investment, keeps capital- and resource-rich U.S. companies out of supercomputer development.

American universities cannot offer immediate help because supercomputers are almost nonexistent in academia, and academicians have little access to supercomputers at other locations. For example, an advanced supercomputer of the early 1970's, the CDC 7600, was not installed on a single American campus. Thus academic research in supercomputer technology, academic applications of such technology, and the training of people in both are not available in the quantity needed. In contrast, several European universities have American supercomputers and Japanese universities are richly endowed with the latest Japanese equipment.

To maintain U.S. leadership, private and government-sponsored research is required to identify areas for development that are likely to yield technical and market success, thereby lowering the investment risk to a point where the private sector will take over. Collaboration among manufacturers, academia, and government laboratories will be necessary. The requirements for research in such areas as very large scale integration, fifth-generation capability, and algorithms may offer timely opportunities to private research organizations, such as the Microelectric and Computer Corporation (*6*) and the Semiconductor Research Corporation (*7*).

References and Notes

1. The Los Alamos representatives, all from the Computing Division, were Robert Ewald, Division Leader; Bill Buzbee, Assistant Division Leader; and Jack Worlton, Assistant Division Leader. The Livermore representatives, all from the Computation Department, were John Ranelletti, Department Head; George Michael, Group Leader, Computer Research; and Harry Nelson, Senior Program Analyst.
2. Key Japanese personnel and organizations visited included S. Fukuoka, Executive Vice President, Nippon Kogaku (Nikon); H. Kashiwagi, Director-General, Electronic Computer Division, Electro-Technical Laboratory; S. Ninomiya, Director and General Manager, Computer Group, Fujitsu; H. Horitoshi, Deputy Manager, 8th Department, Central Research Laboratory, Hitachi; H. Hatta, Engineering Manager, Systems Engineering Department, Computer Engineering Division, Nippon Electric Corporation; C. Tanaka, Manager, Computer Architecture Group, Mitsubishi; Y. Anraku, Corporate Director and General Manager, Systems Laboratory, Oki; S. Takayanagi, Deputy Director, Toshiba Research and Development Center, Toshiba; N. Kuroyanagi, Director of Research, Nippon Telephone and Telegraph; and T. Motooka, Professor of Electrical Engineering, University of Tokyo.
3. The nine completed national projects are: the superhigh performance electronic computer, a desulfurization process, a new method of producing olefin, a seawater desalination process that includes by-product recovery, a remotely controlled undersea oil drilling rig, an electric car, a comprehensive automobile control technology, a pattern information processing system, and a direct steel-making process that operates at a high temperature with a reducing gas.
4. *Management Information Systems Week* **3** (No. 36), 10 (8 September 1982).
5. Japan Information Processing Development Center, Preliminary Report on Study and Research on Fifth-Generation Computers 1979–1980 (Fall 1981).
6. D. Stamps, *Management Information Systems Week* **3** (No. 35), 4 (1 September 1982).
7. P. J. Klass, *Aviat. Week Space Technol.* **116** (No. 20), 181 (17 May 1982).

Max Born's Statistical Interpretation of Quantum Mechanics

A. Pais

Summary. In the summer of 1926, a statistical element was introduced for the first time in the fundamental laws of physics in two papers by Born. After a brief account of Born's earlier involvements with quantum physics, including his bringing the new mechanics to the United States, the motivation for and contents of Born's two papers are discussed. The reaction of his colleagues is described.

The introduction of probability in the sense of quantum mechanics—that is, probability as an inherent feature of fundamental physical law—may well be the most drastic scientific change yet effected in the 20th century. At the same time, this advent marks the end rather than the beginning of a scientific revolution, a term often used but rarely defined.

In the political sphere, revolution is a rather clear concept. One system is swept away, to be replaced by another with a distinct new design. It is otherwise in science, where revolution, like love, means different things to different people. Newspapermen and physicists have perceptions of scientific revolution which need not coincide. Nor would individual members of these or other professions necessarily agree on what a scientific revolution consists of. For example, the London *Times* of 7 November 1919 headed its first article on the recently discovered bending of light by: "Revolution in science . . . Newtonian ideas overthrown." Einstein, on the other hand, in a lecture given in 1921, deprecated the idea that relativity is revolutionary and stressed that his theory was the natural completion of the work of Faraday, Maxwell, and Lorentz. I happen to share Einstein's judgment, while other physicists will quite reasonably object that the abandonment of absolute simultaneity and of absolute space are revolutionary steps.

However, all of us would agree, I would think, that the *Times* statement "Newtonian ideas overthrown," being unqualified, tends to create the incorrect impression of a past being entirely swept away. That is not how science progresses. The scientist knows that it is in his enlightened self-interest to protect the past as much as is feasible, whether he be a Lavoisier breaking with phlogiston, an Einstein breaking with the aether, or a Max Born breaking with classical causality.

These tensions between the progressive and the conservative are never more in evidence than during a revolutionary period in science, by which I mean a period during which (i) it becomes clear that some parts of past science have to go and (ii) it is not yet clear which parts of the older edifice are to be reintegrated in a wider new frame. Such periods are initiated either by experimental observations that do not fit into accepted pictures or by theoretical contributions that make successful contact with the real world at the price of one or more as-

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Max Born and Norbert Wiener at MIT in November or December 1925, during which time they completed the first paper on quantum mechanics written in the United States. [Courtesy of the MIT Museum and Historical Collection]

sumptions which are in violation of the established corpus of theoretical physics.

The era of the so-called old quantum theory, the years from 1900 to 1926, constitutes the most protracted revolutionary period in modern science. Six theoretical papers appeared during that time which are revolutionary in the above sense: Planck's on the discovery of the quantum theory (1900); Einstein's on the light quantum (1905); Bohr's on the hydrogen atom (1913); Bose's on what came to be called quantum statistics (1924); Heisenberg's on what came to be known as matrix mechanics (1925); and Schroedinger's on wave mechanics (1926). If these papers have one thing in common it is that they contain at least one theoretical step which (whether the respective authors knew it then or not) could not be justified at the time of writing.

The end of this revolutionary period (I consider only nonrelativistic quantum mechanics) is not marked by a single date, nor was it brought about by a single person, but rather by three: Heisenberg, Born, and Bohr. The end phase begins in 1925 with the abstract of Heisenberg's extraordinary first paper on quantum mechanics, which reads: "In this paper it will be attempted to secure foundations for a quantum theoretical mechanics which is exclusively based on relations between quantities which in principle are observable." With these words Heisenberg states specific desiderata for a new axiomatics. His paper is the correct first step in the new direction. The end phase continues in 1926 with Born's remarks on probability and causality, and comes to a conclusion in 1927 with Heisenberg's derivation of the uncertainty relations and Bohr's formulation of complementarity. At that stage the basic ingredients had been provided which, in the course of time, were to allow for a consistent theoretical foundation of quantum mechanics, including a judgment of the way the new theory contains the old, the classical, theory as a limiting case.

Let us now turn to Max Born, illustrious descendant of the tribe of Abarbanel (*1*).

Born and the Quantum: 1912 to 1926

Born's active preoccupation with the quantum theory dates back to 1912, when he and Theodore von Kármán became the first to apply quantization conditions to collective modes of a many-body system: the normal modes of vibration of a crystal lattice. Ionic crystals were again at issue when, 6 years later, Born and Alfred Landé computed some of their properties in terms of Bohr's model for an ion: a set of electrons moving in planar orbits around the nucleus. They found that this picture did not work well: crystals were predicted to be too soft, their compressibility came out too high. The calculations indicated "that the electrons in a single atom are uniformly distributed in [all] spatial directions rather than in plane disks. . . . The planar orbits do not suffice, the atoms are evidently [three-dimensional] spatial structures . . . in this sense we must demand a generalization of the theory" (*1a*), a statement memorable for its prescience. Born's third confrontation with the limitations of the old quantum theory occurred another 5 years later, in 1923. This time he addressed that mystery, celebrated in its time, the spectrum of the helium atom. As others had before them, he and his young assistant Heisenberg concluded that the quantum rules of the old theory could not even qualitatively account for the helium spectrum.

Thus Born belongs to that select group of physicists who knew early that there was some truth to the old quantum theory, yet that this theory (if indeed one may call it that) was in deep ways totally inadequate. He had arrived at this knowledge not as a cynic on the sidelines, but as a participant in the struggles with quantum problems. He knew that a new mechanics was called for, and he was the one to name it, in 1924, even before its discovery: quantum mechanics (*2, 3*).

Heisenberg said later that "It was the peculiar spirit of Göttingen, Born's faith that nothing short of a new self-consistent quantum mechanics was acceptable as the goal in fundamental research that enabled [my] ideas to come to full fruition" (*4*). Indeed, during the 1920's, the final decade in which physics at the frontiers was quintessentially European, a new generation was preparing at four main schools for what was to come: Bohr's in Copenhagen, Born's in Göttingen, Rutherford's in Cambridge, and Sommerfeld's in Munich. The list of Born's assistants is impressive: Pauli, Heisenberg, Jordan, Hund, Hückel, Nordheim, Heitler, and Rosenfeld. At least 24 students received their Ph.D. with Born in Göttingen, among them Delbrück, Elsasser, Flügge, Hund, Jordan, Goeppert-Mayer, Nordheim, Oppenheimer, and Weisskopf. Visitors drawn to Göttingen in the 1920's (not only to Born, of course, but also to James Franck and to David Hilbert) include Blackett, Bohr, K. Compton, Condon, Davisson, Dirac, Ehrenfest, Fermi, Ph. Frank, Herzberg, Houtermans, Hylleraas, Joffe, Kapitza, Kramers, von Neumann, Pauling, Reichenbach, H. P. Robertson, Teller, Uhlenbeck, V. Fock, Wentzel, N. Wiener, and Wigner. "In the winter of 1926," Compton recalled, "I found more than twenty Americans in Göttingen at this fount of quantum wisdom" (*5*).

It must be remembered that Born was in his middle forties when he did his work on the statistical interpretation of quantum mechanics. By that time he was already a renowned physicist and teacher, had published more than a hundred research papers, and had written six books. Likewise, Bohr was already a stellar figure in his forties when he gave the complementarity interpretation of quantum mechanics. However, the creators of quantum mechanics—Heisenberg, Dirac, Jordan, and Pauli—were in their twenties in 1925, the years when it began. Thus the period 1925 to 1927 would become known in Göttingen as the years of "Knabenphysik": boy physics. Schroedinger does not easily fit into this simplistic scheme; he was thirty-eight at that time. It does not seem out of place, however, to note here the remark, once made to me by Hermann Weyl, that Schroedinger did his great work during a late erotic outburst in his life. Nor should it be forgotten that Schroedinger was the only one among the creators of the new mechanics who never found peace with what he had wrought.

Let us recall a few dates, all in 1925, all referring to the times of receipt by journals: 29 July, Heisenberg's first paper on quantum mechanics (*6*); 27 September, recognition by Born and Jordan that Heisenberg's mechanics is a matrix mechanics (*7*) and first proof of the relation $pq - qp = h/2\pi i$, where p is a momentum, q is the corresponding coordinate, and h is Planck's constant; 7 November, independent proof of the same relation by Dirac (*8*); and 16 November, first comprehensive treatment of the foundations of matrix mechanics, by Born, Heisenberg, and Jordan (*9*).

It was Born who first brought the new dynamics to America. On 2 November 1925 he left Göttingen for a visit to Massachusetts Institute of Technology (MIT). Part of the series of lectures he gave there, from 14 November 1925 to 22 January 1926, was devoted to quantum theory. Their published version (*10*) is the first book to appear which deals with quantum mechanics. Before returning to Göttingen, Born also lectured at the Universities of Chicago, Wisconsin, and California (Berkeley), at Caltech, and at Columbia University.

At the time Born left for the United States, all the publications (two) on quantum mechanics were Göttingen products. Interest in this work was spreading, however. Others had begun thinking, but few had as yet much of a grasp of what was happening. The mathematics was unfamiliar, the physics intransparent. In September, Einstein wrote to Ehrenfest about Heisenberg's paper: "In Göttingen they believe in it (I don't)" (*11*). At about that same time, Bohr considered the work of Heisenberg to be "a step probably of fundamental importance" but noted that "it has not yet been possible to apply [the] theory to questions of atomic structure" (*12*). Whatever reservations Bohr initially may have had, these were dispelled by early November (*12*, footnote 17), when word reached him (*13*) that Pauli had done for matrix mechanics what he himself had done for the old quantum theory: derive the Balmer formula for the discrete spectrum of hydrogen.

Let us return to MIT. Born was one of the authors of the first paper on quantum mechanics to be written in the United States. Heisenberg's mechanics, as it stood then, was specifically designed for dealing with discrete energy spectra. At MIT, Born and Norbert Wiener developed a general operator calculus that could be applied to the discrete as well as to the continuous case. They were proud to be the first to solve a continuum problem: the motion of a free particle in one dimension (*14*). (Their methods have since been superseded.) As we will see, Born's early involvement with continuum problems was crucial for his discovery of the quantum mechanical probability concept.

Summer of 1926

By the time Born returned to Göttingen from his American journey, Schroedinger had discovered wave mechanics and had derived the complete spectrum of the hydrogen atom (*15*). Uhlenbeck told me: "The Schroedinger theory came as a great relief, now we did not any longer have to learn the strange mathematics of matrices." Rabi told me how he looked through Born's book *Atommechanik* for a nice problem to solve by Schroedinger's method, found the symmetric top, went to Kronig, and said: "Let's do it." They did (*16*). Wigner told me: "People began making calculations but it was rather foggy."

Indeed, until the spring of 1926 quantum mechanics, whether in its matrix or its wave formulation, was high mathematical technology of a new kind, manifestly important because of the answers it produced, but without clearly stated underlying physical principles. Schroedinger was the first, I believe, to propose such principles in the context of quantum mechanics, in a note completed not later than May, which came out on 9 July (*17*). He suggested that waves are the only reality; particles are only derivative things. In support of this monistic view he considered a suitable superposition of linear harmonic oscillator wave functions and showed (his italics): "Our wave group holds *permanently together*, does *not* expand over an ever greater domain in the course of time," adding that "it can be anticipated with certainty" that the same will be true for the electron as it moves in high orbits in the hydrogen atom. Thus he hoped that wave mechanics would turn out to be a branch of classical physics—a new branch, to be sure, yet as classical as the theory of vibrating strings or drums or balls.

Schroedinger's calculation was right; his anticipation was not. The case of the oscillator is very special: wave packets do almost always disperse. Being a captive of the classical dream, Schroedinger missed a second chance at interpreting his theory correctly. On 21 June 1926 his paper (*18*) on the nonrelativistic time-dependent wave equation was received. It contains in particular the one-particle equation (I slightly modify his notations)

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

(where ψ is the wave function, t is time, $\hbar$ is Planck's constant divided by 2π, Δ is the Laplace operator, and V is a potential), and its conjugate and the corresponding continuity equation,

$$\frac{\partial\rho}{\partial t} + \operatorname{div}\mathbf{j} = 0 \tag{1}$$

$$\rho = \psi^*\psi$$

$$\mathbf{j} = \frac{i\hbar}{2m}(\psi^*\nabla\psi - \nabla\psi^* \cdot \psi)$$

Equation 1, Schroedinger believed, had to be related to the conservation of electric charge.

The break with the past came in a paper by Born received 4 days later, on 25 June 1926. In order to make his decisive new step, "It is necessary [Born wrote half a year thereafter (*19*)] to drop completely the physical pictures of Schroedinger which aim at a revitalization of the classical continuum theory, to retain only the formalism and to fill that with new physical content."

In his June paper (*20*), entitled "Quantum mechanics of collision phenomena," Born considers (among other things) the elastic scattering of a steady beam of particles with mass m and velocity v in the z direction by a static potential which falls off faster than $1/r$ at large distances. In modern language, the stationary wave function describing the scattering behaves asymptotically as $\exp(ikz) + f(\theta,\phi)\exp(ikr)/r$, $k = mv/\hbar$. The number of particles scattered into the element of solid angle $d\omega = \sin\theta\, d\theta\, d\phi$ is given by $N|f(\theta,\phi)|^2\, d\omega$, where N is the number of particles in the incident beam crossing a unit area per unit time. In order to revert to Born's notation, replace $f(\theta,\phi)$ by Φ_{mn}, where "n" denotes the initial-state plane wave in the z direction and "m" the asymptotic final state in which the wave moves in the (θ,ϕ) direction. Then, Born declares, "Φ_{mn} determines the probability for the scattering of the electron from the z direction into the direction $[\theta,\phi]$."

At best, this statement is vague. Born added a footnote in proof to his evidently hastily written paper: "A more precise consideration shows that the probability is proportional to the square of Φ_{mn}." He should have said "absolute square." But he clearly had got the point, and so the correct expression for the transition probability concept entered physics by way of a footnote.

I will return shortly to the significant fact that Born originally associated probability with Φ_{mn} rather than with $|\Phi_{mn}|^2$. As I learned from recent private discussions, Dirac had the same idea at that time. So did Wigner, who told me that some sort of probability interpretation was then on the minds of several people and that he, too, had thought of identifying Φ_{mn} or $|\Phi_{mn}|$ with a probability. When Born's paper came out and $|\Phi_{mn}|^2$ turned out to be the relevant quantity, "I was at first taken aback but soon realized that Born was right," Wigner said.

If Born's paper lacked formal precision, causality was brought sharply into focus as the central issue: "One obtains the answer to the question, *not* 'what is the state after the collision' but 'how probable is a given effect of the collision.' . . . Here the whole problem of determinism arises. From the point of view of our quantum mechanics there exists no quantity which in an individual case causally determines the effect of a collision. . . . I myself tend to give up determinism in the atomic world." However, he was not yet quite clear about the distinction between the new probability in the quantum mechanical sense and the old probability as it appears in classical statistical mechanics: "It does not seem out of the question that the intimate connection which here appears between mechanics and statistics may demand a revision of the thermodynamic-statistical principles."

One month after the June paper, Born completed a sequel with the same title (*21*). His formalism is firm now and he makes a major new point. He considers a normalized stationary wave function ψ referring to a system with discrete, nondegenerate eigenstates ψ_n and notes that in the expansion

$$\psi = \Sigma c_n \psi_n$$

$|c_n|^2$ is the probability for the system to be in the state n. In June he had discussed probabilities of transition, a concept that, at least phenomenologically, had been part of physics since 1916, when Einstein had introduced his A and B coefficients in the theory of radiative transitions—and at once had begun to worry about causality (*22*). Now Born introduced the probability of a state. That had never been done before. He also expressed beautifully the essence of wave mechanics: "The motion of particles follows probability laws but the probability itself propagates according to the law of causality."

During the summer of 1926 Born's insights into the physical principles of quantum mechanics developed rapidly. On 10 August he read a paper before the meeting of the British Association at Oxford (*23*) in which he clearly distinguished between the "new" and the "old" probabilities in physics: "The classical theory introduces the microscopic coordinates which determine the individual processes only to eliminate them because of ignorance by averaging over their values; whereas the new theory gets the same results without introducing them at all. . . . We free forces of their classical duty of determining directly the motion of particles and allow them instead to determine the probability of states. Whereas before it was our purpose to make these two definitions of force equivalent, this problem has now no longer, strictly speaking, any sense."

The history of science is full of gentle irony. In teaching quantum mechanics, most of us arrive at Eq. 1, note that something is conserved, and identify that something with probability. But Schroedinger, who discovered that equation, did not make that connection and never liked quantum probability, while Born introduced probability without using Eq. 1.

In this article I do not at all attempt to describe all aspects of the history of probability in quantum physics. However, I cannot refrain from mentioning a remark found in a paper, completed in December 1926, in which for the first time in print the probability for a many-particle system with coordinates q_1, . . . , q_f is introduced: "$|\psi(q_1, \ldots, q_f)|^2 \, dq_1 \cdots dq_f$ is the probability that, in the relevant quantum state of the system, the coordinates simultaneously lie in the relevant volume element of configuration space." The paper is by Pauli and deals with gas degeneracy and paramagnetism. The remark was inspired by Born's work and is found—once again—in a footnote (*24*).

What Made Born Take This Step?

In 1954 Born was awarded the Nobel Prize "for his fundamental research, especially for his statistical interpretation of the wave function." In his acceptance speech Born, then in his seventies, ascribed his inspiration for the statistical interpretation to "an idea of Einstein's [who] had tried to make the duality of particles—light-quanta or photons—and waves comprehensible by interpreting the square of the optical wave amplitudes as probability density for the occurrence of photons. This concept could at once be carried over to the ψ-function: $|\psi|^2$ ought to represent the probability density for electrons" (*25*). Similar statements are frequently found in Born's writings in his late years. On the face of it, this appears to be a perfectly natural explanation. Had Einstein not stated that light of low intensity behaves as if it consisted of energy packets $h\nu$? And is the intensity of light not a function quadratic in the electromagnetic fields? In spite of this plausibility, and in spite of the fact that I must here dissent from the originator's own words, I do not believe that these contributions by Einstein were Born's guide in 1926 (*26*).

My own attempts at reconstructing Born's thinking (necessarily a dubious enterprise) are exclusively based on his two papers on collision phenomena and on a letter he wrote to Einstein, also in 1926. Recall that Born initially thought, however briefly, that ψ rather than $|\psi|^2$ was a measure of the probability. I find this impossible to understand if it was true that, at that time, he had been stimulated by Einstein's brilliant discussions of the fluctuations of quadratic quantities (in terms of fields) referring to radiation. Nevertheless, it is true that Born's inspiration came from Einstein: not Einstein's statistical papers bearing on light, but his never published speculations during the early 1920's on the dynamics of light quanta and wave fields. Born states so explicitly in his second paper (*21*): "I start from a remark by Einstein on the relation between [a] wave field and light-quanta; he said approximately that the waves are only there to show the way to the corpuscular light-quanta, and talked in this sense of a 'ghost field' [Gespensterfeld] [which] determines the *probability* [italics added] for a light-quantum . . . to take a definite path."

It is hardly surprising that Einstein was concerned that early with these issues. In 1909 he had been the first to write about particle-wave duality. In 1916 he had been the first to relate the existence of transition probabilities (for spontaneous emission of light) to quantum theoretical origins—though how this relation was to be formally established he did, of course, not yet know. Little concrete is known about his ideas of a ghost-field or guiding field (Führungsfeld). The best description we have is from Wigner (*27*), who knew Einstein personally in the 1920's: "[Einstein's] picture has a great similarity with the present picture of quantum mechanics. Yet Einstein, though in a way he was fond of it, never published it. He realized that it is in conflict with the conservation principles. . . . This Einstein never could accept and hence never took his idea of the guiding field quite seriously. . . . The problem was solved, as we know, by Schroedinger's theory" (*28*).

Born was even more explicit about his source of inspiration in a letter to Einstein (*29*) written in November 1926 (for reasons not clear to me this letter is not found in the published Born-Einstein correspondence): "About me it can be told that physicswise I am entirely satisfied since my idea to look upon Schroedinger's wave field as a 'Gespensterfeld' in your sense proves better all the time. Pauli and Jordan have made beautiful advances in this direction. The probability field does of course not move in ordinary space but in phase- (or rather, in configuration-) space. . . . Schroedinger's achievement reduces itself to something purely mathematical; his physics is quite wretched [recht kümmerlich]."

Thus it seems to me that Born's thinking was conditioned by the following circumstances. He knew and accepted the fertility of Schroedinger's formalism but not Schroedinger's attempt at interpretation: "He [Schroedinger] believed . . . that he had accomplished a return to classical thinking; he regarded the electron not as a particle but as a density distribution given by the square of his wave function $|\psi|^2$. He argued that the idea of particles and of quantum jumps be given up altogether; he never faltered in this conviction. . . . I, however, was witnessing the fertility of the particle concept every day in [James] Franck's brilliant experiments on atomic and molecular collisions and was convinced that particles could not simply be abolished. A way had to be found for reconciling particles and waves" (*30*). His quest for this way led him to reflect on Einstein's idea of a ghost field. It now seems less surprising that his first surmise was to relate probability to the ghost field, not to the "(ghost field)^2." His next step, from ψ to $|\psi|^2$, was entirely his own. We owe to Born the beginning insight that ψ itself, unlike the electromagnetic field, has no direct physical reality.

Born's work on the statistical interpretation occupies a singular position in his oeuvre. It is his most innovative contribution. At first glance this choice of scientific problem seems somewhat unlike Born. As Heisenberg once said, "Born was more of a mathematician" (*31*), more the man for the "problème bien posé." It seems not entirely farfetched, however, to consider Born's problem of June and July 1926 to be just of that kind: "A way had to be found for reconciling particles and waves." It should also be noted that Born may not have realized at once the profundity of his contribution, which helped bring to an end the quantum revolution. In a later interview he reminisced as follows about 1926: "We were so accustomed to making statistical considerations, and to shift it one layer deeper seemed to us not very important" (*32*).

Changing of the Guard

Born wrote to Einstein about the ghost field on 30 November 1926. Einstein's reply of 4 December is the oft-quoted letter in which he wrote: "The theory [quantum mechanics] says a lot but does not really bring us any closer to the secret of the 'old one.' I, at any rate, am convinced that *He* is not playing at dice" (*33*). Also, the attitudes of the other leaders of the once dominant Berlin school—Planck, von Laue, and Schroedinger—continued to range from scepticism to opposition. In the first week of October 1926 Schroedinger went to Copenhagen, at Bohr's invitation, to discuss the status of the quantum theory. Heisenberg also went. Later, Bohr often told others (including me) that Schroedinger reacted on that occasion by saying that he would rather not have published his papers on wave mechanics, had he been able to foresee the consequences. Schroedinger continued to believe that one should dispense with particles. Born continued to refute him. After Schroedinger's death, Born, mourning the loss of his old friend, wrote of their arguments through the years: "Extremely coarse [saugrob] and tender; sharpest exchange of opinion, never a feeling of being offended" (*34*).

After Born's work, Lorentz could no longer grasp the changes wrought by the quantum theory. In the summer of 1927 he wrote to Ehrenfest: "I care little for the conception of $\psi\psi^*$ as a probability. . . . In the case of the H-atom, the difficulty in making precise what is meant if one interprets $\psi\psi^*$ as a probability manifests itself in that for a given value of E (one of the eigenvalues) there is also a [nonvanishing] probability outside the sphere which electrons with energy E cannot leave" (*35*).

The quantum revolution was over by October 1927, the time of the fifth Solvay Conference. In March of that year, Heisenberg had derived the uncertainty relations; in September, Bohr had lectured for the first time on complementarity. The printed proceedings of this Solvay meeting (*36*) appeared in 1928. They open with a tribute by Marie Curie to Lorentz, who had presided over the conference in October and who had died shortly thereafter. Next follows a list of the participants, which includes Planck, Einstein, Bohr, de Broglie, Born, Schroedinger, and the youngsters, Dirac, Heisenberg, Kramers, and Pauli. Then come the texts of the papers presented. Taken as a whole, this record reads as an account of a changing of the guard.

What was created in those stirring years is still with us. To this day there are physicists, some of them quite thoughtful, who are uncomfortable with the probability interpretation. However, there are neither experimental nor theoretical arguments that force us to believe in the necessity for a revision of the rules of the nonrelativistic quantum theory. I do not care to speculate about the future, but I would like to conclude by repeating a comment, made more than a quarter of a century ago, which is still timely: "It has been well said that the modern physicist is a quantum theorist on Monday, Wednesday, and Friday, and a student of gravitational relativity theory on Tuesday, Thursday, and Saturday. On Sunday the physicist is neither, but is praying to his God that someone, preferably himself, will find the reconciliation between these two views" (*37*).

Postscript: The Born Approximation

It is a bit odd—and caused Born some chagrin—that his papers on the probability concept were not always adequately acknowledged in the early days. Heisenberg's own version (*38*) of the probability interpretation, written in Copenhagen in November 1926, does not mention Born. One finds no reference to Born's work in the two editions of Mott and Massey's book on atomic collisions, nor in Kramers' book on quantum mechanics. In his authoritative *Handbuch der Physik* article of 1933, Pauli refers to this contribution by Born only in passing, in a footnote. Jörgen Kalckar from Copenhagen wrote to me about his recollections of discussions with Bohr on this issue. "Bohr said that as soon as Schroedinger had demonstrated the equivalence between his wave mechanics and Heisenberg's matrix mechanics, the 'interpretation' of the wave function was obvious. . . . For this reason, Born's paper was received without surprise in Copenhagen. 'We had never dreamt that it could be otherwise,' Bohr said." A similar comment was made by Mott: "Perhaps the probability interpretation was the most important of all [of Born's contributions to quantum mechanics], but given Schroedinger, de Broglie, and the experimental results, this must have been very quickly apparent to everyone, and in fact when I worked in Copenhagen in 1928 it was already called the 'Copenhagen interpretation'—I do not think I ever realized that Born was the first to put it forward" (*39*). In response to a query, Casimir, who started his university studies in 1926, wrote to me: "I learned the Schroedinger equation simultaneously with the interpretation. It is curious that I do not recall that Born was especially referred to. He was of course mentioned as co-creator of matrix mechanics." The same comments apply to my own university education, which started a decade later.

It is otherwise with another contribution found in the second of Born's 1926 papers on collisions: the Born approximation, taught in every sensible course on quantum mechanics and still in steady use wherever quantum physics is practiced. Of course, later generations of students rarely had grounds for consulting Born's original paper. Long before preparing this article I had occasion to do so, however: once in the course of refining the Born approximation (*40*), and another time when Jost and I became interested in the convergence of the Born expansion for the scattering by a static, spherically symmetric potential which, with suitable normalization, can be written as $\lambda V(r)$, where λ is the potential strength. Write the scattering wave function ψ as a power series in λ. The question was whether, under certain conditions imposed on $V(r)$, this power series, the Born expansion, converges. We found general conditions on V for which ψ can be written as the quotient of two convergent power series in λ, and from this result obtained a way of determining the radius of convergence for the Born expansion (*41*).

Having finished our work, we wondered what had been done earlier about this convergence question. We searched the literature, found nothing more concrete than assertions that the expansion will be the more trustworthy the higher the energy or the smaller the quantity $|\lambda|$, until we finally discovered that Born had considered our question in his second paper of 1926 on collision theory (*21*). He first discussed the one-dimensional case for potentials such that $|V(x)| <$ constant $\cdot\, x^{-2}$, and correctly showed that under these circumstances his expansion converges uniformly for any finite interval. This result may have led him to conclude, for the three-dimensional case: "The convergence of the procedure can easily be shown on the assumption that V tends to zero as r^{-2}; but we will not go into detail." That statement, alas, was incorrect.

Returning to our own work, we were encouraged to inquire whether we could also do something for relativistic field theories. We failed. The kernels encountered in that case were too singular for our methods to apply. To this day, proofs or disproofs of the convergence of the Born expansion in field theory remain an important challenge, yet to be met.

References and Notes

1. Some time after Born's Sephardic ancestors came to Germany, the family name was changed to Born (Mrs. Irene Newton John-Born, private communication).

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42. I am grateful to Professor K. P. Lieb from Göttingen for a helpful correspondence.