The Genesis of Dirac's Relativistic Theory of Electrons

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Contents

PAUL DIRAC's relativistic quantum mechanics for electrons, published early in 1928, is one of the great landmarks in the history of science. According to NORWOOD RUSSELL HANSON, it has few predecessors in greatness: "Theoretical physics has rarely witnessed such a powerful unification of concepts, data, theories and intuitions: Newton and Universal Gravitation; Maxwell and Electrodynamics; Einstein and Special Relativity; Bohr and the hydrogen atom; these are the high spots before Dirac. From a chaos of apparently unrelated facts and ideas, Newton in his way, and now Dirac in his, built a logically powerful and conceptually beautiful physical theory ..."[1]

Despite the greatness of DIRAC's theory it has never been subject to historical analysis. This may be due to a tacit assumption by most historians of physics that the period suitable for historical investigations ends about 1927, when quantum mechanics became conceptually complete.[2] What lies beyond

[1] N. RUSSELL HANSON, *The Concept of the Positron*, Cambridge, 1963; p. 146.

[2] Of the standard works in the history of quantum physics, HUND's book is the only one which deals with the development of quantum mechanics after 1927, including a chapter on DIRAC's theory (F. HUND, *Geschichte der Quantentheorie*, Mannheim, 1967; pp. 180–195). Other contributions, due to VAN DER WAERDEN, MEHRA, BROMBERG and others, are mentioned below. More recently WEINBERG has pleaded for historical investigation into the development of quantum field theory. See S. WEINBERG, "The Search for Unity: Notes for a History of Quantum Field Theory," *Dædalus*, Fall 1977, 17–34.

this limit is left for physicists' memoirs and sketchy reviews. As a consequence, most of the recent development of physics has never been subjected to a satisfactory historical analysis. There is, however, no reason why recent science should not be included in the realm of history of science and be treated with the same historical care as is applied to the study of earlier periods. The present essay is meant as a contribution to this end.

1. Relativistic Quantum Mechanics, 1926

When ERWIN SCHRÖDINGER published his epoch-making series of papers on wave mechanics in the spring of 1926,[3] the core of his theory appeared to be hidden in two differential equations, which soon became known as the 'SCHRÖDINGER equations':[4]

$$\Delta\psi(\vec{r})+\frac{2m}{\hbar^2}(E-V)\psi(\vec{r})=0 \tag{1}$$

and

$$\left(-\frac{\hbar^2}{2m}\Delta+V\right)\psi(\vec{r},t)=i\hbar\frac{\partial}{\partial t}\psi(\vec{r},t), \tag{2}$$

appropriate to a particle with potential energy *V*. SCHRÖDINGER's theory, as published, did not include relativistic effects. It turns out, however, that SCHRÖDINGER originally designed his wave mechanics to be a genuinely relativistic theory.[5] The main reason why he did not publish his relativistic version lay in its failure to account correctly for the energy levels of the hydrogen atom. Thus in his first communication on wave mechanics he reported, albeit in words only, that he had worked out the energy eigenvalues of the relativistic hydrogen atom, but with a disappointing result: "Das relativistische Keplerproblem, wenn man es genau nach der eingangs gegebenen Vorschrift durchrechnet [führt] merkwürdigerweise auf *halbzahlige Teil*quanten (Radial- und Azimuthquant)."[6] With this remark SCHRÖDINGER referred to what was known as the fine structure of the hydrogen spectrum.

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[3] E. SCHRÖDINGER, "Quantisierung als Eigenwertproblem," Erste Mitteilung, *Ann. d. Phys.*, **79** (1926), 361–376 (received 27 January); Zweite Mitteilung, *ibid.*, 489–527 (received 23 February); Dritte Mitteilung, *Ann. d. Phys.*, **80** (1926), 437–490 (received 10 May); Vierte Mitteilung, *Ann. d. Phys.*, **81** (1926), 109–139 (received 21 June).

[4] Equation (1), the eigenvalue equation, first appeared in Erste Mitteilung, while equation (2), the time-dependent equation, first appeared in Vierte Mitteilung. In what follows, the term "SCHRÖDINGER equation" refers, if nothing else is mentioned, to the eigenvalue equation.

[5] *Cf.* J.U. GERBER, "Geschichte der Wellenmechanik," *Arch. Hist. Exact Sci.* **5** (1969), 349–416; L.A. WESSELS, "Schrödinger's Route to Wave Mechanics," *Stud. Hist. Phil. Sci.*, **10** (1979), 311–340. A more complete history of the relativistic SCHRÖDINGER theory can be found in H. KRAGH, "On the History of Early Wave Mechanics, with Special Emphasis on the Role of Relativity," (mimeographed) Roskilde University Centre, 1979.

[6] Erste Mitteilung, p. 372.

In 1915–16 ARNOLD SOMMERFELD had explained the hydrogen spectrum through quantization of the relativistic BOHR atom. According to SOMMERFELD's result, which was found to be in exact agreement with experiments, the energy levels are expressed by two quantum numbers:

$$E_{n,k}=\frac{m_0c^2}{\sqrt{1+\frac{\alpha^2}{(n-k-\sqrt{k^2-\alpha^2})^2}}}-m_0c^2. \tag{3}$$

In this expression α denotes the fine structure constant $\frac{e^2}{\hbar c}$, n the principal quantum number (with values 1, 2, ...) and k the azimuthal quantum number (with values 1, 2, ..., n); the integer $n-k$ was also known as the radial quantum number, n_r. An expansion in powers of α^2 gives the approximation

$$E_{n,k}=-\frac{m_0e^4}{2\hbar^2}\left[1+\frac{\alpha^2}{n^2}\left(\frac{n}{k}-\frac{3}{4}\right)\right]. \tag{4}$$

SCHRÖDINGER's unpublished treatment of the relativistic hydrogen atom was based on the relativistic version of the eigenvalue equation (1), which is[7]

$$\Delta\psi+\frac{1}{\hbar^2c^2}\left[\left(E+\frac{e^2}{r}\right)^2-m_0^2c^4\right]\psi=0. \tag{5}$$

If solved according to the methods applied to (1), this equation *almost* gives equation (3), the only, but of course crucial, difference being that $n-k$ and k are to be replaced by $n-k+\frac{1}{2}$ and $k-\frac{1}{2}$, respectively. This means that agreement with experiment is destroyed, and hence it accounts for SCHRÖDINGER's remark. It was only in the fourth communication that SCHRÖDINGER took up an examination of the relativistic theory, and even then "nur mit der allergrössten Reserve." What SCHRÖDINGER called "die vermutliche relativistisch-magnetische Verallgemeinerung" of his equation (2) was obtained from the relativistic HAMILTON-JACOBI equation for an electron in an electromagnetic field. The result was[8]

$$\Delta\psi-\frac{1}{c^2}\frac{\partial^2\psi}{\partial t^2}-\frac{2ie}{\hbar c}\left(\frac{\varphi}{c}\frac{\partial\psi}{\partial t}+\vec{A}\cdot\nabla\varphi\right) +\frac{e^2}{\hbar^2c^2}\left(\varphi^2-\vec{A}^2-\frac{m_0^2c^4}{e^2}\right)\psi=0, \tag{6}$$

where $\vec{A}$ and φ are the electromagnetic potentials. The eigenvalue equation (5), which SCHRÖDINGER did not put in print, is easily derived from equation (6): For an electron subject to a COULOMB field, but not to an external magnetic

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[7] This equation appears in SCHRÖDINGER's research notebook entitled "H-Atom, Eigenschwingungen" (*Archive for the History of Quantum Physics* (AHQP), no. 41, section 5), which presumably was written about New Year's Day, 1925.

[8] Vierte Mitteilung, p. 133.

field, $\psi(\vec{r}, t)$ is replaced by the periodic function $\psi(\vec{r}) \exp\left(\frac{i}{\hbar} Et\right)$, and equation (5) comes out.

Equations (5) and (6) are the basic formulae of SCHRÖDINGER's relativistic wave mechanics. They were worked out by several physicists[9] in 1926 and are known today as the KLEIN-GORDON equations (in what follows, the KG equations). Priority in publication belongs to OSKAR KLEIN, who proposed the KG equations in April 1926.[10] The most detailed and lucid treatment of relativistic wave mechanics is probably that of VLADIMIR FOCK, who calculated the relativistic KEPLER motion according to wave mechanics.[11] FOCK naturally arrived at the result that SCHRÖDINGER had already described in words.

The most straightforward derivation of the KG equations results if we apply to the classical-relativistic equation of motion the substitutions

$$\vec{p} \rightarrow \frac{\hbar}{i} \nabla, \quad E \rightarrow i\hbar \frac{\partial}{\partial t},$$

used by SCHRÖDINGER. For example, this was done by LOUIS DE BROGLIE[12] who thus arrived at equations (5) and (6). Consider, for the sake of later discussion, the classical Hamiltonian for a free particle:

$$H^2 = c^2 \vec{p}^2 + m_0^2 c^4. \tag{7}$$

With the substitutions appropriate to wave mechanics this becomes the KG equation for a free electron:

$$\Delta\psi - \frac{1}{c^2} \frac{\partial^2 \psi}{\partial t^2} - \left(\frac{m_0 c^2}{\hbar}\right)^2 = 0. \tag{8}$$

In 1926 the KG theory was worked out by several authors and was applied to several problems. Of particular interest is the interpretation of the relativistic scalar field ψ. In his fourth communication SCHRÖDINGER adopted an electromagnetic interpretation, which was carried over into the relativistic domain by KLEIN and by WALTER GORDON.[13] SCHRÖDINGER found that with the substitutions

$$\rho = \psi\psi^* \quad \text{and} \quad \vec{j} = \frac{\hbar}{2mi} (\psi^* \nabla\psi - \psi \nabla\psi^*), \tag{9}$$

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[9] The equations were found independently by V. FOCK, L. DE BROGLIE, W. PAULI and VAN DUNGEN & TH. DE DONDER. For references, see KRAGH, *op.cit.* (note 5).

[10] O. KLEIN, "Quantentheorie und fünfdimensionale Relativitätstheorie," *Zs. f. Phys.*, **37** (1926), 895–906 (received 28 April 1926).

[11] V. FOCK, "Zur Schrödingerschen Wellenmechanik," *Zs. f. Phys.*, **38** (1926), 242–250 (received 5 June 1926).

[12] L. DE BROGLIE, "Remarques sur la nouvelle mécanique ondulatoire," *Comp. Rend.*, **183** (1926), 272 (19 July 1926).

[13] W. GORDON, "Der Comptoneffekt nach der Schrödingerschen Theorie," *Zs. f. Phys.*, **40** (1927), 117–133 (received 29 September 1926). O. KLEIN, "Elektrodynamik und Wellenmechanik vom Standpunkt des Korrespondenzprinzips," *Zs. f. Phys.*, **41** (1927), 407–442 (received 6 December 1926).

these quantities satisfied a continuity equation, and if multiplied by e, they were therefore interpreted as the scalar charge density and the vector current density. GORDON and KLEIN found that while the current density was not altered by a relativistic treatment, the charge density became

$$\rho_{KG} = -\frac{\hbar}{2mc}\left(\psi^* \frac{\partial \psi}{\partial t} - \psi \frac{\partial \psi^*}{\partial t}\right). \tag{10}$$

The formulae (9) and (10) refer to free electrons only.

In 1926, and in part of 1927, the KG theory was the subject of numerous papers and was considered, at least among those concerned with wave mechanics, to be the correct and natural generalization of SCHRÖDINGER's theory. Appearing in a beautiful symmetric manner, which automatically secures LORENTZ invariance, it appealed instinctively to many theoretical physicists. SCHRÖDINGER adopted the KG formulation in further works on the COMPTON effect[14] and in an attempt to formulate the laws of conservation of energy and momentum in the framework of his ψ field.[15] The trouble with the KG theory was, however, that although it had a mathematical and aesthetic appeal, its range of applicability was limited when compared with, for instance, the usual wave mechanics. As already mentioned, it failed to account for the fine structure of spectra, and of course it was of no use for the anomalous ZEEMAN effect or other doublet phenomena.

The KG theory was not well regarded by many of the leading quantum theorists, and in particular the German physicists affiliated with matrix mechanics. This lack of confidence was strengthened during 1926 as more came to be known about the general principles of quantum mechanics. WOLFGANG PAULI was constantly occupied in trying to formulate a sound relativistic quantum mechanics; in fact he had derived the KG equations in April, 1926.[16] But then in July he told WENTZEL that he doubted the validity of the KG theory. PAULI's doubts stemmed from his having recognized the formal difficulties of the KG theory, which, he argued, was not consonant with the equivalence between wave mechanics and matrix mechanics. "Deshalb habe ich das Vertrauen zur Differentialgleichung [eq. (5)] vollständig verloren!"[17] PAULI, in collaboration with JOHANN KUDAR, continued to investigate the relativistic version of SCHRÖDINGER's theory, but he remained sceptical toward the KG approach. Half a year later he told SCHRÖDINGER: "Von der relativistischen

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[14] E. SCHRÖDINGER, "Über den Comptoneffekt," *Ann. d. Phys.*, **82** (1927), 257–264.

[15] E. SCHRÖDINGER, "Der Energieimpulssatz der Materiewellen," *Ann. d. Phys.*, **82** (1927), 265–272.

[16] Letter, PAULI to JORDAN, 12 April 1926. Reprinted and commented upon in B.L. VAN DER WAERDEN, "From Matrix Mechanics and Wave Mechanics to Unified Quantum Mechanics," pp. 276–293 in J. MEHRA (ed.), *The Physicist's Conception of Nature*, Dordrecht, 1973. PAULI's scientific correspondence up to 1929 has recently been published. See A. HERMANN, K.V. MEYENN & V.F. WEISSKOPF (eds.), *Wolfgang Pauli. Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg u.a., Band I: 1919–1929*, New York 1979.

[17] Letter, PAULI to WENTZEL, 5 July 1926 (AHQP).

Gleichung 2-Ordnungs mit den vielen Vätern glaube ich aber nicht, dass sie der Wirklichkeit entspricht."[18]

If the "straightforward" KG generalization did not prove feasible, how could relativity then be worked into quantum mechanics? A possible way was to include relativistic effects as perturbations or corrections to the non-relativistic equation. This approach was followed by PAULI and by other physicists. In his important work on the hydrogen spectrum[19] PAULI deduced the BALMER terms from the Göttingen mechanics and tried to include a first-order relativistic correction so as to account also for the fine structure; but, as he admitted, mathematical difficulties in calculating the time-average of r^{-2} had prevented him from carrying out the programme.[20] PAULI, who in the winter 1925–26 was still hostile to the idea of spin, wanted to solve the problem of fine structure by a purely relativistic development of quantum mechanics, without any initial assumption about spinning electrons. He realized, however, that a hypothesis combining relativity and spin might possibly solve the problem of fine structure.

PAULI's programme was further developed by WERNER HEISENBERG & PASCUAL JORDAN, who in March 1926 succeeded where PAULI had failed.[21] Conceiving the effects of spin and relativity to be given by two perturbation terms added to the usual Hamiltonian, they were able to obtain SOMMERFELD's fine structure formula, although only in its first-order approximation (4).[22] The

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[18] Letter, PAULI to SCHRÖDINGER, 22 November 1926 (AHQP). Also KUDAR to DIRAC, 21 December 1926 (AHQP): "Herr Pauli, mit dem ich darüber vielfach diskutiert habe, betrachtet ... die relativistische Wellengleichung *zweiter* Ordnung sehr misstrauisch ... es scheint uns aber, dass diese Diskrepanz mit formalmathematischer Untersuchung nicht erledigt werden kann." PAULI's confidence in the KG approach seems to have wavered. In December 1926 he commented on SCHRÖDINGER's attempt to express the law of energy-momentum conservation in terms of wave mechanics and wrote that he had "in letzter Zeit viel über die relativistischen Gleichungen nachgedacht ..." The letter continues: "Ich glaube jetzt ganz sicher, dass Du mit Deinem Standpunkt, dass diese Gleichungen sinnvoll sind und dass der Operatorkalkül verallgemeinert werden muss, Recht hast. Denn ich bin auf verschiedene Eigenschaften der relativistischen Gleichungen und der Ausdrücke für Ladungsdichte u. Stromdichte gekommen, die mein Vertrauen zu diesere sehr gestärkt haben." PAULI to SCHRÖDINGER, 12 December 1926 (AHQP).

[19] W. PAULI, "Über das Wasserstoffspektrum vom Standpunkt der neuen Quantenmechanik," *Zs. f. Phys.*, **36** (1926), 336–363 (received 17 January 1926). Translated in B.L. VAN DER WAERDEN (ed.), *Sources of Quantum Mechanics*, New York, 1967.

[20] The additional energy due to the relativistic correction is given by $\Delta E = (2m_0c^2)^{-1}\{E_0^2 + 2e^2E_0\overline{(r^{-1})} + e^4\overline{(r^{-2})}\}$, where E_0 is the energy of the undisturbed orbit and the bars denote mean values taken over the undisturbed path.

[21] W. HEISENBERG & P. JORDAN, "Anwendung der Quantenmechanik auf das Problem der anomalen Zeemaneffekte," *Zs. f. Phys.*, **37** (1926), 263–277 (received 16 March 1926).

[22] As pointed out many years later by VAN VLECK, the agreement of HEISENBERG & JORDAN's calculations with experiment was rather coincidental. HEISENBERG & JORDAN used a two-dimensional model for the hydrogen atom to work out the mean values of r^{-2} and r^{-3}. However, a rigorous calculation based on two dimensions gives the wrong answer to the energy correction. It was only due to "a happy combination of empiricism and intuition" that HEISENBERG & PAULI applied the correct formulae for calculation of

HEISENBERG-JORDAN approach was made possible only because relativity was *added* as a correction. Therefore it could only give an approximation for the exact SOMMERFELD formula. Furthermore, the effects of relativity and spin were introduced in a rather *ad hoc* manner; the correct doublet formulae, for instance, were obtained by taking advantage of the newly discovered THOMAS factor.[23] The effects were not *explained*, *i.e.* the doublet phenomena were not deduced from quantum mechanics. It is historically interesting that HEISENBERG & JORDAN at first attempted to formulate the new quantum mechanics in a proper relativistic way, and that they hoped to account thus for the spin hypothesis as well.[24] However, they were not able to carry out their programme.

Thus in 1926 the position of quantum mechanics with respect to relativity was much as follows. On one hand, the KG theory was a possible solution of the problem. Being a genuinely relativistic quantum theory, it failed to account satisfactorily for the doublet phenomena, and it seemed difficult to reconcile with the general scheme of quantum mechanics. On the other hand, the HEISENBERG-JORDAN approach accounted for the doublet phenomena and was, of course, in agreement with quantum mechanics. But, not genuinely relativistic, it was to be regarded only as a provisional answer. What was sought was a LORENTZ invariant equation which, without extra assumptions, could give the exact SOMMERFELD formula and, if possible, the spin as well.

The approach taken by PAULI, HEISENBERG and JORDAN was also followed by GREGOR WENTZEL in Munich,[25] who was actually able to derive SOMMERFELD's formula (3) or rather a formula similar to (3). WENTZEL was unable to decide about the normalization of the denominator in (3), *i.e.* whether the quantum numbers were integers or half-integers. That WENTZEL's theory was not the answer sought, was further demonstrated by his arriving at the exact fine structure formula through a first-order relativistic approximation, without taking spin into consideration. Neither PAULI[26] nor SCHRÖDINGER was impressed by WENTZEL's result. Comparing the matrix mechanical attempts with

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the mean values, in fact only valid for three dimensions. See J.H. VAN VLECK, "Central Fields in Two Vis-a-Vis Three Dimensions: an Historical Divertissement," pp. 26–37 in W.C. PRICE *et al.* (eds.), *Wave Mechanics: the First Fifty Years*, New York, 1973.

[23] L.H. THOMAS demonstrated while at BOHR's institute in February 1926 that a relativistic treatment of a precessing electron gives the correct doublet separation, missed by a factor 2 in the original spin hypothesis. The THOMAS factor adds to the Hamiltonian a term of the form $\frac{Z^2}{2m_0^2c^2}\frac{1}{r}\frac{dV}{dr}(\vec{k}\cdot\vec{s})$, or, in the case of the hydrogen atom, $\frac{e^2}{2m_0^2c^2}\frac{1}{r^3}(\vec{k}\cdot\vec{s})$.

[24] "Freilich glaub ich doch auch, dass die endgültige Lösung noch tiefer liegt und wesentlich mit einer vierdimensional-invarianten Formulierung der Quantenmechanik zu tun hat. ... Ich versuche hier jetzt mit Jordan eine vier-dimensionale Formulierung der Quantenmechanik und bin neugierig, was dabei herauskommt." HEISENBERG to GOUDSMIT, 9 December 1925 (AHQP). Quoted from D. SERWER, "*Unmechanischer Zwang:* Pauli, Heisenberg, and the Rejection of the Mechanical Atom, 1923–1925," *Hist. Stud. Phys. Sci.*, **8** (1977), 189–258; p. 252.

[25] G. WENTZEL, "Die mehrfach periodischen Systeme in der Quantenmechanik," *Zs. f. Phys.*, **37** (1926), 80–94 (received 27 March 1926).

[26] *Opt.cit.* (note 17).

his own, SCHRÖDINGER felt that the latter, though not the final answer, was superior: "Dirac (Proc. Roy. Soc.) und Wentzel (Z. f. Phys.) rechnen Seiten lang am Wasserstoffatom, Wentzel auch relativistisch, wobei im Endresultat bloss *das* fehlt, was einen eigentlich interessiert: nämlich, ob 'halbzahlig' oder 'ganzzahlig' zu quanteln ist! So findet Wentzel also zwar 'genau die Sommerfeldsche Feinstrukturformel', aber aus dem angegebenen Grunde ist das Resultat für den Erfahrungsvergleich ganz wertlos. – In der Wellenmechanik ergibt die relativistische Behandlung, die ebenso einfach ist, wie die klassische, unzweideutig halbzahliges Azimuth- und Radialquant. (Ich habe die Rechnung seiner Zeit nicht publiziert, weil dies Ergebnis mir eben zeigte, dass noch etwas fehlt; dieses etwas ist sicher der Gedanke von Uhlenbeck und Goudsmidt.)"[27]

Many of the attempts to construct a relativistic quantum mechanics employed the viewpoint of *general* relativity. For instance, this was KLEIN's programme. The grandiose attempt to create an all-embracing theory out of quantum mechanics, electromagnetism and general relativity turned out to be a blind alley, but at the time it was pursued eagerly by many physicists; not only KLEIN but also FOCK, DE BROGLIE, TH. DE DONDER, FRITZ LONDON, LÉON ROSENFELD and NORBERT WIENER.[28] In hindsight, the general relativistic approach had a speculative and formalistic mark, which probably helped to turn interest away from questions regarding the quantum-logical consistency of the more simple KG equation.

2. P.A.M. Dirac: Methodology and Cultural Background

Originally DIRAC studied engineering, not physics.[29] Under the strained economic situation in postwar England he wished to earn a living so he did not dare engage in such an economically unsafe field as pure physics. After having graduated as an electrical engineer in 1921, he was unable to find a job; the economic depression had caused mass unemployment also among engineers. Only then did he enter Cambridge University, where eventually he began his glorious career in theoretical physics. DIRAC's early training in engineering had a profound impact on his general scientific outlook and in particular upon his understanding of the role played by mathematics in physical theory. DIRAC himself appraised this impact as follows: "It seemed to me that if one worked with approximations there was an intolerable ugliness in one's work, and I very much wanted to preserve mathematical beauty. Well, the engineering training which I received did teach me to tolerate approximations, and I was able to see that even theories based on approximations could sometimes have a consider-

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[27] Letter, SCHRÖDINGER to LORENTZ, 6 June 1926. Reprinted in K. PRZIBRAM (ed.), *Schrödinger, Planck, Einstein, Lorentz. Briefe zur Wellenmechanik*, Wien, 1963.

[28] For references and details, see KRAGH, *op.cit.* (note 5).

[29] See P.A.M. DIRAC, "Recollections of an Exciting Era," pp. 109–146 in C. WEINER (ed.), *History of Twentieth Century Physics*, New York, 1977. In what follows, this article will be referred to as *Recollections*. See also J. MEHRA, "'The Golden Age of Theoretical Physics': P.A.M. Dirac's Scientific Work from 1924 to 1933," pp. 17–60 in A. SALAM & E.P. WIGNER (eds.), *Aspects of Quantum Theory*, Cambridge, 1972.

able amount of beauty in them. ... I think that if I had not had this engineering training, I should not have had any success with the kind of work that I did later on, because it was really necessary to get away from the point of view that one should deal only with results which could be deduced logically from known exact laws which one accepted, in which one had implicit faith."[30]

However, this rather empiricist-pragmatist approach to theoretical physics, which DIRAC learned as an engineering student, was only one aspect of his method. DIRAC was indeed a mathematical physicist and always had a strong inclination towards mathematical-deductive approaches. He often expressed his high confidence in the value of inner consistency and mathematical beauty in physical theories at the expense of an empiricist-inductivist approach. "... it is more important to have beauty in one's equations than to have them fit experiment. ... if one is working from the point of view of getting beauty in one's equations, and if one has really a sound insight, one is on a sure line of progress."[31] Later he developed his views about the heuristic role of mathematical beauty in physical research into metaphysical considerations about a mathematical quality, which he regarded as being inherent in the scheme of nature.[32] Logical deductions of consequences "from known exact laws which one accepted, in which one had implicit faith" (above), played a substantial role in DIRAC's way of doing physics and was also significant in the creation of his theory of 1928. What DIRAC's early training in engineering taught him was that one should not confide blindly in mathematics. Even approximate theories can stand up to the criterion of mathematical beauty.

The theory of relativity made a distinct impression upon intellectual life in postwar Europe. As explained by DIRAC: "It is easy to see the reason for this tremendous impact. We had just been living through a terrible and very serious war. ... Then this terrible war came to an end rather suddenly. The result was that everyone was sick and tired of the war. Everyone wanted to forget it. And then relativity came along as a wonderful idea leading to a new domain of thought. It was an escape from the war."[33] DIRAC was attracted by the general interest in relativity and quickly became an expert in the theory of relativity. His chief inspiration came from ARTHUR EDDINGTON's writings, which no doubt influenced his entire way of thinking; there are close links between EDDINGTON's philosophy of science and DIRAC's physical theories.[34] Much of his time as a student was occupied with relativistic problems. "There was a sort of general problem which one could take, whenever one saw a bit of physics

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[30] *Recollections*, pp. 112–113.

[31] P.A.M. DIRAC, "The Evolution of the Physicist's Picture of Nature," *Sci. Amer.*, **208** (1963), 45–53; p. 47.

[32] P.A.M. DIRAC, "The Relation between Mathematics and Physics," *Proc. Roy. Soc. (Edinburgh)*, **59** (1938–39), 122–129.

[33] *Recollections*, p. 110. See also V.V. RAMAN, "Relativity in the Early Twenties: Many-Sided Reactions to a Great Theory," *Indian Journal of History of Science*, **7** (1972), 119–145.

[34] See J. MERLEAU-PONTY, *Philosophie et théorie physique chez Eddington*, Paris 1965, pp. 111–115. See also H. KRAGH, "Methodology and Philosophy of Science in Paul Dirac's Physics," Roskilde University Centre, 1979.

expressed in a non-relativistic form, to transcribe it to make it fit with special relativity. It was rather like a game, which I indulged in at every opportunity, and sometimes the result was sufficiently interesting for me to be able to write up a little paper about it."[35] Throughout his career, DIRAC remained strongly committed to relativity. A quantum theory which did not agree with relativity was unsatisfactory, and should only be granted a temporary validity until a better, *viz.* relativistic, theory was found. From his very first encounter with HEISENBERG's matrix mechanics in September, 1925, DIRAC objected to its non-relativistic form and immediately tried to rewrite it in such a way as to make it compatible with relativity, although at that time it was in vain.[36]

While DIRAC thought that quantum mechanics had to be formulated ultimately in complete agreement with the theory of relativity, his background in engineering warned him against too comprehensive and too grand a synthesis, even one that might have appealed to him because of its mathematical beauty. He was ready to attack the problem piecemeal, by improving and criticizing existing theories. This was opposed to the KG approach, which led to an ambitious claim that relativity and quantum theory had been unified at one stroke. To KLEIN, who tried to incorporate general relativity as well as electromagnetic theory in quantum mechanics, DIRAC once remarked that the main reason for his failure was that he tried to solve too many problems at the same time.[37] DIRAC has often stressed this moral, to which he attributed a good deal of his own success: "One should not try to accomplish too much in one stage. One should separate the difficulties in physics one from another as far as possible, and then dispose of them one by one."[38]

3. Relativity in Dirac's Earlier Works

Before his final attack on relativistic quantum mechanics, DIRAC touched on the subject at various occasions, as was indeed compatible with his outlook. Neither of these works, published as they were before 1928, attempted directly to relativize quantum mechanics, but they show how DIRAC considered the matter before the break-through late in 1927.

Even in 1924, before the advent of quantum mechanics, DIRAC had considered quantum theory and relativity in a way which was, in retrospect, characteristic of his research programme. Dissatisfied with the non-invariant form of BOHR's frequency relation, $\Delta E = h\nu$, he showed that it could in fact be written as an equation in four-vectors, $\Delta E_\mu = h\nu_\mu$, and thus be made to conform with relativity.[39]

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[35] *Recollections*, p. 120.

[36] *Recollections*, p. 120.

[37] O. KLEIN, "Ur mit liv i fysiken," pp. 159–172 in *Svensk Naturvetenskap 1973;* p. 164.

[38] P.A.M. DIRAC, "Methods in Theoretical Physics," pp. 21–28 in *From a Life in Physics*, IAEA Bulletin, 1969; p. 22.

[39] P.A.M. DIRAC, "Note on the Doppler Principle and Bohr's Frequency Condition," *Proc. Cambr. Phil. Soc.*, **22** (1924), 432.

In its early stages quantum mechanics was developed by two camps, one devoted to matrix mechanics, the other to wave mechanics. DIRAC was much closer to the matrix version of quantum mechanics, developed by the Göttingen physicists. DE BROGLIE's ideas appealed instinctively to him because of their bold unification of quantum theory and the theory of relativity. But, being trained in an entirely different tradition and with an entirely different outlook, he could not accept them as physically sound. "Although I appreciated very much the beauty of de Broglie's work, I could not take his waves seriously",[40] DIRAC has recalled, no doubt expressing an attitude common to most physicists at the time. When SCHRÖDINGER's theory appeared, DIRAC had already developed his own version of quantum mechanics, sometimes known as q-number algebra, and had applied it successfully to a number of problems. Consequently he felt SCHRÖDINGER's theory to be not only unnecessary but also a step backward in the understanding of quantum phenomena. It was only gradually that he gave up his hostility to SCHRÖDINGER's ideas and recognized wave mechanics as being a valuable supplement to matrix mechanics and q-number mechanics.

In 1926 DIRAC published two papers on the COMPTON effect, a subject which invites a relativistic treatment. In the first of these papers, *Relativity Quantum Mechanics with an Application to Compton Scattering*,[41] DIRAC argued from the theory of relativity that the time, t, should be treated like other dynamic variables in quantum theory and thus be considered as a q-number. Then DIRAC showed that in classical Hamiltonian theory, $-E$ and t are conjugate variables; interpreted in the language of quantum mechanics, this implies that they must obey the same basic commutation rules as do other conjugate variables: $tE-Et=i\hbar$.

In this first paper DIRAC did not refer to SCHRÖDINGER's new ideas. He worked "on the basis of Heisenberg's matrix mechanics, which was first modified to be in agreement with the principle of relativity."[42] But in his second paper on the subject,[43] published half a year later, DIRAC recognized the advantages of wave mechanics. He then found that "a more natural and more easily understood method of obtaining the matrices is provided by Schrödinger's wave mechanics."[44] The fact that $-E$ and t are conjugate variables, he transcribed into operator notation as follows: $E=i\hbar\frac{\partial}{\partial t}$. SCHRÖDINGER's wave equation in the relativistic case he wrote essentially as[45]

$$\left(m_0^2 c^2-\frac{E^2}{c^2}+\vec{p}^2\right)\psi=0, \tag{11}$$

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[40] *Recollections*, p. 118.

[41] *Proc. Roy. Soc. (London)*, A**111** (1926), 405–423 (received 29 April 1926).

[42] P.A.M. DIRAC, "The Compton Effect in Wave Mechanics," *Proc. Cambr. Phil. Soc.*, **23** (1926), 500–507 (received 8 November 1926); p. 500.

[43] *Ibid.*

[44] *Ibid.*, p. 500.

[45] In order to deal with the COMPTON effect, DIRAC included a periodic term in the p_y component, referring to the disturbance caused by the incident radiation.

with $\vec{p}=\frac{\hbar}{i}\nabla$ and $E=i\hbar\frac{\partial}{\partial t}$. This is the KG equation (8). DIRAC was able to deduce COMPTON's energy and momentum formulae and to calculate the intensity of the emitted COMPTON radiation in satisfactory agreement with experiments. At that time GORDON and KLEIN[46] had already treated the COMPTON effect working strictly on the basis of relativistic wave mechanics, *i.e.* starting from the KG expressions for charge density and current density. This was not the approach of DIRAC, who stated that the wave equation (11) was "used merely as a mathematical help for the calculation of the matrix elements, which are then interpreted in accordance with the assumptions of matrix mechanics."[47] DIRAC was not converted to the wave mechanics camp. But his attitude towards wave mechanics versus matrix mechanics was, in contrast with some of the Göttingen physicists', undogmatic and pragmatic, an attitude which may have helped him later when he came to create relativistic electron theory.

DIRAC's publications were in general hard to understand, and the papers on the COMPTON effect were no exception. In the autumn of 1926 ALBERT EINSTEIN visited PAUL EHRENFEST and GEORGE UHLENBECK in Leiden, and they all sought to understand DIRAC's paper. EHRENFEST wrote to DIRAC asking him a number of questions to clarify the content of the paper. "Verzeihen Sie bitte, wenn einige der Fragen auf ganz groben Mißverständnissen beruhen sollten. Aber wir können trotz aller Anstrengung nicht durchdringen!"[48] One of the questions EHRENFEST asked, was why DIRAC wrote the Hamiltonian equation as $\frac{E^2}{c^2}-\vec{p}^2=m_0^2c^2$ and not as $m_0c^2\left(1+\frac{\vec{p}^2}{m_0^2c^2}\right)^{\frac{1}{2}}=E$. "Does it make a difference?" EHRENFEST asked. Incidentally, this was the crucial point in DIRAC's later relativistic electron theory, where it *did* make a difference; but this fact could scarcely have been in DIRAC's mind at the time.[49]

DIRAC first considered the KG equation in the form (8), in August 1926, in a discussion of the quantum statistics of a BOSE-EINSTEIN gas.[50] These considerations were followed up in his important work on the quantum theory of radiation, from which the later quantum electrodynamics developed.[51] Starting

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[46] *Op.cit.* (note 13). See also SCHRÖDINGER, *op. cit.* (note 14). The COMPTON effect was also treated wave mechanically. See G. BECK, "Comptoneffekt und Quantenmechanik," *Zs. f. Phys.*, **38** (1926), 144–148; G. WENTZEL, "Zur Theorie des Comptoneffekts," **43** (1927), 1–8 and 779–787.

[47] *Op.cit.* (note 42), p. 507.

[48] Letter, EHRENFEST to DIRAC, 1 October 1926 (AHQP).

[49] What may have been in DIRAC's mind is that the E^2 expression is truly LORENTZ invariant, while the E expression is not. DIRAC's answer is not filed in the AHQP.

[50] P.A.M. DIRAC, "On the Theory of Quantum Mechanics," *Proc. Roy. Soc. (London)*, A**112** (1926), 661–677 (received 26 August 1926); p. 670.

[51] P.A.M. DIRAC, "The Quantum Theory of the Emission and Absorption of Radiation," *Proc. Roy. Soc. (London)*, A**114** (1927), 243–265 (received 2 February 1927). The paper is subjected to a historical analysis in J. BROMBERG, "Dirac's Quantum Electrodynamics and the Wave-Particle Equivalence," pp. 147–157 in WEINER (ed.), *op.cit.* (note 29). See also R. JOST, "Foundation of Quantum Field Theory," pp. 61–77 in SALAM & WIGNER (eds.), *op.cit.* (note 29).

from his understanding that the problem of applying quantum mechanics to radiation processes is connected with the "serious difficulty in making the theory satisfy all the requirements of the restricted principle of relativity",[52] DIRAC stated that "it will be impossible to answer any one question completely without at the same time answering them all."[53] Even if DIRAC in 1927 was not able to develop a completely satisfying, *i.e.*, a strictly relativistic quantum electrodynamics, he managed to proceed in the right direction on a largely non-relativistic basis and to develop his ideas about quantization of photons in a most fruitful way. DIRAC's approach toward a quantum electrodynamics was further developed by JORDAN & PAULI, who in December 1927 succeeded in formulating relativistically invariant quantization principles for the electromagnetic field.[54] They did not, however, consider individual electrons on this occasion.

The concepts evolved in radiation theory were the basis for DIRAC's treatment of dispersion theory.[55] This work did not give results not found in the previous treatments of dispersion,[56] but it tackled the problem in an entirely different way, relying neither on correspondence arguments nor on wave mechanical charge and current densities. In order to deduce the KRAMERS-HEISENBERG dispersion formula, DIRAC started from the relativistic Hamiltonian for an electron in an electromagnetic field:[57]

$$H=c\sqrt{m_0^2c^2+\left(\vec{p}-\frac{e}{c}\vec{A}\right)^2}+e\varphi. \tag{12}$$

Apparently not knowing what to do with this function, he used it only in its classical approximation:

$$H=c\sqrt{m_0^2c^2+\vec{p}^2}+e\varphi-\frac{e}{m_0c}\vec{p}\cdot\vec{A}+\frac{e^2}{2m_0c^2}\vec{A}^2. \tag{13}$$

As this survey shows, DIRAC's occupation with relativistic quantum mechanics before his theory of 1928 was rather provisional. He frequently ran into

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[52] DIRAC, *op.cit.* (note 51), p. 243.

[53] DIRAC, *op.cit.* (note 51), p. 244.

[54] P. JORDAN & W. PAULI, "Zur Quantenelektrodynamik ladungsfreier Felder," *Zs.f. Phys.*, **47** (1928), 151–173 (received 7 December 1927).

[55] P.A.M. DIRAC, "The Quantum Theory of Dispersion," *Proc. Roy. Soc. (London)*, A**114** (1927), 710–728 (received 4 April 1927). Commented upon in JOST, *op. cit.* (note 51).

[56] See H.A. KRAMERS & W. HEISENBERG, "Über die Streuung von Strahlen durch Atome," *Zs. f. Phys.*, **31** (1925), 681–708; M. BORN, W. HEISENBERG & P. JORDAN, "Zur Quantenmechanik, II," *Zs. f. Phys.*, **35** (1926), 557–615. Both of these papers are translated in VAN DER WAERDEN, *op.cit.* (note 19). Dispersion according to wave mechanics was treated by SCHRÖDINGER in his fourth communication, *op.cit.* (note 3); a relativistic treatment was given by KLEIN, *op.cit.* (note 13), and by V. BURSIAN in "Notiz zu den Grundlagen der Dispersionstheorie von E. Schrödinger," *Zs. f. Phys.*, **40** (1927), 708–713.

[57] In DIRAC's original paper there is a misprint in his equation 11, appearing on p. 716, where a plus sign is omitted.

problems where a relativistic treatment was needed, and he clearly recognized that relativity had to be worked completely into the quantum mechanical formalism. But in 1926–27 he hesitated to use the unifying approach proposed in the KG theory. It is characteristic in DIRAC's approach that every time he faced a relativistic problem, he was content to apply an approximation.

4. Spin and Quantum Mechanics

In the events leading up to DIRAC's relativistic theory of electrons, the introduction of spin was of decisive importance. Even if a theory of spin was not yet worked out, it was inevitably clear in 1926 that spin and relativity had to be integrated if quantum theory could account for the peculiarities in spectroscopy. In his third communication on wave mechanics, SCHRÖDINGER thus stated that "den Zeemaneffekt... erscheint mir unlöslich geknüpft an eine korrekte Formulierung des *relativistischen* Problems in der Sprache der Wellenmechanik, weil bei vierdimensionaler Formulierung das Vektorpotential von selbst dem skalaren ebenbürtig an die Seite tritt."[58] Having just become acquainted with the spin hypothesis of SAMUEL GOUDSMIT & UHLENBECK, SCHRÖDINGER conjectured that this was the missing link in accounting for the discrepancy between SOMMERFELD's formula and the result obtained by relativistic wave mechanics.[59]

As the understanding of the general quantum mechanical formalism advanced during 1926, the problem of including spin and relativity in quantum mechanics remained essential. It was widely accepted, not only that spin and relativity were intimately related, but also that spin should find its explanation in relativity, either by the special or the general theory. In the summer of 1926, SOMMERFELD asked EINSTEIN: "Sie versuchen wohl schon, das Spinnende Elektron (das unentbehrlich ist!) in die allgem[eine] Relativitätstheorie einzuordnen? Das wäre der größte Triumph der Rel[ativitäts-]Th[eorie]."[60] However, EINSTEIN never followed SOMMERFELD's request. He replied: "Ich geben Ihnen gerne zu, daß an dem spinnenden Elektron nicht zu zweifeln ist. Aber einstweilen ist wenig Hoffnung, seine Notwendigkeit *von innen heraus* zu begreifen."[61] Not all physicists shared EINSTEIN's reservations. LONDON, for instance, proposed to interpret spin in KLEIN's five-dimensional quantum theory, by identifying the canonical conjugate of its fifth dimension with the spin angular momentum.[62] The first steps in the programme of a quantum mechanical understanding of spin were taken by PAULI and CHARLES DARWIN in the spring of 1927. These attempts gave only a provisional answer to the

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[58] *Op.cit.* (note 3), p. 439.

[59] *Ibid.*, p. 440.

[60] Letter, SOMMERFELD to EINSTEIN, 5 August 1926.

[61] Letter, EINSTEIN to SOMMERFELD, 21 August 1926. Both excerpts are quoted from A. HERMANN (eds.), *Albert Einstein, Arnold Sommerfeld: Briefwechsel*, Basel 1968.

[62] F. LONDON, "Über eine Deutungsmöglichkeit der Kleinschen fünfdimensionalen Welt," *Die Naturwissenschaften*, **15** (1927), 15–16 (dated 17 November 1926).

problem, and they failed to combine spin and relativity. Nevertheless, they were important steps towards the creation of a relativistic spin theory.

At the same time there were attempts to relate spin to the KG theory. E. GUTH from Vienna argued that the charge and current expressions of the relativistic theory reproduce the gyromagnetic ratio correctly.[63] And A. CARELLI from Naples started from the KG equation and tried to introduce the spinning electron in an electromagnetic interpetation of wave mechanics.[64] However, the attempts to connect spin with the second-order relativistic theory turned out to be unfruitful.

In order to make use of GOUDSMIT & UHLENBECK's hypothesis in quantum mechanics, PAULI[65] considered a SCHRÖDINGER wave function depending not only on space coordinates but also on the so-called spin coordinates. Since the component of spin in any direction, say s_z, can attain but two values, $+\frac{1}{2}\hbar$ or $-\frac{1}{2}\hbar$, PAULI considered a two-component wave function: $\psi(q, s_z) = (\psi_\alpha(q), \psi_\beta(q))$. The index α refers to $s_z = +1$ and β to $s_z = -1$, measured in terms of $\hbar/2$. PAULI then stated the energy eigenvalue problem by two coupled equations of the type

$$H\left(\frac{\hbar}{i}\frac{\partial}{\partial q}, \vec{s}\right)\psi = E\psi, \tag{14}$$

where ψ is either ψ_α or ψ_β, and where H and $\vec{s}$ are operators. To apply these equations to physical problems, PAULI had first to find the explicit form of the spin operators. This he did in close agreement with HEISENBERG & JORDAN's approach. It yielded what was soon to be known as the PAULI matrices:

$$\vec{\sigma} = \begin{pmatrix} 0 & 1 \\ 1 & 0 \end{pmatrix}, \quad \begin{pmatrix} 0 & -i \\ i & 0 \end{pmatrix}, \quad \begin{pmatrix} 1 & 0 \\ 0 & -1 \end{pmatrix} = \frac{2}{\hbar}\vec{s}. \tag{15}$$

By writing the Hamiltonian termwise

$$H = H_0 + H_1 + H_2, \tag{16}$$

PAULI applied (14) to simple physical systems such as the hydrogen atom in an external magnetic field, $\mathscr{H}$. In (16) H_0 is the normal, unperturbed energy (the energy operator) while H_1 is the energy contribution due to the magnetic field and to the relativistic variability of mass; H_2 is the energy contribution due to the electron's spin. In quantum mechanical language, PAULI wrote the Hamiltonian as

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[63] E. GUTH, "Spinning Electrons and Wave Mechanics," *Nature*, **119** (21 May 1927), 744.

[64] A. CARELLI, "The Spinning Electron in Wave Mechanics," *Nature*, **119** (2 April 1927), 492–493.

[65] W. PAULI, "Zur Quantenmechanik des magnetischen Elektrons," *Zs. f. Phys.*, **43** (1927), 601–623 (received 3 May 1927). For comments on this and other contributions to spin theory, see B.L. VAN DER WAERDEN, "Exclusion Principle and Spin," pp. 199–244 in M. FIERZ & V.F. WEISSKOPF (eds.), *Theoretical Physics in the Twentieth Century*, New York 1960.

$$H=\left[-\frac{\hbar^2}{2m_0}\Delta-\frac{e^2}{r}\right]+\left[-\frac{1}{2m_0c^2}\left(E_0+2E_0\frac{e^2}{r}+\frac{e^4}{r^2}\right)\right.$$
$$\left.-i\mu_0\vec{\mathscr{H}}\cdot(\vec{r}\times\nabla)\right]+\left[\frac{\hbar^2}{4}\cdot\frac{e^2}{m_0^2c^2}\cdot\frac{1}{r^3}\cdot\frac{1}{i}(\vec{k}\cdot\vec{s})+\mu_0\vec{\mathscr{H}}\cdot\vec{s}\right], \tag{17}$$

where E_0 is the eigenvalue of H_0, μ_0 the magnetic moment of the electron and $\vec{k}$ and $\vec{s}$ are the operators of orbital angular momentum and spin. PAULI thus included spin effects (the last bracket), while relativistic effects were only considered in their first-order approximation (first term in second square bracket). In fact PAULI approached the matter just as had HEISENBERG & JORDAN before him, and with the same result, namely (17), as he himself recognized. The new thing was the explicit spin matrices (15). Because of the equivalence between PAULI's theory and the one of HEISENBERG & JORDAN, there was no need to solve the eigenvalue equation. It was known beforehand to yield equation (4) above.

In his theory of spin PAULI partly adopted a wave mechanical approach, at the same time stressing the need for complying with the formalism of general quantum mechanics. PAULI was, in DARWIN's words, "more disposed to regard the wave theory as a mathematical convenience and less than a physical reality."[66] In this respect, PAULI's approach closely resembled DIRAC's. In contrast with PAULI, DARWIN was devoted to wave mechanics. In a series of papers beginning in 1927[67] he extended SCHRÖDINGER's theory to cover spin phenomena, and showed that the anomalous ZEEMAN effect, as well as other spectroscopic puzzles, could in this way be reproduced. DARWIN's programme was "to proceed by empirically constructing a pair of equations to represent the fine-structure of the hydrogen spectrum."[68] This empirical approach, which he shared with PAULI, consisted in expressing the Hamiltonian by a sum of terms including spin effects and first-order relativistic effects, *i.e.*, equation (16). DARWIN solved his wave equation by a lengthy mathematical analysis based on spherical harmonics and showed that it gave the correct result, equation (4). His wave mechanical translation of UHLENBECK & GOUDSMIT's original ideas was thus equivalent to HEISENBERG & JORDAN's treatment. The main difference between DARWIN's work and PAULI's lay in their different interpretations of the spin wave function. DARWIN interpreted the two-component wave function as the electron's "vector wave", to be understood in analogy with a light wave. PAULI objected to DARWIN's interpretation, and pointed out that only by introducing spin as an extra coordinate in the two scalar wave functions, could spin be properly incorporated in the quantum mechanical transformation

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[66] C.G. DARWIN, "The Electron as a Vector Wave," *Proc. Roy. Soc. (London)*, A**116** (1927), 227–253 (received 30 July 1927); p. 227.

[67] C.G. DARWIN, "The Electron as a Vector Wave," *Nature*, **119** (1927), 282–284 (19 February 1927); "The Zeeman Effect and Spherical Harmonics," *Proc. Roy. Soc. (London)*, A**115** (1927), 1–19 (received 23 March 1927); *op.cit.* (note 66); "Free Motion in Wave Mechanics," *Proc. Roy. Soc. (London)*, A**117** (1927), 258–293 (received 25 October 1927).

[68] *Op.cit.* (note 66), p. 230.

theory. Despite their complete equivalence, DARWIN's and PAULI's theories brought about a very different impact on the further development of physics. Historically, PAULI's contribution turned out to be the most important of the two. At the time when the spin theories emerged, each physicist received them in accord with his own stand regarding quantum mechanics. Wave mechanicists preferred DARWIN's approach, while most others were in favour of PAULI's way of presenting the problems.[69]

As far as the one-electron problem is concerned, neither PAULI's theory nor DARWIN's gave results not already delivered by the matrix mechanics of HEISENBERG & JORDAN. In particular, their theories did not materially change the situation regarding relativity. Both PAULI and DARWIN were most aware of this deficiency. "Die hier formulierte Theorie," PAULI admitted, is "nur provisorisch anzusehen, da man von einer endgültigen Theorie verlangen muss, dass sie von vornherein relativistisch invariant formuliert ist und auch die höheren Korrektionen zu berechnen erlaubt."[70] PAULI tried to calculate a second-order relativistic approximation,[71] but was not able to obtain a better approximation than (4). The problems, PAULI imagined, were probably caused by too primitive a model of the electron, visualized as an infinitesimal magnetic dipole. He thought that a more sophisticated model, including quadrupole and higher effects, would lead to a LORENTZ-invariant equation. But, PAULI admitted, "es ist mir ... bisher nicht gelungen, zu einer relativistisch invarianten Formulierung der Quantenmechanik des magnetischen Elektrons zu gelangen."[72]

DARWIN's theory faced the same difficulties as PAULI's: "... the deduction of the Sommerfeld formula for separation ought to be exact and not merely a first approximation. In view of these considerations we cannot regard the theory as at all complete – as, indeed, is true of the whole interconnection of the quantum theory with relativity ..."[73] DARWIN tried to formulate the wave equations relativistically by means of the KG equations, but he had to fall back on a null-approximation (neglecting terms in c^{-2}) and to admit that the KG theory could not work out correctly.

So, in effect, DARWIN's and PAULI's ingenious theories did not contribute much to the still more delicate problem of integrating quantum mechanics with

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[69] SCHRÖDINGER's and BORN's reactions about how the two spin theories were received may be typical: "You can easily guess that I am *very* much interested in your work on the spinning electron and that I infinitely prefer *your* view of a vector-wave to that of my friend Pauli, whose formalism I am hardly able to understand." (Letter, SCHRÖDINGER to DARWIN, 4 October 1927; AHQP.) "I was much interested in your attempt to interpret the electron as a vector wave, but I do not believe that this is the right way. Pauli has shown (not yet published) that the spinning electron can be described by an operator of a particular kind, and he finds two differential equations (coupled, of course) instead of the one of Schrödinger. But he finds great difficulty in generalizing his results to take account of the relativity modifications." (Letter, BORN to DARWIN, 7 April 1927; AHQP.)

[70] PAULI, *op.cit.* (note 65), p. 619.

[71] According to DARWIN, *op.cit.* (note 66), footnote on p. 236.

[72] PAULI, *op.cit.* (note 65), p. 619.

[73] *Op.cit.* (note 66), p. 253.

the theory of relativity. Rather, they suggested that the integration should be sought in an entirely different direction.

5. Toward a Relativistic Theory

If any particular item is to be singled out as crucial to DIRAC's creation of relativistic quantum mechanics, it is the general understanding of quantum mechanics as it emerged in 1926–27. In the present context, there is no need to discuss the development of "general quantum mechanics", of which MAX JAMMER has given an extensive review.[74] The core of the new understanding was the interpretation in terms of probability, originally proposed by MAX BORN, and the statistical transformation theory. DIRAC was a leading contributor in both fields, to which he directed a large share of his intellectual resources in 1926–27. His works on general methods for physical interpretation of quantum mechanics, DIRAC recalls, gave him "more pleasure than any of the other papers which I have written on quantum mechanics either before or after."[75] In the spring of 1927, the unified formulation of quantum mechanics, as given by the transformation theory, was completed. It comprised the then existing four theories (HEISENBERG's matrix mechanics, DIRAC's q-number algebra, SCHRÖDINGER's wave mechanics, BORN & WIENER's operator formalism) into one general scheme of great logical beauty. During his work with the transformation theory, DIRAC advanced towards a firm conviction concerning the basic truth of general quantum mechanics; agreement with this formalism became the ultimate criterion for the soundness of theories in the quantum domain. DIRAC stressed, for example, that the successful development of his radiation theory was possible only because of the general transformation theory.[76] DIRAC's confidence in the general theory of quantum-mechanical transformations was rooted in a general idea of the nature of physical laws. In 1930 he expressed this philosophy as follows: "The formulation of these laws requires the use of the mathematics of transformations. The important things in the world appear as the invariants (or more generally the nearly invariants, or quantities with simple transformation properties) of these transformations. ... The growth of the use of transformation theory, as applied first to relativity and later to the quantum theory, is the essence of the new method in theoretical physics."[77]

For transformation theory, however, it was crucial that the quantum equations of motion be of first order in $\partial/\partial t$. It was thus irreconcilable with theories of the KG type and appeared to jeopardize the unification of quantum mechanics and the theory of relativity. DIRAC recognized the problem, which

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[74] M. JAMMER, *The Conceptual Development of Quantum Mechanics*, New York, 1966; ch. 6.

[75] *Recollections*, p. 137.

[76] DIRAC, *op.cit.* (note 51), p. 245.

[77] P.A.M. DIRAC, *The Principles of Quantum Mechanics*, Oxford, 1930; p. V. For an appraisal of this philosophy, see KRAGH, *op.cit.* (note 34).

became a source of growing dissatisfaction to him. "There was ... a real difficulty in making the quantum mechanics agree with relativity. That difficulty bothered me very much at the time, but it did not seem to bother other physicists, for some reason which I am not very clear about."[78] As mentioned in Section 1, also PAULI and KUDAR recognized these difficulties.[79] Their hesitation concerning the KG equation were known to DIRAC, for whom KUDAR reviewed the matter as seen from Hamburg. I will give a summary of the two Hamburg physicists' considerations about relativistic quantum mechanics, as they were known to DIRAC at the end of 1926.

In contrast to the non-relativistic wave equation, the KG equation is not self-adjoint, it was pointed out. Its eigenfunctions are, furthermore, not subject to the usual orthogonality relation. This leads to the result that the so-called rule of multiplication from matrix mechanics is not generally satisfied. In GORDON's treatment of the COMPTON effect, for instance, the coordinate matrices X_{lm} were expressed wave mechanically as $\frac{1}{e}\int \rho x\, dx$, where ρ is the relativistic charge density given by equation (10); but then $(X^2)_{lm}$ is not given by the matrix multiplication rule, *i.e.*, it is different from $\sum_\alpha X_{\alpha l} X_{m\alpha}$. These objections show that it is difficult to interpret the KG theory in the same framework as the SCHRÖDINGER theory, and hence that the KG theory cannot be put in the same basket as the various non-relativistic theories, *i.e.*, general quantum mechanics. If the second-order KG equation is unreliable, one might be willing to consider its corresponding first-order version, for example an equation of the form

$$\hbar c \sqrt{m_0^2 c^4 - \Delta}\, \psi = (E - V)\psi. \tag{18}$$

This equation is, in PAULI's words, "etwas mathematisch unbequem, aber an sich sinnvoll und auch selbstadjungiert."[80] The mathematical difficulties are connected with the interpretation of the square root operator; if it is understood as an expansion in series, it leads to a differential equation of infinite order.

When DIRAC received KUDAR's letter, leading physicists were well aware of the general structure of quantum mechanics. They knew, for instance, that if the

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[78] P.A.M. DIRAC, *Directions in Physics*, New York, 1978; p. 14.

[79] PAULI to WENTZEL, 5 July 1926, and 5 December 1926. KUDAR to SCHRÖDINGER, 20 November 1926, and 8 November 1926. PAULI to SCHRÖDINGER, 22 November 1926. KUDAR to DIRAC, 21 December 1926. (All in AHQP.) When KUDAR was still in Budapest, he worked on the relativistic SCHRÖDINGER equation, but apparently without recognizing its conceptual difficulties. See J. KUDAR, "Zur vierdimensionalen Formulierung der undulatorischen Mechanik," *Ann. d. Phys.*, **81** (1926), 632–636; "Schrödingersche Wellengleichung und vierdimensionale Relativitätsmechanik," *Phys. Zeits.*, **27** (1926), 724; see also the letter, KUDAR to SCHRÖDINGER, 8 September 1926 (AHQP). It was only when KUDAR left Budapest to become PAULI's assistant in Hamburg, that he conceived the KG theory in a new and critical light. DIRAC's reply to KUDAR's letter of 21 December is not filed in the AHQP archive, but apparently he proposed to introduce relativistic time in the matrices. See the letter to DIRAC of 20 January, written by GORDON & KUDAR.

[80] Letter, PAULI to SCHRÖDINGER, 22 November 1926 (AHQP).

eigenfunctions of $H\psi=E\psi$ conform to the requirement of orthogonality, then the Hamiltonian must necessarily be expressed as a self-adjoint operator. JORDAN[81] emphasized this point and used the opportunity to consider the KG equation. However, instead of dismissing the second-order relativistic equation on the ground that it is not self-adjoint in the usual sense, he proposed an extended definition of self-adjointness; with this extension, JORDAN showed that the KG equation is self-adjoint. It seems that JORDAN did not fully realize how incompatible the KG equation is with general quantum mechanics. This insight was limited to very few physicists, first of all to PAULI and DIRAC.

I turn now to DIRAC's view concerning the position of spin in quantum mechanics. The whole spin problem was intimately linked to spectroscopy as the concept of spin was introduced in order to supply a rational understanding of doublet structures and similar anomalies. It is a small wonder, then, that DIRAC was not occupied with spin in his earlier works. For he was outside the spectroscopic camp of Continental physics and had no interest in the entangled spectroscopic problems which led to the introduction of spin. It was only in 1927 that DIRAC took up the problem, apparently inspired by PAULI. According to the memoirs of DIRAC,[82] he and PAULI discussed in January, 1927, in Copenhagen how spin could be incorporated into quantum mechanics. It is characteristic of DIRAC's attitude toward physics that his interest in spin was not derived from empirical problems, but in his endeavour to apply the ideas of general quantum mechanics also to spin. DIRAC states that he and PAULI got the idea of the three spin variables, which was published in PAULI's paper, independently of each other.[83] In DIRAC's lecture notes from the autumn of 1927,[84] we can understand how he conceived the spin theories.

As one would expect from DIRAC's outlook, he definitely favoured PAULI's method to DARWIN's, whose procedure is not justified, it was shown, according to "general quantum theory." In contrast, DIRAC highly praised PAULI's method: "It consists in abandoning from the beginning any attempt to follow the classical theory. One does not try to take over into the quantum theory the classical treatment of some model, which incorporates the empirical facts, but takes over the empirical facts directly into the quantum theory. The method provides a very beautiful example of the general quantum theory, and shows that this quantum theory is no longer completely dependent on analogies with the classical theory, but can stand on its own feet." This is another example of DIRAC's unshakable confidence in general quantum mechanics, the guiding principle for all of his research from 1926 onwards. The lecture notes contain a

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[81] P. JORDAN, "Über eine neue Begründung der Quantenmechanik," *Zs. f. Phys.*, **40** (1927), 809–838 (received 18 December 1926); pp. 818–821.

[82] *Recollections*, p. 138.

[83] "I believe I got these variables independently of Pauli, and possibly Pauli also got them independently of me." *Recollections*, p. 138. That DIRAC should independently have obtained the spin variables in 1927, is not indicated by the literature. Neither in private correspondence nor in the papers of PAULI and DARWIN have I found evidence in support of this claim.

[84] P.A.M. DIRAC, "Lectures on Modern Quantum Mechanics," Manuscript (AHQP); undated, but probably from October 1927.

thorough treatment of PAULI's spin theory, which DIRAC interpreted slightly differently: He showed that PAULI's interpretation of the σ matrices does not fully agree with the transformation theory. In PAULI's method, σ_x can, for instance, operate on a single wave function, say ψ_β, and then give $\sigma_x \psi_\beta = \psi_\alpha$. But DIRAC argued that σ_x can only meaningfully be applied to a complete wave function with an α and a β component; so instead of $\sigma_x \psi_\beta = \psi_\alpha$ one gets $(\sigma_x \psi)_\beta = \psi_\alpha$. This difference in interpretation does not, however, call for revision of any of PAULI's results. DIRAC also exemplified the spin theory with a detailed investigation of the hydrogen atom, including magnetism and first-order relativistic effects. Using a simplified and more lucid version of DARWIN's method, he solved PAULI's eigenvalue equation (*i.e.* (14) and (17)) and obtained the approximate SOMMERFELD formula and LANDÉ's g-factor for doublets.

The lecture notes prove that in October 1927 or earlier, DIRAC was closely acquainted with the spin theory. He was thus aware of its deficiencies, *viz.* its semi-empirical eigenvalue equation and its failure to agree better than to the first order with the accepted theory of the hydrogen spectrum. At that time he was not yet on the track of a relativistic quantum mechanics which could solve these problems. In any case, there is no evidence in the lecture notes of an approach towards a linear wave equation.

In 1927 the hydrogen spectrum was no longer regarded a crucial puzzle, which required an entirely new relativistic quantum mechanics. After the incorporation of spin into quantum mechanics, the fine structure was accounted for in one of two ways: either by the PAULI-DARWIN theory or by introducing an extra half integral angular momentum quantum number in the KG equation. When LLEWELLYN THOMAS found the kinematical explanation of the two-factor which had haunted UHLENBECK & GOUDSMIT's original hypothesis, HENDRIK KRAMERS reported to RALPH KRONIG: "Since further Heisenberg and Pauli have succeeded in finding the quantum mechanical mean values of $[r^{-2}]$ and $[r^{-3}]$ for a Keplerian orbit, we know now that the fine structure of the Hydrogen spectrum and the theory of the doublets in X-ray spectra is in the finest order ...".[85] Regarding the wrong fine structure of the KG theory, SCHRÖDINGER stated in October 1927 that this was no longer so problematical because of the spin: "... Ce fait ne nous effraie plus autant que quand il se presenta pour la première fois."[86] DE BROGLIE made a similar point: "Il ne faut pas cependant s'exagérer la portée de cet échec; en realité on est sûr aujourd'hui que la théorie de structure fine de Sommerfeld est insuffisante, et que les phénomenes de dédoublement des raies dans les séries optiques et Röntgen sont en rapport étroit avec l'état magnétique interne de l'atome et avec les effets Zeeman anormaux."[87]

Even if spectroscopic facts were not considered sufficient to press for a radical revision of existing theories, there were other reasons for dissatisfaction with current relativistic quantum theory. As mentioned, there was the incom-

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[85] Letter, KRAMERS to KRONIG, 26 February 1926 (AHQP). The terms r^{-2} and r^{-3} have been inserted by me.

[86] E. SCHRÖDINGER, "La mécanique des ondes," pp. 185–206 in *Électrons et photons, Rapports et discussions du cinquième conseil de physique*, Paris, 1928.

[87] L. DE BROGLIE, *La mécanique ondulatoire*, Paris, 1928; p. 46.

patibility between general quantum mechanics and LORENTZ invariance. The fifth Solvay Congress, held in Brussels 24–29 October 1927,[88] could have provided a forum for a discussion of these problems. But it did not. Although most of the key figures of quantum physics were gathered together, the issue was not touched upon at the conference. Indeed, relativistic quantum mechanics was treated in the reports given by DE BROGLIE and SCHRÖDINGER,[89] who both discussed the KG equation. However, in their contributions there is no trace of dissatisfaction rooted in recognition of the logical inconsistencies of the KG formulation with general quantum mechanics. This is not surprising, as SCHRÖDINGER and DE BROGLIE represented dissident views as to the interpretation of quantum mechanics. SCHRÖDINGER continued in his attempt to develop a quantum mechanics based on classical fields. And in Paris, DE BROGLIE developed his own alternative, later to be known as the theory of double solution; this causal interpretation of quantum mechanics in terms of pilot-waves was the subject of his lecture at the Solvay conference.

DIRAC did not take part in the discussion following either DE BROGLIE's address or SCHRÖDINGER's, and he did not mention the KG theory at all. But he commented at length on BOHR's report. DIRAC's chief interest was in the logical development of general quantum mechanics, which eventually supplied him with the key to the problem of relativity in quantum mechanics. At the Solvay Congress this problem certainly occupied DIRAC's mind, as illustrated by an episode which he recalled as follows: "During the interval before one of the lectures, Bohr came up to me and asked me: 'What are you working on now?' I tried to explain to him that I was working on the problem on trying to find a satisfactory relativistic quantum theory of the electron. And then Bohr answered that that problem had already been solved by Klein. I tried to explain to Bohr that I was not satisfied with the solution of Klein, and I wanted to give him reasons, but I was not able to do so because the lecture started just then and our discussion was cut short. But it rather opened my eyes to the fact that so many physicists were quite complacent with a theory which involved a radical departure from the basic laws of quantum mechanics, and they did not feel the necessity of keeping to these basic laws in the way that I felt."[90]

After his return from Brussels, DIRAC concentrated his work on the relativistic theory of the electron. Within two months he had resolved the whole matter.[91] When DARWIN went to Cambridge at Christmas time in 1927, he was completely surprised to learn about DIRAC's new theory, and reported to BOHR: "I was at Cambridge a few days ago and saw Dirac. He has now got a

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[88] See the report *op.cit.* (note 86). See also J. MEHRA, *The Solvay Conferences on Physics*, Dordrecht, 1975.

[89] SCHRÖDINGER, *op.cit.* (note 86) and L. DE BROGLIE, "La nouvelle dynamique des quanta," pp. 105–132 in the same volume.

[90] *Op.cit.* (note 78), p. 15. A similar version appears in *Recollections*, p. 141. DIRAC's accounts of the event are not entirely concordant. In the AHQP interview conducted by THOMAS KUHN and also in a conversation with JAGDISH MEHRA (of 28 March 1969; *op.cit.* (note 29). p. 44), DIRAC recalls his interrupted talk with BOHR as having taken place in Copenhagen.

[91] AHQP interview.

completely new system of equations for the electron which does the spin right in all cases and seems to be 'the thing'. His equations are first order, not second, differential equations! He told me something about them, but I have not yet even succeeded in verifying that they are right for the hydrogen atom."[92] The result of DIRAC's thinking was *The Quantum Theory of the Electron*,[93] received by the editor of the *Proceedings* on the second day of the new year, 1928.

6. The Genesis of the Theory

So far as I can learn, there is no *prima facie* source material, such as letters or manuscripts, which provides a historically reliable analysis concerning the birth of DIRAC's theory. Apart from the published paper itself, the historian is given only secondary sources, of which DIRAC's recollections are the most important. These statements date from many years after the discovery and are thus to be used with some caution; recollections of events forty years back in time are likely to contain distortions and inaccuracies. The final product of DIRAC's thinking, the article of 1928, cannot be used freely as valid evidence as to how the theory was created. The history of science provides many instances in which the published presentation of a scientific process fails to reflect the path of discovery. Relying on the end product will only distort the historical reconstruction towards an inductivist and too logical pattern. This general warning to the historian of science is also exemplified by DIRAC's writings. In his early works on quantum mechanics, DIRAC constantly used ideas of projective geometry as a visualizing means, but when published, the results were always translated into the more easily understood language of mathematical analysis.[94] Due to DIRAC's particular way of working, it may, however, be more legitimate to use the published paper as historical evidence in the case here considered. DIRAC preferred to produce a paper in one piece of concentrated effort, and he often wrote it down continuously, in his meticulous handwriting. When the draft was written up, it needed few, if any, corrections. "Most of the papers that I wrote followed the line of presenting the ideas in the order in which they had occurred to me."[95]

As argued in the two preceding sections, it was generally felt in the spring of 1927 that if relativity could be completely worked into PAULI's theory, it would probably yield the correct fine structure without approximation. From this standpoint one could assume that the logical way to proceed was to transform the PAULI theory into a LORENTZ invariant formulation, to provide relativistic generalizations of its 2×2 matrices. We are often told that there is a logical as well as a genetic connection between PAULI's theory and DIRAC's. According to SOMMERFELD, "the discovery of the Pauli equation was an important step

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[92] Letter, DARWIN to BOHR, 26 December 1927 (AHQP).

[93] P.A.M. DIRAC, "The Quantum Theory of the Electron, I," *Proc. Roy. Soc. (London)*, **A117** (1928), 610–624 (received 2 January, 1928). Reprinted in J. SCHWINGER (ed.), *Selected Papers on Quantum Electrodynamics*, New York, 1958.

[94] *Recollections*, p. 114 and p. 124.

[95] *Ibid.*, p. 124.

leading to the recognition of the true nature of the electron, *i.e.*, the Dirac equation."[96] The same thing has been suggested also more recently. KRONIG states: "Pauli paved the way for the relativistic theory of the electron and of hydrogen-like atoms which we owe to Dirac (1928)."[97] HEISENBERG viewed the connection as follows: "I cannot doubt that Dirac had been led to his discovery by Pauli's paper and especially by the relation $(p_x C_x - p_y C_y - p_z C_z)^2 = p^2$. The essential progress in Dirac's paper was the connection of Pauli's spin matrices with the Lorentz group ... ."[98]

These accounts agree with the conclusion of BARTEL VAN DER WAERDEN, according to whom PAULI's contribution was the decisive and truly revolutionary step on the way toward the relativistic electron theory. "Pauli's matrices s_k were used by Dirac to form a relativistic first-order wave equation ... Dirac's wave equation contains matrices and is similar to Pauli's, but not to the old relativistic wave equation. The step from one to two ψ components is large, whereas the step from two to four components is small ... in all cases, it was Pauli who made the first decisive step."[99] The historical reconstruction of DIRAC's route to the theory reveals, however, that there was no such close connection with PAULI's ideas as the standard explanation asserts. A procedure beginning from PAULI's theory *could* have yielded the result. But that was not DIRAC's procedure.

Following his general philosophy of science, DIRAC wished to found his theory on general principles rather than to fall back on any particular model of the electron. Contrary to PAULI, DARWIN and SCHRÖDINGER, who all imagined that the troubles of integrating spin with relativity should be solved through a more sophisticated model of the electron, DIRAC was not not at all interested in model-making. "The question remains as to why Nature should have chosen this particular model for the electron instead of being satisfied with the point-charge," DIRAC asked in the beginning of his paper.[100] Consequently he considered the electron as being a point-charge.

DIRAC's basic point of departure was that "we should expect the interpretation of the relativistic quantum theory to be just as general as that of the non-relativity theory."[101] In full agreement with his general outlook on physics, as expressed in the preface to *Principles of Quantum Mechanics*, DIRAC was guided by two requirements of "nearly-invariant": Firstly, the space-time

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[96] A. SOMMERFELD, *Wave-Mechanics*, London, 1930; p. 270.

[97] R. KRONIG, "The Turning Point," pp. 5–39 in FIERZ & WEISSKOPF (eds.), *op.cit.* (note 65); p. 32.

[98] Letter, HEISENBERG to RUSSELL HANSON, 19 September 1960. Quoted from *op.cit.* (note 1), p. 218. If the *C*'s denote the usual spin matrices, the relation stated in HEISENBERG's letter is incorrect; *cf.* equation (20) of this work.

[99] VAN DER WAERDEN, *op.cit.* (note 65), p. 223. Although it is standard to emphasize the role of spin in the creation of DIRAC's equation, there are exceptions. *Cf.* WHITTAKER's account: "In the process of deriving his equation, Dirac took no account of spin: his attention was focused on securing that the equation would be relativistically invariant." E. WHITTAKER, *From Euclid to Eddington*, Cambridge, 1949; p. 177.

[100] DIRAC, *op.cit.* (note 93), p. 610.

[101] *Ibid.*, p. 612.

properties of the equation should transform according to the theory of relativity; secondly, the quantum properties should transform according to the transformation theory of general quantum mechanics. DIRAC recognized that the latter requirement excluded the KG theory. Only if the wave equation is linear in $\partial/\partial t$ is the probability interpretation secured. If the wave equation is also to conform with the principle of relativity, it must furthermore contain energy and momenta in a LORENTZ-invariant way, *i.e.* it must apply to the relativistic Hamiltonian (7). These two requirements suggest the starting procedure

$$i\hbar\frac{\partial}{\partial t}\psi = c\sqrt{m_0^2c^2+p_1^2+p_2^2+p_3^2}\,\psi, \tag{19}$$

applied to a free electron. Equation (19) is taken over directly from the KG equation with the minus sign *arbitrarily* excluded. As mentioned, PAULI had considered the above equation to be a "sinnvoll" candidate, but had not been able to develop it any further. The equation is not only unsatisfactory from a mathematical point of view (the square root operator), but also from the point of view of relativity, since energy and momenta do not appear in a truly symmetric, *i.e.* LORENTZ-invariant, way. If, DIRAC asked himself, the square root could be arranged in a linear form in p_1, p_2, and p_3, this could be a way out of the dilemma. But how can a square root of four quantities possibly be linearized?

Considerations like these were the offspring of DIRAC's creation of his theory. The remarkably simple idea of linearization, an idea derived by a consistent application of the general principles of relativity and quantum theory, was the crucial point. DIRAC also called attention to another difficulty of the KG theory, namely that it allows for solutions with negative energy. One might therefore suppose that DIRAC's extension of PAULI's two-component wave function into a four-component wave function, was forced upon him by recognition of the negative energy states: a wave function which shall accomodate the spin states of a spin-half particle as well as of its antiparticle, must have four components. However, apart from the fact that this argument is unsatisfactory,[102] it was not DIRAC's motive. In 1928 he did not follow up the negative energy objection, which played no role in the creation of the theory. The 'fourness' was caused by mathematics, not physics.

Up to this point, there has been no reference to spin. Did spin play a central role in the genesis of the theory, as suggested by the standard account? DIRAC states that it did not: "I was not interested in bringing the spin of the electron into the wave equation, did not consider the question at all and did not make use of Pauli's work. The reason for this is that my dominating interest was to get a relativistic theory agreeing with my general physical interpretation and transformation theory .... It was a great surprise for me when I later on discovered that the simplest possible case did involve the spin."[103] Thus we may

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[102] *Cf.* that spin-zero particles (*e.g.* pions), as well as their antiparticles, are adequately described by the scalar KG equation.

[103] *Recollections*, p. 139. Also AHQP interview, third session, 1963.

conclude that DIRAC did not work out his theory with an eye on spin. Still, it may be doubted that he "did not make use of Pauli's work" at all. The knowledge of spin matrices did prove materially useful in DIRAC's reasoning, if only in an indirect way. DIRAC, who was always fond of "playing about with mathematics,"[104] realized at some stage the following identity:

$$|\vec{p}|=\sqrt{p_1^2+p_2^2+p_3^2}=\sigma_1 p_1+\sigma_2 p_2+\sigma_3 p_3, \tag{20}$$

where (σ_1, σ_2, σ_3) denote the PAULI spin matrices. The identity holds for arbitrary commuting p's. When DIRAC faced the problem of making a linear form out of (19), equation (20) appealed to him as a possible guide. If it could be generalized to four squares instead of three, it would indicate a solution. For then a linearization of the type wanted,

$$\sqrt{p_1^2+p_2^2+p_3^2+(m_0 c)^2}=\alpha_1 p_1+\alpha_2 p_2+\alpha_3 p_3+\alpha_4 m_0 c, \tag{21}$$

was provided. But were there coefficients with this property, and if so, what would they look like? If one argues that the linear wave equation, as given by (19) and (21), has to contain the KG equation, the following set of conditions is deduced:

$$\left.\begin{aligned}\alpha_\mu\alpha_\nu+\alpha_\nu\alpha_\mu&=0\ \ (\mu\neq\nu); \qquad \mu,\nu=1,2,3,4,\\ \alpha_\mu^2&=1.\end{aligned}\right\} \tag{22}$$

These conditions are in fact fulfilled by the spin matrices:

$$\begin{aligned}\sigma_i\sigma_k+\sigma_k\sigma_i&=0\ \ (i\neq k); \qquad i,k=1,2,3,\\ \sigma_i^2&=1.\end{aligned}$$

DIRAC naturally tried to take $\alpha_i=\sigma_i$ and sought for another 2×2 matrix as a candidate for α_4. Such a candidate does not exist, however, and DIRAC realized that 2×2 matrices just would not work. Then he again got one of those valuable ideas out of the blue. "I suddenly realized that there was no need to stick to quantities, which can be represented by matrices with just two rows and columns. Why not go to four rows and columns?"[105] This idea solved the problem of the linearization (21), and the explicit form of a α matrices was found.

In this crucial phase of the creation of the theory, the problem was purely mathematical, *viz.* to find quantities satisfying (22). As usual, DIRAC preferred to work out the solution all by himself, without consulting the mathematicians. If he had done so, he might have gotten the answer to his problem im-

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[104] *Recollections*, p. 142. DIRAC has often stressed his peculiar way of producing physical theories. *E.g.*: "A great deal of my work is just playing with equations and seeing what they give. ... I don't suppose that applies so much to other physicists; I think it's a peculiarity of myself that I like to play about with equations, just looking for beautiful mathematical relations which maybe can't have any physical meaning at all. Sometimes they do." AHQP interview, 1963.

[105] *Recollections*, p. 142.

mediately.[106] The algebrists in Göttingen, Hamburg or Berlin would easily have recognized DIRAC's conditions (22) as defining a so-called CLIFFORD algebra, based on the 16 units formed from 1, α_μ, $\alpha_\mu\alpha_\nu$, $\alpha_\mu\alpha_\nu\alpha_\lambda$, and $\alpha_1\alpha_2\alpha_3\alpha_4$. This algebra, which was used by RUDOLF LIPSCHITZ as early as 1884,[107] was known to be isomorphic to the algebra of 4×4 matrices. Without knowing that the general solution was already contained in the algebraic theory, DIRAC worked it out in his own way, by "playing about with mathematics."

With the linearization successfully carried out, the ice was broken. The next stages were to formulate the wave equation explicitly and subject it to a close investigation. From (19) and (21) the DIRAC equation for a free electron comes out:

$$(p_0+\vec{\alpha}\cdot\vec{p}+\alpha_4 m_0 c)\psi=0. \tag{23}$$

DIRAC reduced a physical problem to a mathematical one, and mathematics forced him to accept the use of 4×4 matrices as coefficients. Accepting this result, he was now forced to accept a four-component wave function $\psi=(\psi_1, \psi_2, \psi_3, \psi_4)$. This step was a bold one and in no way less decisive than PAULI's step from one to two components. On the contrary, for while the two components of PAULI's wave function were empirically justified, there was in 1927 no physical justification for DIRAC's two extra components. After all, the spin can attain but two values, not four.

This first and crucial stage was, to a considerable extent, rooted in DIRAC's philosophy of science in general, and to his view on the role of physical formalism in particular. If DIRAC had followed an empiricist logic of science, he would never have introduced such "unphysical" terms as 4×4 matrices. Indeed, DARWIN acknowledged that "DIRAC's success in finding the accurate equations shows the great superiority of principle over the previous empirical method."[108] Almost forty years later, DIRAC once again described his philosophy in terms which call to mind the creation of the theory of 1928: "Any physical or philosophical ideas that one has must be adjusted to fit the mathematics. Not the other way round. Too many physicists are inclined to start from preconceived physical ideas, and then try to develop them and find a mathematical scheme that incorporates them. Such a line of attack is unlikely to lead to success."[109]

In the further investigation of (23), DIRAC wrote it as

$$(p_0+\rho_1\vec{\sigma}\cdot\vec{p}+\rho_3 m_0 c)\psi=0, \tag{24}$$

where the quantities ρ_1, ρ_3, and $(\sigma_1, \sigma_2, \sigma_3)$ are new 4×4 matrices. At this stage, the equation was not much more than an inspired guess. Clearly, it had the

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[106] For the following remarks, I am indebted to a private communication from B.L. VAN DER WAERDEN.

[107] R. LIPSCHITZ, *Untersuchungen über Summen von Quadraten*, Bonn, 1884.

[108] C.G. DARWIN, "The Wave Equation of the Electron," *Proc. Roy. Soc. (London)*, **A118** (1928), 654–680 (received 6 March 1928); p. 664.

[109] P.A.M. DIRAC, "The Mathematical Foundation of Quantum Theory," pp. 1–8 in A.R. MARLOW (ed.), *Mathematical Foundations of Quantum Theory*, New York, 1978; p. 1.

advantages of being linear in $\partial/\partial t$ and $\partial/\partial x$ and of conforming with the standards of general quantum mechanics. Linearity in $\partial/\partial t$ and $\partial/\partial x$ does not in itself ensure relativistic invariance, but DIRAC showed that his equation, (23) or (24), is in fact invariant under a LORENTZ transformation. Thus his theory meets the requirements of quantum mechanics *and* relativity.

If the mysterious-looking equation was to claim more than formal validity, it had to face experimental reality. For this purpose, DIRAC investigated the behaviour of an electron when placed in an electromagnetic field. The standard procedure of replacing $\vec{p}$ with $\left(\vec{p}-\frac{e}{c}\vec{A}\right)$ and E by $(E-e\varphi)$ converts the equation to the form

$$\left\{\left(\frac{E}{c}-\frac{\varphi}{c}e\right)+\rho_1\left(\vec{\sigma}\cdot\left(\vec{p}-\frac{e}{c}\vec{A}\right)\right)+\rho_3 m_0 c\right\}\psi=0. \tag{25}$$

This was DIRAC's alternative to the KG equation was well as to the equations of the PAULI-DARWIN theory. To explore the physical meaning of (25), DIRAC used the same procedure as he had used in finding the α matrices, *i.e.* he compared the first-order equation with the KG equation. By squaring (25) one gets after some manipulation

$$\left\{\left(\frac{E}{c}-\frac{\varphi}{c}e\right)^2-\left(\vec{p}-\frac{e}{c}\vec{A}\right)^2-m_0^2c^2+\frac{\hbar e}{c}\vec{\sigma}\cdot\vec{\mathscr{H}}+i\rho_1\frac{\hbar e}{c}\vec{\sigma}\cdot\vec{\mathscr{E}}\right\}\psi=0. \tag{26}$$

This is a remarkable result, for it shows that the rough equivalence with the KG theory does not hold any longer: (26) contains two additional terms, terms which do not appear in the previous theories. DIRAC naturally interpreted the first term as being due to a magnetic moment of the electron, taken to be $\frac{e\hbar}{2mc}\vec{\sigma}$. As to the second term, which refers to an electric moment, DIRAC dismissed it as unphysical on the ground that it is a purely imaginary quantity arising from a real Hamiltonian.[110] The existence of an internal magnetic moment was highly satisfying, and all the more so, as it could be shown to be due to the correct spin angular momentum. DIRAC showed that in his new theory, $\vec{r}\times\vec{p}$ is no longer a constant of motion; it does not commute with the DIRAC Hamiltonian as does $(\vec{r}\times\vec{p}+\frac{1}{2}\hbar\vec{\sigma})$. The electron must therefore be ascribed an internal angular momentum of amount $\frac{1}{2}\hbar\vec{\sigma}$, in perfect harmony with the spin theory.

With the beautiful deduction of the correct spin and magnetic moment of the electron, DIRAC had in principle accounted for all the spectroscopic puzzles.

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[110] Also in the SCHRÖDINGER formulation of the KG equations, there appear imaginary quantities. DE BROGLIE felt uneasy with this situation. He stated: "Il faut cependant remarquer que [eq. (6) of the present paper] contient des termes imaginaires ··· et ceci soulève peut-être quelques objections au point de vue physique." L. DE BROGLIE, "Les principle de la nouvelle mécanique ondulatoire," *Journal de Physique*, **7** (1926), 321–337; p. 332. DE BROGLIE considered the five-dimensional theory of relativistic wave mechanics to provide an escape from the problem. See L. DE BROGLIE, "L'univers à cinq dimensions et la mécanique ondulatoire," *Journal de Physique*, **8** (1927), 66–73.

"The whole of the duplexity phenomena follow without arbitrary assumption,"[111] he stated. The triumph was substantiated by DIRAC in another paper, submitted to *Proceedings* one month after his first paper.[112] In the new paper DIRAC investigated the behaviour of spectral lines in a magnetic field; he showed that in weak fields the ZEEMAN effect comes out, in strong fields the PASCHEN-BACK effect. These results were not new, but it was the first time they were deduced "without arbitrary assumptions" concerning spinning electrons or other models. The anomalous ZEEMAN effect had earlier been treated by DIRAC, who, by applying his q-number algebra to the usual core model, derived the correct g-formula for weak magnetic fields.[113] This derivation, however, rested on the standard, but *ad hoc* assumption, that the gyro-magnetic ratio of the atomic core is twice the classical value.

In 1927 neither the KG theory nor the SCHRÖDINGER theory, in its spin formulation, was able to account for the fine structure of hydrogen. Even if the PAULI-DARWIN theory accounted for the fine structure for all practical purposes, the agreement was incomplete for two reasons: Firstly, it was based on a semi-empirical method which still contained *ad hoc* assumptions; secondly, the agreement could not be extended beyond a first-order approximation. In DIRAC's new theory the first objection was removed. But if the theory was to be considered as completely successful, one should be able to deduce from it the exact SOMMERFELD formula. DIRAC recognized that the deduction of equation (3) was essential to his theory and that it would be, at best, incomplete without this qualification. In his fundamental paper he devoted the last section to a confrontation on this question. He managed to show that the theory *in its first approximation* leads to the same results as the PAULI-DARWIN theory does.

DIRAC showed that in a central field, V, equation (25) gives a radial equation which combines features of the KG theory and the PAULI-DARWIN theory. The exact equation is:

$$\frac{\partial^2}{\partial r^2}\chi+\frac{2}{r}\frac{\partial}{\partial r}\chi+\left[\left(\frac{p_0+V}{\hbar}\right)^2-\left(\frac{m_0 c}{\hbar}\right)^2-\frac{j(j+1)}{r^2}\right]\chi$$
$$-(p_0+V+m_0 c)\frac{\partial V}{\partial r}\left(\frac{\partial}{\partial r}+\frac{j+1}{r}\right)\chi=0. \qquad (27)$$

j is a new quantum number, defined by $\vec{M}^2=(\vec{m}+\vec{s})^2=(j^2-\frac{1}{4})\hbar^2$; it can have any integral value except zero, and it plays the same role as does k in the earlier theories of DARWIN and PAULI. χ is equal to $\psi_\beta r^{-1}$ with ψ_β being half the components of the DIRAC wave function. If (27) is compared with the radial part of the KG eigenvalue equation, it turns out that they are formally identical, apart from the last term in (27). DIRAC showed that this term, in its first-order approximation, corresponds to the spin-orbit term, as found by THOMAS and taken over by DARWIN and PAULI. Having thus demonstrated that the eigen-

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[111] DIRAC, *op.cit.* (note 93), p. 610.

[112] P.A.M. DIRAC, "The Quantum Theory of the Electron, II," *Proc. Roy. Soc. (London)*, A**118** (1928), 351–361 (received 2 February 1928).

[113] P.A.M. DIRAC, "The Elimination of the Nodes in Quantum Mechanics," *Proc. Roy. Soc. (London)*, A**111** (1926), 281–305 (received 27 March 1926).

value equation includes the same terms as the older theories did, DIRAC did not have to solve it explicitly. He concluded his paper: "The present theory will thus, in the first approximation, lead to the same energy levels as those obtained by DARWIN, which are in agreement with experiment."[114]

When DIRAC's theory appeared, its strength lay on the conceptual and methodological level. In fact, the theory did not yield one result, or explain one experimental fact which had not already been covered by the earlier theories. Since to deliver the SOMMERFELD formula (3) would have gone far to justify the new theory, one may wonder why DIRAC did not attack the problem with more determination, *i.e.*, either by an exact calculation or by including higher corrections. According to DIRAC's own account, he did not even attempt to solve equation (27) exactly, but looked for an approximation from the start.[115]

As for the motives in restricting his effort to approximate solution, DIRAC explains it in terms of a pet idea of his, namely that scientists are often motivated by fear in their activities. "I was afraid that maybe they [*i.e.* the higher order corrections] would not come out right. Perhaps the whole basis of the idea would have to be adandoned if it should turn out that it was not right to the higher orders and I just could not face that prospect. So I hastily wrote up a paper giving the first order of approximation and showing it to that accuracy; at any rate, we had agreement between the theory and experiment. In that way I was consolidating a limited amount of success that would be something that one could stand on independently of what the future would hold. One very much fears the need for some consolidated success under circumstances like that, and I was in a great hurry to get this first approximation published before anything could happen which might just knock the whole thing on the head."[116]

Evidently there is much truth in DIRAC's moral about fear: psychological factors like fear and ambition do play a considerable role in scientists' works. Still, DIRAC probably overstates his moral,[117] which does not agree very well with his general philosophy of science. DIRAC emphasizes the confidence one should have in formal beauty associated with simple transformation properties; such matters should be primary to experimental agreement, he explains. DIRAC was actually guided by this philosophy when he created his relativistic theory of the electron. It seems therefore unlikely that he would really have feared that the theory might break down if it were applied to the hydrogen spectrum.

I shall propose a somewhat different version of why DIRAC did not include an exact treatment of the hydrogen spectrum. Having found equation (26) and having realized that it contained the correct spin, DIRAC was in a hurry to publish and was not prepared to waste time in a detailed examination of the exact energy levels of the hydrogen atom. He realized that it was not a mathematically simple problem and decided to publish the first approximation.

[114] DIRAC, *op. cit.* (note 93), p. 624.

[115] P.A.M. DIRAC, *The Development of Quantum Theory*, New York, 1971; p. 42.

[116] *Ibid.*, p. 42.

[117] DIRAC believes that his moral about fear and boldness in scientific creativity "is fairly common and you can accept it as a general rule applying to all research workers who are concerned with the foundations of physical theory." *Op. cit.* (note 115), p. 13. See also P.A.M. DIRAC, "Hopes and Fears," *Eureka*, no. 32 (1969), 2–4.

In this he was justified, in view of the brilliant results already obtained and the logical neatness of the entire procedure. And DIRAC was motivated, I suggest, by competition. As shown below, in 1927 there was a race to get the correct relativistic spin equation. DIRAC knew that other physicists were also on the trail, so he may indeed have been motivated by "fear". But it was, I suppose, fear of not being first to publish, not any fear that the theory have serious shortcomings. This conjecture is substantiated by the fact that he did not *attempt* to obtain the exact agreement, not even after he had published his theory. If the agreement were really so important to the theory, as would be implied by DIRAC's moral of "fear", why did he not attack the matter? I believe that DIRAC was quite satisfied with the approximate agreement and had full confidence that the theory could also provide an exact agreement. He simply did not see any point in engaging in the complicated mathematics of equation (27).

This task was performed independently by DARWIN[118] and by GORDON[119], both experts in the kind of mathematical analysis required for the solution of eigenvalue equations. In February 1927, PAULI reported to KRONIG: "Nun ist ja die Diracsche Arbeit erschienen. Es ist ja wunderbar, wie das alles stimmt! Herr Gordon konnte ohne Schwierigkeiten nachrechnen, dass aus Diracs Gleichungen auf S. 622 unten, für $V=\frac{e^2}{cr}$ *die alte Sommerfeldsche Formel* für die Energieniveaus in Strenge folgt."[120]

At the time when DIRAC created his theory, several other physicists worked hard to construct a relativistic spin quantum theory. None of these little known attempts had any influence on the further development of quantum physics, since they were superseded by DIRAC's theory. But they may illustrate the competitive atmosphere of the time and the way in which other physicists attacked the problem which DIRAC solved.

In Utrecht, KRAMERS presumably started from the KG equation, to which he added a relativistically invariant spin term, and he obtained in this way a LORENTZ-invariant spin equation.[121] This equation was equivalent to DIRAC's equation (25), but it used a two-component PAULI wave function. KRAMERS' equation can be written as two linear equations, equivalent to DIRAC's equation. However, even if KRAMERS seems to have been near a "DIRAC equation", he had obtained it only by introducing the spin beforehand, *i.e.* completing the programme advanced by PAULI and DARWIN. In Göttingen, similar attempts

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[118] *Op.cit.* (note 108).

[119] W. GORDON, "Die Energieniveaus des Wasserstoffatoms nach der Diracschen Quantentheorie des Electrons," *Zs. f. Phys.*, **48** (1928), 11–14 (received 23 February 1928).

[120] Letter, PAULI to KRONIG, 15 February 1928 (AHQP).

[121] The scant information concerning KRAMERS' relativistic equation is only second-hand. See AHQP interview with KLEIN, and *Recollections*, p. 139. KRAMERS' approach probably was similar to the one which appears in § 64 in H.A. KRAMERS, *Quantum Mechanics*, Amsterdam, 1957. Formulations of the same method were first published in H.A. KRAMERS, "On the Classical Theory of the Spinning Electron," *Physica*, **1** (1934), 825–828, and in H.A. KRAMERS, "Classical Relativistic Spin Theory and its Quantization," *Verh. Zeeman Jubil.*, 1935, pp. 403–412. Both papers are reprinted in KRAMERS' *Collected Scientific Works*, Amsterdam, 1956.

were made by EUGENE WIGNER in collaboration with JORDAN. JORDAN's attempt may be glimpsed from the following remark of the Cambridge physicist GEORGE BIRTWISTLE: "Pauli has recently done some work on the application of quantum mechanics to the spinning electron, which is now being extended by Jordan so as to include relativity."[122] In Göttingen, there were no attempts to start out with the construction of a linear Hamiltonian. "We were very near to it, and I cannot forgive myself that I didn't see that the point was linearization," JORDAN is to have said.[123] JORDAN and WIGNER learned about DIRAC's equation from a letter that DIRAC sent to MAX BORN before the publication of his paper.[124] JORDAN and WIGNER at once realized that DIRAC's theory was superior to their own attempt. "We were not satisfied with any of the equations we had found but were not yet ready to give up. The letter to Born changed all that. As Jordan put it 'Well, of course, it would have been better had we found the equation but the derivation is so beautiful, and the equation so concise, that we must be happy to have it.'"[125]

In Leningrad, relativistic spin quantum mechanics was investigated by JAKOV FRENKEL[126] and also by DMITRI IWANENKO and LEV LANDAU.[127] They worked out theories, which in some respects were similar to DIRAC's theory. The Soviet physicists developed the approach taken by DARWIN, and established LORENTZ-invariant wave equations in which SCHRÖDINGER's wave function was generalized to be a tensor. Although the equations found by FRENKEL and by LANDAU & IWANENKO were based on the KG theory, *i.e.* being second order differential equations, they managed to account for spin effects without introducing spin empirically. The laborious tensor theories of the Leningrad physicists appeared, however, incomprehensible and very complicated when compared with DIRAC's theory.

7. Factors Determining Dirac's Procedure

Standard textbooks in quantum mechanics introduce the DIRAC equation via the KG equation, which is shown to be unsatisfactory due to various reasons. The DIRAC equation then comes out as the logical attempt to cope with

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[122] B. BIRTWISTLE, *The New Quantum Mechanics*, Cambridge, 1928. Preface dated 1 October 1927. On p. 213.

[123] According to ROSENFELD, who was in Göttingen at the time when DIRAC's theory appeared. AHQP interview.

[124] See E. P. WIGNER, "Relativistic Equations in Quantum Mechanics," pp. 320–331 in J. MEHRA (ed.), *The Physicist's Conception of Nature*, Dordrecht, 1973.

[125] *Ibid.*, p. 320.

[126] J. FRENKEL, "Zur Wellenmechanik des rotierenden Elektrons," *Zs. f. Phys.*, **47** (1928), 786–803 (received 3 February 1928). From considerations on classical-relativistic invariance, FRENKEL anticipated some of DIRAC's results. In 1926 FRENKEL showed that the spinning electron can be ascribed a six-vector moment, containing three real magnetic and three imaginary electric components. J. FRENKEL, "Die Elektrodynamik des rotierenden Elektrons," *Zs. f. Phys.*, **37** (1926), 243–262.

[127] D. IWANENKO & L. LANDAU, "Zur Theorie des magnetischen Elektrons, I," *Zs. f. Phys.*, **48** (1928), 340–348 (received 8 March 1928).

these difficulties. The standard objections against the KG equation usually fall into four parts: (1) It is not linear in the time derivative. (2) It implies negative energy solutions. (3) The probability density is not positive-definite. These three objections are of a theoretical nature, referring to the logical structure and interpretation of the KG theory; they are, furthermore, closely interrelated.[128] (4) The KG equation fails to reproduce SOMMERFELD's formula and cannot account for spin.

As pointed out, the first objection formed the decisive point of departure for DIRAC, who recognized that an equation of motion, which is quadratic in the time derivative, cannot possibly be brought into harmony with the transformation theory of quantum mechanics. As to the second point, the one of the negative energies, matters are more complex. In his classic paper of 1928, DIRAC was much aware of this difficulty. The KG equation for an electron in an electromagnetic field refers equally well to a positive charge as to a negative one, he pointed out, only the positive charge solution is associated with negative energy. "One gets over the difficulty of the classical theory by arbitrarily excluding those solutions that have a negative *W*. One cannot do this in the quantum theory, since in general a perturbation will cause transitions from states with *W* positive to states with *W* negative. Such a transition would appear experimentally as the electron suddenly changing its charge from $-e$ to e, a phenomenon which has not been observed."[129] That the KG equation formally allows for negative energies, is evident from the fact that it is the wave mechanical translation of the classical-relativistic formula (7) which can be written as $E=(c^2p^2+m_0^2c^4)^{\frac{1}{2}}$ or $E=-(c^2p^2+m_0^2c^4)^{\frac{1}{2}}$. The negative energies are thus not particular to quantum mechanics. As DIRAC wrote two years later: "The difficulty is not a special one connected with the quantum theory of the electron, but is a general one appearing in all relativity theories, also in the classical theory."[130] Since the troublesome negative energies also appeared in DIRAC's equation, and since he was aware of this, they can hardly have motivated his search for a new theory.

The problem of negative energy states was there during the entire period of quantum mechanics, but it was taken seriously only after 1930. Even if the undesirable existence of negative energies cannot have escaped detection, physicists totally ignored them. They were dismissed as "unphysical", not recognized as being problems of relevance. This is, I think, the reason why the negative energies do not appear in the literature before 1928. When, for instance, FOCK solved the KG energy eigenvalue equation for the hydrogen atom, he arrived at a second order algebraic equation with two solutions of the form $E=mc^2(1 \pm F(n,k))$, where $F>1$; but he did not bother to mention the minus sign, which he regarded as trivially unphysical because it refers to negative total energy.

RUSSELL HANSON, in his analysis of the discovery of the positron, wrongly suggested that a more appropriate name for the 'DIRAC jump' (transition of a

[128] See, for example, J.J. SAKURAI, *Advanced Quantum Mechanics*, Reading (Mass.), 1967; pp. 75 ff.

[129] DIRAC, *op. cit.* (note 93), p. 612.

[130] P.A.M. DIRAC, "A Theory of Electrons and Protons," *Proc. Roy. Soc. (London)*, *A* **126** (1930), 360–365; p. 360.

negative-energy "hole" to a positive-energy electron with positive charge) would be 'GORDON jump'. "The original 'negative energy solutions' were in print in 1926", RUSSELL HANSON stated, referring to GORDON's paper.[131] But the statement that negative energies were in print in 1926 can be accepted only if taken in a highly implicit sense. In fact, GORDON did not mention negative energies at all, not even with the slightest hint. The only one, I believe, who ever mentioned the problem was KLEIN, who, in a footnote to the KG equation, remarked that "diese Gleichung [ergibt] eine Klasse von Lösungen, bei denen die Energie negativ ausfällt, und die in keiner direkten Beziehung zu der Bewegung des Elektrons stehen." KLEIN ended his brief comment: "Diese werden wir naturgemäss von der Betrachtung ausschliessen."[132]

The third point, about charge density, is usually presented as *the* argument against the original KG theory. Since ρ, as given by (10), is not positive definite, it cannot be interpreted as a probability density, as is done in the general quantum mechanics. The situation thus seems to imply that one should either abandon the KG theory or the probability interpretation (and with it the entire scheme of general quantum mechanics). It is a fact, however, that difficulties of this kind do not appear in the literature of the period; in 1926–28 the negative KG densities were not mentioned at all. The difficulty depends on accepting the probability interpretation; KLEIN, GORDON and other early contributors to the subject preferred to look upon ρ as being actually $e\rho$, an electrical charge density in accord with SCHRÖDINGER's view. In their many-particle electrical interpretation, they had no obvious reason to dismiss $e\rho<0$. One would expect, however, that during most of 1927 when the probability interpretation was generally accepted, the problem was recognized as being crucial to the KG theory. Whether this was the case or not, is uncertain. DIRAC did not mention the problem in 1927; neither does it play any role in *The Quantum Theory of the Electron*, in the first edition of which he nowhere refers to negative densities in connection with the new wave equation.[133] The fact that the problem was not explicitly mentioned does not prove that it did not enter DIRAC's considerations. Maybe it was obvious to him, so he just did not bother to mention it. After all, it is the same problem as the non-linearity in $\partial/\partial t$, just viewed from another angle: since $\psi_{t=t_0}$ and $\left(\frac{\partial\psi}{\partial t}\right)_{t=t_0}$ can be assigned arbitrary and inde-

[131] *Op. cit.* (note 1), p. 146.

[132] KLEIN, *op. cit.* (note 13), p. 411. The idea of "positrons" also appeared in connection with general relativity. EINSTEIN, for instance, showed in 1925 that if the electromagnetic and gravitational equations have solutions, that represent an electron with mass m and negative charge $+e$, there will also be solutions with mass m and charge $-e$, *i.e.* positive electrons. A. EINSTEIN, "Electron und allgemeine Relativitätstheorie," *Physica*, **5** (1925), 330–334.

[133] DIRAC's only reference to the role played by negative KG probabilities in the creation of his theory dates from more than forty years later: "A difficulty now appeared in connection with the relativistic equation of Klein and Gordon. The theory sometimes gave negative probabilities. It was a satisfactory theory only when it was used non-relativistically. I puzzled over this for some time and eventually thought of a new wave equation which avoided the negative probabilities." P.A.M. DIRAC, "Hopes and Fears," *Eureka*, no. 32 (1969), 2–4; p. 3.

pendent values at the time t_0, the KG theory does not forbid that $\psi \frac{\partial \psi^*}{\partial t} < \psi^* \frac{\partial \psi}{\partial t}$, and then, according to equation (10), $\rho < 0$.

Probably the KG density, as a separate item, did not determine DIRAC's route to relativistic quantum mechanics. The whole concept of wave mechanical charge densities and current densities was central to the wave mechanics camp, but it was largely outside DIRAC's programme of research. It was first in June 1928, at a lecture presented during the *Leipziger Universitätswoche*, that DIRAC explicitly referred to the matter.[134] When $\psi(t_0)$ is known, he argued on this occasion, the KG equation implies that $\left(\frac{\partial \psi}{\partial t}\right)_{t=t_0}$ is totally undetermined and therefore $\psi(t > t_0)$ is also undetermined. Since ρ is a function of ψ and $\partial\psi/\partial t$, knowledge of $\rho(t_0)$ leaves $\rho(t > t_0)$ undetermined, so the electrical charge $\int \rho \, dV$ may attain any value. "Damit würde das Prinzip von der Erhaltung der Elektrizität verletzt sein. Folglich muss die Wellengleichung linear in $\partial/\partial t$ sein ... ."[135]

Lastly, the KG equation's failure to deliver the hydrogen spectrum exactly did not constitute a major challenge to DIRAC, as I have argued; and neither did the KG equation's failure to deliver spin. In the genesis of the relativistic theory of the electron, as in most of DIRAC's works, empirical matters were subordinated to theoretical considerations, derived from principles.

8. Aftermath

DIRAC's theory caused great excitement among the quantum theorists, who immediately recognized it to be the correct solution to the problem of spin, relativity and quantum mechanics. In particular, his deduction of the spin matrices impressed the physicists. JORDAN considered the linearization procedure to be "a beautifully clever trick", and ROSENFELD recalled how the DIRAC equation "was regarded as a miracle ... an absolute wonder."[136] But of course the new theory posed as many questions as it answered. In the following years, much work was devoted to exploring the details of the theory and applying it to various problems. In this development, DIRAC did not take part. Apparently he was satisfied to have broken the ice and did not care much about the finer details of the theory. It was only in 1930 that DIRAC returned to his own theory.

The mathematics involved in DIRAC's theory was at its emergence intuitive and badly founded.[137] Now the mathematical aspects were eagerly taken up by

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[134] P.A.M. DIRAC, "Über die Quantentheorie des Elektrons," *Phys. Zeits.*, **29** (1928), 561–563.

[135] *Ibid.*, p. 561.

[136] AHQP interviews.

[137] HENRY MARGENAU contrasted DIRAC's use of mathematics with VON NEUMANN's: "While Dirac presents his reasoning with admirable simplicity and allows

mathematically minded physicists and by physically minded mathematicians. The mathematics was quickly explored by JOHANN VON NEUMANN, E. MÖGLICH, FOCK and others.[138] The nature of the new DIRAC wave function and its properties under transformation were studied by HERMANN WEYL and also by VAN DER WAERDEN.[139] DIRAC's theory was claimed also to have wide philosophical implications. EDDINGTON, in particular, made DIRAC's theory the basis for philosophical and cosmological speculations.[140]

The early attempts to combine quantum mechanics with general relativity continued after 1928, now on the basis of DIRAC's equation. In the years 1928–33 WEYL, FOCK, SCHRÖDINGER and other physicists managed to show that the linear wave equation could be incorporated into the framework of general relativity.[141] Also the physics of the DIRAC equation was quickly explored. The expressions for charge density and current density were obtained by DARWIN and further investigated by GREGORY BREIT and by GORDON.[142] DIRAC's theory was particularly successful in the study of relativistic scattering processes, first investigated by NEVILLE MOTT and KLEIN & YOSHIO NISHINA.[143]

The great success of the DIRAC equation caused interest in the KG equation to fade away; from 1928 it was largely ignored by the physicists. That the KG equation is really as good as any quantum mechanical equation, was made clear only in 1934 when PAULI & VICTOR WEISSKOPF revived the KG theory.[144] If interpreted correctly, *i.e.* as a field theory for BOSE-EINSTEIN particles, there is nothing wrong with the KG equation, PAULI & WEISSKOPF argued. Ever since, the KG equation has proved an indispensable tool in quantum field theory.

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himself to be guided at every step by physical intuition-refusing at several places to be burdened by the impediment of mathematical rigor-von Neumann goes at his problem equipped with the nicest of modern mathematical tools and analyses it to the satisfaction of those whose demands for logical completeness are most exacting." *Mathematical Gazette*, **17** (1933), 493. Quoted from JAMMER, *op. cit.* (note 74), p. 367.

[138] J.V. NEUMANN, "Einige Bemerkungen zur Diracschen Theorie des relativistischen Drehelektrons," *Zs. f. Phys.*, **48** (1928), 868–881. F. MÖGLICH, "Zur Quantentheorie des rotierenden Elektrons," *ibid.*, 852–867. V. FOCK, "Geometrisierung der Diracschen Theorie des Elektrons," *Zs. f. Phys.*, **57** (1929), 261–277.

[139] H. WEYL, *Gruppentheorie und Quantenmechanik*, Leipzig, 1928. B.L. VAN DER WAERDEN, "Spinoranalyse," *Nach Ges. Wiss. Göttingen Math.-Phys.*, 1929, 100–109.

[140] A.S. EDDINGTON, "On the Value of the Cosmical Constant," *Proc. Roy. Soc. (London), A* **133** (1931), 605–615. The most elaborate presentation of EDDINGTON's project is his *Relativity Theory of Protons and Electrons*, Cambridge, 1936.

[141] See VAN DER WAERDEN, *op. cit.* (note 65), p. 233.

[142] DARWIN, *op. cit.* (note 108). G. BREIT, "An Interpretation of Dirac's Theory of the Electron," *Proc. Nat. Acad. Sci.*, **14** (1928), 553–559. W. GORDON, "Der Strom der Diracschen Elektronentheorie," *Zs. f. Phys.*, **50** (1928), 630–632.

[143] N.F. MOTT, "The Scattering of Fast Electrons by Atomic Nuclei," *Proc. Roy. Soc. (London), A* **124** (1929), 425–442. O. KLEIN & Y. NISHINA, "Über die Streuung von Strahlung durch freie Elektronen nach der neuen relativistischen Quantendynamik von Dirac," *Zs. f. Phys.*, **52** (1928), 853–868.

[144] W. PAULI & V. WEISSKOPF, "Über die Quantisierung der skalaren relativistischen Wellengleichung," *Helv. Phys. Acta*, **7** (1934), 709–731.

However successful the DIRAC equation was, it encountered also great difficulties. For one thing, the DIRAC theory was a theory for a single electron, and it proved difficult to extend to cover many-particle phenomena. More seriously, it was haunted by the problem of negative energies. This difficulty was illuminated by KLEIN,[145] who showed that the DIRAC theory makes transitions from positive to negative energy states possible. KLEIN's work caused many attempts to eliminate the negative energy solutions, not the least on SCHRÖDINGER's part.[146] These attempts, however, merely showed that the negative energies were part and parcel of the theory. DIRAC took up the matter in 1930, in his *Theory of Electrons and Protons*, where he proposed what was then regarded as a rather speculative theory about an infinite "sea" of electrons in negative energy states, only exceptionally interrupted by unoccupied states or "holes". How DIRAC's hole theory was created, and how it evolved to become a celebrated and Nobel Prize winning theory of positrons is a fascinating story. But this story goes beyond the limits of the present essay.

Acknowledgments. I am grateful to ERIK RÜDINGER, the Niels Bohr Archive in Copenhagen, for permission to use the AHQP material. JOAN BROMBERG, of the Smithsonian Institution, has kindly commented on an early draft of my paper. In particular, I have benefitted from B.L. VAN DER WAERDEN's many critical and informative comments.

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(Received February 1, 1980; in revised form, October 1, 1980)

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[145] O. KLEIN, "Die Reflexion von Elektronen an einem Potentialsprung nach der relativistischen Dynamik von Dirac," *Zs. f. Phys.*, **53** (1928), 157–165.

[146] E. SCHRÖDINGER, "Über die kräftefreie Bewegung in der relativistischen Quantendynamik," *Sitzungsber. Preuss. Akad. Wiss.*, 1930, 418–431. "Zur Quantendynamik des Elektrons," *ibid.*, 1931. 63–73. "Sur la théorie relativiste de l'electron et l'interprétation de la mécanique quantique," *Ann. Inst. Poincaré*, **2** (1932), 269–310.