STUDIES IN THE FOUNDATIONS OF QUANTUM MECHANICS

Edited by

PATRICK SUPPES

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PREFACE

The papers contained in the present volume are, mainly, written versions of reports given at the Stanford Seminar on the Foundations of Quantum Mechanics during the years 1975-1978. To a large extent, the authors of these papers are the same as the authors who appeared in the previous volume, Logic and Probability in Quantum Mechanics (P. Suppes, Ed., Reidel, 1976). That volume covered the years 1972-1974 and was longer than this one; more of the papers originated outside the seminar. In the present case, all of the papers originated in the seminar with the exception of Demopoulos's paper, the commentary on it by Bub, and Wessels's paper, which was generated initially as a commentary on MacKinnon's paper.

From a formal academic standpoint, Nancy Cartwright and I share the responsibility of organizing the seminar, but it is fair to say that other regular participants take as much actual responsibility for its organization as do the two of us.

The twelve articles that appear in the present volume have been arranged in a semiconceptual way rather than in chronological order. The opening paper by MacKinnon contains a long historical analysis of the rise and fall of the Schrödinger interpretation of quantum mechanics. Wessels's, in turn, provides a detailed commentary on MacKinnon's main thesis about Schrödinger and offers an alternative analysis.

Noyes's article is concerned with an operational analysis of the double-slit experiment, which has been a subject of discussion in the seminar over several years. Given the intensity of our discussion of Noyes's paper and the continued controversies we have had over the double-slit experiment, future seminars should produce further reports on this matter. Noyes's paper offers a good sample of the issues that have concerned us.

Cartwright provides an interesting analysis of what it means to measure position probabilities and how the Born interpretation needs to be squared with the standard theorems of scattering theory. Her concern is to give a realistic theory of how measurement can actually take place and be consistent with the Born interpretation. The central point of her analysis is that measurement requires an exchange of energy between the object measured and the detector doing the measurement, and so we cannot simply speak abstractly of measuring position.

The most controversial paper in the volume is the one by Demopoulos on locality and the algebraic structure of quantum mechanics. Demopoulos attempts to show that locality, as defined by Bell and others, implies the algebraic homomorphism condition concerning the mapping of closed linear subspaces of a separable Hilbert space onto the two-element Boolean algebra, a matter which is itself closely connected with Gleason's well-known theorem that there are no two-valued measures on the partial Boolean algebra of the closed linear subspaces of a separable Hilbert space of three or more dimensions. Humphreys in his note on Demopoulos's paper shows that Demopoulos's claims are too strong when he asserts that there is no Boolean representation of a certain class of maximal magnitudes that arise in a natural way in Hilbert-space formalism. Bub in his short commentary shows that Demopoulos's argument that Bell's locality condition implies the existence of a homomorphism from the set of closed linear subspaces into a Boolean algebra, and thus into a two-element Boolean algebra, is incorrect. Demopoulos appends a Section V, entitled "Addenda," to his article to respond to these criticisms. Thus, after reading the comments by Humphreys and Bub, the reader should return to page 137, on which Section V of Demopoulos's paper begins.

Norman examines the consequences of assuming spontaneous projection postulates, which lead to a theory of spontaneous reduction of a system that is undisturbed by measurement. He shows that in a certain definite sense such a theory cannot have the same empirical content as standard quantum mechanics. Shiveley picks up on some of the earlier discussions in the seminar, especially a presentation given by Janet Beehner in 1977, to display an example of a nondistributive logic that avoids the undesirable properties of the usual special case.

Suppes and Zanotti continue their work reported in the earlier volume on a probabilistic analysis of hidden variable theories. In the present article they give a general argument requiring no detailed computations from quantum mechanics. They use de Finetti's principle of exchangeability and the principle of identity of conditional distributions under a hidden variable to show that there can be no hidden variable theory satisfying these two principles, together with negative correlation of observable variables such as spin, and locality in the sense of conditional statistical independence.

Domotor outlines a general program for developing a quantum decision theory. This is one of the few papers in the literature to combine statistical decision theory and the kind of probabilistic structures that arise in standard quantum mechanics.

The final article is really a bibliography on quantum logic, put together by Janet Beehner, who has given reports on quantum logic several times in the seminar. Her bibliography covers 381 items, consisting of either articles or books. It includes relevant materials from Donald Nilson's bibliography, published in the 1976 volume.

In preparing this volume for publication, I am very much indebted to Mrs. Dianne Kanerva, who not only was responsible for much of the editing and the preparation of the index, but also for the preparation of the camera-ready copy. I also thank Mrs. Marguerite Shaw for her assistance in various aspects of preparing the volume.

Patrick Suppes

CONTENTS

THE RISE AND FALL OF THE SCHRÖDINGER INTERPRETATION

Edward MacKinnon
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I. THE ORIGIN OF WAVE MECHANICS

The formal development of Schrödinger's wave mechanics is familiar not only from recent historical accounts (Jammer, 1966, pp. 255-280; Scott, 1967; Gerber, 1969; Hund, 1974, pp. 147-153; Medicus, 1974, pp. 38-45), but even more from the fact that his methods and calculations have become an integral part of modern physics. Rather than give one more summary account of this formalism, we shall concentrate almost exclusively on the problems Schrödinger encountered in his attempts to give this formalism an adequate and consistent physical interpretation. Before doing this, however, we should situate Schrödinger's work with respect to the developments considered elsewhere.[1]

Schrödinger's wave mechanics was developed in a series of four papers, "Quantisierung als Eigenwertproblem" (1926b, 1926c, 1926d, 1926e; following Schrödinger's usage, these will be referred to as Q1, Q2, Q3, and Q4), and in two articles separate from this series but written during the same period.[2] Since neither these papers nor the available unpublished material explains how Schrödinger first formulated his wave equation, there have been different conjectures on this point. According to the most widely held view,[3] the fundamental equation of wave mechanics was suggested by Hamilton's analogy between ordinary mechanics and geometrical optics and by the further idea that, just as geometrical optics has to be replaced by a wave theory to treat fine-scale optical phenomena, so de Broglie's 'ray mechanics' had to be replaced by a quantum wave theory when treating phenomena of atomic dimensions. Though this interpretation stems from Q2 and from Schrödinger's later (1928) redevelopment of wave mechanics, it runs into serious difficulties when

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considered as a chronological rather than a pedagogical account.

A different interpretation was suggested by Martin Klein (1964) and is being developed by Linda Wessels.[4] In essence, this is the contention that Schrödinger's initial ideas on wave mechanics stemmed from his work on a quantum theory of an ideal gas and that the adaptation and extension of the analogy Hamilton established between mechanics and optics was a later rationalization of ideas first developed in a more intuitive way. I find this a much more plausible interpretation but will defer to Wessels and others for detailed arguments on the comparative merits of the two interpretations.

My purpose here is not so much to explain where and how Schrödinger got his original interpretation, as to consider the changing use made of it in the development of his articles. Since, unfortunately, Schrödinger has left almost nothing in the way of an autobiographical account of his moves and their motives, it is necessary to have recourse to conjectures. The conjecture that supplies an interpretative framework for the account that follows can be stated fairly succinctly. Schrödinger's work on gas theory and his adaptation of de Broglie's concept of matter waves led him to the idea that particles could be represented as singularities in wave packets. After some searching, Schrödinger developed a wave equation to fit this assumption. When this yielded correct results for the hydrogen spectrum, Schrödinger knew that he was on the threshold of a major breakthrough.

In spite of this success, the interpretation of electrons as singularities in wave packets presented formidable difficulties which Schrödinger was not able to overcome. The strategy he followed in this impasse was quite similar to the strategy Heisenberg had employed some six months earlier when he ran into corresponding difficulties in developing quantum mechanics.[5] Schrödinger simply presented his new equation without attempting to justify it, worked out particular solutions, and showed how they could be interpreted in terms of observable phenomena. In Q1 and Q2 the basic formalism was developed in such a way that its intelligibility did not depend on an acceptance of the wave interpretation. Schrödinger certainly did not suppress this interpretation the way Heisenberg had suppressed his reliance on the virtual oscillator model. However, the problem of a model adequate to this new formalism was removed from a foundational role, relegated to the status of a side issue, and treated in an article that was not a part of the basic quantization series.

After doing this, Schrödinger proved to his own satisfaction--or, perhaps, to his own dissatisfaction--that his mathematical formulation was equivalent to Heisenberg's. This, I believe, precipitated something of a personal crisis. Unless Schrödinger's new theory could be shown to have a physical interpretation different from and superior to the interpretation of Heisenberg's formalism, then Heisenberg, rather than Schrödinger, would have to be accepted as the architect of the new age in quantum physics. Accordingly, in the third, and especially in the fourth, article of his quantization series, Schrödinger endeavored to develop and justify a physical interpretation of his mathematical formalism. This was a new interpretation that differed in significant respects from the interpretation of electrons as wave packets that Schrödinger had originally relied on. This new interpretation was unsatisfactory, and Schrödinger's commitment to it was ultimately self-defeating. Rather than accept the radical reinterpretation of his own results that Born, Bohr, and others developed, Schrödinger effectively opted out of the developments to which he had made such a prominent contribution. This interpretation, it must be admitted, is unprecedented and quite conjectural. Nevertheless, I believe that it does lend a plausible coherence to the account that follows.

It seems that Schrödinger first became acquainted with de Broglie's ideas through a suggestion of Pieter Debye,[6] then his colleague in Zurich, that he give a seminar on de Broglie's thesis. Einstein's enthusiastic endorsement was the decisive factor inducing both men to take de Broglie's ideas seriously. Schrödinger had been working on the quantum theory of ideal gases. After reading Einstein's papers, Schrödinger redeveloped Einstein's theory, introducing significant modifications in both the interpretation of an ideal gas and the interpretation of the particles that compose it. This is the only aspect of Schrödinger's gas theory that we will consider.

Though influenced by Einstein and de Broglie, Schrödinger did not accept either of their versions of the light-quantum hypothesis or gas theory. It is more natural, Schrödinger (1926a) argued, to apply the new statistics to the degrees of freedom of the black-box cavity itself, or the ether resonations, than to light particles. Similarly, instead of assigning energies to molecules in a gas, one can quantize modes of vibration, assigning to the s^{th} mode the possible energies, 0, ε_s, $2\varepsilon_s$, $3\varepsilon_s$, ..., $n\varepsilon_s$, depending on whether it is occupied by 0, 1, 2, ..., n molecules. Though this is similar to de Broglie's idea of many molecules sharing the same phase wave, there are, nevertheless, some significant differences. The de Broglie waves move at a velocity of v^2/c relative to the molecular velocity v; the Schrödinger waves are stationary. The de Broglie waves are coupled with particles in a dualistic system; the Schrödinger waves are introduced to replace particles. At this stage, however, Schrödinger presented the replacement as a suggestion rather than as a developed doctrine, a way of applying the new statistics to the bearers of the energy states rather than to the energy states themselves.

Schrödinger was treating an enclosed gas as a unified whole, almost as if it were an organic unit. This, in turn, necessitated a reinterpretation of the molecules that make up a gas. Schrödinger's reinterpretation hinged on the way he adapted de Broglie's ideas. A molecule of rest mass m and velocity $v = c\beta$ is, as he interpreted de Broglie, nothing but a signal, the crest of the froth (Schaumkamm) of a wave system whose frequency ν lies in the neighborhood of $\nu = mc^2/h\sqrt{1-\beta^2}$ and whose phase velocity u is governed by the dispersion law $u = c/\beta = c^2/v$. These are not the de Broglie relativistic phase waves but the waves that make up a group in a dispersive medium. Even the relativistic formulation itself simply serves as a bridge between de Broglie's work and his own. Once introduced, it is quickly replaced by a different formulation, one in which molecules are treated nonrelativistically and radiation relativistically (but not the way de Broglie treated it). With this, Schrödinger is able to reproduce Einstein's ideal gas results, interpreting them in terms of vibration states of the gas as a whole rather than as the statistical interaction of individual molecules. Though the mathematics is equivalent to Einstein's, the interpretation of molecules in terms of waves in a dispersive medium presents a problem to which

Schrödinger returned in the concluding section of his article.

The question considered there is whether it is possible to treat molecules or light quanta in terms of the interference of plane waves. A superposition of a great number of such waves with a common wave normal and with near-neighboring frequencies can serve as a signal that is sharply localized. But, Schrödinger wondered, will it stay localized, contracted in a small space, especially in the case of three-dimensional motion? This is the problem the paper raised but did not resolve. Schrödinger cited some earlier work of Debye and von Laue indicating that an affirmative answer is possible only when the frequency difference is infinitesimal. In other cases, the singularity passes through a focal point and then disperses. Schrödinger's (1926a) conclusion is worth quoting: "If one can avoid this consequence through a quantum theoretical modification of the classical wave law then it appears that a path is prepared for the solution of the light-quantum dilemma" (p. 101).

This paper was finished some six weeks before the first paper in Schrödinger's series on wave mechanics. It clearly shows that Schrödinger was favorably disposed toward a physical interpretation of particles as wave packets but acutely aware of the dispersion difficulties this presented. It undoubtedly seemed prudent to minimize the physical interpretation and rely on the development of an adequate mathematical formalism. Yet this, too, presented formidable difficulties. Schrödinger later told Dirac, with whom he shared the Nobel Prize, that he spent months trying to develop a relativistic wave equation but despaired when the equations he developed would not yield the correct results for the hydrogen atom.[7] Then he switched to a nonrelativistic equation, probably intending it as an approximation to the relativistic formulation that the situation seemed to require. This approximate equation quickly proved an unprecedented success in supplying a unified solution for problems that could only be solved piecemeal, if at all, by older formulations.

This paradoxical situation explains, I believe, the peculiar way in which Q1 and Q2 are developed. Schrödinger had an equation that worked but could not give any satisfactory reason, apart from this success, why this equation should obtain at all. It could not be developed from general principles, though some principles from classical physics eventually supplied a suggestive point of departure. Nor was it in accord with the principles of relativity that should apply to an exact solution. On the other hand, the equation could not be given the type of semi-intuitive physical justification that physicists regularly employ in setting up differential equations to cover physical situations. This would involve reliance on a novel interpretation of particles that even Schrödinger, then the sole supporter of this view, recognized as unproved and perhaps inconsistent. The best way out seemed to be to present the equation and postpone its justification. This is essentially what Schrödinger did in Q1.

II. THE DEVELOPMENT OF WAVE MECHANICS

Schrödinger's first quantization paper (Q1) attempted to show, for the simple case of the nonrelativistic and unperturbed hydrogen atom, that the usual quantization procedure can be replaced by another postulate in which integers are not assumed but follow in a natural way, as, for example, in the number of nodes of a vibrating string. This hint of a wave analogy, together with a brief allusion to vibrations at the conclusion of the article, supplies the only textual basis for interpreting Schrödinger's new equation as a wave equation. The emphasis in the first two communications is on the fact that the new method of quantizing yields integers in a natural way.

The usual form of the quantum conditions is connected with the Hamilton-Jacobi partial differential equation,

$$H(q, \partial S/\partial q) = E \,. \quad (1)$$

A solution for (1) is sought that is a sum of functions, each of a single one of the independent variables, q. Schrödinger modified this by introducing a new unknown ψ through the substitution

$$S = K \log \psi \,. \quad (2)$$

Here K is a constant that, like S, has the dimensions of action (or of h). The basic advantage resulting from this substitution is that ψ will now appear as a product of the functions of the individual coordinates rather than as a sum. With this substitution, (2) takes the form

$$H(q, K/\psi, \partial\psi/\partial q) = E\psi. \qquad (3)$$

Before developing any particular solutions for (3), Q1 stipulates the conditions that ψ must fulfill (Schrödinger, 1926b).[8] It must be real over the whole of configuration space, unique valued, finite, continuous, and twice differentiable.

These conditions of adequacy are simply presented with no justification, yet they deserve some comment. The most notable feature about them is that they are conditions imposed on ψ considered as a mathematical function. The physical interpretation to be accorded ψ is only indirectly operative through the stipulation that ψ must be real. The reason for this stipulation is undoubtedly the reason given in later parts of the series: If the ψ-function corresponds to something physically real, such as a matter wave, then it should be real rather than imaginary or complex. The force of this reason is somewhat blunted, however, by the fact that the ψ-function is in configuration space (a product space with three coordinates for each particle) rather than ordinary space. Though this is listed as a basic criterion for the acceptability of the ψ-function, it is, in fact, never fulfilled in this or any other article in the quantization series.

The rest of Q1 has a rather chiaroscuro quality. The solution of the Schrödinger equation, to use the now standard term, is developed with such elegance and generality that this treatment has become a stable part of physics ever since. Yet the justification of this virtuoso performance is so obscure that Schrödinger (1927a, p. 13), in the introduction to his next article, referred to it as an unintelligible transformation for an incomprehensible transition; Q2 is primarily concerned with making this transition more intelligible. This peculiar order of development would seem to reflect the fact that Schrödinger had more confidence in the conclusions flowing from his equation than in any justification he was then able to give for its introduction.

After the general specification of requirements that

ψ must fulfill, Q1 is chiefly concerned with solving equation (3) for the hydrogen atom. The method of solution employed need only be outlined here. By applying the variational method to (3), Schrödinger developed two equations. The second of these, concerning the behavior of an integral of ψ, $\partial\psi/\partial_{normal}$, is handled by requiring that physically significant quantities vanish in a suitable way at infinite distances. Though this might seem to be suggested by the wave interpretation of ψ, Schrödinger (1927a, pp. 7-8) relies on mathematical rather than physical reasons.[9] The first, and more familiar, equation is

$$(4) \qquad \nabla^2\psi + 2m/K^2(E + e^2/r)\psi = 0 \ ,$$

where K must, for numerical agreement, have the value h/2.

Solving equation (4) was far from routine. Schrödinger's was the first solution of a partial differential equation exhibiting both a continuous and a discrete eigenvalue spectrum.[10] One significant point about this solution was noted by Schrödinger himself (1927a, p. 9); the solution was developed in a way that was neutral with respect to interpretations of atomic structure. Though he would have preferred to relate ψ to some vibratory process within the atom, something analogous to beats in music, he relied instead on the fact that the correct numerical relations came out in a natural way without the imposition of arbitrary conditions.

Schrödinger's second quantization paper (1926c) related more directly to the work of de Broglie. The paper began with a clarification of the basis of Q1, showing how Schrödinger's variation principle corresponds to Fermat's principle for wave propagation, though Schrödinger's principle is in configuration space. Similarly, the Hamilton-Jacobi equation Schrödinger used in Q1 could be interpreted as expressing Huyghens' principle for wave propagation. The justification that Q2 presented can be described as a nonrelativistic reinterpretation of de Broglie's results. In place of de Broglie's relativistic phase waves, however, Schrödinger (1927a, p. 16) arrived at a system of wave surfaces that form a progressive but stationary wave motion in configuration space.[11]

The problems configuration space presents will be considered later. Of more immediate interest is the comparison drawn in Q2 (Schrödinger, 1927a, 25ff.): De Broglie's theory (rightly reinterpreted) stands to Schrödinger's theory as geometric optics stands to wave optics. Geometric optics represents a valid approximation when one is studying phenomena, such as reflection and refraction, for which wave effects are so negligibly small that light may be treated as a set of rays. Similarly, de Broglie's geometric wave mechanics (or ray mechanics) is an approximation no longer valid when one is treating phenomena of a size comparable to the wave lengths involved. In using such a reinterpretation of de Broglie's work, Schrödinger faced a basic problem of showing how his mathematical formulation of wave surfaces in configuration space related to de Broglie's theory of matter waves.

What Schrödinger took from de Broglie was actually the formulation based on wave velocity and group velocity, rather than the relativistic phase-wave formulation. Though the way Schrödinger handled this is quite involved mathematically, the basic method can be summarized in a fairly qualitative way. One postulates a group of waves and then sets up and solves the Hamilton-Jacobi equation to get a point of agreeing phase for a whole aggregate of wave groups. This singularity in configuration space takes the place of de Broglie's particle riding a wave, and its motion defines the geometric locus of points of agreeing phase. Schrödinger was unable to prove mathematically that a superposition of such wave disturbances really produces a notable disturbance only in a very small region--or that there is no spreading of the singularity. Accordingly, he postulated a lack of spreading and presented this as a physical, rather than a mathematical, hypothesis subject to the test of experimental trial (Schrödinger, 1927a, p. 25). The conclusion Schrödinger drew from this protracted comparison was that in dealing with small-range phenomena, where the ray-mechanics approximation is invalid, one must abandon any images of electrons moving along definite paths and rely on a wave equation. To back up this deliberate disregard of visualizable models, Schrödinger cited the work of Heisenberg, Born, and Jordan and noted that their strivings seemed to manifest the same tendency. Though he had not yet found the link connecting his development with theirs, Schrödinger (1927a, p. 30) expressed the hope that his own method would eventually lead to a more