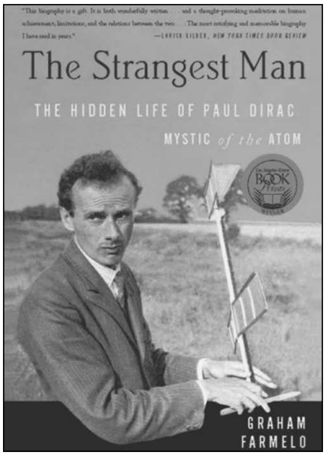
Book Review



The Strangest Man: The Hidden Life of Paul Dirac, Mystic of the Atom

Reviewed by Brian E. Blank

The Strangest Man: The Hidden Life of Paul Dirac, Mystic of the Atom
Graham Farmelo
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In September 1926 Paul Dirac began a five-month visit with Niels Bohr at the Institute for Theoretical Physics in Copenhagen. Dirac, then barely twenty-four years old, had been in possession of his Ph.D. since only May of that year. Bohr was middle aged and already a Nobel laureate, but he was accustomed to working with junior colleagues. Nevertheless, collaboration between the two physicists proved to be impossible. Whereas Dirac was taciturn, Bohr was voluble: his habit of thinking out loud produced a constant stream of sentences in need of revision or retraction. By bouncing half-baked ideas off Dirac, Bohr elicited little more than the response, "At school I was always taught not to start a sentence until I knew how to finish it." When Bohr bemoaned to Ernest Rutherford, "Dirac never says anything," Rutherford replied with a story about a shop owner assuaging a dissatisfied customer who had purchased a parrot that would not speak. "Please forgive me. You wanted a parrot that talks, and I gave you the parrot that *thinks*."

Dirac departed Copenhagen in January 1927 to stay with Max Born in Göttingen. In doing so, he joined the steady flow of promising young physicists who traveled between those two centers of quantum physics in the 1920s. According to Max Delbrück, one of Born's students and a future Nobelist, these peripatetics were "highly bizarre, genially mad, unworldly, and completely, decidedly difficult in their behavior toward their fellow man." Even in that eccentric company, Dirac stood out. In a conversation with Kurt Gottfried in 1959, Bohr remarked that, of all the visitors to his institute, "Dirac was the strangest man."

Graham Farmelo, Senior Research Fellow at London's Science Museum and Adjunct Professor of Physics at Northeastern University, has cleverly co-opted Bohr's characterization of Dirac for the title of his recently published biography. With a spareness that would meet Dirac's approval, *The Strangest Man* does double duty as a title: it intrigues the reader who is only moderately interested in Farmelo's subject, and it announces a trait of Dirac that the reader will find relentlessly stressed. Farmelo's subtitle, *The Hidden Life of Paul Dirac, Mystic of the Atom*, may also be an effective hook, but it is a deceptive one: the phrase "hidden life" suggests a kinky, shady, or salacious side of Dirac that did not exist. He did not deal in nuclear secrets, he abstained entirely from alcohol and tobacco, and he accepted only the weakest doses of caffeine. His love life was conventionally proper. When an incredulous physicist asked Dirac if he had any vices, the reply was, "No obvious ones." Nothing in Farmelo's book contradicts that.

Of course, Dirac did keep many aspects of his life private, and, as a conscientious biographer, Farmelo has made every effort to ferret them out. Dirac could, for example, spend a day at the cinema watching Mickey Mouse films, a vice he kept secret from his Cambridge colleagues. He also concealed his romance with Margit Balázs (née Wigner), Eugene Wigner's younger sister. When anyone knocked at his door, the thirty-three-year-old bachelor took down a picture of Margit and hid it in a drawer. For Dirac, keeping a photograph of his swimsuited

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future wife on the mantelpiece was as lurid as it got.

Paul Adrien Maurice Dirac was born in Bristol on August 8, 1902, to a Cornish mother and francophone Swiss father. Paul's childhood was not a happy one. In interviews he gave late in his life, Paul portrayed his father, Charles, as a cold tyrant who psychologically abused him and his older brother. It is this family dynamic that Farmelo highlights in the prologue with which he begins his work. The setting for the prologue is an evening in 1980 when Margit and Paul entertained a friend in their home. In the course of this social occasion, an innocuous detail Margit mentioned triggered a two-hour monologue during which Paul unburdened himself before his startled guest. "I never knew love or affection when I was a child," Paul volunteered. He also spoke of the tragedy of his brother, who, in 1925, bullied and frustrated by Charles at every turn, took his own life. Paul's visitor, aware of his host's reputation for equability and pathological reticence, was shocked to hear him angrily unleash the demons that had haunted him for seventy years.

Farmelo precedes his prologue with a brief extract from Samuel Butler's novel, *The Way of All Flesh*: "Unkindness and selfishness on the parts of parents towards children…may cast a gloom over their children's lives for many years." This self-evident observation, expressed without the wit or elegance usually found in a quotation, is actually an ingenious reference. Butler's semi-autobiographical story of Ernest Pontifex happened to be published a few months after Paul Dirac's birth. Charles Dirac might as well have used Ernest's father, Theobald, as a model for becoming a cruel, miserly, domineering paterfamilias. Whereas Ernest was forced to sing hymns before Theobald, Paul was forced to speak French to Charles. Just as Ernest was beaten when he mispronounced a word of a hymn, so was Paul punished for every error he made speaking French. When such slips occurred at dinner, Paul was refused permission to leave the table, even if he was sick. Time and again, Paul, who suffered from digestive problems all his life, was forced to stay put and vomit. In Paul's mind, this treatment was the basis of one of his trademark idiosyncrasies: to avoid punishment, he learned to say nothing.

After graduating with a first-class honors degree in electrical engineering from the Merchant Venturers' Technical College, an antecedent institution of the University of Bristol, Dirac won a small scholarship to Cambridge. Because Charles was unwilling to provide the remaining funds necessary for attendance, Dirac searched for a job instead. At the time, England was suffering from its worst economic slump since the Industrial Revolution, and Dirac joined the ranks of the two million unemployed. His luck turned when the University of Bristol arranged for him to pursue a mathematics degree free of charge. On the completion of those studies, Dirac was awarded a more substantial scholarship to Cambridge, which he entered in October 1923.

By 1923 the atom had been actively studied for over a quarter century. Two constituents of the atom were then known: the electron, discovered in 1897, and the proton, discovered in 1919. For more than two decades, beginning with Max Planck's derivation of blackbody radiation in 1900 and Einstein's explanation of the photoelectric effect in 1905, quantum theory had been used to elucidate atomic phenomena. An important confirmation of quantum physics was provided by Arthur Holly Compton when, in 1923, he published his observations of collisions between the photons of incident electromagnetic radiation and the free electrons of an irradiated target, thereby demonstrating the particle nature of light. And yet, when Dirac arrived in Cambridge, he considered atoms to be "very hypothetical things". It was his research supervisor, Ralph H. Fowler, a rising theoretical physicist in close contact with both Bohr and Born, who introduced atomic theory to Dirac.

Despite its successes, quantum theory was then in an unsatisfactory state: the Bohr-Sommerfeld quantization rules for the spectroscopy of the hydrogen atom were puzzling, and the theory was completely inadequate for the treatment of complex atoms. A radically new approach was needed, and Werner Heisenberg, then a Privatdozent at Göttingen, hesitantly supplied it. On July 9, 1925, he completed the final draft of the paper that would introduce quantum mechanics. Among his innovations was a multiplication of atomic quantities, the noncommutativity of which he considered to be "a significant difficulty". Before departing Göttingen for visits in Leiden and Cambridge, Heisenberg, worried that his paper was "crazy", deposited the manuscript with Born, who was to determine whether it was publication worthy.

On July 28 Heisenberg, still uncertain of the value of his recent work, lectured about the old quantum theory in Cambridge. He did, however, discuss his new approach with Fowler in private and, a few weeks later, mailed his English host the proofs of his article, which Born had, in the meantime, submitted. Fowler scribbled "What do you think of this?" on the first page and forwarded Heisenberg's paper to Dirac, who received it in Bristol early in September. Dirac's initial impression was that Heisenberg's work was "of no interest". It was in October, when he was back in Cambridge, that Dirac recognized the similarity between the Poisson bracket he remembered from classical dynamics and the commutator of two of Heisenberg's variables. Very quickly, Dirac reformulated Heisenberg's quantum mechanics, simplified the mathematics, clarified the break with classical theory, and significantly extended Heisenberg's results. The paper resulting from this work, "The fundamental equations of quantum mechanics", was received by the *Proceedings of the Royal Society* on November 7 and rushed to press. No amount of speed, however, could have overcome Born's head start.

As has been mentioned, Born was in possession of Heisenberg's manuscript within a few days of its completion. On July 15 he wrote Einstein that Heisenberg's latest work was "very mysterious, but certainly correct and profound." By July 19 Born had simplified Heisenberg's presentation so that the noncommutative multiplication formula could be expressed as the product of matrices. In particular, he conjectured that Heisenberg's quantum conditions could be written in terms of the position and momentum matrices as $qp - pq = \imath\hbar I$, where $\hbar$ is the reduced Planck constant and I is the unit matrix. Born recruited his former student, Pascual Jordan, to collaborate on the project; their joint paper, "Zur Quantenmechanik", was received by *Zeitschrift für Physik* on September 27, 1925. Nowadays we are well aware of the impetuses quantum mechanics imparted to Hilbert space theory and the theory of group representations. Less well known is that matrix algebra was not in the physicist's toolkit before the Born-Jordan paper, which defined matrix operations *ab initio*.

In September and continuing through October, Heisenberg joined Born and Jordan in writing a substantial extension, "Zur Quantenmechanik II", that treated systems with several degrees of freedom. It was received by *Zeitschrift für Physik* on November 16. Einstein described this paper as "extremely ingenious and, thanks to its complexity, sufficiently protected from disproof." It is therefore noteworthy that Dirac's short, direct paper not only contained all the essential results of both "Zur Quantenmechanik" and "Zur Quantenmechanik II" but also nipped the latter at the finish line. In a letter to Dirac dated November 20, Heisenberg acknowledged that, in some particulars, "Your results…go considerably further" than those of the Göttingen group. Heisenberg continued, "Your paper is also written really better and more concisely than our formulations."

By November 3, Pauli, who was brought into the loop through his correspondence with Heisenberg, was able to use the "Göttingen theory", also known as *matrix mechanics*, to derive Bohr's formulas for the Balmer series of the hydrogen atom. For many physicists, it would be the publication of Pauli's work that confirmed Heisenberg's quantum mechanics. Heisenberg himself seems to have maintained his reservations a bit longer. In his November 20 letter to Dirac that has already been quoted, he apprised Dirac of Pauli's success and yet allowed that there was still room to doubt the foundations on which Dirac and Pauli were building. In the few days that followed that letter, Heisenberg must have digested Dirac's work more completely, because on November 24 he informed Pauli, "An Englishman working with Fowler, Dirac, has independently re-done the mathematics for my work. …Now we really know that the theory is correct."

To reach readers not versed in classical mechanics and the old quantum theory, Farmelo employs simplifications that sometimes reduce the precision of his discussions. Farmelo asserts that, instead of using an electron's position as a variable, Heisenberg substituted a matrix with each entry representing the likelihood of the electron jumping between a pair of energy levels. The choice of "related to the likelihood" rather than "representing the likelihood" would have allowed Farmelo to avoid technical jargon and remain factual. (Or he might have added an endnote, as he would later do for Born's probabilistic interpretation of Erwin Schrödinger's wave function.) With regard to the attribution of matrices to Heisenberg, Farmelo has followed a common practice among physicists who consider Born's matrix reformulation to be comparatively unimportant. Having availed himself of this traditional fiction, Farmelo should have maintained it for consistency. Instead, on page 96, he (accurately) writes, "Heisenberg had never heard of matrices when he discovered the theory [of quantum mechanics]."

The onslaught of closely spaced discoveries did not abate in the immediate aftermath of the creation of quantum mechanics. Electron spin was announced by Samuel Goudsmit and George Uhlenbeck in November 1925. The first of the sequence of papers with which Schrödinger introduced *wave mechanics*, an alternative to matrix mechanics, appeared in March 1926. In April 1926 Dirac used his own quantum algebra to obtain a theoretical demonstration of Compton scattering. Because the formula he derived for the intensity of the scattered radiation yielded values that differed from the experimental data Compton had observed in 1922, Dirac was able to assert, "This is the first physical result obtained from the new mechanics that had not been previously known." Deferentially but unequivocally, he concluded, "The theory gives the correct law of variation of intensity with angle, and suggests that in absolute magnitude Compton's values are 25 per cent too small." Four months later, Compton wrote a letter to Mr. P. A. M. Dirac—nobody then knew the names represented by the initials—to inform him that measurements taken at the University of Chicago confirmed the new theory.

Given that Dirac had not yet written his Ph.D. thesis when he published his theory of Compton scattering, the effect for which Compton would receive Nobel recognition within a year, one might think this episode would interest a biographer. However, 1926 was a productive year for Dirac, and Farmelo confines his discussion to two other important papers from that year: Dirac's independent rediscovery of *Fermi-Dirac statistics* and Dirac's *transformation theory*, which introduced the Dirac δ-function and provided a formalism for passing between matrix mechanics and wave mechanics. When Farmelo first mentions "transformation theory" (page 114), he makes it seem to be a technique from classical mechanics. Because of this awkward handling, the indexer did not cite this page. It seems likely that the three subsequent references to transformation theory will be obscure to most of Farmelo's readers.

From the summer of 1926 through October 1927, the set of physicists who struggled to integrate special relativity and quantum theory included Heisenberg, Jordan, Pauli, Schrödinger, Wigner, Oskar Klein, Walter Gordon, Hendrik Kramers, Vladimir Fock, Yakov Frenkel, and Lev Landau. Some tried incorporating relativistic effects as perturbations of the nonrelativistic theory. Others sought a relativistic extension of Pauli's spin theory. Most concentrated on finding a relativistic wave equation. Schrödinger found such a candidate, the Klein-Gordon equation (as it would be called after its rediscovery) but left it unpublished because it conflicted with Sommerfeld's fine structure formula. In October 1927, at the Fifth Solvay Conference in Brussels, Dirac was dismayed to hear Bohr say that the Klein-Gordon equation did in fact describe the relativistic electron. Dirac thought otherwise: he inferred from his transformation theory that the desired relativistic wave equation would be *first* order in time. On his return to Cambridge after the conference, he devoted himself to the problem and solved it in about six weeks. By rediscovering Clifford algebras, he factored the second-order Klein-Gordon equation, thereby obtaining a relativistic first-order equation—the Dirac equation—for the electron.

The resulting paper, "The quantum theory of the electron", received by the *Proceedings of the Royal Society* on January 2, 1928, is Dirac's most celebrated work. His contemporaries considered his derivation of electron spin from nothing more than Lorentz invariance and transformation theory "a miracle". The ingenuity, originality, and beauty of Dirac's theory of the electron has impressed physicists of the highest rank ever since. Sin-Itiro Tomonaga, for example, after sketching the main steps [5], remarked, "We mortals are left reeling by this staggering outpouring of ideas from Dirac." "The quantum theory of the electron" was more than a watershed for physics: it also stimulated Richard Brauer, Hermann Weyl, John von Neumann, Oswald Veblen, Bartel van der Waerden, Valentine Bargmann, and Wigner to revive and develop the theory of spinors, which had received scant attention since its introduction in 1913 by Élie Cartan.

A feature of Dirac's theory that profoundly troubled physicists, Dirac included, is that it allowed for a positively charged electron. No such thing was expected in 1928. In fact, the proton and the electron were still the only known subatomic particles in May 1931 when Dirac predicted "a new kind of particle, unknown to experimental physics, having the same mass and opposite charge to an electron." He continued, "We may call such a particle an anti-electron." In the same remarkable paper, "Quantised singularities in the electromagnetic field", Dirac observed that a (magnetic) monopole, the magnetic analogue of an electron, is consistent with quantum mechanics. Moreover, he demonstrated that the existence of a single monopole anywhere in the universe would account for the quantization of electric charge. Dirac did not explicitly predict the monopole but noted, "One would be surprised if Nature had made no use of it."

In August 1932, six days before Dirac turned thirty, Carl Anderson announced the discovery at Caltech of an "easily deflectable positive". By March 1933 Anderson was confident that the particle he had detected was a positively charged electron, which he named the *positron*. Experiments at Cambridge demonstrated that Anderson's positron and Dirac's anti-electron were the same. These developments brought respectability to quantum mechanics in the eyes of the Nobel committee, which previously had regarded the theory as an abstraction with no substantiated utility. In November 1933 Heisenberg was awarded the delayed 1932 Nobel Prize, and Dirac and Schrödinger shared the 1933 prize. Hoping to avoid publicity, Dirac considered declining, but Rutherford convinced him that doing so would attract even more attention. Years later Dirac *did* refuse a knighthood, at least in part because he preferred the appellation "Mr. Dirac" to "Sir Paul".

The end of Dirac's bachelorhood began with a chance encounter he had with Margit Wigner Balázs in a Princeton restaurant in September 1934; she spotted him entering and had her brother Eugene, whom she was visiting, invite him to their table. From then until the successful conclusion of her campaign in December 1936, Margit pursued Dirac with the inexorability of a target-tracking missile. Or, as Farmelo states it, Margit "knew that she would have to take the initiative if she were to stir in him the first quantum of romance." With Dirac based in Cambridge and Margit in Budapest, the relationship was prosecuted in large part by correspondence. Farmelo was granted access to these letters and has quoted from them freely. Near the beginning of their postal exchanges, Dirac protested, "I am afraid I cannot write such nice letters to you—perhaps because my feelings are so weak and my life is mainly concerned with facts and not feelings." When the letters continued, Dirac advised, "You ought to think less of me." In response to Margit's complaint that he was not answering all her questions, Dirac constructed a three-column table with the headings: Letter number, Question, and Answer. His tabulated reply to the question "You know that I would like to see you very much?" was "Yes, but I cannot help it."

Even though Margit suggested that Dirac deserved "a second Nobel Prize, in cruelty", she persisted. When Dirac fully understood he was in the crosshairs of a determined woman, he warned, "You should know that I am not in love with you." Undeterred, Margit tried the telephone, a gambit that angered Dirac. Communication by letter surely ought to suffice, he wrote her. Margit resumed her courtship of Dirac with the methods allowed her. Dirac parried for some time but was outmatched. In December 1936 Dirac wrote to a mother figure in whom he sometimes confided, "I have felt very favourably inclined to [Margit] for several months, with occasional relapses, which get less and less as time goes on." He proposed a few days later and, in January 1937, married "his anti-particle, a woman almost opposite to him in character and temperament." Sometimes Farmelo's cute turns of phrase can be too cute.

In his first publication as a married man, Dirac announced his Large Numbers Hypothesis (LNH), which he stated as, "All very large dimensionless numbers which can be constructed from the important natural constants of cosmology and atomic theory are connected by simple mathematical relations involving coefficients of the order of magnitude unity." Dirac based this hypothesis on the approximate equalities $N_1 \approx N_2 \approx \sqrt{N_p} \approx 10^{40}$ that involve three large dimensionless numbers: N_1, the ratio of the radii of the observable universe and the electron; N_2, the electromagnetic-to-gravitational force ratio between the proton and electron; and N_p, the number of protons in the observable universe. Given the relationships $N_1 \propto t$ and $N_2 = e^2/(Gm_e m_p)$, where m_p and m_e are the masses of the proton and electron, e is the electron charge, and t is the age of the universe, Dirac proposed that Newton's gravitational constant satisfies $G \propto 1/t$. "Look at what happens to people when they get married," Bohr exclaimed when he first heard Dirac's idea.

The consensus is that LNH is wrong. Farmelo seems to assert the opposite when he writes, "Robert Dicke demonstrated that the large numbers hypothesis is a consequence of the fact that human life occurs after stars were formed and before they die. If the hypothesis were wrong, astronomers, and all other life forms, would not exist." The error in each of these sentences is that Farmelo has confused the apparently coincidental approximation $N_1 \approx N_2$ with LNH, Dirac's postulated explanation for the coincidence. What Dicke demonstrated was that the approximation $N_1 \approx N_2$ must hold in an epoch in which there is intelligent life capable of measuring N_1 and N_2. Given that we are here, at a time in cosmic history when heavy elements have formed, intelligent life has evolved, and the stars have not all died, we can deduce that t lies in a sufficiently narrow interval for the approximation $N_1 \approx N_2$ to hold.

Even though LNH did not pan out, it was a daring, innovative idea that prompted Dicke and others to conceive the anthropic arguments that were used for its refutation. In the late 1930s Dirac was no longer coming up with comparably original ideas in atomic physics, which was then in the doldrums. Comparing the years 1925–33, which he called his "exciting era", with the frustrating time into which he and other atomic physicists had fallen, Dirac noted, "It was very easy in those days for any second-rate physicist to do first-rate work; it is very difficult now for a first-rate physicist to do second-rate work." Inevitably, Dirac entered a final period during which he suffered from, as Farmelo puts it, "the fate of all aging theoretical physicists: his spirit was outliving his imagination." Dirac had pioneered quantum electrodynamics in 1927 but was unwilling to accept the renormalization program introduced by Richard Feynman, Julian Schwinger, and Tomonaga in the 1940s to overcome the impasse the subject had reached. Until his death, Dirac believed that renormalization theory was ugly, artificially contrived, and most likely wrong. At a conference in Princeton in 1946, Feynman served as leader of the discussion that followed Dirac's lecture. Feynman's criticism was blunt: Dirac was "on the wrong track". More than thirty-five years later Dirac was still on the same track when he wrote what would become his last paper, "The inadequacies of quantum field theory", which was published posthumously in 1987.

Dirac held the Lucasian Professorship of Mathematics at the University of Cambridge from September 1932 until he reached the statutory retirement age in September 1969. In 1971 he accepted a position at Florida State University, where he remained until his death. In his final years, depression seems to have darkened his outlook. The foundational work with which he introduced monopoles should have been a source of pride when, after 1974, Gerard 't Hooft and, independently, Alexander Polyakov showed that certain gauge theories also predict monopoles. Instead, Dirac described his theory as "just a disappointment". According to Big Bang cosmology, a very large number of monopoles were produced in the early universe. And yet, in the 1980s, in a letter to Abdus Salam, Dirac wrote, "I am inclined now to believe that monopoles do not exist."[1] This pessimism extended to his entire oeuvre. In 1983 Dirac declined an invitation to give a lecture at the University of Florida, stating, "I have nothing to talk about. My life has been a failure!"

Dirac's health began to wane in July 1984; he passed away on October 20, 1984. At Margit's prompting, the *Tallahassee Democrat* dispatched a young reporter to cover his funeral. Following the service, the journalist approached one of the lingering mourners, a Florida State physicist, and asked, *Who was this guy?* There are now several books that help answer his question. Two tribute volumes, [3] and [4], were published in 1987, and another, [1], was issued in 1998 on the occasion of Dirac's commemoration at Westminster Abbey. Kragh's superb scientific biography, [2], came out in 1990 and remains indispensable for its focus on Dirac's most important contributions to physics. Farmelo's well-written, engaging book, the first standard biography of Dirac, is a welcome alternative that can be recommended to readers who find the technical prerequisites of Kragh's work too forbidding.

As a biography, *The Strangest Man* is excellent; had its author's background not been disclosed, one might have guessed he was experienced in this genre. Everything has been done right. Pages have been given running heads, such as "October 1923–November 1924", that allow the reader to easily follow the timeline. Pages for the endnotes have heads, such as "Notes to Pages 25–28", that eliminate much of the usual fumbling associated with end-of-book searches. The index was surely compiled by an expert professional. Every lead has been run to ground. For example, when Dirac was a visiting professor at the University of Wisconsin in April and May 1929, he was interviewed by a popular sports columnist, Joseph "Roundy" Coughlin, for the *Wisconsin State Journal*. Or so it was believed. Copies of the article, which Kragh reproduced *in extenso*, are in the archives at Cambridge and Tallahassee. However, by examining the extant microfilm records for the two months Dirac spent in Madison, Farmelo concludes that the frequently anthologized interview "is an example of a probably apocryphal Dirac story that captures his behaviour so accurately that it somehow ought to be true." It is a pity that the following question and answer are not the genuine article:

> They tell me that you and Einstein are the only two real sure-enough high-brows and the only ones who can really understand each other. I won't ask you if this is straight stuff for I know you are too modest to admit it. But I want to know this—Do you ever run across a fellow that even you can't understand?
> "Weyl," says he.

It was Weyl who, along with Wigner, introduced group theory into quantum mechanics between 1926 and 1929. In a letter to Igor Tamm written in January 1929, Dirac acknowledged that Weyl's *Gruppentheorie und Quantenmechanik* was "not very easy", but only after he praised it as "[by] far the most connected account of quantum mechanics that has yet appeared." Perhaps further scrutiny of Dirac's perspective on group theory is warranted. Based on some remarks Dirac made later in 1929, Kragh concluded that Dirac held group theory to be not particularly appealing and "largely unnecessary for physical applications". Kragh added, more emphatically, "He always preferred to do without group theoretical methods." These statements are debatable. As early as 1928 Dirac suggested to Wigner the subject of the latter's 1939 paper concerning the unitary representations of the inhomogeneous Lorentz group. During Dirac's stay in Princeton in 1931, he studied "a good deal" of group theory with the intention of applying it to physics, as he wrote to Tamm in January 1932. Dirac discussed representation theory with Wigner in 1934 and 1935 and wrote a paper on the representations of the Lorentz group in 1945. More importantly for the subject, Dirac pointed one of his research students, Harish-Chandra, in that direction. Late in his life, Dirac apparently thought that "pathological" representations of the Lorentz group might provide a clue for the reformulation of quantum mechanics [3, p. 32]. It is not surprising that there is no discussion of group theory in Farmelo's nontechnical biography, but it is disappointing that nothing is said about Harish-Chandra other than that he was a bright student of Dirac. This passing mention is actually indexed under Lily Harish-Chandra, who is referenced more prominently than her husband due to her frank assessment of the mysterious union of Paul and Margit: "He gave her status and she gave him a life."

Here is a puzzle: Dirac and Wigner were both born in 1902, they were both experts in quantum theory at the highest level, and they were together in Princeton, Wigner's home base, for extended periods. Was there no interaction between them after 1937 when they became brothers-in-law? Farmelo's biography, like Kragh's, does not address this natural question. From Farmelo we

[1] *As of this writing, monopoles have not yet been detected. However, a new search (MoEDAL) has been under way at CERN's Large Hadron Collider since January 2011. A test array is currently taking data at the center-of-mass energy of 7 TeV. Preliminary results are expected to be analyzed beginning in 2012. The MoEDAL detector should start its official run with the full array deployed and the collider operating at its full design energy of 14 TeV in the spring of 2014.*

learn that Wigner remarked, "Feynman is a second Dirac, only this time human." We are also told that Margit's reaction to her brother's death was "Thank God the monster is dead." Some clues to the strained relationship between the Wigner siblings are offered, but Farmelo's readers will inevitably wonder, What went on in this tormented clan?

For those who have already been persuaded to read *The Strangest Man*, the time has come to put aside this review: there is a spoiler ahead.

Much of Farmelo's account contains tiresome family drama involving Dirac's despotic father and suffocating mother. The excessive detail seems to have been included to bolster the hypothesis that Dirac was doomed to strangeness by nurture rather than by nature. There is even a prologue that has no other purpose than to establish this behavioral theory. It is only after 420 pages that Farmelo comes clean: he does not subscribe to the rationale he previously seemed to have been documenting so thoroughly—it was nature, not nurture, after all. "Dirac was born to be a child of few words and was pitiably unable to empathise with others," he announces. Dirac, Farmelo finally reveals, had autism.

Ho-hum. Farmelo reached his conclusion by consulting with Simon Baron-Cohen, one of Cambridge's leading researchers into autism and Asperger's syndrome. In Marcus du Sautoy's *Symmetry*, reviewed in February's *Notices*, Baron-Cohen arrived at the same diagnosis for a living Fields Medalist. Mathematicians supposedly form a rich pool of subjects for such psychologists—according to a report discussed in du Sautoy's book, mathematics departments have a higher proportion of faculty members with Asperger's syndrome than any other university department. I do not know about that, but I do believe we have a high threshold for calling an individual strange. After reading Farmelo's biography, you will likely find Dirac's personality amiable and his character admirable. Chances are, you will not need to know whether Dirac was neurotypical. The strangest man might not even redline your strangeness meter.

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[1] Peter Goddard, *Paul Dirac: The Man and his Work*, Cambridge University Press, 1998.
[2] Helge Kragh, *Dirac, A Scientific Biography*, Cambridge University Press, 1990.
[3] Behram N. Kursunoglu and Eugene P. Wigner, *Reminiscences about a Great Physicist: Paul Adrien Maurice Dirac*, Cambridge University Press, 1987.
[4] J. G. Taylor (ed.), *Tributes to Paul Dirac*, Adam Hilger, 1987.
[5] Sin-Itiro Tomonaga, *The Story of Spin*, The University of Chicago Press, 1997.