Foundations of Physics, Vol. 17, No. 5, 1987

Niels Bohr's Discussions with Albert Einstein, Werner Heisenberg, and Erwin Schrödinger: The Origins of the Principles of Uncertainty and Complementarity

Jagdish Mehra[1,2]

Received February 18, 1987

In this paper, the main outlines of the discussions between Niels Bohr with Albert Einstein, Werner Heisenberg, and Erwin Schrödinger during 1920–1927 are treated. From the formulation of quantum mechanics in 1925–1926 and wave mechanics in 1926, there emerged Born's statistical interpretation of the wave function in summer 1926, and on the basis of the quantum mechanical transformation theory—formulated in fall 1926 by Dirac, London, and Jordan—Heisenberg formulated the uncertainty principle in early 1927. At the Volta Conference in Como in September 1927 and at the fifth Solvay Conference in Brussels the following month, Bohr publicly enunciated his complementarity principle, which had been developing in his mind for several years. The Bohr–Einstein discussions about the consistency and completeness of quantum mechanics and of physical theory as such—formally begun in October 1927 at the fifth Solvay Conference and carried on at the sixth Solvay Conference in October 1930—were continued during the next decades. All these aspects are briefly summarized.

DEDICATION

During over two decades of our friendship and association, Ilya Prigogine has often talked to me about the principal architects of quantum theory—Max Planck, Albert Einstein, Niels Bohr, Werner Heisenberg, Max Born, Paul Dirac, and Pascual Jordan—and asked me questions

[1] Instituts Internationaux de Physique et de Chimie (Solvay), U.L.B. Campus Plaine, Code Postal 231, 1050 Brussels, Belgium.
[2] Address in U.S.A.: 7830 Candle Lane, Houston, Texas 77071.

0015-9018/87/0500-0461$05.00/0 © 1987 Plenum Publishing Corporation

about their respective ideas and attitudes. He is profoundly interested in all the historical and philosophical aspects of physical theories. I take great pleasure in dedicating this article—with affection, admiration, respect, and gratitude—to Ilya Prigogine in honor of his 70th birthday. It also marks the 60th anniversary of Werner Heisenberg's enunciation of the principle of uncertainty and Niels Bohr's principle of complementarity.

1. INTRODUCTION

From 1913 onwards, Niels Bohr engaged in scientific and philosophical discussions about the problems of atomic and quantum theory with various discussion partners. During the decade of 1920–1930, Bohr carried on intense discussions with Hendrik Kramers, Paul Ehrenfest, Albert Einstein, Wolfgang Pauli, Werner Heisenberg, Max Born, Erwin Schrödinger, Oskar Klein, and others. In 1913 Bohr had first given expression to the principle of analogy, which later became the correspondence principle, and which was applied to obtain the solution of atomic problems from 1913 to 1925(1). Bohr's 1913 papers are reprinted in Ref. 1. In the latter half of 1925 and the first half of 1926, the quantum mechanics of Heisenberg(2) and Born, Heisenberg, Jordan,(3) and Dirac,(4) and the wave mechanics of Schrödinger,(5) were formulated; their formal mathematical equivalence was established by Schrödinger,(6) Pauli (letter of Pauli to Jordan, April 12, 1926; see Ref. 7), and Carl Eckart.(8) In the summer of 1926 Born(9) gave the statistical interpretation of the wave function, and in the fall of 1926 the transformation theory of Fritz London,(10) P. A. M. Dirac,(11) and Pascal Jordan(12) was formulated. Early in 1927, Heisenberg employed the transformation theory to derive his indeterminacy relations and formulate the uncertainty principle.(13) In the fall of 1926 Bohr engaged in deep discussions with Schrödinger,(14) during the latter's visit to Copenhagen, and in the early spring of 1927 he carried on discussions with Heisenberg. (See the citations to W. Heisenberg and K. Stolzenburg in Ref. 14; these discussions are dealt with in detail in Vol. 6 of Mehra and Rechenberg.(15) The earlier discussions of Bohr with Heisenberg are dealt with in the citations in Ref. 14, in Vol. 2 of Mehra and Rechenberg,(15) and in Mehra.(15)) At the Como Conference in September 1927, on the occasion of the centenary of Alessandro Volta's death, Bohr first enunciated his principle of complementarity.(16) (See also Ref. 72, pp. 345–361, and footnote 78 on p. 349.) Quantum mechanics, in it various formulations, complete with its physical interpretation, was presented by its various protagonists at the fifth Solvay Conference in late October 1927.(17) On that occasion, Niels Bohr and Albert Einstein engaged in discussions about the consistency and completeness of quantum mechanics with several other participants—including Ehrenfest, Born, Heisenberg, Dirac, Schrödinger, and Louis de Broglie—present. Bohr and Einstein had encountered each other previously in Berlin in April 1920 and in Leyden in December 1925, when other topics of discussion between them had been at the forefront.[18] The discussions between Bohr and Einstein about classical determinism and statistical causality, and whether the quantum mechanical description was "consistent" and "complete," would be resumed in October 1930 in Brussels (Ref. 19; see also Ref. 17), and be continued in various forms until Einstein's death in April 1955 (See Ref. 17, Chapter 6 and Appendix).

In this article, I shall review the essential aspects of the ideas that were expressed in Bohr's discussions with Einstein, Heisenberg, and Schrödinger. Some of these discussions directly led to Bohr's formulation of the principle of complementarity, which had actually been evolving in Bohr's mind for some time.

2. NIELS BOHR AND ALBERT EINSTEIN: FIRST ENCOUNTER AND EARLY DISCUSSIONS[20,21]

In Albert Einstein's work of 1916–1917 on the treatment of emission and absorption of radiation according to quantum theory,[22] it was found that probability laws governed atomic phenomena and a "statistical residue" definitely remained. Einstein had this problem in mind when, on January 27, 1920, he wrote to Max Born: "I myself do not believe that the solution to the quanta has to be found by giving up the continuum. Similarly, it could be assumed that one could arrive at general relativity by giving up the coordinate system. I believe now, as before, that one has to look for redundancy in determination by using differential equations so that the *solutions* themselves no longer have the character of continuum. But how?"[23] (See letter from Einstein to Born, January 27, 1920, Ref. 23, p. 21.) Later on, in the same letter, Einstein remarked: "That business about causality causes me a lot of trouble, too. Can the quantum absorption and emission of light ever be understood in terms of the complete causality requirement, or would a statistical residue remain? I must admit that there I lack the courage of my convictions. But I would be very unhappy to renounce *complete* causality ··· (The question whether strict causality exists or not has a definite meaning, even though there can probably never be a definite answer to it.)" (See letter from Einstein to Born, January 27, 1920, Ref. 23, p. 22.)

These considerations formed the early basis for Einstein's position concerning quantum mechanics several years later. Einstein believed unconditionally that a continuum theory, based on differential equations, had to be retained; he believed that quantum phenomena could be obtained by redundancy in determination, i.e., more equations than unknowns. The latter had been his hope in his formulation of the equations of general relativity theory; there the equations had seemed to be overdetermined, and Einstein had hoped that he could bring in the quantum phenomena (discontinuities) through this overdetermination.[(24)] On March 3, 1920, Einstein wrote to Born: "In my spare time I always brood about the problem of quanta from the point of view of relativity. I do not think the theory can work without the continuum. But I do not seem to be able to give tangible form to my pet idea which is to understand the structure of the quanta by redundancy in determination, using differential equations."[27] (See letter from Einstein to Born, January 27, 1920, Ref. 23, p. 26.) This indeed was the reason why Einstein would become so enthusiastic about Schrödinger's work on wave mechanics in early 1926.

In his essay on "Does Field Theory Provide Possibilities for Solving the Problem of Quanta?"[(25)] in 1923, Einstein would approach the problem of incorporating quanta into a general field theory, based on the principles of causality and continuity. Soon thereafter, he would write to his friend Michele Besso: "The idea I am toiling with in order to reach full understanding of quantum phenomena refers to an overdetermination of the laws by having more differential equations than field variables. For in this way the arbitrariness of the initial conditions could be overcome without renouncing field theory. Although this approach may well turn out to be a failure, it has to be attempted for, after all, it is logically possible.... The mathematics is exceedingly complicated and the relation to experience is even more indirect. But it remains a logical possibility, to do justice to reality, without any *sacrificium intellectus*." (See letter from Einstein to Besso, January 5, 1924, Ref. 26.)

Albert Einstein entertained such thoughts before he ever met Niels Bohr, and several years before the quantum mechanics of Born, Heisenberg, Jordan, and Dirac, and the wave mechanics of Schrödinger, were formulated, and long before the Bohr–Einstein discussions on classical determinism versus statistical causality took place in Brussels and were continued in subsequent years. (For an account of the Bohr–Einstein discussions, see Ref. 27.)

Niels Bohr believed that although classical physics and quantum theory were connected asymptotically through his correspondence principle, they were irreconcilable. Einstein found this view repugnant. He wrote to Max Born on June 4, 1919: "The quantum theory gives me a feeling very much like yours. One really ought to be ashamed of its success, because it has been obtained in accordance with the Jesuit maxim: 'Let not thy left hand know what thy right hand doeth.' (See letter from Einstein to Born, June 4, 1919, Ref. 23, pp. 10, 11.) Einstein had already shown in 1909 that Maxwell's equations might yield pointlike singular solutions in addition to waves.[28] In 1927 Einstein would successfully apply this idea to the field equations of general relativity; he firmly believed in a unified causal theory of all physical phenomena, and this idea would prompt him to support Louis de Broglie's theory of pilot waves at the fifth Solvay Conference. (See de Broglie in Mehra, Ref. 17.)

In the spring of 1920 Niels Bohr visited Berlin, where he gave a lecture before the Berlin Physical Society on April 27, 1920 about "the present state of the theory of spectra and possibilities of its development in the near future." This subject was closely related to the theory of light quanta, but in his talk Bohr referred only once to the conception of "radiation quanta," which also he probably did out of respect for Einstein who was in the audience. Immediately, however, Bohr added: "I shall not discuss here the familiar difficulties to which the 'hypothesis of light quanta' leads in connection with the phenomena of interference, for the explanation of which the classical theory of radiation has shown itself to be so remarkably suited."[29] In Berlin, Niels Bohr met Albert Einstein, Max Planck, and James Franck; with Franck he immediately established a strong rapport.

In the discussions between Bohr and Einstein in Berlin, Einstein expressed his fundamental belief that a complete theory of light must somehow incorporate both corpuscular and undulatory features, whereas Bohr defended the classical wave theory of light. Bohr insisted that the frequency ν appearing in the energy $h\nu$ of the quantum is defined by experiments on interference phenomena "which apparently demand for their interpretations a wave constitution of light" and "the light-quantum theory thus makes nonsense of its own basic equations." (quoted in Ref. 21, p. 121). In the spring of 1920, the roles of Bohr and Einstein were quite opposite of what they became thereafter. However, their characteristic views could be recognized: Bohr sought a complete break with the ideas of classical mechanics, while Einstein endorsed the wave-particle duality of light, and was convinced that these two aspects could be causally related (Ref. 21, pp. 121, 122).

Einstein was deeply impressed by Bohr's personality, in spite of the diversity of their views. Soon after Bohr's return to Copenhagen from Berlin, Einstein wrote to him: "Not often in life was I so delightfully impressed already by the mere presence of somebody as by yours. Now I understand why Ehrenfest is so fond of you." (letter from Einstein to Bohr, May 2, 1920; quoted in Ref. 21, p. 123, and Ref. 20, Vol. 1, Part 1, p. 257). Bohr described his visit to Einstein as "one of the greatest events in my life." (letter from Bohr to Einstein, June 24, 1920, quoted in Ref. 21, p. 123).

And Einstein wrote to Ehrenfest: "Bohr was here and I am just as keen on him as you are. He is a very sensitive fellow and goes about this world as if hypnotized." (letter from Einstein to Ehrenfest, Ref. 21, p. 123). In a letter to Arnold Sommerfeld, Einstein expressed his admiration of Bohr's intuition.[30]

The conflict between Bohr and Einstein reached its first peak after the discovery of the Compton effect in October 1922.[31] This effect was immediately explained by Arthur Holly Compton[32] and, independently, by Peter Debye,[33] as the directed scattering of individual light quanta or photons by electrons, with resultant recoil of the electron, thus conserving momentum and energy in individual atomic processes.

The Compton effect was proof positive of the existence of light quanta, which had been doubted by many serious physicists (including Max Planck) ever since Einstein introduced them in 1905 and explained the photoelectric effect.[34] Bohr himself had used the emission and absorption of light quanta in his theory of the hydrogen spectrum merely as a heuristic device, without ever believing in their existence.[35] Like Planck, Bohr believed that a merely "corpuscular theory of light" would lead to enormous difficulties in explaining electrostatic fields, and one would have to sacrifice some of the proudest achievements of Maxwell's electrodynamics. Bohr did not see how the correspondence limit or analogy between the light quantum and classical wave radiation could be established, and he had declared: "Even if Einstein sends me a cable announcing the proof of the light quantum, the message cannot reach me because it has to be propagated by electromagnetic waves." (W. Heisenberg, in conversations with J. Mehra, Geneva, July 1962).

Yet Bohr was extremely bothered by the problem of explaining the Compton effect *without the light quantum.* He was therefore very glad when, toward the end of 1923, John Slater, the young American from Harvard, brought to Copenhagen the idea of the "virtual oscillator" by means of which Slater attempted to reconcile the discrete theory of light quanta with the continuous wave theory of the electromagnetic field.[36] On the basis of this idea, Bohr developed, in 1924, with Kramers and Slater, the outline of a new "Quantum Theory of Radiation,"[38] which Kramers applied to the theory of dispersion.[39] The Bohr–Kramers–Slater theory of radiation completely abandoned Einstein's conception of a quantum structure of radiation, and replaced it by an entirely probabilistic approach based only on a statistical conservation of energy and momentum in atomic processes. When this notion was first proposed in early 1924, men like Einstein and Pauli, who believed in strict energy-momentum conservation as the divine plan of an orderly universe, regarded Bohr's idea as being completely heretical. (See Ref. 20, Vol. 1, Part 2, Chapter V.)

On 29 April 1924, Einstein wrote to Max Born: "Bohr's opinion about radiation is of great interest. But I should not be forced into abandoning strict causality without defending it more strongly than I have so far. I find the idea quite intolerable that an electron exposed to radiation should *choose of its own free will* [*aus freiem Entschluss*], not only its moment to jump off, but also its direction. In that case, I would rather be a cobbler, or even an employee in a gambling house, than a physicist. It is true, my attempts to give tangible form to the quanta have foundered again and again, but I am far from giving up hope [for a long time yet]." (letter from Einstein to Born, Ref. 23, p. 82). In his letter to Paul Ehrenfest, dated May 1, 1924, Einstein listed a number of suggestions why he rejected Bohr's suggestions, the principal reason being that "a final abandonment of strict causality is very hard for me to tolerate." (letter from Einstein to Ehrenfest, May 1, 1924, quoted in Ref. 21, p. 124).

In April 1925, Walther Bothe and Hans Geiger obtained the results of their *coincidence experiment*, showing that the secondary Compton radiation indeed emerged after scattering by a *single* electron.[39] This simple result represented the demise of the radiation theory of Bohr, Kramers, and Slater, and the triumph of strict energy-momentum conservation in individual atomic processes and its vehicle, the light-quantum. Einstein was convinced that *it had to be so*, and was glad that *it was so*. Bohr wrote a touching letter to Rutherford about the terrible difficulties of physics, or of physics as he had conceived it to be, and told him how miserable he was (letter from Bohr to Rutherford, April 18, 1925, *Bohr Archives*).

In December 1925, Bohr and Einstein met again, this time on the occasion of the celebration of Hendrik Lorentz' fiftieth anniversary of his doctorate on December 11. Paul Ehrenfest had been in Leyden since 1912, where he had become Lorentz' successor. He had enjoyed friendly relations with Einstein ever since he visited him in Prague in 1912; he also greatly admired Bohr, with whom he had frequent contact since May 1918. Hendrik Kramers, Bohr's long-time collaborator, was a student of Ehrenfest. (For an account of the beginning of H. A. Kramers' association with N. Bohr, see Ref. 20, Vol. 1, Part 1, Section III. 4.) In the fall of 1925, Ehrenfest's students George Uhlenbeck and Samuel Goudsmit had introduced the hypothesis of electron spin, based on Pauli's assignation of four quantum numbers to the electron; their hypothesis was published in a short note in *Naturwissenschaften* in November 1925.[40] Their note initiated a quick response: on November 21, 1925, just the day after its publication, Werner Heisenberg from Göttingen wrote to Goudsmit —whom he knew quite well—stating his essential agreement with the idea of the rotating electron but asked how he had got rid of the factor 2 in the doublet formula. In fact, they did not know how to proceed with the calculation. Fortunately, a little afterwards Albert Einstein came to Leyden and provided the necessary hint. He suggested that the calculation should be made in the coordinate system in which the electron was at rest. By performing the calculation Uhlenbeck found that there was indeed a difficulty about the factor 2 in the doublet formula.

The negative result was, however, soon balanced by the response which the Uhlenbeck and Goudsmit hypothesis received from Einstein and Bohr in Leyden. Before arriving in Leyden for the Lorentz jubilee celebration, Bohr had passed through Hamburg and met Pauli, who had warned him against accepting the hypothesis of the rotating electron during his visit to Holland. But then Bohr was completely won over, as he wrote several months later to Ralph Kronig: "When I came to Leyden to the Lorentz festivals, Einstein asked the very first moment I saw him what I believed about the spinning electron. Upon my question about the cause of the necessity of the mutual coupling between spin axis and the orbital motion, he explained that this coupling was an immediate consequence of the theory of relativity. This remark acted as a complete revelation to me, and I have never faltered in my conviction that we at last were at the end of our sorrows." (Letter from Niels Bohr to Ralph Kronig, March 26, 1926. For an account of the history of electron spin, see Ref. 20, Vol. 1, Part 2, Section VI. 4). Thus Bohr, who had shown only little interest in the magnetic electron before, became "completely like a prophet for the electron-magnet gospel." (letter from N. Bohr to P. Ehrenfest, December 22, 1925, quoted in Ref. 20, Vol. 1, Part 2, p. 703).

Very little is known about the discussions between Bohr and Einstein at the Lorentz festival in Leyden. In the meanwhile, Bohr had accepted Einstein's theory of the light-quantum, but he was deeply concerned about the difficulties of applying the notions of classical physics to quantum theory. In a letter to Einstein, dated April 13, 1927, Bohr referred to their encounter in Leyden, which had given him "great pleasure," and—as if continuing the discussion at Leyden—he again emphasized that the concepts of classical physics "give us only the choice between Scylla and Charybdis, depending on whether we direct our attention to the continuous or the discontinuous features of the description." (letter from N. Bohr to A. Einstein, April 13, 1927, quoted in Ref. 21, p. 125).

In his letter to Einstein, Bohr enclosed a copy of the proofs of Heisenberg's paper on uncertainty relations. He sought to connect Heisenberg's indeterminacy principle with his discussion with Einstein in Leyden; he now wrote that, as Heisenberg had shown in his paper, inconsistencies could be avoided only because of the fact that "the limitations of our concepts coincide with the limitations of our capacities of observation" (Ref. 21, p. 125). This indicates that "Bohr had already envisaged his complementarity interpretation in April 1927." (Ref. 21, p. 125). Turning to the problem of light quanta, Bohr wrote: "In view of this new formulation [Heisenberg's indeterminacy relations] it becomes possible to reconcile the requirement of energy conservation with the implications of the wave theory of light, since according to the character of the description the different aspects of the problem never manifest themselves simultaneously." (letter from N. Bohr to A. Einstein, April 13, 1927, quoted in Ref. 21, pp. 125, 126).

3. NIELS BOHR AND WERNER HEISENBERG: EARLY DISCUSSIONS AND THE BIRTH OF QUANTUM MECHANICS

The first time Werner Heisenberg encountered Niels Bohr was on the occasion of the lectures on atomic structure, which Bohr delivered in Göttingen from June 12–22, 1922 at the invitation of the Wolfskehl Commission.[3] Arnold Sommerfeld had taken two of his brightest pupils, Wolfgang Pauli and Werner Heisenberg, to attend Bohr's lectures. Heisenberg was impressed with the pleasant, warm, and glowing personality of Niels Bohr and enjoyed his lectures. Although he had already learned the content of what Bohr had to say in Sommerfeld's courses, he noticed that, "it all sounded quite different from Bohr's own lips. We could clearly sense that he had reached his results not so much by calculation and demonstration as by intuition and inspiration, and that he found it difficult to justify his findings before Göttingen's famous school of mathematics."[(41)] There were discussions after each lecture, in which—especially after the third lecture—Heisenberg participated. Bohr had talked about the calculations of his collaborator Hendrik Kramers on the Stark effect of the hydrogen atom, in particular when the strength of the electric field was very weak and the electric splitting of the components was of the same order as the fine-structure splitting.[(42)] Bohr had concluded by saying: "No experiments have yet been performed on the transition of the fine structure to the usual Stark effect by the gradual increase of an electric field. The quantum theory yields very many details of the phenomenon to be expected. Even if we really should not be unprepared to find that the quantum theory is false, it would surprise us very much if such a detailed picture obtained from the quantum theory should not be valid; for our belief in the formal reality of the quantum conditions is so strong that we should won-

[3] The members of the Wolfskehl Commission were the mathematicians Ernst Ehlers, David Hilbert, Felix Klein, Hermann Minkowski, and Carl Runge. They had invited Bohr to lecture at Gottingen in spring 1921 on the problems of atomic theory. Illness prevented him from doing so in 1921, but he delivered his lectures in June 1922.

der very much if experiments were to give a different answer than what is demanded by the theory."[43] Now Heisenberg, who knew Kramers' paper quite well because he had reviewed it earlier in Sommerfeld's Seminar, dared to dissent from this opinion and this gave rise to his first discussion with Niels Bohr. Heisenberg raised a serious objection because Kramers' result did not agree with any of the classical frequencies of the atom. On the other hand, the phenomenon of the quadratic Stark effect could be related to the dispersion of light of small frequencies by bound electrons in an atom; moreover, in the existing description of dispersion only the classical frequency of the electron's motion always appeared. Heisenberg had put Bohr on the spot with the question concerning the validity of the correspondence principle in treating the quadratic Stark effect. Bohr was not prepared to deal with Heisenberg's objection. (For a discussion of this problem, see Ref. 20, Vol. 2, Section III. 1, and Vol. 1, Part 1, Section III. 4.) "Bohr answered that one should take here into account the reaction of the radiation on the atom, but he was obviously worried by this objection. When the discussion was over, Bohr came to me and suggested that we should go for a walk together on the Hainberg outside Göttingen. Of course, I was very willing."[44]

Heisenberg was very happy that Bohr took the time to talk to him, and he was very impressed by the manner in which these private discussions went on. During the walk, which took Bohr and Heisenberg on one of the trails—passing the *Café 'Zum Rohns'*—to the top of the Hainberg, from where one had an excellent view of Göttingen and its surroundings, Heisenberg learned more about Bohr's ideas than from his previous study of the papers. As Heisenberg recalled: "That discussion which took us back and forth over Hainberg's wooded heights was the first thorough discussion I can remember on the fundamental physical and philosophical problems of modern atomic theory, and it has certainly had a decisive influence on my later career. For the first time I understood that Bohr's view of atomic theory was much more skeptical than that of many other physicists—e.g., Sommerfeld—at that time, and that his insight into the structure of the theory was not a result of a mathematical analysis of the basic assumptions, but rather of an intense occupation with the actual phenomena, such that it was possible for him to sense the relationships intuitively rather than derive them formally." (Ref. 44, p. 95, and Ref. 20, Vol. 2, p. 130).

The discussion about atomic physics between Bohr and Heisenberg on the Hainberg in Göttingen ended by Bohr inquiring about the young man's background and plans. Bohr invited him to come to Copenhagen for a few weeks the following spring, and perhaps later, possibly on a scholarship, to work there for a longer period. Heisenberg was extremely flattered by Niels

Bohr's personal interest in his future; to be invited by Bohr meant a great honor. He knew that his friend Wolfgang Pauli was about to go to Copenhagen in fall 1922; he would be able to follow him very soon. However, it took some time before Heisenberg was able to go to Denmark. In the meantime he remained in contact with Bohr's Institute, mainly through Pauli, discussing in letters the progress of his work on the anomalous Zeeman effects and the helium atom.

In a letter to Heisenberg, dated January 31, 1924, Bohr inquired "whether it would suit you to come to Copenhagen for a few weeks." Bohr added that he would be able to pay the expenses for Heisenberg's travel and stay, and concluded: "I often remember with great joy our meetings in Göttingen, and I very much hope that we shall be able to collaborate here in Copenhagen for a longer period. I shall be grateful if you would write to me about your future plans. Now, however, I hope first that you will be able to accept my invitation for a shorter visit in the near future." (letter from Bohr to Heisenberg, January 31, 1924, quoted in Ref. 20, Vol. 2, p. 131).

Heisenberg went for his first visit to Copenhagen at Easter 1924. (For an account of Heisenberg's first visit to Copenhagen, see Ref. 35 and Ref. 20, Vol. 2, Chapter III.) He had looked forward to criticizing Bohr's methods and results in atomic theory. Before he had had the chance, however, Bohr took him on a walking tour of Denmark, showing him the sights and talking to him about history and philosophy, and finally physics. Heisenberg was charmed. He found Bohr to be friendly, inspiring, kind, and one who had thought about the problems of atomic physics like no one else. Heisenberg had gone to Copenhagen to battle against the correspondence principle with the prophet himself; instead he became its evangelist.

Heisenberg returned to Copenhagen for six months in the fall of 1924. He worked with Bohr and Kramers on specific problems of atomic theory, in which he sought to formulate the content of the correspondence principle in terms of equations from which new physical results could be derived. For instance, he treated the problem of the polarization of resonance fluorescence light emitted by atoms.[45] Together with Kramers, Heisenberg extended Kramers' dispersion formula to the incoherent scattering of light by atoms.[46] The success thus obtained by what he called the *sharpening (Verschärfung)* of the correspondence principle increased Heisenberg's confidence in the Copenhagen approach, and he hoped, as he recalled later, that "Perhaps it would be possible one day, simply by clever guessing, to achieve the passage to a complete mathematical scheme of quantum mechanics."[47] In April 1925 Heisenberg returned to Göttingen to take up his duties as *Privatdozent* during the summer semester.

In Göttingen, Heisenberg sought to guess the intensities of the hydrogen lines on the basis of a "sharpened" correspondence principle, but in this specific problem he failed. He concluded that the difficulties arising from the rules of quantization were of a more general nature and had to be treated first. These difficulties were due, not so much to a departure from classical mechanics, but rather to a breakdown of the kinematics underlying this mechanics. Heisenberg employed a completely new idea: he assumed that the classical equation of an electron could be retained but the kinematical interpretation of the quantity $x(t)$ as a position depending on time had to be rejected. He expressed $x(t)$ as a Fourier series in terms of Fourier coefficients and frequencies that corresponded to the transition from a state n to a state $n-\alpha$. He motivated the introduction of the transition amplitude, $a(n, n-\alpha)$, by saying that the intensities and, therefore, the probabilities proportional to $|a(n, n-\alpha)|^2$, are *observable*, in contrast to the function $x(t)$.

The importance of the idea of employing only observable quantities in physical theories had been discussed often in Göttingen ever since Mach,[(48)] Einstein,[(49)] and Minkowski[(50)] had introduced it. Born, Pauli, Jordan, and Heisenberg had all discussed it at length in the context of quantum theory.[4] But Heisenberg made the idea of employing only observable quantities as the guiding philosophical principle of his quantum-theoretical reinterpretation *(Umdeutung)* of kinematical variables.[(2)] Heisenberg quantum-theoretically reinterpreted the classical combination law of frequencies in terms of frequencies depending upon the transitions between two states. He showed that in the product of two quantum variables, $x(t)\,y(t)$, expressed in terms of Fourier series and reinterpreted quantum-theoretically according to his prescription, the coefficient should obey a noncommutative product rule, i.e., in quantum theory $x(t)\,y(t) \neq y(t)\,x(t)$; the result of the product depends on the order in which the product is taken. By this reinterpretation the correspondence principle was incorporated into the very foundations of the theory.

Heisenberg noticed that this reinterpretation introduced a great new difficulty: whereas in classical theory $x(t)\,y(t)$ is always equal to $y(t)\,x(t)$, this is not necessarily the case in quantum theory. Therefore, he concluded that, in general, it was not clear how to formulate a product of two dynamical variables in quantum theory. (See Ref. 35, p. 22, and Ref. 20, Vol. 2, Chapter IV, Section 4.) Heisenberg looked for an example in which he could employ his quantum-theoretical reinterpretation of classical mechanical quantities by avoiding the new difficulty concerning the product. He chose the example of the anharmonic oscillator, with λ as the

[4] For a full account of the background of employing only observable quantities as a guiding principle, see Ref. 20, Vol. 2, Section V. 2.

perturbation parameter and λx^3 as the perturbation term: he wrote its classical equation, in which he reinterpreted the position $x(t)$ and the frequency $\omega(t)$ by means of his quantum-theoretical scheme and he obtained the necessary equations in the perturbation parameter, $\lambda = 0$ (the harmonic oscillator solution) and the first order $\lambda = 1$. Heisenberg found that the transition amplitudes, $a(n, n-\alpha)$, were determined only up to a constant, and he did not know what to do with this constant. This was the beginning of June 1925, and his program was stuck. (See footnote 3 and Section IV. 5 of Vol. 2 of Ref. 20.)

With the coming of spring in 1925, Heisenberg had developed a case of severe hay fever, and to cure it he decided to take a week or ten days off in June 1925 at the rocky island of Helgoland in the North Sea. (See Ref. 35, pp. 23, 24, and Ref. 20, Vol. 2, Section IV. 4.) At Helgoland, Heisenberg sought to give his vague ideas a more definite shape. There he solved two important problems: First, he reformulated quantum-theoretically the quantum condition in one dimension, $\int m\dot{x}\, dx = J = nh$, thereby obtaining an equation which was equivalent to the Thomas–Kuhn–Reiche sum rule. Since for the ground state no transition is possible, he put $a(n, n-\alpha) = 0$, if n is the quantum number of the ground state. The derivation of the quantum condition, and the subsequent determination of the transition amplitudes, was thus the first problem that was solved. (See Ref. 35, p. 25, and Ref. 20, Vol. 2, Sections IV. 4 and IV. 5.) The second problem was whether energy conservation would hold in the new scheme, a question which had become important after the Bothe–Geiger experiment. He knew that, by suitably writing the classical equation for the anharmonic oscillator, it could be shown that dH/dt, the time derivative of its energy or Hamiltonian H, was equal to zero and the energy was conserved. He reinterpreted the expression for the Hamiltonian of the anharmonic oscillator, and went through the calculation of the terms up to the second order in λ, making errors along the way and rechecking them. He found that no time-dependent terms remained in the kinematically reinterpreted Hamiltonian. The example of the anharmonic oscillator showed him that a dynamical problem in quantum theory could be solved with the help of his scheme. Heisenberg was very excited and elated by this result (see Ref. 41, p. 61).

On his way back from Helgoland to Göttingen, Heisenberg stopped to see Pauli in Hamburg. Pauli was his critical genius, and he had learned to respect Pauli's critical faculties since their first encounter in Sommerfeld's Seminar in the fall of 1920. Pauli encouraged him to go on. During the next couple of weeks Heisenberg exchanged several letters with him, and on July 9, 1925 sent him the manuscript of the finished paper. Pauli's opinion of it was favorable.

Having received Pauli's favorable verdict, Heisenberg gave the paper,[2] around the middle of July, to Max Born and asked him to do with it what he thought was right. When Born read Heisenberg's paper, he was just "fascinated," and "I... was soon involved in it that I thought the whole day and could hardly sleep at night... In the morning I suddenly saw the light: Heisenberg's symbolic multiplication was nothing but the matrix calculus, well known to me since my student days from the lectures of Rosanes in Breslau." (See Ref. 20, Vol. 3, Chapter I, and Ref. 35, pp. 27, 28.) Soon after receiving it, Born sent Heisenberg's paper to *Zeitschrift für Physik* for publication.

Born put Heisenberg's quantum condition into the matrix notation, and determined that the two matrix products **pq** and **qp** (of momenta **p** and coordinates **q**) were not identical. Born guessed that the nondiagonal elements of the matrix **pq** − **qp** were zero, and the quantum condition could be written, in general, as

$$\mathbf{pq}-\mathbf{qp}=\frac{h}{2\pi i}\mathbf{1} \tag{1}$$

where h is Planck's constant and **1** the unit matrix, but it was only a guess and he could not prove it. The proof was given independently by Pascual Jordan and P. A. M. Dirac.

On July 19, 1925, Born travelled to Hanover to attend a meeting of the German Physical Society, where Pauli also came from Hamburg. At the railway station, Born told Pauli about the matrices and his difficulty in finding the value of the nondiagonal elements. Born invited Pauli to collaborate with him, to which Pauli gave a sarcastic refusal. (See Ref. 20, Vol. 3, Chapter I, and Ref. 35, pp. 27, 28.)

On his return from Hanover, Born immediately persuaded Jordan to help him in his work, which led to Born and Jordan's formulation of quantum mechanics, being completed on September 27, 1925.[51] This paper contained a résumé of matrix methods, the interpretation of Heisenberg's symbolic multiplication, the proof of the formula for the product difference of **pq** and **qp**, Eq. (1), proof of energy conservation, and the proof of Bohr's frequency condition. It already contained an attempt, made entirely by Jordan, at the quantization of the electromagnetic field by regarding its components as matrices.

Further development toward the completion of quantum mechanics began immediately afterwards in the collaboration of Born, Heisenberg, and Jordan. This collaboration began when Jordan wrote a letter to Heisenberg early in September 1925—Heisenberg was in Copenhagen for a few weeks before he returned to Göttingen for the winter semester—with Heisenberg and Born and Jordan all contributing their bits. The general editing of the paper was done by Jordan, and the leading introduction was written by Heisenberg. This paper by Born, Heisenberg, and Jordan was thus the third paper in the series after Heisenberg's discovery,[2] and it gave a logically consistent exposition of matrix mechanics. It was completed by the end of October 1925, and is usually called the *"Drei-Männer-Arbeit."*[52] It was really a learned paper bringing in all the mathematical learning of Göttingen, against which Pauli had severely complained personally to Max Born and in his letters to collegues [letter from Pauli to Kronig, October 9, 1925 (Ref. 7); most of Pauli's carly letters to Heisenberg were destroyed during World War II]: eigenvalues and eigenvectors, canonical transformations, principal axis transformation, Hilbert's quadratic form in an infinite number of variables, general commutation relations, and physical applications—including the quantization of the electromagnetic field and the calculation of fluctuations in this field by Jordan. This paper contained essentially the entire apparatus of modern matrix mechanics.

Wolfgang Pauli took up the problem of the hydrogen atom and solved it within the next few weeks by means of matrix methods, employing all the formal mathematical learning against which he had complained earlier. He made an ingenious application of the integration method which Wilhelm Lenz had used earlier for determining the effect of crossed electric and magnetic fields on the energy states of the hydrogen atom in the Bohr–Sommerfeld theory.[53] With the help of the "Lenz vector" Pauli obtained the Balmer formula and showed hos the situation with respect to the forbidden orbits could now be understood naturally.[54]It was exactly two years since Pauli had first seriously doubted Bohr's theory of the hydrogen atom, and now one had come around to full circle. This was indeed a triumphant moment for the new quantum mechanics, and Niels Bohr celebrated it by writing another letter to Rutherford, informing him that the reasons for his misery in the previous spring had now disappeared (letter from Bohr to Rutherford, January 27, 1926, *Bohr Archives*).

Soon after the publication of the papers of Heisenberg[2] and Born and Jordan,[51] Cornelius Lanczos, then at the University of Frankfurt, conceived of a "field-like representation of quantum mechanics."[55] Also, from November 14, 1925 to January 22, 1926, Max Born gave a series of lectures at the Masschusetts Institute of Technology, Cambridge, Massachusetts, which were later published under the title *Problems of Atomic Dynamics.*[56] At M.I.T., Born collaborated with the mathematician Norbert Wiener, and together they developed "a generalization of matrix mechanics into a kind of operator mechanics."[57,58]

However, just before the Born–Heisenberg–Jordan paper was published in *Zeitschrift für Physik* in January 1926, another paper, containing the complete scheme of quantum mechanics, made its appearance in the *Proceedings of the Royal Society.*[4] Its author was Paul Adrien Maurice Dirac and, briefly, it came about as follows. In July 1925, after giving the manuscript of his paper on quantum-theoretical kinematics to Max Born, Heisenberg left for Leyden and Cambridge. In Leyden he stayed as a guest of Paul Ehrenfest, and discussed spectroscopic questions with Ehrenfest, Uhlenbeck, and Goudsmit. Uhlenbeck and Goudsmit would soon propose the hypothesis of electron spin.[40] From Leyden, Heisenberg went to Cambridge, where he stayed as a guest of R. H. Fowler—with whom he had become acquainted in Copenhagen—and gave a talk on "Term Zoology and Zeeman Botany" at the Kapitza Club on July 28, 1925 (W. Heisenberg, Talk on "term zoology and Zeeman botany," *Minute Book* of the Kapitza Club; Heisenberg spoke at the 94th meeting of the Club on July 28, 1925). Privately, he mentioned his new ideas on the quantum-theoretical reformulation of kinematical quantities to Fowler. Fowler requested Heisenberg to send the proof sheets of his paper as soon as they became available, which he did in the beginning of September 1925. Fowler found Heisenberg's paper interesting, and wanted to know what Dirac's reaction would be. At that time, Dirac was too much enamored with the Hamiltonian formalism as the basis of atomic physics, and thought that anything not connected with it would not be much good. At first, Dirac thought there was not much in Heisenberg's paper, and he put it aside for a week or so. When Dirac went back to it, it suddenly became clear to him that Heisenberg's idea had provided the key to the "whole mystery." (See Ref. 35, p. 33; Ref. 20, Vol. 4, Part 1, Chapter IV; and Ref. 59.) During the following weeks Dirac tried to connect Heisenberg's quantum-theoretical reinterpretation of kinematical quantities with the action-angle variables of the Hamilton–Jacobi theory. During a long walk on a Sunday in September 1925, it occurred to Dirac that the commutator might be the analogue of the Poisson bracket. He verified this the next day and found that they fit. (See Ref. 35, p. 34; Ref. 20, Vol. 2, Part 1, Chapter IV; and Ref. 59.)

From the quantum conditions expressed in angular variables Dirac found the correspondence between Heisenberg's commutation brackets and the classical Poisson brackets for the variables X and Y,

$$XY - YX = 2\pi ih \sum_r \left\{ \frac{\partial X}{\partial q_r} \frac{\partial Y}{\partial p_r} - \frac{\partial Y}{\partial q_r} \frac{\partial X}{\partial p_r} \right\} \tag{2}$$

where q_r and p_r can be regarded as the action-angle variables (w_r and J_r).

Dirac was now safely back on Hamiltonian ground. He showed his new results to Fowler, who fully appreciated their importance. Fowler knew what was going on in Copenhagen and Göttingen, and he realized that there would be competition from these places. He thought that the results obtained in England in this field had to be published at once, and urged the *Proceedings of the Royal Society* to give immediate priority to the publication of Dirac's paper on "The Fundamental Equations of Quantum Mechanics."[4] Sir James Jeans, who was then editor of the *Proceedings* and Secretary of the Royal Society, was ready and willing to oblige. All of Dirac's papers from 1925 to 1933 were thus published very fast.

In his fundamental paper,[4] Dirac first gave a summary of Heisenberg's ideas, simplifying the mathematics and making it at once more elegant. He anticipated all the essential results of the papers of Born and Jordan[51] and Born, Heisenberg, and Jordan.[3,52] He developed a quantum algebra, derived Heisenberg's quantization rules, and obtained the canonical equations of motion for quantum systems. In the same paper, Dirac introduced an early form of creation and annihilation operators, pointing out their analogs in classical theory.

Dirac quickly followed this paper by another a few weeks later.[60,61] In it he developed the algebra of q-numbers, that is, the dynamical variables which satisfy all the rules of normal numbers except that their product is not necessarily commutative. He gave detailed theorems on the operations with q-numbers, and applied the rules he had obtained to multiply periodic systems in close analogy with the old quantum rules.

Dirac's aim was to apply his scheme to the hydrogen atom. He wrote its Hamiltonian by simply replacing position and momentum variables in the classical Hamiltonian by q-numbers, and proceeded to obtain the Balmer formula in order to show that this abstract scheme could give results closely related to the experiments. Dirac, however, did not go into the details of this calculation as Pauli[54] (in his paper published during the same month, March 1926) had already shown that this could be done, and Dirac mentioned it in a footnote. (Dirac referred to Pauli's "not yet published paper" on the hydrogen atom in a footnote in Ref. 60, p. 570.) He then went on to calculate the various features of the splitting and intensities of spectral lines in a magnetic field (including the Zeeman effect) in agreement with the experiments.

With all this work on the principles of quantum mechanics Dirac was awarded the Ph. D. degree in May 1926 at Cambridge.[62]

4. THE CREATION OF WAVE MECHANICS

Since 1921 Erwin Schrödinger had been at the University of Zurich, where he occupied the chair of theoretical physics, which Albert Einstein, Peter Debye, and Max von Laue had held previously (Ref. 20, Vol. 5, Part 1, Chapter II). Schrödinger was a Viennese and a man of vast personal culture that included the study of Greek literature and philosophy in the original and the writing of poetry (Ref. 35, p. 37, and Ref. 20, Vol. 5, Part 1, Chapter I). A distinguished physicist by any measure, Schrödinger traced his scientific lineage to Boltzmann through his teacher Fritz Hasenöhrl, but he had himself not yet set the world aglow although he had done excellent work on problems of Brownian motion, specific heat and quantum mechanics, and of general relativity theory (Ref. 35, p. 37, and Ref. 20, Vol. 5, Part 1, Chapter I). By the summer of 1925 Schrödinger had become tired of his stay in Zurich because, as he wrote to Sommerfeld, "the Swiss are just too uncongenial" *('die Schweizer sind gar zu ungemütlich')* and he wanted to go home to Austria (letter from Schrödinger to Sommerfeld, July 21, 1925, in *Sommerfeld Correspondence*, Deutsches Museum, Munich). He was negotiating for the chair of theoretical physics at Innsbruck, but since the University of Innsbruck sought to dicker about the salary, he let the matter drop in favor of Arthur March. Within eighteen months Schrödinger would be appointed as Max Planck's successor at the University of Berlin. (See Ref. 35, p. 37, and Ref. 20, Vol. 5, Part 2, Chapter IV, Section 5.)

In the fall of 1925 Schrödinger suffered not only from the lack of congeniality of his colleagues in Zurich, but the work of Heisenberg and of Born and Jordan on matrix mechanics added to his discomfort, for he remarked: "... I was discouraged *(abgeschreckt)*, if not repelled *(abgestossen)*, by what appeared to me a rather difficult method of transcendental algebra, defying any visualization *(Anschaulichkeit)*. (See Ref. 6, footnote 2, p. 735; Ref. 20, Vol. 5, Part 2, Chapter IV, Section 5.) He decided to sublimate his social and scientific unhappiness by conceiving and delivering a scheme of atomic mechanics which not only seemed to be a genuine alternative to the matrix or q-number mechanics of Heisenberg, Born, Jordan, and Dirac, but helped in completing the edifice of quantum mechanics and in inaugurating the discussions that led to its physical and philosophical interpretation.

In four communications to *Annalen der Physik*, submitted from the end of January to the end of June 1926, Schrödinger developed his theory of wave mechanics, entitled "Quantization as an Eigenvalue Problem." He kept detailed notebooks on his attempts to formulate wave mechanics and its applications to atomic problems; in these attempts, his earlier studies on tensor-analytical mechanics and Hamilton's optical-mechanical analogy—also preserved in his notebooks—came in handy. (Erwin Schrödinger kept almost all of his notebooks in which he had written up his notes on various topics he studied and thought about, for example, notebooks on *Tensoranalytische Mechanik*, *Eigenwertproblem des Atoms*, etc. These notebooks have been made use of in Ref. 20, Vol. 5 on *Erwin Schrödinger and the Rise of Wave Mechanics* to indicate Schrödinger's developing thought processes.) He arrived at his fundamental equation

$$H\left(q, \frac{h}{2\pi i}\frac{\partial}{\partial q}\right)\psi(q) = E\,\psi(q) \tag{3}$$

where H is the Hamiltonian, $\psi(q)$ the wave function, and E the energy eigenvalues, and solved the problem of the spectrum of the hydrogen atom.[63] In the mathematical aspects of some of his work he had invaluable help from Hermann Weyl, then also in Zurich at the *Eidgenössische Technische Hochschule*, and Schrödinger acknowledged it. (Schrödinger thanked Hermann Weyl for help in solving the eigenvalue differential equation for the hydrogen atom. See Ref. 63, footnote 1, p. 363). Weyl's 1908 thesis[64] under Hilbert had dealt with integral equations, eigenvalue problems, orthogonal functions, etc., and it was a fortuitous combination of circumstances that brought Schrödinger and Weyl together.

In his communications, Schrödinger provided the basis of treating all those problems of atomic physics that had been impossible to handle in the Bohr–Sommerfeld theory. In Schrödinger's work the fundamental ideas of Einstein and Louis de Broglie[65] found a natural place. Schrödinger soon recognized that in spite of fundamental disparities the two approaches, his own and Heisenberg and Born's, did not clash but rather complemented each other. In fact, in the early spring of 1926, prior to the publication of his third communication, Schrödinger discovered what he called "a formal, mathematical identity" of wave mechanics and matrix mechanics. (See Ref. 6 and Ref. 20, Vol. 5, Part 2, Chapter IV.) The same formal equivalence was demonstrated, independently, by Carl Eckart (see Ref. 8 and Ref. 20, Vol. 5, Part 2, Chapter IV) in the United States and by Pauli (see Ref. 7 and Ref. 20, Vol. 5, Part 2, Chapter IV) in a letter to Jordan. Many years later, Max Born said in an obituary of Schrödinger: "What is more magnificent in theoretical physics than his first six papers on wave mechanics?"[66]

Soon after the publication of Schrödinger's papers, wave mechanics was successfully applied to a large number of energy-eigenvalue problems of atomic physics. (See Ref. 20, Vol. 5, Part 2, Chapter IV. Heisenberg himself employed Schrödinger's scheme to solve the helium problem; see also Refs. 67 and 68.) It soon became clear that the theory could be extended to deal with types of problems not initially envisaged by Schrödinger.

5. ERWIN SCHRÖDINGER'S LECTURES IN BERLIN AND MUNICH AND VISIT TO COPENHAGEN

From the spring of 1926, Max Planck in Berlin and Wilhelm Wien and Arnold Sommerfeld in Munich had repeatedly invited Erwin Schrödinger to come to Berlin and Munich, respectively, and speak on his new atomic theory. Schrödinger, who was very happy about the appraisal and approval that his work had received in both places, actually fulfilled these pressing and welcome invitations as soon as he could free himself from the obligations of the summer semester in Zurich. He first travelled to Berlin and spoke before the German Physical Society on April 16 on the *"Grundlagen einer Wellenlehre begründeten Atomphysik"* ("Foundations of an Atomic Physics Based on Wave Theory"). (See Ref. 20, Vol. 5, Part 2, Chapter IV, p. 800.) He stayed on in Berlin for a few days, as a personal guest of Max Planck, and then he returned to Zurich via Munich. In Munich he delivered a similar lecture, entitled *"Grundgedanken einer auf Wellenlehre begründeten Atomphysik"* ("Basic Ideas of an Atomic Physics Founded on Wave Theory") to the Bavarian Section *(Gauverein)* of the German Physical Society. (See Ref. 20, Vol. 5, Part 2, Chapter IV, p. 800.)

These two lectures were the first reviews of wave mechanics by its creator. Schrödinger had especially available the results of his fourth communication ("Quantization as an Eigenvalue Problem. Part IV"),[69] in which the theory had received an important generalization and—as it seemed to Schrödinger—a reasonably "visualizable" *("anschauliche")* interpretation also. In these lectures Schrödinger covered all the results he had hitherto obtained in his main communications. (See Ref. 20, Vol. 5, Part 2, Chapter IV, p. 801.) Interestingly enough, he left out a closer discussion of the formal equivalence of the undulatory and matrix mechanics. (See Ref. 20, Vol. 5, Part 2, Chapter IV, p. 801.)

Schrödinger primarily emphasized the progress achieved in his fourth communication,[69] in which he ascribed to the wave function ψ an "electrodynamical significance" in order to account for the fact that a mechanical system can emit electromagnetic waves of a frequency equal to the term difference and to deduce their intensity and polarization. According to Schrödinger, the concept of the electric charge density in the case of many-particle systems could be phrased as follows: "$\psi\bar{\psi}$ [where $\bar{\psi}$ is the Hermitian conjugate of ψ] is a kind of *weight function* in the system's configuration space. The *wave mechanical* configuration is a *superposition* of many, strictly speaking of all, point-mechanical configurations kinematically possible. Thus, each point-mechanical configuration contributes to the true wave mechanical configuration with a certain *weight*, which is given precisely by $\psi\bar{\psi}$." (See Ref. 20, Vol. 5, Part 2, Chapter IV,

p. 797.) While for macroscopic systems and motions the weight function is practically concentrated in small space regions, the varying distribution $\psi\bar{\psi}$ over a region plays a crucial role for microscopic systems. Schrödinger combined his electrodynamic interpretation of the wave function with the idea that particles of corpuscular physics are essentially only wave groups composed of numerous, strictly speaking, infinitely many wave functions.

Schrödinger admitted that the "new interpretation may shock us at first glance", especially since he himself had always insisted on taking the ψ vibrations as something having an easily intelligible physical reality. (See Ref. 20, Vol. 5, Part 2, Chapter IV, p. 797.) Now he found himself forced to modify his earlier statements: the ψ vibrations could still be considered as based on something "tangibly real" *("greifbar Reales")*, he said, namely on "the very real electrodynamically effective fluctuations of the electric space density." (See Ref. 20, Vol. 5, Part 2, Chapter IV, p. 797.) The ψ function, having lost its (primitive) interpretation as directly representing the spatial distribution of the electric charge, still describes the electric fluctuations in a slightly more involved manner. However, if this description is consistent, one has to demand that the $\psi\bar{\psi}$ integral assumes a constant value, preferably unity, for nonconservative systems also, and this constancy had evidently to follow from the time-dependent wave equation. After demonstrating this constancy, Schrödinger proceeded to interpret "the *current-density* of the weight function [$\psi\bar{\psi}$] in the configuration space," which he also called "the *continuity equation* of the weight function, and then derived the *"continuity equation of electricity."* (See Ref. 20, Vol. 5, Part 2, Chapter IX, pp. 789-799, and Ref. 69, pp. 137-138.) Schrödinger remarked: "Since [either zero or stationary current density] occurs in the unperturbed normal state [of the atomic system] at any rate, we may in a certain sense speak of *a return to electrostatic and magnetostatic atomic models*. In this way the lack of radiation in the normal state would, indeed, find a startlingly simple explanation." (See Ref. 69, pp. 138, 139.) This last point, which Schrödinger had already mentioned in his letter to Wilhelm Wien, dated June 18, 1926 (letter from Schrödinger to Wien, June 18, 1926, in the *Wien Collection*, Deutsches Museum, Munich), seemed indeed to support the visualizable *(anschaulische)* interpretation of the new atomic theory, if atoms described by a wave equation need not radiate in what had previously been called the stationary states, i.e., classical electrodynamics then remained valid to a certain extent. As Schrödinger wrote to Wien: "The vibration equation for the ψ function enables us to consider again these static models [for atoms]; although the ψ function oscillates in these models, the distribution of electricity does not change." (See letter from Schrödinger to Wien, June 18, 1926, in the *Wien Collection*, Deutsches Museum, Munich.) Admittedly, the new situation weakened the "reality" of the ψ vibrations, but Schrödinger argued: "This does not matter at all. If one can only control, with their help [i.e., of the ψ-vibrations], distributions and fluctuations of electricity, which are real in the highest sense, then one may be allowed to call them a substitute *(Hilfs)* concept in the same sense as one speaks of the electrodynamic potentials, of which only the derivatives can be observed. (See letter from Schrödinger to Wien, June 18, 1926, in the *Wien Collection*, Deutsches Museum, Munich.)

Schrödinger's communications on wave mechanics were most favorably received by Max Planck, Albert Einstein, and Hendrik Lorentz,[170] as well as by Arnold Sommerfeld and Wilhelm Wien. (See Schrödinger's correspondence with Sommerfeld and Wien, in the *Sommerfeld and Wien Collections*, Deutsches Museum, Munich. It has been amply used in Ref. 20, Vol. 5.) They greatly appreciated the fact that Schrödinger had restored the use of differential equations in quantum theory, with their solutions as various kinds of familiar polynomials, and one did not have to resort to such esoteric devices as matrices and q-numbers. Werner Heisenberg, however, had believed throughout that the solution of the problems of atomic mechanics would lead to *one, unique, general mathematical scheme,* and when he discovered his scheme, well, that was it. With the arrival of Schrödinger's theory, Heisenberg was unhappy, and he believed, indeed hoped, that it was wrong. (See Ref. 35, pp. 39, 40.) When in June and July 1926 Max Born[(9)] applied the Schrödinger method to the collision problems, a work which led to the statistical interpretation of Schrödinger's wave function, Heisenberg reproached him for going over "to the enemy camp." (See Ref. 35, p. 40, and Ref. 67.) Heisenberg attended Schrödinger's lecture at Munich and, even many years later, he vividly recalled Schrödinger's colloquium and the discussion afterwards. Thus, for instance, he recalled in 1968: "In July 1926 Schrödinger was invited to Munich by Wilhelm Wien to report on his theory. The experimental physicists in Munich, headed by Wien, were enthusiastic about the possibility that now perhaps this whole 'quantum mystery of atomic physics' might be dealt with, and one would be able to return to the classical concepts of honest fields, such as one had learned from Maxwell's [electromagnetic] theory. I listened to this lecture by Schrödinger, as I was then staying with my parents in Munich for the [summer] vacation; and I was quite horrified by his interpretation, because I simply could not believe it. I objected (in the discussion) that with such an interpretation one would not even be able to explain Planck's heat radiation law. But general opinion at that time was extremely hostile toward my objection. Wien answered me very harshly in that he could understand how I felt about the fact that the whole quantum jumping, the matrices and all that had become superfluous; anyway, it would be better for me to leave the field to Schrödinger, who would certainly solve all the difficulties in the [near] future. This was not very encouraging; I did not have the slightest chance to get across my point of view in the discussion." (See Ref. 20, Vol. 5, Part 2, Chapter IV. 5, p. 803.) Heisenberg further recalled: "My arguments had clearly failed to impress anyone—even Sommerfeld, who felt more kindly toward me, succumbed to the persuasive force of Schrödinger's mathematics. And so I went home rather sadly. It must have been that same evening that I wrote to Niels Bohr about the unhappy outcome of the discussion. Perhaps it was as a result of this letter that he invited Schrödinger to spend part of September in Copenhagen. Schrödinger agreed, and I, too, sped back to Denmark." (See Ref. 41, pp. 72, 73.)

From the very beginning, Heisenberg had been seriously opposed to the *"anschauliche"* interpretation of wave mechanics. Thus, for instance, he had written to Wolfgang Pauli in June: "The more I ponder on the physical part of Schrödinger's theory, the more detestable I find it. One should imagine the rotating electron, whose charge is distributed over the entire space and which has an axis in a fourth and fifth dimension. What Schrödinger writes on the visualizability of his theory ··· I find rubbish. The great achievement of Schrödinger's theory is the calculation of matrix elements." (See letter from Heisenberg to Pauli, June 8, 1926, in Ref. 71.) Schrödinger's colloquium in Munich had merely confirmed Heisenberg's negative opinion. A few days after the colloquium, he wrote to Pauli: "As nice as Schrödinger is personally, I find his physics so strange: one feels 26 years younger when listening to it. Indeed, Schrödinger throws overboard everything which is 'quantum-theoretical': namely, the photoelectric effect, the Franck[-Hertz] collisions, the Stern–Gerlach effect, etc. It is not then difficult to establish a theory [of the kind Schrödinger has in mind]. However, it does not agree with experience." (See letter from Heisenberg to Pauli, July 28, 1926, in Ref. 71.) But the unfortunate discussion following Schrödinger's Munich lecture persuaded Heisenberg of the necessity of having a really detailed and penetrating discussion with Erwin Schrödinger elsewhere, most profitably in Copenhagen where Niels Bohr especially could participate.

In a letter dated September 11, 1926, Bohr actually invited Schrödinger to come to Copenhagen and deliver a lecture to the Danish Physical Society *(Fysisk Forening)* on wave mechanics. At the same time Bohr expressed the hope "that you will introduce some discussions for the narrower circle of those who work here at the Institute, and in which we can deal more deeply with the open questions of atomic theory." [See Ref. 20, Vol. 1, Part 2, Section V. 2 (for Schrödinger's views on the Bohr–Kramers–Slater theory, see pp. 540, 552, and 553); see also Vol. 5, Part 2, Chapter IV, and K. Stolzenburg (cited in Ref. 14) for an account of

Schrödinger's visit to Copenhagen.] Among this narrower circle belonged, in particular, Werner Heisenberg—from May, 1926 the main *"Assistent"* and *"Lektor"* at the Copenhagen Institute for Theoretical Physics—and Oskar Klein; Paul Dirac was also present at Bohr's Institute at that time. Schrödinger accepted Bohr's invitation quite readily (in a letter to Bohr, dated September 21, 1926) and announced his arrival in Copenhagen on October 1 (Schrödinger to Bohr, telegram of September 27, 1926). There he was received with great eagerness; thus Heisenberg recalled: "Bohr's discussions with Schrödinger began at the railway station and were continued daily from early morning until late at night. Schrödinger stayed at Bohr's house so that nothing would interrupt the conversations." (See Ref. 41, pp. 73-75.)

No contemporary notes about the content of the Copenhagen discussions exist; it does not seem to have occurred to anyone—neither Bohr, nor Schrödinger, nor any of the other participants—to keep such notes. However, Heisenberg later gave many lively accounts of the discussions. (See Ref. 20, Vol. 5, Part 2, Chapter IV, p. 823, and footnote 295.) As Heisenberg recalled, in Copenhagen Schrödinger especially attacked the idea of sudden quantum jumps. Schrödinger believed that the idea of quantum jumps was bound to end in nonsense. He reminded Bohr that "according to his (Bohr's) theory, if an atom is in a stationary state, the atom revolves periodically but does not emit light, when, according to Maxwell's theory it must. Next the electron is said to jump from one orbit to the next and to emit radiation. Is this jump supposed to be gradual or sudden? If it is gradual, the orbital frequency and energy of the electron must change gradually as well. But in that case, how do you explain the persistence of fine spectral lines? On the other hand, if the jump is sudden, Einstein's idea of light-quanta will admittedly lead us to the right wave number, but then we must ask ourselves how precisely the electron behaves during the jump. Why does it not emit a continuous spectrum, as electromagnetic theory demands? And what laws govern its motion during the jump? In other words, the whole idea of quantum jumps is sheer fantasy." (See Ref. 41, pp. 73–75.)

Bohr agreed with Schrödinger's objections, but pointed out that they did not prove that there are no quantum jumps; only we cannot imagine them and the concepts with which we describe events in daily life and experiments in classical physics are inadequate when it comes to describing quantum jumps. "Nor should we be surprised to find it so," Bohr said, "seeing that the processes involved are not the objects of direct experience." (See Ref. 41, pp. 73-75.) Schrödinger countered by saying that "if there are electrons in the atom, and if these are particles—as all of us believe—then they must surely move in some way. Right now I am not concerned with a precise description of this motion, but it ought to be possible to determine the principle of how they behave in the stationary state or during the transition from one state to the next. But from the mathematical form of wave or quantum mechanics alone, it is clear that we cannot expect reasonable answers to these questions. The moment, however, that we change the picture and say that there are no discrete electrons, only electron waves or waves of matter, then everything looks quite different. We no longer wonder about the fine lines. The emission of light is as easily explained as the transmission of radio waves through the aerial of the transmitter, and what seemed to be insoluble contradictions have suddenly disappeared." (See Ref. 41, pp. 73-75.) Bohr disagreed with this and pointed out that the contradictions did not disappear: "You speak of the emission of light by the atom or more generally of the interaction between the atom and the surrounding radiation field, and you think that all the problems are solved once we assume that there are material waves but no quantum jumps. But just take the case of thermodynamic equilibrium between the atom and the radiation field—remember, for instance, the Einsteinian derivation of Planck's radiation law. The derivation demands that the energy of the atom should assume discrete values and change discontinuously from time to time; discrete values for the frequencies cannot help us here. You can't seriously be trying to cast doubt on the whole basis of quantum theory." (See Ref. 41, pp. 73-75.)

Schrödinger conceded that all these relationships had not yet been fully explained, but pointed out that Bohr and his associates had also so far failed to discover a satisfactory physical interpretation of quantum mechanics. Bohr agreed that there were inconsistencies, as for example when one watched sudden flashes of light on a scintillation screen or the sudden rush of an electron through a cloud chamber; one could not just ignore these observations as if they did not exist at all.

On Bohr's defense of the concept of quantum jumps as being essential in describing the behavior of atoms and radiation, Schrödinger became quite despondent, and finally exclaimed: "If all this quantum jumping were here to stay, I should be sorry I ever got involved with quantum theory." To which Bohr replied: "But the rest of us are extremely grateful that you did; your wave mechanics has contributed so much to the mathematical clarity and simplicity that it represents a gigantic advance over all previous forms of quantum mechanics." (See Ref. 41, pp. 73-75.)

Heisenberg also recalled that the continuous, strenuous discussions and conversations with Bohr exhausted Schrödinger. "After a few days Schrödinger fell ill, perhaps as a result of his enormous effort; in any case, he was forced to keep to his bed with a feverish cold. While Mrs. Bohr nursed him and brought in tea and cake, Niels Bohr kept sitting on the edge of the bed talking to Schrödinger: 'But you must surely admit that....'" (See Ref. 41, pp. 73-75.) In other words Bohr—whom Heisenberg described as having been "an almost remorseless fanatic" (Ref. 41, pp. 73-75) in the debate with Schrödinger—still *had to convince* his miserable guest to accept the Copenhagen position. However, "no real understanding could be expected since, at that time, neither side was able to offer a complete and coherent interpretation of quantum mechanics. For all that, we in Copenhagen felt convinced toward the end of Schrödinger's visit that we were on the right track, though we fully realized how difficult it would be to convince even leading physicists that they must abandon all attempts to construct perceptual models of atomic processes." (See Ref. 41, pp. 73-75.)

Schrödinger, who delivered his lecture entitled *"Grundlagen der undulatorischen Mechanik"* ("Foundations of an Undulatory Mechanics") before the Danish Physical Society on October 4, 1926, stayed in Copenhagen only a few days. A couple of weeks later he reported to Wilhelm Wien: "... it was very nice that I was able to become thoroughly acquainted with Bohr—whom I had never known before—in his own surroundings, and to talk with him for hours about these matters which are so very dear to all of us." (See letter from Schrödinger to Wien, October 21, 1926, in the *Wien Collection*, Deutsches Museum, Munich.) In his letter to Wien, Schrödinger briefly sketched the contents of discussions in Copenhagen, especially the points of disagreement. He wrote: "Quite certainly, the point of view of [using] visualizable pictures, which de Broglie and I assume, has not been carried through nearly far enough in order to render an account of the most important facts [of atomic theory]. It is of course probable that here and there a wrong path was taken that must now be abandoned. But that, even if one is Niels Bohr, one could possibly say at this point: the visualizable wave pictures work as little as the visualizable point [-particle] models, there being something in *the results of observation* which cannot be grasped by our erstwhile way of thinking; this I do not believe. I believe it even less since for me the comprehensibility of the external processes in nature is an axiom, say, in the sense: to *grasp* experience means nothing more than establishing the *best possible* organization among the different facts of experience." (See letter from Schrödinger to Wien, October 21, 1926, in the *Wien Collection*, Deutsches Museum, Munich.) In Schrödinger's opinion, the facts of experience could not contradict each other, as Bohr—for many years—had tended to assume in atomic physics; only the "theoretical connections" *("gedankliche Verbindungsglieder")* or the physical interpretations could do so. In particular, it seemed "premature" *("voreilig")* to give the "completely general conceptions of space and time and the connection of the interaction of neighboring space-time points," concepts that had been preserved even in general relativity theory. (See letter from Schrödinger to Wien, October 21, 1926, in the *Wien Collection*, Deutsches Museum, Munich.)

Schrödinger further remarked to Wien: "I can only say that I don't care at all for this whole play of waves, if it should turn out to be nothing more than a comfortable computational device to evaluate matrix elements. (Heisenberg had written to Pauli in his letter of June 8, 1926; for the letters from Heisenberg to Pauli, see Refs. 67, 68, 71; see also letter from Heisenberg to Pauli in Ref. 71; and Ref. 20, Vol. 5, Part 2, p. 741.) In spite of all these theoretical points of dispute, however, the relationship with Bohr, and especially Heisenberg, both of whom behaved toward me in a touchingly kind, nice, caring and attentive manner, was totally, cloudlessly amiable and cordial." (See letter from Schrödinger to Wien, October 21, 1926, in the *Wien Collection*, Deutsches Museum, Munich.)

The reports from Copenhagen on the results of the discussions with Schrödinger sounded a little different. For example, Niels Bohr wrote to Ralph Fowler on October 26, 1926: "... The discussions gradually centered themselves on the problems of physical reality of the postulates of atomic theory. We all agreed that a continuity theory [such as Schrödinger's] leads to expectations fundamentally different from those of the usual discontinuity theory [of Born, Heisenberg, Jordan, and Dirac]. Schrödinger himself continued in the hope that the idea of stationary states and transitions was altogether avoidable, but I think we succeeded at least in convincing him that for the fulfilement of his hope he must be prepared to pay a cost, as regards reformation of fundamental concepts, formidable in comparison with that hitherto contemplated by the supporters of the idea of a continuity theory of atomic phenomena." (N. Bohr to R. H. Fowler, October 26, 1926, *Bohr Archives*, quoted in Ref. 20, Vol. 5, Part 2, Chapter IV, and in K. Stolzenburg, cited in Ref. 14). Schrödinger obviously believed that matrix mechanics implied that single stationary states possessed no physical reality, a point of view which Bohr found to be "a confounding of the means and aims of Heisenberg's theory." (N. Bohr to R. H. Fowler, October 26, 1926, *Bohr Archives*, quoted in Ref. 20, Vol. 5, Part 2, Chapter IV, and in K. Stolzenburg, cited in Ref. 14). On the other hand, Bohr considered wave mechanics "so wonderfully suited to bring out the true correspondence between the quantum theory and the classical ideas." (N. Bohr to R. H. Fowler, October 26, 1926, *Bohr Archives*, quoted in Ref. 20, Vol. 5, Part 2, Chapter IV, and in K. Stolzenburg, cited in Ref. 14). This correspondence was explored by Bohr and his collaborators in the months following Schrödinger's visit to Copenhagen and helped enormously in establishing the physical interpretation of quantum mechanics. The efforts of the Copenhagen physicists on the physical interpretation of quantum mechanics led, in particular, to Heisenberg's uncertainty relations and to Bohr's complementarity view—which became the central parts of the so-called Copenhagen interpretation of quantum mechanics.

6. MAX BORN'S STATISTICAL INTERPRETATION OF THE WAVE FUNCTION

A new interpretation of Schrödinger's wave function was proposed in connection with the quantum mechanical study of atomic scattering processes. In order to account for the quantum mechanical explanation of the process of collision between a free particle—such as an α-particle or an electron—and an atom, Born employed the formalism of Schrödinger's wave mechanics. Born first gave a preliminary account of his quantum mechanical treatment of collision processes.[9] In this paper he made the statement that $|\psi_{nm}^{(E)}(\alpha, \beta, \gamma)|^2$ and not $\psi_{nm}^{(E)}$ measures the probability that the electron which approaches the scattering center, say along the z axis, is found scattered in the direction defined by the unit vector whose components are α, β, γ; $E = h^2/2m\lambda^2$ is the energy of the electron, and $p = h/\lambda$ is its momentum, h being Planck's constant and λ the de Broglie wavelength. Born's probabilistic interpretation of the wave function was noted in Einstein's earlier work.[72] In the collision experiments, Born took into account corpuscular aspects and sought to associate the wave function with the particles. In this, he was primarily influenced by Einstein's conception of the relation between the light quanta and the field of electromagnetic waves. Einstein had regarded the electromagnetic wave field as a kind of "ghost field" *("Gespensterfeld")* whose waves served to guide the motion of the corpuscular light quanta, and the squared wave amplitudes (intensities) determined the probability of the presence of light quanta. (Letter from Born to Einstein, November 30, 1926, quoted in Ref. 73; Einstein's reply to Born, December 4, 1926, Ref. 23, pp. 90-91.) Born argued that just as the intensity of light waves was a measure of the density of light quanta, "it was almost self-understood to regard $|\psi|^2$ as the probability density of particles." (See Ref. 72, p. 285.) In addition, Born's probability interpretation was also rooted in the Bohr–Kramers–Slater theory of radiation and their conception of the virtual radiation field.[37] As Heisenberg, in recalling their treatment of induced and spontaneous emission, remarked: "In the summer of 1926 Born established his theory of collision processes and interpreted correctly the wave in multidimensional configuration space as a probability wave by developing and elaborating an idea previously expressed by Bohr, Kramers, and Slater." (Ref. 47, pp. 40-47).

In two papers, Born developed the theory of collision processes further. (See Ref. 9 and Ref. 72, p. 285; see also Ref. 20, Vol. 5, Part 2, Chapter IV.) After interpreting ψ as a probability wave, Born expanded ψ in terms of a complete orthonormal set of eigenfunctions ψ_n of the Schrödinger equation, $H\psi = E\psi$, Eq. (3), as

$$\psi = \sum c_n \psi_n \tag{4}$$

In accordance with the completeness relation,

$$\int |\psi(q)|^2 \, dq = \sum_n |c_n|^2 \tag{5}$$

How were c_n to be interpreted? Since for a single normalized eigenfunction $\psi(q)$, corresponding to a single particle, the right-hand side of Eq. (5) is unity, Born concluded that $\int |\psi(q)|^2 \, dq$ represents the number of particles and $|c_n|^2$ the statistical frequency of the occurrence of the state characterized by the index n. Born justified this assumption by calculating the "expectation value" of the energy E for ψ and obtained

$$E = \sum_n |c_n|^2 E_n \tag{6}$$

where E_n is the energy eigenvalue of ψ_n. Born's statistical interpretation of the wave function was immediately applied with resounding success to various problems of atomic scattering.[74]

Schrödinger had been confronted with the probability interpretation of the wave function soon after its conception. He had seen the proofs of Max Born's paper on the collision problem in wave mechanics (See Ref. 9, Ref. 72, p. 285, and Ref. 20, Vol. 5, Part 2, Chapter IV.) prior to publication and, in a letter to Wien, he had confided his concern about the probability interpretation of the wave function. (See letter from Schrödinger to Wien, August 23, 1926, in the *Sommerfeld and Wien Collections*, Deutsches Museum, Munich; cited in Ref. 20, Vol. 5, Part 2, p. 827.) In the discussions in Copenhagen, the question of the probability interpretation did play a role. As Oskar Klein recalled after several decades: "Schrödinger wanted to interpret that which Born interpreted as probability density as really the density of the electrons. Then they [i.e., Bohr and Heisenberg] showed that if that was so and if by means of his currents and densities he coupled the thing to the electromagnetic field, then the probability of the spontaneous emission would be proportional to the number of atoms in the upper state multiplied by the number of atoms in the lower state. It would be quite against anything already known." (Oscar Klein, AHQP Interview, July 16, 1963, p. 2). Still, Schrödinger remained unshaken, and shortly afterwards he wrote to Max Born: "I have, however, the impression that you and others, who essentially share your opinion, are too deeply under the spell of those concepts (like stationary states, quantum jumps, etc.), which have obtained civic rights in our thinking in the last dozen years; hence you cannot do full justice to an attempt to break away from this scheme of thought." (See letter from Schrödinger to Born, November 2, 1926, Ref. 20, Vol. 5, Part 2, Chapter IV, p. 829.)

Evidently, Schrödinger counted the probability interpretation among the essential concepts, arising from Bohr's atomic theory of 1913, which he now wanted to abolish. He wrote to Bohr: "What is before my eyes, is only one thesis: one should not, even if a hundred trials fail, give up the hope of arriving at the goal—I do not say by means of classical pictures, but by logically consistent conceptions—of the real structure of space-time processes. It is extremely probable that this is possible." (See letter from Schrödinger to Bohr, Ref. 20, Vol. 5, Part 2, Chapter IV, p. 829.) No, Schrödinger would not easily give up what he considered to be his program of a visualizable *(anschauliches)* understanding of what happens in atoms and molecules. With this program, Schrödinger acted against the entire development which atomic theory had pursued from 1913 under the leadership of Niels Bohr and which had eventually led to the quantum mechanical theory proposed by Heisenberg, Born, Jordan, and Dirac.

7. TRANSFORMATION THEORY OF PAUL DIRAC, PASCUAL JORDAN, AND FRITZ LONDON

With Born's statistical interpretation of the wave function in hand in July 1926 (see Ref. 9, Ref. 72, p. 285, and Ref. 20, Vol. 5, Part 2, Chapter IV), serious and prolonged discussions began about the fundamental physical meaning of quantum mechanics as represented by the two schemes—Born, Heisenberg, Jordan, and Dirac's scheme, on the one hand, and that of Schrödinger, on the other. Their equivalence was established rigorously by the transformation theory of Dirac,[10] Jordan,[12] and Fritz London[11] by late fall 1926, and the question of physical interpretation became paramount. Bohr, Heisenberg, Pauli, and Schrödinger, primarily, took part in these discussions.

The problem of the interpretation of quantum theory had occupied Niels Bohr increasingly since 1923 when the question of the nature of radiation became crucial for the understanding of the Compton effect. For Heisenberg, who had eagerly pressed forward by abandoning the use of classical concepts such as electron orbits in atoms, the problem of the interpretation arose late in 1925 when he thought about the simultaneous existence of the discrete energy spectrum of electrons bound in atoms and the continuous spectrum of free electrons moving along well-defined paths. It now occurred to him that, in some sense which was not yet clear, a space-time description should also be possible for the electrons in atoms.

In the fall of 1926 Heisenberg returned to the question of the space-time description of electron's behavior in the atom. Pauli pointed out to him that Schrödinger's wave function could be considered in momentum space, as $\psi(p)$, just as well as in coordinate space, as $\psi(q)$, to which Heisenberg responded: "The fundamental equivalence of p and q pleases me very much. Thus, in the wave formulation, the equation $pq - qp = h/2\pi i$ always corresponds to the fact that it makes no sense to speak of a monochromatic wave at a definite moment (or in a very small time interval)." (Heisenberg to Pauli, October 28, 1926, Ref. 71). At this place, in the margin of the letter, Pauli noted: "It also makes no sense to speak of a state (energy) in a time interval which is small compared to the period [because the state or the energy can be defined only over the entire period]." In his letter, Heisenberg continued: "If the [spectral] line may be taken as being not too sharp, i.e., the time interval is not too small, that of course makes sense. Analogously, there is no point in talking about the position of a particle of a definite velocity. However, it makes sense if one does not consider the velocity and the position too accurately. It is quite clear that, macroscopically, it is meaningful to talk about the position and velocity of a body." (Heisenberg to Pauli, October 28, 1926, Ref. 71).

Thus far, Heisenberg had only vaguely formulated his ideas about a "coarse" space-time description, reflecting his new understanding based upon wave mechanics. In the fall of 1926 Heisenberg was in Copenhagen, where he had taken up his new duties as a lecturer as the successor of Kramers who had been appointed to a professorship in Utrecht. Bohr, with whom he discussed daily, had been developing his own approach to the problem of the interpretation by emphasizing *the duality of the wave and particle pictures in quantum theory*. Heisenberg preferred to abide by the quantum mechanical scheme, as formulated by Born, Heisenberg, and Jordan, and by Dirac; he believed that the wave features should be brought in only by means of the transformation theory which Dirac(10) had worked out in Copenhagen in the fall of 1926.

Dirac had shown conclusively that the matrix S, employed in solving the problem of the principal axis transformation in the case of a Hermitian Hamiltonian function $H(p, q)$, could be identified with Schrödinger's wave function. In other words, for each column vector, there exists the identity

$$S_{q,E} = \psi_E(q) \tag{7}$$

where E is the discrete or continuous eigenvalue of the energy matrix. In order to handle the problem of continuous indices, Dirac introduced the *delta-function*, δ; with its help, the momentum p, conjugate to a continuous position variable q, could be written formally as

$$p(q', q'') = \frac{h}{2\pi i}\,\delta'(q'-q'') = \frac{h}{2\pi i}\frac{\partial}{\partial q'} \tag{8}$$

and the Born–Jordan matrix equation for diagonalizing the Hamiltonian H,

$$H(p, q)\, S_E(q) = E \cdot S_E(q) \tag{9}$$

could thus be transformed into Schrödinger's wave equation,

$$H\left(q, \frac{h}{2\pi i}\frac{\partial}{\partial q}\right)\psi_E q = E\psi_E q \tag{3), (10}$$

8. NIELS BOHR'S DISCUSSIONS WITH HEISENBERG IN FALL 1926 AND THE FORMULATION OF THE UNCERTAINTY PRINCIPLE

The discussions [Ref. 20, Vol. 2 (1982) and Vol. 6 (1988); Stolzenburg, cited in Ref. 14], sometimes stormy, between Bohr and Heisenberg about the interpretation of quantum mechanics had begun soon after Schrödinger's visit to Copenhagen, and continued in the following winter months. Pauli was kept informed by Heisenberg about the course of these discussions. Heisenberg noted: "During these months I spoke with Bohr almost daily about the fundamental problems of quantum theory. Bohr sought to make the duality between the wave picture and the corpuscular picture as the starting point of the physical interpretation, while I tried to derive my conclusions—without the help of wave mechanics—by appealing only to quantum mechanics and Dirac's transformation theory." (See Ref. 47, p. 45; and O. Klein, AHQP Interview, February 28, 1963, p. 10.) These two different starting points led Heisenberg to the indeterminacy relations and Bohr to the principle of complementarity. As Heisenberg recalled: "Bohr and I tried from different angles and therefore it was difficult to agree. Whenever Bohr could give an example in which I couldn't find the answer, then it was clear that we had not understood what the actual situation was ··· Shortly after Christmas, we both were in a kind of despair. In some way we couldn't agree and so we were a bit angry about it. So about mid-February 1927 Bohr left for a skiing vacation in Norway. Earlier he had thought about taking me with him, but then he decided against it. He wanted to be alone and think, while I stayed on in Copenhagen." (See W. Heisenberg, AHQP Interview, February 25, 1963, p. 16; conversations with J. Mehra, Geneva, July 1962.) Heisenberg made an effort to bring some order into his thoughts and results of the past few months. Bohr spent his winter vacation in Norway from about the middle of February to mid-March 1927. (See letter from O. Klein to M. Saha on behalf of N. Bohr, February 18, 1927, cited in K. Stolzenburg, Ref. 14.)

On February 23, 1927, Heisenberg wrote a long letter to Pauli, in which he dealt with the problem of observing simultaneously the position and momentum of atomic systems. He stated that the "commutation relation," $pq - qp = h/2\pi i$, has the following physical interpretation: Given the exact momentum p of an electron in an atom, its position is then completely undetermined, and *vice versa* (Heisenberg to Pauli, February 23, 1927, in Ref. 71). To support this point of view, and to render it more visual, Heisenberg discussed the thought experiment *(Gedanken-experiment)* for the observation of an electron by means of a γ-ray microscope, an analogy which occurred to him from his doctoral oral examination under Wilhelm Wien several years before (Ref. 20, Vol. 2, Chapter I. 8; also Ref. 35, p. 10) Then he turned to the exact calculation of the accuracy involved in the observation of p and q.

The probability amplitude of the position of an object, which lies within the space interval $q_0 - q_1 < q < q_0 + q_1$, is given by

$$S(q) = \text{const} \cdot \exp\left[-\frac{(q-q_0)^2}{2q_1^2} - \frac{2\pi i p_0(q-q_0)}{h} \right] \tag{11}$$

where the first term represents a Gaussian distribution and the second, the general wave function. From $S(q)$, he determined $S(p)$ with the help of the transformation equation

$$\begin{aligned} S(p) &= \int S(q)\, e^{2\pi i pq/h}\, dq \\ &= \text{const} \cdot \exp\left[-2\pi^2 q_1^2 (p - p_0^2)^2 + \frac{2\pi i}{h}(p - p_0)\, q_0 \right] \end{aligned} \tag{12}$$

Hence, for a given uncertainty $\delta q = q_1$ in the position, the probability distribution, $|S(p)|^2$, of the momentum p is nonzero in the region $p_0 - p_1 < p < p_0 + p_1$, such that

$$\frac{4\pi^2 q_1^2 p_1^2}{h^2} \approx 1 \tag{13}$$

The simultaneous measurement of the position and momentum of an electron is thus limited by the *uncertainty relation*,

$$\delta p \cdot \delta q \approx \frac{h}{2\pi} \tag{14}$$

Heisenberg asked Pauli for his severe criticism [*"unnachsichtige Kritik"*] (Heisenberg to Pauli, February 23, 1927, in Ref. 71) As he recalled: "I wanted to get Pauli's reaction before Bohr was back because I felt that when Bohr comes back he will be angry with my interpretation. So I first wanted to have some support, and see whether somebody else liked it. Now Pauli's reaction was extremely enthusiastic. He said something like *"Morgenröte der Neuzeit"*—the light of day in quantum theory." (See Heisenberg, AHQP Interview, February 25, 1963, pp. 16, 17.) Pauli at once approved Heisenberg's ideas on the uncertainty principle, and thought that this interpretation endowed quantum mechanics with a coherent physical meaning. Heisenberg informed Jordan that he had derived a physical interpretation of Dirac's[10] and Jordan's[12] mathematical work on transformation theory. (See Heisenberg to Jordan, March 7, 1927, quoted in Stolzenburg, Ref. 14, p. 128). After receiving Pauli's favorable verdict, Heisenberg informed Bohr: "I myself have worked very vigorously during recent weeks in order to carry through the program (with respect to the Dirac–Jordan quantum mechanics [i.e., transformation theory]), about which we talked before your departure. I believe that I have fully succeeded. The case, in which p *as well as* q are given with a certain accuracy, can be formulated without going beyond the Dirac–Jordan mathematics.... Further, one finds that the transition from "micro- to macromechanics"[75] can be understood very easily: the classical mechanics is entirely a part of quantum mechanics. As for the old question concerning 'statistical or causal law,' the situation is this: one *cannot* say that quantum mechanics is statistical. However, one can obtain only statistical results, if one wants to calculate 'future events' from the 'present,' since one cannot take into account all the initial conditions of the present." (Heisenberg to Bohr, March 10, 1927, cited in Stolzenburg, Ref. 14, p. 128).

Heisenberg's paper on the uncertainty principle in quantum mechanics was received by *Zeitschrift für Physik* on March 23, 1927.[13] He began his considerations by making the following statement of his conviction: "We regard a physical theory to be perceptual [*anschaulich*] [only] if we can think of the experimental consequences of this theory qualitatively in all simple cases, and [when] we have recognized that the application of this theory never leads to inner contradictions." (Ref. 13, p. 172). The visualizable interpretation of quantum mechanics had thus far been full of contradictions; for this reason Heisenberg believed that this interpretation could not be consistent with the usual kinematic and mechanical concepts. "Quantum mechanics arose exactly out of the attempt to break away from those habitual kinematic concepts, and substitute in their place concrete, experimentally given magnitudes. Since this seems to have been achieved, the mathematical scheme of quantum mechanics would therefore not require a revision." (Ref. 13, p. 172) Heisenberg investigated the conditions under which the concepts of position, orbit, velocity, and energy of classical physics could be taken over in quantum theory. He concluded: "All concepts, which are needed to describe a mechanical system in classical physics, can also be defined—analogously to classical notions—exactly for atomic processes. The experiments, which serve to define them, also lead to an inherent indeterminacy, if we demand from them the simultaneous determination of two canonically conjugate quantities.... In this case, the quantum theory can be closely compared to the relativity theory." (Ref. 13, p. 172). The speed of light enters into the definition of simultaneity in relativity theory. The experiments always yield a finite light velocity in agreement with the postulate of a constant velocity of light; hence this postulate does not contradict the obvious notions of position, velocity, and time in relativity theory.

In his special relativity theory (1905), Einstein[49] had emphasized the fundamental importance of employing "observable magnitudes" only in the construction of a physical theory. This conception of Einstein had guided Heisenberg in his discovery of a quantum-theoretical kinematics.[2] But when in April 1926 Heisenberg met Einstein in Berlin, the latter had told him: "... it may be heuristically useful to keep in mind what one has actually observed. But on principle, it is quite wrong to try founding a theory on observable magnitudes alone. In reality the very opposite happens. It is the theory which decides what we can observe." (Ref. 35, p. 46; Ref. 41, p. 63).

With this conceptual framework in mind, Heisenberg applied the Dirac–Jordan transformation theory to obtain Eq. (14) for the indeterminacy in the simultaneous measurement of p and q, about which he had written to Pauli (Heisenberg to Pauli, February 23, 1927, in Ref. 71). He concluded: "The more accurately is the position determined, the more uncertain is the momentum and *vice versa*; in this we find a directly visualizable explanation of the relation

$$pq - qp = \frac{h}{2\pi i} \qquad (1), (15)$$

(Ref. 13, p. 172.) Heisenberg obtained the relation corresponding to Eq. (14) for the simultaneous determination of the energy E and time t

from an analysis of the Stern–Gerlach experiment. Also, in the experiment on resonance fluorescence light, Heisenberg recognized the fact hat to the extent the phase w is exactly measurable, to that extent the determination of energy is uncertain, corresponding to the relation

$$Jw - wJ = \frac{h}{2\pi i} \tag{16}$$

where J and w are the angular momentum and phase respectively.

Heisenberg finally drew the following consequences from his analysis: "We have not assumed that the quantum theory, in contrast to classical theory, is a statistical theory in the sense that from precisely given data only statistical conclusions can be drawn. Against such assumptions, the well-known experiments of Geiger and Bothe may be cited. Moreover, in all cases, in which in classical theory relations exist between magnitudes that are exactly measurable, corresponding relations also exist in quantum theory (e.g., the momentum-energy conservation). However, in the exact formulation of the law of causality that, "If we know the present precisely, we can determine the future," it is not the consequence but the initial assumption which is wrong. In principle, we cannot determine all the initial conditions of the present.... The real essence can be best characterized as follows: All the experiments are subject to the laws of quantum mechanics and, thereby, to Eq. [(14)] (Ref. 13, p. 191)."

On returning from his vacation in Norway, Bohr was not immediately satisfied with Heisenberg's formulation of the "intuitive content of the quantum-theoretical kinematics and mechanics."[(13)] As Heisenberg recalled: "When Bohr came back I showed him [my] paper... and I showed him Pauli's reaction. I did realize that Bohr was a bit upset about it because he still felt that it was not quite clear what I had written—not in every way clear. At the same time he saw Pauli's reaction, and he knew Pauli was very critical, so he felt it should, in some way, be right." (W. Heisenberg, AHQP Interview, February 25, 1963, p. 17). Bohr felt that Heisenberg had not treated the thought-experiments with the γ-ray microscope and the investigations on the Compton effect and resonance fluorescence light quite properly; his fundamental objection was something quite different. As Heisenberg recalled: "The main point was that Bohr wanted to take this dualism between waves and corpuscles as the central point of the problem ··· I [on the other hand] would say: 'We have a consistent mathematical scheme and [it] tells us everything which can be observed. [There is] nothing in nature which cannot be described by this mathematical scheme.' It was a different way of looking at the problem because Bohr would not like to say that nature imitates a mathematical scheme, that nature does only things which fit into a mathematical scheme." (W. Heisenberg, AHQP Interview, February 25, 1963, p. 18). Bohr suggested making changes in Heisenberg's paper, but Heisenberg was not at all willing. However, in a postscript added in proof, he incorporated Bohr's suggestions (Ref. 13, pp. 157-198, postscript added in proof).

The discussions between Bohr (assisted by Oskar Klein) and Heisenberg about the thought-experiments and their interpretation continued during the following weeks, often leading to misunderstandings. Heisenberg reported about these discussions in letters to Pauli, Kronig, and Dirac (letters from Heisenberg to Pauli, April 4, 1927, to Kronig, April 8, 1927, to Pauli, April 16, 1927, and to Dirac, April 27, 1927, in Ref. 71). By the end of May the points of view of Bohr and Heisenberg had come closer together, as Heisenberg reported to Pauli (letter from Heisenberg to Pauli, May 31, 1927, in Ref. 71), and after Pauli's visit to Copenhagen in June 1927, harmony between Bohr and Heisenberg had been restored (letter from Heisenberg to Bohr, June 18, 1927, *Bohr Archives*; Heisenberg, AHQP Interview, February 25, 1963, pp. 17, 18). On April 13, 1927, Bohr had already sent the proof-sheets of Heisenberg's paper on the uncertainty principle to Einstein (letter from Bohr to Einstein, April 13, 1927, quoted in Ref. 21, pp. 125, 126). Heisenberg's interpretation, embodied in the uncertainty principle, Eq. (14), could be generalized to any pair of conjugate dynamical variables, and was soon accepted as "the real core of the new theory."[76]

9. THE PRINCIPLE OF COMPLEMENTARITY

Following Niels Bohr's discussions with Schrödinger in Copenhagen in early October 1926, during which the question of "quantum jumps" often came up, and his discussions with Heisenberg in the subsequent winter months, especially after Heisenberg's formulation of the uncertainty principle in February–March 1927, Bohr reflected deeply upon the meaning of the fundamental equations of quantum theory. It has been pointed out by several authors (Ref. 72, p. 345; Stolzenberg, Ref. 14; Ref. 20, Vol. 6, Book I, Part 2, 1988; Ref. 77), that—for Bohr—Heisenberg's uncertainty relations were "a confirmation of the conceptions he had been groping for long before Heisenberg derived his principle from the Dirac–Jordan transformation theory. True, Heisenberg's work prompted Bohr to give his thoughts on complementarity a consistent and final formulation, but these thoughts ··· can be traced back at least to July, 1925." (Ref. 72, p. 345). In the four-page letter, which he sent to Einstein on April 13, 1927 together with Heisenberg's paper (letter from Bohr to Einstein, April 13, 1927, quoted in Ref. 21, pp. 125, 126), Bohr gave his own views concerning it and the general questions of quantum theory, and—in a way—gave expression to his developing ideas on the principle of complementarity (Ref. 21, p. 125).

In the public domain, Bohr discussed for the first time his ideas on complementarity in a lecture on September 16, 1927 at the International Congress of Physics that was convened to commemorate the hundredth anniversary of Alessandro Volta's death.[16,72] (Among the participants at the Volta Conference mentioned in Ref. 72, p. 351, footnote 9, "Bose" was not S. N. Bose of Bose–Einstein statistics, but D. N. Bose—who, while at Calcutha, had received the invitation that had been right fully intended for S. N. Bose to attend the Votta Conference. See S. N. Bose to J. Mehra, in conversations on August 30, 1970, Ref. 78.) Bohr gave a lecture on "The Quantum Postulate and the Recent Development of Atomic Theory."[16] Bohr gave essentially the same lecture at the fifth Solvay Conference in Brussels in October 1927,[17] and at the Danish Academy of Sciences (*Videnskabernes Selskab*).[72] Bohr began his lecture at Como by saying: "I shall try by making use only of the simple considerations and without going into any details of technical mathematical character to describe to you a certain general point of view which I believe is suited to give an impression of the general trend of the development of the theory from its very beginning and which I hope will be helpful in order to harmonize the apparently conflicting views taken by different scientists." (Ref. 16 and Ref. 72, footnote 78, p. 349). Bohr emphasized the distinction between the classical description of a natural phenomenon—based on the assumption that it may be observed without significant disturbance—and the quantum description of atomic processes to which a quantum discontinuity, or "individuality," has to be attributed. In his manuscript (dated September 13, 1927) he had considered the causal space-time description according to the "quantum postulate" as follows: "Characteristic of the quantum theory is the acknowledgment of a fundamental limitation in our classical physical ideas when applied to atomic phenomena. Just on account of this situation, however, we meet intricate difficulties when attempting to formulate the contents of the quantum theory in terms of concepts borrowed from classical theories. Still it would appear that the essence of the theory may be expressed through the postulate that any atomic process open to direct observation involves an essential element of discontinuity or rather individuality completely foreign to the classical ideas and symbolized by Planck's quantum of action. This postulate at once implies a resignation as regards the causal space-time coordination of atomic phenomena." (N. Bohr, Manuscript on "Fundamental problems of the quantum Theory," September 13, 1927, *Bohr Archives*; cited in K. Stolzenburg, Ref. 14, pp. 156, 157). At Como, Bohr declared: "The very nature of the quantum theory thus forces us to regard the space-time coordination and the claim of causality, the union of which characterizes the classical theories, as complementary but exclusive features of the desription, symbolizing the idealization of observation and definition respectively." (Ref. 16 and Ref. 72, footnote 78, p. 349). Thus Bohr came to the conclusion that the situation in atomic physics could only be described in terms of dual, *complementary* pictures which, in classical physics, exclude each other.[79] The uncertainty relations ensure that no contradiction will arise in the exercise of the *principle of complementarity* in nature; they exclude the possibility of situations occurring that exhibit both the wave and particle aspects of a phenomenon simultaneously.[80] Bohr's statement about complementarity at Como became the essence of what later on came to be called "the Copenhagen interpretation" of quantum theory.

10. THE FIFTH AND SIXTH SOLVAY CONFERENCES AND BOHR'S DISCUSSIONS WITH EINSTEIN

At the fifth Solvay Conference in Brussels, from October 24 to 29, 1927, quantum mechanics, together with the "Copenhagen interpretation," was publicly presented as a consistent and complete theory of atomic phenomena by its numerous protagonists whose leader was Niels Bohr (see Mehra in Ref. 17). The theme of the Conference was "*Électrons et Photons*," and among those present were N. Bohr, M. Born, L. Brillouin, L. de Broglie, A. H. Compton, P. Debye, P. A. M. Dirac, P. Ehrenfest, A. Einstein, R. H. Fowler, W. Heisenberg, H. Kramers, W. Pauli, E. Schrödinger, and M. Planck. H. A. Lorentz presided over the Conference. Schrödinger, who was the last speaker, gave a report on the development of wave mechanics. At the end of Schrödinger's talk, Lorentz requested Bohr to give a report on the epistemological problems confronting quantum physics. Bohr's report contained essentially the substance of his contribution at the Como Conference on September 16, 1927.[16] Einstein had not attended the Como Conference but was present at the fifth Solvay Conference in Brussels, and he heard a comprehensive account of Bohr's ideas on the interpretation of quantum mechanics. Bohr discussed the question of an appropriate terminology, and stressed the viewpoint of complementarity. Bohr's main argument was that the unambiguous communication of physical evidence required that the experimental arrangement as well as the recording of observations had to be expressed in a common language, "suitably refined by the vocabulary of classical physics." (See Mehra in Ref. 17, p. 151). In all actual experimental work this requirement is fulfilled by using measuring instruments such as diaphragms, lenses, and photographic plates, which are so large and heavy that, notwithstanding the decisive role of the quantum of action for the stability and properties of such bodies, all quantum effects could be disregarded in taking account of their positions and motions.

In classical physics one deals with an idealization, according to which all phenomena can be arbitrarily subdivided. The interaction between the measuring instruments and the object under observation can be neglected, or at any rate compensated, in classical physics. Bohr stressed that such interaction represents an integral part of the phenomena in quantum physics, for which no separate account could be given if the instruments would serve the purpose of defining the conditions under which the observations are obtained.

The recording of observations ultimately rests on the production of permanent marks on the measuring instruments, such as the spot produced on a photographic plate by the impact of a photon or electron. The fact that such recording involves essentially irreversible physical and chemical processes does not introduce further complications, but only emphasizes the element of irreversibility involved in the very concept of observation. In Bohr's view, the characteristic new feature in quantum physics is merely the restricted divisibility of phenomena, which requires a specification of all significant parts of the experimental setup for unambiguous description.

Bohr pointed out that since several different individual effects would in general be observed in one and the same experimental arrangement, the recourse to statistics in quantum physics was therefore unavoidable. "Moreover, evidence obtained under different conditions and rejecting comprehension in a single picture must, notwithstanding any apparent contrast, be regarded as complementary in the sense that together they exhaust all well-defined information about the atomic object. From this point of view, the whole purpose of the formalism of quantum theory is to derive expectations for observations obtained under given experimental conditions." (N. Bohr, quoted in Mehra, Ref. 17, p. 152). Bohr emphasized that the elimination of all contradictions is secured by the mathematical consistency of the formalism; the exhaustive character of the description within its scope is indicated by its adaptability to any imaginable experimental arrangement.

Following Bohr's report on the epistemological questions of quantum theory, there took place an extensive general discussion on the renunciation of pictorial deterministic description and the introduction of probability in the new theory. Almost all the physicists who had been invited to attend the fifth Solvay Conference participated in this discussion. A note has survived (from one of the sessions at this Conference), which

Paul Ehrenfest had passed on to Einstein during one of the lectures saying, "Don't laugh! There is a special section in purgatory for professors of quantum theory, where they will be obliged to listen to lectures on classical physics ten hours every day." To which Einstein replied, "I laugh only at their naïveté. Who knows who would have the laugh in a few years?" (Ref. 35, p. 47; Mehra in Ref. 17, p. 152). Einstein had disliked from the beginning the Göttingen–Copenhagen quantum-theoretical formulation of atomic problems with its emphasis on discontinuity and acausality. Thus, he had written to Ehrenfest: "I look upon quantum mechanics with admiration and suspicion" (letter from Einstein to Ehrenfest, August 28, 1926, Einstein Archives). And, to Born, he had written: "Quantum mechanics is certainly imposing. But an inner voice tells me that it is not the real thing [*der wahre Jakob*]. The theory says a lot, but does not really bring us closer to the secret of the 'old one' [*Geheimnis des Alten*]. I, at any rate, am convinced that *He* is not playing at dice." (letter from Einstein to Born, December 4, 1926; Ref. 23, pp. 90, 91). The exchanges of views that started at the sessions at the fifth Solvay Conference were eagerly continued within smaller groups during the evenings. Bohr had extensive opportunity of having long discussions with Einstein and Ehrenfest. Einstein was particularly reluctant to renounce deterministic description in principle. It was against his scientific belief to accept statistical quantum mechanics as a consistent and complete description of physical reality. He challenged Bohr and the other proponents of the new quantum mechanics with arguments suggesting the possibility of taking the interaction between atomic objects and the measuring instruments more explicitly into account. He proposed cleverly designed thought-experiments to overcome Heisenberg's uncertainty principle, but Bohr was able to recognize the fallacy in Einstein's reasoning and to refute his arguments (Ref. 21, pp. 109-121).

The Bohr–Einstein discussions were resumed at the sixth Solvay Conference, which took place in Brussels from October 20 to 25, 1930. H. A. Lorentz had died in 1928, and Paul Langevin presided over the Conference, which was devoted to the study of the magnetic properties of matter (Ref. 19; Ref. 17, pp. 183-205; Ref. 21, pp. 132-136). Einstein had not been convinced by the answers of Bohr and his colleagues at the previous conference, and the problem of the foundations and interpretation of quantum mechanics became a major subject of discussion outside the official sessions of the sixth Solvay Conference. Einstein again devised clever thought-experiments to demonstrate the violation of the uncertainty principle. Bohr used the conclusion of Einstein's own general relativity theory and the equation $E = mc^2$ to refute Einstein's arguments with his photon-box thought-experiment, showing that he was led to $\Delta E \, \Delta t > h$ in accordance with the uncertainty principle.

In fall 1933 Einstein left for the United States and settled in Princeton, New Jersey, where he continued his scientific activity. By that time he had accepted the "consistency" of nonrelativistic quantum mechanics. In 1935, with Boris Podolsky and Nathan Rosen, Einstein wrote an article in which he raised the question whether quantum mechanical description was "complete",[81] to which Bohr replied immediately.[82] The argument was soon joined in by Schrödinger,[83] and the Bohr–Einstein dialogue concerning classical determinism versus statistical causality was continued until Einstein's death in April 1955. In 1949, in a 70th birthday tribute to Einstein, Niels Bohr gave a beautiful account of his discussions with Albert Einstein over the years (Ref. 27; a full account of the Bohr–Einstein discussions is intended for Ref. 20, Vol. 6, Book I, Part 2, to appear in 1988). Since 1927, when the Bohr–Einstein discussions began in earnest at the fifth Solvay Conference, the questions concerning the physical and philosophical aspects of the interpretation and foundation of quantum mechanics have become the central theme of the investigations of a growing number of physicists and philosophers of science. Initially, however, the discussions between, and the formulations and interpretations of, Bohr, Einstein, Heisenberg, and Schrödinger had sparked this growing debate.

BIBLIOGRAPHY OF PRINCIPAL SOURCES

W. Heisenberg, *Physics and Beyond*, Ref. 41.

M. Jammer, *The Philosophy of Quantum Mechanics*, Ref. 21.

M. Jammer, *The Conceptual Development of Quantum Mechanics*, Ref. 72.

J. Mehra, *The Solvay Conferences on Physics: Aspects of the Development of Physics since 1911*, Ref. 17.

J. Mehra, *The Birth of Quantum Mechanics:* Werner Heisenberg Memorial Lecture, Ref. 35.

J. Mehra and H. Rechenberg, *The Historical Development of Quantum Theory*, Ref. 20.

K. Stolzenburg, "Die Entwicklung des Bohrschen Komplementaritätsgedankens in den Jahren 1924 bis 1929," Ref. 14.

REFERENCES

1. *On the Constitution of Atoms and Molecules*, Papers of 1913 reprinted from *Philosophical Magazine* with an Introduction by L. Rosenfeld (Munksgaard Ltd., Copenhagen; W. A. Benjamin, New York, 1963).
2. W. Heisenberg, "Über quantentheoretische Umdeutung kinematischer und mechanischer Beziehungen," *Z. Phys.* **33**, 879–893 (1925).

3. M. Born and P. Jordan, "Zur Quantenmechanik," *Z. Phys.* **34**, 858–888 (1925); M. Born, W. Heisenberg, and P. Jordan, "Zur Quantenmechanik II," *Z. Phys.* **35**, 557–615 (1926).
4. P. A. M. Dirac, "The fundamental equations of quantum mechanics," *Proc. R. Soc. London A* **109**, 642–653 (1925).
5. E. Schrödinger, *Collected Papers on Wave Mechanics* (Blackie and Son, Ltd, London and Glasgow, 1928).
6. E. Schrödinger, "Über das Verhältnis der Heisenberg–Born–Jordanschen Quantenmechanik zu der meinen," *Ann. Phys.* **79**, 734–756 (1926).
7. W. Pauli, *Briefwechsel/Scientific Correspondence*, Vol. I, (Springer-Verlag, New York, Heidelberg, and Berlin, 1979).
8. C. Eckart, "The solution of the problem of the simple oscillator by a combination of the Schrödinger equation and the Lanczos theories," *Proc. Natl. Acad. Sci. USA* **12**, 473–476 (1926); "Operator calculus and the solution of the equations of quantum dynamics," *Phys. Rev.* **28**, 927–935 (1926).
9. M. Born, "Zur Quantenmechanik der Stoßvorgänge (Vorläufige Mitteilung)," *Z. Phys.* **37**, 863–867 (1926); "Quantenmechanik der Stoßforgänge," *Z. Phys.* **38**, 803–827 (1926).
10. F. London, "Winkelvariable und kanonische Transformationen in der Undulationsmechanik," *Z. Phys.* **40**, 193–210 (1926).
11. P. A. M. Dirac, "The physical interpretation of quantum dynamics," *Proc. R. Soc. London A* **113**, 621–641 (1927).
12. P. Jordan, "Über eine neue Begründung der Quantenmechanik," *Z. Phys.* **40**, 809–838 (1927).
13. W. Heisenberg, "Über den anschaulischen Inhalt der quantentheoretischen Kinematik und Mechanik," *Z. Phys.* **43**, 172–198 (1927).
14. W. Heisenberg, *Physics and Beyond: Encounters and Conversations* (Harper and Row, New York, 1971); J. Mehra and H. Rechenberg, *The Historical Development of Quantum Theory*, Vol. 5, Part 2 (Springer Verlag, New York, Heidelberg, and Berlin, 1977), Chapter IV, Section 5; K. Stolzenburg, "Die Entwicklung des Bohrschen Komplementaritätsgedankens in den Jahren 1924 bis 1929," Doctoral Dissertation, University of Stuttgart, 1975.
15. J. Mehra and H. Rechenberg, *The Historical Development of Quantum Theory*, Vol. 6: *The Completion of Quantum Mechanics*, Book I, Part 2 (Springer-Verlag, New York, to appear in 1988); J. Mehra and H. Rechenberg, *The Historical Development of Quantum Theory*, Vol. 2 (Springer-Verlag, New York, 1982); J. Mehra, "The birth of quantum mechanics." Heisenberg Memorial Lecture (CERN, Geneva, March 30, 1976).
16. N. Bohr, "The quantum postulate and the recent development of atomic theory," in *Atti del Congresso Internazionale dei Fisici, 11–20 Settembre 1927* (Nicola Zanichelli, Bologna, 1928), Vol. 2, pp. 565–568.
17. *Électrons et Photons—Rapports et Discussions du Cinquième Conseil de Physique Tenu à Bruxelles du 24 au 29 Octobre 1927 sous les Auspices de L'Institut International de Physique Solvay* (Gauthier-Villars, Paris, 1928); J. Mehra, *The Solvay Conferences on Physics: Aspects of the Development of Physics since 1911* (D. Reidel, Dordrecht, Holland/Boston, 1975).
18. J. Mehra and H. Rechenberg, *The Historical Development of Quantum Theory*, Vol. 1, Part 1, Section 11.5, and Part 2, Section VI.4 (Springer-Verlag, New York, 1982).
19. *Le Magnétisme—Rapports et Discussions du Sixième Conseil de Physique sous les Auspices de L'Institut International de Physique Solvay* (Gauthier-Villars, Paris, 1932).
20. J. Mehra and H. Rechenberg, *The Historical Development of Quantum Theory*, Vols. 1–6 (Springer-Verlag, New York, Heidelberg and Berlin, 1982–1988).
21. M. Jammer, *The Philosophy of Quantum Mechanics* (Wiley, New York, 1974), Chapter 5.

22. A. Einstein, "Strahlungs-Emission und -Absorption nach der Quantentheorie," *Verh. Dtsch. Phys. Ges.* **18**, 318–323 (1916); "Quantentheorie der Strahlung," *Mitt. Phys. Ges. Zürich* **16**, 47–62 (1916); "Zur Quantentheorie der Strahlung," *Phys. Z.* **18**, 121–128 (1917).
23. *The Born–Einstein Letters* (Macmillan, London and Basingstoke, 1971).
24. A. Einstein, "Grundlage der allgemeinen Relativitätstheorie," *Ann. Phys.* **49**, 769–822 (1916); J. Mehra, *Einstein, Hilbert and the Theory of Gravitation* (D. Reidel, Dordrecht, Holland and Boston, Massachusetts, 1974).
25. A. Einstein, "Bietet die Feldtheorie Möglichkeiten für die Lösung des Quantenproblems?" *Berliner Ber.*, 359–364 (1923).
26. P. Speziali (ed.), *Albert Einstein, Michele Besso, Correspondance 1903–1955* (Hermann, Paris, 1972).
27. N. Bohr, "Discussion with Einstein on epistemological problems in atomic physics," in *Albert Einstein: Philosopher–Scientist*, P. A. Schilpp, ed. (The Library of Living Philosophers, Inc., Tudor Publishing Company, 1949, 1951; Harper and Row, New York, 1959).
28. A. Einstein, "Über die Entwicklung unserer Anschauungen über das Wesen und die Konstitution der Strahlung," *Phys.* **10**, 817–825 (1909).
29. N. Bohr, "Über die Serienspektren der Elemente," *Z. Phys.* **2**, 423–469 1920). English translation: "On the series spectra of elements," in *Collected Works* **3**, 241–260 (1976).
30. *Albert Einstein–Arnold Sommerfeld Briefwechsel*, A. Hermann, ed. (Schwabe and Co., Basel, Stuttgart, 1968), p. 75 (quoted in M. Jammer, Ref. 21, p. 123).
31. A. H. Compton, "Secondary radiation produced by X-rays," *Bull. Natl. Res. Council* **4** (Part 2), No. 20 (October 1922).
32. A. H. Compton, "A quantum theory of the scattering of X-rays by light elements," *Phys. Rev.* **21**, 483–502 (1923).
33. P. Debye, "Zerstreuung von Röntgenstrahlen und Quantentheorie," *Phys. Z.* **24**, 161–166 (1923).
34. A. Einstein, "Über einen die Erzeugung und Verwandlung des Lichtes betreffenden heuristischen Gesichtpunkt," *Ann. Phys.* **17**, 138–148 (1905).
35. J. Mehra, "The birth of quantum mechanics," Werner Heisenberg Memorial Lecture, CERN, Geneva, March 30, 1976 (CERN 76-10, May 14, 1976).
36. J. C. Slater, "Radiation and Atoms," *Nature (London)* **133**, 307–308 (1924).
37. N. Bohr, H. A. Kramers, and J. C. Slater, "The quantum theory of radiation," *Philos. Mag.* **47**, 785–802 (1924). (In German: "Über die Quantentheorie der Strahlung," *Z. Phys.* **24**, 69–87 (1924).)
38. H. A. Kramers, "The Law of Dispersion and Bohr's Theory of Spectra," *Nature (London)* **113**, 673–674 (1924); "The quantum theory of dispersion," *Nature (London)* **114** 310 (1924).
39. W. Bothe and H. Geiger, "Experimentelles zur Theorie von Bohr, Kramers und Slater," *Naturwissenschaften* **13**, 440–441; "Über das Wesen des Compton-effekts, ein experimenteller Beitrag zur Theorie der Strahlung," *Z. Phys.* **32**, 639–663 (1925).
40. G. E. Uhlenbeck and S. Goudsmit, "Ersetzung der Hypothese vom unmechanischen Zwang durch eine Forderung bezüglich des inneren Verhaltens jedes einzelnen Elektrons," *Naturwissenschaften* **13**, 953–954 (1925); "Spinning electrons and the structure of spectra," *Nature (London)* **117**, 264–265 (1926).
41. W. Heisenberg, *Physics and Beyond*, (Harper and Row, New York, Evanston, and London, 1971), p. 38.
42. H. A. Kramers, "Über den Einfluss eines elektrischen Feldes auf die Feinstruktur der Wasserstofflinien," *Z. Phys.* **3**, 199–223 (1920).

43. N. Bohr, *Collected Works, Volume 4: The Periodic System (1920–1923)*. J. Reed Nelson, de. (North-Holland, Amsterdam, New York, and Oxford, 1977), p. 371.
44. W. Heisenberg, "Quantum theory and its interpretation," in *Niels Bohr: His Life and Work as Seen by His Friends and Colleagues*, S. Rozental, ed. (North-Holland, Amsterdam, 1967), pp. 94–95.
45. W. Heisenberg, "Über eine Anwendung des Korrespondenzprinzips auf die Frage nach der Polarization des Fluoreszenzlichtes," *Z. Phys.* **31**, 617–626 (1925).
46. H. A. Kramers and W. Heisenberg, "Über die Streuung von Strahlung durch Atome," *Z. Phys.* **31**, 681–708 (1925).
47. W. Heisenberg, "Erinnerungen an die Zeit der Entwicklung der Quantenmechanik," in *Theoretical Physics in the Twentieth Century—A Memorial Volume to Wolfgang Pauli*, M. Fierz and V. F. Weisskopf, eds. (Interscience, New York, 1960), pp. 40–47.
48. E. Mach, *Die Mechanik in ihrer Entwicklung*, 7th edn. (F. A. Brockhaus, Leipzig, 1912); English translation: *The Science of Mechanics* (Open Court Publishing Company: Chicago, 1893).
49. A. Einstein, "Zur Elektrodynamik bewegter Körper," *Ann. Phys.* **17**, 891–921 (1905).
50. H. Minkowski, "Raum und Zeit," *Phys. Z.* **10**, 104–111 (1909).
51. M. Born and P. Jordan, "Zur Quantenmechanik," *Z. Phys.* **34**, 858–888 (1925).
52. M. Born, W. Heisenberg, and P. Jordan, "Zur Quantenmechanik II," *Z. Phys.* **35**, 557–615 (1926).
53. W. Lenz, "Über den Bewegungsablauf und die Quantenzustände der gestörten Keplerbewegung," *Z. Phys.* **24**, 197–207 (1924).
54. W. Pauli, "Über das Wasserstoffspektrum von Standpunkt der neuen Quantenmechanik," *Z. Phys.* **36**, 236–363 (1926).
55. C. Lanczos, "Über eine feldmäßige Darstellung der neuen Quantenmechanik," *Z. Phys.* **35**, 812–830 (1926).
56. M. Born, *Problems of Atomic Dynamics*, reprint (M.I.T. Press, Cambrige, Massachusetts, 1970).
57. M. Born and N. Wiener, "A new formulation of the laws of quantization of periodic and aperiodic phenomena," *J. Math. Phys.* M.I.T. **5**, 84–98 (1926).
58. M. Born and N. Wiener, "Eine neue Formulierung der Quantengesetze für periodische und aperiodische Vorgänge," *Z. Phys.* **36**, 174–187 (1926).
59. J. Mehra, "The golden age of theoretical physics: P.A.M. Dirac's scientific work from 1924–1933," in *Aspects of Quantum Theory*, A. Salam and E. P. Wigner, eds. (Cambridge University Press, Cambridge, 1972) pp. 17–59.
60. P. A. M. Dirac, "Quantum mechanics and a preliminary investigation of the hydrogen atom," *Proc. R. Soc. London A* **110** 561–573 (1926).
61. P. A. M. Dirac, "On Quantum algebra," *Proc. Cambridge Philos. Soc.* **23**, 412–418 (1926).
62. P. A. M. Dirac, *Quantum Mechanics*, Dissertation, Cambridge University, May 1926.
63. E. Schrödinger, "Quantisierung als Eigenwertproblem. I," *Ann. Phys.* **79**, 361–376 (1926).
64. H. Weyl, "Singuläre Integralgleichungen mit besonderer Berücksichtigung der Fourierschen Integraltheorems," Göttingen dissertation, February 1908.
65. L. de Broglie, *Recherches sur la Théorie des Quanta*, Thèse (Masson & Cie, Paris, 1924); *Ann. Phys.* **3**, 22–128 (1925); Paris dissertation, November 1924.
66. M. Born, "Erwin Schrödinger†," *Phys. Blätter* **17**, 85–87 (1961).
67. W. Heisenberg, "Mehrkörperproblem und Resonanz in der Quantenmechanik," *Z. Phys.* **38**, 411–426 (1926).
68. W. Heisenberg, "Über die Spektra von Atomsystemen mit zwei Elektronen," *Z. Phys.* **39**, 499–518 (1926).

69. E. Schrödinger, "Quantisierung als Eigenwertproblem (Vierte Mitteilung)," *Ann. Phys.* **81**, 109–139 (1926).
70. *Briefe zur Wellenmechanik*, K. Przibram (ed.) (Springer-Verlag, Viennas 1963); English translation (by M. J. Klein) as *Letters on Wave Mechanics* (Philosophical Library, Inc., New York, 1967).
71. W. Pauli, *Scientific Correspondence*, Vol. I (Springer-Verlag, Berlin and New York 1979).
72. M. Jammer, *The Conceptual Development of Quantum Mechanics* (McGraw-Hill, New York, 1966), pp. 285–289; reference to A. Einstein: M. Born, "Albert Einstein und das Lichtquantum," *Naturwissenschaften* **11**, 425–431 (1955).
73. A. Pais, *Inward Bound* (Oxford University Press, New York, 1986), p. 259.
74. G. Wentzel, "Zwei Bemerkungen über die Zerstreuung korpuscularer Strahlen als Beugungserscheinung," *Z. Phys.* **40**, 590–593 (1926).
75. E. Schrödinger, "Der stetige Übergang von der Mikro- zur Makromechenik," *Naturwissenschaften* **14**, 664–666 (1926).
76. E. H. Kennard, "Zur Quantenmechanik einfacher Bewegungstypen," *Z. Phys.* **44**, 326–352 (1927).
77. G. Holton, "The roots of complementarity," in *Thematic Origins of Scientific Thought* (Harvard University Press, Cambridge, 1973), pp. 115–161.
78. J. Mehra, "Satyendra Nath Bose, 1894–1974," *Biog. Mem. Fellows Roy. Soc.* **21** (1975).
79. W. Pauli, "Die allgemeinen Prinzipien der Wellenmechanik," *Handbuch der Physik*, Vol. 24, H. Geiger and K. Scheel, eds. (Springer, Berlin, 1933), and edn., p. 89.
80. C. F. von Weizsäcker, "Komplementarität und Logik," *Naturwissenschaften* **42**, 521–529, and 545–555 (1955).
81. A. Einstein, B. Podolsky, and N. Rosen, "Can quantum-mechanical description of physical reality be considered complete?", *Phys. Rev.* **47** 77–780 (1935).
82. N. Bohr, "Can quantum-mechanical description of physical reality be considered complete?", *Phys. Rev.* **48**, 696–702 (1935).
83. E. Schrödinger, "Die gegenwärtige Situation in der Quantenmechanik," *Naturwissenschaften* **23**, 807–812, 824–828, 844–849 (1935).