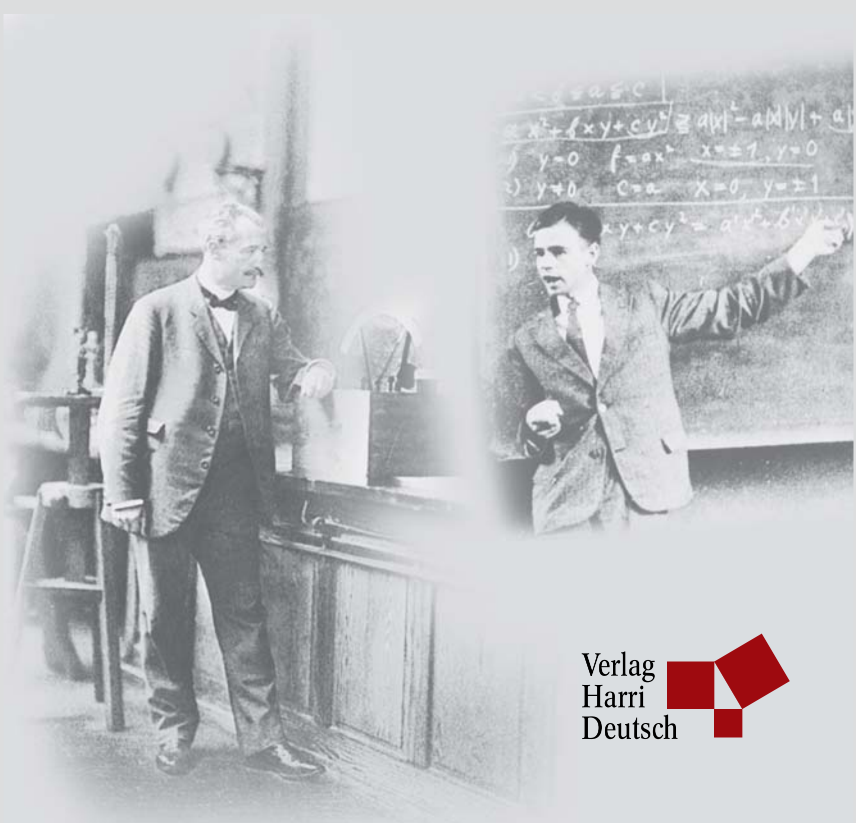
Studien zur Entwicklung von Mathematik und Physik in ihren Wechselwirkungen

Karl-Heinz Schlote
Martina Schneider

Mathematics meets physics

A contribution to their interaction in the 19th and the first half of the 20th century



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Studien zur Entwicklung von Mathematik und Physik in ihren Wechselwirkungen

Die Entwicklung von Mathematik und Physik ist durch zahlreiche Verknüpfungen und wechselseitige Beeinflussungen gekennzeichnet.

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Dabei kann es sich sowohl um historische Darstellungen als auch um die Analyse aktueller Wissenschaftsprozesse handeln; die Untersuchungsgegenstände beziehen sich dabei auf die ganze Disziplin oder auf spezielle Teilgebiete daraus.

Karl-Heinz Schlote, Martina Schneider (eds.)

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19^{th} and the first half of the 20^{th} century

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Die Untersuchung der Wechselbeziehungen zwischen Mathematik und Physik in Deutschland am Beispiel der mitteldeutschen Hochschulen ist ein Projekt im Rahmen des Vorhabens *Geschichte der Naturwissenschaften und Mathematik* der Sächsischen Akademie der Wissenschaften zu Leipzig.

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Vorwort

In vielen Bereichen der Naturwissenschaften wird von mathematischer Durchdringung gesprochen, doch gibt es wohl kaum Gebiete, in denen die wechselseitige Beeinflussung stärker ist als zwischen Mathematik und Physik. Ihr Wechselverhältnis war wiederholt Gegenstand erkenntnistheoretischer und historischer Untersuchungen. Eine wichtige, nur selten im Zentrum der Betrachtungen stehende Frage ist dabei die nach der konkreten Ausgestaltung dieser Wechselbeziehungen, etwa an einer Universität, oder die nach prägenden Merkmalen in der Entwicklung dieser Beziehungen in einem historischen Zeitabschnitt.

Diesem Problemkreis widmete sich ein Projekt der Sächsischen Akademie der Wissenschaften zu Leipzig, das die Untersuchung der Wechselbeziehungen zwischen Mathematik und Physik an den mitteldeutschen Universitäten Leipzig, Halle-Wittenberg und Jena in der Zeit vom frühen 19. Jahrhundert bis zum Ende des Zweiten Weltkriegs zum Gegenstand hatte. Das Anliegen dieses Projektes war es, diese Wechselbeziehungen in ihren lokalen Realisierungen an den drei genannten Universitäten zu untersuchen und Schlussfolgerungen hinsichtlich der Entwicklung und Charakterisierung der Wechselbeziehungen abzuleiten. Die in dem Projekt vorgelegten Ergebnisse dokumentieren die große Variabilität in der Ausgestaltung dieser Wechselbeziehungen, die Vielzahl der dabei eine Rolle spielenden Einflussfaktoren sowie deren unterschiedliche Wirkung in Abhängigkeit von der jeweiligen historischen Situation.

Auf der internationalen wissenschaftshistorischen Fachtagung «Mathematics meets physics – General and local aspects», die vom 22. – 25. März 2010 in Leipzig stattfand, wurden die Ergebnisse dieser lokalen Detailstudien in einen breiteren Kontext eingebettet und mit einem Fachpublikum diskutiert. International anerkannte Wissenschaftshistoriker und Fachwissenschaftler präsentierten ihre Untersuchungsergebnisse zu den Wechselbeziehungen zwischen Mathematik und Physik, wobei sie in ihrer Schwerpunktsetzung die Rolle innerdisziplinärer Entwicklungen, einzelner Wissenschaftlerpersönlichkeiten bzw. wissenschaftlicher

Schulen oder institutioneller Veränderungen in den Mittelpunkt ihrer Analysen rückten und somit die in dem Akademieprojekt gewonnenen Erkenntnisse in vielerlei Hinsicht ergänzten. Außerdem versuchten einzelne Referenten von einem allgemeineren, philosophischen Standpunkt aus, das Wesen und die Entwicklungslinien der Wechselbeziehungen zwischen Mathematik und Physik durch einige Merkmale zu charakterisieren. Im Ergebnis lieferte die Konferenz einen guten Einblick einerseits in die aktuellen Forschungen zu den Beziehungen zwischen Mathematik und Physik mit all ihrer Diversität und andererseits in die auch in der abschließenden Podiumsdiskussion formulierte, wohl etwas überraschende Einsicht, dass die in früheren Darstellungen skizzierte kontinuierliche Entwicklung der Wechselbeziehungen einer deutlichen Revision und Spezifizierung bedarf.

Mit diesem Tagungsband werden die vorgetragenen Ergebnisse nun einer breiten wissenschaftlichen Öffentlichkeit vorgelegt. Dabei will der Band nicht nur Einblicke in die gegenwärtige Forschung gewähren, sondern zugleich neue Untersuchungen anregen. Er enthält 14 der insgesamt 18 präsentierten Vorträge in einer überarbeiteten Fassung. Vier der Tagungsteilnehmer haben aus unterschiedlichen Gründen ihr Referat leider nicht zur Publikation eingereicht. In einigen Fällen werden sie ihre Ergebnisse in ein größeres eigenes Werk einfließen lassen. Um dem Leser einen vollständigen Überblick über die vorgetragenen Themen zu geben, ist am Ende des Buches das Tagungsprogramm angefügt.

Die Konferenzsprachen waren Deutsch und Englisch. Wir haben als Herausgeber diesen zweisprachigen Charakter der Tagung bewusst für diesen Band übernommen und es den Autoren überlassen, die Ausarbeitung ihres Vortrags in Deutsch oder in Englisch zu präsentieren. In den meisten Fällen gab es sowohl gute Gründe für die Wahl der deutschen Sprache, als auch für die Wahl des Englischen.

In die Gestaltung der Artikel haben wir nur sehr vorsichtig und nur formale, keine inhaltlichen Aspekte betreffend eingegriffen. Neben der Anpassung an ein einheitliches Layout wurde jedem Artikel eine inhaltliche Übersicht vorangestellt, die wir aus der vom Autor vorgenommenen Gliederung seines Beitrags erzeugten. Aufgrund der individuellen Gewohnheiten ergaben sich dabei deutliche Unterschiede zwischen den einzelnen Artikeln, die wir als persönliche Note des Autors interpretiert haben und nicht versuchten zu beseitigen. Die Angaben zur verwendeten Literatur und die Zitierweise wurde von uns ebenfalls nicht vereinheitlicht. Dennoch folgen sie im Wesentlichen einem einheitlichen Schema, indem die Verweise in den Fußnoten bei der Literatur durch Angabe des Autors und des Erscheinungsjahres bzw. bei Archivalien entsprechend der offiziellen Abkürzungen der Archive vorgenommen werden. Sind von einem Wissenschaftler mehrere Arbeiten aus einem Jahr aufgeführt worden, so wird an die Jahreszahl der Buchstabe a, b oder c entsprechend der Auflistung im Literaturverzeichnis angefügt. Die Auflösung der Kürzel wird im Literaturverzeichnis vorgenommen, das am Ende des jeweiligen Artikels steht. Während das Literatur- und Quellenverzeichnis bei dem jeweiligen Artikel belassen wurde, sind die Personennamen in einem gemeinsamen Personenverzeichnis am Ende des Buches zusammengestellt. Soweit bekannt bzw. ermittelbar wurden die Lebensdaten der Personen angefügt. Schließlich haben wir die Reihenfolge der Artikel aus inhaltlichen Gründen gegenüber der Vortragsfolge im Programm leicht abgeändert.

Die Durchführung der Tagung in dem geplanten Umfang wurde erst durch die finanzielle Unterstützung seitens der Deutschen Forschungsgemeinschaft möglich. Für diese Hilfe danken wir sehr herzlich. Ebenso danken wir der International Commission on the History of Mathematics, die die Tagung als förderungswürdig anerkannte und ihr eine größere, internationale Aufmerksamkeit verschaffte. Bei der Vorbereitung der Tagung stand uns die Kommission für Wissenschaftsgeschichte der Sächsischen Akademie der Wissenschaften zu Leipzig, insbesondere ihr Vorsitzender Herr Professor M. Folkerts, mit Rat und Tat zur Seite, wofür wir uns herzlich bedanken. Doch was wäre eine Tagung ohne die fleißigen Helfer im Hintergrund. Ein besonderer Dank gilt diesbezüglich mehreren Mitarbeitern in der Verwaltung der Akademie, von denen stellvertretend Frau E. Kotthoff und Herr A. Dill besonders genannt seien.

Bei der Drucklegung des Buches konnten wir wie gewohnt auf die gute Zusammenarbeit mit dem Verlag, speziell Herrn K. Horn, und dessen Kooperationspartner Herrn Dr. S. Naake bauen. Trotz des gegenüber vorangegangenen Publikationen deutlich größeren Aufwandes hat Herr Dr. Naake uns bei der Gestaltung des Buches sehr kompetent beraten sowie unsere Vorstellungen mit viel Geduld und großer Sorgfalt umgesetzt, beiden einen herzlichen Dank. Weiterhin möchten wir Frau

Dr. H. Kühn für ihre tatkräftige Unterstützung und die zahlreichen Hinweise bei der Vorbereitung des Buchmanuskripts und während der Fahnenkorrektur danken.

Die Tagung ist ein wichtiges Element des eingangs genannten Forschungsprojektes der Sächsischen Akademie im Rahmen des Akademievorhabens: «Geschichte der Naturwissenschaften und der Mathematik». Dem Bundesministerium für Bildung und Forschung sowie dem Sächsischen Staatsministerium für Wissenschaft und Kunst danken wir für die finanzielle Absicherung dieses Akademieunternehmens und somit auch des Druckes dieses Tagungsbandes. Der Band bildet die Abschlussveröffentlichung des Projektes und ist zugleich die letzte Publikation in dem in einem Monat auslaufenden Akademievorhaben.

Mainz/Leipzig, November 2010

Martina Schneider
Karl-Heinz Schlote

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Mathematics, Relativity, and Quantum Wave Equations

Helge Kragh

1 Introduction

Mathematical considerations played an important role in the new physics that emerged in the early decades of the twentieth century. This may be best known from the general theory of relativity, but the role of mathematics was no less important in the case of the other revolutionary theory of the period, quantum mechanics. In this paper I exemplify the relationship between mathematics and physics by looking at the development that in the late 1920s led to a relativistic theory of the electron, as described by the Dirac wave equation. The problem that faced the new generation of quantum physicists was to establish a theory that was consistent with the general principles of both quantum mechanics and special relativity; in addition the theory would have to incorporate the spin of the electron which was discovered in 1925 and at first seemed foreign to quantum mechanics. In this process, as it unfolded in the years 1926 – 28, contributions from mathematics were of considerable importance. Likewise, some of the concepts and quantities introduced by the physicists turned out to be of great interest to the pure mathematicians.

2 Schrödinger and his equation

Quantum mechanics took its start with young Werner Heisenberg's *Umdeutung* of atomic mechanics in the early fall of 1925. A couple of months later, the new and mysterious theory was established on a firm basis with the famous *Dreimännerarbeit* of Heisenberg, Max Born and Pascual Jordan. The new abstract mechanics was initially referred to as the Göttingen mechanics, but soon came to be known as the theory of matrix mechanics. Heisenberg was originally uncertain about the meaning of the non-commutative multiplication of quantities that in a symbolic form appeared in his theory. It was only after the intervention of the mathematically accomplished Max Born that it was realized to be a case of matrix calculus and that Heisenberg's quantum variables could similarly be understood as matrices. As Born recalled, he discovered that "Heisenberg's symbolic multiplication was nothing but the matrix calculus, well known to me since my student days from the lectures of [Jacob] Rosanes in Breslau."[1]

Erwin Schrödinger's route to quantum mechanics was entirely different from the one followed by his colleagues in Germany. He was primarily motivated by an attempt to turn Louis de Broglie's theory of matter waves, as propounded in his *Recherches sur la théorie des quanta* from 1924, into a theory of atomic structure and, at the same time, to use it for a quantum theory of gas statistics.[2] By November 1925 Schrödinger was looking for a wave mechanics of atoms, realizing that he needed a wave equation to govern the behaviour of the still mysterious ψ matter waves. The equation that eventually appeared on the second page of his first communication in *Annalen der Physik*, submitted on 27 January 1926, was the celebrated stationary Schrödinger equation, namely, the eigenvalue equation for the energy of a hydrogen atom.[3] It had the form

$$\Delta\psi + \frac{2m}{K^2}\left(E + \frac{e^2}{r}\right)\psi = 0 \tag{1}$$

Where $K \equiv h/2\pi$ (or $\hbar$, to use the notation introduced by Dirac in 1930). However, this was not the equation that Dirac arrived at in mid December. Because his theory was based on de Broglie's ideas of unifying quantum theory and special relativity, it was framed relativistically from the very beginning. As we know from Schrödinger's notebooks, letters and recollections, his original wave equation was relativistically invariant, namely of the form:

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

with $E = h\nu$. In order to solve the equation, he used the standard separation $\psi(\theta, \phi, r) = \Psi(\theta, \phi)\chi(r)$ and focused on the radial equation which would yield the energy spectrum. There was a close mathematical

[1] Born 1975, p. 217. Matrix methods were not unknown in physics in the early twentieth century, but they were not widely used and did only enter quantum theory with the works of Born and Jordan. See Mehra/Rechenberg 1982, pp. 34 – 44.

[2] Detailed analyses of Schrödinger's route to the wave equation, including references to the primary sources, can be found in Kragh 1982 and Mehra/Rechenberg 1987, pp. 377 – 419. See also Joas/Lehner 2009.

[3] Schrödinger 1926.

analogy between the case considered by Schrödinger and Arnold Sommerfeld's earlier analysis of the relativistic Bohr atom, which assumedly guided Schrödinger's approach. At any rate, he knew that his energy eigenvalues would have to comply with the result derived by Sommerfeld in 1916. This result, also known as the fine-structure formula, was confirmed experimentally and enjoyed great authority. In terms of the principal and azimuthal quantum numbers (n and k, respectively), Sommerfeld's formula was the following:

$$E(n,k) = m_0c^2 \left\{ 1 + \frac{\alpha^2 Z^2}{\left[(n-k) - \sqrt{k^2 - \alpha^2}\right]^2} \right\}^{-1/2} - m_0c^2 \tag{3}$$

or approximately

$$E(n,k) \cong -\frac{Rhc}{n^2} \left\{ 1 + \frac{\alpha^2 Z^2}{n^2} \left(\frac{n}{k} - \frac{3}{4} \right) \right\} \tag{4}$$

Here α denotes the dimensionless fine-structure constant $e^2/\hbar c$, Z is the nuclear charge ($Z = 1$ for hydogen), and R is Rydberg's spectroscopic constant. For the difference in wave numbers ($f = 1/\lambda = E/hc$) between the quantum levels $(n,k) = (2,2)$ and $(2,1)$ this gives the experimentally testable fine-structure separation

$$\Delta f = \frac{R\alpha^2 Z^4}{16} = 0.365\,\mathrm{cm}^{-1} \tag{5}$$

for $Z = 1$. It was this prediction which was convincingly confirmed by Friedrich Paschen and other experimentalists.[4]

The calculation of the energy values caused Schrödinger great mathematical difficulties, such as he reported to Wilhelm Wien in a letter of 27 December 1925: "At the moment I am plagued by a new atomic theory. If only I knew more mathematics! … For the time being I must learn more mathematics to be able to get full hold on the vibration problem – a linear differential equation, not unlike that of Bessel but less familiar

[4] How could the semi-classical, no-spin treatment of Sommerfeld result in the very same formula as later derived on the basis of relativistic spin quantum mechanics? On this question, see Biedenharn 1983.

and exhibiting remarkable boundary conditions."[5] It took Schrödinger a week or two to solve the equation, during which work he was assisted by Hermann Weyl, his friend and colleague at the University of Zurich. (It is unknown with what Weyl helped him.) Although the mathematics of the wave equation was unfamiliar to Schrödinger and most other physicists, it was well known to the mathematicians. Schrödinger relied on the third revised edition of a textbook by Ludwig Schlesinger, a mathematics professor at the University of Giessen, which was originally published in 1900 and contained many of the methods Schrödinger needed.[6] It is possible that he also used Richard Courant and David Hilbert's *Methoden der mathematischen Physik* published in 1924, but he did not actually refer to it in his first communication. On the other hand, he did so repeatedly in his later communications on wave mechanics. It seems that he only began to use the Courant-Hilbert work extensively from the end of 1926, possibly at the instigation of Erwin Fues, his assistant at the institute of theoretical physics in Zurich.[7]

To make a long story short, Schrödinger must have found the result of his laborious calculations disappointing. The good thing was that he obtained a fine-structure formula quite similar to Sommerfeld's, but this was more than canceled by the value derived for the fine-structure splitting: it came out too large by a factor of 8/3, which completely destroyed the agreement between theory and experiment. Something had gone wrong and a frustrated Schrödinger was unable to locate the failure. In a much later letter to the American physicist Wolfgang Yourgrau, Schrödinger referred to his early relativistic theory, which "gives a formal expression of the fine-structure formula of Sommerfeld, but it is incorrect owing to the appearance of half-integers instead of integers." He continued: "My paper in which this is shown has ... never been published; it was withdrawn by me and replaced by the non-relativistic treatment."[8] That is, Schrödinger decided to

[5] Schrödinger to Wien, 27 December 1925, Archive for History of Quantum Physics. See also Mehra/Rechenberg 1987, p. 461.

[6] Schlesinger 1900.

[7] See Mehra/Rechenberg 1987, p. 582, who conclude that "all the steps, which Schrödinger had undertaken in early 1926, seem to demand the application of methods displayed in *Courant-Hilbert*, although apparently he was not aware of it."

[8] Mandelstam/Yourgrau 1958, p. 114.

abandon temporarily the fine-structure problem and instead turn to the non-relativistic approximation which could reproduce the simple Bohr formula for the Balmer spectrum. It was this wave theory of atoms he presented in the *Annalen*, with almost no hints that he had derived it from a more ambitious but empirically problematic theory.

3 The Klein-Gordon equation

Schrödinger only published the relativistic generalization of his wave equation in the fourth of his series of communications on wave mechanics, completed in June 1926. Several other physicists arrived at the same equation, some of them independently and earlier than Schrödinger.[9] For example, in the summer of 1926 de Broglie presented in *Comptes Rendus* the eigenvalue equation and the time-dependent wave equation, both in relativistic form. For an electron bound in the potential ϕ he wrote the latter equation as

$$\left(\hbar c^2 \Delta - \hbar \frac{\partial^2}{\partial t^2} - 2ie\hbar \frac{\partial}{\partial t}\right) \psi = \left(m_0^2 c^4 - e^2 \phi^2\right) \psi \tag{6}$$

Which for a free electron reduces to

$$\left(c^2 \Delta - \frac{\partial^2}{\partial t^2}\right) \psi = \left(\frac{m_0 c^2}{\hbar}\right)^2 \psi \tag{7}$$

The same equations were discussed a little earlier by Vladimir Fock in Leningrad who was also the first to publish a detailed solution to the eigenvalue equation. Fock realized, as Schrödinger had known for some time, that the equation failed to reproduce the correct fine-structure separation.

Oskar Klein, the young Swedish physicist who at the time worked at Bohr's institute in Copenhagen, was the first to publish what soon became known as the Klein-Gordon equation, which he did in a paper in the *Zeitschrift für Physik* in the spring of 1926. (The other name refers to the German physicist Walter Gordon, who some months later discussed the same equations.) The relativistic equation was not of great importance to Klein, whose main purpose was to suggest an incorporation

[9] Kragh 1984, which includes references to the primary literature.

of quantum theory within the framework of five-dimensional relativity theory. "I started the whole thing in relativistic mechanics because I had this five-dimensional approach," he later recalled. "I never thought that that was any important thing, just to have the relativistic scalar equation after Schrödinger's equation."[10]

According to Klein, the fifth dimension related to the elementary electrical charge. In this way he hoped to explain the atomicity of electricity as a quantum law and also account for the then known basic building blocks of matter, the electron and the proton. Klein conjectured that what we think of as a point in three-dimensional space is really a tiny circle going round the fifth dimension in a loop with a certain period λ. The loop is not in ordinary space, but in a direction that extends it. As he explained, "the origin of Planck's quantum may be sought just in this periodicity in the fifth dimension." As to the period and its relation to the quantum of action, he suggested

$$\lambda = \frac{hc}{e}\sqrt{2\kappa} = \frac{4h}{e}\sqrt{\pi G} \cong 0.84 \times 10^{-32}\,\mathrm{m} \tag{8}$$

which is sometimes known as the Klein length. "The small value of this length ... may explain the non-appearance of the fifth dimension in ordinary experiments as the result of averaging over the fifth dimension," he wrote.[11] That is, like Theodor Kaluza had done earlier, he assumed that the extra dimension was rolled up to a less than microscopic size – compactified, as it was later called.

The Klein-Gordon equation was well known and generally accepted in 1926 – 27. Pauli was among the few physicists who raised objections to the equations, although he did so only in his correspondence. Latest by the end of 1926 he had lost confidence in the second-order wave equation. As he wrote in a letter to Schrödinger, "I do not believe that the relativistic equation of 2. order with the many fathers corresponds to reality."[12] He proposed to replace it by a linear first-order equation

[10] Interview with Klein of 1963, Archive for History of Quantum Physics, as quoted in Kragh 1984, p. 1026.

[11] Klein 1926. On Klein and five-dimensional quantum theory, see Kragh 1984 and Halpern 2007.

[12] Letter of 22 November 1926. Pauli 1979, p. 356.

involving the square-root operator

$$D = \hbar c \sqrt{m^2 c^4 - \Delta} \tag{9}$$

which “though mathematically uncomfortable makes sense *per se* and is also self-adjoint.” Although this idea would later prove important in Dirac’s derivation of his wave equation, at the time neither Pauli nor others looked seriously into it.

4 The spinning electron

By the end of 1925, the spin of the electron was generally accepted and it was widely recognized that the new phenomenon needed to be taken into account in quantum-mechanical calculations. If relativity alone could not account for the fine structure, perhaps a combination of relativity and spin could solve the problem. In the early months of 1926, Heisenberg and Jordan, assisted by Pauli, attacked the problem by writing the Hamiltonian of the hydrogen atom as the sum of two terms added to the unperturbed energy H_0:[13]

$$H = H_0 + H_1 + H_2 \tag{10}$$

Here H_1 is a relativistic correction to H_0, corresponding to the variation of the electron’s mass with its speed, and H_2 is the energy contribution due to the electron’s spin. Heisenberg and Jordan succeeded to obtain Sommerfeld’s fine-structure formula, albeit only in its first-order approximation. Their result was phenomenologically satisfying in so far that it accounted for all known doublet phenomena, but it failed to provide a proper explanation, that is, a deduction of these phenomena in terms of fundamental theory. Ideally, the physicists wanted a fully relativistic quantum equation from which would follow not only the exact Sommerfeld formula but also the magnetic moment of the electron. In the absence of such an equation, they proceeded less ambitiously.

One might try to make sense of the electron’s spin on the basis of the relativistic Klein-Gordon equation, such as did a few physicists, including Eugen Guth in Vienna and Antonio Carelli in Naples. However,

[13] Heisenberg/Jordan 1926.

none of the attempts to establish a connection between spin and the second-order wave equation were fruitful. Nor was this the case with Fritz London's attempt to interpret the spin within the framework of Klein's five-dimensional theory, namely by identifying the canonical conjugate of the fifth dimension with the spin angular momentum.[14]

Following an approach similar to the one adopted by Heisenberg and Jordan, Pauli proposed in 1927 to conceive the Schrödinger wave function as consisting of two components, each corresponding to one of the values of the spin: $\psi = (\psi_+, \psi_-)$.[15] Stating the spin vector as $\vec{s} = (\hbar/2)\vec{\sigma}$, Pauli found that the three components of σ could be written as 2×2 matrices, often known as Pauli matrices:

$$\sigma_x = \begin{pmatrix} 0 & 1 \\ 1 & 0 \end{pmatrix}, \quad \sigma_y = \begin{pmatrix} 0 & -i \\ i & 0 \end{pmatrix}, \quad \sigma_z = \begin{pmatrix} 1 & 0 \\ 0 & -1 \end{pmatrix} \tag{11}$$

Although the introduction of the spin matrices was an important innovation, Pauli's theory did not provide an explanation of spin and neither did it go beyond the Heisenberg-Jordan theory with respect to explaining the fine structure or incorporating relativity as more than a correction. With regard to phenomenology the two theories were equivalent, and the same was the case with the slightly later wave-mechanical spin theory presented by Charles G. Darwin. As Darwin expressed it: "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."[16]

5 A beautiful exercise in pure reason[17]

The problem of a fully relativistic quantum wave equation and its relation to the electron's spin was not much discussed during the fall of 1927. For instance, it did not turn up in the discussions during the Solvay conference in October that year. Most physicists were satisfied

[14] London 1927.

[15] Pauli 1927. For early spin quantum theories, see Van der Waerden 1960.

[16] Darwin 1927, p. 253.

[17] In recollections of 1985, Nevill Mott called Dirac's relativistic wave equation "the most beautiful exercise in pure reason that I have ever seen." See Mott 1987, p. 75.

with using either the Klein-Gordon equation or the Pauli-Darwin spin quantum mechanics, not worried about the lack of consistency between the two theories. However, Paul Dirac realized that the general structure of quantum mechanics, such as given by the recently developed transformation theory, was incompatible with a wave equation of the second order in the time derivative. To his mind, it was all-important that a relativistic generalization of the Schrödinger equation should be of the form

$$\mathrm{i}\hbar\frac{\partial}{\partial t}\psi = H\psi \tag{12}$$

which excludes the Klein-Gordon equation. Dirac reasoned that linearity in the time derivative was necessary for the probabilistic interpretation of quantum mechanics, and hence that the Hamiltonian must contain energy and momenta in their first order. Fifty years later he recalled:

> "I had the general physical interpretation of quantum mechanics which I felt sure was right, but it required one to work with a wave equation for quantum mechanics which was linear in the operator $\mathrm{d}/\mathrm{d}t$, giving $\mathrm{d}\psi/\mathrm{d}t$ equal to some finite function of ψ. Now, the Klein-Gordon equation involves $\mathrm{d}^2\psi/\mathrm{d}t^2$. This would not fit with my general interpretation. If one tried to fit it in, one was led to a probability which could be sometimes negative, and that of course is physical nonsense."[18]

When Dirac started to look for a linear and relativistic wave equation at the end of 1927, he was thoroughly acquainted with Pauli's spin theory which he exposed in details in his lectures on quantum mechanics that he had prepared during the summer. Indeed, he later claimed that he got the spin matrices independently of Pauli, a claim which however lacks documentary evidence. At any rate, during the creative phase Dirac decided to ignore the spin. "I was not interested in bringing the spin of the electron into the wave equation," he recalled, "It was a great surprise for me when I later on discovered that the simplest possible case did involve the spin."[19] Dirac started out by considering a free

[18] Dirac 1977, p. 141. For more details on Dirac's route to his wave equation, see Kragh 1981, Kragh 1990, pp. 50 – 66 and Mehra/Rechenberg 2000, pp. 287 – 299. Steiner 1998 provides an interesting account of how Dirac arrived at his equation, seeing it as an example of what he calls anthropomorphic physics. See also Dirac 1928, the paper in which the equation was first proposed.

[19] Dirac 1977, p. 139.

spinless electron governed by a wave equation of the form

$$i\hbar \frac{\partial}{\partial t}\psi = c\sqrt{p_1^2 + p_2^2 + p_3^2 + m^2c^2}\psi \tag{13}$$

where $p_1 = -i\hbar\partial/\partial x$, etc. He recognized that to make physical and mathematical sense, the square root had to be linearized, that is, written as

$$\sqrt{p_1^2 + p_2^2 + p_3^2 + m^2c^2} = \alpha_1 p_1 + \alpha_2 p_2 + \alpha_3 p_3 + \alpha_4 mc \tag{14}$$

But how could this be done? At this stage, when Dirac was faced with a purely mathematical problem, he was inspired by a property of the Pauli matrices that he had found by "playing around with mathematics":

> "I was playing around with the three components σ_1, σ_2, σ_3, which I had used to describe the spin of an electron, and I noticed that if you formed the expression $\sigma_1 p_1 + \sigma_2 p_2 + \sigma_3 p_3$ and squared it, p_1, p_2 and p_3 being the three components of momentum, you got just $p_1^2 + p_2^2 + p_3^2$, the square of the momentum. This was a pretty mathematical result. I was quite excited over it. It seemed that it must be of some importance."[20]

That is, Dirac found that the identity

$$\sqrt{p_1^2 + p_2^2 + p_3^2} = \sigma_1 p_1 + \sigma_2 p_2 + \sigma_3 p_3 \tag{15}$$

holds for any commuting numbers p. Obviously, if the identity could be generalized to four squares instead of three, it would indicate a solution. Arguing that the linear wave equation had to contain the Klein-Gordon equation as its square, Dirac derived the following set of conditions for the α-coefficients:

$$\alpha_\mu \alpha_\nu + \alpha_\nu \alpha_\mu = 0 \quad (\mu \neq \nu) \tag{16}$$

$$\alpha_\mu^2 = 1$$

where μ, $\nu = 1, 2, 3, 4$. Dirac knew that similar conditions were fulfilled by the spin matrices, but soon realized that $2{\times}2$ matrices would not be of any help. He consequently considered matrices with four rows and

[20] Dirac 1977, p. 142.

columns, arriving at the first version of what came to be known as Dirac matrices, which he expressed as:

$$\alpha_j = \begin{pmatrix} 0 & \sigma_j \\ \sigma_j & 0 \end{pmatrix} \quad \text{and} \quad \alpha_4 = \begin{pmatrix} 1 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 \\ 0 & 0 & -1 & 0 \\ 0 & 0 & 0 & -1 \end{pmatrix} \tag{17}$$

where $j = 1, 2, 3$. With the new α-matrices he could now formulate the wave equation for a free electron as

$$(p_0 + \vec{\alpha} \cdot \vec{p} + \alpha_4 mc)\psi = 0 \tag{18}$$

where $p_0 = E/c$. The Dirac equation exists in a variety of forms and notations. Modern physicists often use for Dirac's matrices the "gamma matrices" which were introduced by Pauli in 1936 and are related to the α matrices in a simple way.[21] Using Pauli's notation, the Dirac equation can be written in the compact form

$$i\gamma^\mu \partial_\mu \psi = \frac{mc}{\hbar}\psi \tag{19}$$

Here γ^μ are the four Dirac gamma matrices with μ = 0, 1, 2, 3 satisfying

$$\gamma^\mu \gamma^\nu + \gamma^\nu \gamma^\mu = 2g^{\mu\nu} \tag{20}$$

where $g^{\mu\nu}$ is the contravariant Lorentz metric.

The crucial step in Dirac's derivation was the reduction of a physical problem to a mathematical one. It was mathematical reasoning that forced him to introduce 4×4 matrices as coefficients and, as a consequence, a four-component wave function

$$\psi = (\psi_1, \psi_2, \psi_3, \psi_4) \tag{21}$$

"A great deal of my work is just playing with equations and seeing what they give", Dirac said in an interview of 1963. "I think it's a peculiarity of myself that I like to play about with equations, just looking for beautiful relations which maybe can't have any physical meaning at

[21] Pauli 1936.

all. Sometimes they do."[22] He worked for himself, without consulting either mathematicians or other physicists. In this case, where he was trying to find quantities that satisfied the anticommutation relations, mathematicians could have told him that the problem was well known.

The relations (16) define a so-called Clifford algebra, so named after William Kingdon Clifford who introduced his associative but non-commutative "geometric algebra" in a paper of 1878.[23] Two years later the German mathematician Rudolf Lipschitz reinvented the Clifford algebra, on which he gave a full exposition in a book of 1886. With Lipschitz's work it was known that Clifford algebra was isomorphic to the algebra of 4×4 matrices.[24] Without knowing that the general solution was already contained in the algebraic theory, Dirac worked it out in his own way, by "playing with equations." He essentially rediscovered the Clifford algebra. It is possible that Dirac received inspiration from Henry Baker's *Principles of Geometry*, a book of 1922 that he knew well and had earlier influenced him. Although Baker's book did not mention Clifford algebra, it did contain sections on algebraic symbols corresponding to 4×4 matrices.

Having found the wave equation for a free electron, Dirac had of course to show that it really described an electron, and he also had to prove that it was Lorentz invariant. I shall not go into these details except to point out that without introducing the magnetic electron in advance, Dirac was able to show that his equation included a term representing the magnetic moment of the spinning electron. That is, Dirac deduced the correct spin from first principles of relativity and quantum mechanics. In his paper of 1928 he also referred to the four components of the ψ function, of which only two corresponded to the electron. What did the other two components describe? This problem would soon lead to Dirac's celebrated theory of antielectrons, but this is a development outside the scope of this paper.

[22] Archive for History of Quantum Physics, interview of 1963, quoted in Kragh 1990, p. 325.

[23] Clifford 1878. For a brief history of Clifford algebra and its application in physics, see Bolinder 1987.

[24] Lipschitz 1886. On the connection between the Dirac equation and Clifford algebra, see Olive 1997.

6 Dirac, physics, and mathematical beauty

Dirac's approach of 1928 was essentially mathematical or logical, in the sense that he formulated a mathematical problem from basic physical principles and then focused on the solution of this problem, disregarding physics. This approach, together with a willingness to associate formulae with physical meaning, led him to suggest the existence of antiparticles, originally by identifying protons with antielectrons. By 1930 he had reached the conclusion that theoretical physics must follow the route determined by what he later described as "beautiful mathematics." In his influential textbook *Principles of Quantum Mechanics*, he hailed what he called the "symbolic method," namely a formulation of quantum theory that relied only on symbols and which avoided physical interpretation. The symbols, he said, "are used all the time in an abstract way, the algebraic axioms that they satisfy and the connexion between equations involving them and physical conditions being all that is required." On the other hand, while praising mathematics as a most powerful tool in physics, he also made it clear that he favoured a pragmatic attitude: "All the same, the mathematics is only a tool and one should learn to hold the physical ideas in one's mind without reference to the mathematical form."[25]

In his slightly later paper of 1931, in which he introduced the antielectron as a separate particle and also suggested the existence of magnetic monopoles, Dirac extolled the power of pure mathematics stronger than he had done earlier: "The most powerful method of advance that can be suggested at present is to employ all the resources of pure mathematics in attempts to perfect and generalise the mathematical formalism that forms the existing basis of theoretical physics, and *after* each success in this direction, to try to interpret the new mathematical features in terms of physical entities (by a process like Eddington's Principle of Identification)."[26]

Dirac's attitude to the use of mathematics in physics changed over time. He did not strive towards mathematical rigour, an ideal for which he had little respect. In his *Mathematische Grundlagen der Quantenmechanik* of 1932, John von Neumann took Dirac to task for his intuitive use of

[25] Dirac 1930, p. 18 and p. vi.
[26] Dirac 1931, p. 60.

mathematics. "The method of Dirac", he said, "in no way satisfies the requirements of mathematical rigour – not even if these are reduced in a natural and proper fashion to the extent common elsewhere in theoretical physics."[27] Instead to follow the mathematicians' advice, Dirac preferred to rely on his intuition and let others, mathematicians or mathematical physicists, present his ideas in rigorous forms. This relaxed attitude is clearly visible in his invention of the Dirac matrices in 1928 and also in his introduction of the so-called δ-function the year before.[28] There is some similarity between the two cases, since in both of them Dirac was unconcerned with the works of the mathematicians. Laurent Schwartz recalled that some time after having established distribution theory in 1945, he became aware of the earlier works of Dirac and other physicists. The physicists, he realized, had made great progress "without the mathematicians 'given them the right'."[29]

Dirac often stressed the value of approximations and related engineering methods, and in general favoured a pragmatic attitude to mathematics. But latest from about 1940 there emerged a tension between his praise of mathematical pragmatism and his increasing emphasis on mathematical beauty as the only sure guide for progress in theoretical physics. In the James Scott Prize Lecture that he gave on 6 February 1939, Dirac spelled out his new ideas of the relationship between mathematics and physics which focused on the concept of mathematical beauty. Many years before his brother in law Eugene Wigner famously problematized the unreasonable effectiveness of mathematics in the natural sciences, Dirac discussed the same topic. How is it that the mathematical-deductive method is so remarkably successful in physics? According to Dirac:

> "This must be ascribed to some *mathematical quality in Nature*, a quality which the casual observer of Nature would not suspect, but which nevertheless plays an important role in Nature's scheme. One might describe the mathematical quality in Nature by saying that the universe is so constituted that mathematics is a useful tool in its description. However, recent advances in physical science show that this statement of the case is too trivial. The connection

[27] Von Neumann 1943, p. 2.
[28] On Dirac and the δ-function, see Peters 2004 and Bueno 2005.
[29] Schwartz 1972, p. 180.

> between mathematics and the description of the universe goes far deeper than this."[30]

In his James Scott Lecture and at numerous later occasions, Dirac asserted that the modern history of theoretical physics provides convincing evidence that there is a perfect marriage between the rules that mathematicians find interesting by their own standards and the rules that govern natural phenomena. He thought this was not accidental, but that it might reflect some deep identity between mathematics and physics:

> "Pure mathematics and physics are becoming ever more closely connected, though their methods remain different. One may describe the situation by saying that the mathematician plays a game in which he himself invents the rules while the physicist plays a game in which the rules are provided by Nature, but as time goes on it becomes increasingly evident that the rules which the mathematician finds interesting are the same as those which Nature has chosen. It is difficult to predict what the result of all this will be. Possibly, the two subjects will ultimately unify, every branch of pure mathematics then having its physical application, its importance in physics being proportional to its interest in mathematics."[31]

Dirac suggested that future developments in theoretical physics would lead to the "existence of a scheme in which the whole of the description of the universe has its mathematical counterpart." In accordance with this philosophy, he advised physicists to "begin by choosing that branch of mathematics which one thinks will form the basis of the new theory. One should be influenced very much in this choice by considerations of mathematical beauty. … Having decided on the branch of mathematics, one should proceed to develop it along suitable lines, at the same time looking for that way in which it appears to lend itself naturally to physical interpretation."[32]

Dirac went as far as arguing that mathematical beauty was the hallmark of truth for a physical theory. What amounted to an identification of beauty with truth led him to a one-sided emphasis on the mathematical-aesthetic method at the expense of the empirical-inductive

[30] Dirac 1939, p. 122.
[31] Ibid., p. 124.
[32] Ibid., p. 125.

method. As far as fundamental physics was concerned, he wanted to subordinate experimental tests to the admittedly vague idea of mathematical beauty. At least in some cases, mathematical beauty should be assigned a higher priority than comparison with experimental data, implying that ultimately truth would have to be judged on mathematics. "If the equations of physics are not mathematically beautiful that denotes an imperfection, and it means that the theory is at fault and needs improvement. There are occasions when mathematical beauty should take priority over agreement with experiment."[33] This is obviously a controversial and problematic claim, for other reasons because neither Dirac nor others have been able to come up with a definition of mathematical beauty that can serve as a standard for judging the truth of physical theories. There just is no consensus among either physicists or mathematicians as to which equations and mathematical structures should be singled out as particularly beautiful, elegant and interesting.[34]

I have earlier concluded that the strong version of the principle of mathematical beauty proved to be a failure in Dirac's scientific career, and I see no reason to change that conclusion. In his most creative phase, from about 1925 to 1933, he was only guided by mathematical considerations in a limited and fairly conventional sense.[35] Only after the mid-1930s did he turn into an apostle of mathematical beauty, primarily in his long and unfruitful critique of standard quantum electrodynamics. He mostly applied the principle of mathematical beauty rhetorically and destructively, whereas he did not succeed to build up sustainable alternatives of physics and cosmology on the basis of the principle. Contrary to what Dirac preached, the strong principle of mathematical beauty hampered his scientific creativity.

[33] Conversation with Jagdish Mehra from the late 1960s, in Mehra 1972, p. 39.

[34] An analysis of mathematical beauty with special reference to Dirac's claim can be found in Kragh 1990, pp. 275 – 292 and in McAllister 1990.

[35] Bueno 2005 argues in detail that Dirac's work in the period relied more on physical interpretations of the mathematical formalism than on mathematics itself. Mathematical theories played an important but not an indispensable role to Dirac. On this question, see also Steiner 1998.

7 Postscripts

This paper has focused on the Schrödinger equation and the process that led to the relativistic theory for the electron, the celebrated Dirac equation. Schrödinger was among the physicists who considered Dirac's equation a true masterpiece. In 1953 he wrote that "Dirac's relativistic wave equation still stands out as *the* great success that has been scored in this whole subject [relativity and quantum mechanics]."[36] In spite of their very different styles and conceptions of quantum physics, Schrödinger and Dirac had much in common. Dirac acknowledged his mental similarity to Schrödinger, which he ascribed to a shared appreciation of the importance of mathematical beauty in physics: "Schrödinger and I both had a very strong appreciation of mathematical beauty, and this appreciation of mathematical beauty dominated all our work. It was a sort of act of faith with us that any equations which describe fundamental laws of Nature must have great mathematical beauty in them. It was like a religion with us."[37]

The relations between mathematics and physics in the early phase of relativistic quantum theory were reciprocical and not merely an application of mathematical concepts and methods in the domain of quantum physics. On the contrary, the work of the physicists resulted in many ideas that were eagerly explored by both mathematical physicists and pure mathematicians. The operators and matrices introduced by Dirac eventually gave rise to a whole mathematical industry. Early mathematical studies of the Dirac equation and its connection to spinor theory and Clifford algebra included works by John von Neumann (1928), Bartel L. Van der Waerden (1929), Jan A. Schouten (1929), Alexandre Proca (1930), and Richard Brauer and Hermann Weyl (1935). This case of how physics paid back its debts to mathematics has not received much attention among historians of science.

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[36] Schrödinger 1953, p. 329.
[37] Dirac 1977, p. 136.

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