History of Science Department
University of Aarhus

HELGE KRAGH

The Early Phase of Wave Mechanics 1926-1928

Hosta, No. 5, 2000
Work-in-Progress

Hosta (**H**istory **O**f **S**cience and **T**echnology, **A**arhus) is a series of publications initiated in 2000 at the History of Science Department at the University of Aarhus in order to provide opportunity for historians of science and technology outside the Department to get a glimpse of some of the ongoing or recent works at the Department by reseprachers and advanced students. As most issues contain work in progress, comments to the authors are greatly valued.

Publication is only made electronically on the web site of the Department (www.ifa.au.dk / ivh / hosta / home.dk.htm). The issues can freely be printed as pdf-documents. The web site also contains a full list of issues.

ISSN: 1600-7433

History of Science Department
University of Aarhus
Ny Munkegade, building 521
DK-8000 Aarhus C
Denmark

Revised version of lecture given in June 2000 at DESY, Hamburg, in commemoration of the 75-year anniversary of the advent of quantum mechanics.

The Early Phase of Wave Mechanics, 1926-1928

Helge Kragh

Introduction

Wave mechanics or quantum mechanics? From a modern point of view, the two terms are just different words for the same theory. The terms are used synonymously, or wave mechanics is considered a particular version of quantum mechanics that builds on the notion of the wave function first introduced by Schrödinger in the spring of 1926. In elementary quantum mechanics, as taught to undergraduate students of physics, there is no difference between wave mechanics and quantum mechanics. However, from a historical point of view the matter is rather different. Historians' usage often differs from the one adopted by the physicists, namely, in the sense that wave mechanics is explicitly taken to refer to Schrödinger's theory. This theory, the historians are keen to stress, was originally considered to be very different from the theory proposed by Heisenberg and his colleagues in Göttingen. Heisenberg's theory was sometimes known as matrix mechanics, but also frequently referred to as quantum mechanics. In the early years, quantum mechanics was reserved as a name for versions of the new quantum theory that differed from Schrödinger's theory based on wave functions.

Apart from this terminological digression, the aim of the present paper is to outline the early development of wave mechanics, from Schrödinger's seminal contributions in 1926 to Dirac's no less significant work of 1928 on the wave mechanics of relativistic electrons. The paper includes comments on the following topics:

1. The birth of wave mechanics
2. Schrödinger's 1926 theory and the micro-macro problem
3. Reception and impact
4. Wave mechanics and quantum interpretations
5. Spin, relativity, and the Dirac equation

Of course, the outline given here is only a sketchy and incomplete account of a very complex part of the history of modern physics. The story is discussed in detail by Mehra and Rechenberg (1987), and a general survey of physics in the twentieth century is offered in Kragh (1999). These and other sources mentioned in the reference section can be consulted for further details.

The birth of wave mechanics

Wave mechanics is intimately associated with Erwin Schrödinger, the great Austrian physicist who in 1926 suggested the new theory of atomic phenomena. Schrödinger's route to wave mechanics was very different from that followed by the Göttingen atomic physicists, who were crucially inspired by problems of atomic spectra and Niels Bohr's correspondence principle. Although Schrödinger was not unfamiliar with problems of atomic structure, this was not his main field of work. He did not belong to the spectroscopic tradition that was cultivated by most German physicists and which characterised the approach of the Göttingen-Munich-Copenhagen triangle (Forman and Raman 1969). In a sense, Schrödinger was an outsider who had worked in a variety of fields, ranging from radioactivity over colour theory to general relativity. In the 1920s, he was particularly interested in problems of gas theory and statistical physics. It is important to note that he was not initially searching for a new theory of atomic structure, and that he was not inspired by Werner Heisenberg's matrix theory, neither in a negative nor a positive sense. When wave mechanics appeared, Schrödinger stressed that he "was absolutely unaware of any genetic relationship with Heisenberg." He further explained that "because of the to me very difficult-appearing methods of transcendental algebra and because of the lack of visualisability, I felt deterred by it, if not to say repelled [*abgeschrecht, um nicht zu sagen abgeschossen*]" (Moore 1989, p. 205).

Schrödinger's original inspiration for wave mechanics came from physicists who were no less estranged from the Göttingen-Munich-Copenhagen kind of atomic physics than he was himself. It is well known that he received his decisive inspiration from Louis de Broglie's somewhat speculative theory of matter waves. This theory was ignored by most physicists within the atomic and spectroscopic tradition, but Einstein found it valuable with regard to his own work on the quantum theory of gas statistics, subsequently known as the Bose-Einstein theory. It was also through Einstein that Schrödinger became acquainted with de Broglie's 1924 doctoral dissertation "Recherches sur la théorie des quanta," a work that initially appealed to him because Schrödinger could use it in his own work on gas theory that occupied him in the fall of 1925 (Moore 1989; Bitbol and Darrigol 1992).

Later the same year, Schrödinger realised that there might be more in de Broglie's work than merely an interesting approach to gas theory. He began to concentrate on developing a new wave theory of atomic structure based on the particle-wave dualism postulated by the French physicist (Kragh 1994). However, Schrödinger went a step further than de Broglie, whose theory was truly dualistic, building on waves as well as particles. Schrödinger, on the other side, disregarded the particles and sought for a theory in which particle aspects appeared merely as epiphenomena of waves. At some stage, Schrödinger realised that if atoms were somehow to be described as wave phenomena, the behaviour of waves had to be governed by a wave equation. Consequently he set out to find the kind of equation that he needed. In December 1925 he had found the equation through a series of considerations that were closely based on de Broglie's ideas. Interestingly, this very first Schrödinger wave equation was *not* the one that he reported in his first communication on wave mechanics, as it appeared in *Annalen der Physik* some five months later. Somewhat confusingly, both to contemporary physicists and to later historians, he published three different derivations of his wave equation. None of these derivations mirrored the way he had originally found the equation (Kragh 1982; Mehra and Rechenberg 1987).

The main difference is that whereas the published equation – the later so famous Schrödinger eigenvalue equation for the energy of a hydrogen atom – was non-relativistic, his first version was relativistically invariant. In fact, it was identical to what is today known as the Klein-Gordon equation. Schrödinger now concentrated on solving the equation in order to find the energy eigenvalues or the spectrum of atomic hydrogen. This led him into unknown mathematical territory and during Christmas time 1925 he struggled hard to find his way through the jungle and solve his new equation. As he wrote to Wilhelm Wien in Munich on December 1927: "If only I knew more mathematics!" (Kragh 1982, p. 158).

Schrödinger was familiar with Arnold Sommerfeld's fine structure theory of one-electron atoms, based on the old and by then obsolete Bohr-Sommerfeld quantum theory. He knew that this theory had been brilliantly confirmed by experiments made by Friedrich Paschen and others, and that it therefore gave a correct expression of the hydrogen spectrum. In other words, he realised that his own theory, if of any worth, should result in the same energy values as given by Sommerfeld's fine structure formula. However, after weeks of laborious calculations he was forced to conclude that this was not the case. He did get a fine structure formula almost identical with Sommerfeld's, but not completely so; and "almost" was not good enough. The numerical deviation from Sommerfeld's experimentally confirmed theory – the fine structure splitting was too great by a factor 8/3 – meant of course that the wave equation was incorrect. Naturally, this caused Schrödinger great frustration, in particular because he was unable to locate the error. The equation was promising and so close to the mark, and yet it was wrong.

In this situation, Schrödinger decided to turn to the simple hydrogen atom by means of the non-relativistic approximation of his original wave equation. He made the move not because he believed the new equation would be more fundamental, but because he expected the approximation to be a publishable substitute which would yield a correct answer within its restricted domain. In other words, the famous Schrödinger equation was born in despair, as a substitute for the more fundamental but also, unfortunately, empirically incorrect relativistic equation. Yet the approach paid off, for it was with the more restricted equation that he really got started on developing wave mechanics. It was a simple matter to obtain the non-relativistic wave equation, and already a few days after Christmas he found the energy eigenvalues. To his relief, they agreed with Bohr's 1913 formula for the hydrogen spectrum. As to the relativistic equation, Schrödinger much later told Wolfgang Yourgrau that "My paper ... was withdrawn by me and replaced by the non-relativistic treatment" (Mandelstam and Yourgrau 1958, p. 114).

The derivation of the hydrogen spectrum was an important result and a major reason why quantum physicists took an early interest in Schrödinger's theory. By that time, the spring of 1926, the hydrogen spectrum had already been derived by methods of quantum mechanics, first (and independently) by Wolfgang Pauli and Paul Dirac. However, these derivations were widely seen as inelegant and unsatisfactory, and many physicists were impressed by Schrödinger's method which they considered more natural and fundamental. Moreover, it was only after wave mechanics had appeared, and by using its methods, that the intensities of the hydrogen spectral lines could be easily calculated and a fuller understanding of the spectrum thus achieved. For example, in 1926 Sommerfeld and his student Albrecht Unsöld used

Schrödinger's method to calculate the fine structure of the hydrogen spectrum. Their result agreed nicely with the most recent experimental data.

Although Schrödinger's wave mechanics of atoms was an almost instant success, it was not immediately received favourably by the theorists who had developed the first versions of quantum mechanics, that is, Heisenberg, Pauli, Jordan and Dirac. Their intuitive response to wave mechanics was hostile because they felt that, in the light of the new quantum mechanics, Schrödinger's theory was unnecessary and semi-classical, perhaps even somewhat reactionary. Dirac recalled how "I felt at first a bit hostile towards it ... Why should one go back to the pre-Heisenberg stage when we did not have a quantum mechanics and try to build it up anew?" (Kragh 1990, p. 31). Yet the quantum theorists also realised, and soon came to appreciate, that the theory was in many ways computationally preferable and that problems and results of the Göttingen quantum mechanics (and of Dirac's quantum algebra) could be easily and advantageously translated into the more familiar language of wave mechanics. In addition, Schrödinger's theory gave a straightforward definition of a stationary state in terms of wave functions and was in this respect superior to matrix mechanics. This feature was noted by, and impressed, physicists as diverse as Lorentz, Bohr, and Born. Still, several of the key players of quantum mechanics initially found Schrödinger's alternative to be unacceptable – or, as Pauli bluntly expressed it in a letter to Sommerfeld, crazy or foolish (*verrückt*). Heisenberg too expressed his deep dissatisfaction with what he felt was Schrödinger's attempt to restore some measure of classical visualisability or *Anschaulichkeit*. Dirac's early response is mentioned above.

During the spring of 1926 it became increasingly evident that there must be some intimate connection between Schrödinger's new wave mechanics and the conceptually very different theory of the Göttingen physicists. That this is indeed the case was first proved by Pauli in an important letter that circulated among the inner core of quantum physicists. However, Pauli did not bother to publish his insight and the letter only appeared in print many years after his death (Van der Waerden 1973). The first published version of the equivalence between wave mechanics and quantum mechanics was due to Schrödinger, who clarified the question in his "Über das Verhältnis der Heisenberg-Born-Jordanschen Quantenmechanik zur der meinen," a part of his important series of papers in *Annalen der Physik*. Since that time physicists have agreed that although wave mechanics differs conceptually from matrix mechanics, the two theories are formally or instrumentally equivalent: any empirically significant equation can be translated from one version of quantum mechanics to the other.

The equivalence insight did not imply that the German quantum theorists accepted the physical validity of wave mechanics, only that they found it valuable when it came to calculating the matrix elements given by the original version of quantum mechanics. This was the opinion of Heisenberg and also of Jordan, who declared that Schrödinger's theory was devoid of any physical meaning. According to Jordan, it should at most be considered a mathematical approach that might be useful in calculational aspects of the real quantum mechanics, that is, Heisenberg's matrix mechanis. This early, instinctive opposition to Schrödinger's theory was rooted not only in differences with regard to the physical meaning of quantum mechanics, including the problem of *Anschaulichkeit*; it probably also had a social aspect, in the sense that Heisenberg and his allies were reluctant to admit anybody else into the territory that they had opened up and considered their own. Physics in the 1920s was no less competitive than physics today, and Heisenberg wanted to maintain his priority and make sure that his pioneering theory was not overshadowed by the alternative proposed by his Austrian colleague (Beller 1999).

With regard to the proofs of equivalence between matrix and wave mechanics it may be added that, much later, philosophers of science have objected that the proofs are in fact invalid and that the alleged equivalence is a "myth" (Muller 1997). Whatever the validity of this claim, it is undoubtedly interesting from a philosophical and foundational point of view. However, from a historical point of view it is of no particular relevance. Whether the equivalence proofs can be justified or not, it is a fact of history that they were accepted as proofs by the physicists who contributed to the pioneering phase of quantum mechanics.

The micro-macro problem in wave mechanics

Among Schrödinger's seminal papers of 1926 were not only his quintet of fundamental papers in *Annalen der Physik* but also a less well known paper which he published in *Die Naturwissenschaften* in July. In this paper, entitled "Der stetige Übergang von der Mikro- zur Makromechanik," he examined the relationship between a wave-mechanical description of microscopic and macroscopic objects. At the time of publication, Max Born's probabilistic interpretation of the wave function (ψ) had not yet appeared. Schrödinger still maintained his electromagnetic interpretation in which the electron was pictured as an entity smeared out in space, with a distribution of the electrical charge density given by $e\psi^*\psi$ (where ψ^* is the conjugate of ψ).

The main claim of Schrödinger's paper was that the electron can be presented by a wave packet of proper vibrations and is therefore, in a sense, nothing but a wave packet. The claim relied on his analysis of the simple harmonic oscillator, where Schrödinger concluded that the wave packet would remain compact and not spread out as time goes on. Moreover, he believed that the result would be valid also for electrons moving in atomic orbits. As he wrote, "Wave groups can be constructed which move round highly quantised Kepler ellipses and are the representation by wave mechanics of the hydrogen atom." However, even before having submitted his paper to *Die Naturwissenschaften*, Schrödinger became engaged in an interesting correspondence with the 73-year-old Dutch master of theoretical physics, Hendrik A. Lorentz. In spite of his advanced age, Lorentz was deeply interested in wave mechanics, and he pointed out to Schrödinger that he was not justified in extending his analysis of the harmonic oscillator to atomic orbits. "In the present form of your theory," he wrote to Schrödinger on June 19, "you will be unable to construct wave packets that can represent electrons moving in Bohr orbits" (Kragh and Carazza 2000, p. 46). We do not know how Schrödinger responded, but it is likely that Lorentz's arguments contributed to the change in Schrödinger's ontological ideas that occurred in the summer of 1926. Now, he no longer emphasised the wave picture of particles and concluded that his original belief in the ontological primacy of waves was not an integral part of wave mechanics.

The shift in Schrödinger's ontology is interesting, not least in the light of what happened in the spring of 1927, when Heisenberg published his famous uncertainty principle. It is well known that this paper was indebted to discussions with Bohr and Pauli, and also to the recently formulated transformation theory of Dirac and Jordan; it is less well known that it also relied crucially on Schrödinger's wave mechanics, in particular on his paper on the transition from micro- to macromechanics. Yet this connection was emphasised by Heisenberg in a letter of February 1927 and is also evident from the second half of the paper on quantum uncertainties. Here, Heisenberg offered a critical analysis of Schrödinger's position, concluding that, in general, "a wave packet spreads out in the course of time over the whole immediate neighbourhood of the atom," and that an atomic electron can therefore not be represented by a wave packet. According to Heisenberg, Schrödinger's wave packet had to be replaced by Born's probability density, given by $\psi^*\psi$. It followed from Heisenberg's analysis that a wave packet will usually diffuse indefinitely, contrary to what is known about the localisation of electrons and other particles. This was a strong argument against Schrödinger's original position, but hardly one that caused him sleepless nights. As mentioned, Schrödinger was already aware of the objection from his correspondence with Lorentz and therefore can neither have been surprised by Heisenberg's criticism nor disagreed with it.

Reception and impact

Before proceeding with the interpretation issue, I would like to add a little more about how Schrödinger's wave theory of atoms was received by contemporary physicists. In spite of the early hostility of German quantum theorists, in general wave mechanics was a tremendous success. It was received enthusiastically not only by conservative physicists (such as Wilhelm Wien and Erwin Madelung) but also by the majority of the young generation of quantum theorists in England, the United States, Scandinavia, and even Germany. In fact, it was only with Schrödinger's theory that quantum mechanics really took off and became a highly successful theory which was cultivated by physicists around the world and not only by a small group of German theorists. By 1927, wave mechanics was considered the real quantum mechanics by almost everybody, while matrix mechanics was more or less ignored. Clearly, it was Schrödinger's theory that set the agenda for atomic physics, not the matrix theory of Heisenberg, Born and Jordan.

Einstein was among the many who initially were favourably inclined toward wave mechanics. However, contrary to most other physicists he soon reached the conclusion that Schrödinger's theory was no less incorrect than Heisenberg's. None of the theories "smell like reality," as he expressed it. Given the traditional view on Schrödinger's position with regard to causality, it is interesting to note that Einstein dismissed wave mechanics because, as he wrote to Paul Ehrenfest in 1927, "it is non-causal and altogether too primitive" (Kragh and Carazza 2000, p. 53).

Einstein did not represent the physics community, and his rejection of wave mechanics was not typical. On the contrary, the theory was received with such enthusiasm that the world-wide victory of Schrödinger's method was considered alarming by Heisenberg and Jordan in particular. Almost all progress in quantum mechanics took place by means of wave mechanics, and even physicists who had initially contributed to the matrix method turned to the wave theory. They often translated the results obtained within the matrix framework into the language of wave mechanics. By 1928, the computational or instrumentalist victory of wave mechanics over matrix mechanics was almost complete. It was realised that, in most cases, matrix tools simply could not compete with the powerful tools based on Schrödinger's wave theory.

However, although Schrödinger's methods thus won an overwhelming victory, when it came to the physics – the interpretation of quantum mechanics – most physicist declined to follow Schrödinger. They tended to accept the version adopted by his opponents within what would be later known as the Copenhagen school or interpretation. In fact, it can be argued that the rapid development of the Bohr-Heisenberg-Born quantum philosophy was to a large extent stimulated by, and can even be seen as a response to, the threatening popularity of Schrödinger's approach (Beller 1999).

Quantum interpretations

Schrödinger's theory soon came to be considered "conservative" and in opposition to the victorious Copenhagen interpretation. However, it is important to note that it played a very important, positive role in the construction of the physical interpretation of quantum mechanics that occurred in 1926-27. This phase of quantum mechanics can be said to have started with Max Born's famous interpretation of the wave function from the summer of 1926.

It is sometimes stated that Born's theoretical innovation, based on his analysis of the collision problem, depended on an interpretation strictly in terms of particles. Yet a closer look at Born's papers reveals how strongly he was indebted to Schrödinger's wave ideas, without which he might not have reached his insight. Born's first collision paper – like the second one entitled "Quantenmechanik der Stossvorgänge" – was not written in opposition to Schrödinger's ideas. On the contrary, Born praised the new wave mechanics, including its interpretative possibilities, and he characterised Schrödinger's theory as physically more meaningful than matrix mechanics; the theory was, he wrote, "the deepest formulation of the quantum laws."

In this paper, Born did not connect the wave function with a probability of position. This he only did in his second paper, and it is also only in this second paper that we find the beginning of Born's opposition to wave mechanics. In a letter from Born to Schrödinger of November 6, 1926, we have an interesting document that illuminates not only Born's initial enthusiasm but also the change in opinion that occurred in the fall of that year. Born wrote: "You know that immediately after the appearance of your first works I expressed very strongly my enthusiasm for your conceptions in my treatise. Heisenberg from the beginning did not share my opinion that your wave mechanics is more physically meaningful than our quantum mechanics; yet the treatment of the simple aperiodic processes (collisions) led me initially to believe in the superiority of your point of view." After this unqualified praise, he added: "In the meantime, I found myself again in agreement with Heisenberg's position" (Beller 1999, p. 44).

The probabilistic interpretation and the whole problem of determinism only became a theme in the second paper. Here, Born interpreted the absolute square of the ψ-function as the probability of an atom being in a stationary state. It took a little longer until the full version of the probability interpretation was stated, a merit that belongs to Pauli. In an important letter to Heisenberg of October 19, 1926, Pauli first proposed the currently accepted interpretation of the wave function, namely, that $\psi^*\psi$ is a probability density in general and not connected with stationary states only (Pauli 1979).

Born's probabilistic interpretation was quickly developed by Dirac and Jordan, who incorporated it into their general transformation theory. It played a key role in the emergence of the new philosophy of physics because it introduced explicitly into microphysics an irreducible element of probability. Although this implied a change in the meaning of natural laws, Born was careful to stress that it did not do away with causality. He famously argued that "The motion of particles conforms to the laws of probability, but the probability itself is propagated in accordance with the law of causality" (Kragh 1999, p. 166). The highlights of the new quantum philosophy that emerged in 1927 were undoubtedly Heisenberg's uncertainty principle and Bohr's principle of complementarity. I have already commented on the close connection between wave mechanics and Heisenberg's uncertainty paper, and will add a few comments concerning the relation between Schrödinger's wave mechanics and Bohr's understanding of quantum mechanics.

Bohr's original enunciation of his complementarity philosophy, as given in his famous Como lecture of 1927, differed in certain respects from the later instrumentalist or positivist approach that is traditionally associated with the Copenhagen interpretation. Also, it relied closely on notions from wave mechanics such as wave packets and superpositions of waves. In spite of his disagreements with Schrödinger, especially over the significance of discontinuous quantum jumps, Bohr fully realised the profound physical significance of wave mechanics, which he made an indispensable part of his quantum philosophy of observation. Just like Born had initially praised Schrödinger's theory, so Bohr described in a letter to Ralph Fowler from the fall of 1926 the wave-mechanical method as "beautiful." He praised to the British physicist "the advantage which the wave-mechanics in certain respects exhibits when compared with the matrix method" (Beller 1999, p. 129).

In general, and to make a long story short, it is fair to say that the philosophical divide in quantum physics was not there originally. Wave mechanics was of seminal importance in the Copenhagen interpretation, a conception of physics that would hardly have come into existence had it not been for Schrödinger's theory. Moreover, the issue of causality and determinism only became an issue after 1927, then with Schrödinger being classified as a conservative who stubbornly resisted the victorious Heisenberg-Bohr interpretation. Recent scholarship in history of physics indicates that this classification is unfair and the result of a historical reconstruction with roots back to the late 1920s (Bitbol 1996).

Spin, relativity, and wave mechanics

After this brief excursion to the interpretative aspects of quantum mechanics I shall return to the question of relativity. As mentioned, this question was very much in the foreground when Schrödinger originally sought for a quantum wave equation. What he was seeking was not only a new kind of quantum theory, but one that satisfied the principle of relativity as well. Still in the summer of 1926, about the time when Born developed his probabilistic understanding of quantum mechanics, the case of the hydrogen atom had only been partially solved. Although Heisenberg and Jordan had succeeded to derive the fine structure and incorporate the spin, this was only possible in a somewhat ad hoc manner and by using the first-order relativistic approximation. How to derive the exact Sommerfeld formula was still a mystery. In other words, one of the major successes of the old quantum theory still remained outside the power of the new quantum mechanics.

Schrödinger's original candidate for a wave equation was a "translation" of the classical-relativistic equation of motion and hence of the second order in both the space and time derivatives of the wave function. It was first published in the late spring of 1926 by the Swedish physicist Oskar Klein in connection with a heroic but failed attempt to extend Schrödinger's theory to an even more ambitious, unified theory framed in five dimensions. Within a few months the equation was discussed by several other physicists, including de Broglie in France, Walter Gordon in Germany, Vladimir Fock in Russia, and Johannes Kudar from Hungary. No wonder that Pauli referred to the relativistic wave equation as "the equation with the many fathers" (Kragh 1984). However, the impressive number of fathers could not hide the fact that, from an empirical point of view, the promising and appealing equation was a failure. As Schrödinger knew only too well, it did not lead to the correct fine structure formula. Moreover, the Klein-Gordon equation – as it soon became known – did not incorporate the electron's spin, as the correct wave equation was expected to do. The problem of a relativistic extension of the Schrödinger equation was widely known in the physics community, and it was also widely suspected that somehow the problem was intimately connected with the spin problem. However, in the years of 1926 and 1927 no satisfactory solution to the problems was found. The solution came in early 1928. Just for once, it did not come from German-speaking physicists, but from a physicist in Cambridge, England.

When Paul Dirac attacked the problem in late 1927, the inadequacy of the Klein-Gordon equation was not what worried him. His concern was not with experimental anomalies, but with the general principles of quantum mechanics. These, he reasoned, should apply also to the relativistic extension of the Schrödinger wave equation. According to Dirac, this implied that the quantum properties of the equation should transform in conformance with the transformation theory of quantum mechanics that he had formulated half a year earlier. The invariance requirement meant that the equation of motion had to be of the same general form as the Schrödinger equation, that is, linear in the time derivative of the wave function. Mathematically formulated, it must satisfy the expression $H\psi = -(\mathrm{i}h/2\pi)\ \ \psi/\ \ t$. This insight, coupled with the requirement of Lorentz invariance, led Dirac into serious mathematical problems. Unaware of the earlier work of mathematicians, he handled the problems in his own, highly original way by means of a clever linearisation procedure inspired by the theory of spin matrices that Pauli had introduced in the spring of 1927 (Kragh 1990). Only at this stage did he realise that the electron's spin was part of his still unfinished theory. Originally he had not thought of the spin and in fact considered, for reasons of simplicity, a hypothetical spin-zero electron. He now discovered that the correct spin sprang naturally and unexpectedly from the new linear equation.

Of course, physicists immediately asked if Dirac's linear wave equation could do what the Klein-Gordon equation could not do, namely, lead to the exact Sommerfeld fine structure formula and thus solve the old hydrogen puzzle. Dirac did not himself solve the problem, but soon after the appearance of his paper on "The quantum theory of the electron" the complete solution of the Dirac equation for a hydrogen atom was independently obtained by Gordon in Germany and George Darwin in England. The result was as Dirac expected – or hoped – namely, exactly the same equation as the one Sommerfeld had derived twelve years earlier on a very different basis and without knowing about the electron's spin. In order to perform his magic, Dirac had to extend Schrödinger's scalar wave equation into a four-component function the physical meaning of which was not clear. It would only be clarified a couple of years later with Dirac's audacious hypothesis of antiparticles, one of the most far-reaching ideas ever in the history of physics.

I shall not proceed with the story of the Dirac equation but only note that with Dirac's theory of 1928 a chapter in the history of quantum physics closed and the heroic phase of the fabrication of quantum mechanics came to an end. Of course, this did not imply that there were no more fundamental problems in quantum mechanics, it only meant that physicists could now be reasonably confident that they were on the right track. They knew the fundamental equations and the rules of the quantum game. Much of the success, including Dirac's triumph, relied heavily on Schrödinger's wave mechanics, which became further established as *the* working tool of the new theory. Among its many successes should also be mentioned another theory of 1928, the pioneering work of George Gamow in which he applied wave mechanics in order to explain alpha radioactivity. Gamow thereby proved that the validity of Schrödinger's theory could be extended to the tiny and still mysterious atomic nucleus (Stuewer 1986). And this was not all. His work served as the immediate foundation for the first theories of nuclear astrophysics, thereby extending the power of wave mechanics to the realms of the stars and, eventually, the early universe (Kragh 1996).

Conclusion

Although quantum mechanics was to some extent the collaborative result of a European network of physicists, the pioneering contributions came from a small group of individuals. Foremost among these were Heisenberg, Schrödinger, and Dirac. Of course, many other physicists made very important contributions, such as Pauli, Jordan, Born, Klein, Ehrenfest, Fritz London, and Hendrik Kramers – to mention only a few. Yet it is probably fair to say that quantum mechanics was primarily created through the works of the three mentioned physicists, a German, an Austrian, and an Englishman. It is also fair to say that after 1926 Schrödinger's wave mechanics formed the backbone of the continual progress in quantum theory and its many applications. Indeed, given the stormy development of physics in the twentieth century it is quite remarkable that when students of physics are today introduced to the secrets of quantum theory they start with the very same equation which Schrödinger first presented in *Annalen der Physik* in the spring of 1926. It is a fact that speaks to the greatness of Schrödinger's work.

We celebrate this year the centenary of Max Planck's discovery of the quantum discontinuity and also the 75-year anniversary of quantum mechanics. Next year, in 2001, we will celebrate another centennial, that of the Nobel prize. The history of the Nobel institution reflects in many ways the highlights of physics throughout the twentieth century, including the early development of quantum mechanics. Physicists recognised the fundamental nature of quantum mechanics at an early stage, but the Nobel institution is by nature conservative and was at the time suspicious of new theories that were not yet solidly confirmed by experiments. For example, when Heisenberg and Schrödinger were first nominated for a prize, in 1929, the physics committee judged that their theories "have not as yet given rise to any new discovery of a more fundamental nature" (Kragh 1999, p. 433). The statement was of course a major misjudgement, but it reflected the crucial role that "discovery" played (and still plays) within the Nobel institution. This role can be traced directly to the will of Alfred Nobel.

It took three more years before the learned professors in Stockholm realised that quantum mechanics was by itself a fundamental discovery that deserved to be rewarded. As a result, in December 1933 Heisenberg, Schrödinger and Dirac were invited to the Swedish capital to receive the Nobel prizes for the years 1932 and 1933 (the 1932 physics prize had been reserved). Whereas Heisenberg received the full 1932 prize "for the creation of quantum mechanics," Schrödinger and Dirac shared the 1933 prize "for the discovery of new productive forms of atomic theory." From the point of view of later physics, to call Schrödinger's wave mechanics a "new productive form[s] of atomic theory" appears as somewhat of an understatement. One does not have to be a quantum expert in order to realise that it was much more than that.

References

Beller, M. (1999), *Quantum Dialogue: The Making of a Revolution* (Chicago: University of Chicago Press).

Bitbol, M. (1996), *Schrödinger's Philosophy of Quantum Mechanics* (Dordrecht: Kluwer).

Bitbol, M. and Darrigol, O. (eds.), *Erwin Schrödinger: Philosophy and the Birth of Quantum Mechanics* (Paris: Editions Frontières).

Forman, P. and Raman, V. V. (1969), "Why was it Schrödinger who developed de Broglie's ideas?" *Historical Studies in the Physical Sciences 1*, 291-314.

Kragh, H. (1982), "Erwin Schrödinger and the wave equation: The crucial phase," *Centaurus 26*, 154-197.

Kragh, H. (1984), "Equation with many fathers: The Klein-Gordon equation in 1926," *American Journal of Physics 52*, 1024-1033.

Kragh, H. (1990), *Dirac. A Scientific Biography* (Cambridge: Cambridge University Press).

Kragh, H. (1994), "The heritage of Louis de Broglie in the works of Schrödinger and other theoreticians," 65-78 in *La Découverte des Ondes de Matière* (Paris: Académie des Sciences).

Kragh, H. (1996), *Cosmology and Controversy: The Historical Development of Two Theories of the Universe* (Princeton: Princeton University Press).

Kragh, H. (1999), *Quantum Generations: A History of Physics in the Twentieth Century* (Princeton: Princeton University Press).

Kragh, H. and Carazza, B. (2000), "Classical behavior of macroscopic bodies from quantum principles: Early discussions," *Archive for History of Exact Sciences 55*, 43-56.

Mandelstam, S. and Yourgrau, W. (1958), *Variational Principles in Dynamics and Quantum Theory* (London: Isaac Pitman & Sons).

Mehra, J. and Rechenberg, H. (1987), *The Historical Development of Quantum Theory*, Vol. 5 (Berlin: Springer-Verlag).

Moore, W. (1989), *Schrödinger: Life and Thought* (Cambridge: Cambridge University Press).

Muller, F. A. (1997), "The equivalence myth of quantum mechanics," *Studies in the History and Philosophy of Modern Physics 28*, 35-61, 219-247.

Pauli, W. (1979), *Wolfgang Pauli. Wissenschaftlicher Briefwechsel*, Vol. 1 (New York: Springer-Verlag).

Stuewer, R. (1986), "Gamow's theory of alpha-decay," 147-186 in E. Ullmann-Margalit (ed.), *The Kaleidoscope of Science* (Dordrecht: Reidel).

Van der Waerden, B. L. (1973), "From matrix mechanics and wave mechanics to unified quantum mechanics," 276-293 in J. Mehra (ed.), *The Physicist's Conception of Nature* (Dordrecht: Reidel).