visitor in Bohr's institute in

Copenhagen, explained as a relativistic effect the factor of two in what is now called the Thomas precession of the electron spin. With these developments electron spin became the key to our modern understanding of atomic structure.

At this stage Uhlenbeck and Goudsmit were only in a sense beginning graduate students, both had only recently passed the *doctorandus* examinations, Uhlenbeck having lost some time due to his sojourn in Rome. Uhlenbeck now began to work closely with Ehrenfest. At first he found the working sessions every afternoon exhausting, but he soon became used to them. That fall he became Ehrenfest's assistant, a position he held until he left Leiden. This period of close, intense interaction had an enormous impact on Uhlenbeck. He came to respect clear, simple explanations more than the learned expositions he had earlier prized. Ehrenfest wanted always to know first what is the point; *Was ist der Witz*? he would ask.

The discovery of spin was made in the spirit of the old quantum theory. The first paper of Heisenberg on the new quantum mechanics had appeared only a couple of months earlier. This and papers later that year by Born and Jordan and the famous three-man paper of Born, Heisenberg, and Jordan were studied by Uhlenbeck and Ehrenfest. They recognized that with these papers there was a real advance, but the unfamiliar matrix mathematics was daunting. This all changed with the appearance early the following year of Schrödinger's papers on wave mechanics. One knew about wave equations. The spring of 1926 Uhlenbeck and Ehrenfest worked intensely on the new wave mechanics. That summer Oskar Klein came to spend some time in Leiden. He had not long before returned to Europe after three years at the University of Michigan in Ann Arbor, where he had developed his five-dimensional generalization of relativity. He also had the relativistic wave equation, now called the Klein-Gordon

equation. Every afternoon Uhlenbeck and Klein met with Ehrenfest for long discussions. Uhlenbeck and Ehrenfest even wrote a paper on Klein's five-dimensional relativity.

That fall it was time for Uhlenbeck to choose a topic and write a thesis. Since his high school days he had been interested in kinetic theory, so it was decided he would write on the application of the very new Fermi-Dirac and Bose-Einstein statistics to the ideal gas. His aim would be to present something in the style of the Ehrenfest *Enzyklopädie* article that had so impressed him in his undergraduate days. He continued to work daily with Ehrenfest, discussing topics for his thesis along with many others. It was a very exciting time with a rapid development of quantum mechanics and its applications. In the spring of 1927 it seemed that with the many distractions in Leiden he would never get the thesis written, so Ehrenfest sent him off to Bohr's institute with a Lorentz fellowship. There in two months of hard writing he wrote his thesis. In it he gave a consistent description of the consequences of the different statistics on the description of an ideal gas. In his thesis Uhlenbeck criticized Einstein's discussion of what is now called the Bose-Einstein condensation, arguing that for a gas of any finite number of particles there can be no discontinuity. He was technically correct; at the time it was not recognized that the singularities of the thermodynamic functions are limit properties, and even Einstein agreed that he had a point. In fact, the problem of understanding the phenomenon of condensation was a recurring theme throughout his scientific career. Sadly he did not live to see the demonstration of Bose-Einstein condensation (for a finite number of particles) in 1995.

With his thesis written he left Copenhagen and, having some time left on his Lorentz fellowship, went to Gottingen. There he learned of the Pauli paper giving the first description of electron spin within the framework of Schrödinger's

wave mechanics. With that, one could actually calculate the fine structure of simple atoms. While in Gottingen he became friends with Robert Oppenheimer, who had recently gotten his Ph.D. under the direction of Max Born and was a leader among the younger students. When Uhlenbeck returned to Leiden, Oppenheimer went with him.

That summer brought many changes in Uhlenbeck's life. He and Goudsmit already had positions as instructors at the University of Michigan waiting for them. This had come about when Walter Colby came to Europe during the 1926-1927 academic year with the charge from Harrison M. Randall, the chairman of the Michigan Physics Department, to find a replacement for Oskar Klein. When Colby came to Leiden and spoke to him about this, Ehrenfest said that they should hire at least two young people, so that in what was then the wilderness they would have someone to talk to. The result was that the two young Leideners both got offers. Uhlenbeck was especially pleased to accept, since it meant that he would have a position at a university and wouldn't have to teach high school. Then, on July 7 he and Goudsmit both took their thesis defense and received the Ph.D. On August 23 he married Else Ophurst, who had been a chemistry student in Leiden. At the end of that month together with the Goudsmits they took ship for New York. They were met at the dock by Oppenheimer, who showed them some of the sights of the city and persuaded them to extend their stay in New York in order to meet his parents. They were especially impressed by the elegant Oppenheimer apartment on Riverside Drive. Then in early September they took a train for Ann Arbor to take up their positions at the university.

They found there an active department of physics, under the inspired direction of Randall, who had persuaded the powers at the university to let him hire not two but four young theorists. Aside from Goudsmit and Uhlenbeck there

were Otto Laporte, who had received his Ph.D. in 1924 at Munich under the direction of Arnold Sommerfeld and had arrived the year before, and David Dennison, who had received the Ph.D. at Michigan in 1924 under the direction of Oskar Klein and was returning after three years in Europe. Each of these young theorists were already known to the international community for his accomplishments: Uhlenbeck and Goudsmit for their discovery of electron spin, Dennison for his explanation of the low temperature specific heat of hydrogen, and Laporte for his application of parity conservation to atomic spectra embodied in the Laporte rule. They found at Michigan a department active in research and strong in atomic and molecular spectroscopy. It was by no means a wilderness, but it was isolated from the European centers of theoretical physics. They set about to change that.

Since 1923 there had been at Michigan a summer school program with invited lecturers, but its influence had been for the most part local. Under the leadership of Uhlenbeck and his colleagues this changed dramatically. The first summer after their arrival there began a series of summer schools devoted to instruction in theoretical physics that was unique in America, probably the world. The list of resident lecturers (and students) is one of contemporary leaders in the development of physics. These summer schools flourished throughout the 1930s, playing a significant role in the rapid progress of modern physics research in America, until the entry of the United States into World War II brought them to an end. They were revived after the war, but conditions had changed and they were no longer unique.

Uhlenbeck also instituted an Ehrenfest-style colloquium. Held Wednesday afternoons rather than evenings, it nevertheless bore many of the characteristics of the original. Above all was the emphasis on clarity. "What is the point?" he would ask. Even guests were not spared. Most of the

speakers were local and for them the rule was to give an overall view of their subject: what had been achieved and what were the outstanding questions. As with the Ehrenfest colloquium, students were not invited until they had passed the preliminary examinations and then they were expected to attend. The result of these colloquia as well as the summer schools was a remarkably close-knit group of faculty and students, familiar with the progress of research in the various fields of physics.

The pattern of those early years in Ann Arbor was one of a summer session of intense activity and a school term devoted to digesting and extending what had been learned. Shortly after they arrived, Uhlenbeck and Goudsmit wrote together on rotational Brownian motion. In 1930 the classic paper on Brownian motion with L. S. Ornstein appeared. There they introduced what is now called the O-U process, which takes into account the inertia of the Brownian particle. His interest in Brownian motion continued with work on a number of topics, culminating in the 1945 *Reviews of Modern Physics* article with Ming Chen Wang. This review article is still regarded as the standard reference for physicists.

In 1932 Dennison and Uhlenbeck wrote an article on the double minimum problem in quantum mechanics. There they introduced the now standard method using the WKB approximation. Of more general interest was their application of their solution to the calculation of the inversion splitting of vibrational lines of the ammonia molecule. These lines had been measured in the infrared with sufficient accuracy to see the splitting, but a direct measurement, which is in the microwave region, didn't exist. Shortly after, their colleague Neil H. Williams and his student Claud E. Cleeton developed a magnetron source and used it to make the first direct measurement of the ammonia splitting, thus initiating the field of microwave spectroscopy.

Through lectures and discussions at the summer schools, especially with his friend Fermi, Uhlenbeck during those years began work in nuclear physics. Perhaps best known from those days are the papers on the Fermi theory of beta decay published jointly with his student Emil Konopinski. They proposed an alternative to the Fermi theory involving gradients of the wave function, which for a time seemed to fit the observations better. They returned to the subject in 1941 when in a masterly presentation of the most general form of the Fermi theory, they gave the energy distribution of the emitted electron for first and second forbidden transitions.

In the fall of 1935 Uhlenbeck returned to Holland to become professor at the University of Utrecht. This was a position that had become vacant when H. A. Kramers moved to Leiden after the death of Ehrenfest. While he worked on a number of topics in nuclear physics and statistical mechanics during his time at Utrecht, the most important was probably the theory of condensation. The question, which as remarked above was a recurring theme of his scientific life, was this: how to explain the observation that, independent of the detailed form of the interatomic force, a gas condenses into a liquid at a sharp temperature. At a 1937 conference in Amsterdam celebrating the 100th anniversary of the birth of van der Waals, there was a vigorous debate on the question of phase transitions in thermodynamic systems in which Kramers made the point that the sharp discontinuities associated with phase transition could only occur in the thermodynamic limit. While this argument was not accepted by everyone present, Uhlenbeck at once recognized its importance. In his thesis he had argued that there could be no sharp discontinuity in a finite system; now he realized there could be in an infinite one. Together with his student Boris Kahn he proposed a criterion for the existence of the

condensation transition in a gas. The idea was that condensation corresponded to a singularity in the pressure expressed in terms of the fugacity. Theirs was the generally accepted theory of condensation until C. N. Yang and T. D. Lee gave in 1952 a more general criterion based on the behavior in the entire complex fugacity plane.

In the fall of 1938 Uhlenbeck spent a semester as visiting professor at Columbia University. There he shared an office with his old friend Enrico Fermi, who had not returned to Italy after receiving the Nobel Prize. From Fermi he learned about the recent discovery of nuclear fission. At Columbia he gave an Ehrenfest-style oral examination to help Julian Schwinger satisfy the requirements for his doctoral degree. Some years earlier Uhlenbeck had met the very young Schwinger when as a student he attended the 1936 Michigan summer school. Uhlenbeck returned to Utrecht after his semester at Columbia but left the following summer to return again to Michigan as professor of physics.

While at Columbia he had become interested in problems in the theory of cosmic rays, writing a paper on the subject jointly with W. E. Lamb and A. Nordsieck. He continued that work at Michigan, lecturing on the subject in the 1940 summer school and with his student William T. Scott. He also continued to work in the field of nuclear physics, studying with another student, Robert J. Bessey, the problem of internal conversion of gamma rays. A great event was the birth in 1942 of George and Else's son, Olke Cornelius. In later years Olke became a well-known biochemist and a member of the National Academy of Sciences; George was very proud. Then in 1943 George left Michigan on leave to direct the theoretical group at the MIT Radiation Laboratory in Cambridge, Massachusetts. This wartime research was largely devoted to waveguide theory and problems of noise in radar systems and is summarized in the book *Threshold*

Signals (1950) edited jointly by Uhlenbeck and J. L. Lawson. It was in Cambridge that he met the mathematician Mark Kac, who was a member of his group. They developed a close friendship and collaboration that lasted until Kac's death in 1984. Also in his group was Julian Schwinger, who had simply shown up one day having left the atomic energy group at Chicago. It took considerable diplomatic skill on Uhlenbeck's part to mollify the people at Chicago. He later said that he had somehow always been rescuing Schwinger.

In 1945 he returned to Ann Arbor. There he began to work on problems of kinetic theory with C. S. Wang Chang, a research associate supported through an Office of Naval Research contract. In all, they wrote together 11 papers on various problems in kinetic theory. Of these, 10 were published as University of Michigan research reports; one was not published but appeared some years later in an article with J. de Boer. Despite this somewhat clandestine publication, these reports were widely circulated and justifiably famous. At this time he made his last foray into nuclear physics, directing two doctoral students, Daniel S. Ling Jr. and David L. Falkoff, who worked on problems of angular correlations in successive nuclear transitions. It seems that the experimentalists were at first rather skeptical about the existence of such correlations, which soon became an important tool for sorting out the angular momentum and parity of nuclear states.

In 1952 Uhlenbeck became an American citizen. Two years later he was named Henry Carhart Professor of Physics. It was in these years that he returned to the condensation problem. This time it was in the form of a study of the graphs that can be associated with the terms in the Mayer expansion of the equation of state. Together with the mathematician Frank Harary and his students Robert J. Riddell and George W.

Ford he wrote a number of important papers on the mathematical theory of graphs. A summary of this work appeared in a 1962 review article written jointly with Ford.

Since his student days, Uhlenbeck had been fascinated with the problem of the approach to equilibrium, in particular with the description afforded by the Boltzmann equation. In the mid-1950s he learned of the work of the Russian physicist N. N. Bogoliubov, who had sketched out a way to extend the Boltzmann equation to include the effects of multiparticle collisions. Uhlenbeck took up this work, with the dream of obtaining a density expansion of the Boltzmann collision term analogous to the virial expansion of the equilibrium equation of state. With his student S. T. Choh he derived the Choh-Uhlenbeck equation, which includes the effects of two- and three-particle collisions. They used this to calculate first order density corrections to the viscosity and heat conductivity. This work is the subject of the Higgins Lectures given by Uhlenbeck at Princeton University in 1954. Others carried on with this program, but the discovery that the corrections due to collisions of four or more particles are divergent brought an end to the dream. Nevertheless, this work of Uhlenbeck remains a major contribution to the deep question of the approach to equilibrium.

During the academic years 1948-1949 and 1958-1959, Uhlenbeck spent sabbatical leave at the Institute for Advanced Study in Princeton, New Jersey. The year 1959 he was president of the American Physical Society. In 1960 he gave a series of lectures at a Summer Seminar in Applied Mathematics arranged by the American Mathematical Society and held at the University of Colorado. A result was the book, *Lectures in Statistical Mechanics,* authored with G. W. Ford and published in 1963. Then, in the fall of 1960 Uhlenbeck left Michigan for the last time, taking up a position as

professor at the Rockefeller Institute for Medical Research in New York City. There he was joined by his old friends Mark Kac, coming from Cornell University, and Theodore H. Berlin, from Johns Hopkins University. No small part of the attraction of Rockefeller was the dynamic leadership of its president, Detlov W. Bronk, who had a vision of turning the institute into a university with these three as the nucleus of teaching and research in mathematics and physics. Indeed, in 1965 the Rockefeller Institute became Rockefeller University. Uhlenbeck and Berlin had for some time had a plan to write together a book on statistical mechanics. Unfortunately, Berlin died suddenly in 1962 and the book was never written.

Not long after arriving at Rockefeller, Uhlenbeck returned for the last time to the condensation problem. In collaboration with Kac and Per Christian Hemmer, a visiting fellow from Trondheim University in Norway, he began a study of the equation of state of a one-dimensional gas with an interaction potential of the form of a hard core and an exponential attraction. It had been shown earlier by Kac that in the thermodynamic limit this is an exactly soluble problem. Now they studied the further limit where the range of the attraction becomes infinite while the strength goes to zero such that the area under the potential stays finite. It had long been known that a one-dimensional gas cannot have a phase transition, but surprisingly in this limit they were able to show that it can. Moreover, the equation of state they found is exactly of the van der Waals form but with a flat portion of the isotherm corresponding to the gas-liquid transition. Thus for this model the condensation problem is solved exactly. They went on to discuss the two- and three-particle correlations for this model as well as the behavior near the critical point. This work remains a beautifully instructive illustration of the condensation phenomenon.

In 1971 Uhlenbeck retired from Rockefeller. He remained active there, with lively discussions with colleagues over lunch consisting of a sandwich and a beer purchased at a Third Avenue delicatessen. In 1985 he and Else left New York, moving to Champagne-Urbana, Illinois, where their son, Olke, was professor of microbiology. A year later they moved to Boulder when Olke took up a position at the University of Colorado. There George died of a stroke at age 87.

In his later years Uhlenbeck received many honors. In addition to those mentioned above, these included election to the National Academy of Sciences in 1955; the first Lorentz Professor, Leiden University, 1955; Henry Russel Lecturer, University of Michigan, 1956; Oersted Medal of the American Association of Physics Teachers, 1956; van der Waals Professor, Amsterdam University, 1964; Planck Medal of the German Physical Society (with S. Goudsmit), 1965; Lorentz Medal of the Royal Dutch Academy of Sciences, 1970; Medal of Science of the United States, 1977; and the Wolf Prize in 1979.

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